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Calendar rotations: A new approach for studying the impact of timing using earnings announcements[☆]

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ABSTRACT

We develop a novel methodology for studying the causal impact of announcement timing. Our methodology uses firms' earnings announcements and leverages quasi-exogenous variation attributable to the specific day-of-week on which a calendar month begins. We refer to the resulting variation in announcement timing as "calendar rotations," which are uncorrelated with proxies for announcement content. In applying our methodology, we show announcements moved forward by calendar rotations receive heightened media and investor attention, and experience greater earnings announcement premia. Taken together, our study details a method for studying how the timing of information flows impacts outcomes of interest to financial economists.

1. Introduction

This study introduces and implements a novel methodology for studying the impact of the timing of information flows. We do so by leveraging what we refer to as 'calendar rotations,' which reflect quasi-exogenous variation in the timing of certain prescheduled information events driven by the specific day-of-week on which a calendar month begins. We apply our proposed methodology using firms' earnings announcements and, in doing so, offer important insights for the vast literature that studies the role of information flows on market outcomes. Specifically, we demonstrate and calibrate how the sequence of information arrival shapes the behavior of information intermediaries and the dynamics of stock prices.

The timing of information flows is a central construct in many models of, and predictions for, outcomes of interest to financial economists. For example, several theories

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predict the sequence of news announcements shapes investors' reaction to news and the formation of market prices (e.g., Guttman et al. 2014; Hartzmark and Solomon 2018). Empirical tests of these theories more often focus on describing what actions managers or investors take, or how they correlate with market outcomes, rather than identifying the specific role played by timing. One likely reason is that it is challenging to identify the impact of timing in most settings because announcement timing is typically either fixed (i.e., static over time) or endogenous to its information content. We address this challenge by developing a novel methodology that relies on quasi-exogenous variation to isolate the impact of announcement timing using firms' earnings announcements.

An ideal laboratory for researching the impact of announcement timing would involve randomly assigning firms to different announcement dates, and studying how the randomized timing maps into outcomes of interest. In lieu of a randomized experiment, we propose an approach that leverages quasi-exogenous variation in firms' earning announcement timing attributable to the specific day of the week on which a calendar month begins, which changes across calendar years outside managers' control.

The day-of-week on which a month begins is relevant because firms commonly adhere to simple patterns for their earnings announcement dates (e.g., Amazon, Boeing, and Cisco). For example, some firms regularly announce earnings on the first Thursday of a given month, whereas others regularly announce on the first Tuesday. Our methodology exploits the fact that the ordering of the first Thursday versus first Tuesday rotates exogenously across calendar years. Specifically, if a month begins on Wednesday or Thursday, the first Thursday occurs before the first Tuesday; otherwise, the ordering reverses (see Section 2 for details).

We use the term calendar rotations to refer to changes in the timing of prescheduled announcements driven by the day-of-week on which a month begins. To study the impact of calendar rotations, we first identify firms that consecutively follow patterns of earnings announcement timing (see Section 2 for examples). We treat a firm as a "Pattern firm" if it has followed a pattern for at least four consecutive same fiscal quarters, though our results do not appear highly sensitive to this choice. Although a deviation from a long-standing announcement pattern can be due to non-strategic reasons (e.g., holidays or availability of major stakeholders), we conservatively drop all firm quarters that deviate from the pattern by even one day. Across quarters, this approach identifies between 10–25% of firms in the Compustat database (see Section 3.1 and Appendix A).

The earnings announcement patterns we study are sometimes required by firms' bylaws. For example, consider Emerson Electric, which is required under its bylaws to hold shareholder meetings on the first Tuesday of every month (see Appendix B for an excerpt from Emerson's bylaws). Emerson schedules earnings announcements to conform to this Tuesday requirement, and thus the timing of its announcements relative to other firms likely depends on the day-of-week on which a month begins.[1] Of course, the reorderings of firms' announcements due to calendar rotations are only quasi-exogenous because firms often have the option to deviate from their past patterns, which we discuss and address below.

Our study proposes a novel methodology. Our discussion and empirical tests are guided by the idea that studies proposing a methodology should satisfy the following criteria: (C1) "Conceptual Soundness": the study should justify the measure from first-principles (e.g., the proposed approach reduces measurement error based on a statistical or economic theory) and articulate shortcomings of any common alternative methods, (C2) "Empirical Validity": the tests should implement and substantiate that the method works as intended (i.e., the method works as theory would predict), and (C3) "Inference Materiality": the evidence should illustrate the importance of the measure for researchers' inferences (e.g., researchers' inferences hinge upon the proposed methodology relative to alternatives).

Using a sample of 19,252 firm quarters spanning 2004–2017, we construct measures of firms' earnings announcement timing, referred to as "*EA Order*", that capture the relative ordering of announcements for different firms. Specifically, *EA Order* equals the ratio of n over N, where N is the total number of firms in a given comparison group and n is the ranking of a given firm's announcement date among N firms. To account for both firm- and fiscal-quarter-specific reporting behaviors, our main tests rely on within-firm, fiscal-quarter-matched changes in *EA Order*, which we denote as Δ*EA Order*. Positive values of our Δ*EA Order* indicate a delay in the firm's earnings announcement timing driven by calendar rotations, relative to peer firms, whereas negative values indicate an acceleration.

Readers could be initially concerned that firms might strategically deviate from past earnings announcement patterns. To address this concern, we rely on the insight that systematic deviations should induce correlations between changes in *EA Order* among Pattern firms and attributes of their earning news. For example, to the extent firms delay announcements with bad news, as suggested in Johnson and So (2018b), they could deviate from past announcement patterns when reporting poor performance, which would make firms with bad news moved earlier by calendar rotations underrepresented in our Pattern sample relative to firms with good news moved earlier by calendar rotations. This type of under-representation could confound our methodology by inducing a mechanical correlation between changes in *EA Order* and the nature of firms' earnings news.

To address this concern, we start our analyses with validation tests showing that changes in *EA Order* driven by calendar rotations are unrelated to various proxies for the news content of firms' earnings announcements. In fact,

[1] Large-sample data on firms' bylaws are not readily available and we are not aware of a way to systematically identify firms with a similar day-of-week requirement.

when regressing Δ*EA Order* on changes in news proxies and firm characteristics, we find the corresponding R^2 is not significantly different from zero, and we are unable to reject the null that *all* coefficients are jointly equal to zero. These findings provide support for the idea that calendar rotations help isolate the impact of an announcement's timing, rather than its news content. Moreover, our validation tests mitigate concerns that selection effects (e.g., strategic deviations from patterns) or sample biases induce a statistical link between Δ*EA Order* and changes in firms' earnings news in our main sample of Pattern firms.

We also aggregate and study a second sample of "non-Pattern" firms excluded from our main sample of Pattern firms, whose announcement dates are more likely endogenously selected by the firm. In stark contrast to the findings from our main sample of Pattern firms, we find Δ*EA Order* is strongly correlated with changes in firms' earnings news among the non-Pattern sample, consistent with prior research showing firms accelerate good news and delay bad news (e.g., Penman 1987; Bagnoli et al. 2002).

Our main set of tests illustrates how researchers can apply our methodology, specifically, by studying the impact of calendar rotations on the attention firms receive when announcing earnings. We begin this set of tests by studying the impact of announcement timing on media coverage as a summary proxy for attention, which prior studies link to asset pricing, managerial outcomes, and liquidity (e.g., Core et al. 2008; Fang and Peress 2009; Solomon et al. 2014). Our tests are motivated by the idea that earlier announcements likely contain more 'novel' news — particularly about the general state of the economy or industry — as they are less likely to be preceded by announcements of other firms. Thus, to the extent "novel" earnings news attracts more readership (e.g., Gentzkow and Shapiro 2010), we predict early announcements are covered more by the media than later ones. Consistent with this prediction, we find firms' earnings announcements moved earlier by calendar rotations receive greater media coverage. For the average firm, our results suggest a one-standard-deviation acceleration in a firm's *EA Order* elicits a 7–to–8% increase in media coverage.

We contrast the findings from our sample of Pattern firms with opposite results from non-Pattern firms whose announcements dates are more likely endogenously determined. Specifically, we find non-Pattern firms with delayed announcements receive *greater* media coverage, even when controlling for standard proxies for firms' earnings news, consistent with firms delaying bad news announcements and the media preferring to cover negative stories (e.g., Niessner and So 2017). These findings underscore the importance of our methodology for isolating the impact of timing, and suggest that simple regressions of outcomes on timing measures in broad samples are likely to yield biased inferences, even if the researcher attempts to control for standard proxies of announcement news.

We also study the impact of calendar rotations on investors' attention as proxied by their trading activity. We show firms' shares are more frequently traded when their announcements are moved forward by calendar rotations, and less frequently traded when delayed. These findings dovetail nicely with our media-based tests, and are consistent with timing shaping the level of attention firms receive from investors when announcing earnings.

We then apply calendar rotations to study the drivers of earnings announcement premia, i.e., the tendency of stock prices to rise around firms' announcements. Prior research suggests earnings announcement premia are driven in part by short-sale constrained investors who bid up prices in response to heightened attention associated with the announcements (e.g., Frazzini and Lamont 2007; Barber and Odean 2008). The use of calendar rotations is helpful for studying this mechanism because, in most settings, it is challenging to isolate cross-sectional variation in attention that is unrelated to the nature of firms' earnings news.

In applying our methodology, we find firms moved earlier by calendar rotations experience greater announcement premia. Specifically, firms in the highest versus lowest quintile of Δ*EA Order* experience a smaller three-day announcement premia of roughly 40 to 60 basis points, which intuitively aligns with our evidence that accelerations of earnings announcements due to calendar rotations lead to increased investor attention as proxied by higher coverage from the financial press and heightened trading volume. We also find returns predictably reverse following firms' announcements, consistent with announcement premia reflecting, in part, transitory mispricing stemming from periods of heightened investor attention.

Our findings on the earnings announcement premium are important for several reasons. First, the magnitude of our findings suggest a substantial portion of the premium is attributable to attention itself, and thus provides a reasonable micro-foundation for the volume channel discussed in Frazzini and Lamont (2007). Similarly, our paper builds on the highly influential "all-that-glitters" mechanism for investor behavior highlighted in Barber and Odean (2008) by providing corroborating evidence using a more cleanly identified shock to investor attention. Finally, more broadly, our findings suggest variation in attention, and the attendant trading volume and price pressure, are important for explaining other anomalies surrounding recurring events such as calendar- and dividend-based return seasonalities [see Hartzmark and Solomon (2018) for a helpful review of this literature].

In additional analyses, we show our main findings hold even for firms that do not change the number of days between their announcements and fiscal period end. By construction, these cases arise for Pattern firms when *other firms'* announcement dates change due to calendar rotations, rather than the focal firm taking an action. These findings mitigate potential concerns that our results are driven by variation in the speed, rather than the ordering, with which firms announce earnings. Additionally, we show our main results are concentrated earlier on in "earnings seasons," consistent with calendar rotations having greater impacts when market participants have less information about secular economic trends.

An important caveat to our inferences is that our methodology favors identification at the expense of generalizability. Specifically, our methodology relies on studying Pattern firms, which tend to be larger, and have greater institutional ownership and analyst coverage, and thus likely face higher costs of changing announcement dates. These firms incur costs coordinating rescheduled announcements with key stakeholders such as analysts and investors, as well as their own management team needed for conference calls bundled with the announcement, which is helpful in understanding why Pattern firms do not selectively alter announcement dates to capture potential benefits suggested by our findings. These costs also suggest calendar rotations elicit a localized (rather than broadly applicable) treatment effect on timing, particularly among firms in which rescheduling costs are high.

In the final section of our paper, we show our results are robust to a variety of alternative implementations and assumptions. For example, we provide corroborating results using matched samples of Pattern and non-Pattern firms (i.e., that possess similar earnings surprises, size, analyst coverage, and institutional ownership), mitigating concerns that differences in our results for Pattern versus non-Pattern firms reflect the two subsets of firms being compositionally distinct. We also show our results are robust to alternative approaches for grouping firms (e.g., by fiscal quarter end versus calendar quarter of the announcement). Finally, we illustrate how researchers can apply calendar rotations to study alternative types of variation in earnings announcement timing (e.g., within-industry variation).

In sum, our study develops a novel approach for identifying quasi-exogenous variation in earnings announcement timing, and thus is ideally suited for research questions in which separating the effects of an announcement's timing from its content is of first-order importance. In applying our methodology, our findings suggest prior evidence of firms' strategically timing their announcements stems, in part, from managers attempting to influence media coverage and stock price dynamics, which prior research links to career outcomes and litigation risk (e.g., Malmendier and Tate 2009; Graham et al. 2005).

The implications of announcement timing, however, are likely much broader than the outcomes we consider here. By altering the sequence and distribution of information arrival in the economy, calendar rotations are relevant for a variety of constructs including intra-industry information transfers (e.g, Foster 1981; Hartzmark and Shue 2018) non-diversifiable risks (e.g., Barth and So 2014; Savor and Wilson 2016), the role of monitoring (e.g., Bhaskar et al. 2019), and investors' reactions to news (e.g., Hirshleifer et al. 2009). Our evidence suggests, however, that testing the impact of timing on these outcomes is hampered in broad samples where timing is endogenously determined. Our hope is that by providing an alternative methodology, our study will spur research furthering our understanding of how timing shapes outcomes of interest to financial economists.

2. Identification strategy

2.1. Overview and example

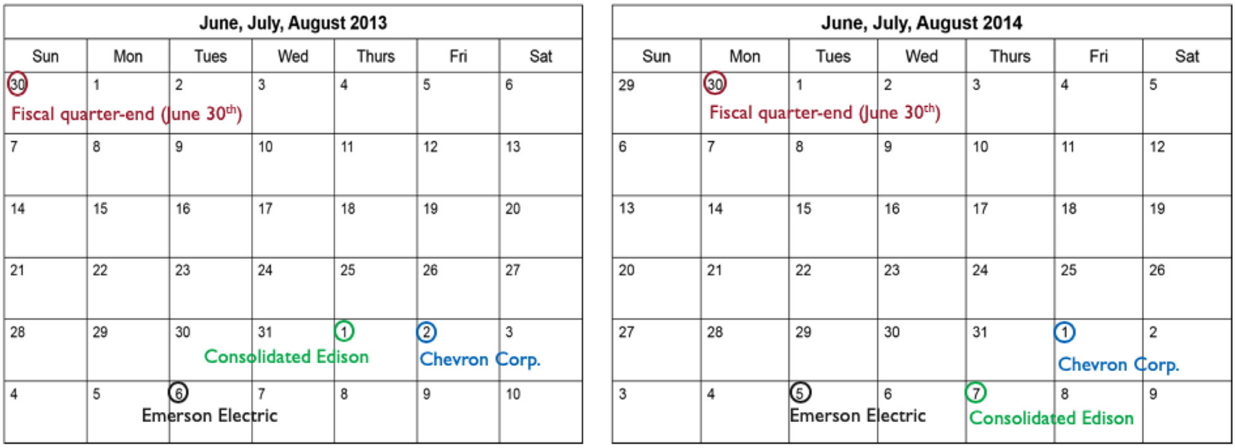
A key challenge involved in identifying how the timing of earnings announcements impacts market outcomes arises from the fact that within-firm variation in earnings announcement timing is typically endogenously determined by the corresponding earnings news, which also affects market outcomes. Therefore, it is challenging to draw causal inferences about the relation between earnings announcement timing and market outcomes.

