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Price drift before U.S. macroeconomic news: private information about public announcements?

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Abstract

We examine stock index and Treasury futures markets around releases of U.S. macroeconomic announcements. Seven out of 21 market-moving announcements 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 half of the total price adjustment. These results imply that some traders have private information about macroeconomic fundamentals. The evidence suggests that the pre-announcement drift likely comes from a combination of information leakage and superior forecasting based on proprietary data collection and reprocessing of public information.

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

Non-technical Summary

Macroeconomic indicators play an important role in business cycle forecasting and are closely watched by financial markets. Some of these indicators appear to influence financial market prices even ahead of their official release time. This paper examines the prevalence of pre-announcement price drift in U.S. stock and bond markets and looks for possible explanations.

We study the impact of announcements on second-by-second E-mini S&P 500 stock index and 10-year Treasury note futures from January 2008 to March 2014. The study is based on 21 market-moving announcements among a sample of 30 U.S. macroeconomic announcements. Eleven out of these 21 announcements exhibit some pre-announcement price drift in the "correct" direction, i.e., in the direction of the price change consistent with the announcement surprise. For seven of these announcements the drift is substantial. Prices start to move about 30 minutes before the official release time, and this pre-announcement price move accounts on average for about a half of the total price adjustment.

These facts are uncovered by an outlier-robust procedure (MM weighted least squares), but are similarly striking in cumulative average return graphs and order flow imbalances. The paper shows that these results are robust to controlling for, among others, outliers, data snooping, nearby announcements and the choice of the event window length.

Extending the sample period back to 2003 with minute-by-minute data reveals both a higher announcement impact and a stronger pre-announcement drift since 2008, especially in the S&P E-mini futures market. Based on a back-of-the-envelope calculation, we estimate that since 2008 in the S&P E-mini futures market alone the profits associated with trading prior to the official announcement release time have amounted to about 20 million USD per year.

The late start of pre-release price drift, which becomes significant only about 30 minutes before the official release time, reveals an interesting property of prevalent trading strategies. Assuming that informed traders possess their informational advantage already more than 30 minutes ahead of the release, the question arises why they wait with trading on their knowledge until shortly before the release time. A possible explanation is that trading close to the release time minimizes the exposure to other risks that are unrelated to macroeconomic announcements.

The difficulty of identifying the causes of pre-announcement drift stems from the relatively small number of announcements that actually move financial markets. Nevertheless, we find that an implementation of strict release procedures makes pre-release drift less likely. This applies in particular to data released under the Principal Federal Economic Indicator (PFEI) guidelines, which impose strict security procedures. There is no evidence that modifying the calculation of market expectations, e.g., a focus on the most recent survey responses, helps in predicting the commonly used announcement surprise. Public information, such as internet activity data, predicts the surprise in a few cases where the public information closely corresponds to the forecasting target. Analogously, improvements in data processing render privately collecting large amounts of comparable information feasible, which can be used for generating proprietary forecasts ahead of time.

This early information – leaked or self-calculated – does not need to be precise in order to a have a large price impact. Under Bayesian learning, even if the information available before the official release is noisy, it can have a large price impact because of its timing. For a Bayesian learner, early availability makes up for less precision and a potentially smaller surprise. Thus, the incentives for privately collecting information and for leakage are high.

The main policy implications of this paper are twofold. First, the total impact of macroeconomic news is larger than measured in most event studies, which ignore the pre-release price drift. Therefore, the total impact of macroeconomic news on financial markets is larger, and financial markets are linked more tightly to the real economy than usually found. Second, information of many macroeconomic announcements is known by some market participants in advance. To ensure fairness in financial markets, strict release procedures need to be implemented for all market-moving announcements including announcements originating in the private sector.

1 Introduction

Numerous studies, such as Andersen, Bollerslev, Diebold, and Vega (2007), have shown that macroeconomic news announcements move financial markets. These announcements are quintessential updates to public information on the economy and fundamental inputs to asset pricing. More than a 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, 2013). 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. Eleven out of the 21 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. For seven of these announcements the drift is substantial. Prices start to move about 30 minutes before the official release time, and this pre-announcement price move accounts on average for about a half 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. However, it remains unclear what share of the price move occurs before the announcement. The second group does separate the pre- and post-announcement effects but concludes that the pre-announcement effect is small or non-existent.

Our results differ from those in previous research for four reasons. First, some studies measure the pre-announcement effect in small increments of time. For example, Ederington and Lee (1995) use 10-second returns in the $[t - 2min, t + 10min]$ window around 21 U.S. macroeconomic announcements from 1988 to 1992 and report that significant price moves occur only in the post-announcement interval in the Treasury, Eurodollar and DEM/USD futures markets. However, if the pre-announcement drift is gradual (which is the case in our data), it will not be detected in such small increments. Our approach uses a longer pre-announcement interval and uncovers the price drift.

Second, other studies consider only short pre-announcement intervals. Andersen et al. (2007), for example, include ten minutes before the official release time. In a sample of 25 U.S. announcements from 1998 to 2002, they find that global stock, bond and foreign exchange markets react to announcements only after their official release time. We show that the pre-announcement interval has to be about 30 minutes long to capture the price drift.

Third, we include a larger and more comprehensive set of influential announcements. We augment the set of Andersen, Bollerslev, Diebold, and Vega (2003) with seven announcements frequently discussed in the financial press. Three of these additional announcements exhibit a drift. Because not all market-moving announcements exhibit a drift, limiting the analysis to a small subset can lead to the erroneous conclusion that the pre-announcement drift does not exist in macroeconomic announcements.

Fourth, the difference may stem from parameter instability. Not only do announcement release procedures change over time but information collection and computing power also 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 previous studies that use older sample periods, we analyze minute-by-minute data extended back to August 2003. The results suggest that the pre-announcement effect was indeed weak or non-existent in the older sample 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 is correlated with the announcement surprise. Bernile, Hu, and Tang (in press) 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) announcements and three macroeconomic announcements (Non-Farm Employment, Consumer Price Index and Gross Domestic Product) between 1997 and 2013. 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 are found before the FOMC meetings but not before the other announcements. Bernile et al. (in press) suggest these findings are consistent with information leakage.[1]

Our study differs from Hautsch et al. (2011) and Bernile et al. (in press) in two important aspects. First, our methodology and an expanded set of announcements allow us to show that pre-announcement informed trading is limited neither to FOMC announcements nor to the last minute before the official release time. Second, instead of *assuming* information leakage, we explore the information leakage explanation 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.[2]

With respect to organization type, we focus on the difference between organizations subject to the Principal Federal Economic Indicator (PFEI) guidelines and other entities.

