The Power of Numbers: Base-Ten Threshold Effects in Reported Revenue

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ABSTRACT
We document that managers have a propensity to disproportionately report total revenue numbers just above base-ten thresholds (e.g., ten million, thirty million, one billion). Further tests investigate whether managers are motivated to beat these thresholds because of pressure to beat revenue targets set in executive compensation contracts, analyst forecasts, or a manager’s own forecasts. We document that base-ten thresholds are unusually prevalent in all three types of revenue forecasts; however, these incentive effects do not offer a complete explanation because threshold beating behavior is observed even in the absence of these targets. We find firms that beat a base-ten threshold for the first time experience an increase in visibility across a wide range of channels even after controlling for whether they have beaten other common benchmarks. Specifically, news coverage, institutional ownership, liquidity, and analyst following increase in the years immediately after a threshold is attained. The salience of beating these thresholds appears to increase investor recognition of these firms.

JEL Classification: G01, M4, M41
Key Words: Base-Ten Thresholds, Firm Visibility, Target-Beating
I. Introduction

Carslaw (1988) and Thomas (1989) demonstrated unusual patterns in reported earnings by showing that firms are disproportionately likely to report positive earnings numbers where the second digit is 0 (e.g., $5,000,000) and disproportionately unlikely to report positive earnings numbers where the second digit is 9 (e.g., $4,900,000). They conclude that managers appear to have a preference to report earnings just above base-ten numbers of the form of \( N \times 10^K \), where \( N \) and \( K \) are integers; that is, numbers that start with an integer followed by only zeroes. Examples are $1 million \((1 \times 10^6)\), $300 million \((3 \times 10^8)\), $7 billion \((7 \times 10^9)\), and so forth. However, neither Thomas (1989) nor Carslaw (1988), nor any paper in the intervening 30 years, has been able to provide evidence for why managers might exhibit this preference, or whether this appears to constitute an economically meaningful phenomenon rather than a peculiar empirical regularity.

We explore this base-ten preference in the context of reported revenue numbers and find results similar to those documented in prior literature – firms appear to favor reporting revenue just above a base-ten threshold compared to just below a base-ten threshold. We next extend the prior literature by examining whether firms tend to report numbers just above base-ten thresholds because of their prevalence in formal and informal contract targets, for example executive compensation plans, management forecasts, and analyst forecasts. Our evidence shows that base-ten thresholds are unusually prevalent in these types of targets, but this base-ten preference exists even in the absence of these explicit targets. As a result, our results appear to document a different type of benchmark-beating behavior than has previously been studied in the accounting literature. Finally, we document that firms which reach a new base-ten revenue target for the first time experience disproportionately large increases in visibility. This suggests a behavioral bias on the part of market participants leads them to pay a disproportionate amount of attention to base-ten
thresholds, where beating a new threshold leads to a shock in firm visibility. As a result, firms appear to be rationally responding to the behavioral bias of market participants by pushing to beat base-ten thresholds.

While base-ten thresholds can arise in the context of a variety of financial metrics, there are several key advantages to and benefits from focusing on revenues. First, we expect the power of our tests to be particularly strong for revenues compared to other financial metrics such as total assets, sales growth, or ROA because reported revenues are an especially prominent performance target. We find empirical evidence that revenues are prevalent performance metrics throughout our sample period and that revenue targets are even more common than EPS targets in the most recent years of our sample.\(^1\)\(^2\) Similarly, the monotonically increasing nature of revenues makes them a powerful setting in which to examine base-ten thresholds, which are reached sequentially by increasing the metric of interest. Therefore, we expect that base-ten thresholds, which also increase monotonically, will be particularly salient thresholds in the context of revenues, unlike earnings which net revenues and expenses.

A large body of research in fields outside of accounting has documented the salience of base-ten numbers in other settings. For example, Rosch (1975) concludes that humans use numbers that are factors of ten as reference points when evaluating all other numbers, and Schindler and Wiman (1989) find that whole numbers are easier to remember and come to mind more readily. As a result of this preference, which amounts to a processing constraint favoring base-ten numbers, base-ten thresholds may be disproportionately common in revenue targets if those setting the

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\(^1\) Descriptive statistics reported in Table 2 show that revenue targets are second only to EPS targets in overall prevalence in executive compensation plans, management forecasts, and analyst forecasts throughout our sample period, with a steady increase in prevalence over time (Figure 1) so that revenue targets are more prevalent than EPS targets for all three metrics by 2009.

\(^2\) This is consistent with prior evidence that shows that revenue is a key target for financial performance management (i.e., real earnings management) (e.g., Roychowdhury, 2006) and that it contains incremental information content beyond earnings (e.g., Jegadeesh and Livnat, 2006).
targets rely on base-ten thresholds as a heuristic to pick a specific target within a potential reasonable range. Further, it may also lead market participants to find base-ten thresholds unusually salient and cause them to pay more attention to firms that report revenues above a base-ten threshold. This increase in attention may then lead to a net increase in buying by investors and a net increase in coverage by the media and analysts (Odean, 1998; Barber and Odean, 2008). If revenue targets are set disproportionately at base-ten thresholds, and if investors, analysts, and business reporters exhibit a preference for base-ten thresholds, then it would be rational for managers to expend effort to reach a base-ten revenue threshold in order to obtain the benefits of beating these revenue thresholds. We examine both of these potential drivers of base-ten threshold beating.³

Empirically, we use approximate randomization techniques to establish that firms are significantly more likely to report revenue just above base-ten thresholds ($10 million, $200 million, $7.0 billion, and so forth) than just below.⁴ We also demonstrate that this base-ten revenue threshold effect is the result of intentional actions and not a byproduct of the data generation process or an innocuous rounding of all financial numbers. In the Appendix we also document that this effect exists for non-US firms reporting revenues in local currencies and that this threshold effect does not appear to be an artifact of rounding by either managers or data aggregators.

We next examine the extent to which base-ten thresholds are prevalent in revenue targets and the extent to which this drives the observed distribution of actual revenues. We show that revenue targets specified in formal and informal contracts (e.g., executive compensation contracts, ³ In our study we focus on the incentive of managers to specifically report revenues higher than base-ten thresholds, as opposed to a general preference for rounding up (e.g., Das and Zhang, 2003). In the former case, managers have an incremental incentive to report revenues just above base-ten thresholds; in the latter they have an equal incentive to round all financial metrics up, regardless of their location on the number line. ⁴ Our approximate randomization technique relies on the properties of randomly-generated distributions and is widely used (see e.g., Noreen 1989; Dichev and Skinner, 2002). We discuss our methodology in greater detail in Section 3.
management forecasts, and analyst forecasts) exhibit a base-ten threshold effect and are far more likely to be set at or just above a base-ten threshold than just below. This phenomenon serves as an additional driver of the base-ten bias in reported revenues, and we observe that firms with targets set at base-ten thresholds are more likely to report revenue beating a threshold than missing it. However, incentives to beat these targets frequently studied in the prior literature provide only a partial explanation for the unusual distribution of reported revenues that we observe. We also find that even firms with no observable revenue targets are disproportionately likely to report revenues just above base-ten thresholds. This represents a new type of threshold-beating behavior that was previously unexamined and indicates that the set of explicit targets commonly studied in the prior literature are not the only targets to which firms respond. Firms also appear to engage in threshold-beating behavior to overcome implicit targets, or those not explicitly stated in a formal or informal contract, such as base-ten thresholds.

We next explore further other incentives firms may have to beat base-ten thresholds by examining firm visibility. We find that firms which exceed a base-ten revenue threshold for the first time experience increases in press coverage, institutional ownership, liquidity, and analyst following. This suggests that market participants use base-ten thresholds in heuristics to allocate their attention. Given that increased visibility and firm recognition can affect other market outcomes with ties to firm value (Bushee et al., 2010; Drake et al., 2014; Fang and Peress, 2009; Merton, 1987), expected increases in visibility provide a powerful incentive for firms to beat base-ten thresholds. This suggests that the visibility outcomes that we document provide a powerful incentive for firms to beat base-ten revenue thresholds, even in the absence of explicit external targets. This is in spite of the fact that these thresholds have no economic significance in and of
themselves, and even after controlling for whether firms beat other commonly studied thresholds such as analyst forecasts.

Our study has important implications for practitioners and academics. First, our results are important in understanding the role of goal-setting in motivation (Locke and Latham, 2002). While Allen et al. (2016) find that round numbers provide an unusually powerful and salient benchmark to motivate individual effort, we demonstrate that base-ten thresholds can affect performance at the organization level.

Second, our revenue results provide evidence that the “one number” mentality (Dichev et al., 2013) in which managers only care about bottom-line income for both internal decision-making and external reporting may not be as dominant as once assumed. Indeed, our results indicate that the level of reported revenue itself is an important market focal point, and that the level of the reported revenue number is an object of strategic management.

Third, our study provides evidence on the motives and consequences for firms that meet base-ten revenue thresholds. Although prior evidence has documented unusual patterns around base-ten thresholds in earnings (Carslaw, 1988; Thomas, 1989), no prior studies have tied these patterns to economic incentives or actions on the part of firms. Not only do we show that firms appear to be responding to revenue targets set around these thresholds; we also find that firms which beat a base-ten revenue threshold for the first time benefit from increased visibility. Far from simply being a curious empirical regularity, we present evidence that the behavior around base-ten revenue thresholds is an economically significant phenomenon.

Lastly, base-ten thresholds provide an ideal setting in which to examine target-beating behavior and its market consequences because they exist independent of the information environment surrounding a firm (Bissessur and Veenman, 2016; Kaplan et al, 2018). In addition,
unlike metrics such as zero earnings which can be interpreted as the breakeven point of the firm, base-ten thresholds have no inherent economic meaning and beating them provides no incremental economic signal relative to any other arbitrary point on the number line (Dyreng et al., 2017; Hayn, 1995). Recent theoretical and empirical work has also provided several reasons why discontinuities may exist in the distribution of earnings around zero even in the absence of strategic actions on the part of firms (Beaver, McNichols, Nelson, 2007; Bonham, 2018; Hemmer and Labro, 2019; Li, 2014) making it even more difficult to identify intentional target-beating in that setting. Base-ten thresholds in revenue allow us to identify threshold-beating behavior and market responses without the effects of these confounding factors, thus providing a key contribution to the literature on earnings discontinuities (Burgstahler and Chuk, 2015; Durtschi and Easton, 2005, 2009) and target-beating more generally (e.g., Bhojraj et al., 2009; Roychowdhury, 2006).

II. Background and Hypothesis Development

Numbers that are factors of ten tend to be easier for humans to process. Rosch (1975) concludes that humans use numbers that are factors of ten as reference points, Schindler and Wiman (1989) find that whole numbers are easier to remember and come to mind more readily, and Tversky and Kahneman (1973) show that when making decisions individuals give excess weight to information that is easily retrieved from memory. Consistent with this, research has shown that individuals have a strong tendency to disproportionately produce whole numbers when required to generate random numbers (Huttenlocher et al., 1990; Turner, 1958; Whynes et al., 2005) and that consumers perceive numbers and prices just below whole numbers (e.g., $29.99) to be disproportionately smaller than the whole number above (e.g., $30.00) (Brenner and Brenner, 1982; Gabor and Granger, 1964).
At the same time, a large body of work in finance has documented that processing constraints and psychological biases may affect financial decision making.\(^5\) If preparers and users of financial information exhibit a preference for base-ten thresholds as result of a processing constraint that favors base-ten numbers, then this may affect the generation and response to reported accounting numbers. Additionally, if those who set compensation targets and analyst forecasts rely on base-ten thresholds as a heuristic, then managers will attempt to surpass base-ten thresholds with higher frequency, all else equal. Consistent with this intuition, two studies in accounting demonstrate that firms tend to disproportionately report *earnings* just above base-ten thresholds (Carslaw, 1988; Thomas, 1989). Additionally, a vast literature provides evidence of earnings management employed to reach a variety of other (non-base-ten) targets (see e.g., Abarbanell and Lehavy, 2003; Bartov et al., 2002; Burgstahler and Dichev, 1997; Dichev and Skinner, 2002; Healy 1985). Together, if market participants imbed a preference for base-ten numbers into their target and goal setting processes, then we predict that base-ten thresholds will be disproportionately common in reported revenue. Formally, we predict:

**H1:**  *Firms are more likely to report revenue just above a base-ten threshold than just below.*

While observing a base-ten threshold effect in reported revenue is consistent with a variety of explanations tied to the salience of base-ten numbers in general, observing evidence consistent with H1 does not help us to distinguish which of these possible explanations is actually driving the observed results. Indeed, while Carslaw (1988) and Thomas (1989) document evidence of the existence of this effect in earnings, neither attempts to disentangle the economic forces driving this regularity. Therefore, we explore several potential drivers of this phenomenon in more detail.

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\(^5\) Models in behavioral finance have examined overconfidence (Daniel et al., 1998; Kyle and Wang, 1997; Odean, 1998), limited attention (Hirshleifer and Teoh, 2003; Peng and Xiang, 2006), and cumulative prospect theory (Barberis and Huang, 2008).
Managers may engage in base-ten threshold beating behavior because they are incentivized to do so by explicit formal or informal contracts. One formal contract, executive compensation plans, has long been examined in the accounting literature. These contracts incentivize managers to exert costly effort to attain predetermined performance targets. If board members are subject to a base-ten threshold bias, then the revenue targets in compensation contracts will be disproportionately set at or above base-ten thresholds. If this is the case, managers will have an incentive to meet these base-ten revenue targets in order to earn bonus compensation, leading to a corresponding base-ten threshold effect in actual reported revenue.

Similarly, management forecasts and analyst forecasts represent informal contracts with market participants, setting a performance expectation for both managers and investors. Therefore, managers have an incentive to report accounting numbers that meet or beat analyst and management forecasts. However, both managers and analysts are subject to processing constraints and may fixate to some extent on base-ten thresholds when generating their forecasts (Bamber et al., 2010; Herrmann and Thomas, 2005). If this is the case, then the presence of a base-ten threshold effect in analyst and management revenue forecasts may lead firms, which have an incentive to meet or beat these biased thresholds, to report revenue which also exhibits this threshold effect.