We help address this empirical challenge by focusing on firms that tend to adhere to a specific announcement pattern and that are unlikely to change their earnings announcement schedule based on their earnings news. Specifically, we examine firms that consistently follow quarter-specific earnings announcement patterns (e.g., announcing on the first Thursday of a month) and thus are subject to variation in timing driven by the day-of-week on which a calendar month begins. In Appendix A, we list the set of potential patterns and their relative frequencies in tabular form. For instance, about 7% of our sample announce earnings in the *k*th weekday of a month (say, the first Thursday in August) whereas another 7% of firms announce earnings on the *k*th weekday since the end of the fiscal quarter.

Calendar rotations stem from the fact that when dividing 365 (i.e., the number of days in a non-leap year) and 366 (i.e., the number of days in a leap year) by 7 (i.e., the number of days in a week), the remainders are 1 and 2, respectively. Thus, the day-of-week corresponding to any date moves one day-of-week later each non–leap calendar year and two days-of-week later each leap calendar year. For example, if June 1st lands on a Wednesday, it will land on Thursday next year if it is a non-leap year, and on Friday if it is a leap year. The duration of each calendar rotation cycle is 28 years consisting of 21 non–leap years and 7 leap years. Over this cycle, a given date (e.g., June 1st) is on each day-of-week four times (i.e., Sunday four times, Monday four times, Tuesday four times, etc.).

As an illustration of calendar rotations, consider the example involving three firms presented in the graphic below corresponding to 2013 and 2014. These three firms operate in different industries, but their earnings likely contain some common information about energy consumption. Chevron Corporation has consistently announced earnings on Friday of the fourth week after every fiscal quarter end since 2002 Q4. Similarly, Consolidated Edison consistently announced earnings for its second fiscal quarter on the first Thursday of August since 2010, and Emerson Electric has also announced earnings on the first Tuesday of the second month after every fiscal quarter end since 2002 Q2.

Although these firms have the same fiscal quarter end date of June 30th, their different patterns give rise to year-to-year variation in announcement ordering. Each side of the graphic spans dates within the months of June, July, and August to illustrate how a change in the day-of-week corresponding to the first date of August shifted the ordering of earnings announcements from 2013 to 2014.



The year-to-year variation stems from the fact that the ordering depends on the day of the week on which a month begins. For example, in 2013, Consolidated Edison announced earnings first on August 1st (which corresponds to the first Thursday of August), followed by Chevron on August 2nd (which corresponds to Friday of the fourth week after quarter end June 30th), and finally, Emerson Electric on August 6th (which corresponds to the first Tuesday of the second month after quarter end June 30th).

However, in contrast to the 2013 ordering, in 2014 Chevron announced first on August 1st, followed by Emerson Electric on August 5th, and then Consolidated Edison on August 7th. The change in the sequence of announcements from 2013 to 2014 stemmed from the fact that the first Thursday of August came before both the first Tuesday of August and Friday of the fourth week after June 30th in 2013, but by contrast came after in 2014.

Calendar rotations create year-to-year variation in the *absolute* number of days between a firm's fiscal period-end and earnings announcement, as well as variation in the *relative* ordering of announcements across firms. In implementing our methodology, we focus on relative ordering as it better aligns with the predictions we seek to test. However, in other settings (e.g., studying the implications of investors having less time to anticipate firms' news), researchers might want to focus on variation in the absolute number of days.

2.2. Implementation

Creating measures of relative announcement timing requires selecting the units of analysis (e.g., firms vs. days) and comparison groups (e.g., firms with shared fiscal periods vs. the calendar quarter of their announcements). To guide these decisions, Fig. 1 details the distributions of earnings announcements in our sample across calendar days.

Panel A of Fig. 1 plots the frequency of announcements across calendar days regardless of firms' fiscal period end. It shows the frequency plots of earnings announcements by day resemble a series of normal distributions. This normality indicates many more firms announce on days toward the middle of each distribution than either of its tails.[2]

Our predictions regarding investor attention depend on how much similar information investors learn from other sources, including peer firms' earnings announcements. Thus, we measure relative announcement timing based on the number of firms' announcements that precede a given firm's announcement to account for more firms announcing closer to the median date. By contrast, measuring relative ordering based on the number of days would implicitly treat the extent of investor learning on days on either tail of the distribution (where there are fewer announcements) as the same as days in the middle of the distribution (where there are many announcements).[3]

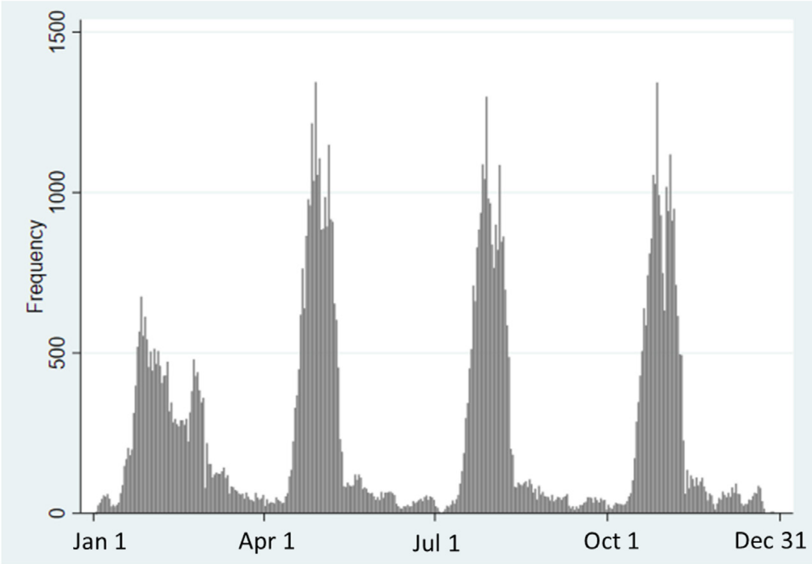
Measuring relative announcement timing also requires deciding whether to group firms by fiscal quarter end versus the calendar quarter of their announcements. To guide this decision, Panel B of Fig. 1 again plots the frequency of announcements across days, but uses two colors to illustrate the relation between firms' fiscal period ends and announcement timing. The blue bars represent announcements corresponding to fiscal quarters aligned with calendar quarters (i.e., ending in Dec., Mar., Jun., and Sep.), whereas the red bars represent all non-standard fiscal quarter end dates (i.e., ending in Jan., Feb., Apr., etc.).

The use of red versus blue bars in Panel B of Fig. 1 shows announcement dates tend to intuitively cluster by fiscal period ends, but also that these clusters often overlap in time and thus cohabit the same calendar quarter despite corresponding to different fiscal periods. This cohabitation helps explain why our main tests rely on relative rankings based on fiscal period end rather than the calendar quarter of the announcement. Specifically, the information investors glean about shared macroeconomic trends from earnings announcements likely depends on their corresponding fiscal period closing date. This is

[2] In our sample, the vast majority of firms (∼75%) use December fiscal year ends. This explains why announcements in the first calendar quarter are more dispersed, as firms face heightened auditing requirements and show greater variation in announcement timing for their fiscal year ends.

[3] In our Online Appendix, we find results similar to our main inferences, but statistically weaker, when using measures of announcement timing based on days rather than firms.

Panel A: All sample firms



Panel B: Standard vs. non-standard fiscal quarter ends

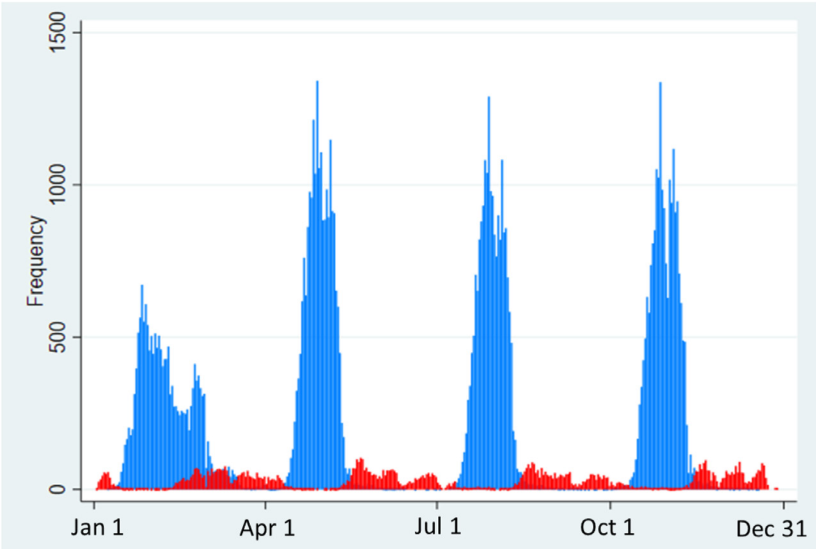


Fig. 1. Frequencies of earnings announcements by date.
Panel A plots frequencies of earnings announcements in our sample across all days in a calendar year. Panel B plots frequencies of earnings announcements for fiscal quarters with "standard" end dates in blue (i.e., Mar. 31, Jun. 30, Sep. 30, and Dec. 31) and others in red. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

because each fiscal period corresponds to a different cross-section of economic activity.

As a motivating example, consider that earnings for firms whose fiscal periods end on November 30 are less likely to convey information about holiday sales concentrated in December. In this case, grouping by calendar quarters likely mismeasures some preemption of common earnings information by implicitly assuming that announcements in early January (corresponding to November fiscal period ends) preempt the information of announcements in late January (corresponding to December fiscal period ends).[4] Because our hypotheses center on this type of information preemption, our main tests rely on groupings based on fiscal period end, though we also

[4] Grouping by calendar quarter is also prone to mismeasure the leakage of common earnings information when firms' announcements span adjacent quarters despite sharing a common fiscal period end. This can happen, for example, if a subset of firms with fiscal quarter ends in January or February announce earnings in March and the remaining announce at the outset of the next calendar quarter beginning in April.

discuss below a series of alternative implementations and robustness tests.

Our main tests rely on the earnings announcement timing measure "*EA Order*," defined as the simple ratio of n over N, where N is the number of firms with the same fiscal period end and n is the ranking of a given firm's earnings announcement timing among N firms. We do not differentially rank earnings announcements on the same day based on their time-of-day because the time is more likely discretionary and difficult to measure cleanly (e.g., due to missing timestamps and/or gaps in the timing of initial press releases vs. detailed discussion during conference calls).[5] Thus, consistent with standard competition rankings, we assign the same ranking to all of the firms announcing earnings on the same day.

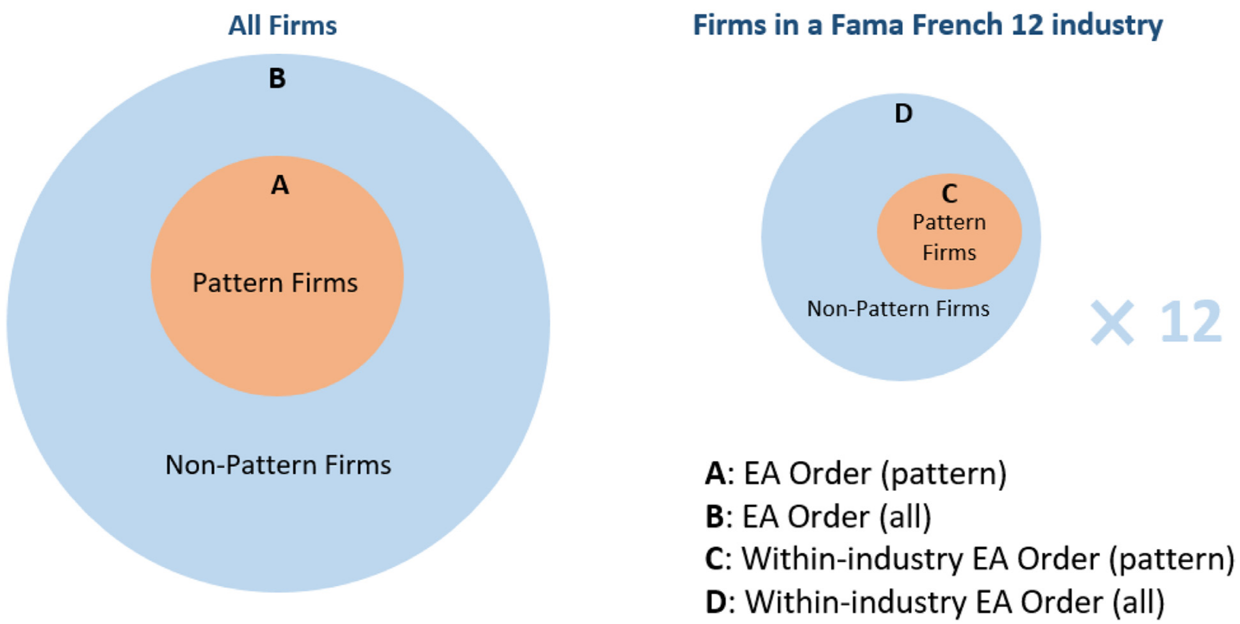
The appropriate definition of N in *EA Order* could depend on the particular research question. For example, when focusing on information arrival in a general sense, the appropriate N could be the number of all firms in the economy. However, depending on the setting, researchers could choose to define N using all firms, or only Pattern firms.

In implementing our methodology, our two primary versions of *EA Order* are illustrated in the left side of the graphic below. The first is "*EA Order (pattern)*," where we measure N as all firms that have followed a pattern for at least four consecutive fiscal quarters; it is depicted by the inner circle. The second is "*EA Order (all)*," which we define across all firms regardless of whether they have followed a pattern, where N is depicted by the outer circle.

In Section 7, we consider two categories of robustness tests that alter the comparison group based on (1) which firms are treated as relevant, and (2) when firms announce. In the first, we calculate versions of *EA Order* that defines the relevant comparison group as industry peers, which helps illustrate how researchers can align their research question and empirical implementation. For example, if a research question focuses more on industry-specific issues such as studying intra-industry information transfers, then the appropriate N is likely the number of firms in the same industry.

To focus on within-industry variation, in one of our robustness tests, we create and test a version of the relative timing measure "*Within-Industry EA Order (pattern)*," defined across all firms in the same Fama-French 12-industry that have followed a pattern for at least four consecutive fiscal quarters, and depicted by the inner circle on the right side of the graphic below. Similarly, "*Within-Industry EA Order (all)*" is an analogous measure defined across all firms in the same Fama-French 12-industry regardless of whether they have followed a pattern, where N is depicted by the outer circle on the right side of the graphic below.