[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 (for example, J. Y. Campbell, Ramadorai, and Schwartz (2009) and Kaniel, Liu, Saar, and Titman (2012) in trading by institutional and individual investors, respectively.

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 have been known to release information to exclusive groups of subscribers before making it available to the public. These documented early releases are in the range of seconds, i.e., shorter than our pre-announcement drift interval, but the fact that early releases exist renders earlier data leakage a possibility worth exploring. 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 readily 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, previously not documented in academic literature, involves an unusual pre-release procedure used in three announcements: 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. Three announcements in this category are among the seven announcements with strong drift.

While these findings are suggestive, a conclusion that leakage causes pre-announcement drift is premature for two reasons. First, the small number of market-moving announcements precludes proving leakage based on public trading data alone. Second, 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 can then be utilized to trade in the "correct" direction before announcements. We show that proprietary information permits forecasting announcement surprises in some cases. Based on an extensive forecasting exercise with public information, we are indeed able to forecast surprises of some announcement variables. However, we find no relation between the forecastability of the surprise and the pre-announcement drift.

While the overall evidence points to leakage and proprietary data collection as the most likely sources of pre-announcement drift, reprocessing of public information may also contribute to some extent. Further research is needed to definitively determine the source of informed trading. Such an investigation would be timely especially following a recent press release by the Securities and Exchange Commission (SEC) about charging two hackers who hacked into news wire services and sold the information on upcoming corporate earnings announcements to traders in six countries including the U.S. which resulted in over \$100 million in illegal profits (SEC, 2015).

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

2 Methodology

We assume that efficient markets react only to the unexpected component of news announcements ("the surprise"), S_{mt}. The effect of news announcements on asset prices can then be analyzed by standard event study methodology (Balduzzi et al., 2001). 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 $[t-\underline{\tau}, t+\overline{\tau}]$. 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.

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, $E_{t-\underline{\tau}}[A_{mt}]$.[3] We standardize the difference by the standard deviation of the respective announcement, σ_m, to convert them to equal units. Specifically,

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

We proxy the expectation, $E_{t-\underline{\tau}}[A_{mt}]$, by the median response of professional forecasters during the days before the release, $E_{t-\Delta}[A_{mt}]$.[4] We use a survey carried out by Bloomberg, which allows the professional forecasters to revise their responses until shortly before the release time. Although $\Delta \neq \underline{\tau}$, the scarcity of revisions shortly before the official release times indicates that the two expectations are more or less identical.[5] We assume that the expectation $E_{t-\Delta}[A_{mt}]$ about a macroeconomic announcement is exogenous, in particular not affected by asset returns during $[t - \underline{\tau}, t]$.

To isolate the pre-announcement effect from the post-announcement effect, we first identify the market-moving announcements among our set of macroeconomics announcements. Markets might focus on a subset of announcements because of their different intrinsic values (Gilbert, Scotti, Strasser, & Vega, 2016) or as a consequence of an optimal information acquisition strategy in presence of private information (Hirshleifer, Subrahmanyam, & Titman, 1994). 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.

We use five seconds before the official release time as start of the post-announcement

[3] 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.

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

[5] 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.

interval for two reasons. 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. 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 release times to the actual release times and show that such accidental early releases are rare and occur only milliseconds before the official release time. Therefore, using five seconds before the official release time as the pre-announcement interval cutoff suffices to ensure that none of the accidental early releases fall into the pre-announcement interval.[6] We use $\overline{\tau} = 5$ minutes after the official release time as the end of the post-announcement interval. Although previous papers such as Hu et al. (2013) indicate that announcements are almost instantaneously reflected in prices once released, we find evidence of price adjustment continuing after the first minute in three of our announcements. We, therefore, use $\overline{\tau} = 5$ minutes to capture the entire price move after the official release time.

Next, we re-estimate equation (1) for the market-moving announcements identified in the first step, using only the pre-announcement window $[t - 30min, t - 5sec]$.[7] Comparing the coefficients from the two regressions yields the pre-announcement effect.[8]

[6]Results with the $[t - 30min, t]$ window are similar, suggesting that the extra drift in the last five seconds before the announcement is not substantial.

[7]We use this pre-announcement window as a benchmark and present a robustness check with other window lengths in Section 4.5.3.

[8]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.

3 Data

We start with 23 macroeconomic announcements from Andersen et al. (2003) which is the largest set of announcements among the previous seminal studies.[9] We augment this set by seven announcements that are 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 compared to previous studies is relevant because, for example, the ADP Employment report did not exist until May 2006. Today, it is an influential announcement constructed with actual payroll data. Table 1 lists these 30 macroeconomic announcements grouped by announcement category.

The Bloomberg consensus forecast serves as a proxy for market expectations.[10] The financial news and media company 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 announcements. Forecasts can be posted until two hours before the announcement, i.e., $\Delta \geq 120min$. On average, the forecasts are five days old as of the release time. Forecasters can update them, but this appears to be done infrequently as discussed in Section 2. Bloomberg calculates the consensus forecast as the median of individual forecasts and continuously updates the consensus forecast when additional individual forecasts are posted.

To investigate the effect of the announcements on the stock and bond markets, we use intraday, nearby contract futures prices. Our second-by-second data from Genesis

[9] The National Association of Purchasing Managers index analyzed in Andersen et al. (2003) is currently called ISM Manufacturing Index. We do not report results for the Capacity Utilization announcement because it is always released simultaneously with the Industrial Production announcement and the surprise components of these two announcements are strongly correlated with a correlation coefficient of +0.8. As a robustness check, we account for simultaneity by using their principal component in equation (1). The results are similar to the ones reported for Industrial Production. We omit four monetary announcements (Money Supplies M1, M2, M3, Target Federal Funds Rate) because these policy variables differ from macroeconomic announcements by long preparatory discussions.

