Price Drift before U.S. Macroeconomic News: Private Information about Public Announcements?*

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Abstract

We examine stock index futures and Treasury futures around the release time of 30 U.S. macroeconomic announcements. Nine of the 20 announcements that move markets show evidence of substantial informed trading before the official release time. Prices begin to move in the "correct" direction about 30 minutes before the release time. The pre-announcement price drift accounts on average for about 40% of the total price adjustment. This implies that some traders have private information about macroeconomic fundamentals. Pre-announcement drift might originate from a combination of information leakage and superior forecasting that incorporates proprietary data.

Keywords: Macroeconomic news announcements; financial markets; pre-announcement effect; drift; informed trading
JEL classification: E44; G14; G15

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1 Introduction

Macroeconomic news announcements move financial markets as noted by, for example, Andersen, Bollerslev, Diebold, and Vega (2007). They are quintessential updates to public information on the economy and provide fundamental inputs to asset pricing. More than one half of the cumulative annual equity risk premium is earned on announcement days (Savor & Wilson, 2013), and the information is almost instantaneously reflected in prices once released (Hu, Pan, & Wang, 2017). To ensure fairness, no market participant should have access to this information until the official release time. Yet, in this paper we find strong evidence of informed trading before several key macroeconomic news announcements.

We use second-by-second E-mini S&P 500 stock index and 10-year Treasury note futures data from January 2008 to March 2014 to analyze the impact of 30 U.S. macroeconomic announcements that previous studies and financial press consider most important. Nine out of the 20 announcements that move markets exhibit some pre-announcement price drift in the "correct" direction, i.e., in the direction of the price change predicted by the announcement surprise. Four of these announcements exhibit drift in the stock market, and all nine announcements exhibit drift in the bond market. The pre-announcement drift begins about 30 minutes before the official release time and accounts on average for about 40% of the total price adjustment.

Previous studies on macroeconomic announcements can be categorized into two groups with regard to pre-announcement effects. The first group does not separate the pre- and post-announcement effects. For example, a seminal study by Balduzzi, Elton, and Green (2001) analyzes the impact of 17 U.S. macroeconomic announcements on the U.S. Treasury bond market from 1991 to 1995. Using a time window from five minutes before to 30 minutes after the official release time t, they show that prices react to macroeconomic news. In this approach, it is unclear how much of this reaction occurs before the announcement release. The second group does separate the pre- and post-announcement effects but concludes that the pre-announcement effect is small or non-existent.

Our approach differs from previous research along four dimensions. First, some previous studies measure the pre-announcement effect in small increments of time. Ederington and Lee (1995), for example, use returns during 10-second intervals. For these short intervals, they find that prices did not change significantly in the two minutes before an announcement release in the Treasury, Eurodollar and DEM/USD futures markets around the year 1990. However, if the pre-announcement drift is gradual (which is the case in our data), it will not be detected in such small increments.

Second, we use a longer pre-announcement interval than other studies. Andersen et al. (2007), for example, include ten minutes before the release time. For the sample period from 1998 to 2002, they find that global stock, bond and foreign exchange markets react to announcements only after their release time. We show that a pre-announcement interval of at least 30 minutes is necessary to capture the price drift.

Third, we include a larger set of announcements. Instead of hand-picking announcements, we start with essentially all macroeconomic announcements that academic research and/or financial press consider relevant. We expand the largest set of announcements among previous seminal studies (Andersen, Bollerslev, Diebold, & Vega, 2003; Andersen et al., 2007) by seven additional announcements that are frequently discussed in the financial press. Although the resulting set of 30 announcements is not a full set of U.S. macroeconomic announcements, it does allow us to see the impact of macroeconomic announcements more comprehensively. In our sample, three of the additional seven announcements exhibit drift.

Fourth, we study a recent sample period. Announcement release procedures change over time, and information collection and computing power increase, which might enable sophisticated market participants to forecast some announcements. The main analysis in our paper is based on second-by-second data starting in January 2008. To compare our results to those in previous studies that use earlier sample periods, we analyze minute-by-minute data extended back to August 2003. The results suggest that the pre-announcement effect was indeed weaker in earlier periods.

Two notable exceptions among the previous studies discuss pre-announcement price dynamics. Hautsch, Hess, and Veredas (2011) examine the effect of two U.S. announcements (Non-Farm Employment and Unemployment Rate) on German Bund futures during each minute in the $[t - 80min, t + 80min]$ window from 1995 to 2005. They find that the return during the last minute before the announcement release is correlated with the announcement surprise. Bernile, Hu, and Tang (2016) use transaction-level data to look for evidence of informed trading in stock index futures and exchange traded funds before the Federal Open Market Committee (FOMC) and three macroeconomic announcements between 1997 and 2013. They find abnormal returns and order imbalances (measured as the difference between buyer- and seller-initiated trading volumes divided by the total trading volume) in the "correct" direction before the FOMC meetings but not before the macroeconomic announcements (Non-Farm Employment, Consumer Price Index and Gross Domestic Product). Bernile et al. (2016) suggest these findings are consistent with information leakage.[1]

Our study differs from Hautsch et al. (2011) and Bernile et al. (2016) in two aspects. First, our methodology and expanded set of macroeconomic announcements allow us to show that pre-announcement informed trading is limited neither to the FOMC announcements nor to the last minute before the official release time. Second, we explore the information leakage explanation[2] in more detail by examining two aspects of the announcement release process – organization type and release procedures – and also consider other possible sources of informed trading around public announcements.

[1]Beyond these studies that investigate responses to announcements *conditional* on the surprise, Lucca and Moench (2015) report *unconditional* excess returns in equity index futures during 24 hours prior to the FOMC announcements. They do not find excess returns for nine U.S. macroeconomic announcements or in Treasury securities and money market futures.

[2]Macroeconomic announcement leakage has been documented in other countries. For example, Andersson, Overby, and Sebestyén (2009) analyze news wires and present evidence that the German employment report is regularly known to investors prior to its official release. Information leakage has also occurred in other settings, for example, in the London PM gold price fixing (Caminschi & Heaney, 2013). In corporate finance, some papers (for example, Sinha and Gadarowski (2010) and Agapova and Madura (2011)) regard price drift before public guidance issued by company management as de facto evidence of information leakage while others remain agnostic about the source of informed trading around company earnings announcements in trading by institutional investors (for example, Campbell, Ramadorai, and Schwartz (2009)) and individual investors (for example, Kaniel, Liu, Saar, and Titman (2012)).

With respect to organization type, we focus on the difference between organizations subject to the Principal Federal Economic Indicator (PFEI) guidelines and other entities. The U.S. macroeconomic data prepared by government agencies is generally considered closely guarded with strict measures aimed at preventing premature dissemination. However, some private data providers are not subject to the same guidelines, and some of them have been known to follow release procedures that would not be allowed for the PFEIs, such as releasing information to exclusive groups of subscribers before making it available to the public. In our analysis, announcements released by organizations that are not subject to PFEI guidelines exhibit a stronger pre-announcement drift.

With respect to release procedures, we are interested in the safeguards against premature dissemination. Surprisingly, many organizations do not have this information available on their websites. We conducted an extensive phone and email survey of the organizations in our sample. The release procedures fall into one of three categories. The first category involves posting the announcement on the organization's website at the official release time, so that all market participants can access the information at the same time. The second category involves pre-releasing the information to selected journalists in "lock-up rooms" adding a risk of leakage if the lock-up is imperfectly guarded. The third category involves the least secure pre-release procedure: Instead of being pre-released in lock-up rooms, these announcements are electronically transmitted to journalists who are asked not to share the information with others. In our analysis, pre-released announcements and, more specifically, the announcements pre-released under the least secure procedure are associated with a stronger pre-announcement drift.

While these findings are suggestive, one cannot conclude that information leakage causes observed pre-announcement drift because other possible causes of informed trading exist. In particular, we consider information generated by informed investors and impounded into prices through their trading (French & Roll, 1986). Some traders may be able to collect proprietary information or analyze public information in a superior way to forecast announcements better than other traders. This knowledge could then be utilized to trade in the "correct" direction before announcement releases. We conduct an extensive forecasting exercise with public information (individual analyst forecasts). We also show that proprietary information permits forecasting announcement surprises in some cases.

Recently, the possibility of data leaks has received a lot of public attention. For example, the Securities and Exchange Commission (SEC) charged two individuals for hacking into news wire services and selling the obtained information on upcoming corporate earnings announcements to traders, which generated over $100 million of illegal profits (SEC, 2015). In the context of macroeconomic news, further research on whether the source of informed trading is leakage, proprietary data collection, or reprocessing of public information would, therefore, be very timely.

The rest of this paper is organized as follows. The next two sections describe the data and methodology. Section 4 presents the empirical results. Explanations for the pre-announcement drift are tested in Section 5, and a brief discussion concludes in Section 6.[3]

2 Data

This section describes the announcements data and markets data.

2.1 Expected and Released Values of Macroeconomic Announcements

We start with the 23 macroeconomic announcements in Andersen et al. (2003) and Andersen et al. (2007), which is one of the largest sets of announcements among the previous seminal

[3]A separate Internet Appendix tests whether our results are robust to data snooping and to conditioning on the sign of post-announcement returns. It also presents results based on the standard event study methodology including potential impact of outliers, event window length, the effect of order flows, and other markets (E-mini Dow stock index and 30-year Treasury bond futures). All tests confirm robustness of our results.

studies.[4] We augment this set by seven announcements that have been frequently discussed in the financial press: Automatic Data Processing (ADP) Employment, Building Permits, Existing Home Sales, the Institute for Supply Management (ISM) Non-Manufacturing Index, Pending Home Sales, and the Preliminary and Final University of Michigan (UM) Consumer Sentiment Index. Expanding the set of announcements beyond the ones used in previous studies reflects the evolution of available data. The ADP Employment report constructed with actual payroll data, for example, did not exist until May 2006, but it has since then become an influential announcement. Table 1 lists these 30 macroeconomic announcements grouped by announcement category.[5]

We assume that efficient markets react only to the unexpected component of news announcements. Following Balduzzi et al. (2001), we compute this "surprise" as the difference between the actual announcement, A_{mt}, of a macroeconomic announcement m released at time t and the market's expectation of the announcement before its release, $E_{t-\tau}[A_{mt}]$, where $\tau > 0$.[6] To convert macroeconomic announcements to common units, we standardize this difference by the standard deviation of the respective announcement, $\sigma_m = \sqrt{\frac{1}{N_m - 1}\Sigma_{i=1}^{N_m}(S_{im} - \overline{S}_m)^2}$ where $\overline{S}_m$ is the mean surprise for announcement m. The

[4]Andersen et al. (2003) and Andersen et al. (2007) list 24 macroeconomic announcements. We do not report results for Capacity Utilization because it is always released simultaneously with Industrial Production and the surprise components of these two announcements are strongly correlated with a correlation coefficient of +0.8. When we account for simultaneity by using their principal component in equation (2), the results are similar to the ones reported for Industrial Production. We omit monetary announcements (Money Supplies M1, M2, M3, Target Federal Funds Rate) because these policy variables differ from macroeconomic announcements by long preparatory discussions. The National Association of Purchasing Managers index is now called ISM Manufacturing Index.

[5]In the remainder of the paper, we refer to these 30 variables as "announcements." Our observations are then "announcement releases." In this terminology, for example, the GDP Advance announcement had 25 announcement releases.

[6]In five instances, Bloomberg shows releases for two or three months released at the same time: Building Permits for September and October 2013, Construction Spending for September and October 2013, Factory Orders for August and September 2013, Housing Starts for September, October and November 2013, and New Homes Sales for September and October 2013. The delays appear to be due to the government shutdown in the fall of 2013. For these releases, we compute the surprise as the arithmetic average of surprises for the respective months.

Table 1: Overview of U.S. Macroeconomic Announcements

Category	Announcement	Frequency	N_m	Source[a]	Unit	Time	Fcts.
Income	GDP advance	Quarterly	25	BEA	%	8:30	82
	GDP preliminary	Quarterly	25	BEA	%	8:30	78
	GDP final	Quarterly	25	BEA	%	8:30	76
	Personal income	Monthly	72	BEA	%	8:30	70
Employment	ADP employment	Monthly	75	ADP	Number of jobs	8:15	34
	Initial jobless claims	Weekly	324	ETA	Number of claims	8:30	44
	Non-farm employment	Monthly	73	BLS	Number of jobs	8:30	84
Industrial Activity	Factory orders	Monthly	73	BC	%	10:00	62
	Industrial production	Monthly	75	FRB	%	9:15	78
Investment	Construction spending	Monthly	73	BC	%	10:00	48
	Durable goods orders	Monthly	72	BC	%	8:30	76
	Wholesale inventories	Monthly	75	BC	%	10:00	31
Consumption	Advance retail sales	Monthly	75	BC	%	8:30	79
	Consumer credit	Monthly	74	FRB	USD	15:00	33
	Personal consumption	Monthly	72	BEA	%	8:30	74
Housing Sector	Building permits	Monthly	74	BC	Number of permits	8:30	52
	Existing home sales	Monthly	75	NAR	Number of homes	10:00	73
	Housing starts	Monthly	72	BC	Number of homes	8:30	76
	New home sales	Monthly	73	BC	Number of homes	10:00	73
	Pending home sales	Monthly	76	NAR	%	10:00	36
Government	Government budget	Monthly	73	USDT	USD	14:00	27
Net Exports	Trade balance	Monthly	75	BEA	USD	8:30	73
Inflation	Consumer price index	Monthly	75	BLS	%	8:30	80
	Producer price index	Monthly	73	BLS	%	8:30	74
Forward-looking indices	CB Consumer confidence index	Monthly	74	CB	Index	10:00	71
	Index of leading indicators	Monthly	75	CB	%	10:00	53
	ISM Manufacturing index	Monthly	74	ISM	Index	10:00	76
	ISM Non-manufacturing index	Monthly	75	ISM	Index	10:00	71
	UM Consumer sentiment (prel.)	Monthly	75	TR/UM	Index	9:55	67
	UM Consumer sentiment (final)	Monthly	74	TR/UM	Index	9:55	61

The sample period covers January 1, 2008 to March 31, 2014. The release time is stated in Eastern Time (ET). The "Fcts." column shows the average number of professional forecasters that submitted a forecast to Bloomberg.

[a] Automatic Data Processing, Inc. (ADP), Bureau of the Census (BC), Bureau of Economic Analysis (BEA), Bureau of Labor Statistics (BLS), Conference Board (CB), Employment and Training Administration (ETA), Federal Reserve Board (FRB), Institute for Supply Management (ISM), National Association of Realtors (NAR), Thomson Reuters/University of Michigan (TR/UM), and U.S. Department of the Treasury (USDT).

standardized surprise, S_{mt}, is then

$$S_{mt} = \frac{A_{mt} - E_{t-\tau}[A_{mt}]}{\sigma_m}. \quad (1)$$

We proxy the expectation, $E_{t-\tau}[A_{mt}]$, by the median response of professional forecasters during the days before the release, $E_{t-\Delta}[A_{mt}]$, where $\Delta > 0$.[7] We assume that the expectation $E_{t-\Delta}[A_{mt}]$ about a macroeconomic announcement is exogenous, in particular not affected by asset returns during $[t-\tau, t]$.[8] We use a survey carried out by Bloomberg, which allows the professional forecasters to revise their responses until shortly before the release time. Although $\Delta \neq \tau$, the scarcity of revisions shortly before the official release times indicates that the two expectations are more or less identical.[9] Bloomberg collects the forecasts during a two-week period preceding the announcements. The first forecasts for our 30 announcements appear on Bloomberg five to 14 days before the announcement releases. Forecasts can be posted until two hours before the announcement release, i.e., $\Delta \geq 120min$. On average, the forecasts are five days old as of the release time. Bloomberg calculates the consensus forecast as the median of individual forecasts and continuously updates the consensus forecast when additional individual forecasts are posted.

2.2 Prices of Stock and Bond Futures

To investigate the effect of the announcements on stock and bond markets, we use intraday, nearby contract futures prices. Our second-by-second transaction data from Genesis Financial Technologies spans the period from January 1, 2008 until March 31, 2014. We report

[7] Survey-based forecasts have been shown to outperform forecasts using historical values of macroeconomic variable. See, for example, Pearce and Roley (1985).

[8] We test for unbiasedness of expectations. Almost all survey-based forecasts are unbiased. The mean forecast error is statistically indistinguishable from zero at the 10% significance level for all announcements except for the Index of Leading Indicators and Preliminary and Final UM Consumer Sentiment Index. These three announcements do not exhibit pre-announcement drift (see Section 4), and our conclusions are, therefore, not affected by them.

[9] For example, for one particular GDP release in 2014, only three out of 86 professional forecasters updated their forecasts during the 48 hours before the announcement release.

results for the E-mini S&P 500 stock index futures market (ticker symbol ES) and the 10-year Treasury notes futures market (ticker symbol ZN) traded on the Chicago Mercantile Exchange (CME). In the remainder of the paper, we refer to the E-mini S&P 500 stock index futures as "S&P 500" and to the 10-year Treasury notes futures as "Treasury note".

We sample trade price data every five minutes for each market. If a price is not available, the most recent price is used. Because the nearby contract becomes increasingly illiquid as its expiration date approaches, we switch to the next maturity contract when its daily trading volume exceeds the nearby contract volume.

Our identification rests on a clear assignment of prices to the pre- or post-announcement period. In the seconds just before an announcement release, this is difficult for two reasons: intentional and unintentional early releases. First, Thomson Reuters used to pre-release the University of Michigan Consumer Sentiment Index two seconds ahead of the official release time to its high-speed data feed clients (Javers, 2013b).[10] We want to capture trading following these pre-releases in the post-announcement interval, so that it does not overstate our pre-announcement price drift. Second, there have been instances of inadvertent early releases such as Thomson Reuters publishing the ISM Manufacturing Index 15 milliseconds before the scheduled release time on June 3, 2013 (Javers, 2013b). Scholtus, van Dijk, and Frijns (2014) compare the official scheduled release times to actual release times and conclude that such accidental early releases occur but are rare.[11] Therefore, using five seconds before the release time as the pre-announcement interval cutoff ensures that accidental early releases do not fall into the pre-announcement interval.

For this reason, we replace every price at the release time of an announcement with the

[10]Thomson Reuters suspended the practice following a probe by the New York Attorney General in July of 2013 (Javers, 2013a).

[11]Scholtus et al. (2014) analyze 20 U.S. macroeconomic and monetary announcements from January 2009 to December 2011. Their sample period includes 800 announcement releases. Only thirteen of these 800 releases arrived before the scheduled release time. Among these thirteen releases, three releases arrived in the second before the official release time, and only four releases arrived more than one minute before the official release time. Three of these four releases were FOMC rate announcements that are not relevant for our paper because we do not analyze monetary announcements; only one release (CB Consumer Confidence Index on June 28, 2011) falls into our sample.

price that was prevailing five seconds before the announcement release.[12] We then compute continuously compounded asset returns, R_t, for the entire sample as the first difference between adjacent log prices in this modified time grid.