As a second type of robustness test, we also re-examine our main findings when grouping firms based on when they announce. Specifically, we group by the calendar quarter of firms' announcements to more closely mimic the ordering within a "calendar-quarter earnings season" (e.g., Jan. through Mar.). These tests are important because grouping by fiscal period end may mismeasure information transfers when firms announce earnings for periods with a



[5] This approach results in a gap in the numerator of *EA Order* between days that is equivalent to one less than the number of firms announcing on that day. To avoid the noise in our ranking measure due to the change in denominator N, we only consider firm quarters that exist both in the current quarter and the same quarter of the previous year. That is, $\Delta EA\ Order = \Delta\frac{n_t}{N_t} = \frac{n_t - n_{t-1}}{N_t}$ because $N_t = N_{t-1}$.

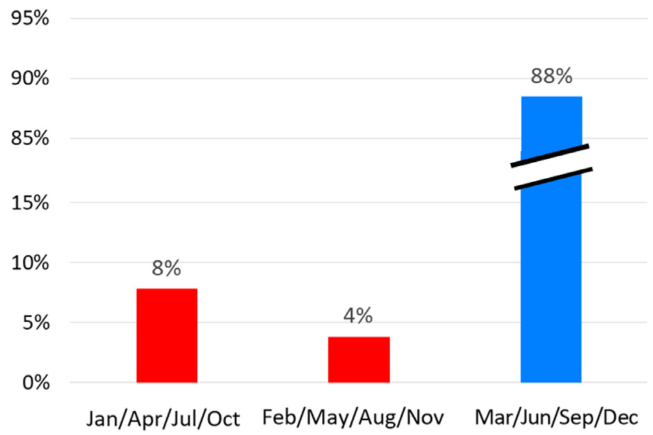


Fig. 2. Percentage share of fiscal quarter ends.
This chart shows the percentages of firms in our sample based on their fiscal cycles. Fiscal cycles indicate the timing of their quarterly fiscal period ends.

high degree of overlap despite different fiscal period ends. For example, grouping by fiscal period ends could create measurement error by implicitly ignoring information on secular trends conveyed by firms with December 31st fiscal period ends announcing in January, which preempts information from firms with January 31st fiscal period ends announcing in February.

To help assuage potential measurement concerns when grouping firms by fiscal quarter, Fig. 2 plots the percentage of firms in our sample based on their fiscal cycle, which shows 88% of firms have fiscal periods that aligns with calendar quarters.[6] Because the overwhelming majority of firms announce earnings within three months of their fiscal period end, the evidence in Fig. 2 suggests versions of *EA Order* are likely highly correlated when grouping firms by the calendar quarter of their announcement versus fiscal period end.

In Section 7, we verify that versions of *EA Order* when grouping by fiscal period end versus calendar quarter are highly correlated and yield similar inferences. Similarly, we show our main inferences are robust to limiting our sample to firms with standard fiscal period ends, which creates four cleanly identified earnings seasons and thus makes grouping by fiscal quarter ends effectively the same as grouping by calendar quarter. These tests mitigate concerns that our inferences are sensitive to a particular implementation.

Regardless of which version we implement, *EA Order* is by construction always greater than zero and less than or equal to one. Larger values of *EA Order* signal the earnings announcement is later relative to other firms in the same comparison group. Because we exploit quarter-specific changes in the timing of the announcements, we construct a within-firm, fiscal-quarter-matched, change-based measure, Δ*EA Order*, which is the difference between *EA Order* for the current fiscal quarter and that for the same fiscal quarter of the previous year. Positive values of Δ*EA Order* indicate a delay in the relative announcement timing (i.e., later), whereas negative values indicate an advancement (i.e., earlier).

3. Sample selection and validation tests

In this section, we first describe our sample selection process and then discuss tests aimed at validating our proposed methodology.

3.1. Sample selection

We obtain data for this study from four main sources. We use Compustat for firm-level characteristics, CRSP for stock-related variables, I/B/E/S for analyst-based market variables, and RavenPack News Analytics Dow Jones Edition for media-initiated articles.

Because we use quasi-exogenous variation in earnings announcement timing as our identification strategy, the accuracy of the earnings announcement dates is critical. Both I/B/E/S and Compustat provide firms' earnings announcement dates. To maximize the number of observations, we use earnings announcement dates available in I/B/E/S for firms with missing earnings announcement dates in Compustat, and vice versa.

In most cases, announcement dates in I/B/E/S and Compustat are identical. Following the approach taken by Dellavigna and Pollet (2009), we use the earlier of the two if the announcement dates differ. We do so because the latter date likely reflects the date of publication in *The Wall Street Journal* or the earnings being released after trading hours on the previous day (Dellavigna and Pollet (2009)). Dellavigna and Pollet (2009) show this approach yields an accuracy rate of greater than 95% and Johnson and So (2018a) show these corrections are important for studying capital market reactions to earnings announcements.

To create the sample of Pattern firms used in our main tests, we start with firms that appear to follow a pattern for same quarter-specific earnings announcements

[6] The percentages in Fig. 2 are comparable to those for the sample of all Compustat firms. For example, approximately 88% of Compustat firms also have fiscal periods that align with calendar quarters.

(see Appendix A for the types of patterns we consider). Although a firm could deviate from a long-standing announcement pattern for non-strategic reasons (e.g., the availability of major stakeholders, holidays), we conservatively drop all firm quarters that deviate from the past pattern by even one day. Moreover, because establishing or switching patterns could be driven by the content of the announcement, we restrict our 'Pattern' sample to firm quarters that have followed the same earnings announcement pattern for at least four consecutive same-quarters. For example, if a firm has followed the same announcement pattern for ten consecutive same quarters from 2004 to 2013, we drop the first three quarters and keep the last seven quarters. This choice reflects the tradeoff of sample coverage in favor of the precision of our inferences.

Appendix A presents firm-level summary statistics of the Pattern sample and the non-Pattern sample consisting of firms not included in the Pattern sample. non-Pattern firms are those that have existed for at least four consecutive same fiscal quarters but have not followed an announcement pattern or have followed an announcement pattern for less than four consecutive same fiscal quarters. As shown in Appendix A, firms following announcement patterns have larger market capitalizations, a higher proportion of institutional investors, and a greater number of analysts following. This suggests these firms likely have higher costs involved in changing their earnings announcement dates based on earnings news, which requires rescheduling with many stakeholders, such as analysts and institutional investors. We also provide evidence, however, that differences in our main findings across Pattern versus non-Pattern firms are unlikely to simply reflect the two subsets of firms being compositionally distinct.[7]

The figure in Appendix A shows the number and the percentage of firms following earnings announcement patterns defined by the threshold of four consecutive same-quarters have increased over time, which highlights the increasing usefulness of our proposed methodology. The figure also suggests the threshold of four consecutive same-quarters is likely a good midpoint between robust identification and sample coverage. Specifically, consecutive-four-quarter Pattern firms account for roughly 10–25% of firms in our sample, compared to 5–15% and 20–40% if we instead relied on consecutive three- and five-quarter patterns.

Panel A of Table 1 details the effect of the calendar rotations on the absolute number of days between firms' fiscal period end dates and earnings announcement dates, as well as Δ*EA Order*. Continuing with our earlier examples from Section 2, the most common scenario is illustrated by the experience of Chevron Corp. in August 2013–2014, where firms have one day less between their fiscal period-end and earnings announcement. This type of change is common because the weekday corresponding to any date of the year moves one weekday later each year (or two weekdays later in leap years). Thus, the number of days between the fiscal period end date, which also varies with calendar rotations, and the earnings announcement date for firms that follow a day-of-week pattern often decreases by one day (or two days in leap years). In relative terms, Panel A shows that these cases of a one-day advance correspond to an average Δ*EA Order* of −0.02 and −0.05 depending on the measure, which translates to a firm announcing earnings before an additional 2% and 5% of comparison firms relative to the same quarter in the prior fiscal year.

Our methodology also commonly identifies delays of six days due to calendar rotations, which is illustrated by the experience of Consolidated Edison in August 2013–2014 as detailed in Section 2. The six-day change corresponds to cases, for example, where a firm with a first-Thursday earnings announcement is delayed by calendar rotations because the beginning of the month shifts from Thursday to Friday and thus delays when the first Thursday occurs (a similar case will result in a five-day change in leap years). This type of delays introduces an average Δ*EA Order* between 0.16 and 0.21, which translates to a firm announcing earnings after an additional 16% and 21% of comparison firms relative to the same quarter in the prior fiscal year. The remaining types of changes (i.e., 0, 1, 2, and 3 days) occur much less frequently and are driven by firms adhering to an announcement pattern that is not based on a particular day of the week (see the first table in Appendix A for examples).

Panel B of Table 1 reports descriptive statistics of key variables, including the two measures of *EA Order*. The mean value of *EA Order (pattern)* is close to 0.50, which is as expected. On the other hand, the mean value of *EA Order (all)* is less than 0.5, suggesting that Pattern firms tend to announce earnings earlier than non-Pattern firms. In addition, the means and medians of Δ*EA Order* range between −0.018 and −0.04, consistent with Table 1 Panel A, suggesting our methodology primarily identifies one- or two-day advances in earnings announcement timing.

After collecting firm-level accounting data from Compustat and stock-market data from CRSP for these firm quarters, we obtain newspaper and newswire coverage data from RavenPack News Analytics Dow Jones Edition (DJ), which collects news from Dow Jones Newswires, *Barron's*, MarketWatch, and regional editions of *The Wall Street Journal*. Recent studies show that DJ contains the largest coverage of financial news received by retail and institutional investors (Tetlock 2007; Tetlock 2011, Chan 2003). Although DJ does not represent all media coverage, prior studies show that DJ and Factiva are highly correlated (e.g., Drake et al. 2014), suggesting that it is a reasonable proxy for the extent of news coverage.

Although prior papers show that editorial discretion is exercised not only at newspapers but also on newswires (Tetlock 2007; Li et al. 2011; Twedt 2016), we only include media-generated articles that have at least one full paragraph or tabular data to mitigate the concern that the decision to produce news flashes is unlikely to be

[7] Although our main analyses compare Pattern firms to the full sample of non-Pattern firms, we also provide results in Table 9 corroborating our main inferences using characteristic-matched samples (i.e., that possess similar earnings surprises, size, analyst coverage, and institutional ownership).

influenced by journalists' desire to reach broader readership.[8] The data for DJ starts in 2000, but it significantly expands in 2004. For example, the number of firms covered jumps by 1,496 (22.9%) from 2003 to 2004 and remains roughly the same after 2004. Hence, we restrict our sample to firm quarters whose quarter end dates take place in or after 2004.

Due to restrictions imposed by data availability for earnings announcement dates, earnings surprise proxies, and control variables, our 'Pattern' sample consists of 19,252 firm quarters that span 2004–2017. The samples we use in market-based tests are slightly smaller, because the tests require additional data on daily stock returns and volume.

3.2. Validation tests

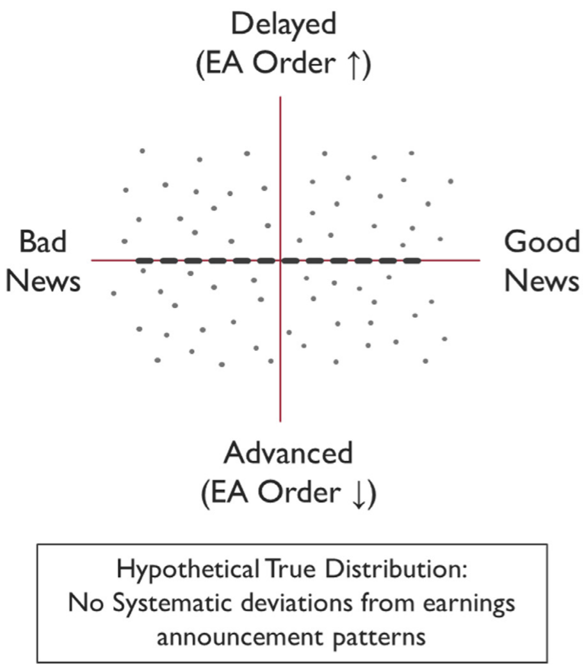
As noted in the introduction, calendar rotations are only quasi-exogenous because firms could adhere to their past behavior depending on their earnings news. To address concerns that firms strategically deviate from past patterns, we rely on the insight that systematic deviations should induce correlations between changes in *EA Order* among Pattern firms and attributes of their earning news. To see why, consider the graphic below. The left side depicts a hypothetical "true" distribution of firms' earnings relative to Δ*EA Order* when firms do not strategically deviate. Because year-to-year changes in the calendar occur exogenously, the true distribution should show no relation between Δ*EA Order* and changes in firms' earnings news, which is depicted by the flat dotted line.

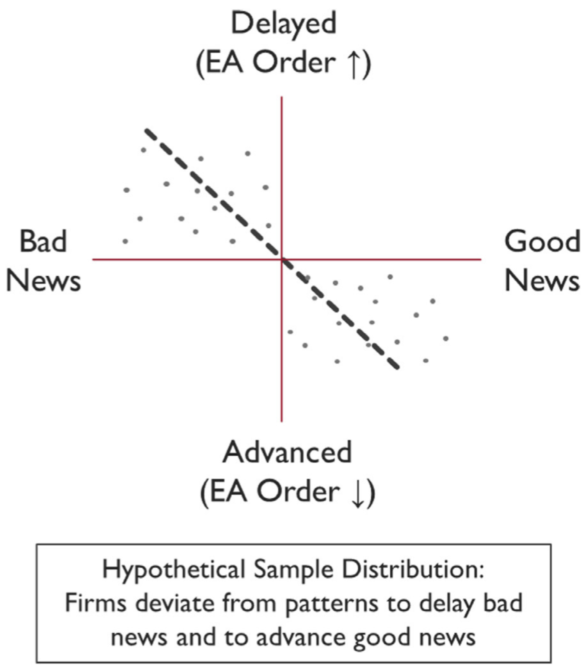
The right side of the graphic below depicts a hypothetical distribution of Δ*EA Order* and firms' earnings news when firms strategically deviate. To the extent firms delay bad news announcements, they could deviate from past announcement patterns when reporting poor performance, which would make firms with bad news whose announcement is advanced in time by calendar rotations underrepresented in our Pattern sample relative to firms with good news advanced in time by calendar rotations. A parallel argument applies if firms deviate from patterns to advance good news. These forms of underrepresentation would induce the downward sloping dotted line depicted on the right side of the graphic below.

