A Tough Act to Follow:
Contrast Effects in Financial Markets*

Samuel M. Hartzmark
University of Chicago
Booth School of Business

Kelly Shue
University of Chicago and NBER
Booth School of Business

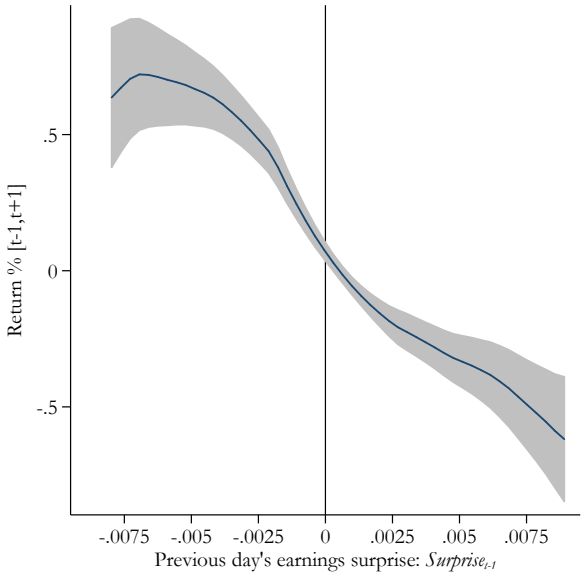
March 21, 2016

Abstract

A contrast effect occurs when the value of a previously-observed signal inversely biases perception of the next signal. We present the first evidence that contrast effects can distort prices in sophisticated and liquid markets. Investors mistakenly perceive earnings news today as more impressive if yesterday's earnings surprise was bad and less impressive if yesterday's surprise was good. A unique advantage of our financial setting is that we can identify contrast effects as an error in perceptions rather than expectations. Finally, we show that our results cannot be explained by a key alternative explanation involving information transmission from previous earnings announcements.

*We are grateful to Ross Garon at Cubist Systematic Solutions for helpful suggestions. We thank Menaka Hampole and Michael Watson for excellent research assistance. We thank Justin Birru, James Choi, Diego Garcia, Nicola Gennaioli, Owen Lamont, Bill Mayew, Josh Schwartzstein, Andrei Shleifer, Doug Skinner, David Solomon, and Boris Vallee as well as seminar participants at Chicago Booth, Colorado Finance Summit, Cubist Systematic Solutions, FIRN, Frontiers in Finance Conference, Gerzensee ESSFM, Harvard Economics, IDC, NBER Behavioral, UBC Winter Finance Conference, and MIT Sloan JFFC for comments.

Figure 1



The value-weighted earnings surprise of large firms that announced earnings in the previous trading day versus the return of firms that announced earnings today (conditional on own earnings surprise).

> ***Socrates:*** *Could you tell me what the beautiful is?*
> ***Hippias:*** *For be assured Socrates, if I must speak the truth, a beautiful maiden is beautiful.*
> ***Socrates:*** *The wisest of men, if compared with a god, will appear a monkey, both in wisdom and in beauty and in everything else. Shall we agree, Hippias, that the most beautiful maiden is ugly if compared with the gods?*
>
> -Plato

People often interpret information by contrasting it with what was recently observed. For example, Pepitone and DiNubile (1976) show that subjects judge crimes to be less severe following exposure to narratives of very egregious crimes. Kenrick and Gutierres (1980) show that male students rate female students to be less attractive after viewing videos of beautiful actresses. References to such "contrast effects" are also pervasive in our popular culture. People complain about having "a tough act to follow" when they are scheduled to perform following a great performance. Writers use literary foils to exaggerate a character's traits through juxtaposition with a contrasting character. Fashion designers use shoulder pads and peplum hips to create the illusion of a comparatively smaller waist. In all of these cases, contrast effects bias our perception of information. We perceive signals as higher or lower than their true values depending on what else was recently observed.

Contrast effects have the potential to bias a wide variety of important real-world decisions. They

may distort judicial perceptions of the severity of crimes, leading to unfair sentencing. At firms, comparisons with the previously reviewed candidate could lead to mistakes in hiring and promotion decisions. An unconstrained firm may pass on a positive NPV project because it does not look as good as other options or invest in a negative NPV project because it looks better than even worse alternatives. Finally, at the household level, contrast effects could cloud key decisions such as mate choice and housing search.

In these examples, contrast effects potentially lead to costly mistakes, but it may be difficult for researchers to cleanly measure the bias. Measurement is complicated by the possibility that the decision-makers face unobserved quotas or resource constraints that make comparisons across multiple cases optimal. In addition, researchers often lack precise data on how decision-makers perceive information. Possibly because of these challenges, most of the existing research about contrast effects has focused on controlled laboratory experiments. Evidence from the field is more limited. Outside of the lab, Bhargava and Fisman (2014) show contrast effects in mate choice using a speed dating field experiment and Simonsohn and Loewenstein (2006) and Simonsohn (2006) show contrast effects in consumer housing and commuting choices.

Our paper tests whether contrast effects operate in another important real world setting: financial markets. The financial setting is particularly interesting because we can test whether contrast effects distort equilibrium prices and capital allocation in sophisticated markets. Full-time professionals making repeated investment decisions may be less prone to such a bias than individuals making infrequent dating or real estate decisions. Moreover, the limited field evidence examines contrast effects in household decision-making, but prices in financial markets are determined through interactions among many investors. Thus, cognitive biases among a subset of investors may not affect market prices given the disciplining presence of arbitrage. And yet, if contrast effects influence prices in financial markets, it would represent an important form of mispricing: prices react not only to the absolute content of news, but also to a bias induced by the relative content of news.

In this paper, we test whether contrast effects distort market reactions to firm earnings announcements. Quarterly earnings announcements represent the main recurring source of firm-specific news

released by publicly-traded US firms. Prior to the announcement, financial analysts and investors form expectations of what they believe earnings will be. Earnings surprises, i.e., the extent to which actual earnings exceed or fall short of those expectations, are associated with stock price movements because they represent new information that shifts expectations of firm prospects. Earnings announcements are typically scheduled weeks beforehand, so whether a given firm announces following positive or negative surprises by another firm is likely to be uncorrelated with the firm's fundamentals.

The theory of contrast effects predicts a *negative* relation between yesterday's surprise and the return reaction to today's earnings surprise, holding today's earnings surprise constant. The intuition is that today's news will seem slightly less impressive than it would otherwise if yesterday's earnings surprises were positive and slightly more impressive if yesterday's earnings surprises were disappointing. While an earnings surprise is a concrete number (e.g., earnings per share was \$0.14, beating analyst forecasts of \$0.10, translating to a positive surprise of \$0.04), there is significant subjectivity in translating a surprise into a return response. A positive surprise is good news, but *how much* the price goes up depends on the interpretation of what the surprise implies for the future of the firm. We test whether the perception of "how good" the good news is (or "how bad" the bad news is) is biased by contrast effects.

The downward sloping pattern in Figure 1 illustrates our main finding. The figure shows a local linear plot of returns surrounding a firm's earnings announcement relative to the value-weighted average earnings surprise announced by large firms in the previous trading day. The figure demonstrates a strong negative relation: controlling for today's earnings news, the return reaction to today's earnings announcement is inversely related to yesterday's earnings surprise. The effect is sizable – a change in yesterday's earnings surprise from the worst to the best decile corresponds to a 53 basis point lower return response to today's earnings announcement.

We find evidence of a simple directional effect whereby a high surprise yesterday makes *any* surprise today (even more positive surprises) look slightly worse than it would appear if yesterday's surprise had been lower. In other words, we find that the magnitude of the return distortion

depends strongly on yesterday's surprise and not significantly on the interaction between today's and yesterday's surprise. Visually, this manifests itself in the data as a vertical shift downward in the return response curve to the firm's own earnings surprise if yesterday's news was good and a shift upward if yesterday's was bad (see Figure 3).

We also find that yesterday's and today's surprises are uncorrelated after accounting for slower-moving monthly trends. This implies that a high surprise yesterday generally leads to lower return reactions for firms announcing today, even without conditioning on today's news. Further, the abnormal returns occur in response to a firm's own announcement, not the previous day when other firms announced their surprises. Thus, we are able to create a trading strategy in which we go long (short) firms scheduled to announce today if yesterday's surprise was low (high), yielding abnormal returns of 7-15% per year. The strategy only includes firms in the top quintile of size, which means that, unlike many anomalies, contrast effects can distort the returns of large firms.

We present three additional pieces of evidence in support of the contrast effects hypothesis. First, consistent with the speed dating evidence in Bhargava and Fisman (2014), we find that investors react more strongly to more recent observations. Returns for firms announcing today are negatively related to earnings surprises released by other firms on $t-1$, but are not significantly related to lagged earnings surprises on $t-2$ and $t-3$ or future earnings surprises on $t+1$ and $t+2$. This also shows that our results are due to the precise ordering of earnings announcements rather than slower-moving time trends. Second, we find similar contrast effects among earnings released sequentially within the same day. In particular, morning earnings surprises have a strong negative impact on the returns of firms that announce in the afternoon. Third, the return distortion reverses over the long run, consistent with contrast effects causing mispricing that is eventually corrected.

While our findings are consistent with the theory of contrast effects, one may be concerned that we are capturing information transmission from earlier earnings announcements. For concreteness, suppose that firm A announces a positive earnings surprise on day $t-1$ and firm B is scheduled to announce earnings on day t. Empirically, we find that B tends to experience low returns, conditional on its actual earnings surprise. Can information transmission explain this empirical pattern?

Most studies of information transmission focus on the case of positive correlation in news, in which good news for "bellwether" firms convey similar information for other firms (e.g., Anilowski et al., 2007 and Barth and So, 2014). Positive correlation in news, where A's positive surprise is good news for B, cannot account for our results because we examine B's *cumulative return* from $t-1$ to $t+1$ (starting at market close on $t-2$ before A announces). If there is positive correlation in news, A's positive surprise should predict positive cumulative returns for firm B, not the negative pattern we find in the data.

Thus, to account for our results, any information transmission explanation requires *negative correlation* in news where A's positive surprise is bad news for B (e.g., A competes with B for resources). In this case, B should experience negative returns on $t-1$ when A announces. We find no support for negative information transmission in the data. Empirically, A's earnings surprise has no predictive power for B's earnings surprise after we account for slower moving time trends at the month level. Further, the market does not behave as if news relevant to firm B is released on day $t-1$, as we find no relation between A's earnings surprise and B's return on day $t-1$.

One may still be concerned that the results are due to a negative correlation in news and a *delayed* reaction to information. For example, A's $t-1$ positive surprise may contain negative news for B, but the market does not react to this information until day t, when B is featured in the media as it announces its earnings. However, this type of delayed reaction is only a concern if A's earnings surprise contains news about B's prospects other than B's earnings. If A's announcement simply provided information for B's earnings, this predicts no relation between A's earnings surprise and B's cumulative return after controlling for B's actual earnings. Delayed reaction, and information transmission more generally, are inconsistent with two important features of the data. First, return reactions are distorted by salient surprises in $t-1$, but not by slightly earlier surprises in $t-2$ or $t-3$. If earlier announcements convey information, one would expect similar effects for these earlier salient surprises. Second, information transmission, delayed or not, should not lead to the long-run reversals observed in the data. These reversals suggest the correction of a short-term bias.

Altogether, we show that plausible variants of the information transmission story cannot explain

our results. The remaining information transmission story that we cannot rule out is the following: A's news is negatively-correlated with B's prospects (beyond B's earnings), and such information is only released on day $t-1$, but not by firms announcing on days $t-2$ or $t-3$. Further, A's $t-1$ announcement contains information for B, but the market does not react to this information until day t. On day t, there is a biased response to this information which reverses over time. While we cannot rule out such a story, we believe that the well-founded psychological motivation based on contrast effects offers the more parsimonious explanation of our findings.

Another potential concern is that firms may strategically advance or delay their earnings announcements or manipulate the earnings announcement itself (e.g., Sloan, 1996; DellaVigna and Pollet, 2009; and So, 2015). However, this manipulation will only bias our results if it occurs as a function of the earnings surprises released by other firms on day $t-1$. Firms publicly schedule when they will announce at least a week before they actually announce (Boulland and Dessaint, 2014). The earnings surprises of other firms are, by definition, difficult to predict because they measure surprises relative to expectations. Therefore, it is unlikely that firms can strategically schedule to follow a firm with more or less positive surprises. Further, manipulation of the earnings number itself is unlikely to occur within a single day in reaction to other firms' earnings surprises. We find similar results for a sample of firms that are unlikely to have manipulated their earnings announcement dates.

A final potential concern is that the return reaction represents compensation for the impact of $t-1$ earnings surprises on risk or trading frictions. Fixed firm-specific loadings on risk factors are unlikely to explain our results because we use characteristic-adjusted returns in our analysis. A risk-based explanation thus requires that a more negative earnings surprise yesterday increases day-specific trading frictions or betas on risk factors. We instead find that risk loadings, return volatility, volume, and other measures of liquidity do not vary by the earnings surprise in $t-1$.

An important advantage of our financial setting is that we are able to identify contrast effects as an error in perceptions rather than an error in expectations. The distinction is mainly in regard to *when* a decision maker makes a quality assessment. Under an expectational error such as the

gambler's fallacy, the agent holds mistaken beliefs about the quality of the next case before seeing the next case, whereas a perceptual error leads to a biased quality assessment only upon seeing the next case. As highlighted in Chen et al. (2015), these two biases can generate observationally equivalent sequences of decision outcomes, making it difficult to distinguish between perception and expectation errors in most field settings. Our financial settings has the advantage of offering continuously traded prices, allowing us to measure investor beliefs at each point in time. Since we find evidence of price distortions for the second firm to announce only after it announces its own earnings, our evidence is consistent with a perceptual bias rather than an expectational bias.

One of the main contributions of our paper is to further the understanding of how psychological biases found in the lab manifest in real-world settings (e.g., Levitt and List, 2007a,b). Our findings suggest that contrast effects persist in a market setting where prices are determined by interactions among many investors including potentially deep-pocketed arbitrageurs. Our findings also contribute to the literature on biased reactions to earnings announcements, which has shown that investors underreact to a firm's own earnings news (Ball and Brown, 1968; Bernard and Thomas, 1989,1990; and Ball and Bartov, 1996), predictable seasonal information (Chang et al., 2014), and information in the timing of announcements (DellaVigna and Pollet, 2009; So, 2015; Boulland and Dessaint, 2014). Relative to the existing research, we show how prices are affected by the announcements of *other* firms that announced recently.