Suppose that there is an isolated macroeconomic announcement release at time t. Then the return R_t covers the $[t - 5min, t - 5sec]$ time window, i.e., the price change in the five minutes before the announcement release excluding five seconds immediately before the release. The return R_{t-1} covers the $[t - 10min, t - 5min]$ interval, i.e., a period without announcement releases. The return R_{t+1} includes the announcement impact at release time, spanning the $[t - 5sec, t + 5min]$ interval.

Returns are sampled from 7:15 to 16:00 over the period from August 1, 2008 to March 31, 2014. The sampling starts one hour before the earliest announcement at 8:15 and ends one hour after the latest announcement at 15:00 per Table 1. If any sum of six subsequent five-minute returns, i.e., a 30-minute return, equals zero, then that day is excluded from the sample similarly to Andersen et al. (2007). Based on this rule, we remove 56 days that correspond to the U.S. holidays.

Finally, we place the announcement surprises of the 30 announcements listed in Table 1 on the same time grid as the returns. If there is no announcement release during a given time interval, then the surprise is zero.

3 Methodology

We follow a time-series approach which embeds all announcements in a single regression (Andersen et al., 2003).[13] Within each market, the asset return is a linear function of three components. The first component, lagged asset returns, accounts for possible autocorrelation

[12] Therefore, in no-release periods the time grid is exactly five minutes, whereas the last pre-release period is five seconds shorter, and the first post-release period is five seconds longer. Results without this timing correction, i.e., with a $[t - 30min, t]$ window, are similar, suggesting that the drift in the last five seconds before the announcement release is not substantial.

[13] The Internet Appendix presents a robustness check based on event study methodology that estimates a separate regression for each announcement.

and cross-serial correlation across the two markets. The second component, lagged surprises of each announcement, captures the impact that an announcement may have on the market in the following periods. The third and most important component, contemporaneous and lead values of each announcement surprise, captures the pre-announcement drift. We assume that the surprise process is exogenous; in particular, macroeconomic surprises are not affected by past asset returns. We analyze $J = 2$ markets, the E-mini S&P 500 futures and the 10-year Treasury note futures market. For a given market, the model becomes

$$R_t = \beta_0 + \sum_{j=1}^{J} \beta_j R_{j,t-1} + \sum_{m=1}^{M} \sum_{k=-1}^{K} \gamma_{mk} S_{m,t+k} + \epsilon_t. \quad (2)$$

We choose one lag of returns for each market based on the Bayesian information criterion. The second sum is over the $M = 30$ announcements listed in Table 1. To capture the regular post-announcement price move, we include one lag of surprise. To capture the pre-announcement drift, we use the contemporaneous surprise and $K = 5$ leads of the surprise which together span the $[t - 30min, t - 5sec]$ window.

To account for heteroskedasticity in the error term ϵ_t, we estimate equation (2) with a two-step weighted least squares procedure. In the first step, equation (2) is estimated with an ordinary least squares regression (OLS). In the second step, an estimate of time-varying volatility is derived from the residuals, e_t, (estimates of ϵ_t) of this OLS regression, which is then used in the weighted least squares estimation analogous to Andersen et al. (2003).

The weight w_t is an estimate of volatility computed by the exponential moving average $w_t = \alpha w_{t-1} + (1 - \alpha)|e_t|$ with the smoothing parameter $\alpha = 0.9$.[14] Standardization of the residuals by w_t removes almost all heteroskedasticity and performs better than other methods such as regressing the absolute value of the residuals on seasonal hourly dummies.[15] We standardize the dependent and explanatory variables by w_t and estimate the OLS regression

[14] The results are robust to other values of this smoothing parameter such as 0.8 and 0.95.

[15] In the first period, $w_1 = |e_1|$. Because the estimator is very volatile in the initial periods, we discard the first 50 observations in the sample which correspond to the morning of January 2, 2008. This leads to discarding one release of Construction Spending and ISM Manufacturing Index announcements.

with these standardized variables.

A statistical test of whether a particular announcement m in a given market exhibits pre-announcement price drift can be based on the sum of coefficients on the contemporaneous and lead surprises corresponding to the $[t - 30min, t - 5sec]$ window. Under the null hypothesis of no drift, $\gamma_m \equiv \sum_{k=0}^{K} \gamma_{mk} = 0$, and under standard assumptions, the resulting test statistic follows the Student's t-distribution. Then, we test the hypothesis that these sums are different from zero for both the stock and the bond market, i.e., whether we can reject the joint hypothesis that $\gamma_m^{S\&P} = 0$ and $\gamma_m^{Tnote} = 0$. The respective Wald test statistic follows a χ^2-distribution with two degrees of freedom. For this test, we use the estimated covariance between the residuals in the stock and bond market equations to account for correlation between the stock and bond market regression coefficients.

4 Empirical Results

This section presents regression and graphical evidence of the pre-announcement price drift. Section 4.1 presents a time-series regression and cumulative average return graphs. Section 4.2 presents cumulative order imbalance graphs. Section 4.3 extends the sample back to the year 2003 using minute-by-minute data.

4.1 Pre-Announcement Price Drift

We start with results based on second-by-second data from January 2008 to March 2014. We first estimate equation (2), followed by the single and joint hypotheses tests, which sum up coefficients corresponding to the window spanning from 30 minutes before the official release time to five seconds before the official release time for each market. Table 2 presents the results of these tests. The last column tests the hypothesis that these sums for the stock and bond markets are jointly different from zero. In the table, the announcements are sorted by the p-value of this joint test. There are nine announcements whose summed drift coefficients are significant at the 5% level indicating a pre-announcement price drift (column 4). Four of these announcements exhibit significant drift in the stock market (column 2), and all nine announcements exhibit significant drift in the bond market (column 3).[16] In all nine announcements, the drift is in the "correct" direction, i.e., the direction of the price change predicted by the announcement surprise.

Stock prices increase and bond prices decrease before good economic news, for example, higher than anticipated ISM Non-Manufacturing Index. Specifically, the S&P 500 futures prices increase on average by 0.104 percent *before* a one standard deviation positive surprise in the ISM Non-Manufacturing Index. The magnitude of the coefficients is sizable. For comparison, one standard deviation of 5-minute returns during our entire sample period for the stock and bond markets is 0.12 and 0.04 percent, respectively. These results stand in contrast to previous studies concluding that the pre-announcement effect is small or non-existent in macroeconomic announcements. The results show that pre-announcement informed trading is limited to neither corporate announcements (Campbell et al., 2009; Kaniel et al., 2012) nor FOMC announcements (Bernile et al., 2016).

The full set of macroeconomic announcements is vast. Most announcements, however, contain information of only secondary importance. These announcements have only a negligible effect on the market and thus no meaningful profit potential for informed traders.[17] To limit the analysis to the set of relevant, i.e., market-moving, announcements we use the sum of the coefficients on the lagged, contemporaneous and lead surprises, $\tilde{\gamma}_m \equiv \sum_{k=-1}^{K} \gamma_{mk}$, as test criterion. This sum corresponds to the window spanning from 30 minutes before the official release time to five minutes after the official release time.[18] We identify the set of

[16] A joint test of the 30 hypotheses overwhelmingly confirms the overall statistical significance of the pre-announcement price drift. The large Wald statistic (12,292 and 11,849 for the stock and bond markets, respectively) implies statistical significance of the pre-announcement drift at the 1% level.

[17] Focusing on a small subset of announcements with high intrinsic value (Gilbert, Scotti, Strasser, & Vega, 2017) can be seen as a consequence of an optimal information acquisition strategy in presence of private information (Hirshleifer, Subrahmanyam, & Titman, 1994).

[18] We use five minutes after the official release time as the end of the post-announcement interval, to capture the full price move after the official release. Although previous papers such as Hu et al. (2017) indicate that announcements are almost instantaneously reflected in prices once released, we find some evidence of ongoing price adjustment after the first minute.

Table 2: Announcement Surprise Impact During $[t - 30min, t - 5sec]$

Announcement	E-mini S&P 500 γ_m	10-year Treasury Note γ_m	Joint Test p-value
ISM Non-manufacturing index	0.104 (0.017)***	-0.044 (0.009)***	<0.001
Pending home sales	0.099 (0.018)***	-0.028 (0.008)***	<0.001
ISM Manufacturing index	0.088 (0.019)***	-0.022 (0.008)***	<0.001
CB Consumer confidence index	0.040 (0.020)*	-0.032 (0.008)***	<0.001
Existing home sales	0.054 (0.021)***	-0.016 (0.007)**	0.012
Advance retail sales	0.003 (0.018)	-0.019 (0.007)***	0.016
GDP preliminary	0.049 (0.030)	-0.031 (0.011)***	0.018
Initial jobless claims	-0.005 (0.007)	0.008 (0.003)***	0.020
GDP advance	0.015 (0.032)	-0.035 (0.015)**	0.049
Factory orders	-0.043 (0.021)**	0.019 (0.010)*	0.060
Industrial production	0.032 (0.018)*	-0.006 (0.010)	0.203
Trade balance	-0.016 (0.016)	0.010 (0.006)*	0.219
Construction spending	0.030 (0.019)	-0.009 (0.007)	0.226
Consumer credit	-0.024 (0.015)	0.000 (0.006)	0.238
Building permits	-0.018 (0.015)	-0.005 (0.007)	0.244
Personal income	-0.020 (0.015)	-0.001 (0.007)	0.296
Government budget	-0.020 (0.024)	0.011 (0.007)	0.333
Personal consumption	0.008 (0.015)	0.005 (0.006)	0.433
New home sales	-0.021 (0.020)	0.009 (0.008)	0.456
Wholesale inventories	0.008 (0.019)	-0.009 (0.008)	0.539
Durable goods orders	-0.004 (0.014)	-0.005 (0.006)	0.644
Consumer price index	-0.014 (0.016)	0.003 (0.007)	0.648
UM Consumer sentim. (prel.)	0.017 (0.020)	-0.005 (0.008)	0.671
Index of leading indicators	0.014 (0.018)	-0.005 (0.008)	0.678
Non-farm employment	0.001 (0.013)	-0.005 (0.006)	0.686
Housing starts	0.009 (0.017)	-0.005 (0.007)	0.704
Producer price index	-0.003 (0.016)	-0.003 (0.007)	0.858
ADP employment	0.005 (0.015)	-0.003 (0.006)	0.859
UM Consumer sentim. (final)	0.005 (0.017)	-0.003 (0.007)	0.895
GDP final	0.003 (0.020)	-0.003 (0.014)	0.978

The sample period is from January 1, 2008 through March 31, 2014. The reported results sum up coefficients corresponding to the $[t - 30min, t - 5sec]$ window estimated using equation (2) with weighted least squares procedure for each market described in Section 3. Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5% and 1% levels, respectively. The last column shows p-values for the joint χ^2-test that sums of coefficients on announcement surprises in the S&P 500 and Treasury note markets are different from zero as described in Section 3.

market-moving announcements by testing the null hypothesis that surprises have no effect in each market, i.e., $\tilde{\gamma}_m = 0$. The two middle columns of Table 3 present – analogous to Table 2 – the results of this t-test on $\tilde{\gamma}_m$ separately for the stock and the bond market. The last column tests the hypothesis that the sums in these two markets are jointly different from zero. The results are sorted by the p-value of this joint test.[19] Based on this p-value, 20 of the 30 announcements have a price impact that is statistically significant at the 5% level, and as expected all announcements with significant pre-announcement drift in both markets fall in this category.

To quantify the relevance of the pre-announcement price drift, we compare it to the total price impact of a given announcement. We measure the total impact again by the price change from 30 minutes before to five minutes after the official release time. Table 4 shows the pre-announcement price drift as a proportion of the total price change, sorted by the ratio obtained for the stock market. We divide the γ_m coefficients from Table 2 by the corresponding $\tilde{\gamma}_m$ coefficients from Table 3 and present the ratios in columns (3) and (6). All values are positive and below 100%. This means that the early signal is informative but noisy. It is either not always present or imperfect. The ratio ranges from 30 percent in the ISM Manufacturing Index announcement to 67 percent in the Pending Home Sales announcement indicating that the pre-announcement price move is a substantial proportion of the total price move. The mean ratio is 49 percent in the stock market and 36 percent in the bond market. Therefore, failing to account for the pre-announcement effect substantially underestimates the total impact of these macroeconomic announcements on financial markets.

A drift of almost 50 percent of the total announcement impact appears large at first sight. However, in a model of Bayesian learning, little information is needed to generate a pre-announcement drift of this magnitude. In Appendix Section A.1, we derive a condition on the relative precision and surprise size of early signals and official release under which the impact of the early signals exceeds the impact of the official release. The appendix presents an example without prior public information, in which an early signal with one half of the precision and with two thirds of the surprise generates the same price impact as the

[19]Similarly to Table 2, the last column is again based on a χ^2-test where we use the estimated covariance between the residuals in the stock and bond market equations to account for correlation between the stock and bond market regression coefficients. We also conduct a joint test of the 30 hypotheses. This test overwhelmingly confirms the overall statistical significance of the total price move. The computed values of the Wald statistic are very large (159,560 and 258,212 for the stock and bond markets, respectively). This translates into statistical significance of the total price move at the 1% level.

Table 3: Announcement Surprise Impact During $[t-30min, t+5min]$

Announcement	E-mini S&P 500 $\tilde{\gamma}_m$	10-year Treasury Note $\tilde{\gamma}_m$	Joint Test p-value
Non-farm employment	0.435 (0.016)***	-0.283 (0.008)***	<0.001
ISM Manufacturing index	0.292 (0.022)***	-0.131 (0.009)***	<0.001
Initial jobless claims	-0.096 (0.008)***	0.052 (0.003)***	<0.001
ADP employment	0.159 (0.017)***	-0.099 (0.007)***	<0.001
Advance retail sales	0.160 (0.020)***	-0.089 (0.008)***	<0.001
ISM Non-manufacturing index	0.167 (0.019)***	-0.090 (0.010)***	<0.001
CB Consumer confidence index	0.171 (0.023)***	-0.078 (0.008)***	<0.001
Pending home sales	0.147 (0.020)***	-0.053 (0.009)***	<0.001
Consumer price index	-0.080 (0.017)***	-0.034 (0.008)***	<0.001
Existing home sales	0.148 (0.023)***	-0.048 (0.008)***	<0.001
GDP preliminary	0.130 (0.034)***	-0.081 (0.012)***	<0.001
Durable goods orders	0.073 (0.015)***	-0.042 (0.007)***	<0.001
Housing starts	0.048 (0.018)***	-0.044 (0.007)***	<0.001
GDP advance	0.134 (0.036)***	-0.064 (0.016)***	<0.001
UM Consumer sentim. (prel.)	0.083 (0.022)***	-0.027 (0.009)***	<0.001
New home sales	0.071 (0.022)***	-0.033 (0.009)***	<0.001
Construction spending	0.040 (0.022)*	-0.027 (0.008)***	0.003
Producer price index	-0.004 (0.018)	-0.021 (0.007)***	0.005
GDP final	0.059 (0.022)***	-0.029 (0.015)*	0.014
Industrial production	0.052 (0.020)***	-0.017 (0.010)*	0.021
Index of leading indicators	0.036 (0.020)*	-0.011 (0.008)	0.158
Personal consumption	0.020 (0.016)	-0.011 (0.007)	0.222
UM Consumer sentim. (final)	0.013 (0.019)	-0.013 (0.008)*	0.233
Building permits	0.002 (0.017)	-0.012 (0.007)	0.236
Wholesale inventories	-0.006 (0.021)	-0.009 (0.009)	0.454
Personal income	0.015 (0.016)	-0.008 (0.007)	0.490
Consumer credit	0.003 (0.017)	-0.005 (0.006)	0.701
Trade balance	-0.001 (0.018)	0.005 (0.007)	0.777
Government budget	-0.011 (0.026)	0.004 (0.008)	0.849
Factory orders	-0.001 (0.024)	-0.004 (0.011)	0.933

The sample period is from January 1, 2008 through March 31, 2014. The reported results sum up coefficients corresponding to the $[t-30min, t+5min]$ window estimated using equation (2) with weighted least squares procedure for each market described in Section 3. Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5% and 1% levels, respectively. The last column shows p-values for the joint χ^2-test that sums of coefficients on announcement surprises in the S&P 500 and Treasury note markets are different from zero as described in Section 3.

news at the official release time itself. Earlier information can get more attention than later information and thus have a larger price impact even if the later information is "official" and more precise.

To illustrate our findings graphically, we present cumulative average return (CAR) graphs.

Table 4: Pre-announcement Price Drift as a Proportion of Total Price Change

	(1)	(2)	(3)	(4)	(5)	(6)
	E-mini S&P 500			**10-year Treasury Note**		
	$[t-30min,$ $t-5sec]$	$[t-30min,$ $t+5min]$	Ratio	$[t-30min,$ $t-5sec]$	$[t-30min,$ $t+5min]$	Ratio
Pending home sales	0.099	0.147	67%	-0.028	-0.053	53%
ISM Non-manufacturing index	0.104	0.167	62%	-0.044	-0.090	49%
Existing home sales	0.054	0.148	37%	-0.016	-0.048	34%
ISM Manufacturing index	0.088	0.292	30%	-0.022	-0.131	17%
GDP advance	n.d.			-0.035	-0.064	55%
CB Consumer confidence index	n.d.			-0.032	-0.078	41%
GDP preliminary	n.d.			-0.031	-0.081	38%
Advance retail sales	n.d.			-0.019	-0.089	22%
Initial jobless claims	n.d.			0.008	0.052	16%
Mean			49%			36%

The sample period is from January 1, 2008 through March 31, 2014. Only announcements showing significant evidence (at the 5% level) of pre-announcement drift in each market in Table 2 are included. "n.d." denotes no significant drift (at the 5% level) in the S&P 500 market.

We classify each event based on whether the surprise has a positive or negative effect on the stock and bond markets using the coefficients in Table 3. Following Bernile et al. (2016), we invert the sign of returns for negative surprises.[20] CARs are then calculated in the $[t-60min, t+60min]$ window for each of the "drift" and "no drift" categories based on Tables 2 and 3: In the stock market, there are four drift and sixteen no-drift announcements, and in the bond market, there are nine drift and eleven no-drift announcements.[21] The CARs in Figure 1 reveal what happens around these announcements. In the no-drift announcements in Panel (a), a significant price adjustment does not occur until after the release time. In the drift announcements in Panel (b), the price begins moving in the correct direction about 30 minutes before the official release time.[22]

In terms of underlying trading strategies it is interesting to note that the significant pre-

[20] Based on Table 3 higher than expected Initial Jobless Claims drive stock markets down and bond markets up. Accordingly, we invert the signs for the Initial Jobless Claims in both stock and bond CARs. For the same reason, we reverse the sign for the Consumer Price Index (CPI) and Producer Price Index (PPI) in the stock market CAR.