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

Table 1: Overview of U.S. Macroeconomic Announcements

Category	Announcement	Frequency	Obs.	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	74	BEA	%	8:30	70
Employment	ADP employment	Monthly	75	ADP	Number of jobs	8:15	34
	Initial jobless claims	Weekly	326	ETA	Number of claims	8:30	44
	Non-farm employment	Monthly	75	BLS	Number of jobs	8:30	84
Industrial Activity	Factory orders	Monthly	74	BC	%	10:00	62
	Industrial production	Monthly	75	FRB	%	9:15	78
Investment	Construction spending	Monthly	74	BC	%	10:00	48
	Durable goods orders	Monthly	75	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	74	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	73	BC	Number of homes	8:30	76
	New home sales	Monthly	74	BC	Number of homes	10:00	73
	Pending home sales	Monthly	76	NAR	%	10:00	36
Government	Government budget	Monthly	74	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	75	CB	Index	10:00	71
	Index of leading indicators	Monthly	75	CB	%	10:00	53
	ISM Manufacturing index	Monthly	75	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).

Financial Technologies spans the period from January 1, 2008 until March 31, 2014. We report results for the E-mini S&P 500 futures market (ticker symbol ES) and the 10-year Treasury notes futures market (ticker symbol ZN) traded on the Chicago Mercantile Exchange (CME), and we present a robustness check for other markets in Section 4.5.7. Because the nearby contract becomes less and less liquid as its expiration date approaches, we switch to the next maturity contract when its daily trading volume exceeds the nearby contract volume. Using these price series, we calculate the continuously compounded return within the intraday event window around each release.

4 Empirical Results

This section presents graphical and regression evidence of the pre-announcement price drift. We start with an event study regression, follow with cumulative average return and cumulative order imbalance graphs and discuss the robustness of our results.

4.1 Pre-Announcement Price Drift

To isolate the pre-announcement effect from the post-announcement effect, we proceed as outlined in Section 2. We begin by identifying market-moving announcements among our set of 30 announcements using regression (1). We examine the event window ranging from five seconds before to five minutes after the official release time t. 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 2 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. 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. Our subsequent analysis is based on these 21 market-moving announcements.

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

Announcement	E-mini S&P 500 Futures γ_m	R^2	10yr 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	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	0.240 (0.034)***	0.46	-0.111 (0.014)***	0.50	<0.001
ISM Non-manufacturing	0.064 (0.037)*	0.07	-0.041 (0.009)***	0.25	<0.001
UM Consumer sent. - Final	0.046 (0.020)**	0.06	-0.014 (0.006)**	0.07	0.005
UM Consumer sent. - 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 focus on the pre-announcement period to determine which of the 21 market-moving announcements exhibit a pre-announcement price drift. We re-estimate equation (1) using an event window ranging from 30 minutes before to five seconds before the scheduled release time. Accordingly, we now use the continuously compounded futures return over the $[t - 30min, t - 5sec]$ window.

Table 3 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.[11] 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.[12] In all seven announcements, the drift is in the "correct" direction, i.e., direction of the price change predicted by the announcement surprise. 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 neither to corporate announcements documented, for example, by J. Y. Campbell et al. (2009) and Kaniel et al. (2012), nor FOMC announcements documented by Bernile et al. (in press).

To account for a potential effect of outliers due to, for example, the turbulent financial crisis, 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 4 shows that all seven announcements significant in Table 3 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 3 joint test. We label them as "no drift" announcements.[13] Four announcements are not significant in the joint test of Table 3 but show significant coefficients in the robust regression using 5% significance level (mainly in the bond market). We label them as "some drift" announcements.

[11] 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 test. The results (available upon request) confirm those reported in Table 3.

[12] Assuming the t-statistics in Table 3 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 1% significance level.

[13] Here, we include the Building Permits announcement that is not significant in Table 3 and shows a drift in the "incorrect" direction in Table 4.

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

Announcement	E-mini S&P 500 Futures γ_m	R^2	10yr Treasury Note Futures γ_m	R^2	Joint Test p-value
ISM Non-manufacturing	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	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	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 sent. - 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 sent. - 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 2) 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.

To quantify the magnitude of the pre-announcement price drift, we divide the γ_m coefficients from Table 3 by the corresponding sum of coefficients from Tables 2 and Table 3, i.e., $\Gamma_m = \gamma_m^{\overline{\tau}=-5sec}/(\gamma_m^{\overline{\tau}=-5sec} + \gamma_m^{\overline{\tau}=+5min})$. Positive values of Γ_m below 100% indicate that the early signal is informative, but noisy. The early signal is either not always present or not perfect. Table 5 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. Therefore, failing to

Table 4: Announcement Surprise Impact During $[t - 30min, t - 5sec]$ (Robust Regression)

Announcement	E-mini S&P 500 Futures γ_m	R^2	10-year Treasury Note Futures 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 2) 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 3 and 4. "Strong drift" announcements show significance at 5% level in Table 3 joint test and at least one market in Table 4. "No drift" announcements are not significant in either Table 3 or 4 at 5% level. "Some drift" announcements are not significant in Table 3 joint test but show significance in Table 4 in at least one market at 5% level.

account for the pre-announcement effect substantially underestimates the total influence that these macroeconomic announcements exert in the 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 A.1 we derive a condition on the relative precision and surprise size of early news and official release under which the impact of the early news exceeds the impact of the official release. In a situation of no prior public information, for example, an early news with one half of the precision and with two thirds of the surprise generates the same price impact as the news at the official release time itself. Earlier information gets more attention than later information and thus has a larger price impact even if the later information is "official" and more precise.

Table 5: Pre-announcement Price Drift as a Proportion of Total Price Change

	E-mini S&P 500 Futures			10-year Treasury Note Futures		
	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 4 are included.

4.2 Cumulative Average Returns

This section illustrates our findings graphically in cumulative average return (CAR) graphs. 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 2. Following Bernile et al. (in press), we invert the sign of returns for negative surprises.[14] 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 4.[15] The CARs in Figure 1 reveal what happens around the announcements.