Our evidence underscores how important decisions are distorted by comparisons to benchmarks that should be irrelevant. Thus, our research is related to a large theory literature on context-dependent choice and reference points (e.g., Kahneman and Tversky, 1979; Koszegi and Rabin, 2006, 2007; Kamenica, 2008; Cunningham, 2013; Bordalo et al., 2015; and Bushong et al., 2015).[1] Finally, our findings are related to research in behavioral finance examining investor behavior based on how positions performed since they were purchased (Shefrin and Statman, 1985; Odean, 1998),

[1]While closely related to this literature, contrast effects (as typically described in the psychology literature) refer to a simple directional phenomenon in which the value of the recently observed signal inversely affects perception of the next signal. Most descriptions of contrast effects do not require discontinuous or kinked responses around a reference point (as in prospect theory, with recent empirical applications in, e.g., Baker et al., 2012 and DellaVigna et al., 2014) or a choice framework to identify which reference points to use or where to allocate attention.

how exciting certain stocks are relative to others in the market (Barber and Odean, 2008), and how a position compares to the other holdings in an investor's portfolio (Hartzmark, 2015). Relative to this literature which focuses on the trading patterns of individual investors, we test how contrast effects in the perception of news affect equilibrium market prices for large cap stocks.

1 Data

1.1 Sources

We use the I/B/E/S detail history file for data on analyst forecasts as well as the value and dates of earnings announcements. The sample is restricted to earnings announced on calendar dates when the market is open. Day t refers to the date of the earnings announcement listed in the I/B/E/S file.[2] Day $t-1$ refers to the most recent date prior to t where the market was open. We examine the quarterly forecasts of earnings per share and merge this to information on daily stock returns from CRSP and firm-specific information from Compustat. Data on the market excess return, risk-free rate, SMB, HML, and UMD portfolios as well as size cutoffs come from the Kenneth French Data Library.

To account for standard risk-based return movements, we use characteristic-adjusted returns, i.e., raw returns in excess of the return of a portfolio of stocks with similar characteristics. We follow the procedure in Daniel et al. (1997) and sort stocks into NYSE quintiles based on size, book value of equity divided by market value of equity (calculated as in Fama and French, 1992), and momentum calculated using returns from $t-20$ to $t-252$ trading days (an analogue to a monthly momentum measure from months $m-2$ to $m-12$). We then match each stock's return to the portfolio of stocks that match these three quintiles of characteristics.

We introduce one modification to ensure that there is no mechanical relation between the returns of the characteristic-matched portfolio and $surprise_{t-1}$. We remove from the characteristic-matched portfolio a stock's own return and the return of firms included in the calculation of $surprise_{t-1}$.

[2] DellaVigna and Pollet (2009) highlight a potential concern regarding earnings announcement dates as reported in I/B/E/S. We address this in Section 8 and show that alternative date adjustments lead to similar results.

This ensures that the characteristic-adjusted return is not distorted by potential earnings-related drift in the return of stocks that announced on the previous day.[3] Our measure of return on day t is a stock's raw return on day t minus the day t return of this characteristic-matched portfolio. In the remainder of the paper, unless otherwise noted, we refer to these characteristic-adjusted returns as returns.

1.2 Measuring earnings surprise

A key variable in our analysis is the surprise for a given earnings announcement.[4] Broadly defined, earnings surprise is the difference between announced earnings and the expectations of investors prior to the announcement. We follow a commonly-used method in the accounting and finance literature and measure expectations using analyst forecasts prior to announcement. This measure is available for a long time-series and does not require us to take a stand on specific modeling assumptions (for example, assuming a random walk with drift as in Bernard, 1992). Analysts are professionals who are paid to forecast future earnings. While there is some debate about what their goal is and how unbiased they are (e.g., McNichols and O'Brien, 1997; Lin and McNichols, 1998; Hong and Kubik, 2003; Lim, 2001; and So, 2013), our tests only require that such a bias is not correlated with the surprises of other firms in the day before a firm announces earnings. Given that we only use forecasts made before the $t-1$ firm announces (forecasts from day $t-2$ or earlier), such a bias is unlikely to exist.

Similar to DellaVigna and Pollet (2009), we take each analyst's most recent forecast, thereby limiting the sample to only one forecast per analyst, and then take the median of this number within a certain time window for each firm's earnings announcement. In our base specification, we take

[3] We thank James Choi for suggesting this modification to the calculation of characteristic-adjusted returns. In Table 8 we show that using raw returns in excess of the market or standard characteristic-adjusted returns without this correction yields similar results.

[4] We follow the literature on earnings announcements in characterizing earnings news as the surprise relative to expectations. We focus on surprise rather than levels because whether a given level of earnings is good or bad news depends on the level relative to investor expectations. In addition, stock prices should reflect current information – the stock market return response to an earnings announcement represents the change in a firm's valuation which should depend on the difference in earnings relative to expectations. Moreover, the financial press typically reports earnings announcement news in terms of how much earnings beat or missed forecasts. Therefore, the earnings surprise is likely to be the measure of earnings news that is most salient to investors.

all analyst forecasts made between two and fifteen days prior to the announcement of earnings. We choose fifteen days to avoid stale information yet still retain a large sample of firms with analyst coverage. To show that these assumptions are not driving the results, we present variations of this measure in Section 8 utilizing longer windows of 30 and 45 days prior to announcement and also using the return reaction to the announcement as a measure of earnings surprise.

We follow DellaVigna and Pollet (2009) and scale the difference between the actual surprise and the median analyst forecast by the share price of the firm from three trading days prior to the announcement. Thus, our estimate of the earnings surprise for firm i on day t can be written as:

$$surprise_{it} = \frac{\left(actual\ earnings_{it} - median\ estimate_{i,[t-15,t-2]}\right)}{price_{i,t-3}}. \tag{1}$$

Scaling by price accounts for the fact that a given level of earnings surprise implies different magnitudes depending on the price per share. For example, a five cent surprise represents a bigger positive surprise if the stock price is valued at \$10/share than \$100/share. However, many media outlets report earnings surprises as the unscaled difference between actual earnings and analyst forecasts, and investors may pay attention to the unscaled surprise. In supplementary results, we find qualitatively similar results using the unscaled earnings surprise.

To test the contrast effects hypothesis, we need a measure of the surprise occurring on the previous day taking into account that multiple firms may have announced earnings. The ideal variable would focus on the earnings announcements in $t-1$ that were salient as this would be the comparison group in the minds of investors when they evaluate the current day's announced earnings. While we do not have an exact measure of the salient surprise in $t-1$, we utilize a number of proxies and focus most of our analysis on large firms. A firm's market capitalization is related to how much attention that firm receives. One measure we use is simply the surprise of the largest firm to announce on day $t-1$. A second measure, which we use as our baseline, is the value-weighted surprise, using each firm's market capitalization three days prior to the firm's announcement, among all large firms announcing on day $t-1$. We define large firms as those with market capitalization

(measured three days before the firm's announcement) above the NYSE 90th percentile of market capitalization in each month. Thus, our baseline measure of yesterday's salient surprise is:

$$surprise_{t-1} = \frac{\sum_{i=1}^{N} (mkt\,cap_{i,t-4} \times surprise_{i,t-1})}{\sum_{i=1}^{N} mkt\,cap_{i,t-4}} \tag{2}$$

To reduce the influence of outliers, we winsorize $surprise_{it}$ at the 1st and 99th percentile and take the weighted average to create $surprise_{t-1}$. After creating $surprise_{t-1}$, we again winsorize at the 1st and 99th percentiles. In addition, we present alternative formulations where we value-weight all firms that announced in $t-1$ or take the equal-weighted average among all large firms.

In later regression analysis, each observation represents an earnings announcement by firm i on day t. When we discuss $surprise_{t-1}$, we refer to the salient earnings surprise released by large firms on the previous trading day.

1.3 Summary statistics

Table 1 describes the data used in our baseline specification. Our sample begins in 1984 and ends in 2013. For our main analysis, we examine how the return reaction for a firm that announces earnings on day t relates to the salient earnings surprise of other firms released on day $t-1$, controlling for the firm's own earnings surprise. Thus, to be included in the sample, a firm must have at least one analyst forecast in our dataset between days $t-2$ and $t-15$ prior to the announcement. In addition, we require a non-missing measure of $surprise_{t-1}$, which means at least one firm above the 90th percentile of market-capitalization announced their earnings on day $t-1$ and at least one analyst forecasted earnings for this firm between days $t-16$ and $t-3$. After applying these filters and requiring the firm with an announcement on day t to have non-missing returns, we are left with 75,923 unique earnings announcements.

Examining the returns row, we see that days with an earnings announcement are associated with positive returns (in excess of the matched characteristic portfolio) of 17 basis points. This

is the earnings announcement premium described in Beaver (1968), Frazzini and Lamont (2007), and Barber et al. (2013). Table 1 also shows that the typical earnings surprise is approximately zero (a mean of -0.0003 and a median of 0.0002). The market cap row shows the mean market capitalization in our sample is roughly $7 billion, while the 25th percentile of market cap is $439 million, implying that we have many small firms in our sample. Our baseline analysis will focus on larger firms because we value-weight each observation. We find a similar pattern when examining analyst coverage (number of forecasts from $t-15$ to $t-2$). For many firms, we see only one analyst forecast and the median number of forecasts is two, while the mean number of forecasts is nearly four. Thus, a small number of firms are covered heavily by many analysts. The final row describes the number of firms used to construct $surprise_{t-1}$, that is firms above the 90th percentile that announced on the previous trading day. The median is 6 with a mean of 7.5, so in general multiple firms comprise the $surprise_{t-1}$ measure.

2 Results

2.1 Baseline results

In our baseline specifications, we test how the return response to a given earnings surprise is impacted by the earnings surprise announced by large firms on the previous trading day. A major determinant of the return response to any earnings announcement will, of course, be the earnings surprise that the firm actually announces. The theory of contrast effects predicts that, conditional on the firm's own surprise, the return response to a given earnings announcement will be inversely related to yesterday's salient earnings surprise. Thus, our baseline specification allows for a direct impact of earnings surprise, contrast effects, and controls for time effects as follows:

$$return_{i,[t-1,t+1]} = \beta_0 + \beta_1 \cdot surprise_{t-1} + own\ surprise\ bin + \delta_{ym} + \varepsilon_{it} \tag{3}$$

The dependent variable is firm i's three-day characteristic-adjusted return from $t-1$ to $t+1$. In later sections, we discuss why including $t-1$ in our return window helps to rule out an alternative

explanation involving information transmission of positively-correlated news. This returns measure is regressed on $surprise_{t-1}$ as well as controls for firm i's own earnings surprise. We impose as little structure as possible on the price response to the firm's own earnings surprise by creating twenty equally-sized bins based on the size of the earnings surprise with an additional indicator for a surprise of zero. By using dummy variables for each bin, we non-parametrically allow each magnitude of surprise to be associated with a different level of average return response. δ_{ym} represents year-month fixed effects. In all regressions, unless otherwise noted, we value-weight each observation using the firm's market capitalization three days prior to the firm's announcement, scaled by the average market capitalization in that year, in order to focus on the more economically meaningful firms.[5] We cluster the standard errors by date.

$Surprise_{t-1}$ is our measure of yesterday's salient earnings surprise and the coefficient β_1 is our main measure of contrast effects. The contrast effects hypothesis predicts that a high surprise yesterday makes any surprise today look slightly worse than it would appear if yesterday's surprise had been lower. Thus, contrast effects predict a negative coefficient on β_1.

Table 2 shows the estimates of β_1 and strongly supports the contrast effects hypothesis. For our first estimate of yesterday's salient surprise, we use the earnings surprise of the largest firm to announce in the previous day. To make sure this firm is salient, we include only observations where the firm is above the 90th percentile of the NYSE market capitalization cutoff. The coefficient is -0.617 and highly significant.

Examining only the largest firm is a coarse measure of the salient earnings surprise from the previous day if there were multiple large firms that announced. For example, if both Apple and Goldman Sachs announced earnings on the same day, both announcements may be salient events to a large number of investors. Column 3 of Table 2 measures $surprise_{t-1}$ using the equal-weighted mean of all firms that announced in the previous day and were large (above the 90th percentile of market capitalization). We estimate a significant β_1 of -1.075. Finally, Column 5 uses the value-weighted

[5] Average market capitalization has increased over time. To avoid mechanically overweighting recent observations, we scale market capitalization by the average in each year. In untabulated results, we find that omitting this scaling leads to materially similar results.

mean of the earnings surprise of all large firms that announced yesterday, leading to a significant β_1 of -0.945. This value-weighted measure implicitly assumes that the relative market cap of large firms that announced on $t-1$ is a good proxy for the relative salience of their announcements.

In the even-numbered columns of Table 2, we add year-month fixed effects and find that the estimates drop slightly in magnitude, but remain highly significant, suggesting that aggregate time trends cannot explain our results. In later tables, we use the value-weighted salient surprise with year-month fixed effects in Column 6 as our baseline specification.

Using the β_1 of -0.887 from Column 6, we estimate that an increase in yesterday's salient surprise from the average in the worst decile (-0.23%) to the average in the best decile (0.37%) is associated with lower returns of 53 basis points. For an alternative measure of magnitude, a one standard deviation increase in $surprise_{t-1}$ is associated with a decrease in returns of 15 basis points. To get a sense of magnitudes, we can compare this result to a robust anomaly in asset pricing: the earnings announcement premium (Frazzini and Lamont, 2007; Barber et al., 2013). In our sample, with no information other than the fact that earnings will be announced on a given day, an equal-weighted strategy going long stocks with earnings announcements earns abnormal returns in our sample of 17 basis points from $t-1$ to $t+1$. If we value-weight, as we do for our estimates of contrast effects, the earnings announcement premium is 8 basis points from $t-1$ to $t+1$. Thus, the impact of contrast effects is of a similar magnitude to, if not greater than, other well-known return anomalies related to earnings announcements.

Table 2 shows the regression analog to the local linear plot in Figure 1, which we discussed in the Introduction. The figure shows that contrast effects induce a negative relation between yesterday's salient surprise and the return reaction to today's earnings surprise.