To ensure that our methodology isolates variation in timing, rather than content, we explore the potential for these selection effects in our sample. Specifically, we conduct validation tests to demonstrate that firms' earnings announcement timing for the Pattern sample is unrelated to common proxies for its news content. Because we exploit fiscal *quarter-specific* earnings announcement patterns, we run a first-difference regression model as follows:

$$\Delta EA\ Order_{i,q} = \sum \beta \cdot \Delta NewsVariables_{i,q} + \sum \beta \cdot \Delta PerformanceVariables_{i,q} + \epsilon_{i,q}, \tag{1}$$

where change (Δ) is defined as the difference between the current fiscal quarter and the same fiscal quarter of the previous year. Although the first-difference regression model controls for time-independent firm quarter effects,





[8] We focus on press releases with relevance scores greater than 75, which DJ defines as being significantly related to a given firm. Our results are robust to requiring a relevance score equal to the max of 100.

Table 1
Summary statistics.
The sample consists of firm quarters from 2004 to 2017 that have followed the same earnings announcement patterns for at least four consecutive same fiscal quarters. See Appendix C for variable definitions. All continuous variables, except for stock returns, are winsorized at the 1% and 99% levels to limit the influence of outliers. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Impact of calendar rotations				
		N	Avg ΔEA Order (pattern)	Avg ΔEA Order (all)
ΔNumber of days between fiscal quarter end and EA	−2	3531	−0.05	−0.06
	−1	12,637	−0.02	−0.05
	0	842	0.05	0.01
	1	57	0.10	0.03
	2	133	0.00	−0.01
	3	27	0.03	−0.01
	5	961	0.17	0.14
	6	1064	0.21	0.16
Panel B: Key variables				
Level variable	Mean	Median	S.D.	*N*
Media-Generated Articles during [EA–1, EA+1]	5.1	2.0	7.3	19,252
ln(1+ Media-Generated Articles during [EA–1, EA+1])	1.3	1.1	0.94	19,252
Average Trading Volume during [EA–1, EA+1]	17.0	12.0	17.0	18,314
Market-Adjusted Stock Return [EA–1, EA+1]	0.22	0.094	7.7	18,314
Market-Adjusted Stock Return [EA+2, EA+30]	0.35	0.1	10.0	18,314
EA Order (pattern)	0.49	0.50	0.27	19,252
EA Order (all)	0.34	0.29	0.24	19,252
Change variable	Mean	Median	S.D.	*N*
Δ Media-Generated Articles during [EA–1, EA+1]	−0.09	0.0	4.2	19,252
Δ ln(1+ Media-Generated Articles during [EA–1, EA+1])	−0.014	0.0	0.64	19,252
Δ Average Trading Volume during [EA–1, EA+1]	0.36	0.14	13.0	18,314
Δ Market-Adjusted Stock Return [EA–1, EA+1]	−0.59	−0.24	14.0	18,314
Δ Market-Adjusted Stock Return [EA+2, EA+30]	−0.47	0.022	19.0	18,314
Δ EA Order (pattern)	−0.002	−0.018	0.08	19,252
Δ EA Order (all)	−0.02	−0.04	0.07	19,252

we further control for time-varying industry effects by standardizing all the continuous variables by fiscal quarter end date × industry to have a mean of zero. See Appendix C for definitions of all variables included in Eq. (1).

Table 2 presents the results for the validation test and contrasts the determinants of Δ*EA Order* for Pattern and non-Pattern firms. Specifically, the left four columns show the results for Pattern firms. In this case, none of the coefficients are significantly different from zero at the 10% confidence level when estimating Eq. (1) among the Pattern sample. Moreover, the *F*-test indicates that we cannot reject the null that *all* the coefficients are jointly equal to zero, and that the R^2 is not significantly different from zero. These tests provide strong evidence that changes in the relative timing of firms' earnings announcements (Δ*EA Order*) in the Pattern sample are unlikely to be explained by changes in firms' performance, firm-level attributes, or earnings news.

In stark contrast to our results from the Pattern sample, the right-four columns show a significant link between timing and earnings news for 'non-Pattern' sample firms' whose earnings announcements could be endogenously determined by earnings content. In particular, changes in performance and earnings news predict changes in the timing of earnings announcements (Δ*EA Order*) among non-Pattern firm. For example, consistent with prior studies, negative coefficients on changes in earnings surprise (Δ*SURP*) and size-adjusted return (Δ*Size-Adjusted BHR*) suggest that firms with negative (positive) earnings news delay (accelerate) their earnings announcements.[9] Additionally, positive coefficients on changes in stock volatility (Δ*Stock Volatility*) and the dispersion in analysts' forecasts (Δ*Analyst Dispersion*) suggest that an increase in a firm's uncertainty leads to a delay in earnings announcements. Finally, the joint *F*-test indicates the coefficients are not jointly equal to zero, and the *R*-squared is significantly greater than zero.

The contrast in the regression results between the Pattern and non-Pattern samples suggests that changes in *EA Order* for the Pattern sample are likely driven by exogenous calendar rotations, rather than changes in the firms' earnings news. These findings also mitigate concerns that firms change the nature of their earnings news in response to calendar rotations or selectively follow patterns, which might induce a correlation between Δ*EA Order* and changes in firms' earnings news. Overall, the results in Table 2 provide support for our proposed methodology that leverages calendar rotations to isolate exogenous variation in firms' announcement timing.

[9] For example, see Niederhoffer and Regan (1972), Kross (1981), Givoly and Palmon (1982), Begley and Fischer (1998), and Johnson and So (2018b).

Table 2
Validation tests.
This table reports estimates from the regression of fiscal-quarter-matched changes in the relative timing of earnings announcements on fiscal-quarter-matched changes in performance-related variables as follows: $\Delta EAOrder_{i,q} = \sum \beta \cdot \Delta NewsVariables_{i,q} + \sum \beta \cdot \Delta Performance\ Variables_{i,q} + \epsilon_{i,q}$. See Appendix C for variable definitions. The first four columns report results for Pattern firm quarters, and the last four columns report results for non-Pattern firm quarters. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	Pattern firm quarters				non-Pattern firm quarters			
	ΔEA Order (pattern)		ΔEA Order (all)		ΔEA Order (non-Pattern)		ΔEA Order (all)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔSURP	−0.028	−0.022	−0.002	0.005	−0.112***	−0.045	−0.108***	−0.043
	(−0.366)	(−0.285)	(−0.035)	(0.070)	(−2.961)	(−1.610)	(−2.776)	(−1.505)
Δ|SURP|		0.097		0.100		0.266***		0.259***
		(1.030)		(1.183)		(7.328)		(7.226)
ΔROA	0.008	0.008	0.005	0.005	−0.154***	−0.157***	−0.151***	−0.154***
	(0.386)	(0.389)	(0.220)	(0.216)	(−5.225)	(−5.813)	(−5.116)	(−5.691)
ΔBTM	0.001	0.001	0.000	0.000	0.008***	0.008***	0.008***	0.008***
	(0.167)	(0.165)	(0.035)	(0.032)	(3.032)	(2.973)	(3.163)	(3.116)
Δln(MVE)	−0.004	−0.003	−0.004	−0.003	−0.011***	−0.007***	−0.010***	−0.007**
	(−1.116)	(−0.884)	(−1.315)	(−1.047)	(−3.903)	(−2.679)	(−3.685)	(−2.458)
ΔInstitutional Ownership	0.004	0.004	0.003	0.003	0.004	0.004	0.003	0.004
	(1.219)	(1.237)	(0.734)	(0.747)	(0.609)	(0.732)	(0.557)	(0.678)
ΔStock Volatility	0.052	0.042	0.058	0.047	0.299***	0.235***	0.297***	0.234***
	(0.660)	(0.537)	(0.956)	(0.759)	(4.314)	(3.505)	(4.214)	(3.445)
ΔSize-Adjusted BHR	−0.001	−0.001	−0.002	−0.002	−0.006*	−0.005	−0.006*	−0.005*
	(−0.547)	(−0.560)	(−0.877)	(−0.897)	(−1.693)	(−1.612)	(−1.777)	(−1.693))
ΔInsider Trading	0.000	0.000	0.000	0.000	−0.001***	−0.001***	−0.001***	−0.001***
	(0.237)	(0.147)	(0.126)	(0.038)	(−2.782)	(−3.062)	(−2.848)	(−3.125)
ΔAnalyst Coverage	−0.003	−0.003	−0.002	−0.002	−0.008***	−0.008***	−0.008***	−0.008***
	(−1.336)	(−1.351)	(−1.115)	(−1.129)	(−2.912)	(−2.884)	(−2.870)	(−2.845)
ΔAnalyst Dispersion	−0.003	−0.003	−0.001	−0.001	0.008**	0.007**	0.009**	0.007**
	(−0.647)	(−0.658)	(−0.161)	(−0.174)	(2.421)	(2.120)	(2.502)	(2.205)
N	19,252	19,252	19,252	19,252	76,622	76,622	76,622	76,622
S.E. clustering	Industry and Year-quarter (two-way)							
Standardization level	Fiscal Quarter End Date × Industry							
R-sq	0.000	0.001	0.001	0.001	0.008	0.010	0.008	0.009
F-stat	0.783	0.843	0.930	0.959	45.118	45.322	44.135	44.473
Prob > *F*	64.53%	59.72%	50.38%	48.20%	0.00%	0.00%	0.00%	0.00%

To further mitigate concerns over look-ahead bias associated with realized announcement dates in our earnings announcement premium tests, we also implement and discuss analogous tests that measure changes in *EA Order* based on firms' expected announcement dates. In finding similar results, these tests mitigate possible alternative explanations about the current announcement driving the effect we document, as well as those based on differences in observable or non-observable factors. See Section 5.2 for details.

4. Application of calendar rotations

In this section, we use calendar rotations to study the impact of timing on the attention of both journalists and investors.

4.1. Media attention as measured by media coverage

In our first set of tests, we investigate the impact of changes in earnings announcement timing on media coverage. Recent studies provide evidence that media content is shaped by the demands of news consumers (e.g., Hamilton, 2004). For example, Gentzkow and Shapiro (2010) show the media, as part of its profit-maximizing strategy, provides biased news to appeal to readers with a preference for a like-minded slant. These studies suggests journalists are incentivized to cater their coverage decisions toward "novel" news stories, which are more likely of greater interest and economic value to readers (Hamilton 2004; Solomon and Soltes 2011; Gentzkow and Shapiro 2010). Thus, we predict the media is more likely to cover earlier announcements because they tend to contain novel earnings news that attracts readership from a broader set of market participants.

To test this prediction, we study the relation between fiscal-quarter-specific changes in earnings announcements timing and changes in subsequent media coverage. We conduct these tests using the following first-difference regression model:

$$\Delta ln(1 + MediaGeneratedArticles)_{i,q} = \alpha_1 \Delta EAOrder_{i,q} + \sum \beta \cdot \Delta Controls_{i,q} + \epsilon_{i,q}, \quad (2)$$

Table 3
Tests on media coverage at earnings announcements.

This table reports estimates from the regression of fiscal-quarter-matched changes in media coverage on fiscal-quarter-matched changes in the relative timing of earnings announcement timing as follows: $\Delta\ln(1+MediaGeneratedArticles)_{i,q} = \alpha_1\Delta EAOrder_{i,q} + \sum\beta\cdot\Delta Controls_{i,q} + \epsilon_{i,q}$. See Appendix C for variable definitions. The first two columns report results for Pattern firm quarters using *ΔEA Order (pattern)* and *ΔEA Order (all)*, respectively, and the last two columns report results for non-Pattern firm quarters using *ΔEA Order (non-Pattern)* and *ΔEA Order (all)*, respectively. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report *t*-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	Δln(1+Media-Generated Articles during [EA−1, EA+1])			
	Pattern firm quarters		non-Pattern firm quarters	
	(1)	(2)	(3)	(4)
ΔEA Order (pattern or non-Pattern)	−0.239***		0.083***	
	(−3.024)		(2.956)	
ΔEA Order (all)		−0.176**		0.080***
		(−2.046)		(2.852)
ΔSURP	−1.047**	−1.047**	−0.026	−0.026
	(−2.245)	(−2.242)	(−0.249)	(−0.250)
Δ|SURP|	2.591***	2.587***	0.983***	0.984***
	(4.470)	(4.445)	(4.948)	(4.951)
ΔROA	0.060	0.056	0.062	0.062
	(0.488)	(0.464)	(0.658)	(0.652)
ΔBTM	−0.054**	−0.054**	−0.003	−0.002
	(−2.560)	(−2.569)	(−0.231)	(−0.228)
Δln(MVE)	0.082***	0.082***	0.082***	0.082***
	(2.639)	(2.642)	(6.204)	(6.199)
ΔCapex	0.118	0.111	0.190	0.189
	(0.374)	(0.353)	(1.514)	(1.512)
ΔLeverage	0.009	0.009	0.016	0.017
	(0.096)	(0.095)	(0.727)	(0.737)
ΔInstitutional Ownership	0.339***	0.339***	0.287***	0.287***
	(4.914)	(4.912)	(4.836)	(4.835)
ΔStock Volatility	3.334***	3.337***	2.289***	2.290***
	(3.508)	(3.513)	(3.556)	(3.556)
ΔSize-Adjusted BHR	−0.007	−0.006	−0.011	−0.011
	(−0.198)	(−0.176)	(−0.705)	(−0.712)
ΔInsider Trading	0.011**	0.011**	0.007***	0.007***
	(2.148)	(2.144)	(2.701)	(2.700)
ΔLoss Indicator	−0.018*	−0.018*	0.006	0.006
	(−1.733)	(−1.757)	(1.362)	(1.370)
ΔFriday Indicator	0.045	0.040	−0.031***	−0.031***
	(1.594)	(1.365)	(−3.623)	(−3.637)
ΔForecast Indicator	0.004	0.004	0.029***	0.029***
	(0.359)	(0.364)	(3.501)	(3.498)
ΔSame-Day EAs	−0.042	−0.056	−0.095***	−0.095***
	(−1.128)	(−1.600)	(−13.343)	(−13.311)
ΔAnalyst Coverage	0.027	0.027	0.030**	0.030**
	(1.205)	(1.223)	(2.468)	(2.466)
ΔAnalyst Dispersion	−0.009	−0.009	0.016	0.016
	(−0.207)	(−0.199)	(1.361)	(1.363)
N	19,252	19,252	76,622	76,622
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.014	0.014	0.020	0.020

Table 4
Tests on trading volume at earnings announcements.