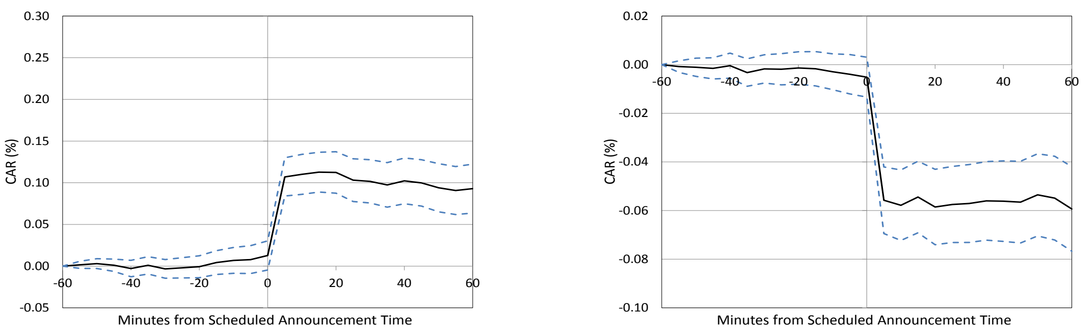
[21] The Internet Appendix Figure B.1 shows CARs for the individual announcements.

[22] The CARs hover around zero during the $[t-180min, t-30min]$ window (in the Internet Appendix) similarly to during the $[t-60min, t-30min]$ window in Figure 1.

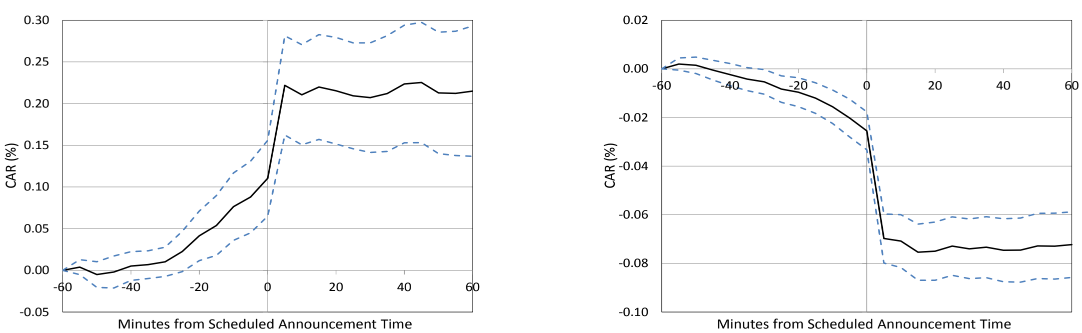
Figure 1: Cumulative Average Returns

E-mini S&P 500 **10-year Treasury Note**

(a) Announcements without evidence of drift



(b) Announcements with evidence of drift



The sample period is from January 1, 2008 through March 31, 2014. We classify each event as "good" or "bad" news based on whether the announcement surprise has a positive or negative effect on the stock and bond markets using the coefficients in Table 3. Following Bernile et al. (2016), we invert the sign of returns for negative surprises. Cumulative average returns (CARs) are then calculated in the $[t-60min, t+60min]$ window for each of the "drift" and "no drift" categories based on Tables 2 and 3. In the stock market, there are four drift and sixteen no-drift announcements, and in the bond market, there are nine drift and eleven no-drift announcements. For each category the solid line shows the mean CAR. Dashed lines mark two-standard-error bands (standard error of the mean).

announcement price drift occurs only about 30 minutes before the release time. If informed traders possessed private information already earlier, the question would arise why they trade on their knowledge only shortly before the respective announcement. We offer three possible explanations for this. In all of these rationales, the source of private information is irrelevant for the optimality of a given trading strategy.

First, it is possible that traders gain access to private information just shortly before the official release time. The recent SEC (2015) press release gave an example of a corporation that transmitted earnings and revenue information to a news release agency 36 minutes before the official release time. Hackers intercepted this information and relayed it to traders in their international criminal ring who started trading ten minutes after the corporation's transmission while the information was still confidential. Similarly, the information might be obtained shortly before the official release time by proprietary data collection, for example, by proprietary surveys, to maximize the accuracy of the collected data.

Second, traders may choose to execute trades close to the release time instead of during the preceding hours in order to minimize exposure to risks that are unrelated to the macroeconomic announcement but are driven by other unpredictable economic or geopolitical events.

Third, informed traders might choose their timing in an attempt to strategically "hide" their trades. Trading on private information is easier when liquidity is high because then it is more likely that informed trades will go unnoticed (Kyle, 1985). Although we do not have limit order data to measure the bid-ask spread, research such as Wang and Yau (2000) shows that the bid-ask spread is inversely related to trading volume in the futures markets. Trading volume increases substantially (more than fivefold) in the S&P 500 futures market at 9:30 due to the opening of the stock market and the beginning of the open outcry trading. All four announcements exhibiting drift in the S&P 500 futures (Existing Home Sales, ISM Manufacturing Index, ISM Non-Manufacturing Index and Pending Home Sales) are released at 10:00, and indeed there is a substantial increase in trading volume 30 minutes before the announcement release time. This timing can allow informed traders to take advantage of this increase in volume not related to the announcement.[23]

4.2 Order Flow Imbalances and Profits to Informed Trading

Evidence of informed trading is not limited to prices but is visible in order imbalances as well. We use data on the total trading volume and the last trade price in each one-second interval. Following Bernile et al. (2016), we classify trading volume as buyer- or seller-initiated using the tick rule. Specifically, the trade volume in a one-second interval is classified as buyer-initiated (seller-initiated) if the price for that interval is higher (lower) than the last different price.[24] Figure 2 plots cumulative order imbalances for the same time window as Figure 1. Similarly to price drift, order flow imbalances start building up about 30 minutes prior to the announcement release, pointing to informed trading during the pre-announcement interval.

The magnitude of the drift is economically significant. To approximate the magnitude of total profit in the S&P 500 futures market earned by market participants trading in the correct direction ahead of the announcements, we proceed as follows: Assume that there is an entry price, P_{Entry}, at which informed traders enter a trade before the release, and an exit price, P_{Exit}, at which they exit shortly after the release. P_{Entry} and P_{Exit} are computed as volume-weighted average prices (VWAP) over the $[t - 30min, t - 5sec]$ and $[t + 5sec, t + 1min]$ windows, respectively. We exclude the five seconds before and after the announcement releases to reduce, in our calculations, the dependence on movements immediately surrounding the release. We then multiply $P_{Exit} - P_{Entry}$ by the sign of the surprise and take the sample average. This average represents the average return of trading

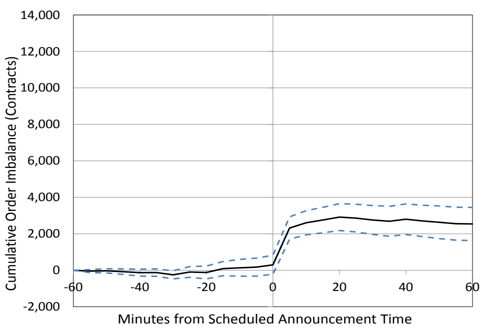
[23] Appendix Figure A1 illustrates both the spike in trading at 9:30 (upper panel) as well as the increase in trading volume 30 minutes before the announcement releases in event time (lower panel). Kyle (1985) and Admati and Pfleiderer (1988) provide a theoretical exposition of how informed speculators trade strategically to avoid revealing their information in the price.

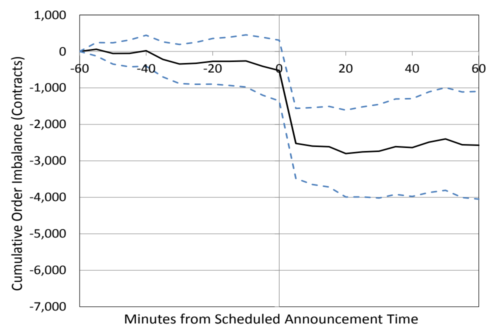
[24] We examine the performance of this volume classification algorithm using detailed limit order book data for our futures contracts that we have available for one month (July 2013). This limit order book data contains accurate classification of each trade as buyer- or seller-initiated. Based on the classification accuracy measure proposed by Easley, Lopez de Prado, and O'Hara (2016), the tick rule correctly classifies 95% and 91% of trading volume in the S&P 500 and the Treasury note futures, respectively.

Figure 2: Cumulative Order Imbalances

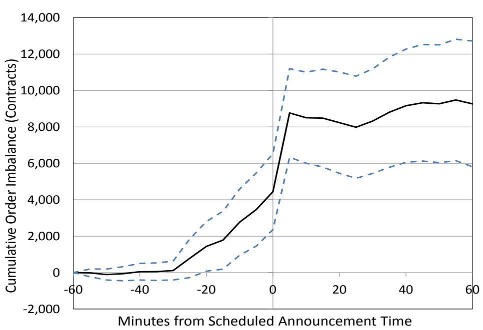
E-mini S&P 500 **10-year Treasury Note**

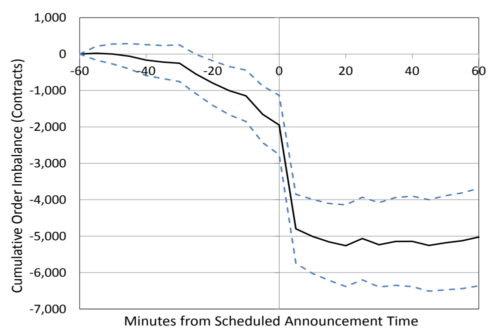
(a) Announcements without evidence of drift





(b) Announcements with evidence of drift





The sample period is from January 1, 2008 through March 31, 2014. Announcements are categorized as no "drift" and "no drift" categories based on Tables 2 and 3. In the stock market, there are four drift and sixteen no-drift announcements, and in the bond market, there are nine drift and eleven no-drift announcements. For each category, we compute cumulative order imbalances in the event window from 60 minutes before the release time to 60 minutes after the release time. Analogous to Figure 1 we invert the sign of returns for negative surprises. We winsorize the order imbalances at the 1^{st} and 99^{th} percentiles to reduce the influence of extreme observations. Dashed lines mark two-standard-error bands (standard error of the mean).

in the direction of the surprise since all the surprises have positive impact on the S&P 500 prices. Because the sign of the surprise is either positive or negative unity, this can also be interpreted as a regression of the VWAP return on the sign of the surprise. To estimate the quantity, we use the fact that the order flow is on average in the direction of the surprise as shown in Figure 2. In fact, the correlation between the sign of the surprise and the order flow in the S&P 500 market is approximately +0.19. Hence, we compute the order flow over the $[t - 30min, t - 5sec]$ window and multiply it by the sign of the surprise.[25] We then compute the sample average and consider this to be the average quantity traded by informed traders. This quantity can be interpreted as the order flow explained by the surprise. Our estimate of profits is the product of the average return times the average quantity times the value of the contract. The contract size of the S&P 500 futures contract is $50 times the index.

Using this methodology, we compute the average profit for each announcement that exhibits a drift (four announcements in the stock market and nine announcements in the bond market per Table 2). We multiply this average profit by the number of observations for the given announcement to compute the total profit for that announcement. We then add up these total profits across announcements. The approximate total profit during a little more than six years adds up to $95 million and $89 million in the E-mini S&P 500 futures and 10-year Treasury note futures markets, respectively.

The median effective bid-ask spread is 0.020% for the E-mini S&P 500 futures and 0.013% for 10-year Treasury notes futures.[26] This is far below the two standard deviation band of the CAR around drift announcements in Figure 1. Sophisticated traders who use execution algorithms are likely able to trade round trip close to the spread midpoint and incur a slippage that is smaller than the spread. Informed trades around drift announcements are, therefore, profitable.

As a robustness check, we also compute the profit obtained by trading in the direction

[25] We winsorize the order flow at the 1^{st} and 99^{th} percentiles to reduce the influence of extreme observations.

[26] We compute the effective bid-ask spread as the average absolute value of price changes (after excluding price changes in the same direction), which is a common approach to estimating the spread with transaction data used in, for example, Kurov (2008).

of the order flow on non-announcement days using the same methodology but without multiplying by the sign of the surprise as no announcement is released on those days. We find that simply trading in the direction of the order flow produces profits that are one order of magnitude lower than trading the pre-announcement price drift with information on the surprise. We conclude that there is evidence that the economic profits of the pre-announcement price drift are substantial.

4.3 Increase in Drift After 2007

Our *second-by-second* data starts on January 1, 2008. The existing literature referenced in Section 1 analyzes earlier sample periods, for which we do not have such high-frequency data. However, we have *minute-by-minute* data for the sample period from August 1, 2003 to December 31, 2007. Therefore, we repeat the analysis of Section 4.1 using the same 30 announcements for this sample period.[27] We use one minute before the official release time as the cutoff for the pre-announcement interval to again ensure that early releases (for example, pre-releases of the UM Consumer Sentiment two seconds before the official release time discussed in Section 2) do not fall into our pre-announcement interval.

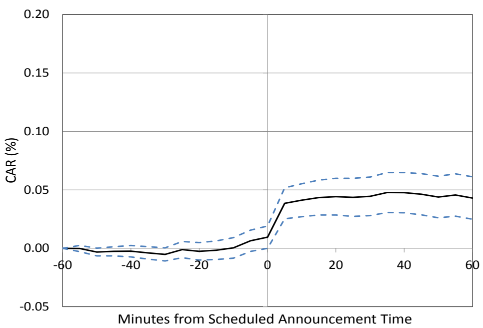
Figure 3 shows CARs for market-moving announcements based on this minute-by-minute data for 2003-2007. Compared to 2008-2014 sample period in Figure 1, two features stand out. First, the total announcement impact is less pronounced particularly in the S&P 500 futures market. Second, the pre-announcement drift is negligible. Only four announcements exhibit a pre-announcement price drift during the pre-2008 period. The pre-announcement effect was weaker or non-existent during the pre-2008 period.

A variety of factors may have contributed to this change. One contributing factor may be a differential impact of macroeconomic announcements on financial markets between recessions and expansions as shown by, for example, Boyd, Hu, and Jagannathan (2005) and Andersen et al. (2007). This state-dependence suggests that the pre-2008 and post-2008

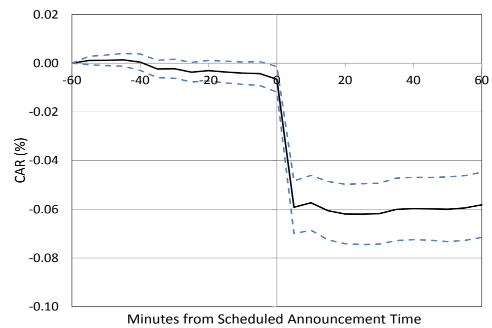
[27]This sample contains intraday data for the bond market starting at 8:20 a.m. ET. Therefore, returns are sampled in the 8:20-16:00 window, whereas in Section 4.1 they are sampled in the 7:15-16:00 window.

Figure 3: Cumulative Average Returns, 2003–2007

E-mini S&P 500 Futures



10-year Treasury Note Futures



The sample period is from August 1, 2003 through December 31, 2007. We classify each event as "good" or "bad" news based on whether the announcement surprise has a positive or negative effect on the stock and bond markets. Analogous to Figure 1 we invert the sign of returns for negative surprises. Cumulative average returns (CARs) are then calculated in the $[t - 60min, t + 60min]$ window for all market-moving announcements. For each category the solid line shows the mean CAR. Dashed lines mark two-standard-error bands (standard error of the mean).

periods should differ because an economic expansion ended and the Great Recession began at the end of 2007. Our results confirm this state-dependence.

Interestingly, the response to surprises does not change its direction again around the (official) end of the Great Recession, dated by the National Bureau of Economic Research to June 2009. Better than expected news boosts prices in the stock market and lowers prices in the bond market throughout the 2008-2014 sample period. Andersen et al. (2007) relate the changing stock market reaction to macroeconomic news across the business cycle to anti-inflationary monetary policy, with good economic news causing a negative response in expansions but a positive response in contractions. The absence of the reversal in 2009 can be explained using an analogous argument related to the effect of post-2008 unconventional monetary policies on the market expectations. The stock market response to news hinges on the expected reaction of monetary policy to macroeconomic news (Kurov & Stan, 2018). After the official end of the Great Recession, the Federal Reserve continued unconventional monetary policies because of a slow recovery and other reasons. The Federal Reserve's quantitative easing and communication with the markets muted expectations of notable tightening of monetary policy until the spring of 2013. Absent any imminent tightening, good economic news continued to be good news for stocks until the end of our sample period. The strong response of the stock market to macroeconomic announcements increased the rewards to informed trading before the official release time.

General macroeconomic conditions and the related monetary policy are not the only changes in recent years. The procedures for releasing the announcements changed, and information collection and computing power increased, which might have enabled sophisticated market participants to forecast some announcements. We discuss these explanations in Section 5.

5 Causes of Pre-Announcement Price Drift

The pre-announcement price drift documented in Section 4 establishes that market prices are based on a broader information set $\Omega_{t-\tau}$ than the information set $\Omega_{t-\Delta}$ reflected in market expectations measured by the Bloomberg consensus forecast, i.e., $\Omega_{t-\Delta} \subset \Omega_{t-\tau}$. An equality of these two information sets would require that, first, there is no information in the market beyond public information, and, second, the public information is fully captured by the Bloomberg consensus forecast.

A popular explanation for a failure of the first requirement is information leakage. The corporate finance literature (for example, Sinha & Gadarowski, 2010; Agapova & Madura, 2011) considers price drift before public guidance issued by company management as de facto evidence of information leakage. We explore this possible explanation in Section 5.1.1. But at least one alternative explanation exists. In Section 5.1.2, we look for evidence that some traders may collect proprietary information themselves which allows them to forecast announcements better than other traders.

A failure of the second requirement could stem from a variety of unavoidable data imperfections. First, the calculation of the consensus forecast by Bloomberg is a plausible summary statistic of the forecasters' responses but not necessarily the best one. Second, the forecasters' responses might not reflect an optimal forecast, which creates room for some traders to analyze public information in a superior way. Third, if the sampling of expectations precedes the beginning of the event window, i.e., if $\Delta > \tau$, market expectations might change by time $t - \tau$. We discuss these possible explanations in Section 5.2.1. Section 5.2.2 discusses the possibility of uninformed traders "jumping on the bandwagon" with informed traders.

