The Speed of Price Responses to Individual Signals in a Bundle

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Abstract

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I investigate whether pricing speed varies across major quantitative signals bundled in earnings announcements. Theory predicts that, in the presence of processing frictions, signals with higher net benefits (benefits less processing costs) are priced faster than signals with lower net benefits. I test this prediction by developing a new methodology to measure and compare the speed of intraday price responses to earnings, revenue, and operating cash flow surprises from within the same announcement. Despite earnings news having the highest benefit, I find that revenue news is priced more quickly, consistent with revenue news having lower processing costs and thus higher net benefits. Operating cash flow news is priced the most slowly, consistent with relatively lower net benefits. My findings provide an economic explanation for investors' differential processing of quantitative signals in a bundle and paint a more complete picture of the price discovery process around disclosure events.

1. Introduction

The effect of processing frictions on investors' information choice and the speed with which prices incorporate disclosure information is a fundamental topic in capital markets research. Much of the existing theory and empirical evidence examine how differences in processing frictions *across* disclosure events affect the speed of price responses (see Blankespoor et al. 2020 for a review). However, nearly all disclosure events contain a bundle of individual signals *within* the event itself. Each signal in a bundle can convey incremental information (e.g., Hoskin et al. 1986; Lev and Thiagarajan 1993), has different informational characteristics (e.g., complexity, persistence), and potentially has different processing costs and benefits. Yet, there is a limited understanding of how investors process signals in a bundle and the forces that drive these processing decisions. I investigate the relative pricing speed of major quantitative signals bundled in quarterly earnings announcements (EAs) to shed new light on these decisions.

The motivation for examining pricing speed among bundled signals is twofold. First, understanding the overall speed of price response to a disclosure event necessitates a closer look at how investors process and trade on the individual pieces of information that make up the event. Only examining a single signal does not fully portray the price discovery process, especially as an increasing number of signals explain EA returns (Hand et al. 2021). Second, when comparing speed across signals in a bundle, the range of economic explanations for differences in speed is limited. Firm- or event-level explanations for delayed price responses, such as limits to arbitrage, liquidity, or transaction costs (e.g., Bhushan 1994; Sadka 2006; Ng et al. 2008), are unlikely to vary across signals within an event and thus are unlikely to explain any differences in speed. I contend that differences in speed are likely driven by a rational assessment of the expected benefits and costs to processing *each* signal in a bundle.

This benefit and cost perspective is based on the theoretical framework in Grossman and Stiglitz (1980). In their model, if processing costs exist and are meaningful, then investors must decide whether the benefits to processing a signal exceed its cost. Simply observing a signal does not immediately reveal its value implications because signals convey information with noise. As a result, investors must analyze how current realizations map into future performance to separate truth from noise. This type of analysis is costly—it requires time, effort, and resources to understand a signal's value implications, which constitute the integration cost component of processing (Blankespoor et al. 2019). Investors incurring these costs expect to benefit via higher risk-adjusted trading gains. While a signal's benefit is difficult to fully characterize, it can be described by its informativeness—the amount of variation it explains about fundamental value. These costs and benefits influence an investor's decision to become informed and the extent to which they trade aggressively on a signal. This framework predicts that a signal's pricing speed is increasing in its expected benefit and decreasing in its expected processing cost.

I use earnings, revenue, and operating cash flow (OCF) to examine whether signals with higher *net* benefits (benefits less processing costs) are priced more quickly. Prior literature indicates that these signals have different informational characteristics and therefore plausibly different costs and benefits.[1] Relative to earnings and OCF, revenue is less costly to process because it is disaggregated and is typically less difficult to forecast (Bradshaw et al. 2016; Cheng et al. 2020). Meanwhile, earnings and OCF aggregate heterogeneous items (Fairfield et al. 1996) which makes forecasting future values using realized values more difficult. OCF's predictive ability is further impaired by timing mismatches that stem from periodic reporting (Dechow

[1] Earnings, revenue, and OCF also have desirable properties for the empirical tests. Namely, each signal is widely available in the EA, is known to explain and move price (e.g., Bowen et al. 1987; Jegadeesh and Livnat 2006), and is frequently forecasted by sell-side analysts (Hand et al. 2021). This allows for relatively consistent measurement of market expectations and signal surprises to facilitate comparisons across signals.

1994), making it the most costly to process. Earnings, as the main summary measure of firm performance, typically explains the most variation in stock returns and thus offers a higher benefit relative to revenue and OCF.

On balance, I predict the following. If processing costs are trivial, then each signal will be priced immediately and there will be no differences in speed. If processing costs are material, then OCF will be priced slower than earnings and revenue because OCF has a lower net benefit. Whether earnings is priced faster than revenue hinges on whether earnings' higher benefit outweighs its relatively higher processing cost.

1.1 Data and Empirical Methodology

I build a sample of 32,994 quarterly EAs spanning 2011 to 2017 and focus on intraday activity during the first EA trading day ("day 0"). I use an intraday time horizon because market activity on day 0 is an order of magnitude higher than on surrounding days (Beaver et al. 2020) and because longer window price drifts have significantly declined in recent years (Chordia et al. 2014; Martineau 2021). Narrowing in on day 0 also better identifies investors' processing of and trading on bundled signals shortly after their release by reducing the influence of subsequent events. In this way, an intraday horizon and three major quantitative signals act as a laboratory to study the link between net benefits and pricing speed for bundled signals.

To test my predictions, I develop a methodology to estimate the speed of price response to individual signals in a bundle and compare speed across signals.[2] I measure speed by estimating how a signal's news is priced over various "short" intervals *relative* to its total news

[2] As discussed in Section 3.2, comparing speed across signals within a disclosure event using existing methodologies is difficult. The methodology I develop is similar to intraperiod timeliness/efficiency (IPT/E) measures used in the price discovery literature (e.g., Butler et al. 2007; Bushman et al. 2010; Blankespoor et al. 2018). A key difference is that IPT/E only uses stock returns and therefore cannot distinguish when specific signals are reflected in price, especially if those signals come from the same disclosure event.

priced over all intervals. In effect, I trace out the path of price adjustment for each signal on day 0 and collapse this path into a single statistic to create signal-specific areas-under-the-curve (AUCs). Importantly, I employ a scaling technique that normalizes the total magnitude of each response. This allows me to empirically isolate speed from strength and measurement error and permits more direct comparisons of speed across signals. I utilize a resampling approach for statistical inference due to the lack of readily available standard errors when creating the AUCs.

1.2 Empirical Analyses

The main analysis reveals differences in pricing speed across signals, as depicted in Figure 3, Panel A. Revenue is priced the most quickly, followed by earnings, and finally by OCF, and these differences are statistically significant. Approximately 89%, 78%, and 43% of total revenue, earnings, and OCF news are priced at market open and 94%, 87%, and 68% are priced by the end of the first 30 minutes of trading, respectively. These findings indicate that investors do not equally process and trade on quantitative signals in a bundle on the first EA trading day, which suggests the existence of material processing costs. Moreover, observing that revenue is priced faster than earnings—despite earnings have a higher benefit (Figure 3, Panel B and Table 3)—implies that processing costs are relatively lower for revenue such that it has a higher *net* benefit. Finding that OCF is priced slower than both earnings and revenue is consistent with OCF unambiguously having lower net benefits.

I extend the time window beyond day 0 and find evidence of additional processing on subsequent days. However, the drifts decay quickly; there is no statistically or economically significant drift after day 3 (through day 60) and the order of pricing speed remains the same. These findings suggest that much of the total price adjustment happens within day 0 and supports the use of an intraday time horizon relative to longer horizons. Further, there does not appear to be evidence of different speed across signals on the first or 10th trading day before the EA. These pre-EA days offer a falsification in that pricing speed should not differ if investors cannot easily process and trade on signals that have not yet been disclosed. They also add comfort that the main results are not merely an artifact of the data or methodology.

Embedded in the main tests is the notion that earnings, revenue, and OCF have different benefits and processing costs which result in different pricing speeds. In this setting, processing costs largely stem from the time and effort it takes to understand how realized values map into future values. I validate this assumption by examining each signal's persistence under the idea that it is more difficult and costly to use realized values to forecast future values for less persistent signals (Bradshaw et al. 2016; Cheng et al. 2020). I find that revenue is more persistent than earnings and OCF, and earnings is more persistent than OCF, consistent with predicted ordering of processing costs. I also find that earnings explains significantly more variation in announcement day returns than revenue and OCF, and revenue explains more than OCF. This further validates different benefits across signals.

I next investigate three sources of cross-sectional heterogeneity to home in on whether differences in signal net benefits or investor sophistication relate to differences in speed. Earnings tend to be less persistent and less informative for firms reporting losses relative to firms reporting profits (Hayn 1995), which suggests lower net benefits and thus slower earnings pricing speed for loss firms. I expect revenue to be priced slower for firms with low or negative sales growth as these firms tend to have less persistent and less informative revenue (Ertimur et al. 2003). I also expect each signal to be priced more quickly when there is a higher proportion of sophisticated investors as these investors have greater ability and resources to process bundled signals (Lee and Zhu 2021). The results are consistent with these expectations and show relevant cross-sectional variation in pricing speed.

Although the evidence so far indicates that pricing speed varies across signals in a manner consistent with different net benefits, an alternative interpretation remains. My prediction is based on rational theory in which costly information is a prerequisite for affecting investors' processing decisions and pricing speed. Alternatively, my results could be consistent with theories that do not require costly information, such as behavioral theory underlying differences-of-opinion models. In these models, investors *freely* process signals but "agree to disagree" about their interpretation (e.g., Kandel and Pearson 1995), such that higher disagreement leads to slower pricing speeds. Banerjee et al. (2020) point out that, because rational models require investors to correctly condition on prices, a test to help differentiate these explanations is to examine return drifts based on past return signals. Specifically, rational models generate no (or negative) return-based drift while behavioral models generate positive return-based drift because investors neglect information in price. I find a negative relation between post-opening interval returns and opening returns, which is more consistent with rational models with costly processing than behavioral models with costless processing.

1.3 Contributions

This paper contributes to literature on disclosure processing costs, investors' information choice, and capital market outcomes (Blankespoor et al. 2020). Prior literature provides a limited understanding of the factors that determine investors' processing of bundled signals and how this affects the price discovery process. For instance, Sloan (1996) and Jegadeesh and Livnat (2006) find that investors do not efficiently price different components of earnings. Yet why this occurs remains an open question. I fill this void by showing that investors' processing of quantitative signals in a bundle is likely driven by a rational assessment of each signal's expected benefit and processing cost, which results in differences in pricing speed. In this way, I complement prior work by dialing in on the economic forces that explain differences in pricing speed for individual signals in a bundle.

Examining pricing speed among bundled signals also provides a more detailed look at the overall speed of price response to disclosure events. Most disclosure events contain a bundle of signals, so understanding the overall speed of price response requires an individual analysis of speed for the signals that make up the event. Further, observing these differences in speed for earnings, revenue, and OCF suggest that processing frictions are important and likely extend to signals with even greater costs (e.g., qualitative information in Engelberg 2008).

I also contribute to the price discovery literature by developing a new method to compare pricing speed across individual signals in a bundle. Prior studies employing measures of speed have used a single signal (e.g., DellaVigna and Pollet 2009; Santosh 2019) or only used stock returns (e.g., Butler et al. 2007; Bushman et al. 2010; Blankespoor et al. 2018). Inherent features of these measures limit meaningful comparisons of speed across signals from the same disclosure event. The methodology developed herein is flexible and can be used for different signals, disclosure events, and time horizons, thereby enriching our understanding of the price discovery process.[3] While the methodology has limitations, it presents a step forward to isolate and analyze pricing speed for events with bundled signals.

Lastly, I complement both early and more recent work on the pricing of accounting signals at an intraday level (e.g., Patell and Wolfson 1984; Lee 1992; Lee et al. 1993; Bochkay et

[3] Richardson et al. (2010) state that "[the] speed with which accounting information is incorporated into stock prices is typically given only a cursory treatment in academic research" (p. 447) My study helps answer this call by developing a parsimonious measure of the speed with which prices incorporate different accounting signals.

al. 2021; Gregoire and Martineau 2021). While I find that most of the total price response to accounting signals occurs shortly after market open, there is still drift within the first trading day and this drift is not equal across signals. Examining delayed price responses on a more granular level is warranted as capital markets have become increasingly efficient over time and as longer window drifts have significantly attenuated (e.g., Richardson et al. 2010; Martineau 2021).

2. Theory and Predictions

In this section I argue that investors' processing of bundled signals is based on a rational assessment of expected processing costs and benefits, and these expected net benefits are the primary economic factor driving differences in pricing speed across signals. I start by discussing theoretical models of costly information and then transition into how costs and benefits can vary among quantitative signals. I summarize and make my predictions at the end.

2.1 Costly Information Models

Classic rational expectations pricing models suggest that information signals are immediately reflected in price in the absence of costly information (Grossman and Stiglitz 1980; Verrecchia 1982; Vives 2010). The underlying rationale is that, when a signal is costless to process, all investors choose to become informed.[4] In turn, prices immediately impound the signal and there will be no relation between a signal and subsequent period's stock returns (i.e., maximum speed or no drift). This holds regardless of other features of the model, such as a signal's informativeness, investors' risk aversion, or variability in the supply of the risky asset.

[4] This class of models typically includes informed, uninformed, and noise traders. Informed traders condition on a signal and price, uninformed traders condition only on price, and noise traders condition on nothing and trade for non-informational reasons (e.g., portfolio re-balancing). If a signal can be perfectly observed at no cost, then the problem reduces to informed and noise traders. In this case, any incoming quotes that differ from the full-information valuation will be known (with certainty) to come from noise traders, and informed traders will instantaneously adjust price back to its full-information level.

Alternatively, when information is costly, these models predict that higher expected processing costs result in slower pricing speeds.[5] This operates through an investor informedness channel—the presence of processing costs means that some investors may not observe a signal at all, or some may observe a signal with additional noise. Notably, a smaller fraction of investors chooses to become informed with respect to a signal as costs increase (e.g., comparative static #7 in Grossman and Stiglitz 1980). The smaller the fraction of informed investors, the less that price incorporates the signal in the first period of trade relative to subsequent periods—that is, slower pricing speed. In effect, investors condition on noisier versions of signals when processing costs are higher, and they initially trade less aggressively on noisier signals.

While processing costs in these models typically pertain to "private" signals, they also apply to "public" signals, such as realized values of sales, earnings, and cash flows in firms' accounting disclosures (Blankespoor et al. 2020). A central part of valuation is forecasting; investors forecast future values as best as they can and often use realized values to do so (e.g., Graham et al. 1962). Because signals imperfectly convey information about firm performance, simply observing a realized value may not immediately reveal how a signal is expected to translate into future performance. It takes time, effort, and resources to analyze and understand how realized signals map into fundamental value and the weight to place on them—the integration cost component of disclosure processing.[6] It can be prohibitively costly for an

[5] Prior empirical research generally finds results consistent with this prediction. For instance, pricing speed is slower for firms with more complicated organizational structures (Cohen and Lou 2012), firms with more foreign operations (Huang 2015), disclosures that are longer and more complex (You and Zhang 2009), and disclosures that are discussed in footnotes versus recognized in financial statements (Michels 2017). These papers identify differences in processing costs *across* firms or disclosure events, whereas I focus on differences across signals *within* a disclosure event.

[6] Following Blankespoor et al. (2019), disclosure processing entails three sequential steps: 1) learning that the signal exists (awareness); 2) acquiring or extracting the signal from a disclosure (acquisition); and 3) analyzing the implications of the signal for firm value (integration). As it relates to major quantitative signals bundled in EAs, awareness and acquisition costs are likely to be minimal relative to integration costs as each signal exists in the EA and is relatively easy to acquire.

investor to completely understand the value implications upon receiving a signal (e.g., it is infeasible to hire 1,000 analysts to analyze the signals in an EA). Instead, an investor may learn about the value implications of a signal by observing market prices, information released shortly after the EA (e.g., conference call, sell-side analyst outputs), and/or combining the signal with other contextual information. In this way, marginal integration costs are expected to gradually decline as additional time and other bits of information help investors more precisely incorporate a signal into their valuations and trades.

2.2 Processing Costs Across Signals

A key aspect is that each signal in a bundle is noisy. In this context, noisier signals have higher integration costs, and investors initially trade less aggressively on noisier signals. They trade more aggressively over time as increased integration efforts and additional information reduce noise, which leads returns to continue in the direction of the signal's news. I contend that integration costs are high initially, vary across quantitative signals, and decline quickly over a short period of time as investors observe market prices and other confirmatory disclosures.