2.1.1 Contrast effects and interaction effects

The contrast effects hypothesis predicts that the return response to a given earnings surprise today will be higher when yesterday's news was bad than when yesterday's news was good. In its simplest form, the magnitude of the bias depends on the value of yesterday's surprise, but not on whether

today's surprise is better or worse than yesterday's surprise. This simple directional effect can be seen in Figure 3, which shows how $surprise_{t-1}$ shifts the return reaction curve to the firm's own earnings surprise. The blue and red lines show the return response for firms that announce following a very positive (top decile) or negative (bottom decile) $surprise_{t-1}$, respectively. Unsurprisingly, for both groups, there is a strong positive relation between a firm's returns around announcement and the firm's own earnings surprise. More importantly, the figure shows that the blue line is consistently below the red line, demonstrating that the return response to a firm's own earnings surprise is shifted down significantly if yesterday's surprise was in the highest decile as compared to the lowest decile. The figure also shows that the magnitude of the contrast effect, i.e., the vertical distance between the two lines, does not vary substantially across the support of earnings surprises released today. In other words, good salient surprises yesterday makes all earnings surprises today (even more positive earnings surprises) look slightly less impressive than if they had followed bad salient surprises yesterday and the magnitude of this difference does not differ substantially based on the level of surprise released today.

We test directly for potential interaction effects in Table 3 by interacting $surprise_{t-1}$ with various measures of the firm's own earnings surprise: the raw level, 20 bins, and quintiles for the firm's own earnings surprise. We find that the magnitude of contrast effects may be slightly larger when the surprise today is more negative, but the interaction effects are all insignificantly different from zero. Further, we continue to find a strong negative direct relation between returns and the previous day's salient surprise, even after we allow for interaction effects. In other words, yesterday's salient surprise negatively impacts the return reaction to today's earnings announcement, and the extent of this distortion does not depend significantly on the level of today's earnings surprise. Therefore, we focus on the main direct effect, but do not claim to reject potential interaction effects which may be too noisy to estimate within our sample.

Overall, we find results strongly consistent with the main prediction of the contrast effects hypothesis. In the next three sections, we present additional evidence in support of contrast effects.

2.2 Lead and lag effects

Previous tests of contrast effects in laboratory or non-financial settings suggest that individuals react more strongly to more recent observations. For example, Bhargava and Fisman (2014) find that the appearance of the person whom you spoke with most recently has the largest impact on the current dating decision. If a similar type of contrast effect accounts for the pattern that we observe in Table 2, the effect should be strongest for salient surprises that occurred at day $t-1$, and weaker for those on days $t-2$ and $t-3$.

The first column of Table 4 Panel A examines this hypothesis by adding further lags of surprises on $t-2$ and $t-3$ to our base specification. To ensure that our return measure allows for a response to information covering the entire time period (see Section 3), we examine the return from $t-3$ to $t+1$ as the dependent variable.

We find a strong and significant negative relation between the previous day's salient surprise and the return response to firms announcing today. Meanwhile, we find little relation between returns and earlier surprises on $t-3$ and $t-2$. We can reject that the return reaction to $t-1$ surprises is equal to the reactions to $t-2$ or $t-3$ surprises with p-values below 0.1. This pattern is also inconsistent with most alternative explanations of the empirical results (explored in later sections) as they do not predict that the specific short-term ordering of past earnings announcements will impact returns. Thus, the results support the hypothesis that contrast effects are responsible for the strong negative coefficient found on $surprise_{t-1}$.

Next, we examine how return reactions to firms announcing today are affected by future surprises announced on days $t+1$ and $t+2$. We use returns from $t-1$ to $t+2$ as our dependent variable, to allow for the return reaction of a firm that announces on day t to respond to these future earnings announcements. Such a response may be less likely to occur as it requires that investors revise their initial perceptions of day t announcements in light of subsequent earnings announcements released in the following two days. In Column 2 of Table 4 Panel A, we find the relations between return responses and salient surprises on days $t+1$ and $t+2$ are small, vary in sign, and insignificant.

Almost any empirical exercise involves the worry that there is a mechanical relation due to specification choice. In addition to providing a test for the transitory nature of contrast effects, these results offer a placebo test for this concern. If the negative coefficient on $surprise_{t-1}$ is mechanically due to our specification, then the coefficients on $t-2$ or $t+1$ should be similarly biased. Given that we do not find such a relation, we feel confident that our empirical choices are not mechanically driving the result.

2.3 Same-day contrast effects

The analysis presented so far has examined contrast effects across consecutive trading days. We can also examine contrast effects within the same day. We present the following analysis as supplementary evidence to our baseline estimates because data on the within-day timing of earnings announcements is only available for announcements after 1995. Further, some firms do not preschedule the exact hour of announcement even though they do pre-commit to the exact date of announcement.

Nevertheless, we can explore whether the within-day data support the contrast effects hypothesis. We use the fact that firms generally announce earnings either slightly before market open or slightly after market close. We expect the earnings surprises of large firms that announce in the morning to have an inverse impact on the return response for firms that announce later in the afternoon. Earnings surprises of large firms that announce in the afternoon could also have an inverse impact on the return response for firms that announce earlier in the morning. While our empirical specification will capture such an effect, it may be less likely to occur because it requires that investors revise their initial perceptions of morning earnings announcements in light of subsequent earnings announcements released in the afternoon.

To explore same-day contrast effects, we categorize firms as announcing before market open (prior to 9:30 am) or after market close (after 4:00pm).[6] We measure the salient earnings surprise as described previously, but with two changes. First, for each day t, we calculate two salient surprises: the surprise of large firms that announced before market open ($AM\ surprise_t$) and the

[6] We exclude firms announcing in the interim time period (roughly 8% of the value-weighted average of firms).

surprise of large firms that announced after market closure ($PM\ surprise_t$). Second, for our return measure, we examine returns from the close on $t-1$ to the close on $t+1$ as this window includes both the response to the AM or PM surprises as well as the response to the firm's own announcement.

We start by regressing the returns of firms that announce their earnings after market close on $AM\ surprise_t$, with the same controls described in Equation 3. Table 4 Column 3 shows a coefficient of -1.47 on the AM surprise variable. This same-day measure of contrast effects is slightly larger than the across-days measures estimated in earlier tables, consistent with more recent observations leading to larger contrast effects.

Next, we explore whether PM surprises have a negative impact on the return response for firms that announce earlier in the morning. Note, the return window (which extends to $t+1$), does not preclude such an effect as investors could revise their response to morning announcements due to new information released in the afternoon. In Column 4 of Table 4 Panel A, we find a negative but small and insignificant coefficient on the $PM\ surprise_t$. Thus, within the same day, investors exhibit behavior consistent with contrast effects, but only significantly with respect to previously observed salient surprises.

2.4 Long run reversals

If the return patterns represent mispricing due to the psychological bias of contrast effects, then the negative coefficient on $surprise_{t-1}$ should reverse over time if prices eventually converge to fundamental values. Table 4 Panel B examines returns subsequent to the earnings announcement and finds evidence consistent with mispricing reversing in the long run. All columns in the table estimate our baseline specification, using different return horizons as the dependent variable. The top row looks at the overall impact of $surprise_{t-1}$ on long run return windows, starting at $t-1$, while the bottom row looks just at return reversals, i.e., starting after the announcement at $t+2$. We find that the negative relation between $surprise_{t-1}$ and the return reaction to the firm's own announcement begins to weaken by 30 trading days after announcement and approaches zero by 40 trading days after announcement. By 50 trading days, the long run returns are nosily measured,

but there evidence of a significant reversal, as shown in the bottom row. This suggests that contrast effects leads to mispricing that is reversed after the earnings announcement.

3 Information transmission

While our empirical findings are consistent with the theory of contrast effects, a potential concern is that information transmission from earlier earnings announcement accounts for the empirical patterns. In this section, we present a series of tests to rule out a large set of information explanations.

We use a simple example to discuss the implications of various theories of information transmission. For this example, assume that firm A announces a positive earnings surprise on day $t-1$ and firm B is scheduled to announce earnings on day t. Our empirical evidence implies that following A's positive surprise, B is likely to experience low returns conditional on its actual earnings surprise. Can information transmission explain this empirical pattern?

Most studies of information transmission in firm news announcements focus on the case of positive correlation in news, in which A's positive surprise is good news for B (e.g., good news for B's earnings or future opportunities). For example, Anilowski et al. (2007) and Barth and So (2014) study "bellwether" firms whose news convey similar information for other firms.

We begin by showing that any information transmission story involving *positive correlation* in news cannot explain our results. If there is positive correlation in news, then A's positive surprise is good news for B, so B should experience positive returns on day $t-1$ when this good news is released. Then, B might experience lower returns on day t for a given level of earnings surprise (measured using analyst forecasts made prior to $t-1$) because its good news was released early, on day $t-1$. However, A's positive surprise should not negatively affect B's *cumulative return* from $t-1$ to $t+1$ (measured starting at market close in $t-2$, before A announces) which is what we use in our analysis. Positive correlation in news implies a positive correlation between $A's$ surprise and B's cumulative returns, not the negative relation we observe in the data.

Thus, for information transmission to explain our results, there must be *negative correlation* in

news, so A's positive surprise is bad news for B (e.g., A competes with B for resources). However, we show that negatively-correlated information transmission, or information transmission of any form, is very unlikely to account for our results for two reasons. First, $surprise_{t-1}$ does not negatively predict day t earnings surprises. Second, markets do not react as though negatively (or positively) correlated information is released on day $t-1$ through the salient surprises of other firms.

In Table 5 Panel A, we examine whether $surprise_{t-1}$ predicts the earnings surprises of firms scheduled to announce in the following day. Column 1 regresses the firm's own earnings surprise on day t (i.e., the surprise relative to analyst forecasts made on or before $t-2$) on $surprise_{t-1}$. We estimate a positive and significant relation, not a negative relation as would be required for information transmission to explain our results. Further, in Column 2, the correlation disappears after we control for year-month fixed effects. In other words, $t-1$ surprises do not predict day t surprises after accounting for month-level time trends. Columns 3 and 4 utilize bin measures of surprise (rather than the level measure used in the first two columns) to ensure the results in Columns 1 and 2 are not driven by outliers or the specific scaling. We again find no relation after accounting for monthly time variation. In general, any serial correlation in earnings surprises is positive or close to zero, not negative as is required for an information transmission story to explain our results.

A's earnings surprise does not predict B's earnings surprise, which means that if A's positive surprise contains negative news about B, it must contain negative news about B's prospects other than just B's earnings.[7] If markets are efficient, then B's stock price should decline on $t-1$ when this information is first released. In Panel B of Table 5, we test whether the market responds as if the salient surprise on day $t-1$ conveys information for the firm scheduled to release earnings on day t. In Columns 1 and 2 (with and without year-month fixed effects), we find no significant relation between $surprise_{t-1}$ and the $t-1$ returns of firms that announce the next day. Columns 3 and 4 examine open-to-open returns to make sure that we account for market reactions to earnings

[7] A secondary reason why A's positive surprise must contain negative news about B's prospects other than just B's earnings to match our results is that we directly control for B's earnings surprise relative to previous analyst forecasts in our baseline regressions. If A's surprise only revealed information about B's earnings surprise, we should estimate a zero coefficient on yesterday's salient surprise after controlling for B's actual earnings surprise.

released after market close on $t-1$. The results are materially unchanged. There is no evidence of either positively or negatively-correlated information transmission. The market does not behave as if there is information released by firm A that is relevant for firm B on day $t-1$.

We also check that these results are not due to aggregating a subsample where information is transmitted with other cases where no information is transmitted, which could add noise to the analysis. To check for this possibility, we look at cases where the market reacted as if no information was transmitted in $t-1$. In this subsample, we expect to find no evidence consistent with contrast effects if the results are actually driven by information transmission.

In Columns 1 and 2 of Table 5 Panel C, we examine only firms that announce on day t with a return of less than 1% in absolute magnitude on day $t-1$. Within this subsample, in which the market response suggests close-to-zero information is transmitted on $t-1$, we continue to find a significant negative relation between cumulative returns around announcement and the $t-1$ salient surprise. We estimate a coefficient of -0.915 for $surprise_{t-1}$, which is very close to the -0.887 we find when examining the entire sample. Column 2 repeats the analysis for firms where the return reaction on $t-1$ was even smaller, less than 0.5% in absolute value, and finds a similar pattern. Finally, in Column 3, we restrict the sample to observations for which no *negatively-correlated* information was transmitted on $t-1$ (i.e., we exclude negative return reactions to positive $surprise_{t-1}$ and positive return reactions to negative $surprise_{t-1}$). We focus on negatively-correlated information transmission because positively-correlated information predicts the opposite empirical pattern observed in the data. We again find similar results. Altogether, we show that limiting the sample to observations where the market reacts as if no information, or no negatively-correlated information, was released on day $t-1$ yields similar results to the rest of the sample. This suggests that we are capturing contrast effects rather than information transmission with our empirical tests.

At this point, one may ask whether *delayed* reaction to information transmission could explain the empirical results. A's $t-1$ positive earnings surprise may contain negative news for B, but the market does not react to this information until t. Fully rational investors should not react with a delay because they should be forward looking. If they expect A's good news to be bad news for B in

expectation, they should react when the information is released on day $t-1$.[8] However, boundedly rational investors may react to A's information about B with a delay because investors do not think about firm B until day t when B becomes more salient due to news coverage surrounding its earnings announcement. Note that this type of delayed reaction is only a concern if A's $t-1$ news is negatively-correlated with news for B (positive correlation would predict the opposite relation to what we observe in the data). In addition, delayed information transmission is only a concern if A's news contains news about B's prospects other than B's earnings (if A's earnings news simply provided information for what B's earnings surprise will be at t, this predicts no relation between A's earnings surprise and B's cumulative return after controlling for B's actual earnings surprise).

Delayed reaction and information transmission more generally are inconsistent with two important features of the data. First, we find that return reactions to $t-1$ surprises are significantly stronger than the close-to-zero return reactions to $t-2$ or $t-3$ surprises. If previous announcements convey information, one would expect similar effects for these earlier surprises. Second, any information transmission, delayed or not, should not lead to the long-run reversals which we observe in the data.

Altogether, we show that most plausible variants of the information transmission story cannot explain our results. While it is impossible to rule out all information explanations, what remains is a very specific and complex information transmission story which must contain the following elements:

1. $A's$ $t-1$ positive surprise contains negative information for B (contrary to the empirical evidence showing that earnings surprises are positively serially correlated without accounting for time trends and not correlated after accounting for time trends).

2. The negative information relates to B's prospects other than just B's earnings.

[8] This logic holds even if the interpretation of A's news for B's prospects depends on the level of B's earnings surprise. For example, A's good news may be bad news for B, but only if B's own earnings surprise is high. If investors are rational, they should realize that the average expected impact of A's positive news implies negative returns for B and react on day $t-1$. We also showed earlier in Table 3 that such interaction effects do not appear to be significant in the data, although we do not claim to reject them entirely. Regardless, rational investors should not wait to react if there is an average expected negative relation, as is the case in the data.