Given the reasoning above, we predict:

\( H2a: \text{ The presence of a base-ten threshold effect in formal and informal contract revenue targets increases the probability that firms will report revenue that is just above a base-ten threshold. } \)

However, even in the absence of explicit targets from formal and informal contracts, firms may still be more likely to report revenues just above base-ten thresholds if the managers or employees of those firms are subject to a base-ten bias which leads them to fixate on base-ten thresholds or if firm managers expect that a base-ten bias on the part of market participants will
lead to disproportionate benefits to firms whose revenues exceed base-ten thresholds. Evidence in support of either of these conjectures would be especially important because it would document target-beating behavior on the part of firms even in the absence of explicit targets, which has not previously been documented in the accounting literature. As a result, our last prediction is:

**H2b:** *Even in the absence of formal and informal contract revenue targets, firms are more likely to report revenue that is just above a base-ten threshold than just below.*

To explore the incentives of firms to beat base-ten thresholds in the absence of explicit targets, we focus on one potential expected benefit of beating a base-ten threshold: increased visibility. Increases in firm visibility benefit firms broadly, including the benefits that come from increases in investor recognition (Merton, 1987). Visibility can take many forms including press coverage, trading or liquidity, analyst following, and institutional ownership. As discussed earlier, whole numbers stand out more and are more likely to be remembered (Schindler and Wiman, 1989). This additional attention can influence journalists when they are choosing which firms to feature in their articles. In addition, research such as Huston and Kamdar (1996) argues that individuals perceive 99-cent prices to be disproportionately lower than whole dollar prices just one cent above. In our setting, this phenomenon could lead journalists to view firms with revenues just above a base-ten threshold as disproportionately larger than firms with revenues just below. Given the prior research documenting a link between firm size and visibility (Miller, 2006), we predict that disproportionate increases in the perceived size of a firm lead to disproportionate increases in firm visibility. Similarly, Odean (1998) proposes that processing constraints lead investors to limit their choice of stocks to those that have caught their attention, and Barber and Odean (2008) find that individual investors are more likely to purchase stocks that have caught their attention by

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6 Our setting is different from targets at zero earnings and last year’s earnings because base-ten thresholds have no economic meaning, whereas last year’s and zero earnings have implications for future performance by providing information on earnings sustainability and whether a firm has met its breakeven point, respectively.
being featured in the financial press. As a result, reaching a base-ten threshold may increase a firm’s stock liquidity if these revenue numbers stand out more to investors.

Furthermore, we predict that base-ten thresholds can also increase visibility with more sophisticated market participants. For example, analysts may be subject to the same cognitive biases as other individuals, or they may respond to greater demand for firm information driven by investors and journalists. Additionally, anecdotal evidence shows that mutual funds, exchanges, and investors use bright-line cutoffs to determine whether they will consider following or investing in a company.7 To the extent that heuristics lead these cutoffs to be set at base-ten thresholds, we expect passing base-ten thresholds to lead to disproportionate increases in institutional ownership.

In sum, we predict that firms which beat a particular base-ten revenue threshold (e.g., $50 million or $200 million) for the first time will experience increases in visibility in the form of increased press coverage, stock liquidity, analyst following, and institutional ownership.8 Because increased public visibility can lead to other benefits such as a greater ability to detect fraud (Miller, 2006), higher price efficiency (Fang and Peress, 2009), and increased stock liquidity (Bushee et al., 2010) which have implications for firm value (Diamond and Verrecchia, 1991), expected increases in firm visibility provide a strong incentive for managers to beat base-ten thresholds.9

7 For example, the minimum market capitalization requirements are $6.1 billion for the S&P 500, $30 million for the Russell 2000 index, and $40 million for the New York Stock Exchange. Additionally, we had several discussions with fund managers related to the use and frequency of base-ten thresholds in fund investment decisions. These managers stated that numerical cut-offs related to sales, assets, and earnings were commonly employed in order to “shortlist” potential investments. All numerical cut-offs were set to be just at or above rather than below a base-ten threshold.
8 We only expect increases in visibility the first time that a firm’s revenues exceed a particular base-ten threshold (e.g., $50 million). Intuitively, we would not expect the attention-grabbing nature of a firm’s level of revenues to continue increasing visibility if the firm is repeatedly beating the same base-ten threshold. Similarly, once a company has experienced a discontinuous increase in its perceived size by beating a base-ten revenue threshold (say, $50 million), beating that same revenue threshold again will not change the market’s perception of the firm’s size. Only when the firm beats another larger base-ten threshold (for example $60 million) will its perceived size experience another discontinuous increase. Consistent with this reasoning, our visibility tests focus on periods when firms beat a new base-ten revenue threshold for the first time.
9 Prior studies also document the value of analyst following. For example, Kirk (2011) provides evidence that firms are willing to pay for artificial coverage.
Formally, our hypothesis related to the incentives to attain base-ten thresholds in reported revenue is as follows:

**H3:** *Firms that exceed a base-ten revenue threshold for the first time will experience increases in firm visibility as measured by press coverage, liquidity, analyst following, and institutional ownership.*

We formally test the above hypotheses and report the results in Section 4.

**III. Research Design and Sample**

As discussed previously, we conduct our base-ten threshold analyses using a sample of revenue thresholds because this enables us to provide new insights on the role of revenue as a market focal point and firm target. However, there are also methodological advantages to focusing on reported revenue. Revenue does not suffer from the scaling problems to which net measures such as earnings are subject and which cause difficulty in statistically identifying threshold-beating behavior (Burgstahler and Chuk, 2015; Durtschi and Easton, 2005; 2009). Additionally, previous work has demonstrated that focusing on a single accounting component provides greater power to detect manipulation than by examining earnings as a whole (Stubben, 2010).

In order to document the presence, drivers, and outcomes of base-ten revenue thresholds, we use a variety of samples and analyses, including regression and approximate randomization tests. Below we provide a general description of the data and tests used in this paper. Further detail is given in the discussion of each analysis.

**i. Approximate Randomization**

We use approximate randomization techniques to document statistically significant discontinuities around base-ten thresholds in reported revenues and revenue targets. These techniques rely on the properties of randomly-generated distributions (Noreen, 1989) to detect
significant deviations from the null hypothesis of no threshold effect. Other papers in the accounting literature, including Dichev and Skinner (2002), have used similar methods.

In order to determine whether there is an unexpectedly large number of firms with total revenue just above base-ten thresholds, we calculate the ratio of the number of observations in the interval (or “bin”) just above base-ten thresholds to the number of observations in the bin just below, and we then compare this above/below ratio to the corresponding ratios for a set of randomly-generated pseudo-thresholds. An above/below ratio around base-ten thresholds that is significantly larger than the ratios around other points along the number line is consistent with an abnormally high number of firms with reported revenue to be just above these thresholds. We use a similar procedure to compare the magnitude of the base-ten threshold effect across different subsamples and financial metrics by comparing the ratio of observations above and below base-ten thresholds between the two samples of interest. For further details see the Appendix.

We identify 53 potential thresholds that we expect to be salient to managers, investors, and other market participants. The potential thresholds range from $100,000 to $80 billion.\textsuperscript{10} As shown in Table 1 Panel A, each order of magnitude (10\textsuperscript{5} through 10\textsuperscript{9}) includes nine thresholds such as $100,000, $200,000, $300,000, up through $900,000; the final order of magnitude, 10\textsuperscript{10}, includes 8 potential thresholds up to $80 billion. We define the width of each “bin” around potential thresholds in percentage terms by calculating the percentage increase in revenues necessary to move from the bottom to the top of the bin. Our smallest bin width is 0.25% and our largest bin width is 20%.

In their work on base-ten thresholds in net income, Carslaw (1988) and Thomas (1989) use a methodology of looking at deviations of second digits from their expected frequencies based on

\textsuperscript{10} We do not examine thresholds outside this range because only a few firms covered by Compustat report revenue values larger or smaller than this.
Benford’s Law. The benefit of our approximate randomization technique, which controls for the empirical distribution of reported numbers, is that we are not required to make any assumptions about the statistical properties of the reported revenue distribution, assuring that our results are not a result of Benford’s Law. Also, our technique allows us to examine the magnitude of the flexibility firms have to reach a revenue threshold because we can precisely vary the width of the interval (bin) above and below the thresholds that we examine. The second-digits methodology employed by Carslaw and Thomas is constrained to examining non-adjustable bin widths of varying relative sizes. Finally, and importantly, our approach allows us to statistically compare the threshold effect across two sets of firms or reported numbers. In robustness tests, we use this feature of our approach to rule out alternative explanations for our findings.

**ii. Revenue Threshold Sample**

We focus on U.S. firms and use all Compustat firm-years over the period 1950 through 2017 for which both total revenue and total assets are non-missing and positive; this yields 367,233 firm-years which we use in our basic approximate randomization tests. Our approximate randomization tests focus on revenue observations just below and just above base-ten thresholds, but we use all of the available data to generate the approximate randomization test statistics based on the empirical distribution of revenue observations just below and just above randomly-chosen revenue thresholds. We provide the 53 base-ten thresholds we consider in each of our tests in Table 1 Panel A and the number of firm-years by decade in Table 1 Panel B.

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11 Benford’s Law is an empirical regularity whereby certain digits are more likely to occur in the leading positions of numbers in naturally-generated data (Newcomb, 1881).
12 For example, the relative size of any revenue management activity to move a firm from $990,000 in revenue to $1,000,000 in revenue is much smaller (1.01%) than to move a firm from $1,900,000 in revenue to $2,000,000 in revenue (5.26%), and yet these cases are treated the same with the second-digits methodology.
13 Although our tests include data from as early as the 1950s, this base-ten revenue threshold phenomenon persists today and can be observed in the data even when restricting the sample to include only those firm-years occurring after the year 2000. We use the widest sample possible to maximize power, particularly in our subsample tests.
14 In any given test using a subsample of these data, we truncate the bottom and top 1% of revenue observations.
iii. **Formal and Informal Contract Datasets**

In order to test for the presence of a base-ten threshold effect in formal and informal contracts, we use executive compensation plan revenue targets from Incentive Lab and management and analyst revenue forecasts from IBES. See the relevant tables for additional information.

iv. **Regression Analyses Sample**

For the regression analyses in the paper, we use a sample of firm-years with accounting data from Compustat, market data from CRSP, analyst forecast data from I/B/E/S, 13F institutional ownership data from Thomson Reuters, institutional investor classifications from Brian Bushee\(^\text{15}\), and press coverage data from RavenPack over the fiscal years 1999 to 2014.\(^\text{16}\) All variable definitions are given in the Variable Appendix. Table 1 Panel C presents descriptive statistics for the variables used in the regression analyses in Tables 6 through 8. Variables are winsorized at the 1% level.

[Insert Table 1 Here]

IV. **Empirical Results**

i. **Importance of Revenue Targets**

In Table 2, we document the prevalence of revenue in three performance targets – executive compensation plans, management forecasts, and analyst forecasts. Panel A reports the five most common accounting metrics on which executive compensation plans are contracted. During our sample, 29% of firm-years had at least one compensation contract with a revenue target. Revenue was present in executive compensation contracts second only to EPS (30.1%).

\(^{15}\) [http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html](http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html).

\(^{16}\) Because our press coverage variables are measured over the subsequent year, our regression sample must end in 2014 because our last year of RavenPack data is 2015.
Panel B reports the five most commonly forecasted accounting numbers in management forecasts. During our sample period, 52.0% of firm-years had at least one management revenue forecast. Again, this proportion was second only to EPS (53.3%). Finally, Panel C reports the five most commonly-forecasted accounting numbers present in analyst forecasts. In our sample, 86.4% of firm-years had at least one analyst revenue forecast and 88.7% had at least one EPS forecast.\textsuperscript{17} Figures 1a, 1b, and 1c display the frequency of revenue and EPS as performance metrics over time. Interestingly, we observe that revenue has become more prevalent in all three samples over time, and it appears more often than EPS in recent years for all three samples.

The descriptive statistics presented in Table 2 and Figures 1a, 1b, and 1c demonstrate the great prevalence of revenue as a benchmark and speak to its importance in performance evaluation. This suggests that revenue is a powerful setting to examine base-ten thresholds.

\textit{ii. Base-Ten Threshold Effect}

Figure 2 presents a histogram of the number of observations contained in the 50 non-overlapping bins immediately above and below each base-ten threshold, summed across all 53 thresholds, using bin widths of 0.25%. There is a noticeable discontinuity in the distribution around the base-ten threshold point, providing preliminary evidence consistent with H1.

In Table 3, we formally test our first hypothesis and provide evidence that firms are significantly more likely to report revenue numbers just above than just below base-ten thresholds. In Panel A we report values of the ratio of the number of revenue observations in the bin just above base-ten threshold points to the number of observations in the bin just below base-ten threshold points for bin widths ranging from 0.25% to 20%. The number of ABOVE observations exceeds the number of BELOW observations for all bin widths, and the approximate randomization p-

\textsuperscript{17} Inferences are unchanged when we restrict the sample periods to be consistent across the performance metrics.
values are statistically significant at the 1% level for bin widths from 0.25% of revenue through 10% of revenue. These results have an extremely low probability of occurring by random chance and support our prediction that managers manage reported revenue to reach these base-ten benchmarks.¹⁸,¹⁹

[Insert Table 3 here]

One issue to consider with respect to the test statistics in Panel A is that the observation counts in the ABOVE and BELOW bins for the different bin widths are cumulative, meaning that if an observation is in the ABOVE bin for the 0.25% bin width, that same observation is also in the ABOVE bin for the 20% bin width. Part, or all, of the significant difference in the ABOVE/BELOW number of observations for the larger bin widths could therefore be the result of the differences for the smaller bin widths. This is analogous to a significant five-day abnormal market return being the result of four days of zero abnormal returns and one day with a large abnormal return. Intuitively, it seems unrealistic that firms would be able to increase reported revenue by 10% or 15% (two of the significant bin widths) in order to reach a base-ten threshold.