As it relates to earnings, revenue, and OCF, the integration efforts needed to understand each signal stem from their informational characteristics. Specifically, integration efforts are largely influenced by the level of aggregation and the difficulty in using current realized values to forecast future values. An aggregated signal with heterogenous components is more costly to integrate (relative to a disaggregated signal) because investors must expend resources to decompose the signal to fully understand its value implications.[7] Earnings and OCF aggregate

[7] Heterogeneity in the components is key. If the individual components equally contribute to the variability of the signal (i.e., it aggregates homogenous components), then it would be less costly to integrate an aggregated signal (Holzman et al. 2021). For instance, consider an aggregated signal with two components—one permanent and one transitory. Upon receiving the aggregated signal, investors may not immediately know to what extent the realized value is primarily driven by the permanent component or the transitory component. In turn, they must expend additional integration efforts to ascertain the value implications of the realized signal.

items that often have different persistence (e.g., Fairfield et al. 1996; Holzman et al. 2021) or other varying implications for a firm's core business operations (e.g., R&D versus SG&A expense). In contrast, revenue is a disaggregated signal and typically exhibits high levels of persistence, which makes it less difficult to forecast (Bradshaw et al. 2016; Cheng et al. 2020). These features suggest it is more costly to integrate earnings and OCF relative to revenue.

Earnings and OCF also likely have different integration costs. While both suffer from timing and mismatch problems that stem from periodic reporting, these problems are more acute for OCF (Dechow 1994). Prior literature finds that earnings—especially operating earnings commonly emphasized in valuation—are more persistent and better predictors of future performance than OCF (e.g., Ball and Nikolaev 2020).[8] Moreover, OCF's importance as an incrementally informative signal may be more context dependent in that understanding the valuation weight to attach to cash flows may depend more on time-varying factors such as the firm's financing needs. These features suggest it is more costly to integrate OCF relative to earnings. Collectively, these differences in informational characteristics suggest that revenue is the least costly to process, earnings more costly, and OCF the most costly, on average.

2.3 Benefits Across Signals

When information is costly, investors expect to be compensated for their costly processing activities via increased risk-adjusted trading gains. Rational investors assess whether the expected benefit from processing a signal exceeds the expected cost when deciding which signals to process and include in their information set.

[8] Admittedly, this evidence is mixed. Some papers find that OCF is a better predictor of future OCF than earnings (e.g., Nallareddy et al. 2020). These papers often use bottom-line earnings, which are known to include transitory components that impair predictive ability and result in lower persistence. Prior research generally finds that investors and analysts exclude these transitory components when analyzing earnings and trading around EAs (Bradshaw and Sloan 2002). Thus, the most apt comparison is between a measure of operating earnings and OCF.

In canonical pricing models, a signal's benefit can be characterized by its informativeness—the amount of variation it explains about fundamental value.[9] Signals that explain more variation are more beneficial in trading decisions, since omitting an informative signal increases the odds that investors misvalue the firm and trade at incorrect prices. Higher informativeness typically incentivizes investors to become informed (e.g., comparative static #6 in Grossman and Stiglitz 1980). For example, if one signal is expected to explain 10% of firm value and another is expected to explain 0.1%, investors are more likely to process the former signal, holding all else constant. As a larger fraction of investors becomes informed, prices incorporate more of the signal in the first period of trade relative to subsequent periods. Hence, benefits are expected to be positively related to pricing speed.[10]

Earnings, revenue, and OCF each offer different benefits to investors. Earnings is a highly beneficial signal that succinctly measures overall performance and frequently explains significant variation in fundamental value and stock returns (Dechow 1994). In contrast, revenue and OCF are less complete measures of true performance or risk. Revenue, which conveys information about top-line growth and market share, omits all information about costs and expenses that is needed to assess profitability and estimate future performance. OCF, an alternate summary measure to earnings, largely omits information about investment and suffers from period-to-period timing issues that are remedied via the accrual process. As less complete

[9] It is worth noting that it is not informativeness per se that yields benefits. Rather, benefits in these models are best captured by the ratio of future return variances for informed investors to uninformed investors (i.e., risk-adjusted trading gains). However, benefits of this form are simultaneously determined with the decision to become informed, so comparative statics are not easily determined. Using a signal's informativeness (a model primitive) to capture benefits is much more tractable, yields cleaner comparative statics, and allows for easier comparison across signals.
[10] Existing empirical research provides limited evidence on whether benefits (informativeness) affect speed (Jennings and Starks 1985). The lack of definitive evidence is potentially due to processing costs and benefits often moving in tandem and challenges in separating informativeness from speed using existing methods.

measures, both revenue and OCF provide less information about true value relative to earnings and thus are less beneficial to investors.

2.4 Summary and Predictions

In summary, there are plausible differences in costs and benefits across signals.[11] Revenue is the least costly to process, earnings more costly, and OCF the most costly. Earnings offers the largest benefit to investors while revenue and OCF offer a smaller benefit. Investors likely consider these costs and benefits when processing and trading on bundled signals at EAs.

Drawing on the theoretical link among processing costs, benefits, and pricing speed, I predict the following. If processing costs are negligible, then each signal in a bundle will be priced immediately. If processing costs are material, then signals with a higher net benefit (benefit less processing cost) will be priced faster than signals with a lower net benefit. Specifically, I predict OCF will be priced slower than earnings and revenue because OCF has both a lower benefit and a higher processing cost. I do not predict earnings will be priced faster or slower than revenue because it is unclear whether the higher benefit to processing earnings exceeds the relatively higher processing cost.

3. Data and Empirical Methodology

3.1 Data

I use data from Compustat, CRSP, IBES, Wall Street Horizons (WSH), RavenPack, and TAQ to construct a sample of quarterly EAs from January 1, 2011 to December 31, 2017, imposing several basic requirements as outlined in Table 1. I use timestamps from IBES, RavenPack (PR edition), and WSH to identify whether the EA occurs before, during, or after

[11] I provide some evidence validating differences in processing costs and benefits across signals in Section 5.

normal trading hours (9:30 AM to 4:00 PM EST). I exclude EAs made during normal trading hours and set after-hours announcements to the next trading day. These steps are crucial as I require a common starting point for all observations—the beginning of the first trading day of the EA. I require each firm-quarter to have at least one analyst forecast in IBES for each signal to maintain comparability in measuring market expectations and signal surprises.[12] I end with a sample of 32,994 firm-quarters (2,383 unique firms) for my analyses, which is roughly evenly distributed across calendar years.

The vast literature on post-earnings announcement drift (PEAD) typically examines pricing speed in the weeks and months after the EA (e.g., Bernard and Thomas 1990). I depart from this typical time frame and instead use an intraday time horizon on the first EA trading day. The first trading day is when the bulk of trading activity and price discovery happens, with much of it in the first 30 to 60 minutes after market open.[13] An intraday horizon helps to better identify investors' processing of and trading on quantitative signals shortly after their release and reduces the influence of subsequent corporate events. Furthermore, other disclosures (e.g., conference calls, analyst outputs) that help investors interpret the contents of the EA frequently occur on the first EA trading day (Kimbrough 2005; Zhang 2008; Driskill et al. 2020).

I collect intraday price data from TAQ and partition the first trading day into 40 discrete trading intervals. The first interval spans the first 30 seconds of trade at market open. Since all announcements in the sample occur before market open or after market close on the preceding

[12] This requirement results in sizable sample attrition, primarily due to the lack of OCF forecasts as tabulated in Table 1, Panel B. Results are similar if I do not impose this requirement (see Section A1 of the Online Appendix). Relatedly, I begin the sample in 2011 as this is the first year IBES had relatively consistent coverage of OCF forecasts (Hand et al. 2021).

[13] While I acknowledge the existence of delayed price responses over extended periods, recent papers have documented a decline in PEAD as well as a concentration in market activity in a short window around the EA relative to non-EA windows in recent years (Beaver et al. 2020; Martineau 2021). Section 4.3 examines pricing delays over longer horizons and finds evidence of drift up to 3 trading days after the EA but no statistically or economically significant drift from 4 to 60 trading days after the EA.

day, this initial interval contains a substantial price adjustment, which primarily reflects out-of-hours activity.[14] The remaining 39 intervals per day are evenly spaced, 10-minute intervals during normal trading hours, except for the second interval that spans 9 minutes and 30 seconds. For each of the intervals, I calculate volume-weighted average prices from all trades during the interval and use these prices to calculate raw returns. I then calculate abnormal returns (*ARet*) by subtracting the corresponding market return using the SPDR S&P 500 ETF Trust (Ticker: SPY).

I estimate earnings, revenue, and OCF surprises using data from IBES.[15] Following prior literature (e.g., Livnat and Mendenhall 2006), I calculate each surprise as the difference between the realized value and the median consensus analyst forecast, scaled by share price at fiscal quarter end. Surprise variables are decile ranked by calendar year-quarter to mitigate the influence of outliers and non-linearities and are centered on zero. The decile ranked surprises for earnings, revenue, and OCF are labeled *UE*, *UR*, and *UCF*, respectively. Correlations between the surprises range from 0.05 (*UR* and *UCF*) to 0.28 (*UE* and *UR*).

Descriptive information on firm-quarters and signals is in Table 2. Firms in my sample tend to be large with an average market capitalization of $12.8 billion and have an average institutional ownership of 73.6%. Slightly over half (50.9%) of EAs are made in the after-hours market and the average number of concurrent EAs is 214.52. The average raw surprise for earnings, revenue, and OCF is 2.3, 2.9, and -1.4 cents per share and the average number of

[14] I start the return measurement window at 9:30 AM rather than when the EA is released for multiple reasons. First, starting at the EA release would result in different calendar time windows depending on the EA timing. Given that some announcements happen after market close and some before market open, this potentially introduces a liquidity confound over the different time windows. Second, because completed trades are less common in out-of-hours sessions, quote data would likely need to be used, which can capture a different aspect of the price discovery process (Gregoire and Martineau 2021). Third, while I use timestamps from IBES, RavenPack, and WSH to identify the timing of the announcement, even small data errors in the timestamps can significantly distort inferences given the path-dependent nature of my empirical analyses.

[15] I do not use management guidance signals even though guidance surprises significantly explain EA returns when present (e.g., Hand et al. 2021). Not all firms provide guidance or only provide certain types of guidance (e.g., revenue but not earnings), so requiring guidance would reduce the sample even further and limit generalizability.

analysts forecasting each signal is 13.58, 11.18, and 2.81, respectively. Differences in the number of analyst forecasts is expected if analysts have different incentives to provide explicit forecasts for each signal. Finally, IBES reported actuals differ from GAAP more for earnings (average of 0.144 per share, or a 35.6% increase from GAAP actuals) than for revenue or OCF.

3.2 Empirical Methodology

3.2.1 Measuring Pricing Speed by Signal

I develop an empirical strategy and research design to test for differential pricing speed across signals in the same disclosure event as existing approaches do not feasibly allow such comparisons.[16] I define pricing speed as the rate at which the total amount of news in a signal is impounded in price. I measure this construct using an area-under-the-curve (AUC) approach that estimates how each signal is priced over various "short" intervals *relative* to the total priced over all intervals. In effect, I trace out the path of price adjustment for each signal over the first EA trading day and collapse this path into a single statistic by creating signal-specific AUCs. The intuition of an AUC metric is that it incorporates the entire trajectory of when a signal is reflected in price over a defined period, where a greater area is consistent with faster pricing, while allowing for heterogeneity in the total amount that each signal moves price. I contend that, for individual signals in a bundle, a signal with higher net benefits will have a higher AUC relative to a signal with lower net benefits. If processing costs are negligible or net benefits are similar between signals, then the AUCs will not be different.

[16] My approach is a blend of the ERC/PEAD and intraperiod timeliness/efficiency (IPT/E) methodologies to estimate pricing speed. It differs from ERC/PEAD by using an intraday horizon, capturing speed using a single statistic, and comparing signals beyond earnings. It differs from IPT/E in that IPT/E only uses stock returns and is therefore agnostic about the specific pieces of information that enter price over a given time period. In contrast, calculating response coefficients over different time intervals allows me to examine when different signals are reflected in price and draw comparisons across signals in a bundle. This feature is central to my study.

Figure 1 provides a visual outline of the steps involved in creating the signal-specific AUCs. I begin by estimating response coefficients over different trading intervals using a pooled linear regression, where the unit of observation is at the firm (i)-quarter (q)-trading interval (t) level (trading intervals are defined in Section 3.1):

$$ARet_{i,q,t} = \sum_{t=1}^{40} \left(\boldsymbol{\beta}_{\boldsymbol{t,U(X=\{E,R,CF\})}} (\gamma_t \times U(X = \{E, R, CF\})_{i,q}) \right) + \gamma_t + \alpha_i + \varepsilon_{i,q,t}$$

I perform the regression for each signal individually, though I include all three signals in a single regression model in Section A2 of the Online Appendix. Indicator variables (γ_t) for each trading interval are interacted with the signals to capture variation in the response coefficients over the different intervals. For instance, $\beta_{1,UE}$ is the earnings response coefficient for the 1st trading interval, $\beta_{14,UR}$ is the revenue response coefficient for the 14th trading interval, and $\beta_{38,UCF}$ is the OCF response coefficient for the 38th trading interval. The model includes firm fixed effects (α_i) to strip out static unobservable differences across firms that may correlate with returns or signal surprises in an unknown way.[17]

Equipped with these response coefficients, I next calculate the cumulative scaled response coefficient (CSRC) for each interval by dividing the cumulative sum through that interval by the cumulative sum through the end of the period, doing so for each signal ($CSRC_{\tau,UX} = \sum_{t=1}^{\tau} \beta_{t,UX} / \sum_{t=1}^{40} \beta_{t,UX}$). For example, a CSRC of 0.90 for *UE* at interval 15 means that 90% of the total earnings news is priced through interval 15, where total is based on the chosen ending point (interval 40, or end of day). The CSRC for each interval will typically range from 0 to 1 and by construction is equal to 1 for the final interval.

[17] I re-estimate the model without firm fixed effects and find qualitatively similar results in Section A3 of the Online Appendix. I do not include fixed effect interactions with the signal surprises to avoid overfitting the data.

I then calculate an AUC for each signal (AUC_{UE}, AUC_{UR}, and AUC_{UCF}) by summing the CSRC over all intervals using a linear trapezoidal rule to approximate a definite integral. Mathematically, this is expressed as $AUC_{UX} = \frac{1}{40}\left(\sum_{t=1}^{39} CRSC_{t,UX} + 0.5\right)$, which is similar to IPT measures used in the price discovery literature.[18] Finally, I calculate pairwise differences in AUCs between signals, labeled ΔAUC. There are three ΔAUCs in total—ΔAUC_{ER} is the difference in AUCs for earnings and revenue (i.e., $AUC_{UE} - AUC_{UR}$), ΔAUC_{ECF} for earnings and OCF, and ΔAUC_{RCF} for revenue and OCF.

A notable feature of this approach is that I scale the cumulative response coefficients by their end of period total when calculating each CSRC and AUC. This is done to best capture the *speed* of price response rather than the *strength* of the response. Not scaling would result in higher AUCs for signals with greater information content or less measurement error, even if it takes longer for those signals to reach their final values. That is, signals have varying implications for fundamental value and varying measurement error, and thus exhibit different total response coefficients (i.e., different strength). Additionally, scaling allows me to combine both the initial and subsequent price response into a parsimonious measure of pricing speed and, importantly, facilitates comparisons of speed across signals. Only examining the magnitude of the response coefficients in periods after the initial response does not necessarily capture how quickly a signal enters price in its entirety.[19]

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[18] I also calculate a version that adjusts for overreactions in the price response. An overreaction occurs if the CSRC for any interval exceeds 1, since this indicates that prices partially reverse in future intervals. I follow Blankespoor et al. (2018) and calculate adjusted AUCs as follows: $AUC_{UX}^{a} = \frac{1}{40}\left(\sum_{t=1}^{39}\left(1 - \frac{|CRC_{40,UX} - \sum_{t=1}^{39} CRC_{t,UX}|}{|CRC_{40,UX}|}\right) + 0.5\right)$.

[19] A simple example illustrates this point. Suppose there are two periods and two signals. The first signal has a response coefficient of 0.8 in period 1 and a coefficient of 0.2 in period 2, for a total of 1.0 over both periods. The second signal has a response coefficient of 0.15 in period 1 and a coefficient of 0.1 in period 2, for a total of 0.25 over both periods. Comparison of the coefficients in period 2 alone would suggest slower pricing speed for the first signal (since 0.2 > 0.1). However, a smaller portion of the *total* news for the first signal is priced in period 2 ((0.2/1.0 = 20%) < (40% = 0.1/0.25)), which indicates that the first signal is priced more quickly based on how I define pricing speed.

While the AUC methodology permits more direct comparisons of speed across bundled signals, the importance of speed should be examined alongside the strength of the response. Signals with very low strength have small denominators in the AUC calculation, which can result in unstable or uninterpretable estimates (see Section 4.3). Furthermore, signals with low strength are unlikely to explain asset prices and thus do little to advance our understanding of overall price responsiveness to disclosure events (Blankespoor et al. 2020). Even if a low strength signal is priced faster than a high strength signal, it is possible that a high strength signal has a greater overall impact on capital markets. Nevertheless, differences in speed inform us about investors' processing of bundled signals and complement prior work on the strength of price responses for different signals (e.g., Hoskin et al. 1986; Hand et al. 2021).