3. Rational investors should not wait until day t to react to information released on day $t-1$. Nevertheless, the market does not react to this information until day t.

4. When the market does react to this information on day t, it reacts in a biased manner, leading to a long run reversal.

5. The relevant information for firm B is only contained in $t-1$ salient surprises, but not in earlier salient surprises released on $t-2$ or $t-3$.

While this complex information transmission explanation is impossible to reject, we feel that the contrast effects hypothesis offers a more parsimonious explanation of the empirical results that is based on a well-known and intuitive psychological phenomenon.

4 Expectations vs. perceptions

A unique advantage of our financial setting is that we can identify contrast effects as an error in perceptions rather than an error in expectations. An expectational error occurs when exposure to a previous case biases expectations about the quality of the next case. For example, a gambler's fallacy is an expectational error in which, after seeing a high quality case, a judge mistakenly believes that the next case is more likely to be low quality, and this prior belief clouds the ultimate decision on the next case (Chen et al., 2015; Rabin and Vayanos, 2010). A perceptual error, such as a contrast effect, occurs if after viewing a high quality case, the judge examines the characteristics of the next case and perceives the case as lower in quality. The distinction is mainly in regard to *when* the judge makes a biased quality assessment. Under an expectational error, the judge holds mistaken beliefs about the quality of the next case before seeing the next case, whereas a perceptual error leads to a biased quality assessment only after seeing the next case. As highlighted in Chen et al. (2015), these two biases can generate observationally equivalent sequences of decision outcomes, making it difficult to distinguish between perception and expectation errors in most field settings.[9]

[9] Rabin and Vayanos (2010) develops a model of the gambler's fallacy in which expectations depend on the continuous value of the previously-observed signal, leading to decisions that are inversely related to the quality of the

Our financial setting allows us to distinguish between expectational and perceptual biases because it offers continuously traded prices. At each point in time, prices reflect current market beliefs about each firm. To see how continuously traded prices allows us to distinguish these two classes of biases, return to our example in which firm A announces a positive earnings surprise on $t-1$ and firm B announces on t. If A's announcement changes expectations about B's announcements or value, we should see B's price change on $t-1$. If these beliefs are biased, we would see a partial or full reversal on day t when B's information is revealed. If A's announcement biases perceptions of B's announcements without changing expectations, B's price will not move on $t-1$, but will move in a biased manner on day t. Since we find evidence of price distortions only after B has announced (see Table 5 Panel B), our evidence is consistent with a perceptual bias rather than an expectational bias.

Our focus on a perceptual bias also offers a novel contribution to the behavioral finance literature, which has largely focused on expectational biases. For example, in the context of earnings, Thomas and Zhang (2008) shows an expectational bias in which the market overreacts to industry-specific news released early in an earnings season. This expectational error is corrected when a firm announces its actual earnings later in the same season. More broadly, investors form biased expectations by overextrapolating from past information (Greenwood and Shleifer, 2014; Barberis et al., 2015). These are expectational errors as they manifest themselves upon receiving signals about a future outcome (e.g., a mistaken belief that past positive returns forecast positive future returns) rather than when the future outcome is observed (e.g., a firm's earnings announcement).[10]

previously case. Chen et al. (2015) discuss why Rabin and Vayanos (2010) implies that conditioning on continuous quality measures of the previously case is insufficient to distinguish between contrast effects and the gambler's fallacy.

[10]Other research has shown how price responses depend on mood, sentiment, or weather (e.g., Mian and Sankaraguruswamy, 2012; Gulen and Hwang, 2012; Hirshleifer and Shumway, 2003). These return patterns may represent errors in expectations or perceptions. However, the settings usually lack the specific timing necessary to disentangle the two types of errors.

5 Contrast effects without conditioning on today's surprise

So far, we have shown that the return response to a given earnings announcement is inversely related to yesterday's salient earnings surprise, *conditional* on the level of surprise today. We showed in Section 3 that the earnings surprise of the firm announcing on day t is not correlated with the earnings surprises of other firms released in the previous day, after controlling for slower moving time trends. Therefore, we should continue to find a negative relation between the return response to a given earnings announcement and yesterday's salient earnings surprise, *unconditional* on the firm's own surprise today. Omitting the firm's own earnings surprise as a control variable should lead to more noise in our regression fit, but should not systematically bias the coefficient on $surprise_{t-1}$.

Table 6 Panel A presents results without controlling for a firm's own earnings surprise. We continue to find a robust negative coefficient on yesterday's salient surprise, although the R^2 declines as expected. Column 1 examines the impact without year-month fixed effects and finds a coefficient of -0.590 while Column 2 adds the fixed effects and finds a coefficient of -0.926. The estimates are not statistically different than the results where we controlled for the firm's own earnings surprise in Table 2 Columns 5 and 6. Figure 2 Panel B shows the graphical analogue of these tests using a local linear regression. Similar to the pattern in Panel A, we see a strong negative relation between $surprise_{t-1}$ and the return response to the earnings announced on day t.

The unconditional analysis also shows that our results cannot be explained by a potential bias caused by controlling for the firm's own earning surprise. In our earlier conditional analysis, we estimated $return_{i,[t-1,t+1]} = \beta_0 + \beta_1 \cdot surprise_{t-1} + own\,surprise\,bin + \delta_{ym} + \varepsilon_{it}$. If high $surprise_{t-1}$ indicates that market conditions are generally good, then any level of earnings released by the next firm may represent less of an accomplishment by the firm's manager, because he/she benefited from luck. If prices partly reflect inferences about managerial skill, then returns will be slightly lower for any particular level of the firm's own earnings. In the conditional regression, the return response would be below the average captured by *own surprise bin*, leading to downward bias on β_1. However, this bias cannot explain the unconditional negative relation between $surprise_{t-1}$ and

return reactions to earnings scheduled to be announced on the following day. If high $surprise_{t-1}$ is positive news overall for the next firm to announce, the unconditional relation between $surprise_{t-1}$ and the cumulative $[t-1, t+1]$ return for the next firm to announce should be positive.

5.1 Trading strategy

One important implication of not conditioning on a firm's own earnings surprise is that we can predict day t and future returns using information available on day $t-1$. Thus, it would be possible to trade based on the previous day's salient earnings surprise and earn predictably higher or lower returns for firms that release earnings the next day. To accurately measure return responses without any look-ahead bias, we modify our regression specification slightly. First, we exclude year-month fixed effects because they are estimated using future days within the same month. Second, we change our return window from $[t-1, t+1]$ to $[t, t+1]$ so it does not include returns on $t-1$. Many firms announce earnings immediately after market close, so to examine portfolios that can be constructed using ex-ante information, we move from close-to-close returns (the conventional measure in the finance literature) to open-to-open returns.

Table 6 Panel A shows that our results are similar using these adjustments. The odd-numbered columns exclude year-month fixed effects. Column 3 and 4 examine open-to-open returns from $t-1$ to $t+1$ while Columns 5 and 6 limit the return period from t to $t+1$. We estimate coefficients of -0.829 without year-month fixed effects and -1.112 with year-month fixed effects, both highly significant.

This finding is also shown in graphical form in Figure 4. The red line represents the value-weighted average cumulative characteristic-adjusted returns of a simple strategy that buys firms announcing earnings today if the salient surprise in $t-1$ was below the 25th percentile of $surprise_{t-1}$ in the previous quarter. The blue line represents the cumulative returns of a strategy that buys firms announcing earnings today if the salient surprise in $t-1$ was above the 75th percentile of $surprise_{t-1}$ in the previous quarter. We find that the red line lies above the blue, indicating that the average return after announcement is significantly higher when $surprise_{t-1}$ was in the lowest

quartile than when $surprise_{t-1}$ was in the highest quartile.[11]

As a final robustness check, we examine whether it is possible to construct a calendar-time trading strategy based on contrast effects that generates abnormal returns. The purpose of this analysis is not to find the maximum alpha attainable to traders, but rather to show the robustness of our results to a different specification. Calendar time asset pricing offers a different risk adjustment than the characteristic-adjusted returns used elsewhere in the paper. In addition, the trading strategy uses daily diversified value-weighted portfolios that more closely resemble what investors might hold. The strategy equal-weights trading days (and value-weights multiple earnings announcements within the same day) while the baseline regressions value-weight each earnings announcement.

The trading strategy is a daily long-short strategy. On days where the salient surprise at $t-1$ is below a certain cutoff, we buy firms scheduled to announce on day t and short the market, holding this portfolio for days t and $t+1$. On days where the salient surprise at $t-1$ is above a certain cutoff, we go long the market and short firms scheduled to announce on day t. Again, we hold this portfolio on days t and $t+1$. Each daily portfolio is value-weighted based upon the market capitalization at $t-3$ of the firms announcing earnings on each day. Following the asset pricing literature, which assumes that investors will only invest if they are able to diversify their holdings across several firms, we require at least five stocks to announce per day for the strategy to be active in Columns 1 and 2. We relax this restriction in Columns 3 and 4. We focus our trading strategy on large firms in the top quintile of the market that account for our findings (see Table 9). We utilize Fama-French regressions in which portfolio returns are regressed on the market, size, book to market and momentum factors.

Table 6 Panel B presents the results. First, we examine the trading strategy utilizing the cutoff of zero: if $surprise_{t-1}$ is not positive, we go long firms that announce earnings on day t and short the market. If $surprise_{t-1}$ is positive, we short announcers on day t and go long the market. We find a significant daily alpha of 10 basis points. Next, we go long when $surprise_{t-1}$ is below the

[11] As discussed earlier, we usually observe positive returns on earnings announcement days. This is the earnings announcement anomaly, as shown in Frazzini and Lamont (2007) and Barber et al. (2013). The magnitude of the coefficients in Table 6 and the fact that the blue line in Figure 4 is not significantly positive shows that contrast effects are strong enough to counteract the impact of the earnings announcement premium.

25th percentile (relative to the distribution of $surprise_{t-1}$ in the previous quarter) and short if $surprise_{t-1}$ is above the 75th percentile. With these more extreme cutoffs, we expect contrast effects to be more pronounced. Consistent with this, we see an alpha of 22 basis points with a t-statistic greater than 3. In Columns 3 and 4, we allow portfolios with fewer than five stocks per day, which increases the exposure to idiosyncratic risk, but increases the number of days in which the strategy can be implemented. We see a similar pattern with slightly lower alphas for both choices of cutoffs.

We can compound these daily alphas to estimate the annual alpha of a contrast effects trading strategy (shown in the bottom row of the table). If the trading strategy could be implemented every trading day (which is not the case), 15 basis points per day would yield an annual abnormal return of roughly 45%. However, earnings announcements cluster at the end of each quarter and not all trading days contain earnings announcements. Further, the strategy can only be implemented if there is a non-missing salient surprise in the relevant cut-off categories in the previous trading day. For example, in the first column which assumes that investors only trade when they are able to diversify across five or more stocks, we can implement the strategy an average of 64 trading days per year (roughly 25% of total trading days) which yields an abnormal annual return of 6.5%. The slightly lower alphas from the last two columns of Table 6 Panel B can be earned on more trading days per year, leading to higher annual abnormal returns of between 10% to 15%.

6 Strategic timing of earnings announcements

Previous research has shown that firms may advance or delay earnings announcements relative to the schedule used in the previous year or manipulate the earnings announcement itself (e.g., through adjustment of discretionary accruals). However, these types of strategic manipulation will only bias our results *if they alter firm earnings announcements as a function of the earnings surprises released by other firms on day* $t-1$. Such short-run manipulation within a single trading day is unlikely. Firms typically publicly schedule when they will announce their earnings more than a week before

they actually announce (Boulland and Dessaint, 2014). The earnings surprises of other firms are, by definition, difficult to predict because they measure surprises relative to expectations. Therefore, it is unlikely that firms can strategically schedule to follow other firms with more or less positive surprises. Further, manipulation of the earnings number itself takes time and is unlikely to occur within a single day as a reaction to the earnings surprises made by other firms on day $t-1$.

To directly test strategic timing, we separately examine earnings announcements that moved or stayed the same relative to the calendar date of the announcement for the same quarter the previous year. Firms typically report their earnings on roughly the same day every year, with small changes, e.g., to announce on the same day of the week (So, 2015). Thus, in order for strategic timing to explain our results, it must be the firms that deviate from their normal earnings announcement date that drive our results. We categorize firms as having moved their earnings date forward or backwards if it differs from their previous same-quarter date by five or more days. Roughly 80% of firms keep the date the same, 10% move it forward by more than 5 days and 10% move it backwards.

We examine these sets of firms in Table 7 Panel A and find that strategic timing cannot account for the negative relation between return reactions and salient surprises at $t-1$. Firms that did not greatly move their announcement date have a large negative coefficient of -0.896 that is statistically significant at the 1% level. Firms that moved their announcements forward or backwards have insignificant estimates of contrast effects with large standard errors. Under the strategic timing hypothesis, we should have found that firms that shifted their earnings announcement data accounted for the negative relation.

7 Risk and trading frictions

Another possible concern is that the return reaction represents compensation for the impact of $t-1$ earnings surprises on risk or trading frictions. Stable firm-specific loadings on risk factors are unlikely to explain our results because we use characteristic-adjusted returns in our analysis. A risk-based explanation thus requires that a more negative earnings surprise yesterday increases

day-specific trading frictions or betas on risk factors, leading investors to demand a higher return as compensation.

Table 7 Panel B tests for such a channel. We modify our base specification so the return is regressed on four factors (market excess return, SMB, HML, and momentum) along with interactions of those factors with $surprise_{t-1}$. If a firm's covariation with market factors is systematically larger when there are more negative surprises on the previous day, we would expect to see large negative coefficients for the interaction terms. Examining characteristic-adjusted returns in Column 1 and raw returns in Column 2, we find no support for this hypothesis. Two coefficients are significant at the 10% level, but they are positive. None of the coefficients are significantly negative. Thus, fixed or time-varying loadings on standard risk factors are unlikely to account for our results.