To explore this issue, in Table 3 Panel B we tabulate observations in incremental (or non-nested) revenue bands. For example, we observe in Panel A that there are 8,220 observations in the ABOVE bins within 0.5% of a base-ten threshold. However, only 3,606 of these observations

¹⁸ Prior studies examining base-ten thresholds for earnings identified these irregularities by examining the distribution of the digits in reported numbers. A digits-based methodology must take into account Benford’s Law, which demonstrates that digits in naturally-occurring numbers are not uniformly distributed and numbers with a second digit of 0 (e.g., 500) are more likely to occur than numbers with a second digit of 9 (e.g., 69,000). However, Benford’s Law does not explain our evidence of discontinuities around base-ten thresholds because Benford’s Law explains the distribution of digits in naturally occurring data and not the distribution of observations at adjacent points on the number line (i.e., Benford’s Law does not imply any sort of discontinuity in the distribution of numbers) (Newcomb, 1881; Benford, 1938). Similarly, Amiram et al. (2015) uses deviations in the distributions of digits from Benford’s Law to identify earnings management, but our evidence of discontinuities cannot be inferred from their results.

¹⁹ To mitigate the concern that our results are driven by observations with very low revenues, in an untabulated test we restricted our sample to firms with revenues greater than $100 million and still find a significant effect, ruling out the possibility that our results are driven by the smallest thresholds such as $1 million.
are in the band close to the upper limit of 0.5% in the band between 0.25% and 0.5%. We use these incremental observation counts (i.e., observations in the current bin width that were not included in the next smallest width) to generate new ratios and associated approximate randomization p-values. The incremental p-values of three of the four smallest bins are significant at the 1% level. These incremental results suggest that firms are desirous and able to manage reported revenue within approximately a 2.5% range to reach base-ten threshold points. The statistical significance for wider bins results from the holdover effect from the ABOVE/BELOW difference for the narrower bins. As a result, additional approximate randomization tests reported in Tables 4 and 5 and in the Appendix use only the 4 smallest bin widths. We point out that the results in Panel B are interesting in their own right because they allow us to quantify the degree of flexibility managers have to manage reported revenue and, by extension, reported earnings. This information can allow future researchers to more accurately calibrate tests of revenue or earnings management.

To further validate these results, we conduct a battery of additional analyses described in the Appendix. To rule out the possibility that our results are driven by rounding of accounting

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20 Note that the incremental p-value in Panel B for 0.25% is the same as in Panel A because it is the smallest bin width and, therefore, incremental to a bin width of 0. Also, there are some minor discrepancies between the cumulative number of observations in Panels A and B because we truncate the distribution of bins at the 1% level for each analysis. 21 Although we report results using percentage bin widths, we recognize that there are a variety of ways to define and choose bin widths, each with its own set of advantages and disadvantages. While one benefit of bin widths defined in percentage terms is that they increase in size with the magnitude of revenues to take into account greater dollar amounts of flexibility, this results in overlapping bins for some adjacent thresholds when the bin width is large (for example, the 20% bin above $500 million completely overlaps with the 20% bin below $600 million). This can be seen in the last two rows of Table 2 Panel A where the sum of observations in the top and bottom bins exceeds the total number of observations in our sample because some observations fall in the top bin for one threshold and the bottom bin for another threshold. However, this does not bias our p-values because it also occurs in the random "pseudo" thresholds used in the randomization procedure. Further, it only occurs when bin widths are wide (7.5% and greater) where firms are unlikely to feasibly be managing their revenues in the overlapping regions. However, to allay any concerns about inferences drawn from our percentage bin width tests, we ran our approximate randomization tests using absolute dollar bin widths (i.e., $10,000, $50,000) and flexible bin widths defined as the minimum percentage bin width around each potential threshold required to capture the nearest 30, 100, and 200 observations. All methods of defining bin widths strongly indicate the presence of a base-ten threshold effect in revenue. We use the percentage bin width method because we think it is the most intuitive and theoretically sound.
numbers introduced by the data-provider or by managers subconsciously rounding up all reported accounting numbers, we use approximate randomization to compare the magnitude of the base-ten threshold effect of revenue with that of cost of goods sold, total assets, and market value of equity (Table A.1). These other metrics are of a similar magnitude as revenue and serve as reasonable benchmarks. We demonstrate that the base-ten effect in revenue is significantly stronger (p-value < 0.01 in all cases) than in any of these other metrics, further supporting our prediction that managers deliberately manage revenue to exceed base-ten thresholds.\textsuperscript{22} We also document that this effect exists for non-US firms reporting in local currency (Table A.2). See the Appendix for more detailed descriptions of these and other tests.

In summary, Table 3 and the Appendix provide support for our prediction that firms are more likely to report revenue that is just above a base-ten threshold than just below, and this is not an artifact of the data-gathering process or rounding. These results are similar to those of Carslaw (1988) and Thomas (1989) with respect to revenue numbers instead of earnings numbers, and provide the motivation for subsequent tests.

\textit{ii. Base-Ten Threshold Effect in Formal and Informal Contracts}

In Table 4 we provide the results of additional analyses which explore another potential explanation for the base-ten revenue threshold phenomenon, consistent with H2a. In particular, we examine additional incentives that may induce managers to report revenues just above a base-ten

\textsuperscript{22} These results also serve the additional purpose of further justifying our decision to examine revenues as the focus of our study of base-ten thresholds, because we predict and find that this phenomenon is particularly important in this setting compared to several other common metrics. Further, in preliminary untabulated tests we also compared the magnitude of the base-ten threshold effect in revenues with that of earnings. Net income is not an ideal measure to examine in the context of base-ten thresholds because of interpretation issues around zero earnings (e.g., Hemmer and Labro, 2019) and because scaling issues add an additional layer of complexity and confounding factors (Durtschi and Easton 2005, 2009), but we still think this comparison is of interest. We found insignificant or mixed evidence on the difference of the magnitude of the base-ten threshold effect in revenues and earnings, which suggests that the strength of the phenomenon appears to be roughly similar. However, in order to account for scaling issues in earnings, we redefined bin widths using the number of observations around thresholds instead of fixed bin widths. To the extent that this did not overcome comparability issues between the two distributions, these results are only preliminary and suggestive.
threshold by documenting that revenue targets set in formal and informal contracts (e.g., executive compensation plans, management revenue forecasts, and analyst revenue forecasts) also have a base-ten threshold effect. We think this additional motivation for base-ten revenue threshold beating is interesting and sheds light on the process by which these targets are set.

First, we use executive compensation plan revenue targets from Incentive Lab. During our sample, 29% of firm-years had at least one compensation contract with a revenue target. Revenue was present in executive compensation contracts second only to EPS (30.1%). In Panel A we investigate executive compensation contracts and document that revenue targets in these contracts are significantly more likely to be set at or just above a base-ten threshold than just below. This effect is most pronounced within the smallest bin (0.25%) around a base-ten threshold where contracts are 10 times more likely to have a target at or just above a threshold than just below the threshold. Even in wider bins (0.50% and 1.00%), contracts are 4.5 and 2.5 times more likely to have a revenue target at or just above a threshold than just below. In other words, the numeric revenue targets themselves which are specified in executive compensation plans exhibit a discontinuity around base-ten thresholds. A graphical depiction of this result is shown in Figure 3a, which shows a large spike in compensation plan revenue targets just above base-ten thresholds.

We examine the threshold effect in management and analyst revenue forecasts using data from IBES in Panels B and C of Table 4. Once issued by managers and analysts, these informal benchmarks become relevant performance targets. Similar to the results in Panel A, managers are more likely to forecast revenues at or just above a threshold than just below a threshold. This effect is strongest in the smallest bin around a threshold, with management forecasts in the 0.25% bin-width being 27 times more likely to be at or above a threshold than below, and even in a bin-width of 2.50%, managers are still more than twice as likely to forecast revenues at or just above a
threshold than just below. In Panel C, we find that the consensus analyst revenue forecast is also significantly more likely to fall at or above a base-ten threshold, with the consensus forecast almost twice as likely to fall in the 0.25% above a base-ten threshold as below. Figures 3b and 3c show the distribution of management and analyst revenue forecasts around base-ten thresholds and provide striking visual confirmation of these statistical tests.

Although Table 4 documents the presence of a base-ten threshold effect in the revenue targets specified in formal and informal contracts, simply demonstrating the presence of this effect in these targets does not necessarily indicate that they drive any of the observed threshold effect in actual revenue. In untabulated tests, we partition our sample by whether firms either did or did not beat all of their explicit revenue targets. “Beaters” are firms which either met or beat all of the revenue targets defined by their executive compensation plans, management revenue forecasts, analyst revenue forecasts, and last year’s revenue. Although both revenue target beaters and non-beaters are more likely to report revenue just above rather than just below base-ten thresholds, the effect is significantly stronger for beaters, consistent with beating a base-ten revenue threshold being a byproduct of beating other revenue targets. Taken together, these results reveal a preference for setting both formal and informal contracts at or just above base-ten thresholds.\(^\text{23}\)

We provide additional evidence for explicit targets in section 5 of the Appendix.

\textit{iii. Threshold Effects in the Absence of Explicit Targets}

\(^{23}\) It is possible that analyst revenue forecasts exhibit a base-ten threshold effect because analysts rationally expect that managers will try to beat base-ten thresholds. However, in untabulated analyses we find that the magnitude of the threshold effect in analyst revenue forecasts is significantly stronger for the first forecast an analyst makes for a given period relative to the last forecast, consistent with analysts relying more on base-ten heuristics earlier in the period when they have less information and less later in the period when their information is more precise (Herrmann and Thomas, 2005). This systematic time-series variation in the base-ten threshold effect is inconsistent with it being solely driven by analysts’ attempt to forecast managers’ base-ten beating behavior.
In Table 5 we formally test H2b and investigate whether the revenue threshold effect exists for firms which have no explicit revenue targets available.\textsuperscript{24} We find a significant base-ten threshold effect in revenue for the two smallest bin widths for firms in this sub-sample, indicating that firms still exert effort to report revenues above base-ten thresholds even when doing so will not help them beat the explicit targets commonly studied in the prior literature.\textsuperscript{25} We provide a graphical depiction of this result in Figure 4, which shows a clear spike in reported revenue observations just above base-ten thresholds. This as an interesting result because it suggests that the set of performance targets commonly studied in the prior literature does not capture the full set of targets that firms respond to. Firms may be responding to other explicit targets not previously studied, or they may respond to implicit targets, such as the base-ten threshold examined in this study, or other implicit targets, for example, the maximum revenue ever reached by other firms in the same industry.

\textit{iv. Firm Visibility: Media Coverage, Liquidity, Analyst Following, Institutional Ownership}

As predicted in H3, one potential benefit of reaching a base-ten revenue threshold is that doing so attracts attention and leads to increases in firm visibility. To test this hypothesis, we investigate the effects of reaching a base-ten threshold for the first time on media coverage, liquidity, analyst following, and institutional ownership and present the results in Table 6.

We study the effects of beating a specific base-ten threshold (say $300 million) for the first time, and not the effects of beating the same base-ten threshold in subsequent periods, because we do not expect firms to experience increases in visibility when beating the same threshold for a

\textsuperscript{24} In order to alleviate concerns that firms are missing explicit revenue targets because of a lack of data coverage, we restrict the sample period in this test from 2004 to 2016 when we have substantial coverage for all three data sources.  
\textsuperscript{25} One concern with Table 4 is that the firms we examine may have explicit targets not captured in our data. To allay this concern, we examine the set of firms which have explicit revenue targets but fail to beat all of them. These firms have a significant base-ten revenue threshold effect; in other words, firms have incentives to beat base-ten revenue thresholds even when doing so does not help them beat their observable explicit targets.
second time. For example, when a firm reaches $300 million in sales for the first time, it may attract the attention of new analysts or investors who previously would not have followed or traded in the firm. Once these market participants have made the decision to follow a firm, they will likely continue to do so now that the firm has passed their size “threshold.” Other market participants who chose not to follow the firm after it surpassed $300 million for the first time are unlikely to change this decision if the firm continues to report revenues just above $300, but they may reevaluate the firm if it passes the next base-ten threshold of $400 million, as this may be perceived as a signal that the firm is now large enough to merit attention. In other words, we believe market participants update their assessments each time a firm reaches a new base-ten threshold, but observing a firm beating the same threshold repeatedly is not perceived as an information event.

In order to ensure that the results in our visibility analyses are not driven by whether firms beat explicit targets which have been well studied in prior papers, each of our tests controls for whether firms have beaten a variety of earnings and revenue targets. Specifically, we control for whether firms beat last year’s level of revenue or earnings (Beat_Last_Year’s_Rev, Beat_Last_Year’s_Inc), whether they beat analysts’ revenue or earnings forecasts (Beat_Analysts’_Rev, Beat_Analysts’_Inc), and whether they report a loss (Loss). Thus, the visibility benefits of base-ten revenue thresholds that we document occur even after controlling for whether a firm beats a variety of other common targets. In addition, we control for revenue growth (Revenue Growth) and the magnitude of revenue (ln(Revenue)). Lastly, any time a firm reaches a previously unattained level of revenue it may attract additional attention (for example by being able to tout “record” sales). In order to ensure that visibility outcomes are not driven by “record” sales in general, and not base-ten thresholds specifically, we include an indicator variable.
for whether current revenues are greater than those in any prior period (Record_Sales). We also control for a variety of other firm fundamentals (listed in the description of each table).

In Panel A of Table 6 we present results for our analysis of press coverage for firms crossing a base-ten threshold for the first time. This table shows that threshold-beaters see an increase in news coverage over the two years following a base-ten beating earnings announcement. Specifically, the coefficient on Threshold is positive and significant in Columns 1 through 4 which include industry and year (Columns 1 and 3) and firm and year fixed effects (Columns 2 and 4). The magnitude of this increase appears to be material, with firms experiencing an increase in press coverage of roughly 5 articles compared to an average (median) total of 170 (115).\textsuperscript{26} Given the potential economic benefits of increased press coverage (Bushee et al., 2010; Drake et al., 2014; Fang and Peress, 2009), we interpret these results as evidence of an incentive for firms to beat base-ten thresholds to the extent that firm managers anticipate these increases in press coverage.