3.2.2 Test Statistics

My prediction is a test of whether the ΔAUCs are statistically different from zero, as ΔAUCs are expected to be zero if pricing speed between two signals is equal. The primary test statistic for this prediction is ΔAUC itself, and to estimate statistical significance, I create a sampling distribution of ΔAUC using a simulation (or resampling) analysis. I use a simulation-based approach for statistical inference as standard errors and standard test statistics are not readily available due to the transformation of observed data (e.g., *ARet*, *UE*) into regression coefficients when creating AUC and ΔAUC. Resampling approaches are common in the price discovery literature (Butler et al. 2007; Bushman et al. 2010).

I employ a procedure that utilizes the structure of my main regression and follows the guidelines in econometrics textbooks such as Davidson and MacKinnon (2004) and Hansen (2020). I leave most of the technical details to the Appendix and outline the approach in Figure 2. Broadly, the procedure generates variation in the observed AUCs and ΔAUCs (labeled $\widehat{AUC}$

and $\widehat{\Delta AUC}$) by resampling; it draws random errors from the empirical distribution of regression residuals and goes through the steps in Section 3.2.1, calculating test statistics for each iteration. I resample 1,000 times and create bootstrapped percentiles and p-values, which effectively count the number of iterations that each ΔAUC equals zero or changes signs (e.g., positive to negative). I use these bootstrapped values to assess statistical significance.

4. Main Empirical Analyses

I primarily present the pricing speed results in graphical form to more clearly portray the time-series nature of my analyses. Formal statistical tests of ΔAUCs are presented in table form.

4.1 Main Pricing Speed Results – Graphical Evidence

Figure 3 presents the main results. The different curves in Panel A—for earnings, revenue, and OCF—trace out the amount of news in each signal that is priced through each interval relative to the total priced at period end (i.e., the CSRC at each interval *t*). The shaded areas estimate the variability in each of the curves using the 2.5th and 97.5th percentiles of the simulated distributions. As expected, there are significant jumps for the 1st interval as price changes between the prior day's close and the first 30 seconds of market open reflect investors' processing of and trading on information released outside of normal trading hours. The figure reveals that approximately 89%, 78%, and 43% of total *UR*, *UE*, and *UCF* are priced by the end of the 1st trading interval and approximately 94%, 87%, and 68% are priced by the end of the 4th trading interval (30 minutes after market open). The results of the response coefficient regressions used to construct the curves are in Table 3.

Optically, the difference in curves suggests that *UE*, *UR*, and *UCF* are not reflected in price at the same rate. *UR* is priced very quickly, consistent with investors rapidly processing and trading on this signal. Investors also price *UE* quickly, though not at the same rate as *UR*. An implication of this finding is that, even though the expected benefits to trading on *UE* are higher on average than for *UR* (see below and Section 5.2), processing costs for *UE* appear to be higher than for *UR* such that *UR* has higher *net* benefits. In contrast, *UCF* is more gradually priced. This suggests that investors do not aggressively trade on a cash flow signal initially and instead do so more throughout the day. Overall, the graph in Panel A provides initial evidence of different pricing speed across quantitative signals bundled at EAs.

The importance of scaling the cumulative response coefficient (CRC) by the end of day total becomes apparent in Panel B, which presents the *unscaled* CRCs by trading interval. There are significant differences across the curves—the end of day CRCs are approximately 0.92, 0.60, and 0.33 for earnings, revenue, and OCF, respectively.[20] These differences result not only from different weights investors attach to each signal, but also from econometric issues related to uncorrelated measurement error that bias the CRCs towards zero. On the former point, investors will react more strongly to a signal if it has greater information content (i.e., conveys more new information about firm value not currently reflected in price). On the latter point, this classic errors-in-variables problem likely varies across signals and therefore complicates across-curve comparisons of unscaled coefficients. As discussed in Section 3.2.1, scaling by end of period totals allows for differences in strength that otherwise confound the speed comparisons. Not scaling would bias the AUCs towards signals with higher strength or lower measurement error and thus would not necessarily capture pricing speed as a construct.

[20] These values can be interpreted as the abnormal returns between the lowest and highest decile for each signal. For instance, moving from the lowest *UE* decile to the highest *UE* decile produces abnormal returns of 9.2% (0.92*10), which is highly comparable to prior literature. Similar interpretation exists for *UR* and *UCF*.

A concern with the analysis is that the observed differences in speed at an intraday level are small and lack economic importance. The findings imply that, after the opening interval, the abnormal returns between top and bottom deciles are 2.0%, 0.7%, and 1.9% for *UE*, *UR*, and *UCF*, respectively.[21] Given the sheer amount of dollar trading volume occurring throughout day 0, the magnitude of these intraday drifts suggests sizable processing frictions and pricing delays over a short horizon. Small amounts individually can be significant in the aggregate.

4.2 Main Results – Formal Statistical Tests

Table 4 presents the main metrics (AUC and ΔAUC) used to estimate the speed with which each signal enters price. Panel A shows that the realized AUC ($\widehat{AUC}$) is 0.912, 0.950, and 0.848 for earnings, revenue, and OCF, respectively. This panel also presents the realized pairwise differences, which are -0.038 for $\Delta\widehat{AUC}_{ER}$, 0.064 for $\Delta\widehat{AUC}_{ECF}$, and 0.102 for $\Delta\widehat{AUC}_{RCF}$. The simulated distributions are in Table 4, Panel B and are presented as histograms in Figure 4. Both the $\widehat{AUC}$ and $\widehat{\Delta AUC}$ distributions are centered on the realized values given my resampling approach (Hansen 2020). Notably, the $\widehat{\Delta AUC}$ distributions do not overlap with zero, which is what would be expected if two signals were priced at the same speed.

Panel C of Table 4 presents formal statistical tests using bootstrapped percentiles and p-values of the test statistic with values in absolute terms. For earnings versus revenue, the realized $|\widehat{\Delta AUC}|$ is 0.038, the 97.5th bootstrap percentile is 0.012, and the corresponding bootstrap p-value is 0.000. The respective realized values, bootstrap percentiles, and p-values are 0.064, 0.019, 0.000 for earnings versus OCF and 0.102, 0.021, and 0.000 for revenue versus OCF. Because the realized values exceed the bootstrap percentiles in each comparison, I reject equality of AUCs

[21] These amounts reflect the CRCs from intervals 2 to 40, or from 30 seconds after market open to market close on the first EA trading day.

across signals. I view this as formal evidence of prices most quickly incorporating news about revenue, then earnings, and finally OCF on an intraday level.

A variation of this test by trading interval is in Panel D. In this panel, I calculate the cumulative difference in the total amount of news priced for each signal and each interval (the CSRC). I then similarly construct bootstrap percentiles and p-values based on the simulated distributions. The results indicate that the differences across curves are statistically different throughout most of the trading day and begin to become insignificant only later in the day.

4.3. Adjacent Trading Days and Time Horizons

The preceding analysis focuses on day 0 of the EA as this day has substantially more trading activity and price adjustment than other days (Beaver et al. 2020). I next extend the main results to include trading days adjacent to day 0 as well as longer horizons using daily data. These analyses serve two important purposes. First, they reveal whether prices continue to incorporate information in each of the signals after day 0, and if so, whether speed continues to differ. This is crucial as my measures of signal-specific pricing speed (and other speed-based measures) require an arbitrarily defined ending point. Second, they indicate whether there is any systematic difference in speed before the signals are publicly disclosed.

Panel A of Figure 5 plots the CSRCs for days [0, 3] relative to the EA. I find evidence of delayed price responses for each signal, which suggests investors are still partially processing and trading on signals after the first day. Approximately 8%, 12%, and 29% of total *UR*, *UE*, and *UCF* are priced in the three trading days after day 0. This is also presented in Panel A of Table 5, which shows the percent of the total priced each day. Although there is drift for each signal and each day, the drift decays relatively quickly and is similar to the magnitude of drift within day 0. Importantly, inferences about the order of pricing speed remain unchanged.

In Panel B of Table 5 I use daily data to examine whether there is a delayed price response over longer horizons, such as those commonly used in the PEAD literature. I calculate cumulative abnormal returns over various windows (through trading day 60) and regress this on each of the signal surprises. I find no statistically significant evidence of drift after day 3 for *UE*, after day 2 for *UR*, and after day 5 for *UCF*. While these results contrast with a large literature on PEAD, they are consistent with evidence of limited longer horizon price drifts in recent years, especially among larger firms (Martineau 2021) which tend to dominate my sample.

Back to intraday data, in Figure 5, Panel B I plot the CSRCs over days [-1, 1] relative to the EA. This analysis offers a useful falsification—there should be no differences in the curves *before* the EA if investors do not differentially trade on signals that are not yet publicly available or if it is too costly to privately process the signals in advance. This is exactly what is observed. There are no differences across the curves on day -1 even though some of the information is reflected in price before the EA. Panel C plots day -1 only to zoom in on the curves to further shed light on the lack of differences across signals.

Finally, I examine the 10^{th} trading day before the EA (day -10). I use this day as a placebo test to further alleviate concerns that the results are an artifact of the data or methodology and to illustrate issues with the AUC approach when signals do not significantly explain returns. Figure 6, Panel A depicts the CSRCs. As expected, there are no differences in speed across signals and the distribution of the curves is very wide. The former suggests that the results immediately around the EA are not present on other trading days. The latter is due to small cumulative response coefficients (CRC) for each signal, which are presented in Panel B (*unscaled* CRCs). The ending CRCs on day -10 are around 0.003, which are less than 1% of the corresponding values on day 0. Because the final CRC is the denominator of the CSRC and

AUC, extremely small CRCs result in unstable and uninterpretable AUC estimates.[22] This is a limitation of the AUC methodology and underscores the importance of analyzing signals that significantly move price over an appropriate window.

5. Additional Analyses

Implicit in the main analyses is the idea that each signal has a different benefit and a different processing cost, and it is net benefits that drive differences in investors' processing decisions and pricing speed for individual signals in a bundle. In this section I provide evidence of differences in informational characteristics that likely relate to each signal's benefit and processing cost as a validation test. I then return to the pricing speed tests to provide evidence of cross-sectional differences in speed related to variation in net benefits or investor sophistication. Lastly, I discuss and evaluate an alternative interpretation to net benefits as the primary economic factor in explaining differences in speed.

5.1 Validating Signal Processing Costs

I validate differences in processing costs using quarterly autoregressive (AR) properties, as reported in Table 6. I use AR properties because investors often use realized values to forecast future values and it is plausibly easier to process a less aggregated and more persistent signal (Bradshaw et al. 2016; Cheng et al. 2020). For each signal, I obtain reported values for the current quarter and the same quarter one-year prior from the IBES actuals file, scale these values by lagged total assets, and perform AR regressions. I use the same fiscal quarter one-year prior as a lagged value due to seasonality in quarterly data. I find that revenue has the highest AR parameter (0.810) and adjusted R^2 (74.1%) of the three signals, followed by earnings (0.619 and

[22] CSRCs typically range from 0 to 1 for relatively smooth price adjustment processes that do not exhibit significant overreaction and reversal. However, as seen in Figure 5, Panel D, the simulated CSRCs routinely exceed this range.

54.8%), and finally by OCF (0.515 and 36.0%), and the differences in R^2s are statistically significant using a bootstrap t-test. These differences likely arise because earnings and OCF aggregate heterogeneous items (Fairfield et al. 1996; Holzman et al. 2021) and because OCF suffers more from timing mismatches (Dechow 1994). In turn, the higher AR properties for revenue suggests it is less costly to process compared to earnings and OCF, and the higher AR properties for earnings suggest it is less costly to process compared to OCF.

5.2 Validating Signal Benefits

The results in Figure 3, Panel B point to different benefits across signals based on the end of day CRCs. I further validate differences in benefits by examining each signal's explanatory power for stock returns, as signals with greater explanatory power are expected to have a higher benefit. Specifically, I analyze adjusted R^2s from regressions of announcement day returns on each of the signal surprises.[23] From the regression results in Table 3, I find that earnings explains the most variation in stock returns (Adjusted R^2 = 13.7%), then revenue (Adjusted R^2 = 7.5%), and then OCF (Adjusted R^2 = 4.2%). The differences in adjusted R^2s are statistically significant using a bootstrap t-test. These results are consistent with the predicted differences in benefits—earnings has the highest benefit, followed by revenue, and finally by OCF—and suggest that the differences are large.

5.3 Cross-Sectional Variation in Pricing Speed

I use three sources of cross-sectional variation to analyze whether pricing speed varies with net benefits or the ability of market participants to process each signal. I first examine loss vs. profit firm-quarters. Relative to firms reporting a profit, firm reporting a loss tend to have less

[23] I use announcement day returns rather than longer-window returns as this window aligns most closely to the benefits investors could expect from processing a signal in a short time horizon. Differences in explanatory power over longer horizons (e.g., a full fiscal quarter) are similar.

persistent and less informative earnings (Hayn 1995). This reduces the benefit and increases the cost to processing earnings, which should result in slower speed. Consistent with this notion, in Figure 7, Panel A I find that earnings is priced more slowly for firms reporting losses in the current quarter (24% of observations, based on GAAP EPS), a difference significant at the 5% level. I also find that revenue is priced more slowly and OCF more quickly for loss firms, though the differences are not statistically significant. Interestingly, although revenue is a more informative signal in the presence of losses (and has a higher benefit), processing costs appear to be high enough to offset the benefit such that there is no significant change in speed.

Second, I examine whether pricing speed for revenue varies with sales growth. Firms with low or negative sales growth typically have less persistent revenues and revenues that explain less variation in firm value (Ertimur et al. 2003), each of which predict slower speed. I partition the sample into the lower one and upper three quartiles of sales growth using the percentage change in revenue in the current quarter compared to the same quarter one year prior. Average sales growth is 3.8% for the upper group and -3.1% for the lower group. The results in Figure 7, Panel B reveal that revenue is priced more slowly for the low sales growth group, a difference significant at the 1% level. Meanwhile, speed differences are not significant for earnings or OCF, which suggests that the partition primarily captures heterogeneity in net benefits for revenue but not for earnings or OCF.

Third and finally, I examine whether each signal is priced more quickly when the average investor is more likely to be sophisticated at processing fundamental information (e.g., Bartov et al. 2000). I do so by separately forming quartiles of institutional ownership and analyst following and split the sample in two. The low sophistication group contains observations in the lowest quartile of institutional ownership or analyst following (42% of observations) and the high sophistication group contains the remaining observations (58%). In Figure 7, Panel C, I find that earnings, revenue, and OCF are all priced more quickly for the high sophistication group, though the differences are statistically significant only for earnings and OCF. Observing statistically significant differences for earnings and OCF but not revenue suggests that sophisticated investors primarily improve price discovery for more costly signals.

5.4 Alternative Interpretation – Costless Processing Models

A unique feature of bundled signals is that many economic forces that impact pricing speed largely exist at the firm- or disclosure-level (e.g., liquidity) and thus are unlikely to vary across signals within a bundle. As a result, I argue that differences in pricing speed across bundled signals are driven by an assessment of the net benefits to processing each signal. In motivating and developing my predictions, I leverage the structure from noisy rational expectations pricing models with costly information (e.g., Grossman and Stiglitz 1980). I use this theory and class of models as it offers a useful rational benchmark when investors face imperfect information or other frictions in their decision making (Veldkamp 2011).

I acknowledge that behavioral theory can predict delayed price responses to public signals in the absence of costly information. Most notable are differences-of-opinion models in which investors imperfectly (but *freely*) interpret a signal and "agree to disagree" about its interpretation (e.g., Harris and Raviv 1993; Kandel and Pearson 1995). In these models, a higher level of disagreement among investors leads to a slower pricing speed (Banerjee et al. 2020). If investors agree more about the interpretation of revenue relative to earnings and OCF, and agree more about earnings relative to OCF, then a differences-of-opinion model would generate the same speed prediction as a costly processing model. Thus, behavioral theory offers an alternative interpretation to net benefits as the primary driver of differences in speed.

Distinguishing among alternative theories is often challenging (Brav and Heaton 2002). Nevertheless, a defining feature of differences-of-opinion models is that investors overweight their own interpretation of a public signal and underweight or ignore others' interpretation, and do not fully condition on prices. Investors thus underweight price signals, which leads to return drifts in the same direction of past returns (positive return drift). In contrast, in noisy rational expectations models investors correctly condition on prices such that there is either no return drift or negative return drift based on past return signals. Even though both models predict that prices underreact to public signals about fundamentals, they offer different predictions about the autocorrelation of returns (Banerjee et al. 2009; Banerjee et al. 2020).[24] Thus, examining the relation between post-announcement returns and announcement returns can shed light on which theory appears more consistent with the data.

I investigate this matter by including the opening trading interval's stock returns as a regressor in my main specification. I then repeat the main regression, excluding the opening interval, which effectively results in a regression of post-announcement returns on announcement returns (and the other signals). The results are reported in Table 7. The coefficient on opening interval stock returns is generally negative over the different intervals, and the cumulative effect is negative (but a very small economic magnitude) at the end of the first EA trading day. Untabulated analyses also indicate that returns in the days and weeks after the EA are not predictable based on announcement returns. These findings are more consistent with noisy

[24] Models that emphasize parameter uncertainty, such as the "unknowable relevance" model of Banerjee et al. (2020), also generate an underreaction to public signals in the absence of costly information. In these models, the value implications or relevance (i.e., valuation parameter) of a signal is unknowable in real-time at any cost; instead, value implications become known over time as investors *freely* observe more data on signals and returns. I view this form of learning as costly. If investors are uncertain about the value implications of a signal, then they must spend time, effort, and resources—integration costs—to understand how a signal maps into firm value. At any rate, models of parameter uncertainty with costless information typically predict positive drift in stock returns based on past returns and thus are similar to disagreement models in this regard.

rational expectations models with costly processing than they are with differences-of-opinions models featuring costless processing. Despite this, no single test or piece of evidence can perfectly discriminate among competing theories, and the results should be interpreted as such.