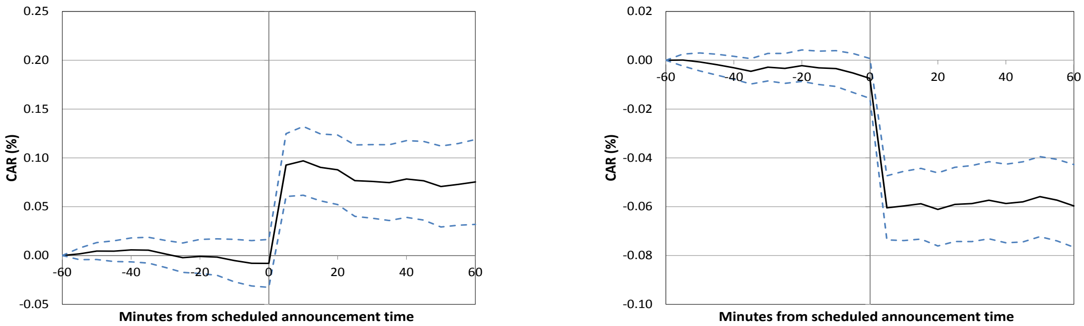
[14]Therefore, if there were a deterministic trend, for example, a positive price change before any announcement, the positive and negative changes would offset each other in our CAR calculations. Note that signs are reversed for the Initial Jobless Claims releases because higher than expected unemployment claims drive stock markets down and bond markets up. Signs are also reversed for the Consumer Price Index (CPI) and Producer Price Index (PPI) in the stock market CAR because higher than expected inflation is often considered bad news for stocks.

[15]We also plotted CAR graphs for longer windows starting, for example, 180 minutes before the announcement. The CARs for $[t-180min, t-30min]$ hover around zero similarly to the $[t-60min, t-30min]$ window in Figure 1.

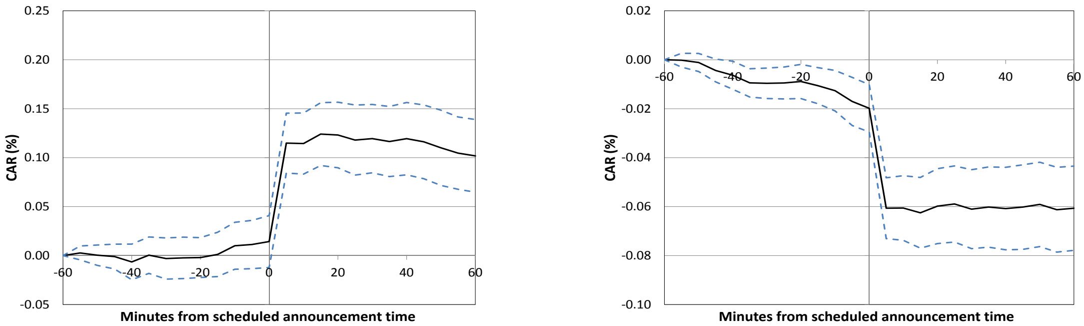
Figure 1: Cumulative Average Returns

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

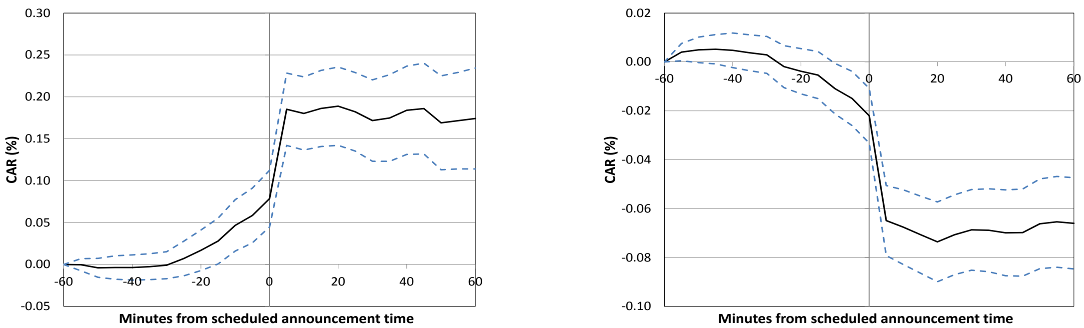
(a) Announcements with no evidence of drift



(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 2. Following Bernile et al. (in press), 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 4. For each category the solid line shows the mean CAR. Dashed lines mark two-standard-error bands (standard error of the mean).

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 significant about twenty minutes later.[16]

We also use the CARs to quantify the magnitude of the pre-announcement price drift as a proportion of the total price adjustment similarly to Sinha and Gadarowski (2010) and Agapova and Madura (2011) in the corporate finance literature. We calculate the proportion as the CAR during the $[t-30min, t-5sec]$ window divided by the CAR during the $[t-30min, t+5min]$ window. In contrast to the Table 5 methodology that takes into account both the sign and the size of the surprise, the CAR methodology takes only the sign into account. The results (available upon request) are similar to Table 5 confirming substantial pre-announcement price drift in both stock and bond markets.

In terms of underlying trading strategies, it is interesting to note that the significant pre-announcement price drift occurs only about 30 minutes before the release time. If informed traders do possess informational advantage already earlier, the question arises why they trade on their knowledge only shortly before the announcements. Perhaps traders execute trades closer to the release time instead of trading in the preceding hours to minimize exposure to risks that are not related to the macroeconomic announcements but are driven by other unpredictable economic or geopolitical events.

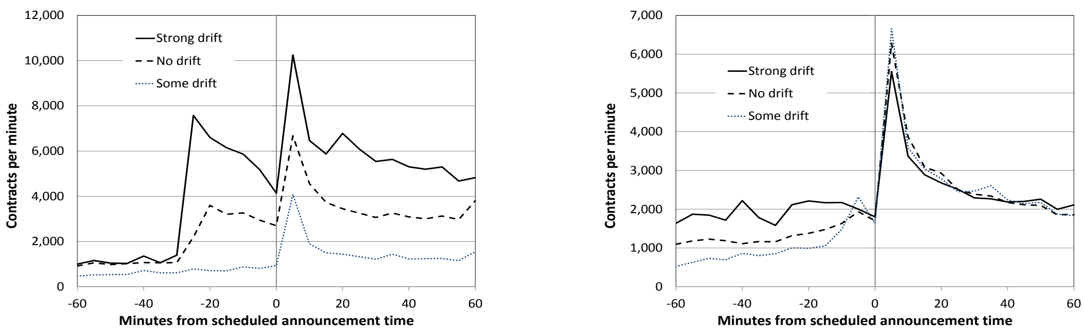
The informed traders could also be strategizing the timing in an attempt to "hide" their trades. Trading on private information is easier when trading volume is high because

[16]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 4. 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. The Appendix Figure A1 shows CARs for the individual announcements.

it is likelier that informed trades will go unnoticed (Kyle, 1985). The trading volume increases especially in the E-mini S&P 500 futures market at 9:30 due to the opening of the stock market and the beginning of open outcry trading. Five out of our seven drift announcements (CB Consumer Confidence Index, Existing Home Sales, ISM Manufacturing Index, ISM Non-Manufacturing Index and Pending Home Sales) are released at 10 a.m. which would allow informed traders to execute trades while taking advantage of the increased volume not related to the announcements (Figure 2).