5.1 Private Information

This section considers possible links between the pre-announcement drift and private information. We start with private information obtained by leakage and follow with private information obtained by proprietary data collection.

5.1.1 Information Leakage

Insider trading based on leaked information can seriously impair markets. It reduces risk sharing and the informational efficiency of prices in the long run (Brunnermeier, 2005). The U.S. macroeconomic data is generally considered closely guarded as federal agencies restrict the number of employees with access to the data, implement computer security measures and take other actions to prevent premature dissemination. The procedures of the U.S. Department of Labor (DOL), for example, are described in Fillichio (2012). The last documented case of a U.S. government employee fired for data leakage dates far back: In 1986, one employee of the Commerce Department was terminated for leaking the Gross National Product data (Wall Street Journal, 1986). However, the possibility of leakage in more recent times still exists. In this section, we examine two aspects of the release process that may affect leakage: organization type and release procedures.

With respect to organization type, we distinguish organizations subject to the Principal

Federal Economic Indicator (PFEI) guidelines and other entities. Guidance on releasing data is provided to statistical agencies by the Office of Management and Budget. Key economic announcements are designated as PFEIs, and the agencies are required to follow strict security procedures when releasing them to ensure fairness in markets (Office of Management and Budget, 1985). This includes government agencies and the Federal Reserve Board.

However, ensuring that market participants receive all market-moving macroeconomic data at the same time is complicated by the fact that some important data is collected and released by private entities that are not subject to the PFEI guidelines. Some of these data providers have been known to follow release procedures that would not be allowed for the PFEIs. For example, Thomson Reuters created a high-speed data feed for paying subscribers where the Consumer Sentiment Index prepared by the University of Michigan was released two seconds earlier to an exclusive group of subscribers before being made available to the public as discussed in Section 2.2. We, therefore, examine the possibility that the organization type is related to the pre-announcement drift. In Table 5 there are thirteen PFEI and seven non-PFEI announcements among our 20 market-moving announcements. Five of the seven non-PFEI announcements show evidence of pre-announcement drift.

With respect to release procedures, we are interested in safeguards against premature dissemination. We conducted a thorough phone and email survey of the organizations in our sample. We distinguish three types of release procedures summarized in the "Pre-release" and "Safeguarding" columns of Table 5.

The first type refrains from any pre-release and involves posting the announcement on the organization's website that all market participants can access at the same time. It is used in four announcements in our sample. The second type involves pre-releasing the information to journalists in designated "lock-up rooms." The purpose of the preview is to allow the journalists to understand the data before writing their news stories and thus provide more informed news coverage to the public.[28] This release type is wide-spread and

[28]The pre-release period is 60 minutes in the Bureau of Economic Analysis announcements and 30 minutes in the Bureau of Labor Statistics, Bureau of Census, Conference Board (until 2013), Employment and

Table 5: Principal Federal Economic Indicators and Pre-release Procedures

Announcement	Source	PFEI	Pre-release	Safeguarding
		Pre-Announcement Drift		
Advance retail sales	BC	Y	Y	Lockup room
CB Consumer confidence index	CB	N	Y/N[b]	Embargo only[b]
Existing home sales	NAR	N	Y	Lockup room
GDP advance	BEA	Y	Y	Lockup room
GDP preliminary	BEA	Y	Y	Lockup room
Initial jobless claims	ETA	Y[a]	Y	Lockup room
ISM Non-manufacturing index	ISM	N	N	n.a.
ISM Manufacturing index	ISM	N	N	n.a.
Pending home sales	NAR	N	Y	Embargo only
		No Pre-Announcement Drift		
ADP employment	ADP	N	N	n.a.
Consumer price index	BLS	Y	Y	Lockup room
Construction spending	BC	Y	Y	Lockup room
Durable goods orders	BC	Y	Y	Lockup room
GDP final	BEA	Y	Y	Lockup room
Housing starts	BC	Y	Y	Lockup room
Industrial production	FRB	Y	Y	Embargo only
New home sales	BC	Y	Y	Lockup room
Non-farm employment	BLS	Y	Y	Lockup room
Producer price index	BLS	Y	Y	Lockup room
UM Consumer sentiment - Prel[c]	TRUM	N	N	n.a.

[a] The Initial Jobless Claims is not a PFEI. We mark this announcement as PFEI because it is released by the Department of Labor (DOL) Employment and Training Administration under the same release procedures as the DOL PFEIs such as Non-Farm Employment.
[b] The Conference Board eliminated the pre-release in June 2013.
[c] Until July of 2013, the Preliminary University of Michigan Consumer Sentiment Index was pre-released via Thomson Reuters two seconds before the official release time to high-speed data feed clients.

used in thirteen market-moving announcements in our sample. A testimony in front of the U.S. House of Representatives by the DOL official responsible for lock-up security highlights the challenges of preventing premature dissemination from lock-up rooms. For example, news media installed their own computer equipment in the DOL's lock-up room without the DOL staff being able to verify what exactly the equipment does (Fillichio, 2012; Hall, 2012). A wire service accidentally transmitted the data during the lock-up period (Fillichio, 2012; Hall, 2012). Cell phones were supposed to be stored in a designated container, but one individual accessed and used his phone during the lock-up (Fillichio, 2012). Some organizations have

Training Association, and National Association of Realtors announcements. We were unable to determine the pre-release period length for the Federal Reserve Board.

exploited the loose definition of what constitutes a media outlet and obtained access to the lock-up rooms designed for journalists. Mullins and Patterson (2013) write about the "Need to Know News" outlet. After the DOL realized that this entity was in the business of transmitting data via high-speed connections to financial firms, the DOL revoked its access to the lock-up room. Recognizing that securing pre-release is a formidable task, the DOL has been reported to consider eliminating the lock-up room (Mullins, 2014).

In addition, our survey uncovers a third type of release procedures that has not been documented in academic literature. Three announcements are pre-released to journalists electronically. The Pending Home Sales announcement is transmitted by the National Association of Realtors to journalists who are asked not to share the information with individuals other than those working on the news story. The Industrial Production announcement is pre-released by the Federal Reserve Board through an electronic system to selected reporters at credentialed news organizations that have written agreements governing this access (Federal Reserve Board, 2014). The Conference Board (CB) used to pre-release the CB Consumer Confidence Index to a group of media outlets that had signed an agreement not to distribute the information prior to the release time; this pre-release was eliminated in June of 2013, and the information is now posted directly on the CB website.

We examine the possibility that the release procedures play a role in our findings. A cursory look at Tables 4 and 5 reveals that two of the three announcements with the least secure release procedure (CB Consumer Confidence Index and Pending Home Sales) are among our nine drift announcements.

To test this more formally, we introduce three indicator variables $X^i_{m,t}$, $i \in \{1, 2, 3\}$ that capture the organization type and release procedures. The "PFEI" indicator $X^1_{m,t}$ takes on the value of unity if the announcement is released by an organization required to follow PFEI procedures, the "pre-release" indicator $X^2_{m,t}$ equals unity if the announcement is pre-released,[29] and the "embargo-only" indicator $X^3_{m,t}$ is unity if the announcement is

[29]The pre-release variable does not capture leakage outside of the lock-up, for example, via staff that prepares and disseminates the information or the government officials that receive the information ahead of

pre-released under a simple embargo. In all other cases, the indicator variables are zero.

Only for the CB Consumer Confidence Index the release procedure changed during our sample period. Otherwise, the indicator variables for a given announcement are constant over time. The identification of the effect of release procedures must, therefore, rely on cross-sectional variation. To allow the pooling of announcements, we adjust the sign of the surprises such that a positive surprise increases stock and lowers bond prices based on the sign of the sum of $\tilde{\gamma}_m$ coefficients in Table 3.[30]

Let us denote the sign-adjusted surprise by $\tilde{S}_{m,t}$. We define $\bar{S}_t$ as the cross-sectional average of all non-zero surprises at time t:

$$\bar{S}_t = \sum_{m=1}^{M} \tilde{S}_{m,t} \Bigg/ \sum_{m=1}^{M} \mathbb{1}(|\tilde{S}_{m,t}| > 0).$$

Further, we define $\bar{X}_{i,t}$ which interacts the release procedure dummies $X^i_{m,t}$ with the surprise. Here the cross-sectional average at time t is conditional on the release procedure:

$$\bar{X}_{i,t} = \sum_{m=1}^{M} \left[\tilde{S}_{m,t} \mathbb{1}(X^i_{m,t} = 1)\right] \Bigg/ \sum_{m=1}^{M} \mathbb{1}(|\tilde{S}_{m,t}| > 0).$$

By including these averages in equation (2) we obtain:

$$R_t = \beta_0 + \sum_{j=1}^{2} \beta_j R_{j,t-1} + \sum_{k=-1}^{5} \left[\gamma_k \bar{S}_{t+k} + \sum_{i=1}^{3} \delta_{ik} \bar{X}_{i,t+k}\right] + \epsilon_t. \tag{3}$$

Table 6 reports the sum of the contemporaneous and leading coefficients in equation (3). This sum of coefficients captures the pre-announcement drift.[31] The first row gives the 30-

time (Javers, 2012) or leakage via information technology systems accessed by hackers (SEC, 2015). Other factors that might affect the likelihood of leakage include the number of individuals involved in the release process and the length of time from data collection to release. However, this information is not publicly available, and we were unable to obtain it from all organizations.

[30] For the E-mini S&P 500 market, the surprises are multiplied by the sign of the sum of $\tilde{\gamma}_m$ coefficients (on one lagged, one contemporaneous and five lead terms as explained in Section 4.1). For the 10-year Treasury note, the surprises are multiplied by minus one times the sign of the sum of the $\tilde{\gamma}_m$ coefficients. This ensures that surprises are signed to have the same impact as in equation (2).

[31] Similarly to Tables 2 and 3, we test as well whether the sums of coefficients in the S&P 500 and Treasury

Table 6: Effect of Organization Type and Release Procedures

	E-mini S&P 500		10-year Treasury Note	
	(1)	(2)	(3)	(4)
Surprise	0.028 (0.007)***	0.032 (0.006)***	-0.014 (0.003)***	-0.015 (0.003)***
PFEI	-0.057 (0.013)***	-0.025 (0.008)***	0.017 (0.005)***	0.008 (0.004)**
Pre-release	0.040 (0.014)***	n.a.	-0.011 (0.006)**	n.a.
Embargo-only	n.a.	0.034 (0.012)***	n.a.	-0.012 (0.006)**

The sample period is from January 1, 2008 through March 31, 2014. The results show weighted least squares estimates of equation (3) for each market as described in Section 3. Reported coefficients in the first row are the sum $\sum_{k=0}^{5} \gamma_k$ and in the bottom three rows the sum $\sum_{k=0}^{5} \delta_{ik}$. Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5% and 1% levels, respectively.

minute pre-announcement drift of an average non-PFEI announcement, which is either not prereleased or released in a lockup room, i.e., $\sum_{k=0}^{5} \gamma_k$. The bottom three rows show how release procedures affect the pre-announcement price drift, reflected in the respective sums of the interaction variables, $\sum_{k=0}^{5} \delta_{ik}$. We present two regression specifications: The first specification in columns (1) and (3) includes the Surprise, PFEI and Pre-release variables. The second specification in columns (2) and (4) includes the Surprise, PFEI and Embargo-only variables.

The first row in Table 6 confirms that the benchmark announcements display significant pre-announcement drift, with the correct sign in both markets. This drift varies systematically with the release procedure. In particular, the coefficients on the PFEI indicator in row 2 are all significant and have the opposite sign of the benchmark announcements in row 1.[32] This indicates that announcements subject to the PFEI guidelines are less affected by pre-announcement drift.

The coefficients on the Pre-release indicator in the third row and on the Embargo-only indicator in the fourth row share the sign of the coefficient on the surprise itself, i.e., they are positive and statistically significant in the stock market and negative and statistically signif-

note markets are different from zero. The χ^2-statistic of this joint test is significant at 1% level for all variables for both markets.

[32] All PFEI announcements in our sample are pre-released, as shown in Table 5. The average effect of subjecting an announcement to PFEI standards is, therefore, the sum of the PFEI and the Pre-Release coefficients, which implies a dampening but no reversal of the pre-announcement drift.

icant in the bond market. This shows that pre-releasing announcements is associated with a stronger drift. This applies in particular to the least secure release procedure, "Embargo-only". However, caution needs to be exercised in interpreting these results. Fully examining the leakage explanation would require a thorough analysis of individual trader data that is available only to the futures exchanges and the Commodity Futures and Trading Commission (CFTC) that oversees the U.S. futures markets.

5.1.2 Proprietary Information

In addition to information leakage, private information can be created by market participants generating their own *proprietary* information by collecting data related to macroeconomic announcements. In the context of company earnings announcements, Kim and Verrecchia (1997) interpret this pre-announcement information as "private information gathered in anticipation of a public disclosure." If the private data acquisition merely attempts to anticipate the official announcement but does not generate any new information beyond it, then its effect on the informativeness of prices parallels the effect of leakage in Brunnermeier (2005).

Typically this proprietary information is never published and remains a noisy private signal of the official announcement. The nature of proprietary information usually makes it impossible for researchers to verify its existence.[33] However, proprietary data that is released to researchers or the public later provides an opportunity to explore the role of proprietary information in the pre-announcement price drift.

We were able to obtain three examples of such proprietary data collection: The State Street "PriceStats" automatically scrapes online prices to compute daily estimates of the U.S. inflation, the State Street Investor Confidence Index measures investor confidence based on buying and selling activity of institutional investors on a monthly basis, and the Case-Shiller Home Price Index by S&P Dow Jones provides monthly data on home prices. We test

[33] Examples of proprietary data collection could be exclusive subscriptions (for example, credit-card "SpendingPulse" data of MasterCard), tailor-made data from surveillance helicopters monitoring activity around industrial complexes (see, for example, Rothfeld and Patterson (2013)), or even the proprietary insights that trading platforms may gain from monitoring order flow.

whether information at its collection time (when it was still proprietary) is useful for forecasting macroeconomic announcement surprises by regressing the announcement surprise, S_{mt}, on the proprietary data. We pick the macroeconomic announcement most closely related to the proprietary data: CPI for the State Street PriceStats inflation indicator, CB Consumer Confidence Index for the State Street Investor Confidence Index, and housing sector announcements for the Case-Shiller Home Price Index. We find predictive power in the PriceStats inflation indicator but no predictive power in the State Street Investor Confidence Index and the Case-Shiller Home Price Index. This result may be due to the PriceStats data collection occurring daily which would allow traders with access to this information to trade more in real-time than monthly indicators. Although a comprehensive test of the effect of proprietary information is not feasible by construction, the results (in the Internet Appendix) for these three proprietary data sets raise the possibility that early access to proprietary information permits forecasting announcement surprises.

5.2 Public Information

In this section, we discuss the possibility that published market expectations are mismeasured and explore the possibility of a "bandwagon effect." We show that neither of these two explanations can convincingly explain the pre-announcement drift.

5.2.1 Individual Analyst Forecasts

The definition of a surprise in equation (1) involves market expectations, $E_{t-\tau}[A_{m,t}]$. Section 4 uses the Bloomberg consensus forecast. However, Bloomberg's way of calculating a consensus forecast as the median of individual forecasts is not innocuous. Individual forecasters might differ in their forecasting abilities and loss functions.[34] The forecasts of individual

[34] In such a situation, the median of individual forecasts may not be optimal. Nevertheless, such parameter-free approaches perform well in many situations due to the elimination of the estimation error on combination weights (Elliott & Timmermann, 2005).

analysts are available to Bloomberg subscribers.[35] If the announcement surprises are predictable with individual forecasts, but most traders rely on the consensus forecast, then traders with deeper insight obtained from individual forecasts could trade on this insight before the announcement, which would explain the drift.[36]

Bloomberg provides a rank for a subset of up to ten active professional forecasters who have issued accurate forecasts in previous months. We compute the median consensus for the ranked forecaster subset, $E_{t-\Delta}^{Ranked}[A_{mt}]$, using forecasts submitted no more than seven days before the release date to avoid stale forecasts.[37] We use this variable as a predictor of the actual announcement, A_{mt}. Our forecast of the surprise is the difference between the median values of the professional forecasters ranked by Bloomberg and all forecasters in the Bloomberg survey:

$$P_{mt} = E_{t-\tau}^{Ranked}[A_{mt}] - E_{t-\tau}[A_{mt}]. \tag{4}$$

To determine whether P_{mt} is a reasonable forecast of the unstandardized surprise, $\hat{S}_{mt}$,[38] we regress the unstandardized surprise, $\hat{S}_{mt}$, on a constant and the prediction, P_{mt}. Nine announcements out of the 20 market-moving announcements show significance of the slope coefficient at 10% level.[39]

[35]We build on previous research that uses individual forecasts. Energy markets react more to inventory forecasts by professional forecasters with a track record of higher forecasting accuracy (Chang, Daouk, & Wang, 2009; Gay, Simkins, & Turac, 2009). In forecasts of macroeconomic announcements, Brown, Gay, and Turac (2008) use individual forecasts to construct a forecast that improves on the Bloomberg consensus forecasts for 26 U.S. macro announcements. In contrast, Genre, Kenny, Meyler, and Timmermann (2013) caution that picking the best combination of forecasts in real time using the European Central Bank's Survey of Professional Forecasters data for GDP growth, inflation and unemployment is difficult because the results vary over time, across forecasting horizons and between target variables.

[36]Forecasting a nonlinear data generating process under an asymmetric loss function can give an optimal forecast with non-zero mean (Patton & Timmermann, 2007). Insights into the data generating process and the loss functions of individual analysts might allow predicting this bias. Some investment institutions indeed place considerable resources in building models of announcement surprises.

[37]Since some individual forecasters submit their forecasts days before the releases as described in Section 2 and Bloomberg equal-weights the forecasts, we also test whether more up-to-date forecasts are better predictors of the surprise and find that removing stale forecasts does not improve forecasts of the surprise.

[38]We use a forecast of the *unstandardized* surprise $\hat{S}_{mt} = A_{mt} - E_{t-\tau}[A_{mt}] = \sigma_m S_{mt}$ to avoid the estimation of additional parameters.

[39]These announcements are Advance Retail Sales, CB Consumer Confidence Index, CPI, Durable Goods Orders, Existing Home Sales, GDP Advance, Industrial Production, Pending Home Sales and PPI. Detailed results are reported in the Internet Appendix.