We next examine how institutional ownership changes in the two years after a firm crosses a base-ten threshold for the first time. Institutional investors may experience a base-ten bias or recognize the benefits experienced by firms crossing a base-ten threshold (increased press coverage, investor attention, analyst following) and decide to increase their holdings in these firms. Further, in discussions with fund managers, we have been told that they often use numerical size cutoffs set at or above base-ten thresholds in order to generate the shortlist of potential investments. We examine changes in total institutional ownership as well as changes in three sub-categories of institutional ownership – quasi-indexers, transient investors, and dedicated investors – (as defined in Bushee (2001)) in the two years after a firm first reaches a base-ten threshold.\textsuperscript{27} The result of

\textsuperscript{26} In untabulated tests, we also find that firms increase coverage of their own performance through firm-initiated press releases, potentially as a way of drawing further attention to their base-ten threshold beating revenue.

\textsuperscript{27} Transient institutions have high portfolio turnover and highly diversified portfolio holdings. These characteristics reflect the short-term focus of transient institutions who are interested in short-term trading profits. Quasi-indexers
these tests are presented in Panel B. Columns 1 and 2 present results for changes in total institutional ownership in the two years after a threshold is reached. The coefficient on \textit{Threshold} is positive and significant in Column 1 but not Column 2, indicating that threshold beating firms have significantly higher institutional ownership in the first year after they reach a threshold. These results suggest that institutions respond to base-ten threshold beating with increased investment.

We further examine what types of institutional investors drive this observed effect. We examine quasi-indexer institutional investors ( Columns 3 and 4), transient institutional investors ( Columns 5 and 6), and dedicated institutional investors ( Columns 7 and 8). The coefficient on \textit{Threshold} is positive and significant in Columns 3 and 5, indicating that quasi-indexer and transient institutional investors increase their ownership of first-time threshold beating firms in the year after a threshold is reached. However, we do not observe any effect on the holdings of dedicated investors. These results suggest that the types of institutional investors who are likely to respond to a firm reaching a base-ten threshold (transient and quasi-indexers who often base trades on trading strategies or index membership) quickly rebalance their portfolios to include these now more visible stocks. Furthermore, dedicated institutional investors, who are long horizon investors that do little portfolio rebalancing and have low turnover, do not respond to this attention-grabbing event, which is consistent with their investment strategy. Finally, these results are consistent with transient institutional investors being short-term and stock price focused, and they are consistent with quasi-indexer institutional investors having specific requirements to be included in a fund.

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\(^{28}\) For brevity, we only tabulate the specifications which include firm and year fixed effects. However, our inferences are unchanged when we instead include industry and year fixed effects.

\(^{8}\) For brevity, we only tabulate the specifications which include firm and year fixed effects. However, our inferences are unchanged when we instead include industry and year fixed effects.

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28 For brevity, we only tabulate the specifications which include firm and year fixed effects. However, our inferences are unchanged when we instead include industry and year fixed effects.
In addition to the increase in press coverage and institutional ownership we document in Panels A and B, we predict that firms which cross a base-ten threshold for the first time may also experience increased attention from investors who are drawn in by the additional visibility of the firm (Barber and Odean, 2008) or their own inherent base-ten threshold preferences. We examine the trading liquidity of firms in the two years subsequent to crossing a base-ten threshold and present the results in Panel C. In Columns 1 and 2 we examine changes in share turnover in the first year (Column 1) and second year (Column 2) following the first instance of beating a threshold and find that firms experience increases in share turnover in the year following a threshold, with a smaller (and only marginally significant effect) after two years. We next examine changes in bid-ask spread in the first year (Column 3) and second year (Column 4) following the first instance of beating a threshold. Similar to the turnover measure, we find that firms experience decreases in their median bid-ask spread (increases in liquidity) in the year following this event but that this decrease does not extend to the second year after the event. Overall, these results provide strong evidence that firms experience an increase in liquidity in the year after they beat a base-ten threshold for the first time. These results are especially important because higher liquidity is linked to greater firm value through the cost of capital (Diamond and Verrecchia, 1991). This further highlights the potential benefits and incentives that managers face when reporting revenues just above a base-ten threshold.

H3 also predicts that reaching a base-ten revenue threshold will lead to an increase in analyst following. We argue that reaching a threshold will attract the attention of analysts (or analysts’ intended audience) who will then decide to follow the firm. We present results for our analysis of analyst following for firms crossing a threshold for the first time in Panel D. In Columns 1 and 2 we examine the change in analyst following in the year after first crossing a base-ten
threshold with industry and year and firm and year fixed effects, respectively. We find that firms that cross a base-ten threshold for the first time experience an increase in analyst following over the subsequent year when controlling for industry and year but not when including firm and year fixed effects. However, in Columns 3 and 4 we extend this examination to the second year following the reaching of a threshold and find that firms experience an increase in analyst following under both specifications. These results suggest that crossing a base-ten threshold for the first time captures the attention of analysts (an important information intermediary in capital markets), leading to an increase in subsequent analyst following.

Overall, the results of Table 6 provide support for H3 and indicate that firms that beat a base-ten threshold for the first time experience significant increases in visibility in the form of increased media coverage, institutional ownership, liquidity, and analyst following. These results also provide a straight-forward rationale for beating base-ten thresholds even after controlling for whether firms have beaten their explicit revenue and earnings targets, and suggest that the expected increases in visibility and firm recognition that base-ten thresholds provide are a powerful incentive to managers.29,30,31

VII. Additional Analysis

i. Predictable Cross-Sectional Variation in the Base-ten Revenue Threshold Effect

29 While we focus on visibility outcomes in this study, we do not claim that this is an exhaustive set of the potential benefits of beating base-ten revenue thresholds. For example, beating a base-ten threshold may also increase visibility with potential customers or increase the morale of base-ten biased employees.
30 All results in Table 6 are robust to including continuous measures of M&A activity rather than simply including an indicator variable equal to one if the firm engaged in M&A during the year.
31 Both press coverage and analyst following are count data. As such, count data econometric models may be more appropriate. We re-estimate the models in Panels A and C of Table 6 using both negative binomial and Poisson regressions. Under both alternative specifications our inferences in Panel A remain unchanged. The negative binomial model does not converge with firm FE in Panel C (analyst coverage). We manually stop the estimation process after 25 iterations. The Poisson models run as expected and our inferences remain unchanged.
If the base-ten effect in revenue is the result of intentional actions by firms, as opposed to the data-generating process or innocuous rounding, then we would expect that the base-ten threshold effect would be stronger for firms which have high incentives to beat revenue thresholds. In particular, we expect that firms with high market expectations of growth will be particularly sensitive to revenue news and will exhibit a stronger base-ten revenue threshold effect. In Table 7, we report results examining the magnitude of the base-ten threshold effect for firms with high versus low lagged revenue growth and price-to-sales ratios, respectively.  

Table 7 Panel A reports the results of approximate randomization tests comparing the magnitude of the base-ten revenue threshold effect of high past revenue growth firm-years (the highest revenue growth decile) with low past revenue growth (the lowest revenue growth decile) firms. These results indicate that past revenue growth appears to have a significant effect on the tendency of a firm to reach a base-ten threshold. For three of the four bin widths, the ratio of firms with revenues just above relative to just below a base-ten threshold is significantly greater for high-growth firms relative to low growth firms. Similarly, in Panel B we find that the base-ten threshold effect in revenues is stronger for high (top decile) relative to low (bottom decile) price-to-sales ratio firms for all four bin widths. These results are consistent with the idea that managers in firms where revenues are subject to more attention are more motivated to seek the positive attention stemming from reaching base-ten reference points. Interestingly, in untabulated results we find no significant difference in the magnitude of the base-ten revenue threshold effect between groups of

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32 We define revenue growth as the percentage change in revenue from year t-1 to t (where t is the current year) and the price-to-sales ratio as a firm’s total market capitalization divided by total revenue. Because revenue growth is correlated with current revenue, we use lagged values to sort firms into revenue growth deciles. Before constructing deciles, we exclude the extreme high/low 2% of past revenue growth (price-to-sales ratio) firm-years for Panel A (B) to avoid our results being skewed by extreme growth firms.
firms with high and low price-to-earnings ratios, suggesting that incentives tied to revenue performance are not necessarily the same as those tied to earnings performance.

**ii. Discretionary Revenues**

We examine the nature and quality of revenue used to reach base-ten thresholds for the first time. Specifically, we use the Stubben (2010) measure of discretionary revenues (with some modifications proposed by McNichols and Stubben, 2018, and Collins et al., 2017) which identifies discretionary revenues by identifying deviations from the normal relation between revenues and collections. In Table 8 we present results of our discretionary revenue regression analyses. In Columns 1 and 2, we see some evidence of greater positive discretionary revenues for the average firm that crosses a base-ten threshold for the first time, but this result does not hold with firm fixed effects. However, when we partition the sample into groups of firms that have especially high incentives to beat revenue thresholds we see a positive and significant effect. Specifically, in Columns 3 through 6 the coefficient on Threshold x Split is positive and significant. The coefficients on this interaction term indicate that in the year firms first meet or beat a particular base-ten threshold, firms which have high past growth (and therefore a greater expectation of high future growth), firms which are small (and have a greater need for visibility), and firms which are operating at a loss (and have a greater need to counteract negative earnings news) have a larger amount of positive discretionary revenue. Overall, these results suggest that some managers engage in more revenue management activities in the years in which they reach a base-ten revenue threshold, consistent with managers actively using the tools at their disposal to reach these targets.
VIII. Summary and Conclusions

In this paper we show that a preference for base-ten numbers, which have no inherent economic meaning, has economic implications for firms. This preference has a measurable effect on the actions of managers, investors, financial analysts, the business press, and, by implication, the broader set of market participants who respond to financial information. Overall, these results open the door to a new range of managerial targets previously unexplored.

While prior research such as Carslaw (1988) and Thomas (1989) provided evidence of an unusually high number of earnings numbers just above base-ten revenues, we focus on revenues, which are more often tied to goal-setting and growth targets, in order to explore potential explanations for this preference, something which prior research has not yet addressed. Part of the unusual patterns in revenues that we document appears to also reflect firms’ response to a preference for base-ten thresholds in their revenue targets set in executive compensation plans, management forecasts, and analyst forecasts. However, firms disproportionately report revenues just above base-ten thresholds even in the absence of these targets, indicating that the visibility results that we document appear to be a powerful driver of firm actions. This target-beating behavior is different from that documented in prior studies because it occurs in the absence of explicit targets. Furthermore, we document that firms which beat a base-ten threshold for the first time experience subsequent increases in visibility in the form of increased press coverage, institutional ownership, liquidity, and analyst following.

These results are consistent with a behavioral bias on the part of market participants which favors base-ten thresholds as cognitive benchmarks when comparing numbers and which leads them to be perceived as discontinuously larger than numbers just below. An implication of our findings is that managers who anticipate this behavioral preference for base-ten thresholds on the
part of market participants may rationally fixate on and exert effort to reach these thresholds. We also document that first-time threshold beaters experience negative future abnormal returns on average, consistent with investors initially overestimating the benefits of reaching a threshold. Together, these results are the first to date which document economic consequences of base-ten threshold beating and provide an explanation for why managers have a preference for these targets.

Our paper is the first to document specific economic drivers of the base-ten threshold phenomenon. We believe this phenomenon is especially important with respect to revenues given its monotonically increasing nature and because revenues are an important performance target. For example, in the last years of our sample, a higher proportion of firms had revenue targets than had either raw earnings or EPS targets. Additionally, base-ten thresholds are an ideal setting to examine target-beating behavior because they lack inherent economic meaning (Bissessur and Veenman, 2016). Thus, the discontinuities that we document are unlikely to be driven by economic fundamentals that can lead to discontinuities in earnings (Bonham, 2018; Hemmer and Labro, 2019; Li, 2014). As a result, our study provides a key contribution to the literature examining earnings discontinuities (Burgstahler and Dichev, 1997; Durtschi and Easton, 2005, 2009; Burgstahler and Chuk, 2015).

Our study provides important evidence on pervasive goal-setting behavior around base-ten thresholds. Our results tie into research demonstrating the importance of goal-setting for motivation more generally (Locke and Latham, 2002), as well as research examining the importance of salient thresholds specifically (Allen et al., 2016). We are, however, the first to explore economic consequences of this phenomenon in the context of corporate reporting.