Figure 2: Trading Volumes

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



The sample period is from January 1, 2008 through March 31, 2014. The figure shows the average trading volume in number of contracts per minute for each of the "strong drift", "some drift" and "no drift" categories defined in Table 4.

It is also possible that traders gain access to the valuable information only shortly before the official release time. The recent SEC 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. The 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 (SEC, 2015).

4.3 Order Flow Imbalances and Profits to Informed Trading

Evidence of informed trading is not limited to prices but 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. (in press), we classify the 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.[17] Figure 3 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, pointing to informed trading during the pre-announcement interval. The pre-announcement imbalances are particularly pronounced for strong (price) drift announcements. Interestingly, all announcements show some pre-announcement order imbalance in the Treasury note futures market.[18]

The magnitude of the drift is economically significant. We estimate the magnitude of the total profit in the E-mini S&P 500 futures market earned by market participants trading in the correct direction ahead of the announcements based on volume-weighted average prices (VWAP). We 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 VWAPs over the $[t - 30min, t - 5sec]$ and $[t + 5sec, t + 1min]$ windows, respectively. We exclude the five seconds before and after the announcement 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 in the direction of the surprise since all the surprises have positive impact on the E-mini S&P 500 prices. Given that the sign of the surprise is either plus or minus

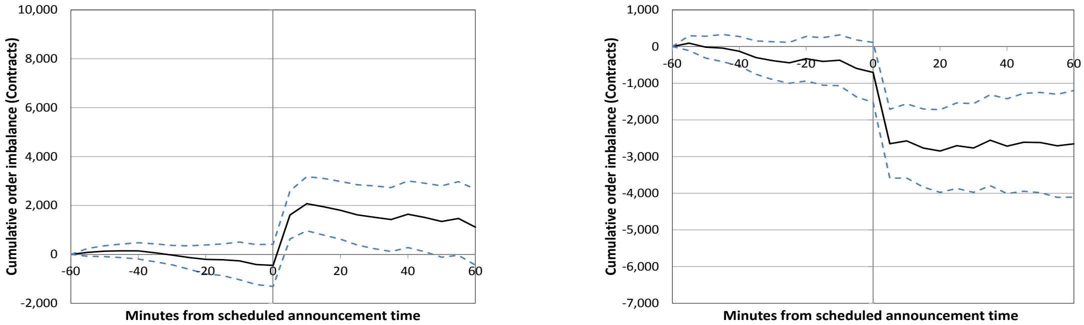
[17] We examine the performance of this volume classification algorithm using detailed limit order book data for our futures contracts that we have available for one month (July 2013). This limit order book data contains accurate classification of each trade as buyer- or seller-initiated. Based on the classification accuracy measure proposed by Easley, Lopez de Prado, and O'Hara (2012), the tick rule correctly classifies 95% and 91% of trading volume in the E-mini S&P 500 and the 10-year Treasury note futures, respectively. We also find that the tick rule performs better than the bulk volume classification method of Easley et al. (2012).

[18] We verify in Section 4.5.5 that the price impact of the order flow does not vary between announcement and non-announcement days.

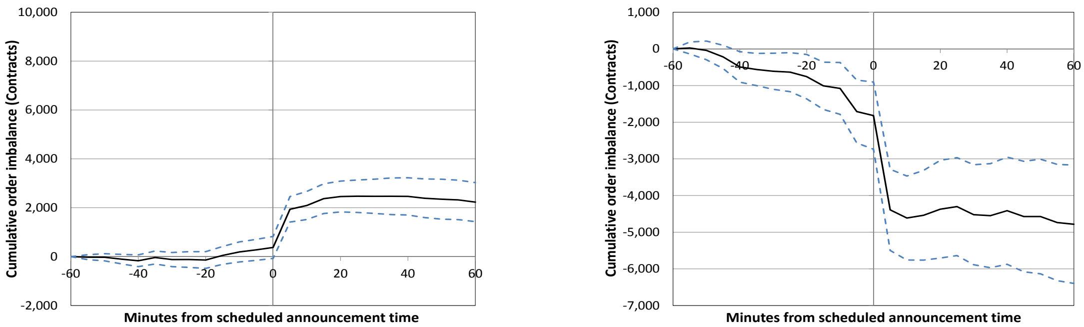
Figure 3: Cumulative Order Imbalances

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

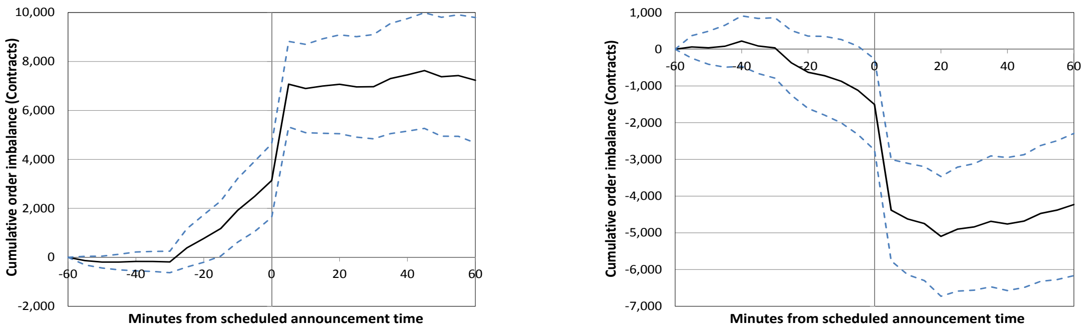
(a) Announcements with no evidence of drift



(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. Announcements are categorized as no drift, some evidence of drift and strong drift using the classification in Table 4. 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. 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).

one, this can also be interpreted as the 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 3. In fact, the correlation between the sign of the surprise and the order flow in the E-mini 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.[19] We then compute the sample average and consider this to be the average quantity traded by informed traders. By the previous remarks, 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 E-mini S&P 500 futures contract is $50 times the index.