Another possible concern is that our findings are due to a liquidity premium. For a liquidity premium to explain our results, it must be that a more negative salient surprise yesterday predicts lower liquidity for firms announcing today, so that the higher return is compensation for the lower liquidity. In Table 7 Panel B, we show that yesterday's salient surprise is not correlated with today's volume or bid-ask spread, two proxies for liquidity.[12]

An alternative version of liquidity relates to capital constraints. Suppose there is limited capital to be invested in the purchase of stocks, and on day $t-1$, a firm releases especially good news. Capital may flow into this firm, so that there is less capital available to invest in other firms, leading to a lower price response when a firm announces at t. We first note that this story is unlikely to apply in our context because even a large firm announcing on $t-1$ is small relative to the substantial amount of liquid capital invested in US large cap stocks. A limited capital story also suggests abnormally low volume following high $surprise_{t-1}$, which we do not find in the data. Further, these capital constraints imply that there should be lower returns for all firms, not only for firms announcing on day t. In untabulated results, we find that, if anything, there is a positive correlation between $surprise_{t-1}$ and the market return (excluding firms announcing on $t-1$ and t)

[12] In addition to our standard set of control variables, we also include firm fixed effects to account for the substantial heterogeneity in liquidity across different firms. The firm fixed effects mean that we are identifying changes in within-firm announcement day liquidity as a function of variation in the salient earnings surprise released by other firms in the previous day.

suggesting that liquidity issues due to limited capital do not account for the results.

Finally, we check that our results cannot be explained by a risk premium associated with tail risk. For example, if a lower salient surprise in $t-1$ leads to greater crash risk for firms scheduled to announce on day t, rational investors will demand a premium for the risk. Figure 5 shows the distribution of returns for the highest and lowest decile of $surprise_{t-1}$. There is not a significant difference in either tail of the two distributions, suggesting that the empirical results are not explained by a rational fear of extreme negative returns based on the previous day's salient surprise. Instead the mode of the distribution of returns conditional on low $surprise_{t-1}$ lies to the right of the distribution conditional on high $surprise_{t-1}$, consistent with a contrast effects hypothesis.

8 Robustness and heterogeneity

8.1 Alternative measures

This section examines whether our results are robust to alternative choices in the construction of the variables used in the baseline analysis. One concern is that analyst forecasts may not represent market expectations (because they are stale or because analysts are biased or uninformed). If so, our measure of $surprise_{t-1}$ may not capture true market surprise. Therefore, we utilize an alternative measure, the value-weighted $[t-2, t]$ return for large firms that announced on day $t-1$. Our returns-based measure of the salient surprise on $t-1$ is:

$$return\ surprise_{t-1} = \frac{\sum_{i=1}^{N} \left(mkt\ cap_{i,t-4} \times return_{i,[t-2,t]} \right)}{\sum_{i=1}^{N} mkt\ cap_{i,t-4}} \tag{4}$$

Table 8 Panel A Column 1 uses this measure and finds a similar result as in our baseline. We find a significant coefficient of -0.043 on the new $return\ surprise_{t-1}$ measure.[13] The average return response in the lowest and highest deciles of salient return surprise is -4.1% and 4.5%, respectively.

[13] In untabulated results we repeat the analysis without controlling for the firm's own surprise and find similar results. This represents a test of contrast effects that does not use any data on analyst forecasts.

Thus, an increase from the lowest to the highest decile for $return\ surprise_{t-1}$ is on average associated with a decrease in returns of 37 basis points.

In our baseline analysis, firms above the 90th percentile of market capitalization were used to calculate $surprise_{t-1}$. Columns 2 and 3 of Table 8 Panel A measure yesterday's value-weighted surprise using firms above the 85th and 95th percentiles of market capitalization, respectively. Both measures yield similar values to the measure using the 90th percentile cutoff. The next column value-weights all firms that announced earnings on $t-1$ in the calculation of the salient surprise, regardless of market capitalization. This causes the coefficient on salient surprise to decrease in absolute magnitude, although it remains significant. The reduced magnitude is consistent with the earnings announcements of small firms yesterday receiving less attention and being noticed by fewer people, thereby adding noise to the estimate of what investors were actually paying attention to yesterday.

In Panel B, we continue to show that the results are robust to alternative measures of $surprise_{t-1}$. In Column 1, we measure each firm's earnings surprise as the difference between actual earnings and the median analyst forecast, without scaling by the share price. We continue to find a similar negative relation. In Column 2, we find similar results after scaling our measure of $surprise_{t-1}$ by the sum of the squared size weights of each firm comprising the weighted-mean calculation. This accounts for the fact that the weighted average over a greater number of firms has a smaller standard deviation.

Until this point, all analyst-based measures of earnings surprise have been constructed with forecasts from $t-15$ to $t-2$. The last two columns of Table 8 Panel B measure earnings surprise using analyst forecasts starting from $t-30$ and $t-45$. Including more stale forecasts causes the coefficient on salient surprise to decline in absolute magnitude to -0.631 and -0.419, respectively. These results are consistent with more stale forecasts being worse measures of the actual $t-1$ salient surprise, although the results also reflect the inclusion of a number of small firms with one forecast occurring more than 15 days before their announcement.

In Panel C, we examine alternative return measures, date calculations and weighting schemes.

All our regression analysis so far used returns in excess of a characteristic-matched portfolio that excluded the firm announcing today as well as all firms that announced in $t-1$. Column 1 shows results using a simpler return calculation, estimated as raw returns minus the market return over the same period. Column 2 shows the standard characteristic-adjusted return typically used in the finance literature. Using both return calculations, we find similar results.

Our main analysis uses I/B/E/S dates which, in the early years of our sample, sometimes records the date when the earnings announcement was first published in the Wall Street Journal rather than when the information was released through other means (usually one day earlier). We use I/B/E/S announcement dates because we hope to capture when investors pay attention to earnings announcements. Especially early in the sample (which contains the bulk of the errors), the date of publication in the Wall Street Journal as listed in I/B/E/S may be a better measure of when each firm's earnings announcement is most salient. In Column 3, we show that our results are similar utilizing the alternative DellaVigna and Pollet (2009) date correction, which compares the announcement date listed in I/B/E/S with that in Compustat.[14] The results are also similar in the recent sample period, which has a lower rate of date-related errors.

Finally, in Column 4, we equal-weight each observation. Using equal-weights, we find a negative but insignificant coefficient on $surprise_{t-1}$. This is consistent with later results in Sections 8.2 and 8.3, in which we show that our measure of contrast effects is driven by investors comparing the earnings surprises of large firms to those of other large firms. Contrast effects also affect the return response for smaller firms, but the comparisons primarily occur within an industry.

[14]For the DellaVigna and Pollet (2009) date correction, we only include announcements contained in both datasets where the date is the same or is different by no more than one trading day. We then use the following rules: 1) If I/B/E/S has a time stamp for the time of the announcement within the day, we use the I/B/E/S date. 2) If the announcement dates in Compustat and I/B/E/S agree, we use this date if it is on or after January 1, 1990 and the previous trading date if it occurred prior to January 1, 1990. 3) If the Compustat date is the trading day before the I/B/E/S date, we use the Compustat date. 4) If the I/B/E/S date is the trading day before the Compustat date, we use the I/B/E/S date.

8.2 Heterogeneity

In our baseline analysis, we focus on large firms both in the measurement of yesterday's salient surprise and the weighting of observations for firms announcing earnings today. In Table 9 Panel A, we explore how the magnitude of the contrast effect varies with the size of the firm announcing earnings today. The first column breaks the coefficients down by size quintile of the firm releasing earnings on day t. We find that the smaller quintiles have the expected negative coefficients, but these coefficients are smaller in magnitude and insignificant, while the largest (fifth) quintile is driving the results. These results show that our early findings are not driven only by small firms as is the case with many other asset pricing anomalies.

However, these results do not prove that contrast effects are weak for small firms. Rather, we could measure strong contrast effects for large firms announcing today because investors tend to contrast large firms releasing earnings today with other large firms that released earnings yesterday. Investors of smaller firms may contrast the earnings of small firms with that of other similar firms that released earnings yesterday. However, because multiple firms release earnings on $t-1$, it is difficult for us, as econometricians, to identify which firms are salient to investors for each small firm announcing earnings today. This is a point that we explore in detail in Section 8.3, where we show that contrast effects are sizable and significant for smaller firms once we look within industries.

The second column explores heterogeneity in the number of analysts covering firms that release earnings today. In general, the more interest the market has in a given firm, the more analysts will cover that firm's earnings announcement. We examine contrast effects separately for firms covered by one, two, and three or more analysts. We find a monotonic increase in contrast effects of 0.073 for firms with one analyst, -0.793 for two analysts, and -1.027 for three or more analysts and a similar increase in the statistical significance of each coefficient. This shows that our findings are not driven by small firms with little analyst coverage. However, we again caution that these results do not imply that investors in firms with little analyst coverage do not suffer from contrast effects. Rather, these investors may contrast these smaller, niche firms with a specific set of other similar

small firms that we have difficulty identifying.[15]

Finally, we explore how our results vary over time. We examine the effect separately decade by decade and find that our results have not declined in recent years. The effect does not differ significantly over time, from -0.663 in the 1980s, -0.912 in the 1990s, -0.883 in the 2000s, and -0.997 after 2010. The large and significant estimate of contrast effects in the 2000s and after shows that our results are unlikely to be driven by date recording errors in the early period in I/B/E/S.

$Surprise_{t-1}$ typically occurs on the previous calendar day, except for Mondays. Attention paid to $surprise_{t-1}$ could differ based on the calendar time between the prior and current announcements. One possibility is that the salience of $surprise_{t-1}$ decays over the weekend leading to less of a contrast effect on Mondays. The salience of $surprise_{t-1}$ could also increase over the weekend, perhaps because investors have more time to think about Friday announcements, leading to stronger contrast effects on Mondays. Finally, it could be that ordering is the only aspect of timing that matters for attention (as in classic studies of recency, e.g., Murdock Jr, 1962), in which case, contrast effects on Mondays will be similar to that of other days.

In Table 9 Panel B, we examine contrast effects separately depending on whether today's announcement occurs on Monday or other days of the week. We find an insignificant coefficient of 0.0759 for Mondays, while for other days of the week the coefficient is a significant -0.724. Columns 3 and 4 repeat the analysis for the recent sample to ameliorate concerns about errors in the recording of announcement dates to ensure that we are accurately capturing true day of the week and finds similar results. Ignoring significance, the point estimates are consistent with the first scenario of salience decaying over time. However, the difference in coefficients is very noisy. The Monday coefficient is estimated with large standard errors, and the p-value indicates that the two coefficients are not statistically distinct.

[15] We face the additional measurement challenge that the earnings surprises of small firms are measured with greater error because our measure of market expectations is likely to be noisier due to reduced analyst coverage. This implies that we may control for the actual earnings surprises of small firms with more error.

8.3 Industry contrast effects

As discussed in the previous section, while we measure stronger contrast effects among larger firms, contrast effects could also affect the returns of smaller firms. Investors may compare smaller firms to a subset of similar firms that announced in the previous day. If so, our baseline empirical specification will underestimate the true magnitude of contrast effects for small firms announcing on day t because we measure the salient surprise in $t-1$ as the value-weighted average of earnings surprises among all large firms that announced in $t-1$.

It is difficult to know what the right comparison group is for any firm, but one reasonable possibility is other firms in the same industry. In this section, we explore how contrast effects depend on whether the firms announcing today and yesterday belong to the same industry. We find that contrast effects for large firms can be strong both within and across industries. However, across-industry contrast effects are only strong if there is no same-industry comparison firm available. If the previous day had announcements from large firms in both the same and different industries, we find a larger effect for the same-industry announcement. In addition, for smaller firms announcing today, we find that contrast effects primarily operate through within-industry comparisons.

In Table 10, we modify our baseline specification to include two measures of $surprise_{t-1}$: one based on other firms announcing in the same industry as the firm announcing on day t and one based on other firms in different industries. To form these two salient surprise measures, we continue to use the value-weighted average surprises of firms above the 90th percentile of market capitalization, under the assumption that, even within industry, larger firms are more likely to be more salient.[16] We present results using the very broad Fama French 5 industry classification. We do not use narrowly-defined industry classification systems because a limited set of firms announce earnings on $t-1$. If we use narrowly-defined industries, we often lack another firm announcing within the same industry. We also caution that companies may be related in a variety of ways that matter to investors, and these relations will be imperfectly captured by any industry classification system.

[16] In untabulated results, we find a similar pattern if we expand the definition of salient surprise to allow for the inclusion of smaller firms that announced on $t-1$.

Thus, the results are based on a noisy proxy of what investors are paying attention to.

A limited number of large firms (median of 6) announce earnings on $t-1$ and there are usually fewer firms in the same industry as the firm announcing on day t than firms in different industries. This implies that the standard deviation of the different-industry salient surprise will be relatively smaller, as the average of a larger sample has a smaller standard deviation. To make the magnitudes of the coefficients on the $t-1$ salient surprises in the same- and different-industry samples comparable, we scale each salient surprise by the sum of the squared size weights of each firm comprising the weighted-mean calculation. While this scaling makes the coefficients for the same and different industry salient surprises comparable to one another, the magnitude of these coefficients should not be compared to those in other tables (with the exception of Table 8 Panel B Column 2). In addition, if no firm announced within the same (different) industry on $t-1$, we set the relevant $surprise_{t-1}$ variable to zero and include a dummy variable equal to one when the same (different) industry $surprise_{t-1}$ is missing.

Table 10 modifies our baseline specification to use the two separate measures of salient surprise on day $t-1$. Column 1 is value-weighted by the market capitalization of the firm announcing earnings today while Column 2 is equal-weighted. Thus, Column 1 overweights larger firms relative to Column 2. We find that, when large firms are weighted more heavily, the magnitude of the contrast effect is similar within and across industries. When smaller firms are weighted more heavily as in Column 2, the contrast effect is large and significant only within the same industry. In Column 3, we again value-weight the regression to focus on large firms, but include only days where both same industry and different industry $surprise_{t-1}$ are not missing. On such days, large firms exhibit a stronger contrast effect in response to firms in the same industry. However, the same and different industry coefficients are not statistically different from one another, even though only the same industry coefficient is statistically different from zero.

The final four columns separately examine the sample of large firms (above median market capitalization in each year) and small firms (below the median in each year) announcing on day t. We find that small firms exhibit contrast effects with same industry firms and not different industry

firms regardless of whether there was a same industry announcement the previous trading day. Large firms tend to be contrasted with other large firms regardless of industry. However, if there was a same industry announcement in the previous trading day, the contrast effect within the same industry dominates that of different industry. Again, the differences are not statistically significant, as indicated by the p-values at the bottom of the table.

Overall, these results are consistent with a world in which investors in smaller firms pay more attention to previous announcements by other firms in the same industry. Meanwhile, investors in larger firms pay attention to the recent earnings announcements of other large firms, but pay relatively more attention to same-industry announcements if such a comparison is available. This suggests that the magnitude of contrast effects depend on whether agents consider signals to belong to the same category. In this paper, we have shown that industry and size affect relative comparisons among earnings announcements. We leave the important question of how the boundaries of comparison sets are formed more generally for future research.