Considering the large number of base-ten thresholds present in revenue numbers (53 in this study) and the potential for similar incentives to arise in unaudited revenue numbers used in
internal reports or for privately held businesses, we believe the existence of this base-ten phenomenon has far-reaching implications. For example, given the prevalence of the base-ten threshold effect we document in reported revenue, it is likely that market participants are subject to this base-ten bias in other settings. Additionally, this study does not attempt to identify the extent to which these threshold effects are driven by real activities versus accruals management. We leave this exploration, and its implications for revenue growth, to future research.
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### VARIABLE APPENDIX
(For Regression Analyses)

| **Main Independent Variable of Interest** | **Description** |
|------------------------------------------|-----------------|
| \( \text{Threshold}_t \)                | An indicator variable coded 1 if the firm has revenue in the current period that has exceeded a particular base-ten threshold of the form \( T = N \times 10^K \) (e.g., \$50 million) for the first time, and 0 otherwise, where the full set of potential values of \( T \) is given in Table 1 Panel A. That is, \( \text{Threshold}_t=1 \) if there exists a \( T \) (a base-ten threshold) where \( \text{Revenue}_t \geq T \) and \( \text{Revenue}_{t-n} < T \) for all \( n>0 \). All firm-years are required to have at least two prior years of revenue data to calculate this variable. |

| **Main Dependent Variables of Interest** | **Description** |
|-----------------------------------------|-----------------|
| \( \text{Analyst Following}_{t+1} \)    | Analyst following for the next fiscal year, calculated as the number of unique analysts issuing forecasts for firm \( i \)'s annual earnings in year \( t+1 \), as of the period immediately preceding the earnings announcement. Obtained from I/B/E/S. |
| \( \text{Bid-Ask Spread}_{t+1} \)       | Median value of the daily bid-ask spread calculated over the 12-month period starting after the current earnings announcement. Daily bid-ask spread is defined as \( (\text{ask} - \text{bid})/((\text{ask}+\text{bid})/2) \). |
| \( \text{Dedicated Ownership}_{t+1} \)  | The percentage of a firm’s shares held by dedicated 13F institutional investors according to the classification of Bushee (2001). |
| \( \text{Discretionary}_\text{Rev}_{t} \) | The discretionary revenues for a firm in a given year. Defined similar to Stubben (2010) but also incorporating recommendations of Collins et al. (2017) and McNichols and Stubben (2018). Specifically, it is defined as the residual from the following regression: \( \Delta \text{AR}_t = \alpha + \beta_1(1/\text{AT}_t) + \beta_2 \Delta \text{R1}_3 + \beta_3 \Delta \text{R4} + \sum \beta \text{ROA}_\text{Dum}_{ik} + \sum \beta \text{SG}_\text{Dum}_{ik} + \sum \beta \text{MB}_\text{Dum}_{ik} + \epsilon_t \), run by 2-digit SIC industry-year. \( i \) and \( t \) denote firm \( i \) and year \( t \). Where \( \Delta \text{AR} \) is the annual change in accounts receivable, \( \text{AT} \) is total assets, \( \Delta \text{R1}_3 \) is the change in revenue for quarters 1-3, \( \Delta \text{R4} \) is the change in revenue for quarter 4, \( \text{ROA}_\text{Dum}_k \) is an indicator for the \( k \)th quintile of return on assets, \( \text{SG}_\text{Dum}_k \) is an indicator for the \( k \)th quintile of current year sales growth, \( \text{MB}_\text{Dum}_k \) is an indicator for the \( k \)th quintile of the current market to book ratio. The third quintile indicators are omitted because of collinearity. |

Electronic copy available at: https://ssrn.com/abstract=2756306
| **Press Coverage**<sub>t+1</sub> | The total number of articles written about a firm over the 12-month period starting after the current earnings announcement which were not issued by the firm itself. |
|-------------------------------|-------------------------------------------------------------------------------------------------|
| **Quasi-Indexer Ownership**<sub>t+1</sub> | The percentage of a firm’s shares held by quasi-indexer 13F institutional investors according to the classification of Bushee (2001). |
| **Total Institutional Ownership**<sub>t+1</sub> | The percentage of a firm’s shares held by 13F institutional investors at the end of the next fiscal year (using the latest 13F filings filed on or before the fiscal year end). |
| **Transient Ownership**<sub>t+1</sub> | The percentage of a firm’s shares held by transient 13F institutional investors according to the classification of Bushee (2001). |
| **Turnover**<sub>t+1</sub> | Median value of daily turnover calculated over the 12-month period starting after the current earnings announcement. Daily turnover is defined as total volume divided by the total number of shares outstanding. |

**Independent and Control Variables**

| **12-Month Return**<sub>t</sub> | The firm’s returns for the current period, calculated over the 12-month period starting 3 months after the prior fiscal year end. |
| **Beat Analysts Inc**<sub>t</sub> | An indicator variable coded 1 if the firm’s annual earnings equal or exceed consensus (median) analyst forecasts for the current period, using the last set of estimates issued before the earnings announcement. |
| **Beat Analysts Rev**<sub>t</sub> | An indicator variable coded 1 if the firm’s annual revenue equals or exceeds consensus (median) analyst forecasts for the current period, using the last set of estimates issued before the earnings announcement. |
| **Beat Last Year’s Inc**<sub>t</sub> | An indicator variable coded 1 if the firm’s current annual earnings equal or exceed annual earnings for the prior year. |
| **Beat Last Year’s Rev**<sub>t</sub> | An indicator variable coded 1 if the firm’s current annual revenue equals or exceeds annual revenue for the prior year. |
| **Big4**<sub>t</sub> | An indicator variable coded 1 if the firm has a Big-4 auditor. |
| **Book-to-Market**<sub>t</sub> | Book-to-market ratio, using book value of common equity divided by market value of common equity, both measured at the beginning of the year. |
| **Equity Volatility**<sub>t</sub> | The standard deviation of ln(1+ret<sub>t</sub>), where ret<sub>t</sub> is the firm’s equity returns over the fiscal year. Requires at least 9 months of data. |
| Variable                  | Definition                                                                                                                                 |
|--------------------------|---------------------------------------------------------------------------------------------------------------------------------------------|
| Firm Age<sub>t</sub>     | Natural log of 1 plus the age of a firm in years. The age is approximated using the earliest of the first year a firm is covered in Compustat or CRSP or its IPO year. |
| Record_Sales<sub>t</sub> | Indicator variable coded 1 if current revenue is greater than revenue in any prior year (i.e., this is the first year the company has ever reached the current level of revenue). All firm-years are required to have at least two prior years of revenue data to calculate this variable. |
| Leverage<sub>t</sub>     | Beginning total long-term debt divided by total assets.                                                                                     |
| ln(MVE)<sub>t</sub>      | Natural log of the beginning of year market capitalization (in millions).                                                                    |
| ln(Revenue)<sub>t</sub>  | Natural log of current revenue (in millions).                                                                                              |
| Loss<sub>t</sub>         | An indicator variable coded 1 if the firm reports a loss.                                                                                   |
| Merger<sub>t</sub>       | An indicator variable coded 1 if the firm was involved in a merger in the current year.                                                    |
| Revenue Growth<sub>t</sub>| Change in revenue, scaled by lagged revenue growth (Revenue<sub>t</sub> – Revenue<sub>t-1</sub>/Revenue<sub>t-1</sub>)                        |
| ROA<sub>t</sub>          | Return on assets. Net income before discontinued operations and extraordinary items divided by beginning of year total assets.     |
APPENDIX

1. Approximate Randomization Procedure

In order to identify whether there are significantly more firms with total revenue just above than just below base-ten thresholds, we assign observations into intervals or bins around each potential threshold point according to the value of annual revenue. Bins above and below thresholds are defined in percentage terms by calculating the percentage increase in revenues necessary to move from the bottom to the top of the bin. For example, when the bin width is 1%, the bin above $100,000 includes all numbers greater than or equal to $100,000 and less than $101,000. Similarly, the bin below $100,000 includes all numbers greater than or equal to $99,009.90 and less than $100,000 (because a 1% increase in $99,009.90 results in revenues of $100,000). The bin widths used in our main tests are 0.25%, 0.5%, 1.0%, and 2.5%. The set of base-ten thresholds that we study are all points of the form $T = N \times 10^k$, where $100,000 \leq T \leq 80,000,000,000$. This results in 53 base-ten thresholds, as shown in Table 1 Panel A.

We compute the test statistic, $S_j$, by summing the number of observations in the bins just above all base-ten thresholds, the ABOVE bins, and dividing by the sum of the number of observations in the bins just below all base-ten thresholds, the BELOW bins. The subscript $j$ denotes the various bin widths. If managers increase revenue to be above base-ten thresholds, we would expect the $S_j$ ratio to be greater than 1.

$$S_j = \frac{\sum_{i=1}^{W_j} A_i}{\sum_{i=1}^{W_j} B_i}$$

where $i=1$, $W_j$ are the base-ten threshold points, $W_j$ is the number of threshold points for a given

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33 Defining the bin widths to be equal in percentage terms implies that the upper bin for a given revenue number is always larger in absolute terms. In untabulated results, we perform our analysis with ABOVE and BELOW bins constrained to be equal in absolute terms with no change in our inferences. The empirical distributions generated in our approximate randomization tests naturally control for this difference in absolute size. Additionally, our procedure essentially results in a logged distribution, where the bin widths above and below each point are equal in log terms.
bin width j, up to a maximum of 53, and A_i and B_i represent the number of observations in the ABOVE and BELOW bins within width j of threshold i, respectively.

We use an approximate randomization test to determine whether the S_j ratio is significantly greater than one. We generate pseudo values of S_j, \( \hat{S}_j \), by randomly selecting W_j pseudo threshold points without replacement from the entire set of reported revenue numbers and then counting the number of observations in the ABOVE and BELOW bins surrounding these pseudo threshold points. We repeat this process 9,999 times and count the number of times, r, that the randomly-generated pseudo-statistic \( \hat{S}_j \) is greater than or equal to the originally-computed value of S_j. Following Noreen (1989), the approximate randomization one-tailed p-value is computed as follows:

\[
P(S_j) = \frac{r+1}{9,999+1}
\]

where \( r = \sum_{j=1}^{9,999} I(\hat{S}_j \geq S_j) \), I is the indicator function, and j is the bin width. We repeat this procedure for each different bin width, j, from 0.25% of revenue to 2.5% of revenue. For example, assume that the actual value of S_j is 1.20 and that of the 9,999 randomly-selected sets of bins, none resulted in a pseudo-statistic \( \hat{S}_j \) value as large as 1.20. In this case, the value of P(S_j) is 0.0001 = (0+1)/(9,999+1).

2. Comparing the Base-Ten Threshold Effect in Revenue with Cost of Goods Sold, Total Assets, and Market Value of Equity

To rule out the possibility that database providers introduce rounding or that managers prefer base-ten values for all numbers, Table A.1 compares the base-ten threshold effect of revenue with that of cost of goods sold, total assets, and market capitalization. In order to do this, we perform additional approximate randomization tests where we compare the S_j statistics (aggregate threshold ABOVE/BELOW ratio) for revenue with the S_j statistics of the other metrics. Finding
elevated ABOVE/BELOW ratios in revenue data beyond those in these other items would suggest that managers take actions to increase revenue to reach these base-ten thresholds to a greater extent than they do these other metrics. We choose these metrics as controls because they are the same order of magnitude as revenue. Cost of goods sold is a particularly apt benchmark because it is generated by similar processes that lead to revenue and is less likely to be strategically managed to exceed base-ten thresholds. Total assets are another reasonable benchmark because they are of a similar magnitude as revenues; assets are a particularly conservative benchmark because, as a measure of size, firms are also likely to have incentives to beat base-ten asset thresholds, although we expect these incentives to be relatively weaker because total assets are not frequently used as performance targets to the extent that revenues are, as demonstrated in Table A.1. Market value of equity is not subject to direct manipulation by firms, but managers could have incentives to manipulate it upward through indirect actions, making it another conservative benchmark.\textsuperscript{34}

Using cost of goods sold as an illustration, the test statistic, $V_j$, for these tests is the ratio of the separate $S_j$ statistics for revenue and cost of goods sold.

\begin{equation}
V_j = \frac{S_{j,\text{Revenue}}}{S_{j,\text{COGS}}}
\end{equation}

where $S_{j,\text{Revenue}}$ is the ratio of the number of ABOVE and BELOW observations in bin width $j$ around all base-ten thresholds for revenue and $S_{j,\text{COGS}}$ is the same ratio for cost of goods sold. If managers have incentives to manage reported revenue to exceed base-ten thresholds above and beyond any threshold effect that may exist for cost of goods sold, then this $V_j$ ratio should be greater than one. Similar test statistics are computed using assets and market capitalization.

\textsuperscript{34} Although firms have more difficulty directly managing market capitalization than in managing reported revenue, Iliev (2010) suggests that firms can exercise some influence on the level of end-of-period market values.
We then use a similar approximate randomization test as described above to determine whether the $V_j$ ratio is significantly greater than one, now randomly selecting points from both the revenue and comparison metric distributions. Data comparing the magnitude of the base-ten threshold effect in reported revenue to the corresponding effect in cost of goods sold, total assets, and market value of equity are presented in Table A.1. For each of the bin widths reported, there is a significantly greater proportion of revenue observations just above a base-ten threshold relative to all three comparison metrics, with an approximate randomization p-value reported of less than 0.01 in each case. These results support our prediction that managers take strategic actions to report revenue just above base-ten thresholds, and this base-ten revenue threshold effect is not caused by a general preference for base-ten numbers or because of rounding introduced by the data provider.

Table A.1 Comparison of the Base-Ten Threshold Effect in Revenues to Other Metrics

For all Thresholds of the form $T = N \times 10^K$, for integers N (1 through 9) and K (5 through 10)

Panel A: Revenue and Cost of Goods Sold Comparison

| Bin Width (%) | Revenue ABOVE/BELOW (1) | COGS ABOVE/BELOW (2) | Ratio of Ratios (1)/(2) | Approximate Randomization p-value |
|---------------|-------------------------|----------------------|-------------------------|----------------------------------|
| 0.25%         | 1.588                   | 1.111                | 1.429                   | 0.0001***                        |
| 0.50%         | 1.344                   | 1.077                | 1.248                   | 0.0001***                        |
| 1.00%         | 1.163                   | 1.027                | 1.133                   | 0.0001***                        |
| 2.50%         | 1.079                   | 1.006                | 1.072                   | 0.0001***                        |
| Total Observations: | 367,233                | 363,907              |                         |                                  |

Panel B: Revenue and Total Assets Comparison

| Bin Width (%) | Revenue ABOVE/BELOW (1) | Assets ABOVE/BELOW (2) | Ratio of Ratios (1)/(2) | Approximate Randomization p-value |
|---------------|-------------------------|------------------------|-------------------------|----------------------------------|
| 0.25%         | 1.588                   | 1.273                  | 1.248                   | 0.0001***                        |
| 0.50%         | 1.344                   | 1.152                  | 1.166                   | 0.0001***                        |
| 1.00%         | 1.163                   | 1.080                  | 1.077                   | 0.0001***                        |
| 2.50%         | 1.079                   | 1.052                  | 1.026                   | 0.0062***                        |
| Total Observations: | 367,233                | 367,233               |                         |                                  |
3. Existence of the Base-Ten Revenue Threshold Effect in International Data

If our evidence of the base-ten revenue threshold effect is driven by issues unique to the data aggregation process of Compustat, then we should not find evidence of a base-ten revenue threshold effect when examining revenues of international firms covered by the Osiris database. However, if investors in other countries are subject to a base-ten bias, then we should continue to find the threshold effects in this sample. This should be the case even when revenues are measured in the original reporting currency (as opposed to converting all reported revenues into a common currency) because the processing constraints driving the base-ten threshold effect are unrelated to actual economic value and instead driven by the nominal amounts themselves.