Using this methodology for the seven drift announcements, the average profit per announcement release in the E-mini S&P 500 futures market is about $262,000. Multiplying by the number of observations for each of the seven drift announcements, we approximate the total profit at $119 million during a little more than six years. The same methodology is applied to the 10-year Treasury note futures market.[20] We find that for the 10-year Treasury note futures the profits over our sample period amount to about $46 million. Profits in other stock and bond markets can be calculated similarly. The median bid-ask spread is 0.020% for the E-mini S&P 500 futures and 0.013% for 10-year Treasury notes futures. This is far below the two standard deviation band (Figure 1) of the CAR around strong drift announcements for the E-mini, and at this band for the 10-year Treasury notes. Sophisticated traders who use execution algorithms are likely able to trade close to the spread midpoint round trip and incur a slippage which is smaller than the spread. Informed trades around strong drift announcements are therefore profitable.

As a robustness check, we also compute the profit obtained by trading in the direction of the order flow on non-announcement days using the same methodology but without

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

[20]The impact of a positive surprise on the Treasury note futures prices is negative, and the correlation between the sign of the surprise and order flow is approximately -0.14. Hence, one should multiply both the return and the quantity by the opposite sign of the surprise. However, due to arithmetic simplifications, the end result is invariant to such sign changes of both returns and order flow.

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 there is evidence that the economic profits of the pre-announcement price drift are substantial.

4.4 Increase in Drift After 2007

Our *second-by-second* data starts on January 1, 2008. The existing literature referenced in Section 1 uses older sample periods, for which we do not have such high-frequency data. Therefore, we repeat the analysis of Section 4.1 for the sample period from August 1, 2003 to March 31, 2014 and the subperiod ending on December 31, 2007 using *minute-by-minute* data.[21] The beginning of this extended sample is limited by data availability: prior to August 1, 2003, intraday data for the bond market does not start until 8:20 a.m. ET whereas we need data before 8:20 a.m. ET to calculate returns that occur during 30 minutes before 8:30 a.m. announcements.

Figure 4 shows CARs for market-moving announcements based on minute-by-minute data for 2003–2007 and 2008–2014 subperiods.[22] Two features stand out. First, the total announcement impact is less pronounced before 2007 particularly in the E-mini S&P 500 futures market. Second, the pre-announcement drift before 2007 is negligible. Only three announcements exhibit a pre-announcement price drift during the pre-2008 period (UM Consumer Sentiment Preliminary at 5% significance level, and Industrial Production and ISM Manufacturing at 10% significance level). This shows that the pre-announcement

[21] We estimate equation (1) for the $[t - 30min, t - 1min]$ window with minute-by-minute data. We use one minute ($\overline{\tau} = -1min$) 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. To facilitate a comparison of the pre-announcement effects between the two sample periods, we re-estimate equation (1) for the period from January 1, 2008 until March 31, 2014 with *minute-by-minute* data for the same $[t - 30min, t - 1min]$ window. The results match those for the $[t - 30min, t - 5sec]$ window reported in Table 3, confirming that the drift is not driven by price movement in the last minute before the announcement.

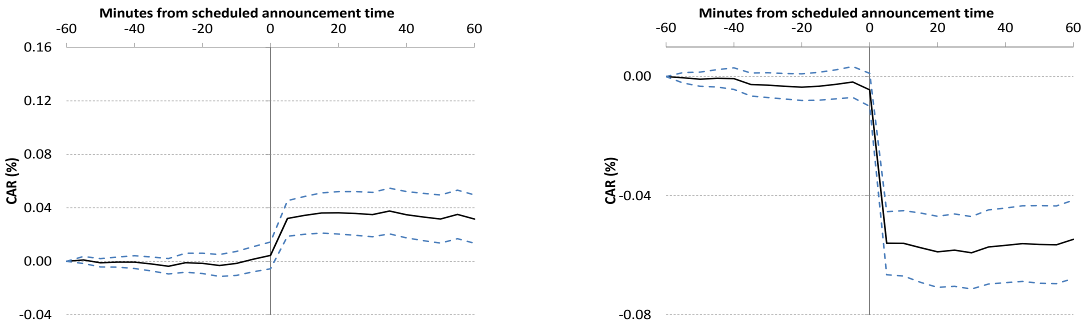
[22] During 2008-2014, the set of market-moving announcements based on minute-by-minute data is identical to the set based on second-by-second data. The set of market-moving announcements during 2003-2007 differs. Factory Orders, Personal Spending, PPI, Trade Balance, and Wholesale Inventories move markets whereas Building Permits, Consumer Credit, CPI, GDP Preliminary, GDP Final, and Housing Starts do not.

effect was weaker or non-existent in our announcements during the pre-2008 period.

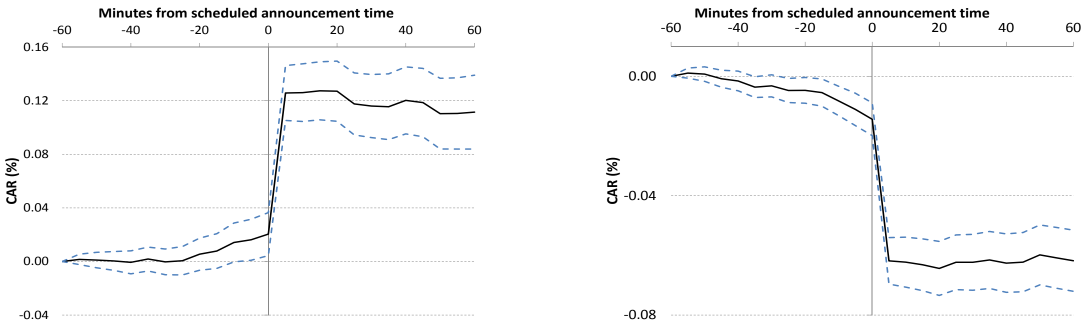
Figure 4: Cumulative Average Returns with Minute-by-Minute Data, 2003–2014

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

(a) 2003–2007



(b) 2008–2014



The figure plots CARs around market-moving announcements for August 1, 2003 - December 31, 2007 and January 1, 2008 - March 31, 2014 in the upper and lower panels, respectively. Dashed lines mark two-standard-error bands (standard error of the mean).