This table reports estimates from the regression of fiscal-quarter-matched changes in trading volume on fiscal-quarter-matched changes in the relative timing of earnings announcement timing as follows: $\Delta TradingVolume_{i,q} = \alpha_1 \Delta EAOrder_{i,q} + \sum \beta \cdot \Delta Controls_{i,q} + \epsilon_{i,q}$. See Appendix C for variable definitions. The first two columns report results for Pattern firm quarters using Δ*EA Order (pattern)* and Δ*EA Order (all)*, respectively, and the last two columns report results for non-Pattern firm quarters using Δ*EA Order (non-Pattern)* and Δ*EA Order (all)*, respectively. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report *t*-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	ΔAverage Trading Volume during [EA–1, EA+1]			
	Pattern firm quarters		non-Pattern firm quarters	
	(1)	(2)	(3)	(4)
ΔEA Order (pattern or non-Pattern)	−3.798***		0.036	
	(−3.377)		(0.064)	
ΔEA Order (all)		−2.964**		−0.084
		(−1.999)		(−0.149)
ΔSURP	−0.039	−0.036	7.067	7.062
	(−0.003)	(−0.003)	(1.389)	(1.388)
Δ|SURP|	80.180***	80.103***	37.731***	37.763***
	(3.828)	(3.819)	(5.267)	(5.275)
ΔROA	17.726***	17.683***	9.615	9.604
	(2.900)	(2.892)	(1.481)	(1.480)
ΔBTM	−0.262	−0.268	−0.403	−0.402
	(−0.253)	(−0.258)	(−0.701)	(−0.698)
Δln(MVE)	3.010***	3.012***	4.380***	4.379***
	(2.825)	(2.824)	(6.843)	(6.843)
ΔCapex	6.812	6.700	15.574***	15.566***
	(1.232)	(1.207)	(3.707)	(3.705)
ΔLeverage	6.460***	6.463***	5.707***	5.714***
	(2.844)	(2.847)	(4.480)	(4.482)
ΔInstitutional Ownership	2.355**	2.355**	3.151***	3.152***
	(2.237)	(2.241)	(3.152)	(3.152)
ΔStock Volatility	386.284***	386.374***	364.307***	364.304***
	(12.590)	(12.600)	(16.693)	(16.696)
ΔSize-Adjusted BHR	−4.111***	−4.098***	−2.695***	−2.697***
	(−4.069)	(−4.041)	(−2.815)	(−2.816)
ΔInsider Trading	0.016	0.016	0.144**	0.144**
	(0.172)	(0.178)	(2.574)	(2.571)
ΔLoss Indicator	−0.133	−0.136	−0.228	−0.226
	(−0.364)	(−0.374)	(−0.825)	(−0.820)
ΔFriday Indicator	1.254**	1.173**	−0.902***	−0.895***
	(2.546)	(2.382)	(−4.038)	(−4.001)
ΔForecast Indicator	0.342	0.341	0.204	0.203
	(1.540)	(1.536)	(0.800)	(0.798)
ΔSame-Day EAs	−0.933*	−1.158**	−1.210***	−1.204***
	(−1.649)	(−2.112)	(−6.804)	(−6.767)
ΔAnalyst Coverage	0.564	0.571	0.004	0.003
	(0.977)	(0.993)	(0.010)	(0.007)
ΔAnalyst Dispersion	0.684	0.686	0.817***	0.817***
	(0.527)	(0.530)	(2.608)	(2.609)
ΔStock Illiquidity	−4.566***	−4.560***	−3.644***	−3.643***
	(−4.163)	(−4.157)	(−5.722)	(−5.720)
N	18,314	18,314	72,792	72,792
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.079	0.079	0.087	0.087

Table 5
Tests on earnings announcement premia.

This table reports estimates from the regression of fiscal-quarter-matched changes in stock returns on fiscal-quarter-matched changes in the relative timing of earnings announcement timing as follows: $\Delta MarketAdjustedStockReturn_{i,q} = \alpha_1 Quintile for \Delta EAOrder_{i,q} + \sum \beta \cdot Quintiles for \Delta Controls_{i,q} + \epsilon_{i,q}$. Panel A uses fiscal-quarter-matched changes in market-adjusted returns for the three-day window around the earnings announcement [EA–1, EA+1] as the dependent variable, and Panel B uses fiscal-quarter-matched changes in market-adjusted returns for the 29-day period after the earnings announcement [EA+2, EA+30] as the dependent variable. See Appendix C for variable definitions. The first two columns report results for Pattern firm quarters using Δ*EA Order (pattern)* and Δ*EA Order (all)*, respectively, and the last two columns report results for non-Pattern firm quarters using Δ*EA Order (non-Pattern)* and Δ*EA Order (all)*, respectively. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level. We estimate and report *t*-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Earnings announcement returns

	ΔMarket-Adjusted Stock Return [EA–1, EA+1]			
	Pattern firm quarters		non-Pattern firm quarters	
	(1)	(2)	(3)	(4)
Quintile for ΔEA Order (pattern or non-Pattern)	−0.443***		−1.139***	
	(−2.817)		(−6.166)	
Quintile for ΔEA Order (all)		−0.576***		−1.167***
		(−2.781)		(−6.159)
Quintile for ΔSURP	8.721***	8.744***	9.028***	9.032***
	(14.505)	(14.670)	(14.058)	(14.049)
Quintile for ΔBTM	−1.087***	−1.038***	−1.043***	−1.031***
	(−5.264)	(−4.983)	(−5.218)	(−5.170)
Quintile for Δln(MVE)	−6.949***	−6.899***	−6.497***	−6.487***
	(−14.758)	(−13.919)	(−15.464)	(−15.520)
N	18,258	18,258	72,674	72,674
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.105	0.106	0.078	0.078

Panel B: Earnings announcement drift

	ΔMarket-Adjusted Stock Return [EA+2, EA+30]			
	Patternfirmquarters		non-Pattern firm quarters	
	(1)	(2)	(3)	(4)
Quintile for ΔEA Order (pattern or non-Pattern)	0.740*		0.461	
	(1.802)		(0.914)	
Quintile for ΔEA Order (all)		0.716*		0.454
		(1.671)		(0.920)
Quintile for ΔSURP	1.612***	1.613***	2.700***	2.702***
	(4.515)	(4.549)	(6.643)	(6.659)
Quintile for ΔBTM	3.248***	3.261***	3.279***	3.282***
	(7.743)	(7.592)	(5.898)	(5.954)
Quintile for Δln(MVE)	−5.883***	−5.873***	−6.775***	−6.773***
	(−12.354)	(−12.313)	(−9.706)	(−9.681)
N	18,258	18,258	72,660	72,660
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.035	0.035	0.021	0.021

where the dependent variable equals the fiscal-quarter-matched change in the log of 1 plus the number of media-generated articles from day $t-1$ to t+1, where day t is the announcement date.[10] The notation Δ again denotes the fiscal-quarter-matched difference, which naturally controls for time-independent firm quarter effects. To further control for time-varying industry-specific effects, we standardize all the continuous variables in Eq. (2) by fiscal quarter end date × industry to have a mean of zero.

The left two columns of Table 3 report significantly negative coefficients on both measures of Δ*EA Order*, which suggests that earnings announcements moved earlier by calendar rotations receive more media coverage than those that are delayed. The coefficients on Δ*EA Order* range from –0.17 to –0.24. To put these numbers in perspective, the coefficient of –0.239 on Δ*EA Order (pattern)* in column (1) implies that, for a firm with average media coverage, a one standard-deviation delay (acceleration) in

[10] Following Solomon (2012), Tetlock (2010), and Tetlock (2011), we use the natural logarithm of one plus the number of media articles to mitigate the influence of outliers.

EA Order leads to a 7.5% reduction (7.9% increase) in media coverage.[11]

The right two columns of Table 3 illustrate the importance of our methodology by conducting parallel tests of media coverage for a set of 'non-Pattern' firms excluded from the 'Pattern' sample, whose announcement dates are more likely endogenously selected. Using the non-Pattern sample, we find the relation between *EA Order* and media coverage is positive. The positive coefficient on Δ*EA Order* among non-Pattern firms points to the opposite inference as our main results based on calendar rotations: delayed announcements receive *greater* media coverage, consistent with firms delaying announcements with bad news and the media preferring to cover negative stories (e.g., Niessner and So (2017)). These results suggest studying endogenously determined announcement timing in the non-Pattern sample is problematic for identifying firms' incentives to advance or delay their announcements, even when researchers include controls and common earnings news proxies.[12]

4.2. Investor attention as measured by trading volume

To the extent announcement accelerations lead to increased attention to firms' earnings news, we predict decreases in *EA Order* due to calendar rotations result in higher announcement trading volume. We test this by re-estimating Eq. (2) when the dependent variable is firms' three-day announcement window trading volume, measured as average share volume scaled by shares outstanding. We also include additional controls for firms' bid-ask spreads, denoted *Stock Illiquidity*, to account for factors impacting investors' trading activity.

The left two columns of Table 4 document a robust negative relation between both measures of Δ*EA Order* and within-firm variation in announcement trading volume. Specifically, the coefficient of 3.798 on Δ*EA Order* (*pattern*) in the first column indicates a one-standard-deviation delay (acceleration) in *EA Order (pattern)* leads to a 6% reduction (increase) in trading volume for an average firm during the period [EA–1, EA+1].[13] The higher volume in response to calendar rotation advances is consistent with investors paying greater attention to earnings announcements when other firms' announcements are less likely to have preempted some of the news.

The right two columns of Table 4 contain results for analogous tests of announcement trading volume for the sample of non-Pattern firms whose announcement timing is more likely endogenous. Using this alternative sample, we find no significant relation between Δ*EA Order* and changes in trading volume. This non-result is potentially confounded by non-Pattern firms delaying the release of bad or large-surprise earnings news—as shown in Table 2—which prior literature has shown to create more trading volume at the time of earnings announcements (e.g., Bamber 1987; Johnson and So 2018a). The contrasting findings across Pattern versus non-Pattern firms illustrate the importance of the methodology we propose, as well as potential inference problems that arise when researchers study the impact of timing when the timing is more likely endogenous to the announced news.

5. Earnings announcement premia

5.1. Link between calendar rotations and returns

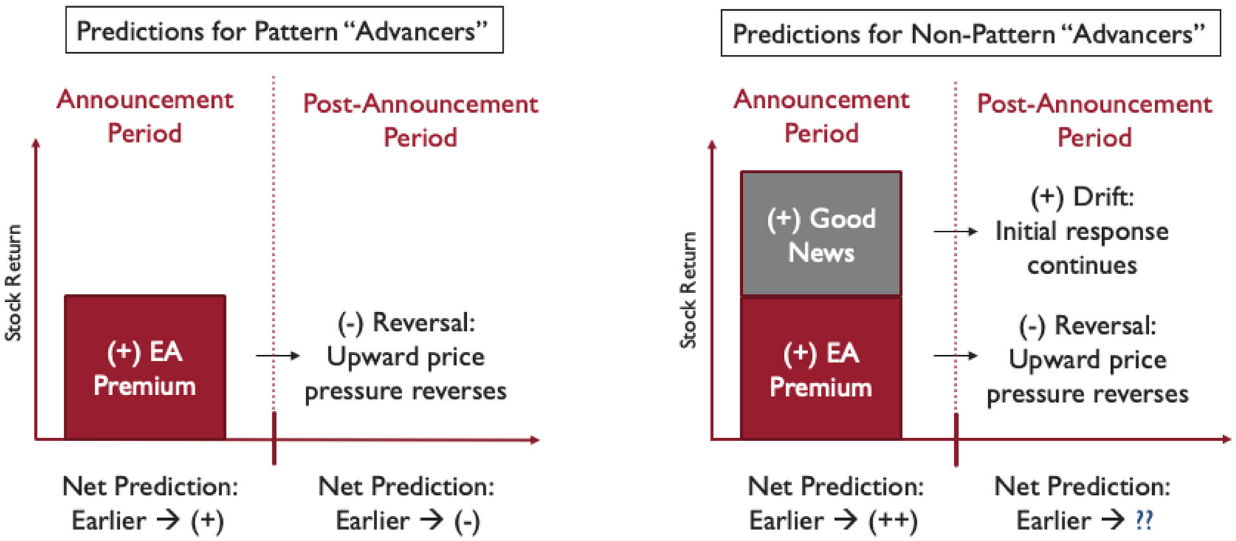
Our results in Section 4 suggest calendar rotations lead to exogenous variation in attention as measured by media coverage and trading volume. Our next tests apply calendar rotations to study the drivers of earnings announcement premia (i.e., the tendency for stock prices to rise around firms' announcements). Prior research suggests announcement premia are driven in part by short-sale-constrained investors who bid up prices in response to heightened attention associated with the announcements (e.g., Frazzini and Lamont 2007; Barber and Odean 2008; Johnson et al. 2020). Use of calendar rotations is helpful for studying this mechanism because, in most settings, it is challenging to isolate cross-sectional variation in attention unrelated to the sign (i.e., good versus bad) and intensity (i.e., small versus large quantities) of firms' earnings news.

The graphic below illustrates our predictions for firms' returns plotted along the Y-axis both during and after firms' earnings announcements. The graphic is divided into two plots corresponding to Pattern firms on the left and non-Pattern firms on the right. Both sides of the graphic reflect our predictions for firms whose earnings announcements are advanced (i.e., moved earlier) relative to other firms. Our (net) predictions for firms' announcement-window and post-announcement returns are listed at the bottom of the graphic.

[11] −7.5% = (exp(ln(1+5.1) − 0.239 × 0.27)−1)/5.1−1 and 7.9% = (exp(ln(1+5.1) + 0.239 × 0.27)−1)/5.1−1, where 5.1 is the average media coverage and 0.27 is the standard deviation of *EA Order (pattern)* for Pattern firms (Table 1, Panel B). Alternatively, for a firm with average media coverage, a one standard deviation delay (acceleration) in Δ*EA Order* leads to 2.3% reduction (2.3% increase) in media coverage. These estimates come from −2.3% = (exp(ln(1+5.1) − 0.239 × 0.08)−1)/5.1−1 and 2.3% = (exp(ln(1+5.1) + 0.239 × 0.08)−1)/5.1−1, where 5.1 is the average media coverage and 0.08 is the standard deviation of Δ*EA Order (pattern)* for Pattern firms (Table 1, Panel B).

[12] One likely reason for why researchers cannot simply control for earnings news proxies stems from the difficulty of capturing the multidimensional nature of news. Consistent with this view, prior research shows low *R*-squared values in regressions of announcement-window returns (e.g., Kothari, 2001).

[13] −6% = (−3.798 × 0.27)/17, where 17 is the average trading volume for [EA−1, EA+1] and 0.27 is the standard deviation of *EA Order (pattern)* for Pattern firms (Table 1 Panel B). Alternatively, a one standard-deviation delay (acceleration) in Δ*EA Order* leads to 1.8% reduction (1.8% increase) in trading volume. This estimate comes from −1.8% = (−3.798 × 0.08)/17, where 17 is the average trading volume for [EA−1, EA+1] and 0.08 is the standard deviation of *EA Order (pattern)* for Pattern firms (Table 1 Panel B).



We first test our predictions for Pattern firms whose announcements are moved earlier, as illustrated on the left of the graphic. We predict that heightened attention to earnings announcements contributes to predictably higher returns due to increased investor attention, which intuitively aligns with our evidence that accelerated announcements receive increased coverage from the financial press and are more actively traded.

In Panel A of Table 5, we study earnings announcement premia by examining the link between announcement timing and firms' earnings announcement returns. Specifically, we run tests analogous to Eq. (2), where the dependent variable equals the within-firm change in market-adjusted returns from day $t-1$ to $t+1$, where day t is the firm's announcement date. These regressions also control for firms' earnings surprise, size, and book-to-market ratio as firm characteristics known to explain the cross-section of returns. Moreover, to facilitate the interpretation of our results and mimic forming long-short portfolios, we assign independent variables into quintiles each calendar quarter ranging in value from 0 to 1.