Panel C: Revenue and Market Value of Equity Comparison

| Bin Width (%) | Revenue ABOVE/BELOW | MVE ABOVE/BELOW | Ratio of Ratios (1)/(2) | Approximate Randomization p-value |
|---------------|---------------------|----------------|------------------------|----------------------------------|
| 0.25%         | 1.588               | 1.085          | 1.463                  | 0.0001***                        |
| 0.50%         | 1.344               | 1.031          | 1.304                  | 0.0001***                        |
| 1.00%         | 1.163               | 1.026          | 1.134                  | 0.0001***                        |
| 2.50%         | 1.079               | 1.015          | 1.063                  | 0.0001***                        |

Total Observations: 367,233 291,526

This table presents the results of one-tailed approximate randomization tests estimating the significance of the Threshold Effect for reported revenues relative to that of cost of goods sold, total assets, and market value of equity. Rows present the ratio of the number of observations in the bins just above and below threshold points for each financial metric, as well as the ratio of these ratios (the test statistic used to generate the approximate randomization p-value). A test statistic significantly greater than 1 is evidence that the threshold effect in revenue is stronger than in the comparison metric. ***,**,* Significant at the 0.01, 0.05, 0.1 level for one-tailed test.
Table A.2 Ratio-of-Ratios Threshold Tests: Non-U.S. Firms

Demonstrating the Existence of the Threshold Effect in a Variety of Countries and Currencies

| Bin Width (%) | Revenue ABOVE/BELOW (1) | Assets ABOVE/BELOW (2) | Ratio of Ratios (1)/(2) | Approximate Randomization p-value |
|---------------|--------------------------|------------------------|------------------------|----------------------------------|
| 0.25%         | 1.183                    | 0.991                  | 1.194                  | 0.0001***                       |
| 0.50%         | 1.101                    | 0.999                  | 1.102                  | 0.0010***                       |
| 1.00%         | 1.083                    | 0.971                  | 1.114                  | 0.0001***                       |
| 2.50%         | 1.044                    | 1.003                  | 1.040                  | 0.0024***                       |

Total Observations: 390,104

This table presents the results of one-tailed approximate randomization tests estimating the significance of the Threshold Effect for reported revenues relative to that of total assets for a set of non-U.S. firms reporting in a variety of currencies. The data in this table are taken from the Osiris database and comprise all firm-years with available revenue and total asset data for fiscal years 1982-2014 that are available from Osiris, excluding U.S. firms. Rows present the ratio of the number of observations in the bins just above and below threshold points for both revenue and total assets, as well as the ratio of these ratios (the test statistic used to generate the approximate randomization p-value). A test statistic significantly greater than 1 is evidence that the threshold effect in revenue is stronger than in total assets. ***,**,,* Significant at the 0.01, 0.05, 0.1 level for one-tailed test.

4. Importance of Specific Thresholds

In order to identify whether the threshold effect we observe in Table 3 is driven by the most salient thresholds, we examine only the 6 “major” thresholds of the form $10^k$ (i.e., $10$ million, $1$ billion, etc.) and report the results in Panel A below. We find that the magnitude of the threshold effect (the above/below ratio) is larger than for the full set of thresholds. In Panel B, we examine the remaining 47 “minor” thresholds and still find a strongly significant threshold effect—the approximate randomization p-values are statistically significant at the 1% level for all bin widths. Together, these results validate the importance of all base-ten thresholds and support our use of all 53 thresholds.
Table A.3 Major and Minor Thresholds

Panel A: Major Thresholds
For all Thresholds of the form $T = 1 \times 10^K$, for integers $K$ (5 through 10)

| Bin Width (%) | Bin BELOW Threshold Points | Bin ABOVE Threshold Points | Ratio ABOVE/BELLOW | Approximate Randomization p-value |
|---------------|---------------------------|---------------------------|-------------------|----------------------------------|
| 0.25%         | 300                       | 590                       | 1.967             | 0.0001***                       |
| 0.50%         | 644                       | 961                       | 1.492             | 0.0001***                       |
| 1.00%         | 1,392                     | 1,747                     | 1.255             | 0.0001***                       |
| 2.50%         | 3,736                     | 4,184                     | 1.120             | 0.0001***                       |

Total Observations: 367,233

Panel B: Minor Thresholds
For all Thresholds of the form $T = N \times 10^K$, for integers $N$ (2 through 9) and $K$ (5 through 10)

| Bin Width (%) | Bin BELOW Threshold Points | Bin ABOVE Threshold Points | Ratio ABOVE/BELLOW | Approximate Randomization p-value |
|---------------|---------------------------|---------------------------|-------------------|----------------------------------|
| 0.25%         | 2,645                     | 4,053                     | 1.532             | 0.0001***                       |
| 0.50%         | 5,564                     | 7,357                     | 1.322             | 0.0001***                       |
| 1.00%         | 11,683                    | 13,450                    | 1.151             | 0.0001***                       |
| 2.50%         | 30,000                    | 32,195                    | 1.073             | 0.0001***                       |

Total Observations: 367,233

The results of one-tailed approximate randomization tests estimating the significance of the Threshold Effect in reported revenues for a variety of logged bin widths. Panel A provides results only for "major" thresholds of the form $1 \times 10^K$. Panel B provides results for base-ten thresholds that are not "major" thresholds. Statistics include the number of observations in the bins just above and below the threshold points, and the ratio of the observations in the above and below bins (the test statistic used to generate the approximate randomization p-value). A ratio significantly greater than 1 is evidence of the existence of a base-ten threshold effect. ***,**, Significant at the 0.01, 0.05, 0.1 level for one-tailed test.

5. Further Evidence for Revenue Targets

Figures A.1a through A.1f provide graphical support of H3a related to the existence of revenue targets influencing the distribution of reported revenue. These figures provide histograms of the observations around base-ten revenue thresholds partitioned on whether the original target (compensation plan, management forecast, or analyst forecast) was above or below a base-ten threshold. We present these figures using bin widths of 0.25% (with the except of Figure A.1 which
uses 0.5% bins because of the sparse sample). When revenue targets are in the 5% above a base-
ten threshold (Figures A.1a, A.1c, and A.1e), we find strong evidence of observations “heaping”
just above the threshold (p-value > 0.01). When revenue targets are in the 5% below a base-
ten threshold (Figures A.1b, A.1d, and A.1f), we find less evidence of observations “heaping” above
the threshold. Consistent with prior evidence on efforts to beat these targets, we see strong
clustering in the distribution in each figure around where the target is set. Importantly, however,
the base-ten threshold effect is only statistically significant (using untabulated approximate
randomization tests) when the original target was set above a base-ten threshold, providing
evidence that these targets provide firms with an incentive to beat base-ten thresholds. The base-
ten threshold effect is significantly stronger when targets are set above versus below base-ten
thresholds for all three types of targets, although the main base-ten threshold effect is statistically
insignificant for compensation plan targets in Figure A.1a because the small sample size leads to
greater variability in the number of observations in adjacent bins. These results indicate that formal
and informal contracts provide another incentive for base-ten threshold beating. However,
although the basic threshold effect is not significant in Figures A.1b, A.1d, or A.1f, visually we
see some evidence of a bump in observations just after base-ten thresholds, even when firm targets
are set below these benchmarks. Figure 4 and Table 5 above show that firms experience a
significant base-ten threshold effect when they have no revenue targets at all, consistent with
formal and informal contracts providing only a partial explanation for base-ten threshold beating.
Our visibility tests presented in the body of the paper explore other explanations for this
phenomenon.
Figure A.1 Distribution of Reported Revenue Around Base-Ten Thresholds When Target Revenues Are Within 5% of a Base-Ten Threshold

Figure A.1a Compensation Plan Target is Within 5% ABOVE a Threshold

Figure A.1b Compensation Plan Target is Within 5% BELOW a Threshold

Figure A.1c Last Management Forecast is Within 5% ABOVE a Threshold

Figure A.1d Last Management Forecast is Within 5% BELOW a Threshold

Figure A.1e Last Consensus Analyst Forecast is Within 5% ABOVE a Threshold

Figure A.1f Last Consensus Analyst Forecast is Within 5% BELOW a Threshold

Bin widths are 0.5% for Figures A.1a and A.1b (because of limited sample size), and 0.25% for Figures A.1c-A.1f
Figure 2. Distribution of Revenue Around Base-Ten Thresholds
0.25% Bin Width

The number of observations contained in the 50 non-overlapping bins immediately above and below each base-ten threshold, summed across all 53 thresholds, using bin widths of 0.25%. The red line represents the bin just above a threshold point.
Figure 3. Distribution of Executive Compensation Revenue Targets, Management Revenue Forecasts, and Consensus Analyst Revenue Forecasts Around Base-Ten Thresholds (0.25% Bin Width)

Figure 3a. Executive Compensation Plan Revenue Targets

Figure 3b. Management Revenue Forecasts

Figure 3c. Consensus Analyst Revenue Forecasts

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Figure 4. Distribution of Revenue Around Base-Ten Thresholds for Firms Lacking Explicit Revenue Targets
(0.25% Bin Width)
Table 1. Descriptive Information

Panel A: List of Base-Ten Thresholds Studied in the Paper

|   |   |   |   |   |
|---|---|---|---|---|
| 1 | $100,000 | 15 | $6,000,000 | 29 | $200,000,000 |
| 2 | $200,000 | 16 | $7,000,000 | 30 | $300,000,000 |
| 3 | $300,000 | 17 | $8,000,000 | 31 | $400,000,000 |
| 4 | $400,000 | 18 | $9,000,000 | 32 | $500,000,000 |
| 5 | $500,000 | 19 | $10,000,000 | 33 | $600,000,000 |
| 6 | $600,000 | 20 | $20,000,000 | 34 | $700,000,000 |
| 7 | $700,000 | 21 | $30,000,000 | 35 | $800,000,000 |
| 8 | $800,000 | 22 | $40,000,000 | 36 | $900,000,000 |
| 9 | $900,000 | 23 | $50,000,000 | 37 | $1,000,000,000 |
| 10 | $1,000,000 | 24 | $60,000,000 | 38 | $2,000,000,000 |
| 11 | $2,000,000 | 25 | $70,000,000 | 39 | $3,000,000,000 |
| 12 | $3,000,000 | 26 | $80,000,000 | 40 | $4,000,000,000 |
| 13 | $4,000,000 | 27 | $90,000,000 | 41 | $5,000,000,000 |
| 14 | $5,000,000 | 28 | $100,000,000 | 42 | $6,000,000,000 |

All points of the form \( T = N \times 10^k \), where $100,000 \leq T \leq $80,000,000,000.

Panel B: Revenue Approximate Randomization Analysis Sample

Firm-Year Revenue Observations by Decade

| Decade | N       |
|--------|---------|
| 50s and 60s | 32,651   |
| 70s     | 52,257  |
| 80s     | 67,233  |
| 90s     | 89,136  |
| 2000s   | 79,423  |
| 2010s*  | 46,533  |

\( 367,233 \)

*2010-2017

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Panel C: Regression Analysis Sample

| Variable                        | N   | Mean  | Median | Std  | P25 | P75 |
|--------------------------------|-----|-------|--------|------|-----|-----|
| Threshold<sub>t</sub>          | 46,865 | 0.29  | 0.00   | 0.45 | 0.00 | 1.00 |
| Press Coverage<sub>t+1</sub>   | 34,655 | 170.37| 115.00 | 195.74| 68.00 | 187.00 |
| Turnover<sub>t+1</sub>         | 46,865 | 0.01  | 0.01   | 0.01 | 0.00 | 0.01 |
| Bkd-Ask Spread<sub>t+1</sub>  | 45,250 | 4.62  | 1.40   | 8.89 | 0.64 | 4.30 |
| Analyst Following<sub>t+1</sub>| 45,783 | 7.63  | 5.00   | 6.68 | 2.00 | 11.00 |
| Institutional Ownership<sub>t+1</sub> | 46,796 | 54.55 | 62.54  | 35.12| 23.43 | 84.15 |
| Quasi-Indexer Ownership<sub>t+1</sub> | 46,796 | 36.67 | 40.35  | 25.08| 13.15 | 57.03 |
| Transient Ownership<sub>t+1</sub> | 46,796 | 13.56 | 12.37  | 11.29| 3.34 | 20.92 |
| Dedicated Ownership<sub>t+1</sub> | 46,796 | 2.01  | 0.02   | 4.61 | 0.00 | 1.23 |

Control Variables

| Variable                        | N   | Mean  | Median | Std  | P25 | P75 |
|--------------------------------|-----|-------|--------|------|-----|-----|
| First-Ever<sub>t</sub>         | 46,865 | 0.56  | 1.00   | 0.50 | 0.00 | 1.00 |
| ln(Revenue)<sub>t</sub>        | 46,865 | 6.36  | 6.34   | 2.04 | 5.03 | 7.69 |
| Revenue Growth<sub>t</sub>     | 46,865 | 0.17  | 0.08   | 0.54 | -0.02 | 0.23 |
| Beat Last Year's Rev<sub>t</sub> | 46,865 | 0.72  | 1.00   | 0.45 | 0.00 | 1.00 |
| Beat Analysts Rev<sub>t</sub>  | 46,865 | 0.57  | 1.00   | 0.50 | 0.00 | 1.00 |
| Loss<sub>t</sub>               | 46,865 | 0.27  | 0.00   | 0.44 | 0.00 | 1.00 |
| Beat Last Year's Inc<sub>t</sub> | 46,865 | 0.59  | 1.00   | 0.49 | 0.00 | 1.00 |
| Beat Analysts Inc<sub>t</sub>  | 46,865 | 0.63  | 1.00   | 0.48 | 0.00 | 1.00 |
| ROA<sub>t</sub>                | 46,865 | 0.00  | 0.03   | 0.23 | -0.01 | 0.08 |
| 12-Month Return<sub>t</sub>    | 46,865 | 0.18  | 0.07   | 0.76 | -0.21 | 0.37 |
| Equity Volatility<sub>t</sub>  | 46,865 | 0.13  | 0.11   | 0.08 | 0.07 | 0.16 |
| ln(MVE)<sub>t</sub>            | 46,865 | 6.74  | 6.63   | 1.81 | 5.47 | 7.90 |
| Big<sub>t</sub>                | 46,865 | 0.86  | 1.00   | 0.35 | 1.00 | 1.00 |
| Book-to-Market<sub>t</sub>     | 46,865 | 0.56  | 0.46   | 0.66 | 0.26 | 0.74 |
| Leverage<sub>t</sub>           | 46,865 | 0.22  | 0.13   | 0.27 | 0.00 | 0.32 |
| Firm Age<sub>t</sub>           | 46,865 | 2.79  | 2.71   | 0.72 | 2.20 | 3.26 |
| Merger<sub>t</sub>             | 46,865 | 0.17  | 0.00   | 0.38 | 0.00 | 0.00 |