A variety of factors may have contributed to this change. The end of 2007 marks the end of an economic expansion and the beginning of the financial crisis. One contributing factor might have been a differential impact of macroeconomic announcements between recessions and expansions. The study by Boyd, Hu, and Jagannathan (2005), for example, reports that from 1957 to 2000 higher unemployment pushed the stock market up during expansions but drove it down during contractions. Andersen et al. (2007) show that the stock market reaction to macroeconomic news differs across the business cycle with good economic news causing a negative response in expansions but a positive response in contractions because in expansions the discount factor component of the news prevails compared to the cash flow component due to anti-inflationary monetary policies. This state-dependence suggests that the pre-2008 and post-2008 periods should differ, and our results confirm this. Interestingly, in contrast to previous studies, the response to surprises in our data does not change its direction around the end of the recession (dated by the National Bureau of Economic Research as June 2009). Better than expected news boosts prices in the stock market and lowers prices in the bond market throughout the 2003–2014 sample period.

Another contributing factor might have been the unconventional monetary policies since 2008, such as quantitative easing. The enlargement of the set of policy instruments, their magnitude, and the additional liquidity increase the direct impact of monetary policy on fixed income markets. Because monetary policy responds to macroeconomic data, the existence of a more-powerful-than-ever set of policy choices amplifies the relevance of macroeconomic announcements for financial markets. As the Federal Reserve continues to operate an expanded set of policy instruments and uses it in response to macroeconomic announcements, the rewards to informed trading prior to the official release time continue to be high.

General macroeconomic conditions and the related monetary policy are not the only changes in recent years. Not only do the procedures for releasing the announcements change but information collection and computing power also increase, which might enable sophisticated market participants to forecast some announcements. We discuss these explanations in Section 5.

4.5 Robustness Checks

In this subsection, we test whether our results are robust to (potential) impact of outliers, data snooping, event window length, effects stemming from other announcements, order flows having a different impact before the drift announcements, conditioning on sign of post-announcement return, asymmetries between positive and negative surprises, and choice of the asset market. All tests confirm robustness of our results.

4.5.1 Effect of 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 already tested robustness to outliers using the procedure of Yohai (1987) in Section 4.1. Here, we conduct an additional test by splitting surprises by size into deciles and estimating 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 4.[23] 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 6 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. Our results are, therefore, not driven by a few unusual, large observations.

Table 6: Announcement Surprise Impact During $[t-30min, t-5sec]$ by Decile

Surprise Size	Surprise Decile	n	E-mini S&P 500 Futures γ	R^2	10-year Treasury Note Futures γ	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 4 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.

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

4.5.2 Multiple Hypotheses Testing and Data Snooping

In Section 4 (for example, in Table 3), we test multiple hypotheses. When testing 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. In Section 4, we present results of squaring and summing the t-statistics; the resulting χ^2- statistic is significant at 1% level. In this section, we present another test. Failure to adjust the p-values can be viewed as data snooping. To rule out this possibility in our joint tests for 21 announcements, we use Holm (1979) step-down procedure. This procedure adjusts the 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)). Based on this conservative approach, four announcements ranked at the top of Table 3 show a significant drift (ISM Manufacturing, ISM Non-Manufacturing and Pending Home Sales at 1%, and Existing Home Sales at 5% significance levels).[24]

4.5.3 Event Window Length

The analysis in Section 4.1 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 equation (1) with $[t - \underline{\tau}, t - 5sec]$ for various $\underline{\tau} \in [5min, 120min]$. Figure A2 plots estimates of the corresponding γ_m coefficients for the seven drift announcements. The results confirm the conclusions from the lower panel of Figure A1: 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.

4.5.4 Effect of Other Recent Announcements

On some days, the market receives news about multiple announcements. Six out of the seven strong drift announcements follow 8:30 announcements on some days (Industrial

[24] We report these results in the Internet Appendix Table B1 along with a description of the data snooping robustness check procedure.

Production at 9:15, and CB Consumer Confidence Index, Existing Home Sales, ISM Manufacturing Index, ISM Non-Manufacturing Index and Pending Home Sales at 10:00). This opens the possibility that the pre-announcement drift is driven by a post-announcement reaction to earlier announcements because traders may be able to "improve" on the consensus forecast using data announced earlier in the day. We test for this possibility in two ways.

First, we add a control variable to the event-study equation (1) that measures the cumulative return from 90 minutes before to 30 minutes before the official release time t. For example, for 10:00 announcements this corresponds to the window from 8:30 to 9:30. This control variable is usually insignificant, and the results from Section 4.1 maintain, which is consistent with the CARs in Figure 1 remaining near zero until 30 minutes before release time.

Second, we employ a time-series approach following, for example, Andersen et al. (2003) where all announcements are embedded in a single regression. Here, the returns R_t are the first differences of log prices within a fixed time grid. We model this return, separately for each market, as a linear function of lagged surprises of each announcement to capture the impact that an announcement may have on the market in the following periods, lead values of each announcement surprise to capture the pre-announcement drift, and lagged values of the return itself to account for possible autocorrelation. We assume that the surprise process is exogenous and in particular not affected by past asset returns. We estimate an ordinary least squares regression where ε_t is an i.i.d. error term reflecting price movements unrelated to the announcements:

$$R_t = \beta_0 + \sum_{i=1}^{I} \beta_i R_{t-i} + \sum_{m=1}^{M} \sum_{j=0}^{J} \beta_{mj} S_{m,t-j} + \sum_{m=1}^{M} \sum_{k=1}^{K} \tilde{\beta}_{mk} S_{m,t+k} + \epsilon_t \tag{3}$$

We use 15-minute returns.[25] To measure the pre-announcement price drift, we use $K = 2$ leads of surprises. Their coefficients capture the effect in the $[t-30min, t-15min]$

[25]Ideally, we would use 5-minute returns to separate the effects of all release times (8:15, 8:30, 9:15, 9:55, 10:00, 14:00 and 15:00). We use 15-minute returns to keep the number of estimated parameters manageable. Because of the 15-minute returns, we omit the two University of Michigan Consumer Sentiment Index announcements released at 9:55, so $M = 28$.

and $[t-15min, t-5sec]$ windows, i.e., the windows for which we detect price drift in Section 4.

To control for potential effects of 8:30 announcements on 10:00 announcements on the same day, we use $I = 6$ lags of returns. Similarly, there is one contemporaneous and five lagged terms of each announcement surprise. To reduce the number of estimated parameters, we test the specification with $J = 5$ against a parsimonious $J = 1$ specification with only one contemporaneous and one lagged term of the surprise. The sum of surprise coefficients on lags 2 through 5 representing the $[t-30min, t-90min]$ window is rarely different from zero.[26] Since the pre-announcement drift coefficients do not differ when the number of lags is reduced, we follow the parsimony principle and report in Table 7 results for $J = 1$.[27]

The statistical test for the drift sums up the two coefficients of the surprise leads, $\tilde{\beta}_m$, and jointly tests the hypothesis that these sums for the stock and bond markets are different from zero. We reject this hypothesis at 5% significance level for the Industrial Production announcement and at 1% significance level for the other six announcements listed in Table 7. These results confirm that seven of the 21 market-moving announcements exhibit a strong pre-announcement price drift and suggest that the drift is not driven by forecast updating based on earlier announcements.