Consistent with our predictions, the left two columns in Panel A of Table 5 show firms' announcement returns are negatively related to Δ*EA Order*, indicating firms moved earlier by calendar rotations (i.e., that have lower Δ*EA Order*) tend to earn higher returns when announcing earnings. The coefficient of −0.443 in the first column indicates firms in the highest quintile of Δ*EA Order* outperform those in the lowest by roughly 44 basis points.

In Panel B of Table 5, we complement our earnings announcement premia tests by exploring firms' post announcement returns from days $t+2$ to $t+30$. To the extent our results in Panel A of Table 5 reflect transitory mispricing stemming from periods of heightened investor attention, we predict the heightened announcement returns for firms moved earlier by calendar rotations will subsequently reverse after the announcement.

Consistent with our predictions, the left two columns of Panel B of Table 5 focus on Pattern firms and show a significant positive relation between changes in *EA Order* and firms' post-announcement returns from $t+2$ to $t+30$. The positive relation indicates that Pattern firms moved earlier by calendar rotations (i.e., negative Δ*EA Order*) tend to predictably earn lower returns following their announcement as the transitory mispricing reverses. In terms of economic magnitude, the coefficient of 0.740 in the first column suggests firms in the highest versus lowest quintile of Δ*EA Order* experience an increase in the post-earnings announcement return of roughly 74 basis points.[14]

We next test our predictions for the returns of non-Pattern firms illustrated by the right side of the graphic above. We predict non-Pattern firms' announcement returns are being driven by two complementary forces. The first is that later announcements likely garner lesser attention from investors who bid up prices, resulting in a less positive announcement return (i.e., the same as our prediction for Pattern firms). The second force is that non-Pattern firms with bad news are more likely to delay their announcement dates, leading to a lower earnings announcement return on average.

The right two columns of Table 5 present results from analogous regressions among non-Pattern firms. In the right two columns of Panel A, the regressions using non-Pattern firms also yield a negative relation between changes in firms' earnings announcement returns and Δ*EA Order*. Notably, and as predicted, the effect is larger than those documented for Pattern firms. Specifically, the coefficient of −1.139 in the third column highlights an approximate 114-basis-point spread in three-day earnings announcement premia for non-Pattern firms in the highest versus lowest quintile of Δ*EA Order*, which is roughly twice the return spread shown in the left two columns for Pattern firms.

[14] In untabulated results, we find the magnitudes of the announcement return and subsequent post-announcement return associated with calendar rotations are statistically indistinguishable.

The larger coefficients and t-statistics for Δ*EA Order* among non-Pattern compared to Patern firms shown in Panel A is consistent with their announcement returns being driven by the two complementary forces we predict. Moreover, these differences underscore that the use of announcement timing to study investor attention in broad samples is likely confounded by firms endogenously selecting announcement timing based on their earnings news, which—in this case—could lead researchers to overstate the economic effect of interest.

The right two columns of Panel B examine post-announcement returns among non-Pattern firms where our predictions again reflect two forces. However, in the context of post-announcement returns, we predict these forces have offsetting, rather than complementary, effects. Specifically, we expect transitory overpricing associated with earlier announcements of non-Pattern firms to reverse, which should result in lower post-announcement returns. However, at the same time, because earlier announcements by non-Pattern firms tend to convey good news, we expect non-Pattern firms to subsequently earn higher returns due to post–earnings announcement drift. Consistent with these forces having offsetting effects, we find no significant relation between *EA Order* and non-Pattern firms' post-announcement returns from t+2 to t+30. These findings help further illustrate the importance of our methodology by showing that researchers' inferences can materially change when relying on endogenous measures of announcement timing.

The magnitude of our findings in Table 5 are important in that they suggest a substantial portion of the earnings announcement premium is attributable to attention. These findings thus help substantiate the micro-foundations of the volume channel discussed in Frazzini and Lamont (2007) and corroborate the "all-that-glitters" mechanism for investor behavior highlighted in Barber and Odean (2008) using a more cleanly identified shock to investor attention. More broadly, the findings in Table 5 suggest variation in attention and the accompanying volume and price pressure are important for explaining other anomalies surrounding recurring events such as calendar-month- and dividend-based return seasonalities [see Hartzmark and Solomon (2018) for a helpful review of this research].

5.2. Expected announcement dates

To mitigate concerns over look-ahead bias associated with realized announcement dates, we implement analogous tests that measure changes in *EA Order* based on firms' expected announcement dates. The sample for this analysis consists of firms that have followed a pattern for three consecutive quarters, regardless of whether the firm maintained the pattern for the focal earnings announcement. Using this alternative approach, we find a similar result: a significant negative association between calendar rotations and returns around expected announcement dates. These tests, contained in our Online Appendix, show our results are concentrated among firms that maintain their patterns, which is consistent with two broad mechanisms. The first is that investors are less likely to pay attention to firms during their expected announcement dates when they announce early (e.g., Johnson and So 2018b). The second is that investors likely infer that firms possess negative news when they delay their announcements past their expected date (e.g., Bagnoli et al. 2002), which mitigates the impact of increased attention. In finding similar results using this alternative approach, these tests mitigate possible alternative explanations about the current announcement driving the effect we document, as well as those based on differences in observable or non-observable factors.

6. Mechanism tests

In Table 6, we refine our main tests by focusing on firm quarters that experience changes in *EA Order* due to the announcement ordering of other firms. Specifically, we identify a subsample of observations with the same number of trading days between firms' fiscal quarter end and corresponding announcement date relative to the same fiscal quarter of the previous year. Because our methodology generates variation in both the absolute and relative timing of announcements, these tests mitigate concerns our results depend on variation in the speed, rather than the ordering, with which firms announce earnings.

Using this more strict sample requirement in Table 6, we continue to find a statistically significant positive relation between the relative timeliness of announcements as measured by *EA Order* and the extent of media coverage. This result implies that journalists' decisions to write an article about earnings news depend on the *relative* timing of earnings news, which is consistent with the extent of novel and non-preempted news determining media coverage. We also continue to find significant negative relations between changes in *EA Order* and both trading volume and returns around firms' earnings announcements.

To provide further support for the notion that our findings are driven by information transfers, we split our sample of earnings announcements into firms announcing earnings during the early versus late half of the range of announcement dates. Specifically, our 'Pattern' sample is divided into two groups depending on whether each measure of *EA Order* in the same fiscal quarter of the previous year was equal to or smaller than 0.5 ("Announcement in first half") or greater than 0.5 ("Announcement in second half").

The Table 6 tests are motivated by our prediction that a change in the relative timing of earnings announcement will have a greater impact earlier on when the information environment is being newly shaped, relative to late in the season, when investors have learned about macro- and industry trends from other announcements. Consistent with this argument, Table 7 shows our findings are concentrated among firms announcing earnings earlier on in the range of announcement dates, whereas we find statistically weaker and insignificant effects of relative timing on media coverage, trading volume, and earnings announcement premia for firms announcing later in the range of announcement dates.

Table 6
Tests on firms with no change in absolute announcement lag.
The three panels below report estimates from running the same regressions used in Tables 3–5, respectively, on a sample of Pattern firm quarters that have the same number of trading days between the fiscal quarter end date and the earnings announcement date as the same quarter of the previous fiscal year. See Appendix C for variable definitions. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. Stock returns are not winsorized. We estimate and report *t*-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Media coverage			
	ΔIn(1+Media-Generated Articles during [EA–1, EA+1])		
	(1)		(2)
ΔEA Order (pattern)	−0.579***		
	(−3.072)		
ΔEA Order (all)			−0.568**
			(−2.534)
Controls?		Yes	
N	4536		4536
S.E. clustering		Industry and Year-quarter (two-way)	
Standardization level		Fiscal Quarter End Date × Industry	
R-sq	0.020		0.019

Panel B: Trading volume			
	ΔAverage Trading Volume during [EA–1, EA+1]		
	(1)		(2)
ΔEA Order (pattern)	−5.549*		
	(−1.886)		
ΔEA Order (all)			−9.698**
			(−2.311)
Controls?		Yes	
N	4372		4372
S.E. clustering		Industry and Year-quarter (two-way)	
Standardization level		Fiscal Quarter End Date × Industry	
R-sq	0.102		0.102

Panel C: Earnings announcement premia			
	ΔMarket-Adjusted Stock Return [EA–1, EA+1]		
	(1)		(2)
Quintile for ΔEA Order (pattern)	−0.549*		
	(−1.664)		
Quintile for ΔEA Order (all)			−1.059**
			(−2.538)
Controls?		Yes	
N	4,362		4362
S.E. clustering		Industry and Year-quarter (two-way)	
Standardization level		Fiscal Quarter End Date × Industry	
R-sq	0.103		0.104

7. Extensions: applications and robustness

In our final section, we present three sets of results designed to establish robustness and guide future researchers that make use of calendar rotations.

7.1. Applications

A point of emphasis is that calendar rotations elicit a localized treatment effect in the spirit of Angrist and Pischke (2008), rather than a generalizable effect applicable to all firms. Because we study firms adhering to a pattern, the sign and magnitude of the inferences we draw may not apply to firms that deviate from patterns due to extreme circumstances (e.g., the need for time to determine the financial impact of a natural disaster).

For the intensive margin of pattern firms that make up our main sample, our validation tests in Table 2 show no association between changes in *EAOrder* and firms' earnings news or characteristics, which suggest calendar rotations help isolate shocks to announcement timing. However, in Table 8 we also study a sample from the extensive margin of firms that deviate from past patterns. The goal of the Table 8 tests is to provide readers with greater clarity as to the types of firms or circumstances in which our findings are less likely to apply.

In Table 8, we estimate logit models that identify cases in which firms follow an announcement pattern for three

Table 7
Tests for early versus late half of the range of announcement dates.

The three panels below report estimates from running the same regressions used in Tables 3–5, respectively, on two groups of Pattern firm quarters. For each measure of Δ*EAOrder*, the sample is divided into two groups depending on whether *EAOrder* in the same fiscal quarter of the previous year was equal to or smaller than 0.5 ("Announcement in first half") or greater than 0.5 ("Announcement in second half"). Therefore, the summations of *N*'s for columns (1) and (3) and for columns (2) and (4) are equal to the total number of firm quarters available for a given dependent variable. See Appendix C for variable definitions. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. Stock returns are not winsorized. We estimate and report *t*-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Media coverage

	ΔIn(1+Media-Generated Articles during [EA–1, EA+1])			
	Announcements in first half		Announcements in second half	
	(1)	(2)	(3)	(4)
ΔEA Order (pattern)	−0.337***		0.033	
	(−3.185)		(0.252)	
ΔEA Order (all)		−0.259**		0.096
		(−2.246)		(0.685)
Controls?	Yes		Yes	
N	9756	13,685	9496	5567
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.019	0.016	0.014	0.015

Panel B: Trading volume

	ΔAverage Trading Volume during [EA–1, EA+1]			
	Announcements in first half		Announcements in second half	
	(1)	(2)	(3)	(4)
ΔEA Order (pattern)	−4.303***		−2.104	
	(−3.414)		(−0.960)	
ΔEA Order (all)		−4.311**		1.511
		(−2.553)		(0.504)
Controls?	Yes		Yes	
N	9418	13,141	8896	5173
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.076	0.082	0.087	0.082

Panel C: Earnings announcement premia

	ΔMarket-Adjusted Stock Return [EA–1, EA+1]			
	Announcements in first half		Announcements in second half	
	(1)	(2)	(3)	(4)
Quintile for ΔEA Order (pattern)	−0.609***		−0.265	
	(−4.064)		(−0.857)	
Quintile for ΔEA Order (all)		−0.647***		−0.460
		(−2.607)		(−0.800)
Controls?	Yes		Yes	
N	9,388	13,101	8,870	5157
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.108	0.109	0.104	0.100

consecutive quarters, but deviate for the fourth. We first model deviations from past announcement patterns as a function of firms' earnings announcement surprises. We do so by dividing firms' earnings surprises into two sets of quartile indicator variables designed to capture extreme positive and negative earnings news.

Column (1) of Table 8 shows deviations from patterns are generally unrelated to firms' earnings surprises, but do appear more likely for the extreme tails of the surprise distribution. Fig. 3 plots the estimates graphically, highlighting insignificant coefficients for the middle six groupings, but significant positive coefficients for tail observations corresponding to the first quartile of negative surprises, and fourth quartile of positive surprises.[15]

[15] Economically, our results suggest being in the most extreme negative news quartile increases the probability of deviating by 5.6%, and 9.9% when in the extreme positive news quartile.

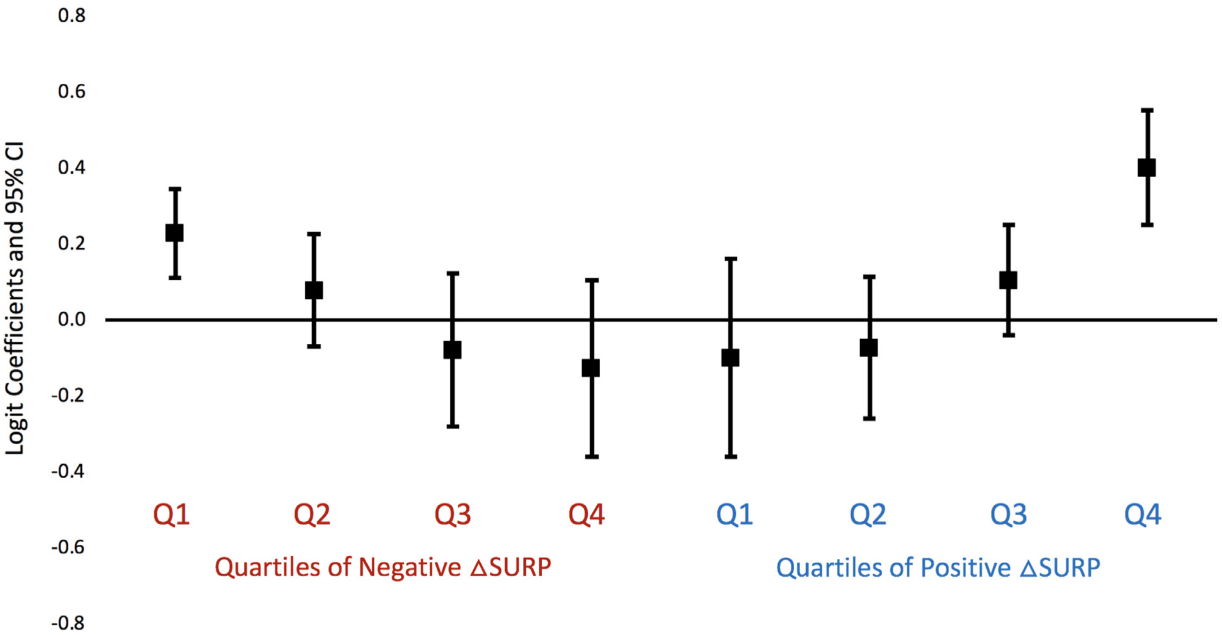


Fig. 3. Coefficient estimates for the likelihood of pattern deviations.
This figure plots coefficients on quartile indicators and their 95% confidence intervals from running the following logistic regression: $logit(Pr(Pattern\ Deviation = 1)) = ln\frac{Pr(Pattern\ Deviation=1)}{1-Pr(Pattern\ Deviation=1)} = \sum_{k=1}^{4}\beta \cdot Kth\ Quartile\ of\ Negative\ \Delta SURP_{i,q} + \sum_{k=1}^{4}\beta \cdot Kth\ Quartile\ of\ Positive\ \Delta SURP_{i,q} + \sum\beta \cdot \Delta Controls_{i,q} + \epsilon_{i,q}$. *Pattern Deviation* takes the value of 1 if the firm quarter deviates from its earnings announcement pattern after following it for three consecutive same quarters, and 0 if the firm quarter continues to follow its pattern for the fourth consecutive time. *Kth Quartile of Negative* (*Positive*) $\Delta SURP$ takes the value of 1 if the firm is in the *K*th quartile of negative (positive) $\Delta SURP$, and 0 otherwise. The tabular results of this regression are shown in Table 8. See Appendix C for variable definitions. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report *t*-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

The results in Fig. 3 and Table 8 are consistent with firms with large earnings surprises having greater incentives to deviate from their earnings announcement patterns. More generally, these findings suggest researchers should exercise caution in applying inferences from calendar rotations to firms in the tails of the surprise distribution, which are by construction underrepresented in our sample of Pattern firms due to deviations.