We compare the predictive accuracy of this surprise forecast with a white noise forecast under quadratic loss (Diebold & Mariano, 1995; Diebold, 2015). The forecast error in predicting the next surprise is $\hat{S}_{mt} - P_{mt}$ under (4), and $\hat{S}_{mt}$ under white noise. The test of the null hypothesis $H_0 : E\left(\hat{S}_{mt} - P_{mt}\right)^2 = E\left(\hat{S}_{mt}\right)^2$ against the one-sided alternative hypothesis $H_1 : E\left(\hat{S}_{mt} - P_{mt}\right)^2 < E\left(\hat{S}_{mt}\right)^2$ reveals that the improvement over the white noise forecast for five of the 20 market-moving announcements is significant at the 10% level. Only one of these announcements (Existing Home Sales) shows a drift in Table 2, whereas the other four (CPI, Durable Goods Orders, Industrial Production, and PPI) do not.[40]

Thus, while there is some limited forecastability of announcement surprises, it is unlikely that the weighting of individual analyst forecasts in the Bloomberg consensus and trading on refined forecasts generates the pre-announcement effect.[41]

5.2.2 Bandwagon Effect

A possibility arises that uninformed speculators manage to "jump on the bandwagon" with informed traders by observing the trading activity and returns before the announcement releases.[42] However, the markets that we examine are very liquid. The order imbalances before these announcements are sizable, but they represent only a small fraction of the overall trading activity. For example, the average trading volume in the 30-minute window before drift announcements is about 247,000 and 89,000 contracts in the E-mini S&P 500 and 10-year Treasury note futures, respectively. As discussed at the end of Section 4.1, such high trading activity likely allows informed traders to camouflage their information and trade profitably before announcement releases.

To replicate this strategy, we consider uninformed traders observing price movements at the beginning of the drift period and trading accordingly. For example, we analyze correlations of returns in the $[t - 30min, t - 15min]$ window with returns in the $[t - 15min, t - 5sec]$

[40]Appendix Table A1 shows the results.

[41]Recently, Zhou (2016) describes traders predicting announcements by other public information.

[42]For example, Brunnermeier (2005) shows that leakage makes prices before the news announcement more informative.

window. Such correlations are not significant, however, suggesting that simply observing price movements cannot be easily used for profitable trading ahead of announcement releases.

6 Conclusion

There is evidence of substantial pre-announcement informed trading in equity index and Treasury futures markets for nine out of 20 market-moving U.S. macroeconomic announcements during 2008–2014. About 30 minutes before the release time, prices begin to drift in the direction of the market's subsequent reaction to the news. This drift accounts for 49 percent and 36 percent of the overall price adjustments in the E-mini S&P 500 and 10-year Treasury note futures markets, respectively, and the estimated magnitude of profits of informed traders underscores the economic significance of these price moves. Therefore, failing to account for the pre-announcement effect substantially underestimates the total impact that these macroeconomic announcements have on financial markets.

We examine possible sources of informed trading. We focus on two features of the release process that may affect information leakage: organization type and release procedures. The results suggest that announcements from organizations that are not subject to the Principal Federal Economic Indicator guidelines and announcements released under less secure release procedures are associated with a stronger drift. Resource-intensive legwork creating original proprietary datasets that proxy the data underlying public announcements might also permit anticipating their values before their release. It is also possible that a combination of various factors causes the drift.

The definite source of the drift remains an open question. In view of the public interest in the safeguarding of macroeconomic data and considering the public and regulatory attention that leakage has received, for example, in the recent hacking scandal (SEC, 2015), the source of informed trading merits more research. Of particular interest is the effect of proprietary real-time data collection on announcement surprises and prices, and a comparison of pre-announcement effects across countries with different regulations and supervisory structures.

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A Appendix

A.1 Impact of Early Signals

A pre-announcement price drift of 50% of the total announcement impact shown for some macroeconomic announcements in Table 4 appears large at first sight. This appendix illustrates in a model of Bayesian learning that very little information is needed to generate a pre-announcement drift of such a large magnitude. The earlier information gets more attention than the later information and thus has a larger price impact even if the later information is "official" and more precise.

We consider an economy with one risky asset with payoff X, which could also be seen as the state of the economy. Traders have access to two sources of information. First, (select) traders observe a private signal A_1 about the state of the economy via leakage or own information collection at $t < 2$:

$$A_1 = X + \varepsilon_1.$$

The official announcement, which is released to the public at time $t = 2$, is

$$A_2 = X + \varepsilon_2.$$

Both private signal and official announcement are subject to normally distributed noise $\varepsilon_i \sim N\left(0, \frac{1}{\rho_{Ai}}\right)$ for $i = 1, 2$ where ρ_{Ai} denotes the precision of signal i. Investors form homogeneous expectations about X at each point in time. We denote by μ_{X0} the normally distributed prior market expectation of the state of the economy X at time $t = 0$ with precision ρ_{X0}.

Traders update their conditional expectations by Bayesian learning. Their first update before the official release time, immediately after observing the leaked or proprietary information, changes their expectation of X to

$$E[X|A_1] \equiv \mu_{X1} = \rho_{X1}^{-1}(\rho_{X0}\mu_{X0} + \rho_{A1}A_1) \tag{5}$$

with precision $\rho_{X1} = \rho_{A1} + \rho_{X0}$. After the official announcement release, they update their expectation again, now to

$$E[X|A_1, A_2] \equiv \mu_{X2} = \rho_{X2}^{-1}(\rho_{X1}\mu_{X1} + \rho_{A2}A_2) \tag{6}$$

with precision $\rho_{X2} = \rho_{A2} + \rho_{X1}$.

We assume that traders choose their asset holdings D to maximize their expected CARA utility of next period's wealth

$$E\left[U\left(W\right)\right] = E\left[-exp\left(-DX\right)\right],$$

which generates a linear demand function. Under an exogenous, zero mean, and normally distributed supply of the risky asset, using the conditional expectations (5) and (6), market clearing implies that the price change equals the conditional expected net payoff in the respective period. In the pre-announcement period, the price changes by

$$p_1 - p_0 = \frac{\rho_{A1}}{\rho_{X1}}(A_1 - \mu_{X0}).$$

At the official release time, the price changes again, now by

$$p_2 - p_1 = \frac{\rho_{A2}}{\rho_{X2}}(A_2 - \mu_{X1}).$$

For concise notation, we write for each surprise $S_i \equiv A_i - \mu_{Xi-1}$. The following proposition provides a condition for the price change in the pre-release period exceeding the price change at the official release time.

Proposition (Impact of Early News)

$$p_1 - p_0 > p_2 - p_1 \;\Leftrightarrow\; \frac{\rho_{A1}}{\rho_{A2}} + \frac{\rho_{A1}}{\rho_{X0} + \rho_{A1}} > \frac{S_2}{S_1} \tag{7}$$

Proof:

$$\begin{aligned} p_1 - p_0 &> p_2 - p_1 \\ \Leftrightarrow \frac{\rho_{A1}}{\rho_{X1}} S_1 &> \frac{\rho_{A2}}{\rho_{X2}} S_2 \\ \Leftrightarrow \frac{(\rho_{A2} + \rho_{A1} + \rho_{X0})\rho_{A1}}{(\rho_{A1} + \rho_{X0})\rho_{A2}} &> \frac{S_2}{S_1} \\ \Leftrightarrow \frac{\rho_{A1}}{\rho_{A2}} + \frac{\rho_{A1}}{\rho_{A1} + \rho_{X0}} &> \frac{S_2}{S_1} \end{aligned}$$

q.e.d.

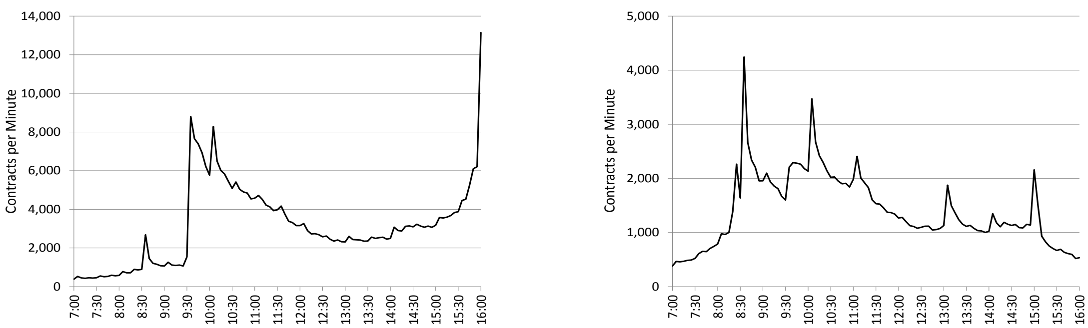
The proposition shows that even vague proprietary information can have a large price impact. To see this in a specific example, suppose that there is no prior public information ($\rho_{X0} \to 0$), and that the pre-release information is less precise and less surprising than the official release later on ($\rho_{A2} = 2\rho_{A1}$, $S_2 = 1.5 S_1$). Substituting into condition (7), we find that the pre-release price change is equal to the price impact at the official release time. Therefore, even a modest amount of private information suffices to explain a price drift amounting to 50% of the total price adjustment. In our example, pre-release information with only one half of the precision and with only two thirds of the surprise suffices. The reason for the amplified impact of the private information is its early availability.

A.2 Additional Figures and Tables

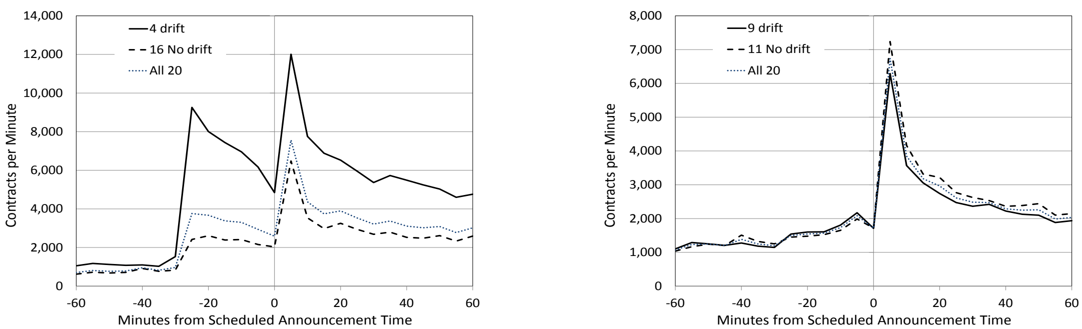
Figure A1: Trading Volumes

E-mini S&P 500 | **10-year Treasury Note**

(a) Calendar time



(b) Event time



The sample period is from January 1, 2008 through March 31, 2014. The figure shows the average trading volume in the number of contracts per minute. The top panel shows the average trading volume in calendar time. The bottom panel shows the average trading volume in event time for each of the "drift" and "no drift" categories based on Tables 2 and 3.

Table A1: Results of Forecasting the Announcement Surprise Using Individual Forecasts

	DM	p-value
ADP employment	-1.06	0.86
Advance retail sales	0.69	0.25
CB Consumer confidence index	1.01	0.16
Construction spending	-4.42	1.00
Consumer price index	2.81	0.00
Durable goods orders	2.56	0.01
Existing home sales	1.32	0.09
GDP advance	1.00	0.16
GDP final	-3.20	1.00
GDP preliminary	-0.75	0.77
Housing starts	-0.83	0.80
Industrial production	1.81	0.04
Initial jobless claims	-0.41	0.66
ISM Manufacturing index	0.71	0.24
ISM Non-manufacturing index	-0.70	0.76
New home sales	-0.51	0.69
Non-farm employment	-1.61	0.95
Pending home sales	0.68	0.25
Producer price index	1.77	0.04
UM Consumer sentiment (prel.)	0.37	0.36

The sample period is from January 1, 2008 through March 31, 2014. The Diebold-Mariano test statistic in column DM is computed for the prediction, P_{mt}, of the unstandardized surprise, $\hat{S}_{mt}$, based on the consensus of the ranked professional forecasters against a zero surprise benchmark. A large value means rejection of the null hypothesis, $H_0 : E\left(\hat{S}_{mt} - P_{mt}\right)^2 = E\left(\hat{S}_{mt}\right)^2$, in favor of an alternative hypothesis of an improved prediction using the consensus of the ranked professional forecasters, $H_1 : E\left(\hat{S}_{mt} - P_{mt}\right)^2 < E\left(\hat{S}_{mt}\right)^2$.

Price Drift before U.S. Macroeconomic News: Private Information about Public Announcements? INTERNET APPENDIX*

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1 Overview

This Internet Appendix presents additional details and robustness checks for the "Price Drift before U.S. Macroeconomic News: Private Information about Public Announcements?" paper. Section 2 shows summary statistics for the announcements listed in Table 1 in the paper. Section 3 provides additional detail for Figure 1 in the paper by showing cumulative average returns for individual announcements. Section 4 compared cumulative average returns in the expanded $[t-180min, t+60min]$ window to the $[t-60min, t+60min]$ window reported in the paper. Section 5 checks the robustness of testing multiple hypotheses using the Holm (1979) step-down procedure. Section 6 analyzes the pre-announcement drift conditional on the sign of the post-announcement return. Complementing the time-series methodology followed in the paper, Section 7 repeats the analysis based on event study methodology including robustness checks for outliers, event window length, effect of order flows, and other markets (E-mini Dow futures and 30-year Treasury bond futures). Section 8 provides additional information on forecasting the announcement surprise using proprietary data sets. Section 9 provides additional information on forecasting the announcement surprise using individual analyst forecasts.

2 Summary Statistics for Announcements Data

Table B1 shows summary statistics for the 30 announcements listed in Table 1 in the paper.

3 Cumulative Average Returns for Individual Announcements

Figure 1 in the paper presents cumulative average returns (CARs) averaged across announcements. Here, in Figure B1 we present CARs for the individual announcements that exhibit drift per Table 2 in the paper (four in the E-mini S&P 500 market and nine in the 10-year Treasury note market).

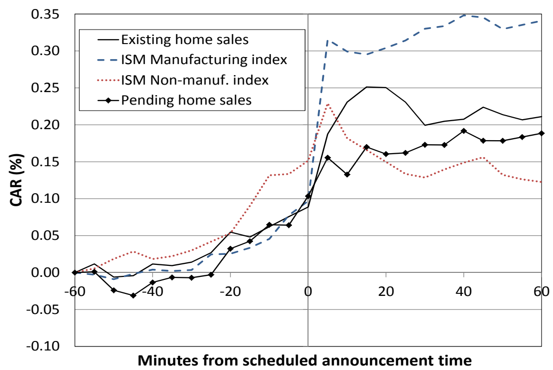
Table B1: Macroeconomic Announcements - Summary Statistics

Announcement	Unit	Mean	Median	Min	Max	Std
GDP advance	%	1.44	2	-6.1	5.7	2.39
GDP preliminary	%	1.40	2	-6.2	5.9	2.62
GDP final	%	1.40	1.9	-6.3	5.6	2.58
Personal income	%	0.25	0.3	-3.6	2.6	0.66
ADP employment	Number of jobs (1,000)	13.39	93	-742	325	240.56
Initial jobless claims	Number of claims (1,000)	421.95	399.5	292	669	85.16
Non-farm employment	Number of jobs (1,000)	-4.97	69	-663	431	234.83
Factory orders	%	0.09	0.6	-5.2	4.8	1.99
Industrial production	%	0.07	0.1	-2.8	1.3	0.76
Construction spending	%	-0.13	0	-3.3	2.7	1.01
Durable goods orders	%	-0.04	0.05	-13.2	9.9	3.60
Wholesale inventories	%	0.32	0.5	-1.7	1.9	0.86
Advance retail sales	%	0.18	0.3	-2.8	2.7	0.86
Consumer credit	USD (Billion)	4.99	6.35	-21.6	21.36	10.50
Personal consumption	%	0.26	0.3	-1	1.3	0.36
Building permits	Number of permits (1,000)	737.12	680	494	1091	179.20
Existing home sales	Number of homes (Million, Annual rate)	4.93	4.91	3.83	6.54	0.41
Housing starts	Number of homes (1,000)	720.73	658	458	1066	175.90
New home sales	Number of homes (1,000)	383.15	368	250	604	83.09
Pending home sales	%	0.37	0.25	-30	10.4	6.08
Government budget	USD (Billion)	-88.29	-94.3	-237.2	159.3	89.95
Trade balance	USD (Billion)	-44.04	-42.9	-63.1	-26	9.02
Consumer price index	%	0.14	0.1	-1.7	1.1	0.39
Producer price index	%	0.20	0.2	-2.8	1.8	0.88
CB Consumer confidence index	Index	59.60	59.6	25	87.9	13.37
Index of leading indicators	%	0.31	0.3	-0.8	1.4	0.45
ISM Manufacturing index	Index	51.56	52.5	32.4	61.4	6.30
ISM Non-manufacturing index	Index	51.96	53	37.3	59.7	4.53
UM Consumer sentiment - Final	Index	71.23	72.5	55.3	85.1	7.67
UM Consumer sentiment - Prel	Index	70.39	71.8	54.9	84.9	7.73

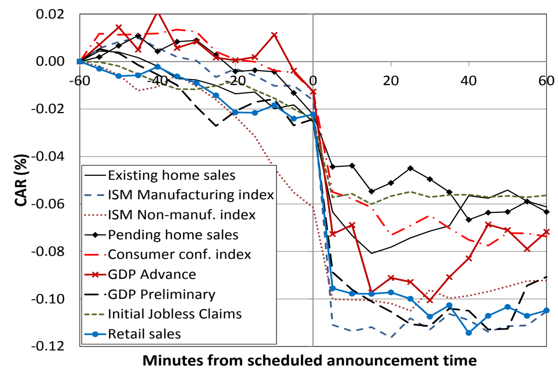
The sample period covers January 1, 2008 to March 31, 2014. The columns show the mean, median, minimum, maximum and standard deviation values for each announcement listed in Table 1 in the paper.

Figure B1: Cumulative Average Returns for Individual Announcements

E-mini S&P 500



10-year Treasury Note



The sample period is from January 1, 2008 through March 31, 2014. We classify each event as "good" or "bad" news based on whether the announcement surprise has a positive or negative effect on the stock and bond markets using the coefficients in Table 3 in the paper. Cumulative average returns (CARs) are then calculated in the $[t-60min, t+60min]$ window. Only announcements showing evidence of pre-announcement drift in each market in Table 2 in the paper are included (four in the E-mini S&P 500 market and nine in the 10-year Treasury note market).