Panel A lists the base-ten thresholds studied in this paper. In Panel B, observations are required to have non-missing values of current total assets and revenues and correspond to the full revenue approximate randomization sample in Table 2. Descriptive statistics of the variables used in the regression analyses are given in Panel C. For the control variables and Threshold, the statistics given correspond to the sample used for the t+1 turnover regression in Table 6 panel C (the regression specification with the largest sample size).
### Table 2. Importance of Revenue Targets

#### Panel A. Prevalence of Performance Metrics in Executive Compensation Plans

| Performance Metric          | Proportion |
|----------------------------|------------|
| EPS                        | 0.301      |
| Revenue                    | 0.288      |
| Operating Income           | 0.236      |
| Net Income                 | 0.205      |
| Operating Cash Flows       | 0.198      |

#### Panel B. Prevalence of Performance Metrics in Management Forecasts

| Performance Metric          | Proportion |
|----------------------------|------------|
| EPS                        | 0.533      |
| Revenue                    | 0.520      |
| Capital Expenditures       | 0.440      |
| EBITDA                     | 0.130      |
| Net Income                 | 0.090      |

#### Panel C. Prevalence of Performance Metrics in Analyst Forecasts

| Performance Metric          | Proportion |
|----------------------------|------------|
| EPS                        | 0.887      |
| Revenue                    | 0.864      |
| Net Income                 | 0.699      |
| Pre-Tax Income             | 0.671      |
| EBITDA                     | 0.596      |

This table demonstrates the prevalence of revenue targets for U.S. firms. Each panel lists the top five most common accounting metric targets, by the proportion of firm-years which use each metric for a given type of target. Panel A demonstrates the prevalence of various accounting metrics as targets in executive compensation contracts for grants granted for 16,247 firm-years over 1998 to 2016 with at least one accounting metric. Panel B demonstrates the prevalence of various accounting metrics in management forecasts for 36,992 firm-years over 2003 to 2018. Panel C demonstrates the prevalence of various accounting metrics in analyst forecasts for 111,556 firm-years over 2000 to 2018, using the IBES Detail files and including stopped forecasts (the Summary files are restricted to only firms which have at least one EPS forecast). The sample period for each of the three metrics starts in the year when revenue targets are tracked in the data.
Table 3. Approximate Randomization Threshold Tests
For all Thresholds of the form $T = N \times 10^K$, for integers $N$ (1 through 9) and $K$ (5 through 10)

**Panel A: All Bin Widths**

| Bin Width (%) | Number of Observations in | Approximate Randomization p-value |
|---------------|---------------------------|----------------------------------|
|               | Bin BELOW Threshold Points | Bin ABOVE Threshold Points | Ratio ABOVE/BELOW |
| 0.25%         | 2,894                     | 4,596                           | 1.588              | 0.0001*** |
| 0.50%         | 6,116                     | 8,220                           | 1.344              | 0.0001*** |
| 1.00%         | 12,896                    | 15,000                          | 1.163              | 0.0001*** |
| 2.50%         | 33,289                    | 35,903                          | 1.079              | 0.0001*** |
| 5.00%         | 66,789                    | 69,491                          | 1.040              | 0.0001*** |
| 7.50%         | 99,278                    | 102,244                         | 1.030              | 0.0001*** |
| 10.00%        | 131,313                   | 133,918                         | 1.020              | 0.0002*** |
| 15.00%        | 193,775                   | 196,244                         | 1.013              | 0.0286**  |
| 20.00%        | 253,308                   | 255,676                         | 1.009              | 0.1231    |

Total Observations: 367,233

**Panel B: Incremental Observations – All Bin Widths**

| Bin Width (%) | Number of Observations in | Approximate Randomization p-value |
|---------------|---------------------------|----------------------------------|
|               | Band BELOW Threshold Points | Band ABOVE Threshold Points | Ratio ABOVE/BELOW |
| 0 to 0.25%    | 2,894                     | 4,596                           | 1.588              | 0.0001*** |
| 0.25% to 0.50%| 3,222                     | 3,606                           | 1.119              | 0.0001*** |
| 0.50% to 1.00%| 6,780                     | 6,780                           | 1.000              | 0.4861    |
| 1.00% to 2.50%| 20,393                    | 20,903                          | 1.025              | 0.0017*** |
| 2.50% to 5.00%| 33,500                    | 33,588                          | 1.003              | 0.3502    |
| 5.00% to 7.50%| 32,489                    | 32,753                          | 1.008              | 0.1688    |
| 7.50% to 10.00%| 32,035                   | 31,674                          | 0.989              | 0.8863    |
| 10.00% to 15.00%| 62,462                   | 62,326                          | 0.998              | 0.5903    |
| 15.00% to 20.00%| 59,533                   | 59,432                          | 0.998              | 0.5405    |

Total Observations: 367,233

The results of one-tailed approximate randomization tests estimating the significance of the Threshold Effect in reported revenues for a variety of logged bin widths. Results for the full set of Base-Ten thresholds given in Panel A. In Panel B are results for the INCREMENTAL observations for each bin width (those within the given bin width not included in the next smaller bin width). Statistics include the number of observations in the bins just above and below the threshold points, the associated binomial test p-value (assuming an equal probability of observations falling in either bin), and the ratio of the observations in the above and below bins (the test statistic used to generate the approximate randomization p-value). A test statistic significantly greater than 1 is evidence of the existence of a base-ten threshold effect. ***, **, * Significant at the 0.01, 0.05, 0.1 level for one-tailed test.
Table 4. Threshold Effect in Formal and Informal Revenue Contracts

For all Thresholds of the form $T = N \times 10^K$, for integers $N$ (1 through 9) and $K$ (5 through 10)

### Panel A. Executive Compensation Plan Revenue Targets

| Bin Width (%) | Bin BELOW Threshold Points | Bin ABOVE Threshold Points | Ratio ABOVE/BELOW | Approximate Randomization p-value |
|---------------|---------------------------|---------------------------|-------------------|----------------------------------|
| 0.25%         | 21                        | 213                       | 10.143            | 0.0001***                       |
| 0.50%         | 56                        | 253                       | 4.518             | 0.0001***                       |
| 1.00%         | 127                       | 317                       | 2.496             | 0.0001***                       |
| 2.50%         | 410                       | 556                       | 1.356             | 0.0001***                       |
| **Total Observations:** | **4,855**                             |                           |                   |                                 |

### Panel B: Management Revenue Forecasts

| Bin Width (%) | Bin BELOW Threshold Points | Bin ABOVE Threshold Points | Ratio ABOVE/BELOW | Approximate Randomization p-value |
|---------------|---------------------------|---------------------------|-------------------|----------------------------------|
| 0.25%         | 52                        | 1,419                     | 27.288            | 0.0001***                       |
| 0.50%         | 126                       | 1,512                     | 12.000            | 0.0001***                       |
| 1.00%         | 309                       | 1,723                     | 5.576             | 0.0001***                       |
| 2.50%         | 1,176                     | 2,603                     | 2.213             | 0.0001***                       |
| **Total Observations:** | **18,001**                             |                           |                   |                                 |

### Panel C: Consensus Analyst Revenue Forecasts

| Bin Width (%) | Bin BELOW Threshold Points | Bin ABOVE Threshold Points | Ratio ABOVE/BELOW | Approximate Randomization p-value |
|---------------|---------------------------|---------------------------|-------------------|----------------------------------|
| 0.25%         | 664                       | 1,283                     | 1.932             | 0.0001***                       |
| 0.50%         | 1,325                     | 2,017                     | 1.522             | 0.0001***                       |
| 1.00%         | 2,630                     | 3,411                     | 1.297             | 0.0001***                       |
| 2.50%         | 6,905                     | 7,791                     | 1.128             | 0.0001***                       |
| **Total Observations:** | **75,109**                             |                           |                   |                                 |

This table presents the results of one-tailed approximate randomization tests estimating the significance of the Threshold Effect for three revenue targets specified in formal and informal contracts: executive compensation plan revenue targets, management revenue forecasts, and consensus (median) analyst revenue forecasts. The data used are not actual reported revenues but the target numbers themselves. Panel A uses grant-level data (sometimes multiple observations per firm). In Panel B (C), the last forecast (consensus forecast) issued by the firm (analysts) preceding the earnings announcements for the period is used. Panel B uses all management revenue point forecasts as well as the bottom value of management revenue range forecasts. A test statistic significantly greater than 1 is evidence of the existence of a base-ten threshold effect. ***,**, Significant at the 0.01, 0.05, 0.1 level for one-tailed test.
Table 5. The Revenue Threshold Effect for Firms Without Explicit Targets
For all Thresholds of the form $T = N \times 10^K$, for integers $N$ (1 through 9) and $K$ (5 through 10)

| Bin Width (%) | Bin BELOW Threshold Points | Bin ABOVE Threshold Points | Ratio ABOVE/BELOW | Approximate Randomization p-value |
|---------------|----------------------------|----------------------------|-------------------|----------------------------------|
| 0.25%         | 341                        | 472                        | 1.384             | 0.0051***                        |
| 0.50%         | 720                        | 840                        | 1.167             | 0.0435**                         |
| 1.00%         | 1,456                      | 1,551                      | 1.065             | 0.2139                           |
| 2.50%         | 3,690                      | 3,759                      | 1.019             | 0.4586                           |

Total Observations: 40,391

This table presents the results of one-tailed approximate randomization tests which investigate the significance of the revenue threshold effect for firms which have no available compensation plan revenue targets, management revenue forecasts, or analyst revenue forecasts.***,**,* Significant at the 0.01, 0.05, 0.1 level for one-tailed test.

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Table 6. Visibility Effects of Beating a Base-Ten Revenue Threshold for the First Time

Panel A. Media Coverage After Beating a Base-Ten Revenue Threshold for the First Time

| VARIABLES            | (1)  | (2)  | (3)  | (4)  |
|----------------------|------|------|------|------|
| **Threshold, t**     | 5.343** | 4.295*** | 5.919** | 5.681*** |
|                      | (2.296) | (2.911) | (2.429) | (3.402) |
| **First-Ever, t**    | -7.780** | 2.115 | -8.932** | -0.929 |
|                      | (-2.291) | (0.938) | (-2.536) | (-0.361) |
| ln(Revenue), t       | 32.02*** | 13.00*** | 32.99*** | 10.57*** |
|                      | (11.32) | (5.158) | (11.23) | (4.035) |
| Revenue Growth, t    | 5.181*** | 1.583 | 4.645** | 1.081 |
|                      | (3.183) | (1.408) | (2.563) | (0.881) |
| Beat Last Year's Rev, t | -1.434 | -0.166 | -0.873 | 1.657 |
|                      | (-0.627) | (-0.0926) | (-0.354) | (0.865) |
| Beat Analysts Rev, t | 0.481 | 0.727 | 1.939 | 1.906 |
|                      | (0.311) | (0.670) | (1.202) | (1.628) |
| Loss, t              | 23.48*** | 5.990*** | 19.57*** | -0.773 |
|                      | (7.875) | (2.922) | (6.542) | (-0.383) |
| Beat Last Year's Inc, t | 9.019*** | 1.897* | 12.24*** | 3.058*** |
|                      | (5.359) | (1.711) | (7.082) | (2.598) |
| Beat Analysts Inc, t | 0.294 | 1.368 | 2.035 | 1.845 |
|                      | (-1.629) | (1.738) | (-1.914) | (1.562) |

Controls
Y       Y       Y       Y
Industry & Year FE
Y       N       Y       N
Firm & Year FE
N       Y       N       Y
Observations
34,655   34,655   31,485   31,485
Adjusted R-squared
0.519    0.824    0.516    0.819

Regression results estimating the effect of beating a base-ten revenue threshold for the first time on future press coverage. For fiscal years 1999-2014. 3-digit SIC code industry. Press coverage excludes firm-initiated articles (press releases). Controls untabulated for parsimony comprise: ROA, 12-Month Return, Equity Volatility, ln(MVE), Big4, Book-to-Market, Leverage, Firm Age, Merger. Robust t-statistics clustered by firm in parentheses. ***, **, * Significant at the 0.01, 0.05, 0.1 level for two-tailed test.
## Panel B. Institutional Ownership After Beating a Base-Ten Revenue Threshold for the First Time