9 Conclusion

We present evidence of contrast effects in sophisticated financial markets: investors mistakenly perceive information from earnings announcements in contrast to what preceded it. The scheduling of when earnings are announced is usually set several weeks before the announcement, so whether a given firm announces following positive or negative surprises by other firms is unlikely to be correlated with the firm's fundamentals. We find that the return reaction to an earnings announcement is inversely related to the level of earnings surprise announced by large firms in the previous day. This implies that market prices react to the relative content of news instead of only reacting to the absolute content of news.

The existing empirical literature on contrast effects mainly comes from laboratory settings and the limited field evidence focuses on households making infrequent dating or real estate decisions. Our results show that contrast effects impact equilibrium prices and capital allocation in sophisti-

cated markets. In this setting, professionals make repeated investment decisions based on earnings announcements and market prices are determined through the interactions of many investors.

Our results suggest that contrast effects have the potential to bias a wide variety of important real-world decisions, including judicial sentencing, hiring and promotion decisions, firm project choice, and household purchase decisions. While we focus on showing that contrast effects bias perceptions of news, contrast effects may also provide a psychological basis for preferences, such as internal habit formation, that are assumed in many influential models in macroeconomics and finance. Under internal habit formation, individuals value gains in consumption relative to previous experience rather than only its absolute level. These preferences could arise because past high levels of consumption lead individuals to perceive any amount of current consumption as lesser in comparison.

To attain a clean measure of contrast effects, we chose a financial setting in which firms cannot strategically use contrast effects to their advantage because they precommit to when they will announce earnings. However, in other settings, agents with discretion over the timing of information disclosure may schedule the release of news in order to take advantage of contrast effects bias. For example, a firm with very bad news to release may try to release that news after another firm releases bad news, so that the perception of its own news is not as negative. Such strategic manipulation of market biases may be a promising direction for future research.

References

Anilowski, Carol, Mei Feng, and Douglas J Skinner, 2007, Does earnings guidance affect market returns? the nature and information content of aggregate earnings guidance, *Journal of Accounting and Economics* 44, 36–63.

Baker, Malcolm, Xin Pan, and Jeffrey Wurgler, 2012, The effect of reference point prices on mergers and acquisitions, *Journal of Financial Economics* 106, 49–71.

Ball, Ray, and Eli Bartov, 1996, How naive is the stock market's use of earnings information?, *Journal of Accounting and Economics* 21, 319–337.

Ball, Ray, and Philip Brown, 1968, An empirical evaluation of accounting income numbers, *Journal of Accounting Research* 159–178.

Barber, Brad M, Emmanuel T De George, Reuven Lehavy, and Brett Trueman, 2013, The earnings announcement premium around the globe, *Journal of Financial Economics* 108, 118–138.

Barber, Brad M, and Terrance Odean, 2008, All that glitters: The effect of attention and news on the buying behavior of individual and institutional investors, *Review of Financial Studies* 21, 785–818.

Barberis, Nicholas, Robin Greenwood, Lawrence Jin, and Andrei Shleifer, 2015, X-capm: An extrapolative capital asset pricing model, *Journal of Financial Economics* 115, 1–24.

Barth, Mary E, and Eric C So, 2014, Non-diversifiable volatility risk and risk premiums at earnings announcements, *The Accounting Review* 89, 1579–1607.

Beaver, William H, 1968, The information content of annual earnings announcements, *Journal of Accounting Research* 67–92.

Bernard, Victor L, and Jacob K Thomas, 1989, Post-earnings-announcement drift: Delayed price response or risk premium?, *Journal of Accounting Research* 1–36.

Bernard, Victor L, and Jacob K Thomas, 1990, Evidence that stock prices do not fully reflect the implications of current earnings for future earnings, *Journal of Accounting and Economics* 13, 305–340.

Bernard, Victor Lewis, 1992, *Stock Price Reactions to Earnings Announcements: A Summary of Recent Anomalous Evidence and Possible Explanations, in: R. Thaler, ed., Advances in Behavioral* (Russell Sage Foundation, New York, NY).

Bhargava, Saurabh, and Ray Fisman, 2014, Contrast effects in sequential decisions: Evidence from speed dating, *Review of Economics and Statistics* 96, 444–457.

Bordalo, Pedro, Nicola Gennaioli, and Andrei Shleifer, 2015, Memory, attention and choice, *Working Paper* .

Boulland, Romain, and Olivier Dessaint, 2014, Announcing the announcement, *Working Paper* .

Bushong, Benjamin, Matthew Rabin, and Joshua Schwartzstein, 2015, A model of relative thinking, *Working Paper* .

Chang, Tom, Samuel M Hartzmark, David H Solomon, and Eugene F Soltes, 2014, Being surprised by the unsurprising: Earnings seasonality and stock returns, *Working Paper* .

Chen, Daniel L, Tobias J Moskowitz, and Kelly Shue, 2015, Decision-making under the gambler's fallacy: Evidence from asylum judges, loan officers, and baseball umpires, *Working Paper* .

Cunningham, Tom, 2013, Comparisons and choice, *Working Paper* .

Daniel, Kent, Mark Grinblatt, Sheridan Titman, and Russ Wermers, 1997, Measuring mutual fund performance with characteristic-based benchmarks, *The Journal of Finance* 52, 1035–1058.

DellaVigna, Stefano, Attila Lindner, Balázs Reizer, and Johannes F Schmieder, 2014, Reference-dependent job search: Evidence from hungary, *Working Paper* .

DellaVigna, Stefano, and Joshua M Pollet, 2009, Investor inattention and friday earnings announcements, *The Journal of Finance* 64, 709–749.

Fama, Eugene F, and Kenneth R French, 1992, The cross-section of expected stock returns, *The Journal of Finance* 47, 427–465.

Frazzini, Andrea, and Owen A Lamont, 2007, The earnings announcement premium and trading volume, *Working Paper* .

Greenwood, Robin, and Andrei Shleifer, 2014, Expectations of returns and expected returns, *Review of Financial Studies* 27, 714–746.

Gulen, Huseyin, and Byoung-Hyoun Hwang, 2012, Daily stock market swings and investor reaction to firm-specific news, *Available at SSRN 1934873* .

Hartzmark, Samuel M., 2015, The worst, the best, ignoring all the rest: The rank effect and trading behavior, *Review of Financial Studies* 28, 1024–1059.

Hirshleifer, David, and Tyler Shumway, 2003, Good day sunshine: Stock returns and the weather, *The Journal of Finance* 58, 1009–1032.

Hong, Harrison, and Jeffrey D Kubik, 2003, Analyzing the analysts: Career concerns and biased earnings forecasts, *Journal of Finance* 313–351.

Kahneman, Daniel, and Amos Tversky, 1979, Prospect theory: An analysis of decision under risk, *Econometrica: Journal of the Econometric Society* 263–291.

Kamenica, Emir, 2008, Contextual inference in markets: On the informational content of product lines, *The American Economic Review* 98, 2127–2149.

Kenrick, Douglas T., and Sara E. Gutierres, 1980, Contrast effects and judgments of physical attractiveness: When beauty becomes a social problem, *Journal of Personality and Social Psychology* 38, 131–140.

Koszegi, Botond, and Matthew Rabin, 2006, A model of reference-dependent preferences, *Quarterly Journal of Economics* 121, 1133–1165.

Koszegi, Botond, and Matthew Rabin, 2007, Reference-dependent risk attitudes, *American Economic Review* 7, 1047–1073.

Levitt, Steven D, and John A List, 2007a, Viewpoint: On the generalizability of lab behaviour to the field, *Canadian Journal of Economics* 40, 347–370.

Levitt, Steven D, and John A List, 2007b, What do laboratory experiments measuring social preferences reveal about the real world?, *The Journal of Economic Perspectives* 153–174.

Lim, Terence, 2001, Rationality and analysts' forecast bias, *The Journal of Finance* 56, 369–385.

Lin, Hsiou-wei, and Maureen F McNichols, 1998, Underwriting relationships, analysts' earnings forecasts and investment recommendations, *Journal of Accounting and Economics* 25, 101–127.

McNichols, Maureen, and Patricia C O'Brien, 1997, Self-selection and analyst coverage, *Journal of Accounting Research* 167–199.

Mian, G Mujtaba, and Srinivasan Sankaraguruswamy, 2012, Investor sentiment and stock market response to earnings news, *The Accounting Review* 87, 1357–1384.

Murdock Jr, Bennet B, 1962, The serial position effect of free recall., *Journal of experimental psychology* 64, 482.

Odean, Terrance, 1998, Are investors reluctant to realize their losses?, *The Journal of Finance* 53, 1775–1798.

Pepitone, Albert, and Mark DiNubile, 1976, Contrast effects in judgments of crime severity and the punishment of criminal violators., *Journal of Personality and Social Psychology* 33, 448.

Rabin, Matthew, and Dimitri Vayanos, 2010, The gambler's and hot-hand fallacies: Theory and applications, *The Review of Economic Studies* 77, 730–778.

Shefrin, Hersh, and Meir Statman, 1985, The disposition to sell winners too early and ride losers too long: Theory and evidence, *The Journal of Finance* 40, 777–790.

Simonsohn, Uri, 2006, New yorkers commute more everywhere: Contrast effects in the field, *Review of Economics and Statistics* 88, 1–9.

Simonsohn, Uri, and George Loewenstein, 2006, Mistake 37: The effect of previously encountered prices on current housing demand, *Economic Journal* 116, 176–199.

Sloan, R, 1996, Do stock prices fully reflect information in accruals and cash flows about future earnings?, *Accounting Review* 71, 289–315.

So, Eric, 2015, Time will tell: Information in the timing of scheduled earnings news, *Working Paper* .

So, Eric C, 2013, A new approach to predicting analyst forecast errors: Do investors overweight analyst forecasts?, *Journal of Financial Economics* 108, 615–640.

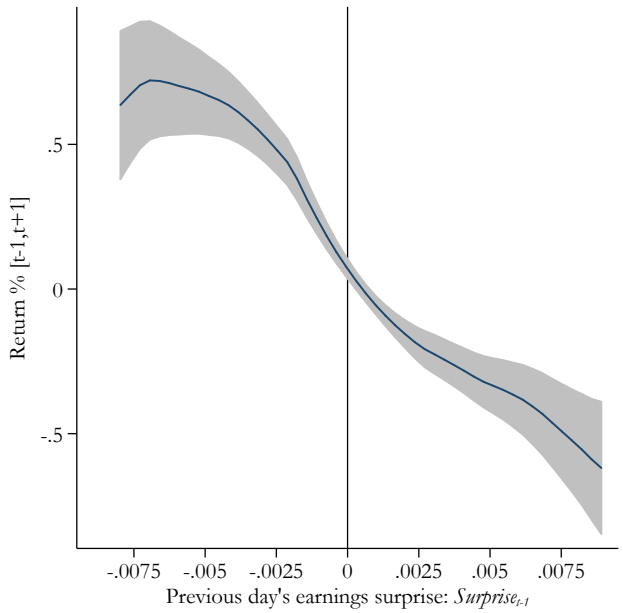
Thomas, Jacob, and Frank Zhang, 2008, Overreaction to intra-industry information transfers?, *Journal of Accounting Research* 46, 909–940.

Figure 2

Return Reaction to Earnings $Surprise_{t-1}$

This graph shows the relation between the returns from $[t-1, t+1]$ of firms that announced earnings on day t and the salient surprise ($surprise_{t-1}$) announced by other firms on day $t-1$ (calculated as the value-weighted earnings surprises of large firms that announced earnings on day $t-1$), estimated using a value-weighted local linear regression with the optimal bandwidth. We define a "large" firm as a firm with market capitalization at $t-4$ exceeding the 90th percentile cutoff of the NYSE index in that month. Gray areas indicate 90 percent confidence intervals. Panel A reports return residuals after controlling for 20 bins in terms of the firm's own earnings surprise. Panel B reports unconditional returns without controlling for the firm's own earnings surprise, demeaned by the value-weighted average return in the sample.

Panel A: Conditional on Own Earnings Surprise



Panel B: Unconditional

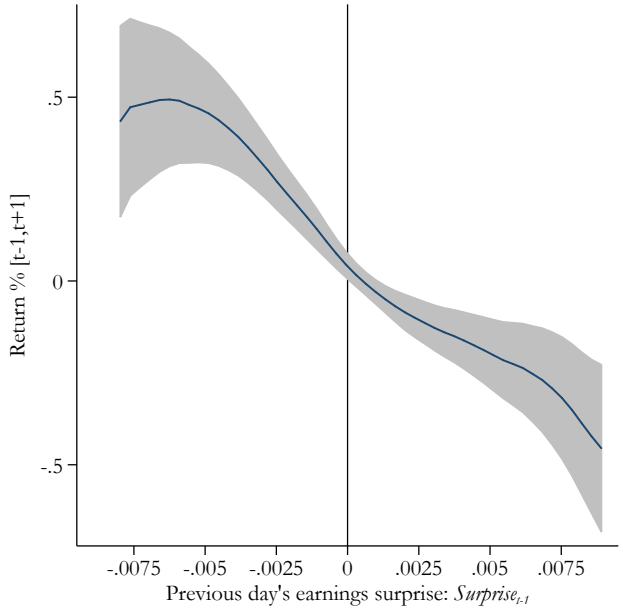


Figure 3
Return Reaction to Own Earnings Surprise

This graph shows the return reaction to a firm's own earnings surprise, and how that varies with $surprise_{t-1}$. Each line plots the value-weighted return $[t-1, t+1]$ of firms that announced earnings on day t against the percentile ranks of the firm's own earnings surprise, estimated using a value-weighted local linear regression with the optimal bandwidth. The graph shows two subsamples: return reactions following $surprise_{t-1}$ in either the lowest or highest deciles. Gray areas indicate 90 percent confidence intervals.