6. Concluding Remarks

I investigate whether pricing speed varies among quantitative signals in a bundle to deepen our understanding of the forces that motivate investors' processing decisions. Most disclosure events contain a bundle of individual signals, and investors must decide whether to process each signal in the bundle if processing frictions are material. I use theory to predict that signals with higher net benefits (higher benefits and/or lower processing costs) are priced more quickly than signals with lower net benefits. I test this prediction using an intraday setting and a new methodology to estimate and compare the speeds of price responses to earnings, revenue, and OCF surprises in quarterly EAs. The results show that revenue news is priced the fastest, earnings news more slowly, and OCF the slowest. Supporting tests are consistent with net benefits as the primary driver of the differences in speed. These findings provide an economic explanation for investors' differential processing of quantitative signals in a bundle and render a more complete picture of the price discovery process around disclosure events.

This paper is not without limitations, which also provide opportunities for future research. I focus on three quantitative signals (earnings, revenue, and OCF) in my tests rather than on a wider set of bundled signals for theoretical and practical reasons. Future research could examine other bundled signals with varying benefits and processing costs (e.g., qualitative information or management guidance) to expand on and disentangle the role of benefits and costs on pricing speed. Furthermore, future research can develop better designed tests or identify settings to help distinguish among competing theories and explanations.

While I use an intraday time horizon to conduct the empirical analysis, the theory and methodology can also be applied to longer time horizons, such as weeks or months. An interesting avenue for future research would be to examine whether pricing speed differs over longer horizons. For instance, does the arrival of subsequent information in the days and weeks after the EA (e.g., peer firm announcements, other corporate events) alter benefits or processing costs for some signals more than others? Are some signals that are relatively easier to process over a short horizon more difficult to process over a longer horizon, perhaps because they are less complete measures of firm performance? Questions such as these can help further refine our understanding of how investors process individual signals in a bundle and its effect of the price discovery process.

Finally, the methodology developed herein represents a step forward in separating the speed of price response from the strength of price response for bundled signals. I focus on speed because it is the construct more closely related to processing frictions and investors' processing choices. However, both speed and strength matter when evaluating the overall price responsiveness to disclosure events. Future research should be careful in analyzing the economic importance of speed when strength is very low or differs considerably across signals.

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Appendix: Description of Test Statistics and Simulation Approach

In Section 3.2.2 I provide a brief overview of my main test statistics, ΔAUC (e.g., ΔAUC_{ER}), and the simulation-based approach I use for statistical inference. As discussed there, standard errors and standard test statistics are not readily available due to the transformation of regression coefficients when creating the ΔAUCs. This is because I collapse the variation from my observed data (e.g., *ARet*, *UE*) into regression coefficients for the entire pooled sample. This transformation leaves me with no variation in the ΔAUCs other than through resampling.[25]

I deal with this issue by using a resampling approach—the residual bootstrap—detailed in econometrics textbooks such as Davidson and MacKinnon (2004; Chapter 4.6) and Hansen (2020; Chapter 10). This type of approach can be used to estimate the sampling distribution of a test statistic, which in my case is the ΔAUCs. The underlying idea is to use resampling to generate variation in the AUCs (and ΔAUCs) and to compare whether the sampling distributions of AUCs overlap with one another (or ΔAUCs are different from zero).

I provide an outline of the procedure in Figure 2. I begin by drawing random errors from the empirical distribution of the regression residuals using a semiparametric bootstrap. I add these random errors (ε*) to the fitted regression values (e.g., $\widehat{\beta_{t,UE}} \times UE_{i,q}$) to create a simulated dependent variable, $ARet*$. I then re-estimate my main regression using *ARet** as the dependent variable, which is written as:

[25] At least two other approaches potentially exist to circumvent this issue, though neither are panacea. First, I could estimate regressions by firm (or some other grouping such as industry) and calculate AUC by firm rather than using firm fixed effects. This would generate an AUC (and therefore ΔAUC) for each firm in the sample, thereby generating variation in AUC and ΔAUC without resampling. A major concern with this approach is that firm-specific AUCs are extremely noisy and yield inconsistent estimates, even with a long time-series that would further restrict the sample. Second, because AUC is effectively the sum of the ratio of regression coefficients, and because regression coefficients themselves are transformations of random variables, standard errors can be derived using a Taylor series expansion (e.g., via the delta method) in the absence of resampling. However, this approach is very difficult to implement with more than two trading intervals, and my analysis uses 40 trading intervals.

$$ARet^*_{i,q,t} = \sum_{t=1}^{40} \left(\boldsymbol{\beta}^*_{\boldsymbol{t,U(X=\{E,R,CF\})}} (\gamma_t \times U(X = \{E, R, CF\})_{i,q}) \right) + \gamma^*_t + \alpha^*_i + \varepsilon'_{i,q,t}$$

This process yields simulated regression coefficients (e.g., $\beta^*_{1,UE}$) for each iteration and allows me to construct simulated AUCs and ΔAUCs (e.g., AUC* and ΔAUC*) using the steps in Section 3.2.1. I repeat this process 1,000 times to create a sampling distribution of AUC and ΔAUC.

To distinguish the different parameters used in the statistical tests, I use the following definitions: ΔAUC refers to the difference under the null of equal pricing speed, $\widehat{\Delta AUC}$ refers to the observed or realized differences in my data, and $\widehat{\Delta AUC}^*$ refers to simulated values. Each ΔAUC should be accompanied by a subscript to denote the signals being compared, but I omit the subscripts for brevity.

The sampling distribution is centered not around zero but around the value from my observed data (i.e., $\widehat{\Delta AUC}$).[26] I construct bootstrap percentiles and bootstrap p-values to assess whether the realized value of my test statistic ($\widehat{\Delta AUC}$) is statistically different from zero. Specifically, my test statistic is $\widehat{\Delta AUC} - \Delta AUC$ where $\widehat{\Delta AUC}$ is an estimated parameter based on my data and $\Delta AUC = 0$ under the null of equal pricing speed. The bootstrap test statistic is then $\widehat{\Delta AUC}^* - \widehat{\Delta AUC}$, which I use to create bootstrap percentiles and bootstrap p-values. If $q^*_{1-\alpha}$ is the $(1 - \alpha)$th quantile of the sampling distribution, then a bootstrap test rejects equal pricing speed across signals if $|\widehat{\Delta AUC} - \Delta AUC| > q^*_{1-\alpha}$ (or $|\widehat{\Delta AUC} - 0| > q^*_{1-\alpha}$). The bootstrap p-value

[26] This occurs because the random errors I generate maintain the path-dependence of my empirical analysis. It differs from approaches that construct a sampling distribution (centered on zero) using randomized permutations and then count the number of instances that the observed test statistic exceeds the iterated test statistic (e.g., Butler et al. 2007; Bushman et al. 2010). These approaches typically reshuffle abnormal returns to different intervals in the permutation process based on the null hypothesis of equal information arrival. Unfortunately, this type of reshuffling destroys the path-dependent nature of my analysis, primarily because of the discrete jumps I observe in the opening trading intervals. Specifically, reshuffling with discrete jumps leads to sampling distributions with extremely high variances that make it near impossible to detect statistically significant effects at any reasonable level.

is $p^* = \frac{1}{B}\sum_{b=1}^{B} \mathbf{1}(|\widehat{\Delta AUC}^* - \widehat{\Delta AUC}| > |\widehat{\Delta AUC} - \Delta AUC|)$, which effectively counts the number of simulations where the differences in AUCs are equal to zero or change signs. I reject the null if the bootstrap p-value is less than α (i.e., $p^* < \alpha$).

The preceding test focuses on the summary AUC metric and therefore obscures information about whether curves statistically differ over varying trading intervals. As a result, I also use the cumulative scaled response coefficient and repeat the same steps and tests for each trading interval. This allows me to examine at which point during the trading day the various curves are different (or not different) from one another and supplements the main AUC analysis.

Figure 1: Visual Outline of Creating Signal-Specific Pricing Speed

This figure depicts the steps involved in constructing the cumulative scaled response coefficients and areas-under-the-curve (AUCs) for each signal, as discussed in Section 3.2.1.

Step 1: Obtain response coefficients by trading interval from the following regressions (by signal)

$$ARet_{i,q,t} = \sum_{t=1}^{40}\left(\boldsymbol{\beta_{t,U(X=E,R,CF)}}(\gamma_t \times U(X=E,R,CF)_{i,q})\right) + \gamma_t + \alpha_i + \varepsilon_{i,q,t}$$

Example:

Interval #	β_{UE}	β_{UR}	β_{UCF}
1	0.700	0.540	0.150
2	0.040	0.020	0.050
3	0.030	0.010	0.030
.	.	.	.
40	0.001	0.000	0.001

Step 2: Calculate cumulative unscaled and scaled response coefficients by trading interval

$$Unscaled:\ CRC_{t=\tau,U(X=E,R,CF)} = \sum_{t=1}^{\tau}\beta_{t,UX} \qquad Scaled:\ CSRC_{t=\tau,UE} = \sum_{t=1}^{\tau}\beta_{t,UX}\Bigg/\sum_{t=1}^{40}\beta_{t,UX}$$

Example - UNSCALED:

Interval #	CRC_{UE}	CRC_{UR}	CRC_{UCF}	
1	0.700	0.540	0.150	cumulative sums
2	0.740	0.560	0.200	
3	0.770	0.570	0.230	
.	.	.	.	
40	0.900	0.600	0.350	

Example - SCALED:

Interval #	$CSRC_{UE}$	$CSRC_{UR}$	$CSRC_{UCF}$	
1	0.778	0.900	0.429	sum through interval divided by total
2	0.822	0.933	0.571	
3	0.856	0.950	0.657	
.	.	.	.	
40	1.000	1.000	1.000	

Step 3: Calculate areas-under-the-curve (AUCs) using cumulative scaled response coefficients

Basic version:

$$AUC_{UX} = \frac{1}{40}\left(\left(\sum_{t=1}^{39} CRSC_{t,UX}\right) + 0.5\right)$$

Over-reaction adjusted version:

$$AUC^{a}_{UX} = \frac{1}{40}\left(\sum_{t=1}^{39}\left(1 - \frac{|CRC_{40,UX} - \sum_{t=1}^{39} CRC_{t,UX}|}{|CRC_{40,UX}|}\right) + 0.5\right)$$

Example:			
	UE	**UR**	**UCF**
AUC	**0.900**	**0.950**	**0.830**

Figure 2: Outline of Resampling Approach and Bootstrap Tests

This figure depicts the steps involved in resampling procedure (residual bootstrap) used to assess the statistical significance of pricing speed across signals. See Section 3.2.2 and the Appendix for additional discussion.

Step 1: Draw random errors from the empirical distribution of regression residuals

Run the main regression:

$$ARet_{i,q,t} = \sum_{t=1}^{40} \left(\boldsymbol{\beta}_{\boldsymbol{t},\boldsymbol{U}(\boldsymbol{X}=\boldsymbol{E},\boldsymbol{R},\boldsymbol{CF})} (\gamma_t \times U(X = E, R, CF)_{i,q}) \right) + \gamma_t + \alpha_i + \varepsilon_{i,q,t}$$

Collect the regression residuals into a variance-covariance matrix by trading interval (σ_t^2)

Draw random errors (ε *) by trading interval (t): $\varepsilon_{i,q,t}^* \sim N(0, \sigma_t^2)$

Step 2: Calculate simulated dependent variable by adding random error to fitted values

$$ARet_{i,q,t}^* = \sum_{t=1}^{40} \left(\widehat{\boldsymbol{\beta}_{\boldsymbol{t},\boldsymbol{U}(\boldsymbol{X}=\boldsymbol{E},\boldsymbol{R},\boldsymbol{CF})}} (\gamma_t \times U(X = E, R, CF)_{i,q}) \right) + \widehat{\gamma_t} + \widehat{\alpha_i} + \varepsilon_{i,q,t}^*$$

This essentially creates a new "Y" variable using the randomness provided by the regression residuals It uses the estimated parameters from the regression above and leaves the "X" values the same.

Step 3: Repeat the main regression using the simulated dependent variable

$$ARet_{i,q,t}^* = \sum_{t=1}^{40} \left(\boldsymbol{\beta}^*_{\boldsymbol{t},\boldsymbol{U}(\boldsymbol{X}=\boldsymbol{E},\boldsymbol{R},\boldsymbol{CF})} (\gamma_t \times U(X = E, R, CF)_{i,q}) \right) + \gamma_t^* + \alpha_i^* + \varepsilon_{i,q,t}'$$

This creates simulated regression coefficients for each trading interval, which are then used to create simulated AUCs and ΔAUCs, denoted as AUC* and ΔAUC* (see Figure 1 for AUC calculation).

Repeat the process B = 1,000 times to create a sampling distribution of test statistics.

$\Delta AUC = 0$	under the null of equal pricing speed
$\widehat{\Delta AUC}$	is the realized value from my observed data
$\widehat{\Delta AUC}^*$	is a simulated value for any given iteration

$\widehat{\Delta AUC} - \Delta AUC$	is a test statistic
$\widehat{\Delta AUC}^* - \widehat{\Delta AUC}$	is a bootstrapped test statistic

Step 4: Calculate bootstrap test statistic, percentiles, and p-values

Use distributions from Step 3 to calculate bootstrap test statistics, percentiles, and p-values.

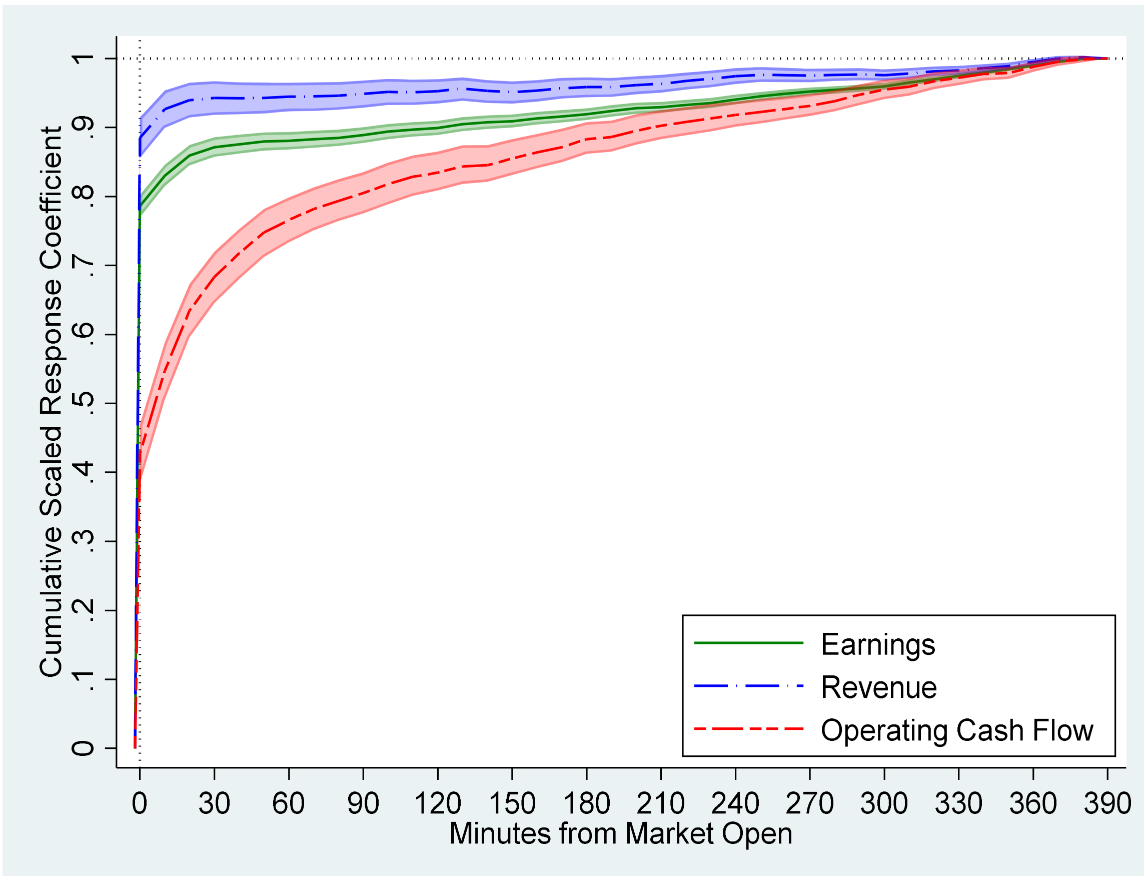
The bootstrap p-value is: $p^* = \frac{1}{B} \sum_{b=1}^{B} \mathbf{1}(|\widehat{\Delta AUC}^* - \widehat{\Delta AUC}| > |\widehat{\Delta AUC} - \Delta AUC|)$

Reject null if $p^* < \alpha$ (e.g., $\alpha = 0.05$)

Figure 3: The Speed of Price Responses to Individual Signals in Bundle

This figure plots the cumulative response coefficients over each of the 40 trading intervals (expressed in minutes from market open) on the first trading day of the EA (day 0). Each trading interval spans 10 calendar minutes during normal trading hours, except for trading intervals 1 and 2 which span the first 30 seconds and the next 9 minutes and 30 seconds, respectively. Panel A uses the cumulative scaled response coefficient, which for each interval is equal to the cumulative sum of the individual response coefficients through interval *t* divided by the cumulative sum through interval 40. Panel B uses the cumulative unscaled response coefficient, which for each interval is equal to the cumulative sum of the individual response coefficients through interval *t*. Shaded areas represent the 2.5th and 97.5th percentiles of the simulated distributions.