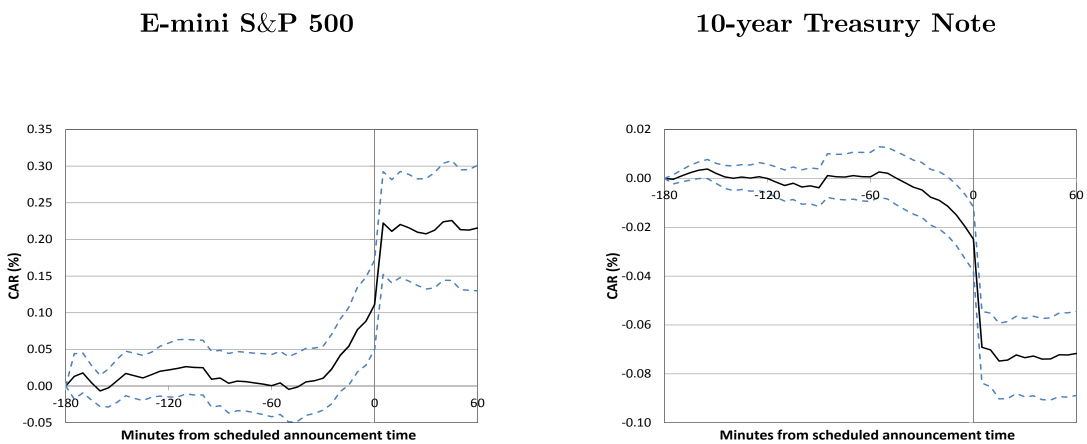
4 Cumulative Average Returns for $[t-180min, t+60min]$ Window

Figure 1 in the paper presents CARs for the $[t-60min, t+60min]$ window. Figure B2 presents CARs in the expanded $[t-180min, t+60min]$ window. The CARs during the $[t-180min, t-60min]$ window hover around zero similarly to the $[t-60min, t-30min]$ window.

5 Robustness Check: Multiple Hypotheses Testing and Data Snooping

Table 2 in Section 4.1 in the paper presents results showing the pre-announcement price drift. In that table, we test multiple hypotheses. Increasing the number of hypotheses leads to the rejection of an increasing number of hypotheses with probability one, irrespective of the sample size. Failure to adjust the p-values can be viewed as data snooping. To rule out this possibility, we use the Holm (1979) step-down procedure. This procedure adjusts the

Figure B2: Cumulative Average Returns for $[t - 180min, t + 60min]$ Window



The sample period is from January 1, 2008 through March 31, 2014. We classify each event as "good" or "bad" news based on whether the announcement surprise has a positive or negative effect on the stock and bond markets using the coefficients in Table 3 in the paper. Following Bernile, Hu, and Tang (2016), we invert the sign of returns for negative surprises. Cumulative average returns (CARs) are then calculated in the $[t - 180min, t + 60min]$ window for the "drift" category based on Table 2 in the paper. In the stock market, there are four drift announcements. In the bond market, there are nine drift announcements. The solid line shows the mean CAR. Dashed lines mark two-standard-error bands (standard error of the mean).

hypothesis rejection criteria to control the probability of encountering one or more type I errors, the familywise error rate (see, for example, Romano and Wolf (2005)). Denote the hypotheses by $H_1, ..., H_M$, one for each of the $M = 30$ announcements in Table 2. Denote the corresponding p-values by $p_1, ..., p_M$. Consider the significance level of 0.05. The procedure orders the Table 2 joint test p-values from the lowest to the highest. Denoting the ordered hypotheses by $k = 1 \dots 30$, it computes $\frac{0.05}{M+1-k}$ for each k and compares this computed value to the Table 2 p-value. The null hypothesis of no drift is rejected if $\frac{0.05}{M+1-k}$ exceeds the p-value in Table 2. Based on this *conservative* approach, four announcements ranked at the top of Table 2 (ISM Manufacturing, Pending Home Sales, ISM Non-Manufacturing and CB Consumer Confidence Index) show a statistically significant drift.

6 Robustness Check: Conditioning on Sign of Post-Announcement Return

The results in Section 4 in the paper show that the pre-announcement drift is in the direction of the *surprise*. In this section, we focus instead on returns and show that the pre-announcement drift exists also conditional on the sign of the post-announcement *return*.

Table B2: Holm's Step-down Procedure

Announcement	Table 2 Joint Test p-value	$\frac{0.05}{M+1-k}$	Null Hypothesis of No Drift Rejected
ISM Non-manufacturing index	8.033E-11	0.0017	Yes
Pending home sales	7.560E-08	0.0017	Yes
ISM Manufacturing index	0.150E-05	0.0018	Yes
CB Consumer confidence index	0.109E-04	0.0019	Yes
Existing home sales	0.012	0.0019	No
Advance retail sales	0.016	0.0020	No
GDP preliminary	0.018	0.0021	No
Initial jobless claims	0.020	0.0022	No
GDP advance	0.049	0.0023	No
Factory orders	0.060	0.0024	No
Industrial production	0.203	0.0025	No
Trade balance	0.219	0.0026	No
Construction spending	0.226	0.0028	No
Consumer credit	0.238	0.0029	No
Building permits	0.244	0.0031	No
Personal income	0.296	0.0033	No
Government budget	0.333	0.0036	No
Personal consumption	0.433	0.0038	No
New home sales	0.456	0.0042	No
Wholesale inventories	0.539	0.0045	No
Durable goods orders	0.644	0.0050	No
Consumer price index	0.648	0.0056	No
UM Consumer sentim. - Prel	0.671	0.0063	No
Index of leading indicators	0.678	0.0071	No
Non-farm employment	0.686	0.0083	No
Housing starts	0.704	0.0100	No
Producer price index	0.858	0.0125	No
ADP employment	0.859	0.0167	No
UM Consumer sentim. - Final	0.895	0.0250	No
GDP final	0.978	0.0500	No

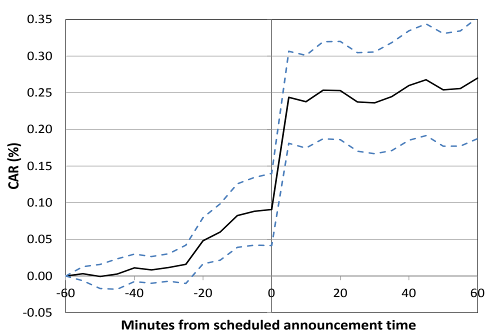
The sample period is from January 1, 2008 through March 31, 2014. All 30 announcements are included.

For announcements showing drift in Table 2 in the paper, the returns in the $[-30min, -5sec]$ window are strongly correlated with the returns in the $[-5sec, +1min]$ window. The correlation of returns in these two windows is highly significant with values of 0.19 and 0.15 in the stock and bond markets, respectively. In contrast, for no-drift announcements this correlation is not significant with values of -0.01 and -0.02 in the stock and bond markets, respectively.

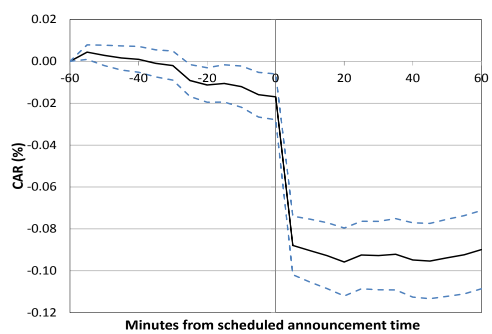
We show CARs conditioned on the sign of the returns in the $[-5sec, 1min]$ window in Figure B3 following Ederington and Lee (1995). The CARs suggest that the pre-announcement drift is in the direction of the post-announcement price move.[1]

Figure B3: Cumulative Average Returns Conditional on Sign of Return in $[-5sec, 1min]$ Window for Drift Announcements

E-mini S&P 500



10-year Treasury Note



The sample period is from January 1, 2008 through March 31, 2014. Similarly to Ederington and Lee (1995), if the return in the $[-5sec, +1min]$ window in the stock market is negative, we multiply the returns by -1. In the bond market, if the return in the $[-5sec, +1min]$ window is positive, we multiply the returns by -1. Cumulative average returns (CARs) are then calculated in the $[t-60min, t+60min]$ window for each of the drift announcements per Table 2 in the paper. We omit the weekly Initial Claims announcement to avoid this announcement disproportionately affecting the results comprised of monthly and quarterly announcements. The solid line shows the mean CAR. Dashed lines mark two-standard-error bands (standard error of the mean).

7 Robustness Check: Event Study Methodology

Complementing the time-series methodology used in the paper, we repeat the analysis here based on event study methodology. We start with an OLS regression, followed by outlier robustness checks, then present cumulative average return graphs and perform additional robustness checks with event window length, the effect of order flows, and other markets.

[1] As we would expect, the *magnitude* of the pre-announcement price move as a proportion of the total price move is slightly lower in Figure B3 (about a third) compared to Figure 1 in the paper (about a half) because returns are not predictable. Therefore, even an informed trader that perfectly forecasts the announcement surprises and enters a position based on this information before the announcement release may experience the market move against this position due to reasons unrelated to the announcement.

7.1 OLS Regression

Let $R_{t-\underline{\tau}}^{t+\overline{\tau}}$ denote the continuously compounded asset return around the official release time t of announcement m, defined as the first difference between the log prices at the beginning and at the end of the intraday event window $[t-\underline{\tau}, t+\overline{\tau}]$. Let S_{mt} denote the unexpected component of news announcements ("the surprise") as in the paper. The effect of news announcements on asset prices can then be analyzed by standard event study methodology (Balduzzi, Elton, & Green, 2001). The reaction of asset returns to the surprise is captured by the ordinary least squares regression

$$R_{t-\underline{\tau}}^{t+\overline{\tau}} = \gamma_0 + \gamma_m S_{mt} + \varepsilon_t, \tag{1}$$

where γ_0 captures the unconditional return around the release time (Lucca & Moench, 2015), and ε_t is an i.i.d. error term reflecting price movements unrelated to the announcements.

As in the paper, the standardized surprise, S_{mt}, is based on the difference between the actual announcement, A_{mt}, released at time t and the market's expectation of the announcement before its release, $E_{t-\underline{\tau}}[A_{mt}]$, proxied by the median response of professional forecasters during the days before the release, $E_{t-\Delta}[A_{mt}]$.[2] As in the paper, we standardize the difference by the standard deviation of the respective announcement, σ_m, to convert them to equal units. Specifically,

$$S_{mt} = \frac{A_{mt} - E_{t-\underline{\tau}}[A_{mt}]}{\sigma_m}. \tag{2}$$

To isolate the pre-announcement effect from the post-announcement effect, we first identify market-moving announcements among our set of 30 macroeconomic announcements. We estimate equation (1) with an event window spanning from $\underline{\tau} = -5$ seconds before the official release time to $\overline{\tau} = 5$ minutes after the official release time. Analogously, the dependent variable $R_{t-\underline{\tau}}^{t+\overline{\tau}}$ is the continuously compounded futures return over the $[t-5sec, t+5min]$ window.

Table B3 shows that there are 21 market-moving announcements based on the p-values from the joint test of both stock and bond markets using a 5% significance level. The coefficients have the expected signs: Good economic news (for example, higher than anticipated GDP) boosts stock prices and lowers bond prices. Specifically, a one standard deviation positive surprise in the GDP Advance announcement increases the E-mini S&P 500 futures price by 0.171 percent, and its surprises explain 22 percent of the price variation within the announcement window. Our subsequent analysis is based on these 21 market-moving

[2] We also estimate equation (1) including the market's expectation of the announcement, $E_{t-\Delta}[A_{mt}]$, on the right-hand side. The coefficients are not significant suggesting that markets indeed do not react to the *expected* component of news announcements.

announcements.

Table B3: Announcement Surprise Impact During $[t-5sec, t+5min]$ Using Event Study Methodology

Announcement	E-mini S&P 500 Futures γ_m	R^2	10-year Treasury Note Futures γ_m	R^2	Joint Test p-value
GDP advance	0.171 (0.052)***	0.22	-0.028 (0.026)	0.04	0.002
GDP preliminary	0.113 (0.051)**	0.15	-0.056 (0.015)***	0.25	<0.001
GDP final	0.053 (0.039)	0.06	-0.042 (0.018) **	0.17	0.025
Personal income	0.020 (0.012)	0.01	0.000 (0.012)	0.00	0.253
ADP employment	0.178 (0.023)***	0.59	-0.093 (0.017)***	0.49	<0.001
Initial jobless claims	-0.115 (0.013)***	0.23	0.043 (0.006)***	0.19	<0.001
Non-farm employment	0.420 (0.046)***	0.50	-0.261 (0.043)***	0.43	<0.001
Factory orders	0.035 (0.026)	0.04	-0.017 (0.009)*	0.07	0.060
Industrial production	0.043 (0.013)***	0.17	-0.008 (0.004)*	0.04	0.001
Construction spending	-0.005 (0.039)	0.00	0.007 (0.013)	0.00	0.863
Durable goods orders	0.096 (0.020)***	0.23	-0.045 (0.012)***	0.20	<0.001
Wholesale inventories	-0.033 (0.021)	0.04	0.005 (0.007)	0.01	0.239
Advance retail sales	0.161 (0.024)***	0.42	-0.073 (0.015)***	0.27	<0.001
Consumer credit	0.036 (0.015)**	0.07	-0.004 (0.003)	0.03	0.019
Personal consumption	0.007 (0.014)	0.00	-0.015 (0.008)*	0.02	0.147
Building permits	0.045 (0.022)**	0.06	-0.020 (0.013)	0.04	0.037
Existing home sales	0.120 (0.030)***	0.20	-0.038 (0.010)***	0.17	<0.001
Housing starts	0.050 (0.024)**	0.08	-0.039 (0.015)***	0.17	0.003
New home sales	0.122 (0.026)***	0.25	-0.044 (0.006)***	0.39	0.001
Pending home sales	0.087 (0.032)***	0.11	-0.032 (0.008)***	0.18	<0.001
Government budget	0.013 (0.013)	0.02	0.001 (0.007)	0.00	0.612
Trade balance	0.024 (0.016)	0.01	-0.003 (0.007)	0.00	0.280
Consumer price index	-0.111 (0.041)***	0.15	-0.030 (0.013)**	0.06	0.002
Producer price index	0.013 (0.033)	0.00	-0.023 (0.011)**	0.06	0.124
CB Consumer confidence index	0.196 (0.029)***	0.47	-0.051 (0.008)***	0.41	<0.001
Index of leading indicators	0.058 (0.027)**	0.05	-0.009 (0.008)	0.01	0.058
ISM Manufacturing index	0.240 (0.034)***	0.46	-0.111 (0.014)***	0.50	<0.001
ISM Non-manufacturing index	0.064 (0.037)*	0.07	-0.041 (0.009)***	0.25	<0.001
UM Consumer sentim. - Final	0.046 (0.020)**	0.06	-0.014 (0.006)**	0.07	0.005
UM Consumer sentim. - Prel	0.071 (0.025)***	0.10	-0.017 (0.007)**	0.08	0.001

The sample period is from January 1, 2008 through March 31, 2014. The reported response coefficients γ_m are the ordinary least squares estimates of equation (1) with the White (1980) heteroskedasticity consistent covariance matrix. Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5% and 1% levels, respectively. The p-values are for the joint Wald test that the coefficients of announcement surprises for the E-mini S&P 500 and 10-year Treasury note futures are equal to zero. The intercept, γ_0, is significant only for the Pending Home Sales announcement in the stock and bond markets.

Next, we re-estimate equation (1) for the 21 market-moving announcements identified in Table B3 using the pre-announcement window $[t-30min, t-5sec]$. Accordingly, we now use the continuously compounded futures return over the $[t - 30min, t - 5sec]$ window.[3]

Table B4 shows the results sorted by the p-values of the joint test for stock and bond markets. There are seven announcements significant at 5% level.[4] Most of these announcements show evidence of significant drift in both markets. A joint test of the 21 hypotheses overwhelmingly confirms the overall statistical significance of the pre-announcement price drift.[5] In all seven announcements, the drift is in the "correct" direction, i.e., direction of the price change predicted by the announcement surprise.

Although there are some differences in the results using the above event study methodology compared to the results using the time-series methodology in Section 4 in the paper, overall the event study methodology results confirm the time-series methodology results: A substantial number of announcements exhibits substantial pre-announcement drift.

7.2 Outliers

Since our sample period includes the turbulent financial crisis, a possibility arises that our results are driven by a few unusual, large observations. We verify that this is not the case. We conduct two robustness checks. First, we re-estimate equation (1) with the robust procedure of Yohai (1987). Second, we split surprises by size into deciles and estimate equation (1) using the pre-announcement $[t - 30min, t - 5sec]$ window for each decile.

7.2.1 Yohai (1987) Procedure

We re-estimate equation (1) with the robust procedure of Yohai (1987). This so-called MM-estimator is a weighted least squares estimator that is not only robust to outliers but also refines the first-step robust estimate in a second step towards higher efficiency. Table B5 shows that all seven announcements significant in Table B4 remain significant. We label them as "strong drift" announcements. Ten announcements do not display significant drift either in the robust regression or in the Table B4 joint test. We label them as "no drift"

[3] At first sight, this "two-step" procedure could be subject to a sample selection bias. The bias would be present if selection of market-moving announcements based on the estimated surprise regression coefficient using the post-announcement $[t-5sec, t+5min]$ window is correlated with the surprise regression coefficient using the pre-announcement $[t - 30min, t - 5sec]$ window. However, if this were the case, the error terms in the pre- and post-announcement regressions would have to be (conditionally) correlated. This would violate market efficiency, and it would be evidence of a significant pre-announcement drift.

[4] As a robustness check, we estimate the model using seemingly unrelated regressions to allow for the covariance between parameters γ_m in the stock and bond markets to be used in the joint Wald tests. The results confirm those reported in Table B4.

[5] Assuming the t-statistics in Table B4 are independent and standard normal, squaring and summing them gives a χ^2-statistic with 21 degrees of freedom. The computed values of this statistic for the E-mini S&P 500 and 10-year Treasury note futures are 63.5 and 79.1, respectively. This translates into statistical significance of the pre-announcement drift at the 1% level.