The results in column (2) of Table 8 show deviations are also related to changes in some control variables, including changes in firm size, book-to-market, and return momentum. For parsimony, we do not examine the role of extreme changes in these control variables. However, researchers can adopt an approach similar to our analysis of extreme earnings surprises to understand the types of firms or circumstances in which inferences from calendar rotations more likely apply (i.e., where the effect is likely localized). The results in Table 8 also provide a helpful transition to discussing differences in the characteristic profiles of Pattern versus non-Pattern firms.

As noted in the introduction, differences in sample composition for Pattern versus non-Pattern firms subject our study to the concern that the contrasting results across the two samples simply stem from differences in sample composition (e.g., smaller firms are unlikely to ever receive media coverage, which could result in no variation among non-Pattern firms). To address these types of concerns, in Table 9, we rerun our main tests using an additional sample of non-Pattern firms that have statistically indistinguishable from Pattern firms for the control variables used in our main tests (i.e., they possess similar average values of earnings surprise, size, analyst coverage, and institutional ownership).[16] We report sample averages of the two subsamples after matching in Appendix A.

Table 9 reports results from rerunning our main tests using characteristically-matched subsamples. A key result is that we continue to find results that differ in terms of economic and/or statistical significance for matched Pattern versus non-Pattern firms, despite the two subsamples having similar characteristic profiles. These findings are consistent with the discrepancies stemming from the endogenous selection of announcement timing among non-Pattern firms, rather than discrepancies in sample compositions.

7.2. Robustness

We also illustrate how researchers can apply calendar rotations when using modified measures of earnings announcement timing and show the robustness of our find-

[16] We implement propensity score matching on observable fundamentals captured by the control variables used in our tests. We use nearest neighbor matching within a caliper of 0.001 that yields covariate balance. We find the resulting matched samples used in Table 9 have sample averages of the control variables that are statistically indistinguishable at the 10% level.

Table 8
Tests on pattern deviation from the third to fourth consecutive quarters.

This table reports estimates from running the following logistic regression: $logit(Pr(Pattern\ Deviation = 1)) = ln\frac{Pr(Pattern\ Deviation=1)}{1-Pr(Pattern\ Deviation=1)} = \sum_{k=1}^{4}\beta \cdot Kth\ Quartile\ of\ Negative\ \Delta SURP_{i,q} + \sum_{k=1}^{4}\beta \cdot Kth\ Quartile\ of\ Positive\ \Delta SURP_{i,q} + \sum\beta \cdot \Delta Controls_{i,q} + \epsilon_{i,q}$. *Pattern Deviation* takes the value of 1 if the firm quarter deviates from its earnings announcement pattern after following it for three consecutive same quarters, and 0 if the firm quarter continues to follow its pattern for the fourth consecutive time. *Kth Quartile of Negative* (*Positive*) $\Delta SURP$ takes the value of 1 if the firm is in the *K*-th quartile of negative (positive) $\Delta SURP$, and 0 otherwise. See Appendix C for variable definitions. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report *t*-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	Indicator for pattern deviation	
	(1)	(2)
1st Quartile of Negative ΔSURP	0.252***	0.227***
	(3.944)	(3.860)
2nd Quartile of Negative ΔSURP	0.090	0.078
	(1.181)	(1.001)
3rd Quartile of Negative ΔSURP	−0.064	−0.079
	(−0.633)	(−0.779)
4th Quartile of Negative ΔSURP	−0.123	−0.127
	(−1.049)	(−1.082)
1st Quartile of Positive ΔSURP	−0.094	−0.100
	(−0.723)	(−0.763)
2nd Quartile of Positive ΔSURP	−0.073	−0.073
	(−0.764)	(−0.756)
3rd Quartile of Positive ΔSURP	0.101	0.105
	(1.389)	(1.431)
4th Quartile of Positive ΔSURP	0.350***	0.401***
	(4.935)	(5.257)
ΔROA		−1.057
		(−1.522)
ΔBTM		0.378***
		(4.316)
Δln(MVE)		0.475***
		(4.913)
ΔCapex		−1.353**
		(−2.207)
ΔLeverage		1.604***
		(8.630)
ΔInstitutional Ownership		0.061
		(0.218)
ΔStock Volatility		5.391
		(1.276)
ΔSize-Adjusted BHR		−0.105**
		(−2.083)
ΔInsider Trading		−0.003
		(−0.439)
ΔLoss Indicator		0.106**
		(2.035)
ΔAnalyst Coverage		0.005
		(0.066)
ΔAnalyst Dispersion		0.045
		(0.390)
N	17,867	17,867
S.E. clustering	Industry and Year-quarter (two-way)	
Standardization level	Fiscal Quarter End Date × Industry	
Pseudo *R*-sq	0.008	0.018

ings to alternative implementations. Our main tests discussed so far focus on information arrival in a general sense, such that we focus on the ordering of information arrival using earnings announcements for all firms. However, if a specific research question is more industry-specific, such as when studying intra-industry information transfers as in Hartzmark and Shue (2018), the researcher may prefer to implement a within-industry version of *EA Order* as detailed in Section 2. Similarly, our methodology is potentially useful in studying alternative economic linkages, such firms within a supply chain or strategic alliance.

In Table 10, we rerun our main tests after implementing industry-based measures of *EA Order* based on a firm's announcement order relative to other firms within the same Fama-French 12-industry classification. The results in Table 10 reinforce our main findings, again highlighting the implications for announcement timing on media coverage, trading volume, and announcement premia, and indicating

Table 9
Tests on matched samples of Pattern and non-Pattern firms.

The three panels below report estimates from running the same regressions used in Tables 3–5, respectively, on propensity-score-matched Pattern and non-Pattern firm quarters. Propensity scores are measured using level variables listed in the second table of Appendix A. See Appendix C for variable definitions. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. Stock returns are not winsorized. We estimate and report *t*-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Media coverage				
	ΔIn(1+Media-Generated Articles during [EA–1, EA+1])			
	Matched Pattern sample		Matched non-Pattern sample	
	(1)	(2)	(3)	(4)
ΔEA Order (pattern or non-Pattern)	−0.239***		0.102**	
	(−3.015)		(2.453)	
ΔEA Order (all)		−0.176**		0.099**
		(−2.040)		(2.340)
Controls?	Yes			
N	19,247	19,247	19,247	19,247
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.014	0.014	0.024	0.024

Panel B: Trading volume				
	ΔAverage Trading Volume during [EA–1, EA+1]			
	Matched Pattern sample		Matched non-Pattern sample	
	(1)	(2)	(3)	(4)
ΔEA Order (pattern or non-Pattern)	−3.777***		−1.160	
	(−3.317)		(−1.485)	
ΔEA Order (all)		−2.954**		−1.251
		(−1.983)		(−1.578)
Controls?	Yes			
N	18,308	18,308	18,308	18,308
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.079	0.079	0.078	0.078

Panel C: Earnings announcement premia				
	ΔMarket-Adjusted Stock Return [EA–1, EA+1]			
	Matched Pattern sample		Matched non-Pattern sample	
	(1)	(2)	(3)	(4)
Quintile for ΔEA Order (pattern or non-Pattern)	−0.446***		−1.059***	
	(−2.838)		(−4.679)	
Quintile for ΔEA Order (all)		−0.579***		−1.090***
		(−2.798)		(−4.625)
Controls?	Yes			
N	18,253	18,253	18,253	18,253
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Fiscal Quarter End Date × Industry			
R-sq	0.106	0.106	0.083	0.083

that our main inferences are largely unchanged when focusing on within-industry variation.

Finally, as discussed in Section 2, grouping by fiscal period ends in our main tests could inherit measurement errors that stem from ignoring information preemption from announcements of firms with different fiscal periods. Additionally, for research contexts focused on the relative timing of news arrival rather than preemption, grouping by calendar quarter could make more sense. To establish the robustness of our findings and guide future researchers, we explore the sensitivity of our findings to alternative groupings for defining relative timing.

Table 11, columns (1) and (2), establish our main findings are robust to grouping firms by calendar-quarter earnings seasons in which they announce (e.g., Jan.–Mar. vs. Apr.–Jun.), rather than by the date of their fiscal period end, as in our main tests. Similarly, the columns (3) and (4) of Table 11 qualitatively reproduce our main results

Table 10
Tests using within-industry earnings announcement order measures.
The three panels below report estimates from running the same regressions used in Tables 3–5, respectively, except using within-industry measures of $\Delta EAOrder$. See Appendix C for variable definitions. All continuous variables are standardized to have a mean of zero at the fiscal quarter end date × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. Stock returns are not winsorized. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Media coverage		
	ΔIn(1+Media-Generated Articles during [EA–1, EA+1])	
	(1)	(2)
ΔWithin-Industry EA Order (pattern)	−0.178***	
	(−2.700)	
ΔWithin-Industry EA Order (all)		−0.196**
		(−2.455)
Controls?	Yes	
N	19,252	19,252
S.E. clustering	Industry and Year-quarter (two-way)	
Standardization level	Fiscal Quarter End Date × Industry	
R-sq	0.014	0.014

Panel B: Trading volume		
	ΔAverage Trading Volume during [EA–1, EA+1]	
	(1)	(2)
ΔWithin-Industry EA Order (pattern)	−3.457***	
	(−3.259)	
ΔWithin-Industry EA Order (all)		−2.615**
		(−2.068)
Controls?	Yes	
N	18,314	18,314
S.E. clustering	Industry and Year-quarter (two-way)	
Standardization level	Fiscal Quarter End Date × Industry	
R-sq	0.079	0.079

Panel C: Earnings announcement premia		
	ΔMarket-Adjusted Stock Return [EA–1, EA+1]	
	(1)	(2)
Quintile for ΔWithin-Industry EA Order (pattern)	−0.455***	
	(−2.890)	
Quintile for ΔWithin-Industry EA Order (all)		−0.444**
		(−2.247)
Controls?	Yes	
N	18,258	18,258
S.E. clustering	Industry and Year-quarter (two-way)	
Standardization level	Fiscal Quarter End Date × Industry	
R-sq	0.106	0.105

after limiting our sample to firms with standard fiscal period ends (i.e., the blue bars in Panel B of Fig. 1), which creates four cleanly identified earnings seasons and thus makes grouping by fiscal quarter ends effectively the same as grouping by calendar uarter earnings seasons. The robustness of our findings is unlikely surprising given these within-firm changes in measures are more than 90% correlated with each other (correlations untabulated). However, the results in Table 11 suggest the relative attractiveness of these alternative approaches likely depends on the researchers' intended question as well as how they prioritize sample size.

The collective results in Tables 10 and 11 mitigate concerns our inferences depend on a particular implementation. Moreover, the results of this section highlight the broader potential use of calendar rotations for future researchers. Specifically, these are tests that researchers can use to understand drivers of sample composition, differing results across Pattern versus non-Pattern firms, as well as the potential for studying hypotheses based on within-industry or calendar-based variation in information arrival.

8. Conclusion

In this study, we develop a novel methodology for studying the impact of the timing of information flows. Our methodology leverages quasi-exogenous variation in prescheduled information events attributable to the specific day of the week on which a calendar month begins,

Table 11
Tests using alternative groupings.
The three panels below report estimates from running the same regressions used in Tables 3–5, respectively. The first two columns of each panel report results from using measures of Δ*EA Order* grouped by calendar quarter. These alternative measures are fiscal-quarter-matched changes in the ratio of n of N, where N is the number of earnings announcements in the same calendar quarter and n is the ranking of an earnings announcement among N announcements. The last two columns of each panel report results from using measures of Δ*EA Order* grouped by fiscal quarter end date on a subsample of firms with "standard" fiscal quarter end dates (i.e., Mar. 31, Jun. 30, Sep. 30, and Dec. 31). See Appendix C for variable definitions. All continuous variables are standardized to have a mean of zero at the calendar year-quarter × industry level, and are winsorized at the 1% and 99% levels to limit the influence of outliers. Stock returns are not winsorized. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by industry and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Media coverage

	Δln(1+Media-Generated Articles during [EA–1, EA+1])			
	Grouping by calendar quarter		Using "standard" fiscal quarters	
	(1)	(2)	(3)	(4)
ΔEA Order (pattern)	−0.302***		−0.249***	
	(−3.058)		(−2.837)	
ΔEA Order (all)		−0.231**		−0.176*
		(−2.194)		(−1.916)
Controls?	Yes			
N	19,252	19,252	16,644	16,644
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Year-quarter × Industry			
R-sq	0.014	0.013	0.013	0.012

Panel B: Trading volume

	ΔAverage Trading Volume during [EA–1, EA+1]			
	Grouping by calendar quarter		Using "standard" fiscal quarters	
	(1)	(2)	(3)	(4)
ΔEA Order (pattern)	−4.456***		−3.988***	
	(−3.133)		(−3.203)	
ΔEA Order (all)		−3.620**		−3.101**
		(−2.218)		(−2.167)
Controls?	Yes			
N	18,314	18,314	15,710	15,710
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Year-quarter × Industry			
R-sq	0.076	0.076	0.080	0.080

Panel C: Earnings announcement premia

	ΔMarket-Adjusted Stock Return [EA–1, EA+1]			
	Grouping by calendar quarter		Using "standard" fiscal quarters	
	(1)	(2)	(3)	(4)
Quintile for ΔEA Order (pattern)	−0.609***		−0.469**	
	(−2.895)		(−2.481)	
Quintile for ΔEA Order (all)		−0.661***		−0.483*
		(−3.238)		(−1.869)
Controls?	Yes			
N	18,258	18,258	15,554	15,554
S.E. clustering	Industry and Year-quarter (two-way)			
Standardization level	Year-quarter × Industry			
R-sq	0.109	0.109	0.106	0.106

which changes across calendar years outside managers' control. We refer to the resulting changes in announcement timing as calendar rotations, which are designed, and best suited, for studies in which separating the effects of an announcement's timing from its content is of first-order importance.