Panel A: Scaled Response Coefficients



Panel B: Unscaled Response Coefficients

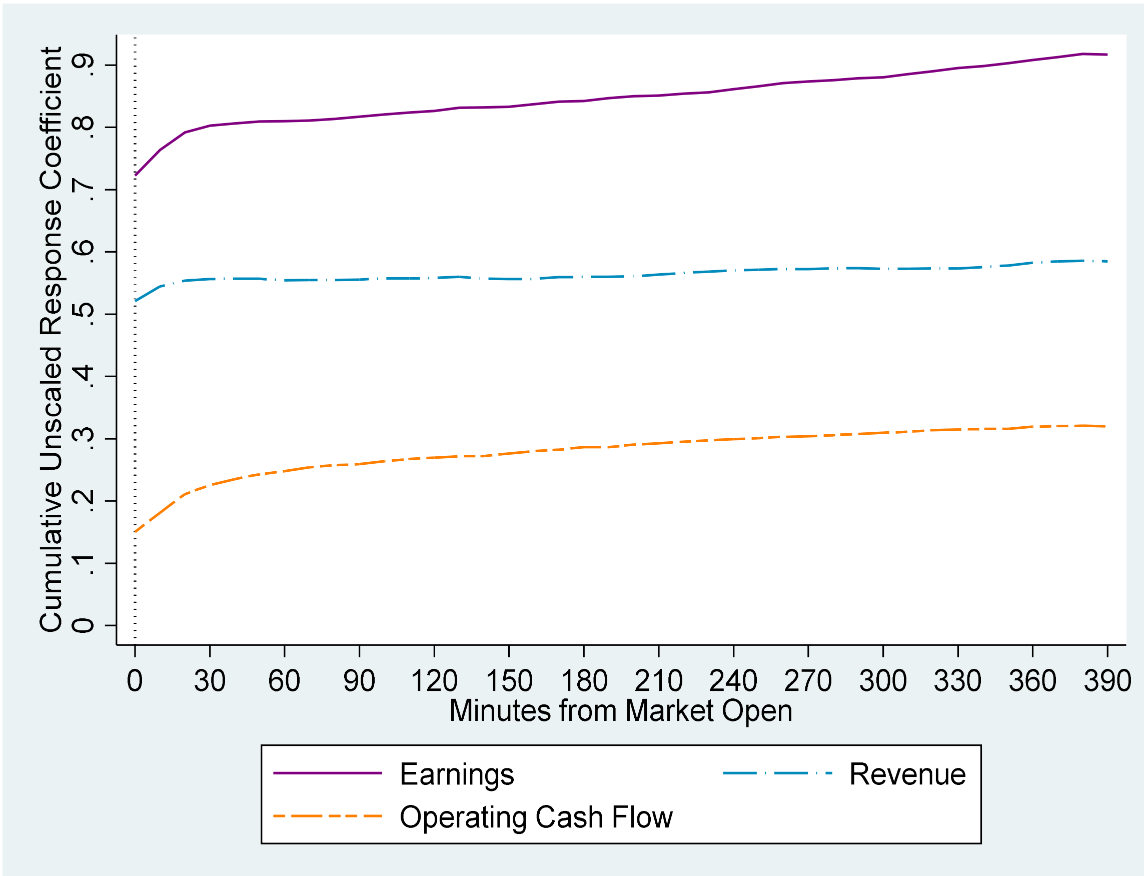
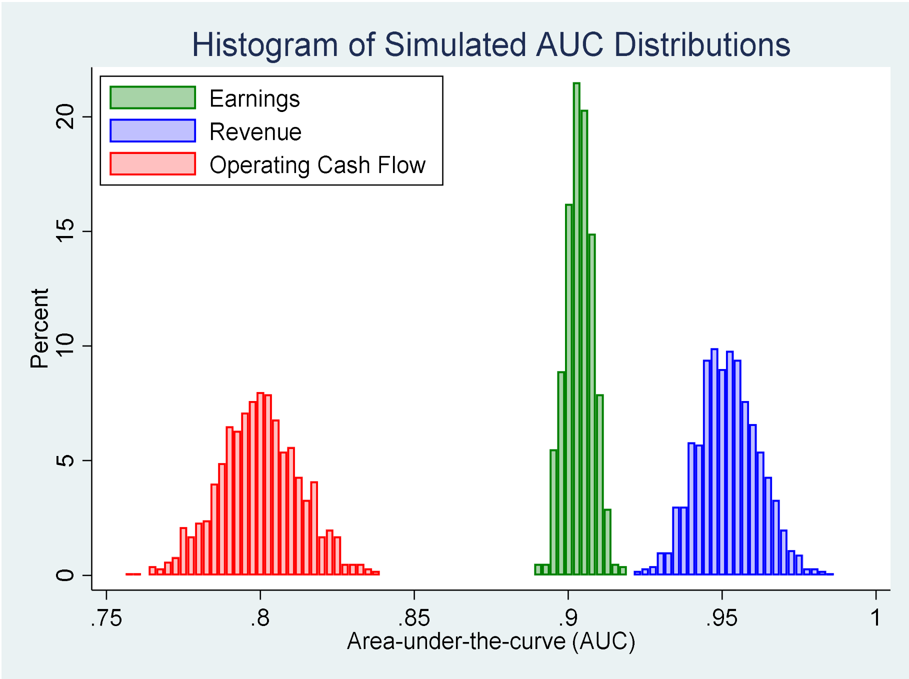


Figure 4: Simulated Area-under-the-Curve Distributions

This figure plots histograms of the simulated area-under-the-curve (AUC and ΔAUC) distributions for each signal based on the approach described in Section 3.2.2 and the Appendix. Panel A depicts the AUC distributions for each signal. Panel B depicts the ΔAUC distributions for each signal comparison (e.g., pairwise difference in earnings AUC and revenue AUC). Each histogram is approximately centered on the realized values.

Panel A: Signal-Specific AUCs



Panel B: Pairwise Difference in Signal-Specific AUCs (ΔAUC)

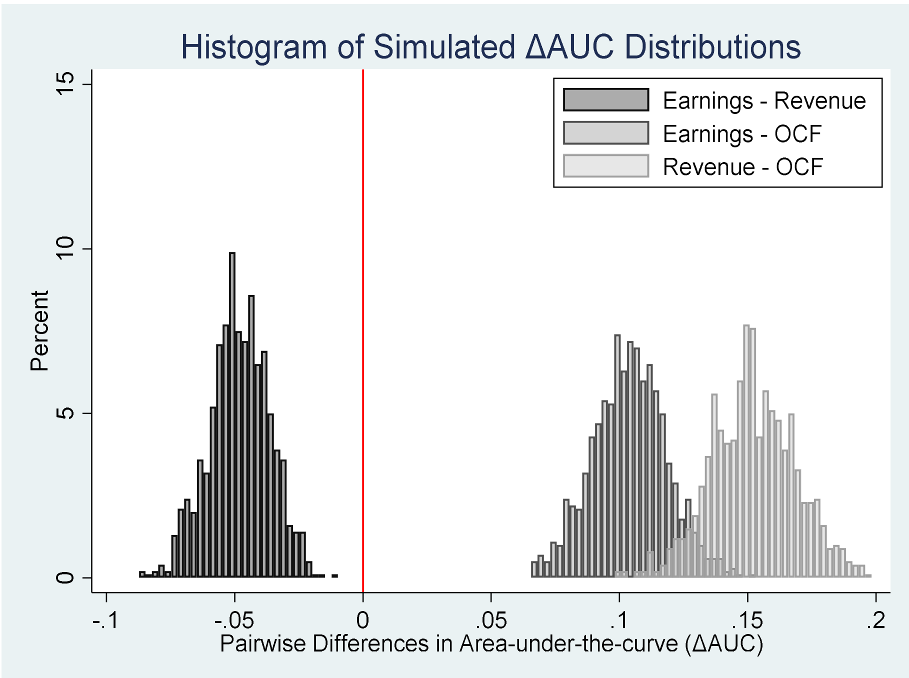
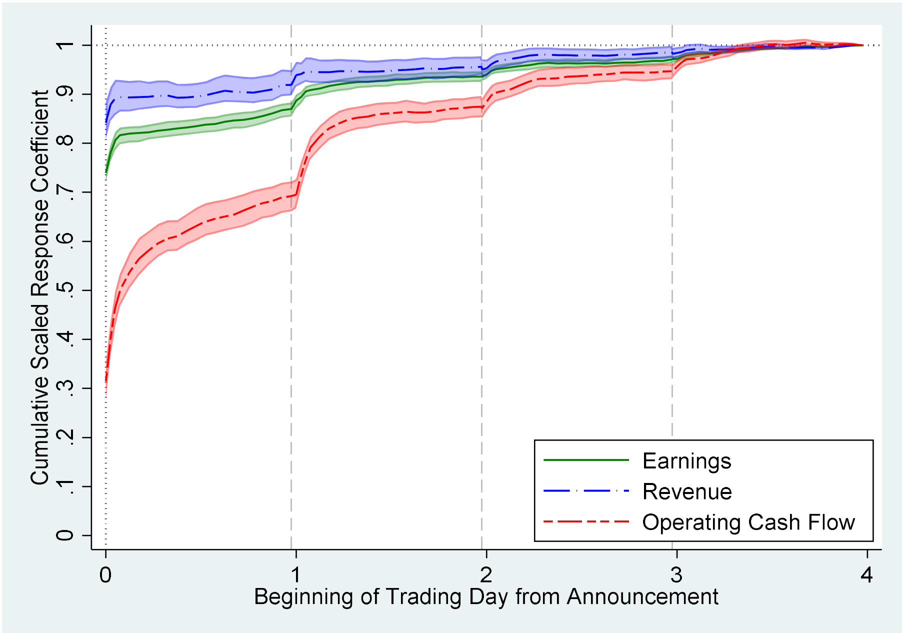


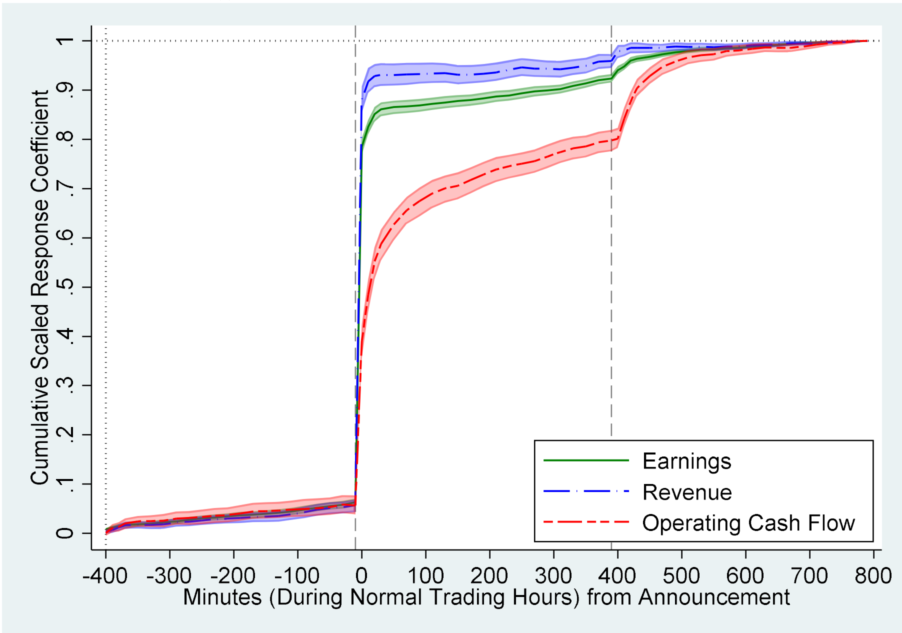
Figure 5: Pricing Speed on Adjacent Trading Days

This figure extends the window of analysis to trading days around day 0. Panel A extends the window to days [0, 3] and Panels B and C to days [-1, 1]. Each trading day uses 40 trading intervals, where intervals span 10 calendar minutes during normal trading hours, except for the first two intervals per day which span the first 30 seconds and the next 9 minutes and 30 seconds, respectively. Each panel plots the cumulative scaled response coefficient, which for each interval is equal to the cumulative sum of the individual response coefficients through interval *t* divided by the cumulative sum through the final interval. Shaded areas represent the 2.5th and 97.5th percentiles of the simulated distributions. The vertical dashed lines indicate the end of a trading day.

Panel A: Scaled Response Coefficients on Days [0, 3]



Panel B: Scaled Response Coefficients on Days [-1, 1]



Panel C: Scaled Response Coefficients Zoomed in on Day -1 of Days [-1, 1]

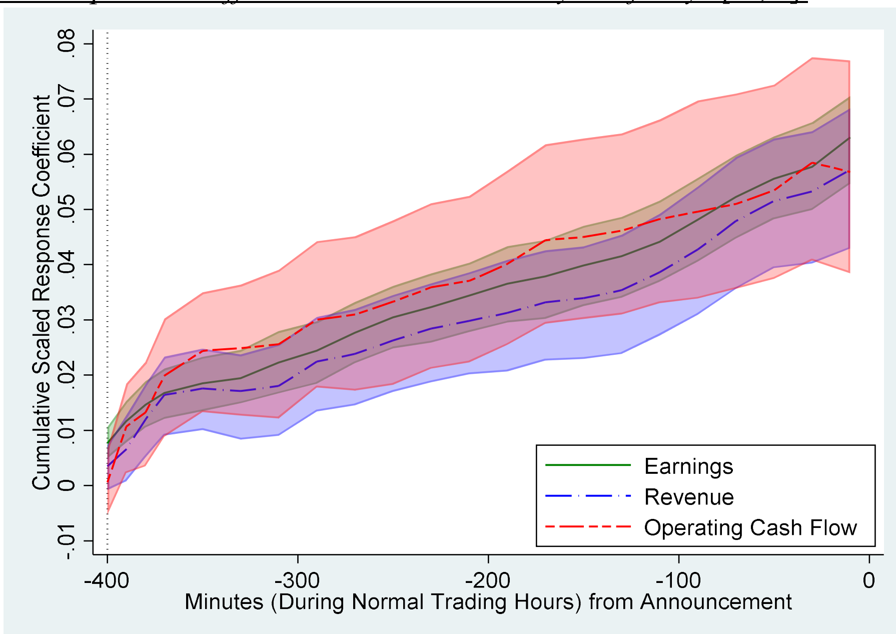
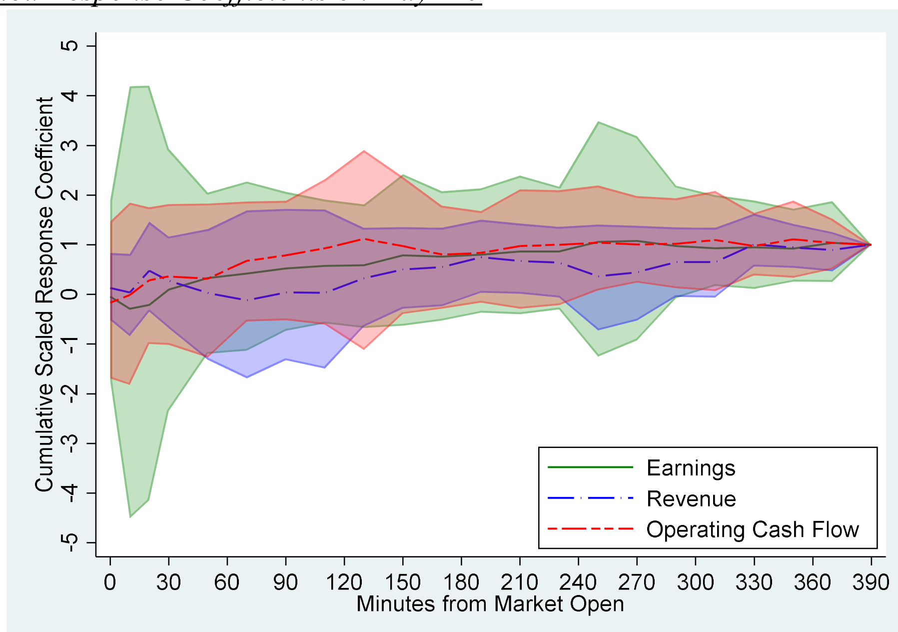


Figure 6: Pricing Speed – Placebo Test

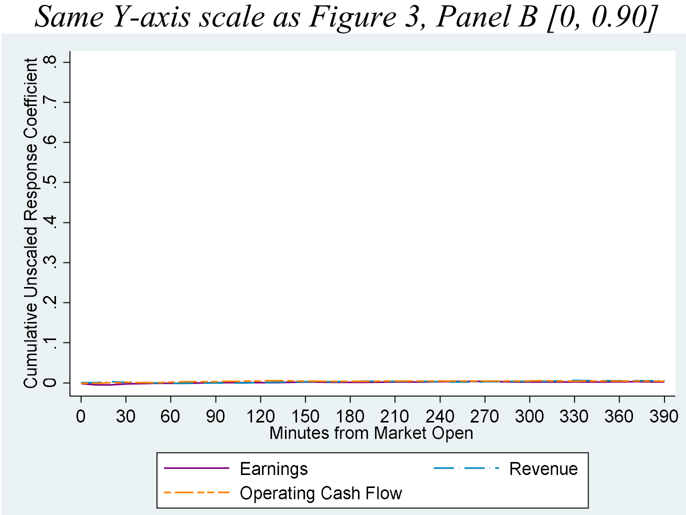
This figure focuses on the 10th trading day before the EA (day -10) as a placebo test and plots the cumulative response coefficients over each of the 40 trading intervals (expressed in minutes from market open). Each trading interval spans 10 calendar minutes during normal trading hours, except for trading intervals 1 and 2 which span the first 30 seconds and the next 9 minutes and 30 seconds, respectively. Panel A uses the cumulative scaled response coefficient, which for each interval is equal to the cumulative sum of the individual response coefficients through interval *t* divided by the cumulative sum through interval 40. Panel B uses the cumulative unscaled response coefficient, which for each interval is equal to the cumulative sum of the individual response coefficients through interval *t*. Shaded areas represent the 2.5th and 97.5th percentiles of the simulated distributions.