Table B4: Announcement Surprise Impact During $[t-30min, t-5sec]$ Using Event Study Methodology

Announcement	E-mini S&P 500 Futures γ_m	R^2	10-year Treasury Note Futures γ_m	R^2	Joint Test p-value
ISM Non-manufacturing index	0.139 (0.030)***	0.19	-0.058 (0.011)***	0.30	<0.0001
Pending home sales	0.154 (0.083)*	0.09	-0.035 (0.010)***	0.16	0.001
ISM Manufacturing index	0.091 (0.036)**	0.06	-0.027 (0.009)***	0.09	0.001
Existing home sales	0.113 (0.040)***	0.10	-0.019 (0.009)**	0.04	0.002
CB Consumer confidence index	0.035 (0.052)	0.01	-0.031 (0.010)***	0.12	0.007
Industrial production	0.066 (0.023)***	0.15	-0.007 (0.008)	0.01	0.013
GDP preliminary	0.146 (0.068)**	0.15	-0.022 (0.011)*	0.08	0.013
Housing starts	0.000 (0.021)	0.00	-0.020 (0.010)**	0.05	0.112
Non-farm employment	0.040 (0.021)*	0.07	-0.009 (0.010)	0.01	0.123
Advance retail sales	0.009 (0.029)	0.00	-0.020 (0.011)*	0.06	0.190
Consumer credit	-0.072 (0.051)	0.03	0.007 (0.009)	0.01	0.271
ADP employment	0.035 (0.027)	0.03	-0.006 (0.007)	0.01	0.291
UM Consumer sentiment - Final	-0.055 (0.042)	0.04	-0.007 (0.014)	0.00	0.361
Initial jobless claims	-0.009 (0.012)	0.00	0.007 (0.006)	0.01	0.369
New home sales	0.030 (0.033)	0.01	-0.005 (0.009)	0.01	0.539
Building permits	-0.023 (0.025)	0.02	-0.007 (0.012)	0.01	0.567
GDP advance	0.024 (0.044)	0.01	-0.023 (0.027)	0.06	0.608
GDP final	0.005 (0.022)	0.00	0.008 (0.011)	0.01	0.739
UM Consumer sentiment - Prel	-0.023 (0.055)	0.00	-0.005 (0.012)	0.00	0.845
Durable goods orders	-0.004 (0.016)	0.00	-0.003 (0.007)	0.00	0.852
Consumer price index	-0.005 (0.035)	0.00	-0.001 (0.011)	0.00	0.981

The sample period is from January 1, 2008 through March 31, 2014. Only the announcements with a significant effect on the E-mini S&P 500 and 10-year Treasury note futures prices (based on the joint test in Table B3) are included. The reported response coefficients γ_m are the ordinary least squares estimates of equation (1) with the White (1980) heteroskedasticity consistent covariance matrix. Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5% and 1% levels, respectively. The p-values are for the joint Wald test that the coefficients of announcement surprises for the E-mini S&P 500 and 10-year Treasury note futures are equal to zero. The intercept, γ_0, is significant only for the Initial Claims announcement in the stock market, CPI announcement in the bond market, and Non-Farm Employment announcement in both markets.

announcements.[6] Four announcements are not significant in the joint test of Table B4 but show significant coefficients in the robust regression using 5% significance level (mainly in the bond market). We label them as "some drift" announcements. Overall, the Yohai (1987) outlier-robust procedure confirms results from the OLS regression in Section 7.1.

Similarly to the paper, we quantify the magnitude of the pre-announcement price drift. We divide the γ_m coefficients from Table B4 by the corresponding sum of coefficients from Ta-

[6]We include the Building Permits announcement among the ten announcements that do not move markets because this announcement is not significant in Table B4 and shows a drift in the "incorrect" direction in Table B5.

Table B5: Announcement Surprise Impact During $[t - 30min, t - 5sec]$ Using Event Study Methodology and Robust Regression

Announcement	E-mini S&P 500 γ_m	R^2	10-year Treasury Note γ_m	R^2
Strong Evidence of Pre-Announcement Drift				
CB Consumer confidence index	0.023 (0.035)	0.01	-0.036 (0.009)***	0.14
Existing home sales	0.091 (0.034)***	0.02	-0.016 (0.007)**	0.05
GDP preliminary	0.063 (0.034)*	0.06	-0.026 (0.013)**	0.16
Industrial production	0.077 (0.016)***	0.10	-0.007 (0.001)	0.01
ISM Manufacturing index	0.076 (0.034)**	0.03	-0.025 (0.009)***	0.09
ISM Non-manufacturing index	0.139 (0.033)***	0.12	-0.042 (0.009)***	0.15
Pending home sales	0.087 (0.031)***	0.09	-0.028 (0.007)***	0.16
Some Evidence of Pre-Announcement Drift				
Advance retail sales	0.028 (0.016)*	0.01	-0.021 (0.009)**	0.07
Consumer price index	-0.051 (0.013)***	0.08	0.001 (0.009)	0.00
GDP advance	0.035 (0.032)	0.05	-0.067 (0.015)***	0.16
Initial jobless claims	-0.009 (0.007)	0.00	0.013 (0.005)***	0.01
No Evidence of Pre-Announcement Drift				
ADP employment	0.008 (0.014)	0.01	-0.006 (0.008)	0.01
Building permits	-0.036 (0.016)**	0.05	0.005 (0.009)	0.00
Consumer credit	-0.043 (0.028)	0.02	0.004 (0.007)	0.00
Durable goods orders	0.005 (0.015)	0.00	-0.007 (0.006)	0.01
GDP final	0.005 (0.025)	0.00	0.010 (0.013)	0.00
Housing starts	-0.006 (0.016)	0.00	-0.016 (0.009)*	0.02
New home sales	0.021 (0.031)	0.01	-0.005 (0.008)	0.00
Non-farm employment	0.018 (0.016)	0.00	0.000 (0.009)	0.00
UM Consumer sentiment - Final	-0.019 (0.031)	0.00	0.003 (0.011)	0.00
UM Consumer sentiment - Prel	0.003 (0.035)	0.00	-0.009 (0.009)	0.00

The sample period is from January 1, 2008 through March 31, 2014. Only the announcements that have a significant effect on the E-mini S&P 500 and 10-year Treasury note futures prices (based on the joint test in Table B3) are included. The reported response coefficients γ_m of equation (1) are estimated using the MM weighted least squares (Yohai, 1987). Standard errors are shown in parentheses. *, ** and *** indicate statistical significance at 10%, 5% and 1% levels, respectively. Classification as "strong drift", "some drift" and "no drift" uses combined results from Tables B4 and B5. "Strong drift" announcements show significance at 5% level in Table B4 joint test and at least one market in Table B5. "No drift" announcements are not significant in either Table B4 or B5 at 5% level. "Some drift" announcements are not significant in Table B4 joint test but show significance in Table B5 in at least one market at 5% level.

bles B3 and Table B4, i.e., $\Gamma_m = \gamma_m^{\overline{\tau}=-5sec}/(\gamma_m^{\overline{\tau}=-5sec} + \gamma_m^{\overline{\tau}=+5min})$. Table B6 shows these ratios sorted by the proportion obtained for the stock market. The ratio Γ_m ranges from 15 percent in the CB Consumer Confidence Index up to 69 percent in the ISM Non-Manufacturing Index indicating that the pre-announcement price move is a substantial proportion of the total price move. The mean ratio across all seven announcements and both markets is 44 percent.

Table B6: Pre-announcement Price Drift as a Proportion of Total Price Change Using Event Study Methodology

	E-mini S&P 500			10-year Treasury Note		
	γ_m $[t-5sec, t+5min]$	γ_m $[t-30min, t-5sec]$	Γ_m	γ_m $[t-5sec, t+5min]$	γ_m $[t-30min, t-5sec]$	Γ_m
ISM Non-manufacturing index	0.064	0.139	69%	-0.041	-0.058	59%
Pending home sales	0.087	0.154	64%	-0.032	-0.035	52%
Industrial production	0.043	0.066	60%	-0.008	-0.007	46%
GDP preliminary	0.113	0.146	56%	-0.056	-0.022	28%
Existing home sales	0.120	0.113	49%	-0.038	-0.019	34%
ISM Manufacturing index	0.240	0.091	28%	-0.111	-0.027	20%
CB Consumer confidence index	0.196	0.035	15%	-0.051	-0.031	37%
Mean			49%			39%

The sample period is from January 1, 2008 through March 31, 2014. Only the announcements classified as having strong evidence of pre-announcement drift in Table B5 are included.

7.2.2 Decile Analysis

We split surprises by size into deciles and estimate equation (1) using the pre-announcement $[t-30min, t-5sec]$ window for each decile. In these estimations, we pool together all seven announcements exhibiting strong drift in Table B5.[7] Since our sample includes positive and negative surprises, deciles 1 and 10 correspond to the largest surprises in absolute value, and deciles 5 and 6 correspond to the smallest surprises in absolute value. Table B7 shows that all deciles except for 5 and 6 in the stock market and 3 and 8 in the stock and bond market exhibit a significant drift. These results, therefore, again confirm that the results in Section 7.1 using the OLS regression are not driven by a few unusual, large observations.

7.3 Cumulative Average Returns

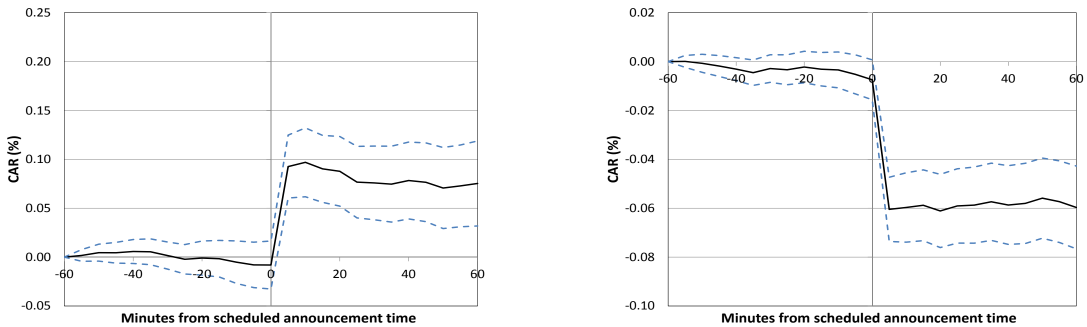
This section illustrates our findings from the above Sections 7.1 and 7.2 graphically using cumulative average return (CAR) graphs. As in the paper, we classify each event as "good" or "bad" news based on whether the surprise has a positive or negative effect on the stock and bond markets using the coefficients in Table B3. Following Bernile et al. (2016), we invert the sign of returns for negative surprises. CARs are then calculated in the $[t-60min, t+60min]$ window for each of the "strong drift", "some drift" and "no drift" categories defined in Table B5. The CARs in Figure B4 reveal what happens around the announcements.

[7] This approach assumes the same coefficients for all announcements, but it provides a larger sample size.

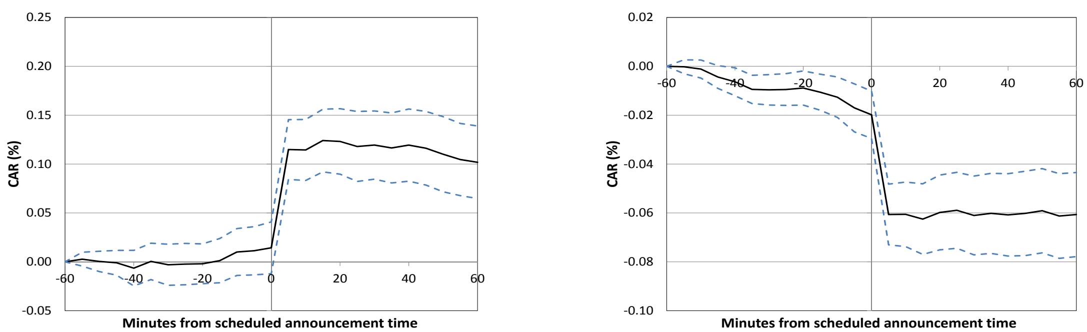
Figure B4: Cumulative Average Returns

E-mini S&P 500 **10-year Treasury Note**

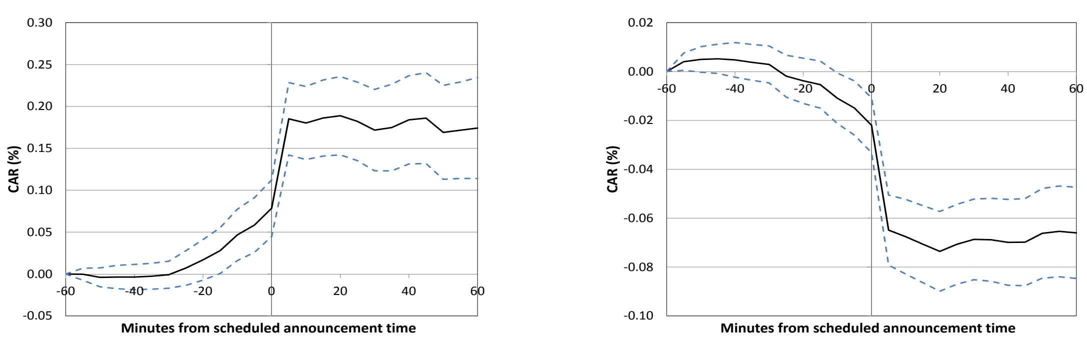
(a) Announcements with no evidence of drift



(b) Announcements with some evidence of drift



(c) Announcements with strong evidence of drift



The sample period is from January 1, 2008 through March 31, 2014. We classify each event as "good" or "bad" news based on whether the announcement surprise has a positive or negative effect on the stock and bond markets using the coefficients in Table B3. Following Bernile et al. (2016), we invert the sign of returns for negative surprises. Cumulative average returns (CARs) are then calculated in the $[t-60min, t+60min]$ window for each of the "strong drift", "some drift" and "no drift" categories defined in Table B5. For each category the solid line shows the mean CAR. Dashed lines mark two-standard-error bands (standard error of the mean).

Table B7: Announcement Surprise Impact During $[t-30min, t-5sec]$ by Decile

Surprise Size	Surprise Decile	n	E-mini S&P 500 γ	E-mini S&P 500 R^2	10-year Treasury Note γ	10-year Treasury Note R^2	Joint Test p-value
1	5 and 6	96	-0.269 (0.234)	0.01	-0.164 (0.061)***	0.06	0.015
2	4 and 7	95	0.228 (0.093)**	0.06	-0.055 (0.029)*	0.03	0.009
3	3 and 8	95	0.063 (0.051)	0.01	0.001 (0.014)	0.00	0.464
4	2 and 9	96	0.075 (0.030)**	0.06	-0.031 (0.009)***	0.11	0.000
5	1 and 10	94	0.115 (0.027)***	0.16	-0.030 (0.005)***	0.26	<0.0001
All		476	0.102 (0.020)***	0.08	-0.029 (0.004)***	0.09	<0.0001

The sample period is from January 1, 2008 through March 31, 2014. Only the announcements classified as having strong evidence of pre-announcement drift in Table B5 are included. These announcements are pooled together and split into deciles by surprise size. Since our sample includes positive and negative surprises, deciles 1 and 10 correspond to the largest surprises in absolute value, and deciles 5 and 6 correspond to the smallest surprises in absolute value. The reported response coefficients γ are the ordinary least squares estimates of equation (1) with the White (1980) heteroskedasticity consistent covariance matrix. Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5% and 1% levels, respectively. The p-values are for the joint Wald test that the coefficients of announcement surprises for the E-mini S&P 500 and 10-year Treasury note futures are equal to zero.

The left column shows CARs for the stock market. In the no-drift announcements in Panel (a), a significant price adjustment does not occur until after the release time. In the strong-drift announcements in Panel (c), the price begins moving in the correct direction about 30 minutes before the official release time, and the move becomes significant about ten minutes later. In the intermediate group in Panel (b), there is a less pronounced price adjustment in the correct direction before the releases. The second column presents CARs for the bond market. Panel (c) shows the same pattern as the stock market with the price starting to drift about 30 minutes before the official release time and the move becoming statistically significant about 20 minutes later.[8] Overall, Figure B4 tells the same story as Figure 1 in the paper that illustrates substantial pre-announcement drift for a substantial number of announcements.

7.4 Event Window Length

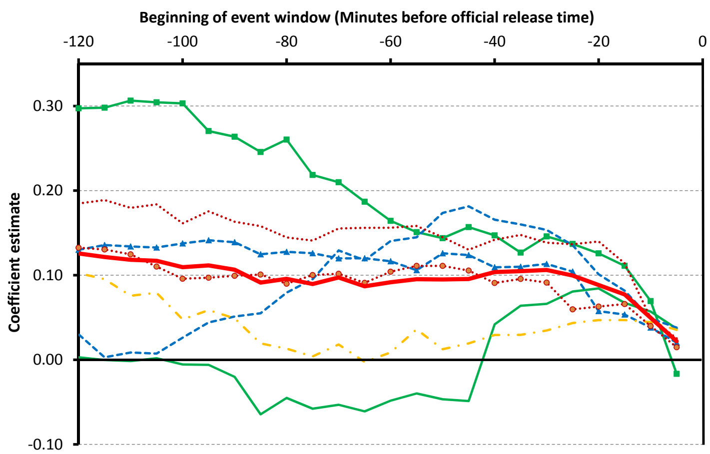
The analysis in the above Sections 7.1 and 7.2 uses a $[t-30min, t-5sec]$ event window. To show that our results are not sensitive to the choice of the window length, we re-estimate

[8]For the bond market, Panels (b) and (c) look similar. This is because the classification of announcements as "some evidence of drift" is mainly driven by the bond market results in Table B5. Panels (a) and (b) for the bond market appear to show some drift (only about one basis point) starting about 60 minutes prior to the announcement. Therefore, we estimate the regression in equation (1) for the $[t-60min, t-30min]$ window. Only the ADP Employment announcement is significant.

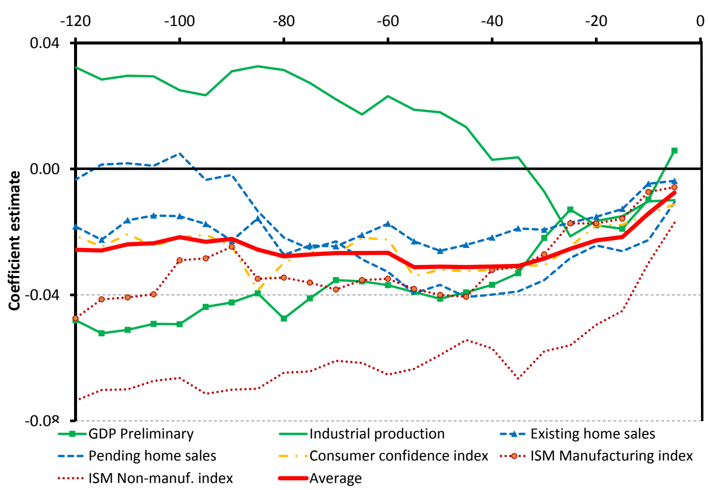
equation (1) with $[t - \underline{\tau}, t - 5sec]$ for various $\underline{\tau} \in [5min, 120min]$. Figure B5 plots estimates of the corresponding γ_m coefficients for the seven drift announcements. The results confirm the conclusions from the lower panel of Figure B1: For most of the announcements, the drift starts at least 30 minutes before the release time. Shortening the pre-announcement window generally results in lower coefficients (and lower standard errors). This is typical for intraday studies where the ratio between signal (i.e., response to the news announcement) and noise increases as the event window shrinks and fewer other events affect the market.

Figure B5: Sensitivity of Coefficients to Event Window Length

(a) E-mini S&P 500 Futures



(b) 10-year Treasury Note Futures



The sample period is from January 1, 2008 through March 31, 2014. The figure plots response coefficients, γ_m, based on the ordinary least squares estimates of equation (1) against $\underline{\tau}$, the beginning of the pre-announcement window $[t - \underline{\tau}, t - 5sec]$, for seven strong drift announcements identified in Table B5.