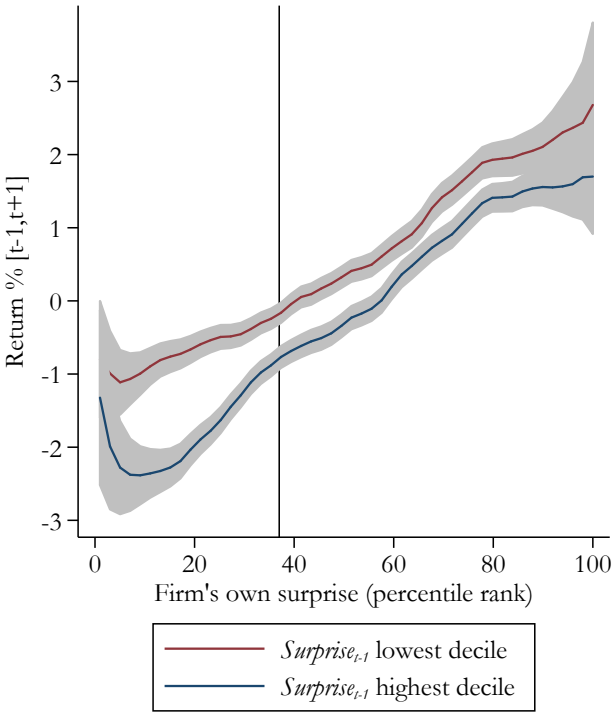


Figure 4
Cumulative Returns

This graph plots the cumulative value-weighted returns starting at market open on day t of firms that announce on day t, conditional on $surprise_{t-1}$. We examine subsamples whether $surprise_{t-1}$ was below the 25th percentile or above the 75th percentile relative to its distribution over the previous quarter. The dotted lines indicate 90 percent confidence intervals.

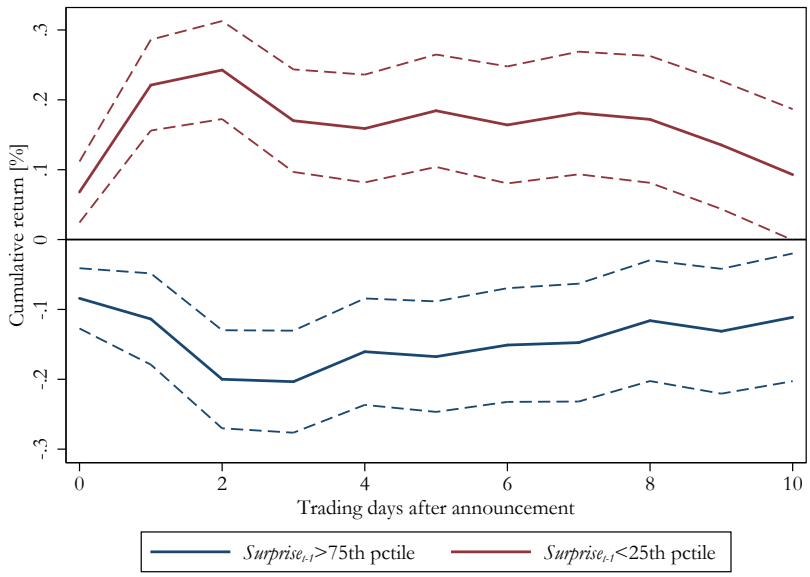


Figure 5
Distribution of Returns by $Surprise_{t-1}$

This graph shows the distribution of returns $[t-1, t+1]$ of firms that announced earnings on day t split into two samples based on $surprise_{t-1}$. The red line contains firms that announced the day after a $surprise_{t-1}$ in the highest decile while the blue lines contains firms that announced after a $surprise_{t-1}$ in the lowest decile. Distributions are estimated using a kernel density estimator.

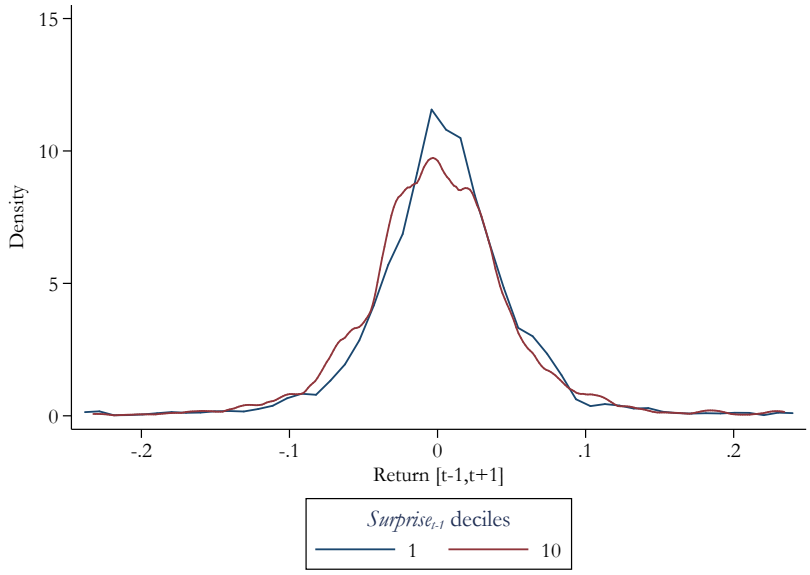


Table 1
Summary Statistics

This table presents summary statistics for the main variables used in our analysis using data from 1984 to 2013. The earnings surprise is measured as $(actual - forecast)/price_{t-3}$ where $forecast$ is the median of each analyst's most recent forecast that is released within 15 days of the announcement, excluding t and $t-1$. Returns are the return of a firm minus the return of a portfolio matched on quintiles of market capitalization, book-to-market ratio, and momentum (excluding firms used in the calculation of $surprise_{t-1}$ and the announcing firm). $Surprise_{t-1}$ is our baseline measure of the salient surprise released by other firms in the previous trading day. It is calculated as the value-weighted earnings surprise of all large firms that announced in the previous trading day. We define a "large" firm as a firm with market capitalization three days before its earnings is announced that exceeds the 90th percentile cutoff of the NYSE index in that month.

	N	Mean	SD	P25	P50	p75
Own earnings surprise	75923	-0.0003	0.0138	-0.0003	0.0002	0.0015
Return [t-1, t+1]	75923	0.0017	0.0701	-0.0316	0.0007	0.0351
Market Cap_{t-3} ($M)	75923	7670	24100	439	1487	5052
Number of analysts [t-15, t-2]	75923	3.722	3.669	1	2	5
$Surprise_{t-1}$	75923	0.0005	0.0017	0.0000	0.0004	0.0010
Number of surprises [t-1], large firms	75923	7.549	5.782	3	6	12

Table 2
Baseline Results

This table explores the relation between return reactions for firms that announce earnings today and the earnings surprises of other firms that announced in the previous trading day. The return from $[t-1, t+1]$ for announcing firms is regressed on various measures of the salient earnings surprise from $t-1$ and additional controls. Returns are the return of a firm minus the return of a portfolio matched on quintiles of market capitalization, book-to-market ratio, and momentum (excluding firms used in the calculation of $surprise_{t-1}$ and the announcing firm). Surprises for the firms announcing today and in the previous trading day are measured as $(actual - forecast)/price_{t-3}$ where $forecast$ is the median of each analyst's most recent forecast that is released within 15 days of the announcement, excluding t and $t-1$. We define a "large" firm as a firm with market capitalization three days before its earnings is announced that exceeds the 90th percentile cutoff of the NYSE index in that month. Columns 1 and 2 measure $surprise_{t-1}$ as the earnings surprise of the largest firm (conditional on it being a large firm) the announced in the previous trading day. Columns 3 and 4 measure $surprise_{t-1}$ using the equal-weighted earnings surprise of all large firms that announced in the previous trading day. Columns 5 and 6 measure $surprise_{t-1}$ as the value-weighted earnings surprise of all large firms that announced in the previous trading day. All regressions include controls for 20 equally sized bins in terms of the earnings surprise of the firm that announced today, plus a dummy for zero earnings surprise. Even-numbered columns also include controls for year-month fixed effects. We refer to Column 6 as our baseline specification in later tables. Observations are value-weighted by the $t-3$ scaled market capitalization of the firm announcing earnings today. Standard errors are clustered by date. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Return $[t-1, t+1]$					
	(1)	(2)	(3)	(4)	(5)	(6)
$Surprise_{t-1}$ of largest firm	-0.617***	-0.422**				
	(0.179)	(0.188)				
$Surprise_{t-1}$ large firms, EW mean			-1.075***	-0.944***		
			(0.255)	(0.277)		
$Surprise_{t-1}$ large firms, VW mean					-0.945***	-0.887***
					(0.225)	(0.244)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	No	Yes	No	Yes	No	Yes
R^2	0.0587	0.0833	0.0592	0.0838	0.0591	0.0838
Observations	75923	75923	75923	75923	75923	75923

Table 3
Potential Interaction Effects

This table examines whether contrast effects are related to an interaction between $surprise_{t-1}$ and the announced surprise on day t. Column 1 measures the surprise today using the level, Column 2 measures it using 20 equally sized bins, and Column 3 uses quintiles. For brevity, we report only the interaction effects, but all direct effects are included in the regressions. All other variables and weights are as defined in Table 2. Standard errors are clustered by date. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Return $[t-1, t+1]$		
	(1)	(2)	(3)
$Surprise_{t-1}$	-0.935***	-1.482***	-1.502**
	(0.256)	(0.525)	(0.677)
$Surprise_{t-1}$ x own surprise	17.79		
	(38.11)		
$Surprise_{t-1}$ x own surprise (20 bins)		0.0660	
		(0.0483)	
$Surprise_{t-1}$ x own surprise quintile 2			0.296
			(0.877)
$Surprise_{t-1}$ x own surprise quintile 3			0.811
			(0.903)
$Surprise_{t-1}$ x own surprise quintile 4			0.986
			(0.811)
$Surprise_{t-1}$ x own surprise quintile 5			0.849
			(1.023)
Year-month FE	Yes	Yes	Yes
R^2	0.0375	0.0809	0.0801
Observations	75923	75923	75923

Table 4
Additional Support for Contrast Effects

This table provides further evidence of contrast effects. Panel A Columns 1 and 2 examine the impact of $t-3$, $t-2$, $t-1$, $t+1$, and $t+2$ salient surprises. The dependent variable in Columns 1 and 2 is the return over the windows $[t-3,\ t+1]$ and $[t-1,\ t+2]$, respectively. Dummy variables are included for instances where there is a missing salient surprise of the indicated day. p-values are from the test of whether the $t-1$ coefficient is equal to the indicated coefficient. Panel A Columns 3 and 4 explore contrast effects within the same day. We classify an earnings announcement as "AM" or "PM" based on whether it was released before market open or after market close. Column 3 regresses the $[t,\ t+1]$ returns of firms that released PM announcements on the value-weighted surprises of large firms that released AM announcements. Column 4 regresses the open-to-open $[t,\ t+1]$ returns of firms that released AM announcements on the value-weighted surprises of large firms that released PM announcements. Panel B shows the relation between $surprise_{t-1}$ and long run return reactions. Return windows are as labeled in column headers. All other variables and weights are as defined in Table 2. Standard errors are clustered by date. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Lags and Leads and Same-Day Contrast Effects

	Longer lags and leads		Own PM announcement	Own AM announcement
	(1)	(2)	(3)	(4)
$Surprise_{t-3}$	-0.332			
	(0.215)			
$Surprise_{t-2}$	0.124			
	(0.268)			
$Surprise_{t-1}$	-0.841***	-0.875***		
	(0.272)	(0.310)		
$Surprise_{t+1}$		0.199		
		(0.387)		
$Surprise_{t+2}$		-0.101		
		(0.394)		
AM surprise of others			-1.472**	
			(0.673)	
PM surprise of others				-0.417
				(0.312)
p-value: (t-3) = (t-1)	0.0931			
p-value: (t-2) = (t-1)	0.00591			
p-value: (t+1) = (t-1)		0.0260		
p-value: (t+2) = (t-1)		0.118		
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
R^2	0.0824	0.0727	0.161	0.107
Observations	75870	75885	19346	17874

Table 4
Continued: Additional Support for Contrast Effects
Panel B: Long Run Return Windows

	$[t-1, t+10]$	$[t-1, t+20]$	$[t-1, t+30]$	$[t-1, t+40]$	$[t-1, t+50]$
	(1)	(2)	(3)	(4)	(5)
$Surprise_{t-1}$	-0.837**	-0.831**	-0.317	-0.0945	0.493
	(0.405)	(0.409)	(0.497)	(0.561)	(0.686)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes
R^2	0.0616	0.0465	0.0375	0.0373	0.0359
Observations	75736	75567	75362	74995	74149

	$[t+2, t+10]$	$[t+2, t+20]$	$[t+2, t+30]$	$[t+2, t+40]$	$[t+2, t+50]$
	(1)	(2)	(3)	(4)	(5)
$Surprise_{t-1}$	0.00969	0.0371	0.472	0.755	1.327*
	(0.340)	(0.371)	(0.482)	(0.559)	(0.677)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes
R^2	0.0228	0.0215	0.0215	0.0247	0.0272
Observations	75783	75607	75397	75028	74179

Table 5
Information Transmission

Panel A examines whether $surprise_{t-1}$ predicts earnings surprises on day t. The dependent variable in Columns 1 and 2 is the surprise of the firm that announces on day t. The dependent variable in Columns 3 and 4 is the bin (1 through 20, equally sized) for the surprise of the firm that announces on day t. Columns 1 and 3 exclude year-month fixed effects while Columns 2 and 4 include year-month fixed effects. Panel B explores the day $t-1$ return reaction of the firm scheduled to announce on day t to $surprise_{t-1}$. The dependent variable is the $t-1$ return for the firm scheduled to announce on day t, measured as close-to-close returns in Columns 1 and 2 and open-to-open returns in Columns 3 and 4. Panel C re-estimates our baseline test of contrast effects within the subsample of observations for which information transmission is unlikely to have occurred. In Column 1 and 2, the sample is restricted to observations for which the $t-1$ returns of the firm announcing earnings today moved by less than 1% and 0.5%, respectively, in either direction. Column 3 examines the sample with no negatively correlated information transmission, i.e., we exclude negative (positive) return reactions to positive (negative) $surprise_{t-1}$. The dependent variable is the open-to-open $[t, t+1]$ return of the firm announcing earnings on day t. All other variables and weights are as defined in Table 2. Standard errors are clustered by date. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Surprise Predictability

	$Surprise_{it}$		20 bins in $surprise_{it}$	
	(1)	(2)	(3)	(4)
$Surprise_{t-1}$	0.157***	0.0115	132.0***	-27.00
	(0.0603)	(0.0602)	(32.44)	(27.24)
Own $surprise_{it}$ controls	No	No	No	No
Year-month FE	No	Yes	No	Yes
R^2	0.00204	0.0324	0.00280	0.0650
Observations	75923	75923	75923	75923