Panel A: Scaled Response Coefficients on Day -10



Panel B: Unscaled Response Coefficients on Day -10

Same Y-axis scale as Figure 3, Panel B [0, 0.90]



Y-axis scale of [-0.01 to 0.08] instead of [0, 0.90]

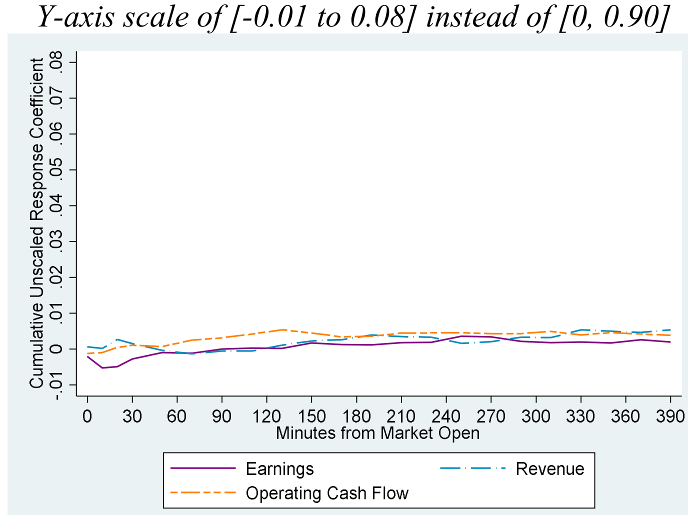
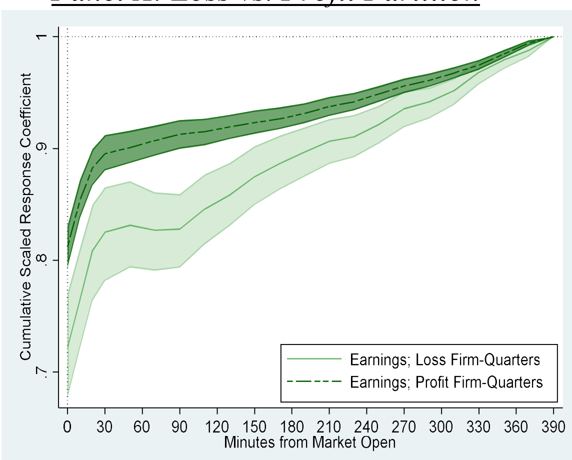


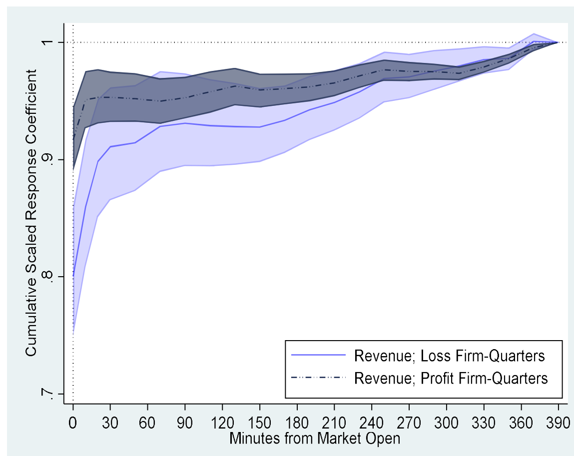
Figure 7: Pricing Speed – Cross-Sectional Variation

This figure presents three tests of signal pricing speed based on cross-sectional variation in expected net benefits or average investor sophistication. Panel A partitions the sample into loss firm-quarters (GAAP EPS less than zero) versus profit firm-quarters (GAAP EPS greater than or equal to zero). Panel B partitions the sample based on sales growth. Sales growth is calculated as current quarter revenue less revenue from the same quarter one year prior, scaled by revenue from the same quarter one year prior. Low (high) sales growth consists of observations in the lowest (upper three) quartile(s) of sales growth. Panel C partitions the sample based on investor sophistication. Low investor sophistication contains observations in either the lowest quartile of institutional ownership or analyst following while high investor sophistication contains all other observations. Each panel plots the cumulative scaled response coefficient, which for each interval is equal to the cumulative sum of the individual response coefficients through interval *t* divided by the cumulative sum through the final interval. Shaded areas depict the 5th and 95th percentiles of the simulated distributions. Differences in the cumulative response coefficient at the final interval (CRC) and areas-under-curve (AUC) across partitions are presented below each plot. Statistical differences for the AUCs are based on bootstrap p-values described in Section 3.2.2 and the Appendix. *** indicates significance at 1%; ** at 5%; and * at 10%.

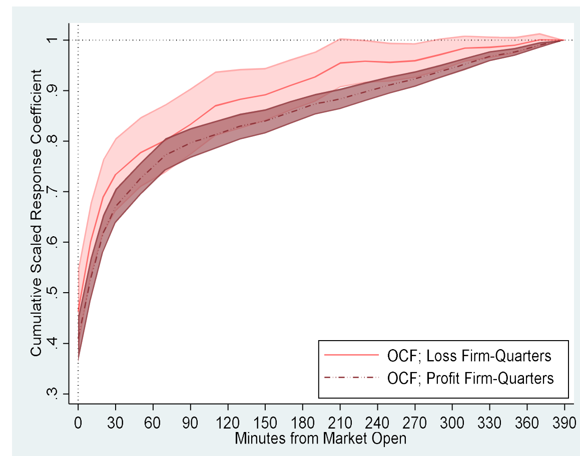
Panel A: Loss vs. Profit Partition



EARNINGS	Difference	Bootstrap p-value
CRC_Loss - CRC_Profit	-0.166	
AUC_Loss - AUC_Profit	-0.032	0.012**

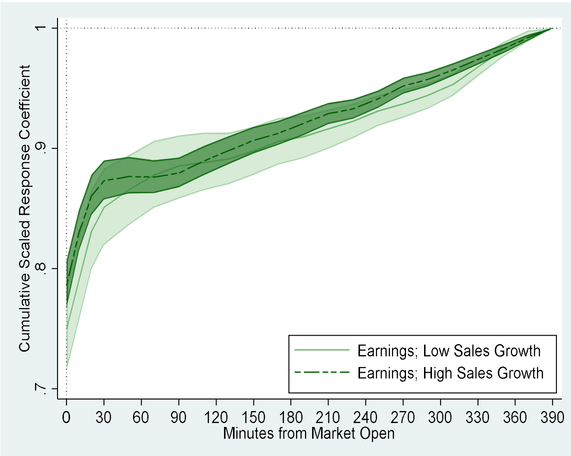


REVENUE	Difference	Bootstrap p-value
CRC_Loss - CRC_Profit	0.116	
AUC_Loss - AUC_Profit	-0.024	0.182

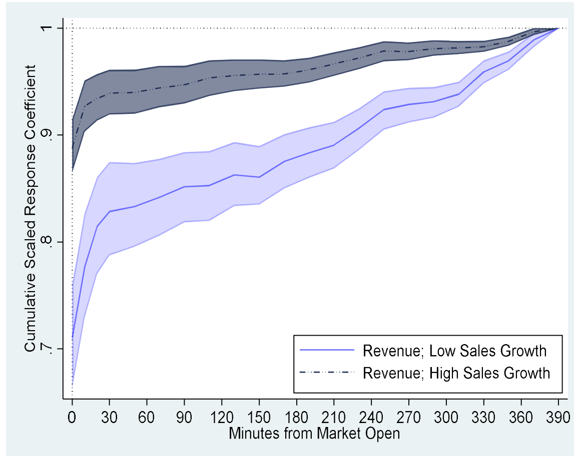


OPERATING CASH FLOW	Difference	Bootstrap p-value
CRC_Loss - CRC_Profit	0.051	
AUC_Loss - AUC_Profit	0.041	0.158

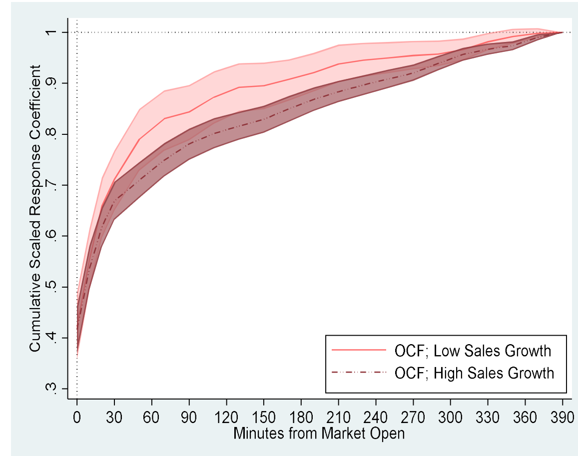
Panel B: Sales Growth Partition



EARNINGS	Difference	Bootstrap p-value
CRC_LowSG - CRC_HighSG	-0.007	
AUC_LowSG - AUC_HighSG	-0.011	0.330

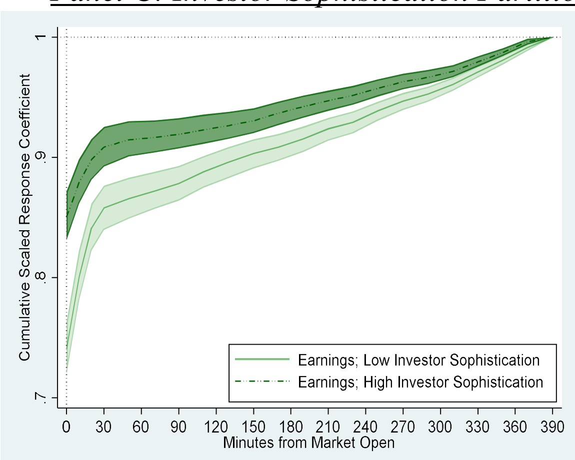


REVENUE	Difference	Bootstrap p-value
CRC_LowSG - CRC_HighSG	-0.050	
AUC_LowSG - AUC_HighSG	-0.119	0.000***

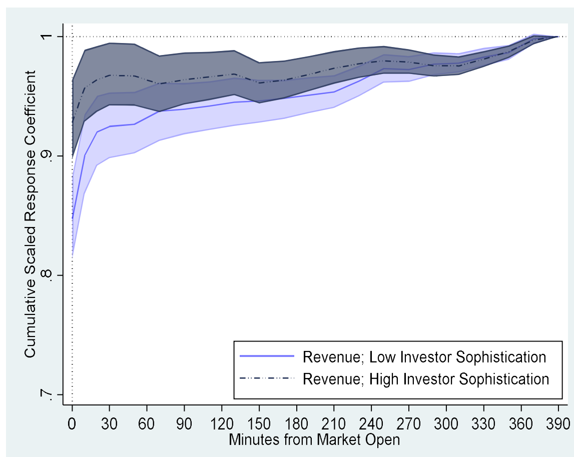


OPERATING CASH FLOW	Difference	Bootstrap p-value
CRC_LowSG - CRC_HighSG	0.019	
AUC_LowSG - AUC_HighSG	0.042	0.104

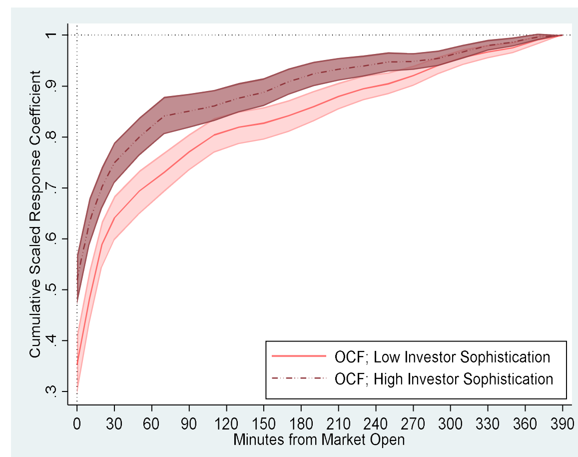
Panel C: Investor Sophistication Partition



EARNINGS	Difference	Bootstrap p-value
CRC_LowIS - CRC_HighIS	-0.068	
AUC_LowIS - AUC_HighIS	-0.032	0.000***



REVENUE	Difference	Bootstrap p-value
CRC_LowIS - CRC_HighIS	-0.013	
AUC_LowIS - AUC_HighIS	-0.020	0.108



OPERATING CASH FLOW	Difference	Bootstrap p-value
CRC_LowIS - CRC_HighIS	-0.015	
AUC_LowIS - AUC_HighIS	-0.062	0.000***

Table 1: Sample Selection

This table presents the sample selection procedure in Panel A and summarizes the availability of observations by signal and year in Panel B.

Panel A: Sample Selection Procedure

	# of Firm-Quarters
Merged Compustat/CRSP with earnings announcement date (RDQ) between January 1, 2011 and December 31, 2017 and with non-missing/non-zero total assets or share price and CRSP share codes equal to 10 or 11	102,314
Less: share price under $1 or EA date more than 90 days after fiscal quarter end	(2,602)
Less: missing EA date in both IBES and Wall Street Horizons (WSH) or EA dates in IBES and WSH both differ from Compustat	(7,912)
Less: EAs made during normal trading hours	(2,860)
Less: earnings, revenue, or operating cash flow (OCF) IBES announcement occurs on different days	(57)
Less: missing or incomplete data needed to calculate intraday abnormal returns	(8,291)
Sample available for main analysis before IBES forecast requirement	**80,592**
Less: observations without analyst forecast data for earnings, revenue, or OCF from IBES	(47,598)
Sample with analyst forecast data for all signals (MAIN SAMPLE)	**32,994**

Panel B: Distribution of Sample by Year and Availability of IBES Forecasts

Year	# Firm-quarters before IBES forecast requirement	% with IBES Earnings forecast	% with IBES Revenue forecast	% with IBES OCF forecast	# with all three IBES forecasts
2011	11,363	93.5%	90.6%	40.0%	4,485
2012	10,841	94.1%	91.4%	41.2%	4,411
2013	11,073	94.2%	91.2%	39.1%	4,288
2014	11,913	93.4%	90.5%	40.4%	4,784
2015	11,894	94.0%	92.2%	43.5%	5,128
2016	11,699	94.1%	92.3%	43.4%	5,041
2017	11,809	92.6%	91.1%	41.4%	4,857
Total	80,592				32,994

Table 2: Descriptive Statistics

This table provides descriptive statistics for the sample. Panel A presents variables that do not vary by signal. Panel B presents variables at the signal level for earnings, revenue, and operating cash flow. Market Capitalization and Share Price are calculated at the end of the fiscal quarter. # of Concurrent EAs is the number of other earnings announcements occurring on the same day. Institutional Ownership is the percentage of shares outstanding held by institutions based on Thomson Reuters 13F data. After-Hours Announcement is an indicator variable equal to one if earnings are released after market close. Loss is an indicator variable equal to one if the firm reported a loss based on GAAP EPS. Sales Growth is calculated as current quarter revenue less revenue from the same quarter one year prior, scaled by revenue from the same quarter one year prior. # of Analyst Forecasts is the number of analysts forecasting each signal. Per share values are IBES reported actuals. Surprise is the difference between IBES reported actuals and the median consensus forecast on a per share basis. Continuous variables are winsorized at the 0.5th and 99.5th percentiles.