7.5 Effect of Order Flows

We verify that our results in Sections 7.1 and 7.2 of this appendix are not driven by order flows having a different impact before drift announcements than at other times. We introduce the identifier $\tilde{m}$ to distinguish the returns around m announcements and the returns during corresponding time windows on non-announcement days. $\tilde{m}$ can take on 33 different values because there are 30 announcements and three time windows for which we compute the order flow impact on non-announcement days. These non-announcement day windows are [8:30 – 30min, 8:30 – 5sec], [9:15 – 30min, 9:15 – 5sec], [10:00 – 30min, 10:00 – 5sec] because all of our announcements with evidence of drift are released during these windows.[9]

Let $R_{\tilde{m}t}$ be the return on day t during the $[t-30min, t-5sec]$ window around the release of announcement m or during one of the three time windows on non-announcement days. Let OF_{mt} be the corresponding order flow. Now consider the relation

$$sign\left(OF_{\tilde{m}t}\right) R_{\tilde{m}t} = c + a_{\tilde{m}} + b_0\sqrt{|OF_{\tilde{m}t}|} + b_1 \mathbb{1}_{NoDrift}\left(\tilde{m}\right)\sqrt{|OF_{\tilde{m}t}|} + b_2 \mathbb{1}_{Drift}\left(\tilde{m}\right)\sqrt{|OF_{\tilde{m}t}|} + \varepsilon_{\tilde{m}t}, \tag{3}$$

where $\mathbb{1}_{NoDrift}\left(\tilde{m}\right)$ and $\mathbb{1}_{Drift}\left(\tilde{m}\right)$ are indicator variables. $\mathbb{1}_{NoDrift}$ equals 1 only if $\tilde{m}$ stands for an announcement without strong evidence of drift, and $\mathbb{1}_{Drift}$ is 1 only if $\tilde{m}$ is an announcement with strong evidence of drift. They are zero otherwise.

By this specification, significant estimates of b_1 and/or b_2 would indicate that the impact of the order flow for those announcement types is different from the usual impact on non-announcement days captured by the coefficient b_0. To account for announcements happening at different times, we also include the fixed effects $a_{\tilde{m}}$ which depend on the announcement m and, for the non-announcement days, on the three time windows.

The square root impact of order flow on returns in the above specification reflects the concave impact of trades on returns commonly accepted in the literature (for example, Hasbrouck and Seppi (2001) and Almgren, Thum, Hauptmann, and Li (2005)). The use of absolute order flow and of $sign\left(OF_{\tilde{m}t}\right) R_{\tilde{m}t}$ as dependent variable allows us to capture the heterogeneity among announcement types using the fixed effects $a_{\tilde{m}}$. Taking the first difference Δ within each $\tilde{m}$, the fixed effects drop out, and we estimate the equation

$$\begin{aligned}\Delta sign\left(OF_{\tilde{m}t}\right) R_{\tilde{m}t} \;&=\; c_1 + b_0\Delta\sqrt{|OF_{\tilde{m}t}|} + b_1 \mathbb{1}_{NoDrift}\left(\tilde{m}\right)\Delta\sqrt{|OF_{\tilde{m}t}|} \\ &+\; b_2 \mathbb{1}_{Drift}\left(\tilde{m}\right)\Delta\sqrt{|OF_{\tilde{m}t}|} + \Delta\varepsilon_{\tilde{m}t}, \end{aligned} \tag{4}$$

where we keep an intercept and test whether it equals zero. Hence, testing the hypothesis

[9] To keep comparisons meaningful, we do not include time windows around other release times, i.e., 8:15, 9:55, 14:00 and 15:00, because no drift announcements are released during these times.

that the impact of order flow on returns on announcement days with drift is the same as on other days involves a t-test on the estimated coefficient for b_2. The results in Table B8 show that this is the case because the t-statistic is insignificant. We conclude that order flow impact on announcement days with drift is no different from its impact on other days.

Table B8: Order Flow Analysis

	E-mini S&P 500 Futures	10-year Treasury Note Futures
b_0	1.282 (0.067)***	0.037 (0.002)***
b_1	0.069 (0.117)	0.004 (0.003)
b_2	-0.178 (0.137)	-0.003 (0.004)
R^2	0.321	0.219

The sample period is from January 1, 2008 through March 31, 2014. The reported response coefficients b_0, b_1 and b_2 are the ordinary least squares estimates of equation (4). Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5% and 1% levels, respectively.

7.6 Other Markets

This section presents results for two other major markets: E-mini Dow stock index futures and 30-year Treasury bond futures. Table B9 confirms the results from Table B4: Pre-announcement price drift is evident not only in the E-mini S&P 500 futures and 10-year Treasury note futures but also in E-mini Dow stock index futures and 30-year Treasury bond futures.

8 Forecasting with Proprietary Information

This section provides additional information for Section 5.1.2 in the paper about predicting the announcement surprise using proprietary data sets. As described in Section 5.1.2, we use three examples of proprietary data collection to predict surprises in announcements most related to this proprietary data. Tables B10, B11 and B12 show results for the Consumer Price Index, Conference Board (CB) Consumer Confidence Index, and housing sector announcements, respectively. We find predictive power in the PriceStats inflation indicator but no predictive power in the State Street Investor Confidence Index and the Case-Shiller Home Price Index.

Table B9: Announcement Surprise Impact During $[t - 30min, t - 5sec]$ for E-mini Dow and 30-year Treasury Bond Futures

Announcement	E-mini Dow γ_m	R^2	30-year Treasury Bond γ_m	R^2	Joint Test p-value
ISM Non-manufacturing index	0.105 (0.025)***	0.15	-0.079 (0.016)***	0.25	<0.0001
Pending home sales	0.148 (0.063)**	0.11	-0.073 (0.029)**	0.15	0.002
ISM Manufacturing index	0.074 (0.035)**	0.04	-0.041 (0.015)***	0.08	0.003
Existing home sales	0.092 (0.038)**	0.07	-0.043 (0.015)***	0.07	0.001
CB Consumer confidence index	0.021 (0.054)	0.00	-0.061 (0.016)***	0.17	0.001
Industrial production	0.047 (0.018)**	0.10	-0.016 (0.016)	0.01	0.023
GDP preliminary	0.135 (0.049)**	0.16	-0.037 (0.019)*	0.06	0.004
Housing starts	0.003 (0.018)	0.00	-0.026 (0.016)	0.03	0.279
Non-farm employment	0.034 (0.018)*	0.07	-0.007 (0.018)	0.00	0.164
Advance retail sales	0.004 (0.027)	0.00	-0.047 (0.019)**	0.10	0.050
Consumer credit	-0.057 (0.045)	0.02	0.014 (0.015)	0.02	0.301
ADP employment	0.029 (0.022)	0.03	-0.006 (0.012)	0.00	0.392
UM Consumer sentim. - Final	-0.064 (0.040)	0.05	0.007 (0.017)	0.00	0.247
Initial jobless claims	-0.006 (0.011)	0.00	0.014 (0.008)	0.01	0.220
New home sales	0.005 (0.030)	0.00	-0.010 (0.016)	0.01	0.808
Building permits	-0.012 (0.023)	0.01	-0.012 (0.020)	0.01	0.733
GDP advance	0.037 (0.039)	0.04	-0.043 (0.035)	0.09	0.296
GDP final	0.005 (0.021)	0.00	-0.005 (0.022)	0.00	0.950
UM Consumer sentim. - Prel	-0.025 (0.045)	0.00	-0.008 (0.017)	0.00	0.770
Durable goods orders	-0.001 (0.015)	0.00	-0.013 (0.015)	0.01	0.664
Consumer price index	-0.005 (0.031)	0.00	0.000 (0.013)	0.00	0.987

The sample period is from January 1, 2008 through March 31, 2014. Only the announcements that have a significant effect on the E-mini S&P 500 and 10-year Treasury note futures prices (based on the joint test in Table B3) are included. The reported response coefficients γ_m are the ordinary least squares estimates of equation (1) with the White (1980) heteroskedasticity consistent covariance matrix. Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5% and 1% levels, respectively. The p-values are for the joint Wald test that the coefficients of announcement surprises for the E-mini Dow stock index and 30-year Treasury bond futures are equal to zero. The intercept, γ_0, is significant only for the Pending Home Sales announcement in the stock market, GDP Advance and Initial Jobless Claims announcements in the bond market, and Non-Farm Employment announcement in both markets.

9 Forecasting with Individual Analyst Forecasts

This section provides additional information for Section 5.2.1 in the paper about forecasting the announcement surprise using the forecasts of individual analysts. As described in Section 5.2.1, we regress the unstandardized surprise, $\hat{S}_{mt}$, on a constant and the prediction, P_{mt}. The results for this regression are reported in Table B13 where the p-values are for a two-sided test. The intercept is significant for only one announcement (UM Consumer Sentiment - Final), indicating that our forecast for the surprise is generally unbiased. Nine announcements show significance of the slope coefficient at 10% level (Advance Retail Sales,

Table B10: Predicting CPI surprises with State Street PriceStats data

Predictor	N	Coefficient
Average daily value PriceStats for month t	68	0.157 (0.049)***
Last daily value PriceStats for month t	68	0.155 (0.048)***

The sample period is from August 1, 2008 through March 31, 2014 because the PriceStats data begins in August of 2008. N denotes the number of observations. The dependent variable is the Consumer Price Index surprise for month t. The reported response coefficients are estimated using the MM weighted least squares (Yohai, 1987). Standard errors are shown in parentheses. *, ** and *** indicate statistical significance at 10%, 5% and 1% levels, respectively.

Table B11: Predicting CB Consumer Confidence Index surprises with State Street Investor Confidence

Predictor	N	Coefficient
Monthly State Street Investor Confidence Index	74	0.082 (0.063)

The sample period is from January 1, 2008 through March 31, 2014. N denotes the number of observations. The dependent variable is the Consumer Confidence Index surprise for month t. The reported response coefficients are estimated using the MM weighted least squares (Yohai, 1987). Standard errors are shown in parentheses. *, ** and *** indicate statistical significance at 10%, 5% and 1% levels, respectively.

Table B12: Predicting surprises for housing sector announcements with the Case-Shiller Home Price Index

Dependent Variable	N	Coefficient
Building permits	72	95.951 (50.65)*
Existing home sales	72	-0.074 (0.233)
Housing starts	72	-9.065 (68.13)
New home sales	71	21.925 (40.83)
Pending home sales	73	-0.113 (0.050)**

The sample period is from January 1, 2008 through March 31, 2014. N denotes the number of observations. The dependent variables are surprises in announcements related to the housing sector for month t. The reported response coefficients are estimated using the MM weighted least squares (Yohai, 1987). Standard errors are shown in parentheses. *, ** and *** indicate statistical significance at 10%, 5% and 1% levels, respectively.

CB Consumer Confidence Index, CPI, Durable Goods Orders, Existing Home Sales, GDP Advance, Industrial Production, Pending Home Sales and PPI), only five of which are announcements with a pre-announcement drift.

A significant linear relation between the predictions and surprises does not necessarily imply that the forecasts have superior predictive power for *returns*. To explore this, we estimate equation (1) using the prediction, P_{mt}, instead of the surprise, S_{mt}. Table B14 Panel (a) shows the slope coefficients for predicting the pre-announcement return during the $[t-30min, t-5sec]$ window using the surprise prediction for the E-mini S&P 500 and 10-year Treasury note futures markets. The reported p-values are for a two-sided test. Similarly, Table B14 Panel (b) reports the results for the $[t-5sec, t+5min]$ window. P_{mt} is a useful predictor of returns only for a handful of announcements.

Table B13: Regression of Unstandardized Surprise, $\hat{S}_{mt}$, on a Constant and Prediction, P_{mt}

	Slope Coefficient	s.e.	p-value	R^2
ADP employment	0.173	0.371	0.320	0.02
Advance retail sales	1.096	0.724	0.065	0.07
CB Consumer confidence index	1.188	0.586	0.021	0.06
Construction spending	-0.004	0.002	0.984	0.08
Consumer price index	0.961	0.113	<0.001	0.35
Durable goods orders	1.946	0.468	<0.001	0.17
Existing home sales	1.621	0.767	0.017	0.09
GDP advance	1.371	0.784	0.040	0.17
GDP final	-0.0005	0.0001	1.000	0.22
GDP preliminary	0.118	0.593	0.421	0.04
Housing starts	-0.039	0.453	0.466	0.01
Industrial production	1.026	0.318	0.001	0.22
Initial jobless claims	0.360	0.289	0.106	0.01
ISM Manufacturing index	0.580	0.540	0.141	0.03
ISM Non-manufacturing index	-0.149	0.782	0.575	0.01
New home sales	-0.324	1.157	0.610	0.01
Non-farm employment	-0.052	0.332	0.562	0.01
Pending home sales	0.762	0.405	0.030	0.08
Producer price index	1.206	0.397	0.001	0.15
UM Consumer sentiment - Prel	0.608	0.821	0.229	0.02

The sample period is from January 1, 2008 through March 31, 2014. The unstandardized surprise is defined as $\hat{S}_{mt} = A_{mt} - E_{t-\underline{\tau}}[A_{mt}] = \sigma_m S_{mt}$. The prediction of the unstandardized surprise is the difference between the median values of the professional forecasters ranked by Bloomberg and the whole set of forecasters in the Bloomberg survey: $P_{mt} = E^{Ranked}_{t-\underline{\tau}}[A_{mt}] - E_{t-\underline{\tau}}[A_{mt}]$. Results are from the ordinary least squares regression, where the standard errors are based on a heteroskedasticity consistent covariance matrix.

Table B14: Regression of Returns on Prediction

a) $[t - 30min, t - 5sec]$ **Window**

	E-mini S&P 500			10-year Treasury Note			Wald	
	γ_m	s.e.	R^2	γ_m	s.e.	R^2	Test	p-value
ADP employment	0.030	0.015	0.03	-0.019	0.007	0.09	11.108	0.004
Advance retail sales	0.002	0.019	0.01	-0.009	0.010	0.02	0.781	0.677
CB Consumer confidence idx	-0.004	0.039	0.01	-0.019	0.007	0.06	7.788	0.020
Construction spending	-0.008	0.053	0.01	-0.009	0.012	0.02	0.592	0.744
Consumer price index	0.001	0.022	0.01	-0.002	0.009	0.01	0.050	0.975
Durable goods orders	0.019	0.013	0.03	-0.007	0.007	0.03	3.334	0.189
Existing home sales	0.014	0.065	0.01	-0.021	0.018	0.05	1.424	0.491
GDP advance	0.087	0.055	0.19	-0.016	0.016	0.07	3.495	0.174
GDP preliminary	0.005	0.044	0.04	-0.007	0.013	0.05	0.278	0.870
GDP final	-0.001	0.028	0.04	-0.022	0.013	0.12	3.088	0.214
Housing starts	0.006	0.016	0.01	-0.015	0.006	0.04	6.959	0.031
Industrial production	0.012	0.020	0.02	-0.002	0.005	0.07	19.136	$<$0.001
Initial jobless claims	-0.025	0.010	0.02	0.006	0.005	0.01	7.340	0.025
ISM Manufacturing index	-0.010	0.070	0.01	0.004	0.014	0.02	0.113	0.945
ISM Non-manufacturing index	0.012	0.032	0.01	-0.009	0.017	0.02	0.384	0.825
New home sales	-0.015	0.030	0.02	-0.008	0.006	0.03	2.167	0.338
Non-farm employment	0.009	0.019	0.02	-0.006	0.011	0.02	0.514	0.774
Pending home sales	-0.023	0.032	0.02	-0.012	0.007	0.03	3.649	0.161
Producer price index	-0.027	0.022	0.03	0.013	0.009	0.04	3.691	0.158
UM Consumer sentim. - Prel	-0.076	0.036	0.04	0.001	0.009	0.01	4.561	0.102

b) $[t - 5sec, t + 5min]$ **Window**

	E-mini S&P 500			10-year Treasury Note			Wald	
	γ_m	s.e.	R^2	γ_m	s.e.	R^2	Test	p-value
ADP employment	-0.001	0.023	0.01	0.018	0.013	0.03	2.028	0.363
Advance retail sales	0.043	0.031	0.04	-0.020	0.014	0.03	3.947	0.139
CB Consumer confidence idx	0.016	0.037	0.02	0.001	0.010	0.01	0.214	0.899
Construction spending	-0.037	0.032	0.02	0.039	0.014	0.08	9.063	0.011
Consumer price index	-0.040	0.035	0.03	-0.006	0.012	0.02	1.541	0.463
Durable goods orders	0.046	0.020	0.07	-0.027	0.011	0.08	11.136	0.004
Existing home sales	-0.039	0.031	0.03	-0.009	0.013	0.02	2.089	0.352
GDP advance	-0.015	0.089	0.04	0.035	0.023	0.09	2.270	0.321
GDP final	0.069	0.047	0.13	0.006	0.012	0.04	2.458	0.293
GDP preliminary	-0.055	0.037	0.07	0.040	0.021	0.17	5.883	0.053
Housing starts	0.021	0.019	0.03	-0.005	0.008	0.02	1.688	0.430
Industrial production	0.000	0.014	0.01	0.003	0.004	0.02	0.595	0.743
Initial jobless claims	-0.018	0.013	0.00	0.004	0.005	0.00	0.865	0.649
ISM Manufacturing index	0.004	0.040	0.01	-0.001	0.017	0.01	0.017	0.991
ISM Non-manufacturing index	0.022	0.033	0.02	-0.005	0.008	0.02	0.892	0.640
New home sales	0.020	0.022	0.02	0.005	0.009	0.02	1.205	0.547
Non-farm employment	-0.066	0.076	0.03	0.020	0.043	0.02	0.964	0.618
Pending home sales	-0.016	0.038	0.02	0.016	0.006	0.06	8.110	0.017
Producer price index	0.010	0.023	0.02	-0.004	0.017	0.02	0.238	0.888
UM Consumer sentim. - Prel	0.019	0.020	0.02	0.002	0.006	0.01	0.945	0.623

The sample period is from January 1, 2008 through March 31, 2014. The response coefficients γ_m are the ordinary least squares estimates of equation (1) using the prediction P_{mt} of the standardised surprise S_{mt}, where $S_{mt} = \frac{A_{mt} - E_{t-\mathcal{I}}[A_{mt}]}{\sigma_m}$ and $P_{mt} = E^{Ranked}_{t-\mathcal{I}}[A_{mt}] - E_{t-\mathcal{I}}[A_{mt}]$. The standard errors are based on a heteroskedasticity consistent covariance matrix.

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