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|-----------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|
| **Threshold** <sub>t</sub> | 0.673*** (2.699) | 0.373 (1.442) | 0.481** (2.525) | 0.292 (1.499) | 0.227*** (2.040) | 0.0620 (-0.0455) | -0.00215 (0.123) | 0.00590 |
| **First-Ever** <sub>t</sub> | -1.065*** (-2.701) | -0.613 (-1.429) | -0.227 (-0.786) | -0.0185 (-0.0605) | -0.621*** (-3.744) | -0.298* (-1.762) | -0.220*** (-2.753) | -0.248*** (-2.867) |
| ln(Revenue) <sub>t</sub> | 0.299 (1.306) | -0.112 (-0.444) | 0.0161 (0.109) | -0.249 (-1.535) | 0.178* (1.715) | 0.0456 (0.390) | 0.0181 (0.351) | 0.0206 (0.410) |
| Revenue Growth <sub>t</sub> | 0.596* (1.928) | 0.604* (1.800) | 0.00318 (0.0142) | 0.316 (1.326) | 0.524*** (3.854) | 0.0199 (1.401) | 0.0943 (1.479) | 0.0877 (1.220) |
| Beat Last Year's Rev <sub>t</sub> | 0.372** (2.066) | 0.371* (1.945) | 0.0523 (0.389) | 0.153 (1.083) | 0.400*** (4.854) | 0.240*** (2.899) | -0.0428 (-1.184) | -0.0456 (-1.127) |
| Beat Analysts Rev <sub>t</sub> | 0.211 (1.108) | 0.348* (1.745) | 0.0654 (0.466) | 0.354** (2.407) | 0.177** (2.128) | -0.05026 (-0.650) | -0.246 (0.889) | -0.0692 |
| Loss <sub>t</sub> | -2.254*** (-5.901) | -1.587*** (-3.976) | -1.675*** (-6.205) | -1.442*** (-5.012) | -0.717*** (-4.497) | -0.295* (-1.786) | 0.120 (1.602) | 0.104 (1.375) |
| Beat Last Year's Inc <sub>t</sub> | 0.247 (1.212) | 0.277 (1.267) | -0.0308 (-0.207) | 0.255 (1.616) | 0.371*** (4.190) | 0.111 (1.209) | -0.0490 (-1.175) | -0.0692 (-1.582) |

Regression results estimating the effect of beating a base-ten revenue threshold for the first time on future institutional ownership. For fiscal years 1999-2014. Institutional owners are classified into three categories following the classifications of Bushee (2001): Quasi-Indexer, Transient, and Dedicated. Controls untabulated for parsimony comprise: ROA, 12-Month Return, Equity Volatility, ln(MVE), Big4, Book-to-Market, Leverage, Firm Age, Merger. Specifications using industry and year fixed effects omitted for parsimony. Robust t-statistics clustered by firm in parentheses. ***, **, * Significant at the 0.01, 0.05, 0.1 level for two-tailed test.
Table C. Liquidity After Beating a Base-Ten Revenue Threshold for the First Time

| VARIABLES                  | (1) Turnover $t+1$ | (2) Turnover $t+2$ | (5) Bid-Ask Spread $t+1$ | (6) Bid-Ask Spread $t+2$ |
|----------------------------|-------------------|-------------------|--------------------------|--------------------------|
| Threshold$_t$              | 0.000290***       | 0.000107*         | -0.191***                | -0.0313                  |
|                           | (4.802)           | (1.686)           | (-2.726)                 | (-0.512)                 |
| First-Ever$_t$             | 1.93e-05          | 0.000108          | -0.132                   | -0.150*                  |
|                           | (0.224)           | (1.202)           | (-1.427)                 | (-1.919)                 |
| ln(Revenue)$_t$            | -0.000226**       | -0.000368***      | 0.374***                 | 0.293***                 |
|                           | (-1.995)          | (-2.838)          | (2.925)                  | (2.761)                  |
| Revenue Growth$_t$         | 0.000608***       | 0.000182**        | -0.601***                | -0.0820                  |
|                           | (7.072)           | (2.387)           | (-7.090)                 | (-8.025)                 |
| Beat Last Year’s Rev$_t$  | 2.70e-05          | 6.99e-06          | -0.238***                | -0.114                   |
|                           | (0.370)           | (0.0911)          | (-2.885)                 | (-1.508)                 |
| Beat Analysts Rev$_t$      | 2.03e-05          | 8.53e-05*         | -0.135**                 | -0.115**                 |
|                           | (0.447)           | (1.707)           | (-2.536)                 | (-2.386)                 |
| Loss$_t$                   | -0.000137         | 8.66e-05          | 0.957***                 | 0.702***                 |
|                           | (-1.389)          | (0.917)           | (8.072)                  | (6.963)                  |
| Beat Last Year’s Inc$_t$  | -6.87e-06         | -5.13e-05         | 0.161***                 | -0.00912                 |
|                           | (-0.145)          | (-1.021)          | (2.711)                  | (-0.165)                 |
| Beat Analysts Inc$_t$      | 4.09e-05          | -4.20e-05         | -0.268***                | -0.175***                |
|                           | (0.842)           | (-0.812)          | (-4.459)                 | (-3.218)                 |
| Controls                   | Y                 | Y                 | Y                        | Y                        |
| Firm & Year FE             | Y                 | Y                 | Y                        | Y                        |
| Observations               | 46.865            | 42.922            | 45.250                   | 41.944                   |
| Adjusted R-squared         | 0.657             | 0.652             | 0.744                    | 0.756                    |

Regression results estimating the effect of beating a base-ten revenue threshold for the first time on future liquidity. For fiscal years 1999-2014. Controls untabulated for parsimony comprise: ROA, 12-Month Return, Equity Volatility, ln(MVE), Big4, Book-to-Market, Leverage, Firm Age, Merger. Specifications using industry and year fixed effects omitted for parsimony. Robust t-statistics clustered by firm in parentheses. ***,**, * Significant at the 0.01, 0.05, 0.1 level for two-tailed test.
Panel D. Analyst Following After Beating a Base-Ten Revenue Threshold for the First Time

| VARIABLES                | Analyst Following $t+1$ | Analyst Following $t+1$ | Analyst Following $t+2$ | Analyst Following $t+2$ |
|--------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| **Threshold**$_t$        | 0.292*** (4.655)        | 0.0215 (0.473)          | 0.381*** (5.681)        | 0.0941** (1.959)        |
| **First-Ever**$_t$       | 0.457*** (4.803)        | 0.0703 (1.021)          | 0.404*** (3.950)        | 0.0564 (0.797)          |
| **ln(Revenue)**$_t$      | -0.0924 (-1.542)        | 0.316*** (3.916)        | -0.106 (1.592)          | 0.0991 (1.142)          |
| **Revenue Growth**$_t$   | 0.219*** (4.373)        | 0.0623 (1.302)          | 0.212*** (3.753)        | 0.0405 (0.833)          |
| **Beat Last Year's Rev**$_t$ | 0.0254 (0.356)         | 0.0661 (1.269)          | 0.0676 (0.868)          | 0.108* (1.294)          |
| **Beat Analysts Rev**$_t$ | 0.326*** (6.606)       | 0.127*** (3.766)        | 0.299*** (5.708)        | 0.0527 (1.520)          |
| **Loss**$_t$             | 0.249*** (2.933)        | -0.291*** (-4.601)      | 0.301*** (3.202)        | -0.240*** (-3.596)      |
| **Beat Last Year's Inc**$_t$ | 0.290*** (6.261)      | 0.162*** (4.668)        | 0.355*** (7.014)        | 0.197*** (5.356)        |
| **Beat Analysts Inc**$_t$ | 0.658*** (11.79)       | 0.169*** (4.806)        | 0.619*** (10.07)        | 0.0682* (1.777)         |

Controls: Y, Y, Y, Y
Industry & Year FE: Y, N, Y, N
Firm & Year FE: N, Y, N, Y
Observations: 45,783, 45,783, 42,078, 42,078
Adjusted R-squared: 0.540, 0.830, 0.523, 0.830

Regression results estimating the effect of beating a base-ten revenue threshold for the first time on future analyst following. For fiscal years 1999-2014. 3-digit SIC code industry. Controls untabulated for parsimony comprise: ROA, 12-Month Return, Equity Volatility, ln(MVE), Big4, Book-to-Market, Leverage, Firm Age, Merger. Robust t-statistics clustered by firm in parentheses. ***, **, * Significant at the 0.01, 0.05, 0.1 level for two-tailed test.
Table 7. Revenue Threshold Effect for High Expected Revenue Growth Firms

For all Thresholds of the form $T = N \times 10^K$, for integers $N$ (1 through 9) and $K$ (5 through 10)

Panel A: Firms with High Compared to Low Past Revenue Growth

| Bin Width (%) | High Growth Firms ABOVE/BElOW (1) | Low Growth Firms ABOVE/BElOW (2) | Ratio of Ratios (1)/(2) | p-value |
|---------------|-----------------------------------|----------------------------------|-------------------------|---------|
| 0.25%         | 2.010                             | 1.679                            | 1.197                   | 0.0124**|
| 0.50%         | 1.563                             | 1.446                            | 1.081                   | 0.0760* |
| 1.00%         | 1.262                             | 1.223                            | 1.032                   | 0.1978  |
| 2.50%         | 1.147                             | 1.079                            | 1.063                   | 0.0187**|

Total Observations: 29,068

Panel B: Firms with a High Compared to Low Price-to-Sales Ratio

| Bin Width (%) | High Growth Firms ABOVE/BElOW (1) | Low Growth Firms ABOVE/BElOW (2) | Ratio of Ratios (1)/(2) | p-value |
|---------------|-----------------------------------|----------------------------------|-------------------------|---------|
| 0.25%         | 2.188                             | 1.267                            | 1.727                   | 0.0735* |
| 0.50%         | 1.564                             | 1.156                            | 1.353                   | 0.0947* |
| 1.00%         | 1.248                             | 1.043                            | 1.196                   | 0.0838* |
| 2.50%         | 1.172                             | 1.051                            | 1.115                   | 0.0312**|

Total Observations: 26,358

The results of one-tailed approximate randomization tests estimating the significance of the difference in the Threshold Effect for high and low past revenue growth firms and price-to-sales ratios, where high (low) growth (P/S ratio) is defined as the top (bottom) percentage revenue growth (P/S ratio) decile. Rows present the ratio of the number of observations in the bins just above and below threshold points for each comparison group, as well as the ratio of these ratios (the test statistic used to generate the approximate randomization p-value). A test statistic significantly greater than 1 is evidence that the threshold effect in revenue is stronger for firms with greater incentives to meet revenue targets. ***, **, * Significant at the 0.01, 0.05, 0.1 level for one-tailed test.
### Table 8. Discretionary Revenue for First Time Base-Ten Threshold Beaters

**Dependent Variable: Discretionary Rev**

| VARIABLES                  | (1)          | (2)          | (3)          | (4)          | (5)          | (6)          |
|----------------------------|--------------|--------------|--------------|--------------|--------------|--------------|
| **Threshold:**             | 0.000864***  | 0.000317     | -3.67e-05    | -0.000228    | -0.000285    | -0.000103    |
|                           | (2.202)      | (0.712)      | (-0.0835)    | (-0.517)     | (-0.639)     | (-0.228)     |
| **Threshold x Split**      |              |              | 0.00402***   | 0.00399***   | 0.00392***   | 0.00266***   |
|                           |              |              | (2.424)      | (3.017)      | (2.853)      | (2.342)      |
| **Split:**                 |              |              | 4.57e-05     | -0.00105     | -0.00114     | -0.000961    |
|                           |              |              | (0.0359)     | (-0.743)     | (-0.801)     | (-1.495)     |
| **First-Ever:**            | 1.99e-05     | -7.57e-06    | 3.07e-05     | -0.000122    | -7.65e-05    | -6.18e-05    |
|                           | (0.0433)     | (-0.0131)    | (0.0531)     | (-0.210)     | (-0.132)     | (-0.107)     |
| **ln(Revenue):**           | -0.00104***  | -0.00152***  | -0.00146***  | -0.00166***  | -0.00215***  | -0.00157***  |
|                           | (-4.525)     | (-2.799)     | (-2.695)     | (-3.054)     | (-2.942)     | (-2.892)     |
| **Revenue Growth:**        | -0.00123***  | -0.00167***  | -0.00193***  | -0.00186***  | -0.00157*    | -0.00184***  |
|                           | (-2.618)     | (-2.545)     | (-2.870)     | (-2.785)     | (-1.959)     | (-2.770)     |
| **Beat Last Year's Rev:** | 0.000101     | -0.000422    | -0.000437    | -0.000432    | -0.000639    | -0.000501    |
|                           | (0.198)      | (-0.725)     | (-0.751)     | (-0.743)     | (-1.086)     | (-0.861)     |
| **Beat Analysts Rev:**    | 0.000545*    | 0.000727**   | 0.000739**   | 0.000744**   | 0.000629*    | 0.000744**   |
|                           | (1.775)      | (2.079)      | (2.117)      | (2.131)      | (1.792)      | (2.130)      |
| **Beat Last Year's Inc:** | 9.48e-06     | 0.000283     | 0.000277     | 0.000323     | 0.000347     | 0.000358     |
|                           | (0.0290)     | (0.768)      | (0.754)      | (0.878)      | (0.928)      | (0.973)      |
| **Beat Analysts Inc:**    | -0.000416    | 7.87e-05     | 8.78e-05     | 9.62e-05     | 0.000125     | 8.03e-05     |
|                           | (-1.279)     | (0.208)      | (0.232)      | (0.254)      | (0.326)      | (0.212)      |
| **Controls**               | Y            | Y            | Y            | Y            | Y            | Y            |
| **Industry & Year FE**     | Y            | N            | N            | N            | N            | N            |
| **Firm & Year FE**         | N            | Y            | Y            | Y            | Y            | Y            |
| **Observations**           | 39,971       | 39,971       | 39,971       | 39,971       | 39,039       | 39,971       |
| **Adjusted R-squared**     | 0.007        | 0.093        | 0.094        | 0.094        | 0.100        | 0.093        |

Regression results estimating the magnitude of discretionary revenues of firms beating a base-ten revenue threshold for the first time. **Split** refers to High Growth firms (top decile lagged sales growth), Small firms (revenues < $100 million), and Loss firms. For fiscal years 1999-2014. 3-digit SIC code industry. Controls untabulated for parsimony comprise: ROA, 12-Month Return, Equity Volatility, ln(MVE), Big4, Book-to-Market, Leverage, Firm Age, Merger, and the main effects of the **Split** variables. Robust t-statistics clustered by firm in parentheses. ***,**, ** Significant at the 0.01, 0.05, 0.1 level for two-tailed test.

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