Panel A: Descriptive Statistics at Firm-Quarter Level

Variable	N	Mean	St. Dev.	P5	Median	P95
Market Capitalization (millions)	32,994	12,788	30,541	233	3,204	55,430
Share Price	32,994	46.22	47.54	5.14	33.89	123.62
# of Concurrent EAs	32,994	214.52	132.86	23	200	435
Institutional Ownership	32,994	0.736	0.279	0.02	0.83	1.00
After-Hours Announcement	32,994	0.509	0.500	0	1	1
Loss	32,994	0.239	0.427	0	0	1
Sales Growth	32,994	0.020	0.058	-0.04	0.01	0.11

Panel B: Descriptive Statistics by Signal

Variable	N	Mean	St. Dev.	P5	Median	P95
# of Analyst Forecasts						
Earnings	32,994	13.58	7.69	4	12	28
Revenue	32,994	11.18	6.85	3	10	24
Operating Cash Flow	32,994	2.81	3.74	1	2	10
Actual Per Share Values						
Earnings	32,994	0.55	0.76	-0.36	0.42	1.84
Revenue	32,994	9.00	12.05	0.46	5.22	31.32
Operating Cash Flow	32,994	0.98	1.55	-0.65	0.67	3.69
Surprise (raw per share)						
Earnings	32,994	0.023	0.147	-0.16	0.02	0.22
Revenue	32,994	0.029	0.603	-0.69	0.01	0.76
Operating Cash Flow	32,994	-0.014	1.127	-1.59	0.01	1.50
Difference between GAAP and IBES Actuals (raw per share)						
Earnings	32,994	0.144	0.538	-0.16	0.02	0.77
Revenue	32,994	0.060	0.623	-0.15	0.00	0.43
Operating Cash Flow	32,994	-0.001	0.178	-0.04	0.00	0.02

Table 3: Response Coefficient Regressions

This table presents results of estimating the response coefficient regressions for the first trading day of the EA. The dependent variable, *ARet*, is the raw stock return less the S&P 500 value-weighted return for each trading interval using volume-weighted average prices from TAQ, multiplied by 100 to be in basis points. Each trading interval spans 10 calendar minutes during normal trading hours, except for trading intervals 1 and 2 which span the first 30 seconds and the next 9 minutes and 30 seconds, respectively. The signal surprises for earnings (*UE*), revenue (*UR*), and operating cash flow (*UCF*) are calculated as IBES reported actuals less the median analyst consensus, scaled by share price at fiscal quarter end, and are decile ranked within calendar year-quarter. Indicator variables for each of the 40 trading intervals are interacted with *UE*, *UR*, and *UCF*. Each regression contains one of the three signals and firm fixed effects. Standard errors are clustered by firm and EA date. T-statistics are in parentheses. *** indicates significance at 1%; ** at 5%; and * at 10%.

Dependent Variable =		*ARet*					
Signal Surprise =		**Earnings (*UE*)**		**Revenue (*UR*)**		**Operating Cash Flow (*UCF*)**	
Trading Interval	**Calendar End Time**	**Coef.**	**T-Stat**	**Coef.**	**T-Stat**	**Coef.**	**T-Stat**
1	9:30:30	0.724***	(45.58)	0.527***	(32.78)	0.142***	(12.64)
2	9:40:00	0.041***	(8.17)	0.025***	(5.57)	0.040***	(8.70)
3	9:50:00	0.027***	(7.25)	0.008**	(2.05)	0.029***	(8.05)
4	10:00:00	0.011***	(3.78)	0.002	(0.87)	0.016***	(6.89)
5	10:10:00	0.004**	(2.00)	-0.000	(-0.05)	0.012***	(5.94)
6	10:20:00	0.004**	(2.01)	0.000	(0.28)	0.010***	(5.93)
7	10:30:00	0.001	(0.64)	0.001	(0.59)	0.006***	(4.08)
8	10:40:00	0.002	(1.41)	0.000	(0.32)	0.005***	(4.02)
9	10:50:00	0.002	(1.62)	0.001	(0.56)	0.004***	(2.96)
10	11:00:00	0.004***	(2.75)	0.001	(1.25)	0.004***	(3.17)
11	11:10:00	0.005***	(4.15)	0.002*	(1.70)	0.004***	(4.14)
12	11:20:00	0.003**	(2.31)	-0.000	(-0.16)	0.004***	(3.22)
13	11:30:00	0.002**	(2.02)	0.001	(0.81)	0.002*	(1.81)
14	11:40:00	0.005***	(4.69)	0.002**	(2.25)	0.003***	(2.87)
15	11:50:00	0.002**	(2.20)	-0.002**	(-2.10)	0.001	(0.60)
16	12:00:00	0.002	(1.52)	-0.001	(-0.89)	0.003***	(3.45)
17	12:10:00	0.004***	(4.30)	0.001	(1.52)	0.003***	(3.50)
18	12:20:00	0.003***	(2.67)	0.002**	(2.38)	0.003***	(2.89)
19	12:30:00	0.003***	(2.88)	0.001	(1.01)	0.004***	(4.72)
20	12:40:00	0.004***	(5.04)	-0.000	(-0.10)	0.001	(1.24)
21	12:50:00	0.004***	(4.33)	0.002**	(2.17)	0.003***	(3.84)
22	13:00:00	0.001*	(1.66)	0.001	(1.43)	0.002***	(3.10)
23	13:10:00	0.003***	(3.57)	0.002***	(3.37)	0.002**	(2.53)
24	13:20:00	0.003***	(2.95)	0.002**	(2.34)	0.002**	(2.14)
25	13:30:00	0.005***	(5.92)	0.002***	(3.00)	0.002**	(2.05)
26	13:40:00	0.004***	(5.19)	0.001	(1.47)	0.001*	(1.89)
27	13:50:00	0.003***	(4.16)	-0.000	(-0.30)	0.001**	(2.03)
28	14:00:00	0.003***	(3.78)	-0.001	(-0.61)	0.002**	(2.09)
29	14:10:00	0.002**	(2.56)	0.001	(0.77)	0.002***	(3.00)
30	14:20:00	0.002***	(2.73)	0.001	(0.66)	0.003***	(4.15)
31	14:30:00	0.003***	(3.29)	-0.001	(-0.88)	0.003***	(3.47)

32	14:40:00	0.005***	(6.08)	0.002**	(2.05)	0.001*	(1.87)
33	14:50:00	0.004***	(5.22)	0.002**	(2.16)	0.003***	(3.49)
34	15:00:00	0.005***	(5.98)	0.001	(1.28)	0.002**	(2.24)
35	15:10:00	0.004***	(5.35)	0.002**	(2.40)	0.002**	(2.37)
36	15:20:00	0.005***	(6.15)	0.002***	(2.59)	0.000	(0.62)
37	15:30:00	0.005***	(5.71)	0.004***	(4.93)	0.003***	(4.00)
38	15:40:00	0.005***	(6.64)	0.003***	(3.84)	0.003***	(3.33)
39	15:50:00	0.004***	(4.64)	0.001	(1.31)	0.001	(1.63)
40	16:00:00	0.001	(0.72)	-0.001	(-1.17)	0.000	(0.42)
Cumulative Coef.		0.922		0.596		0.333	
Observations		1,319,760		1,319,760		1,319,760	
Adjusted R^2		13.7%		7.5%		4.2%	

Table 4: Statistical Tests of Differential Pricing Speed

This table presents the realized and simulated AUCs for each signal and ΔAUC for the signal comparisons. Calculation of the AUCs and ΔAUCs, discussion of the simulations, and the formal statistical tests based on bootstrapped statistics are described in Section 3.2 and the Appendix. Panel A presents the realized values based on observed data. Panel B presents the simulated empirical distributions. Panel C shows the bootstrap percentiles and p-values for the main test statistic, ΔAUC. Panel D shows the bootstrap percentiles and p-values of cumulative differences across signals by trading interval. *** indicates significance at 1%; ** at 5%; and * at 10%.

Panel A: Realized Values

Realized Areas-under-the-Curve (AUC)

Signal	$\widehat{AUC}$
Earnings (E)	0.912
Revenue (R)	0.950
Operating Cash Flow (CF)	0.848

Realized ΔAUCs

Signal Comparison	$\widehat{\Delta AUC}$
E - R	-0.038
E - CF	0.064
R - CF	0.102

Panel B: Simulated $\widehat{AUC}$ and $\widehat{\Delta AUC}$ Distributions

Simulated Distributions of $\widehat{AUC}$ and $\widehat{\Delta AUC}$ (# of Simulations = 1,000)

Signal	Mean	St. Dev.	P1	P5	P25	Median	P75	P95	P99
E	0.912	0.004	0.903	0.906	0.909	0.912	0.914	0.918	0.920
R	0.950	0.006	0.935	0.940	0.946	0.950	0.955	0.960	0.965
CF	0.848	0.010	0.828	0.833	0.842	0.848	0.854	0.865	0.872
Signal Comparison									
E - R	-0.038	0.006	-0.053	-0.049	-0.043	-0.039	-0.035	-0.029	-0.024
E - CF	0.064	0.010	0.040	0.046	0.057	0.064	0.070	0.080	0.086
R - CF	0.102	0.011	0.074	0.084	0.095	0.102	0.109	0.120	0.126

Panel C: Formal Tests over all Trading Intervals

Bootstrap Percentiles and p-values

Signal Comparison	$\lvert\widehat{\Delta AUC} - 0\rvert$	95th Percentile	97.5th Percentile	99th Percentile	p-value	
E - R	0.038	0.010	0.012	0.016	0.000	***
E - CF	0.064	0.016	0.019	0.024	0.000	***
R - CF	0.102	0.018	0.021	0.026	0.000	***

Panel D: Formal Tests by Trading Interval

Bootstrap Percentiles and p-values by Trading Interval

	E - R				E - CF				R - CF			
Trading Interval	**| Cumu. Δ |**	**97.5th Pctile**	**p-value**		**| Cumu. Δ |**	**97.5th Pctile**	**p-value**		**| Cumu. Δ |**	**97.5th Pctile**	**p-value**	
1	0.099	0.027	0.000	***	0.358	0.049	0.000	***	0.457	0.055	0.000	***
2	0.097	0.025	0.000	***	0.282	0.044	0.000	***	0.379	0.047	0.000	***
3	0.080	0.024	0.000	***	0.225	0.041	0.000	***	0.305	0.042	0.000	***
4	0.071	0.023	0.000	***	0.188	0.038	0.000	***	0.259	0.041	0.000	***
5	0.067	0.022	0.000	***	0.158	0.038	0.000	***	0.225	0.041	0.000	***
6	0.063	0.022	0.000	***	0.132	0.037	0.000	***	0.195	0.040	0.000	***
7	0.064	0.021	0.000	***	0.115	0.035	0.000	***	0.179	0.036	0.000	***
8	0.062	0.020	0.000	***	0.101	0.034	0.000	***	0.163	0.035	0.000	***
9	0.061	0.019	0.000	***	0.091	0.032	0.000	***	0.153	0.034	0.000	***
10	0.060	0.019	0.000	***	0.084	0.032	0.000	***	0.144	0.034	0.000	***
11	0.058	0.019	0.000	***	0.076	0.030	0.000	***	0.134	0.034	0.000	***
12	0.054	0.018	0.000	***	0.068	0.030	0.000	***	0.123	0.032	0.000	***
13	0.053	0.018	0.000	***	0.065	0.028	0.000	***	0.118	0.031	0.000	***
14	0.052	0.017	0.000	***	0.061	0.027	0.000	***	0.113	0.030	0.000	***
15	0.046	0.017	0.000	***	0.062	0.027	0.000	***	0.108	0.029	0.000	***
16	0.042	0.016	0.000	***	0.054	0.026	0.000	***	0.096	0.027	0.000	***
17	0.040	0.015	0.000	***	0.049	0.026	0.000	***	0.090	0.026	0.000	***
18	0.041	0.015	0.000	***	0.045	0.025	0.000	***	0.086	0.025	0.000	***
19	0.039	0.014	0.000	***	0.036	0.023	0.001	***	0.075	0.026	0.000	***
20	0.035	0.014	0.000	***	0.038	0.024	0.000	***	0.072	0.025	0.000	***
21	0.033	0.013	0.000	***	0.033	0.023	0.003	***	0.066	0.024	0.000	***
22	0.034	0.013	0.000	***	0.027	0.022	0.018	**	0.061	0.024	0.000	***
23	0.035	0.012	0.000	***	0.025	0.021	0.024	**	0.059	0.022	0.000	***
24	0.035	0.012	0.000	***	0.022	0.020	0.034	**	0.057	0.022	0.000	***
25	0.034	0.012	0.000	***	0.023	0.019	0.027	**	0.056	0.020	0.000	***
26	0.031	0.011	0.000	***	0.023	0.018	0.018	**	0.054	0.019	0.000	***
27	0.027	0.010	0.000	***	0.022	0.017	0.016	**	0.049	0.018	0.000	***
28	0.023	0.010	0.000	***	0.021	0.016	0.015	**	0.044	0.018	0.000	***
29	0.022	0.009	0.000	***	0.016	0.015	0.049	**	0.038	0.017	0.000	***
30	0.020	0.008	0.000	***	0.010	0.015	0.220		0.029	0.016	0.000	***
31	0.016	0.008	0.000	***	0.007	0.014	0.500		0.021	0.015	0.006	***
32	0.013	0.007	0.000	***	0.008	0.013	0.353		0.019	0.014	0.009	***
33	0.011	0.007	0.000	***	0.006	0.012	0.615		0.014	0.013	0.036	**
34	0.008	0.006	0.011	**	0.005	0.011	0.603		0.011	0.012	0.081	*
35	0.006	0.006	0.023	**	0.004	0.010	0.682		0.009	0.011	0.139	
36	0.004	0.005	0.104		0.006	0.009	0.210		0.010	0.010	0.054	*
37	0.005	0.004	0.014	**	0.003	0.007	0.636		0.007	0.009	0.088	*
38	0.005	0.003	0.006	***	0.003	0.006	0.965		0.005	0.007	0.198	
39	0.002	0.003	0.088	*	0.002	0.005	0.901		0.003	0.005	0.301	
40	0.000	0.000	1.000		0.000	0.000	1.000		0.000	0.000	1.000	

Table 5: Horizon Analysis – Pricing Drift and Decay

This table presents pricing speed analyses over different time horizons. Panel A uses intraday data and provides a summary of the cumulative scaled responsive coefficient for each signal at different points over days [0, 3] relative to the EA. Each day uses 40 trading intervals as defined in Section 3. The upper portion of the panel partitions the day [0, 3] window into open, midday, and close periods during normal trading hours. The lower portion of the panel presents the portion of the total priced within each day using close-to-close values. Panel B uses daily data and presents the results of regressions of cumulative abnormal returns over varying windows on earnings (*UE*), revenue (*UR*), and operating cash flow (*UCF*). Each regression contains one of the three signals and firm fixed effects. Standard errors are clustered by firm and EA date. T-statistics are in parentheses. *** indicates significance at 1%; ** at 5%; and * at 10%.

Panel A: Intraday Pricing Speed over Days [0, 3]

Percent of Total Priced over Days [0, 3]

Day	Earnings	Revenue	OCF
Day 0 open	75.3%	85.0%	34.3%
Day 0 midday	84.1%	89.9%	63.7%
Day 0 close	87.7%	92.3%	70.5%
Day 1 open	89.2%	94.3%	70.8%
Day 1 midday	93.1%	94.9%	86.5%
Day 1 close	93.9%	95.8%	87.9%
Day 2 open	94.0%	95.1%	87.6%
Day 2 midday	96.5%	98.2%	93.8%
Day 2 close	97.7%	98.7%	95.1%
Day 3 open	97.8%	98.0%	95.0%
Day 3 midday	99.3%	99.7%	99.8%
Day 3 close	100.0%	100.0%	100.0%

Day Total Changes	Earnings	Revenue	OCF
Day 0	87.7%	92.3%	70.5%
Day 1	6.3%	3.5%	17.4%
Day 2	3.8%	2.9%	7.2%
Day 3	2.3%	1.3%	4.9%

Panel B: Pricing Analyses over Longer Horizons at Daily Level

Dep. Var.: Cumulative Abn. Return	Return Window: Day 0	Day 1	Day 2	Day 3	Days [4, 5]	Days [4, 20]	Days [4, 40]	Days [4, 60]
UE	0.932***	0.061***	0.026***	0.013**	0.010	-0.010	0.033	0.009
	(41.59)	(8.24)	(4.41)	(2.43)	(1.32)	(-0.44)	(1.06)	(0.24)
UR	0.609***	0.028***	0.013**	0.007	0.004	0.002	0.011	0.029
	(26.42)	(4.24)	(2.56)	(1.57)	(0.50)	(0.12)	(0.41)	(0.82)
UCF	0.340***	0.087***	0.031***	0.013***	0.014**	0.001	0.027	-0.020
	(19.51)	(13.18)	(5.88)	(2.63)	(1.97)	(0.03)	(0.91)	(-0.52)

Table 6: Validation of Processing Costs – Signal Persistence

This table presents results of processing costs across signals using quarterly autoregressive properties. For earnings, revenue, and operating cash flow, the current quarter realized value is regressed on the lagged value from the same quarter one year prior. Realized values for each signal are obtained from the IBES actuals file, converted out of per-share values, and are scaled by lagged total assets. Comparisons of R^2 across regressions are based on t-tests of differences using bootstrapped standard errors. Continuous variables are winsorized at the 0.5^{th} and 99.5^{th} percentiles. Standard errors are clustered by firm. T-statistics are in parentheses where applicable. *** indicates significance at 1%; ** at 5%; and * at 10%.