Panel B: Return Response to Potential Information Release

	Close-to-close ret $[t-1]$		Open-to-open ret $[t-1]$	
	(1)	(2)	(3)	(4)
$Surprise_{t-1}$	0.0589	-0.0684	0.128	0.0655
	(0.131)	(0.126)	(0.155)	(0.145)
Own $surprise_{it}$ controls	No	No	No	No
Year-month FE	No	Yes	No	Yes
R^2	0.0000319	0.0257	0.000153	0.0253
Observations	75923	75923	61732	61732

Panel C: Sample with No Evidence of Information Transmission

	$\lvert Ret_{t-1})\rvert < 0.01$	$\lvert Ret_{t-1}\rvert < 0.005$	No neg corr info transmission $[t-1]$
	(1)	(2)	(3)
$Surprise_{t-1}$	-0.915**	-0.868**	-1.454***
	(0.362)	(0.410)	(0.335)
Return type	Open-open	Open-open	Open-open
Own $surprise_{it}$ controls	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
R^2	0.115	0.162	0.0900
Observations	25907	14043	31137

Table 6
Unconditional Relation (Not Controlling for Own Surprise)

Panel A presents regressions similar to those in Table 2, except that they exclude the firm's own surprise as control variables. Odd-numbered columns also exclude year-month fixed effects. All other variables and weights are as defined in Table 2. Standard errors are clustered by date. Panel B presents the abnormal returns to portfolios formed based upon $surprise_{t-1}$ using open-to-open returns. On days where $surprise_{t-1}$ is below a cutoff, we long stocks with an earnings announcement on day t and short the market and do the opposite when $surprise_{t-1}$ is above a cutoff. The position is held for days t to $t+1$ beginning at market open. We include only stocks with a market capitalization above the 80th percentile of the NYSE. Columns 1 and 2 include only portfolios where there are at least 5 stocks with earnings announcements on each day while Columns 3 and 4 include any day with at least one stock announcing earnings. Columns 1 and 3 utilize a cutoff of 0 for $surprise_{t-1}$, while Columns 2 and 4 utilize a cutoff of being below the 25th or above the 75th percentile of $surprise_{t-1}$, respectively. We compute abnormal returns from a four factor model by regressing portfolio returns on the market, SMB, HML, and UMD risk factors. Each portfolio is value-weighted by the stocks announcing earnings on day t. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Unconditional Results

	Close-to-close $[t-1, t+1]$		Open-to-open $[t-1, t+1]$		Open-to-open $[t, t+1]$	
	(1)	(2)	(3)	(4)	(5)	(6)
$Surprise_{t-1}$	-0.590***	-0.926***	-0.665***	-1.015***	-0.829***	-1.112***
	(0.223)	(0.256)	(0.258)	(0.290)	(0.238)	(0.281)
Own $surprise_{it}$ controls	No	No	No	No	No	No
Year-month FE	No	Yes	No	Yes	No	Yes
R^2	0.000473	0.0268	0.000560	0.0249	0.000965	0.0248
Observations	75923	75923	61729	61729	61729	61729

Panel B: Abnormal Returns to Trading Strategy

	5 or More Stocks		Any Number of Stocks	
	(1)	(2)	(3)	(4)
Alpha [%]	0.0985**	0.216***	0.0855*	0.182***
	(0.0447)	(0.0532)	(0.0487)	(0.0556)
MktRf	-0.0233	0.00119	-0.0877**	-0.0489
	(0.0353)	(0.0392)	(0.0405)	(0.0451)
SMB	0.0868	-0.0555	0.136*	0.0729
	(0.0675)	(0.0767)	(0.0779)	(0.0871)
HML	-0.0539	-0.133*	-0.0234	-0.180**
	(0.0708)	(0.0771)	(0.0757)	(0.0825)
UMD	0.0503	0.0380	-0.0179	-0.00971
	(0.0478)	(0.0537)	(0.0538)	(0.0591)
Long Cutoff	$Surprise_{t-1}<0$	$Surprise_{t-1}<$25th Pctile	$Surprise_{t-1}<0$	$Surprise_{t-1}<$25th Pctile
Short Cutoff	$Surprise_{t-1}>0$	$Surprise_{t-1}>$75th Pctile	$Surprise_{t-1}>0$	$Surprise_{t-1}>$75th Pctile
Observations	1275	837	2150	1525
Annual Return[%]	6.48	9.47	9.62	14.9

Table 7
Strategic Timing of Earnings Announcements, Changes in Risk and Trading Frictions

This table tests whether the negative relation between return reactions and $surprise_{t-1}$ is driven by changes in the scheduling of announcements or changes in risk or trading frictions. In Panel A, $\Delta date$ is the difference between the day of the current earnings announcement and the previous year's same-quarter earnings announcement (e.g., for a firm announcing on March 15, 2004 that previously announced on March 12, 2003, $\Delta date = 3$). Panel B Columns 1 and 2 test whether the negative relation is driven by changes in risk, as measured by the betas of the market, SMB, HML, and UMD risk factors. We regress our baseline return measure (Column 1) or the raw return (Column 2) on the four factors, year-month fixed effects, $surprise_{t-1}$, and the interaction between $surprise_{t-1}$ and the four factors. Panel B Columns 3 and 4 test whether the negative relation is driven by changes in liquidity, measured as the log of daily dollar volume in Column 3 and the log of the bid-ask spread in Column 4. Measures of liquidity vary greatly across firms so Columns 3 and 4 include firm fixed effects. All other variables and weights are as defined in Table 2. Standard errors are clustered by date. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Strategic Timing of Earnings Announcements

	Return $[t-1, t+1]$	
	(1)	(2)
$Surprise_{t-1}$ x abs(Δ date)<=5	-0.896***	
	(0.267)	
$Surprise_{t-1}$ x abs(Δ date)>5	-0.723	
	(0.704)	
$Surprise_{t-1}$ x Δ date<-5		0.913
		(0.845)
$Surprise_{t-1}$ x abs(Δ date)<=5		-0.903***
		(0.267)
$Surprise_{t-1}$ x Δ date>5		-1.334
		(0.918)
Own $surprise_{it}$ controls	Yes	Yes
Year-month FE	Yes	Yes
R^2	0.0850	0.0854
Observations	70135	70135

Panel B: Changes in Risk and Trading Frictions

	Return $[t-1, t+1]$	Raw ret $[t-1, t+1]$	Log(volume)	Log(bid-ask)
	(1)	(2)	(3)	(4)
$Surprise_{t-1}$	-0.934***	-1.039***	3.094	1.418
	(0.245)	(0.249)	(4.825)	(5.533)
Mkt-rf x $surprise_{t-1}$	0.817	-0.666		
	(9.069)	(9.307)		
SMB x $surprise_{t-1}$	-22.60	-24.06		
	(19.83)	(22.56)		
HML x $surprise_{t-1}$	29.91	42.62		
	(28.77)	(29.41)		
UMD x $surprise_{t-1}$	31.05*	51.48***		
	(16.51)	(16.26)		
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
R^2	0.0851	0.215	0.891	0.754
Observations	75923	76062	75910	68909

Table 8
Alternative Measures of Surprise

This table shows that our baseline results are robust to alternative measures and sample restrictions. All variables and weights are as defined in Table 2, except for the following changes. Panel A Column 1 measures the salient surprise in $t-1$ as the value-weighted average of the return response to the $t-1$ earnings announcements of other firms above the 90th percentile of market capitalization. In Columns 2 and 3, $surprise_{t-1}$ is calculated using firms that announced in $t-1$ that exceeded the 85th and 95th percentile size cutoffs of the NYSE index in that month, respectively. In Column 4, $surprise_{t-1}$ is calculated using the value-weighted surprise of all firms that announced in the previous trading day, regardless of size. Panel B Column 1 examines a measure of $surprise_{t-1}$ equal to actual earnings minus median forecast, without scaling by lagged price. Column 2 scales $surprise_{t-1}$ by the sum of the squared size weights of each firm comprising the weighted-mean calculation of $surprise_{t-1}$. Columns 3 and 4 calculate own surprise and $surprise_{t-1}$ using the median of each analyst's most recent forecast released with the past 30 or 45 days, respectively, excluding days t and $t-1$. In Panel C, Column 1 uses returns in excess of the market and Column 2 uses standard characteristic adjusted returns without removing the firm announcing on t and firms included in the calculation of $surprise_{t-1}$ from the characteristic matched portfolio. Column 3 uses announcement dates based on the filters from DellaVigna and Pollet (2009). Column 4 re-estimates the baseline regression, but equal-weights each observation instead of of value-weighting. Standard errors are clustered by date. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Different Value-Weighted Measures

	Return $[t-1, t+1]$			
	(1)	(2)	(3)	(4)
Return $surprise_{t-1}$, VW mean	-0.0429*			
	(0.0237)			
$Surprise_{t-1}, > 85^{th}$ pctile		-0.949***		
		(0.224)		
$Surprise_{t-1}, > 95^{th}$ pctile			-0.894***	
			(0.259)	
$Surprise_{t-1}$, all firms				-0.734***
				(0.230)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
R^2	0.0817	0.0842	0.0813	0.0837
Observations	74927	79728	66484	75923

Panel B: Different Scaling and Forecast Windows

	Return $[t-1, t+1]$			
	(1)	(2)	(3)	(4)
$Surprise_{t-1}$, no price scaling	-0.0265***			
	(0.00695)			
$Surprise_{t-1}$, scaled std dev		-0.450***		
		(0.125)		
$Surprise_{t-1}$, forecasts [t-30,t-2]			-0.631***	
			(0.220)	
$Surprise_{t-1}$, forecasts [t-45,t-2]				-0.419**
				(0.198)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
R^2	0.0839	0.0837	0.0738	0.0735
Observations	75923	75923	121440	150040

Table 8
Continued: Alternative Measures of Surprise

Panel C: Different Return Measures

	Excess return	Char adj ret	Adjusted dates	Equal weighted
	(1)	(2)	(3)	(4)
$Surprise_{t-1}$	-1.000***	-0.721***	-0.823***	-0.240
	(0.256)	(0.197)	(0.264)	(0.168)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
R^2	0.0807	0.0747	0.0880	0.0748
Observations	76062	76062	63463	75923

Table 9
Heterogeneity

This table shows how contrast effects vary by size, analyst coverage of the firm announcing today, decade and day of the week. In Panel A Column 1, $surprise_{t-1}$ is interacted with indicators for five quintiles for the size (as measured in $t-3$, using quintile cutoffs of the NYSE index in that month). In Column 2, $surprise_{t-1}$ is interacted with indicators for the number of analysts covering the firm announcing earnings today (the number of distinct analysts that released forecasts in the past 15 days excluding day t and $t-1$). In Column 3, we estimate separate effects for each decade in the sample. All direct effects of size quintiles or number of analysts are included in the regression. In Panel B, regressions are estimated separately for observations corresponding to Monday announcements and other days of the week. Columns 1 and 2 examine the whole sample while columns 3 and 4 examine only the year 2000 and after. p-values are for the test of whether the Monday coefficient equals the coefficient for other days of the week. All other variables and weights are as defined in Table 2. Standard errors are clustered by date. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Heterogeneity by Size, Number of Analysts, and Decade

	Return $[t-1, t+1]$		
	(1)	(2)	(3)
$Surprise_{t-1}$ x size quintile 1	-0.393		
	(0.485)		
$Surprise_{t-1}$ x size quintile 2	-0.398		
	(0.478)		
$Surprise_{t-1}$ x size quintile 3	-0.391		
	(0.430)		
$Surprise_{t-1}$ x size quintile 4	0.200		
	(0.324)		
$Surprise_{t-1}$ x size quintile 5	-0.997***		
	(0.265)		
$Surprise_{t-1}$ x (num analysts = 1)		0.0726	
		(0.587)	
$Surprise_{t-1}$ x (num analysts = 2)		-0.793*	
		(0.477)	
$Surprise_{t-1}$ x (num analysts >= 3)		-1.027***	
		(0.279)	
$Surprise_{t-1}$ x 1980s			-0.663
			(0.419)
$Surprise_{t-1}$ x 1990s			-0.912
			(0.743)
$Surprise_{t-1}$ x 2000s			-0.883**
			(0.344)
$Surprise_{t-1}$ x 2010s			-0.997**
			(0.487)
Own $surprise_{it}$ controls	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
R^2	0.0842	0.0842	0.0839
Observations	75923	75923	75923

Table 9
Continued: Heterogeneity

Panel B: Heterogeneity by Day of Week

	Baseline sample		Year>=2000	
	(1) Mondays	(2) Other	(3) Mondays	(4) Other
$Surprise_{t-1}$	0.0759	-0.724***	-0.272	-0.767***
	(1.147)	(0.249)	(0.927)	(0.289)
p-value: Mondays = Other		0.490		0.605
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
R^2	0.186	0.0865	0.208	0.0958
Observations	7815	68108	3926	41317

Table 10
Industry Match

This table explores how contrast effects vary with industry match between the firm announcing earnings today and the firm announcing in the previous trading day. $Surprise_{t-1}$ *same ind* is the salient earnings surprise in $t-1$, calculated using only firms in the same industry as the firm announcing today. $Surprise_{t-1}$ *dif ind* is the salient earnings surprise in $t-1$, calculated using only firms in a different industry as the firm announcing today. To make the magnitudes of the coefficients on the $t-1$ salient surprises comparable, we scale each salient surprise by the sum of the squared size weights of each firm comprising the weighted-mean calculation. Small (large) firm is a dummy variable equal to one if the $t-3$ size of the firm announcing earnings today is below (above) the median NYSE market capitalization in that month. Columns with "Both $surprise_{t-1}$ non-missing" listed as Yes only include observations where same and different industry $surprise_{t-1}$ measures are non-missing. p-values are for the test of whether a given same-industry coefficient is equal to its different-industry analogue. All other variables and weights are as defined in Table 2. Standard errors are clustered by date. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Full sample			Small firms		Large firms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Surprise_{t-1}$ same ind	-0.418**	-0.334***	-0.417**	-0.565**	-0.662***	-0.417**	-0.417**
	(0.168)	(0.122)	(0.178)	(0.226)	(0.236)	(0.173)	(0.183)
$Surprise_{t-1}$ dif ind	-0.425**	-0.0365	-0.180	-0.151	-0.0545	-0.436**	-0.189
	(0.178)	(0.117)	(0.197)	(0.224)	(0.290)	(0.183)	(0.202)
Both $surprise_{t-1}$ non-missing	No	No	Yes	No	Yes	No	Yes
Regression weights	Value	Equal	Value	Value	Value	Value	Value
p-value: same=dif	0.978	0.112	0.386	0.232	0.129	0.944	0.421
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0840	0.0749	0.0879	0.0974	0.104	0.0854	0.0896
Observations	75923	75923	49300	33861	20829	42062	28471