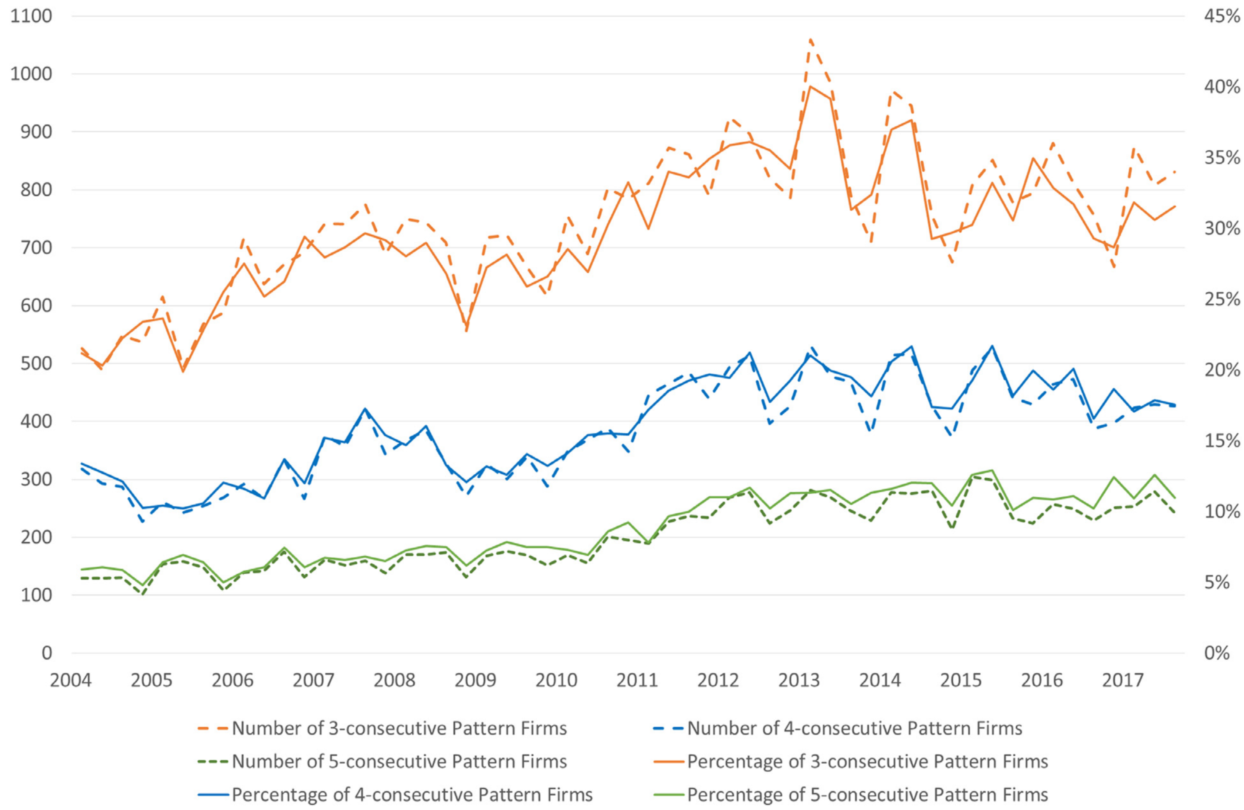
In applying our methodology, we show firms whose earnings announcements are moved forward by calendar rotations experience greater media coverage, heightened attention from analysts and investors, and increases in earnings announcement premia. Taken together, our study details a method for studying how the timing of information flows impacts outcomes of interest to financial economists, and provides evidence that the sequence of news shapes the behavior of informational intermediaries and the dynamics of market prices.

Appendix A. Pattern sample

The first table reports the frequency of each earnings announcement pattern from 2004 to 2017. One firm quarter can be counted more than once if its pattern is consistent with more than one pattern definition. Only the firm quarters that have existed for equal to or more than four consecutive same quarters are considered. The second table reports firm-level averages of various characteristics for Pattern and non-Pattern firms and their differences. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively. The figure below plots time trends in the number and percentage of firms following earnings announcement patterns. A firm is considered to have an earnings announcement pattern if it has followed the pattern for at least 3/4/5 consecutive firm quarters. Only the firm quarters that have existed for more than 3/4/5 consecutive same quarters are considered.

Pattern	Frequency	Percentage
same k-th weekday of same month (e.g., 1st Thursday)	10,359	7.1%
same k-th weekday since fiscal period end (e.g., 1st Thursday)	10,187	6.9%
same weekday of same n-th week since fiscal period end (e.g., Thursday of 2nd week)	8486	5.8%
same weekday of same n-th week of same month (e.g., Thursday of 2nd week)	5629	3.8%
same k-th trading day (i.e, nonweekend and nontrading holiday) of same month	743	0.5%
same k-th nonweekend day of same month (e.g., 11th nonweekend day)	742	0.5%
same k-th nonweekend day since fiscal period end (e.g., 11th nonweekend day)	610	0.4%
same k-th trading day (i.e, nonweekend and nontrading holiday) since fiscal period end	583	0.4%
same calendar month and date (e.g., May 11th)	111	0.1%
same k-th day since fiscal period end	110	0.1%
Total firm quarters (basis for %)	131,108	

	Main samples			Propensity score matched samples		
	Avg. for Pattern	Avg. for non-Pattern	Diff.	Avg. for Pattern	Avg. for non-Pattern	Diff.
SURP	0.0005	−0.0007	0.0012***	0.0004	0.0005	−0.00003
|SURP|	0.004	0.008	−0.004***	0.004	0.004	−0.00004
ROA	0.010	0.002	0.009***	0.010	0.010	0.001
BTM	0.52	0.55	−0.03***	0.52	0.53	−0.001
MVE (in millions $)	9101	6024	3,077***	2369	2396	-26
Capex	0.024	0.026	−0.001***	0.024	0.024	−0.0001
Leverage	0.570	0.575	−0.006***	0.570	0.570	−0.00002
Institutional Ownership	0.66	0.61	0.05***	0.66	0.66	0.0002
Stock Volatility	0.022	0.026	−0.004***	0.022	0.022	−0.00002
Size Adjusted BHR	0.004	0.001	0.003**	0.005	0.005	-0.0004
Insider Trading	0.313	0.317	−0.004	0.347	0.344	0.003
Loss Indicator	0.15	0.24	−0.09***	0.15	0.15	0.001
Num. of Analysts Following	10.94	9.80	1.14***	8.85	8.90	-0.04
Analyst Dispersion	0.10	0.15	−0.05***	0.11	0.11	0.001
N	19,252	76,622		19,247	19,247	



Appendix B. Excerpt from Emerson Electric's bylaws

This appendix shows an excerpt from Emerson Electric's bylaws. By its bylaws, Emerson Electric holds annual meetings with shareholders as well as annual meetings of the Board on the first Tuesday in February of each year, and additional regular meetings with shareholders on the first Tuesday of each month. Refer to Emerson Electric's web page for a complete list of its bylaws (https://www.emerson.com/en-us/investors/corporate-governance/bylaws).

ARTICLE II: MEETINGS OF SHAREHOLDERS. Section 2. Annual Meeting. The annual meeting of the shareholders shall be held on the first Tuesday in February of each year if not a legal holiday, or, if a legal holiday, then on the next business day following, at such hour as may be specified in the notice of the meeting; provided, however, that the day fixed for such meeting in any year may be changed by resolution of the Board to such other day not a legal holiday as the Board may deem desirable or appropriate. [...]

ARTICLE III: DIRECTORS. Section 8. Additional Regular Meetings. Additional regular meetings of the Board shall be held once each month on the first Tuesday thereof, or on such other day thereof as the Board may, by resolution, prescribe, and at such hour of such day as shall be stated in the notice of the meeting; [...] If the first Tuesday of any month shall be a legal holiday, the regular meeting for such month shall be held on the Thursday following, and if the Monday preceding the first Tuesday of any month shall be a legal holiday, the regular meeting for such month shall be held on the Wednesday following, in each case unless the Board shall otherwise prescribe by resolution. [...]

Appendix C. Variable definitions

Main variables	
ΔEA Order	The fiscal-quarter-matched change in the ratio of *n* over *N*, where *N* is the number of firms that have the same fiscal quarter end date in a given comparison group and *n* is the ranking of a firm's earnings announcement timing among *N* firms. *N* varies for four different measures of *ΔEA Order*.
ΔEA Order (pattern)	*ΔEA Order*, where *N* = number of firms with the same fiscal quarter end date that have followed an earnings announcement pattern for at least four consecutive same fiscal quarters.
ΔEA Order (non-Pattern)	*ΔEA Order*, where *N* = number of firms with the same fiscal quarter end date that have existed for at least four consecutive same fiscal quarters but have not followed an earnings announcement pattern or have followed an earnings announcement pattern for less than four consecutive same fiscal quarters.
ΔEA Order (all)	*ΔEA Order*, where *N* = number of firms with the same fiscal quarter end date that have existed for at least four consecutive same fiscal quarters.
ΔWithin-Industry EA Order (pattern)	*ΔEA Order*, where *N* = number of firms in the same Fama-French 12-industry classification that have the same fiscal quarter end date and have followed an earnings announcement pattern for at least four consecutive same fiscal quarters.
ΔWithin-Industry EA Order (all)	*ΔEA Order*, where *N* = number of firms in the same Fama-French 12-industry classification that have the same fiscal quarter end date and have existed for at least four consecutive same fiscal quarters.
Quintile for ΔEA Order	A variable that takes the value of 0 for the lowest quintile of *ΔEA Order*, 0.25 for the second lowest quintile of *ΔEA Order*, 0.50 for the third lowest quintile of *ΔEA Order*, and so on and so forth. The variable takes one of the five values: 0, 0,25, 0,5, 0.75, and 1.
Δln(1+Media-Generated Articles)	The fiscal-quarter-matched change in the natural logarithm of one plus the number of media-generated articles about a firm in the three-day window around the earnings announcement [EA–1, EA+1]. The media coverage data is obtained from RavenPack News Analytics Dow Jones Edition.
ΔAverage Trading Volume	The fiscal-quarter-matched change in the average of daily share volume divided by the number of shares outstanding in the three-day window around the earnings announcement [EA–1, EA+1].
ΔMarket-Adjusted Stock Return	The fiscal-quarter-matched change in market-adjusted stock returns. They are measured for the three-day window around the earnings announcement [EA–1, EA+1] or the 29-day period after the earnings announcement [EA+2, EA+30].
Other variables	
ΔSURP	The fiscal-quarter-matched change in the difference between the current EPS before extraordinary items and the median of analysts' forecasts in the 90 days prior to the earnings announcements scaled by the stock price per share at the fiscal quarter end.
Δ|SURP|	The fiscal-quarter-matched change in the absolute value of *SURP*, which is the difference between the current EPS before extraordinary items and the median of analysts' forecasts in the 90 days prior to the earnings announcements scaled by the stock price per share at the fiscal quarter end.
ΔROA	The fiscal-quarter-matched change in net income scaled by average total assets.
ΔBTM	The fiscal-quarter-matched change in book value of equity scaled by market value of equity.
Δln(MVE)	The fiscal-quarter-matched change in the natural logarithm of price per share × number of shares outstanding. The unit for MVE is millions of USD.
ΔCapex	The fiscal-quarter-matched change in capital expenditure scaled by total assets.
ΔLeverage	The fiscal-quarter-matched change in total liabilities scaled by total assets.
ΔInstitutional Ownership	The fiscal-quarter-matched change in the percentage of institutional investors by the quarter end obtained from Thomson Reuters.
ΔStock Volatility	The fiscal-quarter-matched change in the standard deviation of daily stock returns over the three-month period before the earnings announcement date.
ΔSize-Adjusted BHR	The fiscal-quarter-matched change in size-adjusted buy-and-hold abnormal return over the three-month period before the earnings announcement date.
ΔInsider Trading	The fiscal-quarter-matched change in the total insider trades (i.e., sales + purchases) of the CEO and CFO over the three-month period before the earnings announcement date scaled by shares outstanding at the beginning of the last fiscal quarter. The insider trades data is obtained from Thomson Reuters.
ΔLoss Indicator	The fiscal-quarter-matched change in *Loss Indicator. Loss Indicator* takes the value of 1 if the firm makes a loss in the fiscal quarter, and 0 otherwise.
ΔFriday Indicator	The fiscal-quarter-matched change in *Friday Indicator. Friday Indicator* takes the value of 1 if the firm announces earnings on a Friday, and 0 otherwise.
ΔForecast Indicator	The fiscal-quarter-matched change in *Forecast Indicator. Forecast Indicator* takes the value of 1 if at least one management forecast is provided on the earnings announcement date (i.e., bundled forecast).

(*continued on next page*)

(*continued*)

Δ*Same-Day EAs*	The fiscal-quarter-matched change in the number of earnings announcements provided on the same day divided by 100.
Δ*Analyst Coverage*	The fiscal-quarter-matched change in the natural logarithm of one plus the number of analysts following the firm during the last fiscal quarter obtained from I/B/E/S.
Δ*Analyst Dispersion*	The fiscal-quarter-matched change in the standard deviation of analyst EPS forecasts obtained from I/B/E/S.
Δ*Stock Illiquidity*	The fiscal-quarter-matched change in the quarterly average of daily bid-ask spreads measured by $100 \times (ask - bid)/[(ask + bid)/2]$ over the three-month period before the earnings announcement date. It is included as a control variable when the dependent variable is Δ*Average Trading Volume*.
Quintile for Δ*Variable*	A variable that takes the value of 0 for the lowest quintile of Δ*Variable*, 0.25 for the second lowest quintile of Δ*Variable*, 0.50 for the third lowest quintile of Δ*Variable*, and so on. The variable takes one of the five values: 0, 0,25, 0,5, 0.75, and 1.
Pattern Deviation	An indicator variable that takes the value of 1 if the firm quarter deviates from its earnings announcement pattern after following it for three consecutive same quarters, and 0 if the firm quarter continues to follow its pattern for the fourth consecutive time.
K-th Quartile of Negative (Positive) Δ*SURP*	An indicator variable that takes the value of 1 if the firm is in the *K*-th quartile of negative (positive) Δ*SURP*, and 0 otherwise.

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