Persistence Regressions (# Firm-Quarters = 31,276)

Variable:	Earnings	Revenue	OCF
Lagged DV (same quarter one year prior)	0.619***	0.810***	0.515***
	(47.46)	(67.36)	(40.31)
Observations	31,276	31,276	31,276
Adjusted R^2	0.548	0.741	0.360
Bootstrap R^2 Std. Error	0.020	0.016	0.017

R^2 Comparisons (T-Test):	Difference in R^2	p-value	
Earnings - Revenue	-0.193	0.000	***
Earnings - OCF	0.188	0.000	***
Revenue - OCF	0.381	0.000	***

Table 7: Opening Returns as an Additional Signal

This table presents results of estimating the response coefficient regression on day 0 to include the opening interval's abnormal return in addition to earnings (*UE*), revenue (*UR*), and operating cash flow (*UCF*) surprises. The regression is only estimated for trading intervals 2 through 40. *ARet* is the raw stock return less the S&P 500 value-weighted return for each trading interval using volume-weighted average prices from TAQ, multiplied by 100 to be in basis points. The signal surprises for *UE*, *UR*, and *UCF* are calculated as IBES reported actuals less the median analyst consensus, scaled by share price at fiscal quarter end, and are decile ranked within calendar year-quarter. Indicator variables for each of the 39 trading intervals are interacted with the opening interval *ARet* and each of the signal surprises. Standard errors are clustered by firm and EA date and the regression includes firm fixed effects. T-statistics are in parentheses. *** indicates significance at 1%; ** at 5%; and * at 10%.

Dependent Variable =			*ARet*		
Signal =			**Opening Interval *ARet***		
Trading Interval	**Coef.**	**T-Stat**	**Trading Interval**	**Coef.**	**T-Stat**
1	n/a	n/a	21	-0.001	(-1.03)
2	-0.014***	(-3.76)	22	-0.001***	(-3.02)
3	0.001	(0.32)	23	0.002***	(2.78)
4	0.001	(0.45)	24	0.000	(0.69)
5	-0.001	(-1.00)	25	-0.001	(-1.05)
6	-0.002	(-1.44)	26	0.001*	(1.94)
7	0.001	(0.69)	27	-0.000	(-0.53)
8	0.003***	(3.03)	28	0.000	(0.28)
9	-0.000	(-0.30)	29	0.002***	(2.83)
10	-0.003***	(-4.11)	30	0.001**	(2.53)
11	0.001	(1.38)	31	0.000	(0.07)
12	0.000	(0.24)	32	0.001	(1.39)
13	-0.003***	(-3.29)	33	0.001**	(2.11)
14	0.002***	(2.83)	34	-0.001	(-1.25)
15	0.000	(0.62)	35	0.001**	(2.44)
16	-0.001	(-1.51)	36	0.000	(0.51)
17	0.000	(0.39)	37	0.001**	(2.25)
18	0.000	(0.24)	38	0.000	(0.45)
19	-0.000	(-0.30)	39	-0.001***	(-2.61)
20	-0.000	(-0.13)	40	-0.004***	(-6.03)
	Cumulative Total			-0.013***	(-2.82)
Observations			1,286,766		
Adjusted R^2			2.42%		
UE, *UR*, and *UCF* Signals Included?			Yes		

<u>Supplementary Materials for Online Appendix:</u>

The speed of price responses to individual signals in a bundle

John Wertz

These Supplementary Materials contain additional discussion and analyses referenced in the main paper.

A1. IBES Forecast Requirement and Surprise Calculation

In my main sample I require all observations to have analyst forecast data for the quarter to construct *UE*, *UR*, and *UCF*. This requirement reduces the sample size by approximately 59%, primarily due to the lack of OCF forecasts by analysts. I assess the sensitivity of the results to this sample selection choice and to how I calculate signal surprises. In doing so, I use a larger sample of 80,592 firm-quarters (Table 1) that do not require an analyst forecast for each signal.

First, I re-construct *UE*, *UR*, and *UCF* by creating decile ranks based on an analyst surprise if available, and then creating decile ranks based on a seasonal random walk surprise (using Compustat data) if the analyst surprise is not available. I combine the two decile ranks to form a composite decile rank. Figure A1, Panel A plots the results using this alternative sample and calculation of *UE*, *UR*, and *UCF*. The figure reveals that the same order of pricing speed is retained—revenue is most quickly priced, then earnings, and finally OCF. The plot is similar to Figure 3, Panel A, although there is slightly more delay in the pricing of *UR* and *UE* in the first few intervals, and more precision in estimating the curves due to a larger sample.

Second, I re-construct *UE*, *UR*, and *UCF* using seasonal random walk surprises for all observations and plot the results in Figure A1, Panel B. Each of the curves has shifted upward, revenue now appears to exhibit a very slight over-reaction, and the shaded areas are much wider. I caution against interpreting the seasonal random walk results, given that the total response coefficients for earnings and revenue attenuate by approximately 50 to 65% and the adjusted R^2's decrease considerably (down to under 3%) relative to models using analyst forecast-based surprises. In other words, results using analyst forecast-based surprise measures are likely much closer to the information that investors use to re-price equities around EAs.

A2. Separate Regressions versus All-in-One Regression

The main analysis creates AUCs and other variables of interest by running separate regressions for each signal, rather than including all three signals in a single regression. I utilize separate regressions as a starting point under the assumption that investors are processing each signal independently. In reality, the signals are not independent since both revenue and OCF are components of earnings (i.e., are nested). Non-zero correlations between the signal surprises indicate that new information contained in one signal is partially included in other signals.

I address the lack of signal independence by including all three signals in the main regression model. Doing so changes the interpretation of the coefficients—each signal is now evaluated conditional on the average level of the other signals (i.e., incrementally).[27] The results are depicted in Figure A2. In comparison to Figure 3, Panel A, the curves for earnings and OCF have shifted downward, all curves have wider shaded areas, and differences between curves becomes more apparent. This is expected if, for each individual signal, partialling out the average surprises contained in the other signals leaves more of the information and processing costs unique to that signal. On balance, performing individual regressions for each signal or including all signals in a single regression does not alter the main inferences.

[27] Unfortunately, including all three signals in the same model introduces a new set of complications that does not make it an obvious candidate over the separate regression approach. The primary issue with including all signals in the same regression is a bias created by correlated measurement error in each of the regressors (e.g., Klepper and Leamer 1984). This issue is particularly acute since any measurement error in revenue or OCF will also show up in earnings, though the direction or extent of any resulting bias is unknown and hard to measure (Swaminathan and Weintrop 1991; Richardson et al. 2005).

A3. Excluding Firm Fixed Effects

The main analysis includes firm fixed effects in the response coefficient regressions to strip out static unobservable differences that may correlate with returns or signal surprises in an unknown way. Importantly, firm fixed effects alter both abnormal returns and the signal surprises by identifying deviations from each firm's own average rather than from the sample average. This fixed effect structure intends to better capture true surprises in each of the signals for a given firm and to control for relatively sticky differences across firms that may impact the price discovery process (e.g., liquidity differences due to index inclusion). Nevertheless, it is possible that including firm fixed effects affects the response coefficients and AUCs in a way that biases the results towards the observed findings.[28]

I repeat the main regressions and simulations by removing firm fixed effects and thus only using interval fixed effects. The results are presented in Figure A3. As compared to Figure 3, Panel A, each of the curves is slightly shifted upward and the earnings and revenue curves begin to overlap earlier in the trading day. The upward shift is due to proportionally smaller coefficients in the later intervals, which are potentially the result of more noise in returns or the signal surprises that bias the coefficients towards zero. As discussed in Section 6.2, this leads the AUCs to be closer to one since less of the total response occurs in later intervals. The realized $|\Delta AUC|$ for earnings and revenue is now 0.022, which is approximately 42% of what it is with firm fixed effects (0.038). However, I find this difference is still statistically significant the 5% level (bootstrap p-value = 0.024). While the methodology is somewhat sensitive to the inclusion/exclusion of firm fixed effects, the main tenor of the results remains unchanged.

[28] Firm fixed effects also create a look-ahead bias (deHaan 2021). Look-ahead bias is not necessarily a concern for my study as I am not forming a trading strategy. Moreover, the AUC methodology requires perfect-foresight returns which also induces a severe look-ahead bias.

A4. Availability of Operating Cash Flow at the Earnings Announcement

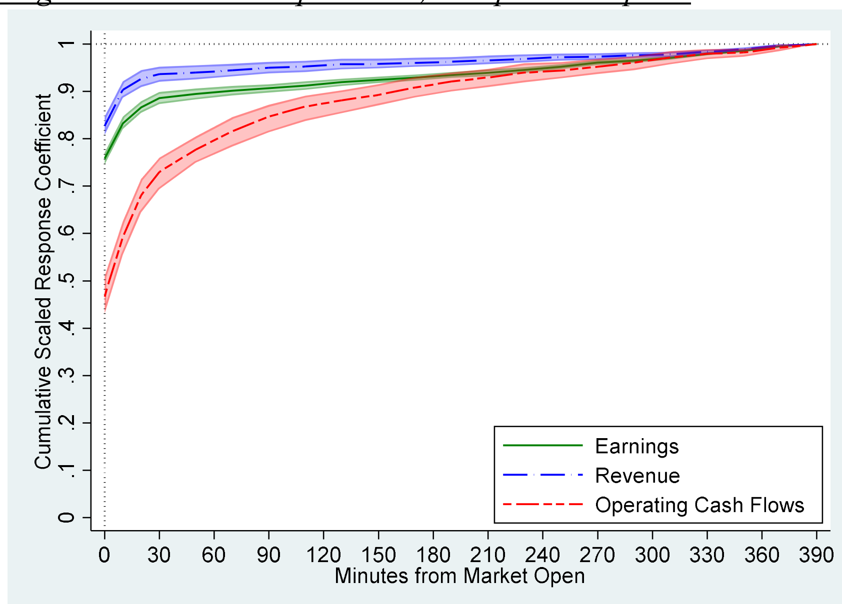
An important assumption of the bundled signal analysis is that each signal in the bundle is available to investors at the time of the EA. In particular, my prediction and analyses are predicated on earnings, revenue, and OCF each being available to investors at approximately the same time. While earnings and revenue are included in nearly all EAs, prior literature finds that not all EAs contain a statement of cash flows where OCF is typically disclosed (D'Souza et al. 2010; Miao et al. 2016). This raises concerns about the validity of the availability assumption.

I address this concern in several ways. First, my main sample requires at least one analyst forecast of OCF for each firm-quarter. These observations are much more likely to have a statement of cash flows in the EA press release relative to the average publicly traded firm. Using a Python script, I find that nearly 80% of observations in my sample contain explicit reference to OCF in the EA press release, which is significantly higher than prior literature (e.g., less than 50% in Miao et al. 2016). Second, even if OCF is not explicitly disclosed in the EA press release, it can still be estimated using the income statement and comparative balance sheets that are almost always disclosed in the press release. While a lack of explicit disclosure of OCF could increase processing costs, it nevertheless can be reasonably calculated from other information at the EA. Third, and most importantly, I use timestamps from IBES to verify that an actual OCF signal was announced on the same day as actual earnings and revenue signals. The main sample removes observations where the signals are announced on different days (see Table 1). This provides comfort that the OCF signal used in my analysis—OCF surprise based on IBES actuals and forecasts—is in fact bundled with the earnings or revenue signal. Collectively, the institutional features of EA press releases, IBES' reporting of actuals, and construction of the main sample alleviates concerns that OCF is not bundled with earnings or revenue.

Figure A1: Pricing Speed – Alternate Sample and Surprise Calculation

This figure presents additional analyses for the pricing speed tests that do not require analyst forecast data from IBES for each signal. The resulting sample is 80,592 firm-quarters as in Table 1. Panel A uses composite decile ranks for *UE*, *UR*, and *UCF*, first creating decile ranks using the median analyst forecast if available and then creating decile ranks using a seasonal random walk (based on Compustat data) if no forecast is available. Panel B uses the same sample but constructs the decile ranks using a seasonal random walk for all observations. Deciles are formed within calendar year-quarter and are centered on zero. Each panel plots the cumulative scaled response coefficient, which for each interval is equal to the cumulative sum of the individual response coefficients through interval *t* divided by the cumulative sum through the final interval. Shaded areas depict the 2.5th and 97.5th percentiles of the simulated distributions.

Panel A: Removing IBES Forecast Requirement, Composite Surprise



Panel B: Removing IBES Forecast Requirement, Seasonal Random Walk Surprise

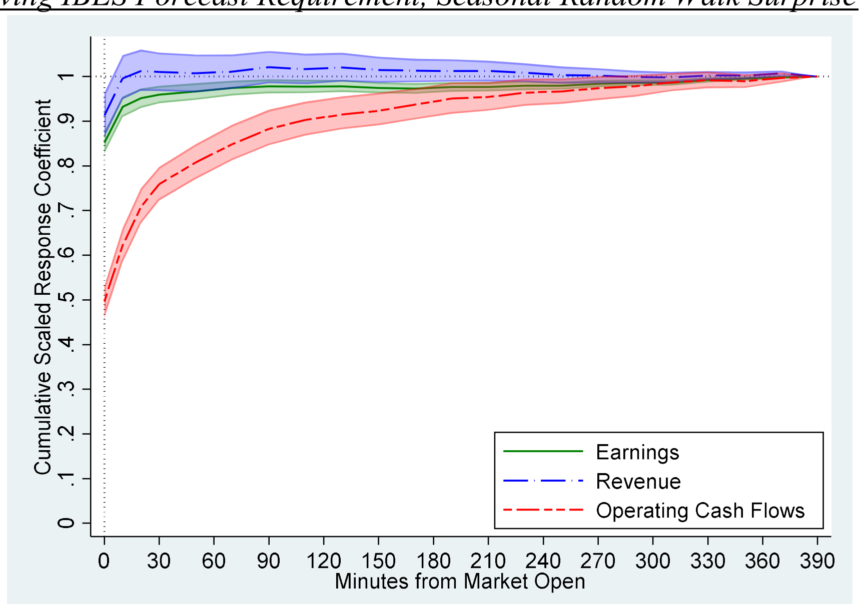


Figure A2: Pricing Speed – All-in-One Regression

This figure presents additional analyses for the pricing speed tests to include *UE*, *UR*, and *UCF* all in the same regression model rather than in separate models. The sample used consists of 32,994 firm-quarters with non-missing analyst forecast data as in Table 1. The figure plots the cumulative scaled response coefficient, which for each interval is equal to the cumulative sum of the individual response coefficients through interval *t* divided by the cumulative sum through the final interval. Shaded areas depict the 2.5th and 97.5th percentiles of the simulated distributions.

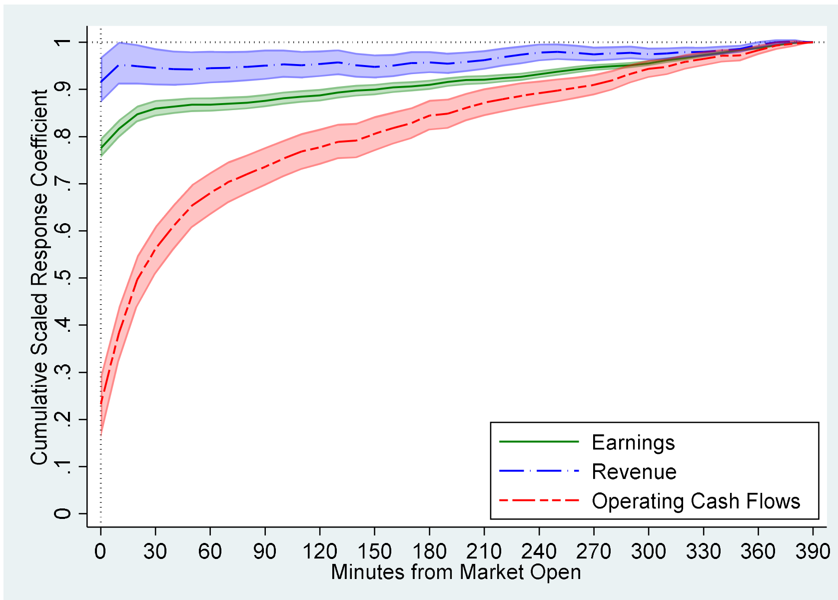


Figure A3: Pricing Speed – Excluding Firm Fixed Effects

This figure presents additional analyses for the pricing speed tests to remove firm fixed effects from the response coefficient regressions. The sample used consists of 32,994 firm-quarters with non-missing analyst forecast data as in Table 1. The figure plots the cumulative scaled response coefficient, which for each interval is equal to the cumulative sum of the individual response coefficients through interval *t* divided by the cumulative sum through the final interval. Shaded areas depict the 2.5th and 97.5th percentiles of the simulated distributions.