4.5.5 Effect of Order Flows

We verify that our results 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 $-$ $30min$,

[26] Only three of 28 announcements (GDP Advance, GDP Preliminary and ISM Manufacturing Index) show significance at 10% level. The sign is consistent with some return reversal during the $[t-30min, t-90min]$ window.

[27] This specification involves estimating 119 parameters: four terms for each of 28 announcements, one intercept and six lags of return. In intervals without a surprise for a given type of announcement, we set the corresponding surprise to zero. We have 1,680 observations with non-missing surprises.

Table 7: Announcement Surprise Impact During $[t - 30min, t - 5sec]$ (Time-Series Regression)

Announcement	E-mini S&P 500 Futures $[t - 30min, t - 5sec]$	10yr Treasury Note Futures $[t - 30min, t - 5sec]$	Joint Test p-value
CB Consumer confidence	0.035 (0.046)	-0.031 (0.011)***	0.010
Existing home sales	0.110 (0.047)**	-0.019 (0.010)*	0.010
GDP preliminary	0.137 (0.056)**	-0.022 (0.011)**	0.006
Industrial production	0.063 (0.026)**	-0.004 (0.010)	0.041
ISM Manufacturing	0.084 (0.034)**	-0.023 (0.010)**	0.003
ISM Non-manufacturing	0.167 (0.043)***	-0.072 (0.013)***	<0.001
Pending home sales	0.149 (0.072)**	-0.035 (0.011)***	<0.001

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 4 are shown to save space. The reported response coefficients are the estimates of $\tilde{\beta}_1 + \tilde{\beta}_2$ from equation (3). 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 sums of coefficients $\tilde{\beta}_1$ and $\tilde{\beta}_2$ for the E-mini S&P 500 and 10-year Treasury note futures are equal to zero.

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.[28]

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

$$\begin{aligned} sign\left(OF_{\tilde{m}t}\right) R_{\tilde{m}t} &= c + a_{\tilde{m}} + b_0 \sqrt{|OF_{\tilde{m}t}|} + b_1 I_{NoDrift}\left(\tilde{m}\right) \sqrt{|OF_{\tilde{m}t}|} \\ &+ b_2 I_{Drift}\left(\tilde{m}\right) \sqrt{|OF_{\tilde{m}t}|} + \varepsilon_{\tilde{m}t}, \end{aligned} \quad (4)$$

where $I_{NoDrift}(\tilde{m})$, and $I_{Drift}(\tilde{m})$ are indicator variables. $I_{NoDrift}$ equals 1 only if $\tilde{m}$ stands for an announcement without strong evidence of drift, and I_{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

[28]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.

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 absolute order flow and $sign\left(OF_{\tilde{m}t}\right)R_{\tilde{m}t}$ 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

$$\begin{aligned}\Delta sign\left(OF_{\tilde{m}t}\right)R_{\tilde{m}t} &= c_1 + b_0\Delta\sqrt{\left|OF_{\tilde{m}t}\right|} + b_1 I_{NoDrift}\left(\tilde{m}\right)\Delta\sqrt{\left|OF_{\tilde{m}t}\right|} \\ &+ b_2 I_{Drift}\left(\tilde{m}\right)\Delta\sqrt{\left|OF_{\tilde{m}t}\right|} + \Delta\varepsilon_{\tilde{m}t},\end{aligned} \tag{5}$$

where we keep an intercept and test whether it equals zero. Hence, testing the hypothesis that the impact of order flow on returns on announcement days with drift is the same as on other days is a simple t-test on the estimated coefficient for b_2. The results in Table 8 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 other days.

Table 8: 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 (5). Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5% and 1% levels, respectively.

4.5.6 Conditioning on Sign of Post-Announcement Return

The above analysis shows 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*.

For strong drift announcements, returns in the $[-30min, -5sec]$ window are strongly correlated with returns in the $[-5sec, +1min]$ window. Pooling the announcements by evidence of drift, the correlation of returns in these two windows is larger than 0.17 and highly significant in both the stock and Treasury note markets for strong drift announcements. In contrast, for no drift announcements this correlation is less than 0.05 in absolute value and insignificant in both markets.

We show CARs conditioned on the sign of the returns in the $[-5sec, 1min]$ window, following Ederington and Lee (1995), in Figure A3. The CARs suggest that the pre-announcement drift is in the direction of the post-announcement price move.[29]

4.5.7 Other Robustness Checks

We also test for asymmetries between positive and negative surprises as a robustness check. The results (available upon request) show no significant difference between the coefficients for positive and negative surprises. Finally, we conduct robustness checks based on other stock index and bond futures markets (E-mini Dow and 30-year Treasury bonds). The results[30] are similar to those in Table 4 which is consistent with other studies such as Baum, Kurov, and Wolfe (2015) who report that results do not differ much across markets within a given asset category.

5 Causes of Pre-Announcement Price Drift

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

[29]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 A3 (about a third) compared to Figure 1 (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.

[30]See Internet Appendix Table B2.

A popular explanation for a failure of the first requirement is information leakage. The corporate finance literature (for example, Sinha and Gadarowski (2010) and Agapova and Madura (2011)) considers price drift before public guidance issued by company management as de facto evidence of information leakage. Bernile et al. (in press) also point to information leakage as the cause of informed trading before the FOMC announcements. But at least one alternative explanation exists. Some traders may collect proprietary information which allows them to forecast announcements better than other traders. We investigate these two possible causes in Sections 5.1.1 and 5.1.2.

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 but not necessarily the best summary statistic of the forecasters' responses. 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 $\Delta > \underline{\tau}$, market expectations might change by time $t - \underline{\tau}$. We discuss these possible explanations in Section 5.2.

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 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 indicators 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 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 (Javers, 2013c).[31] Such timing difference creates profit opportunities for high-frequency traders (Y. Chang, Liu, Suardi, & Wu, 2014) and might entail an extremely fast price discovery (Hu et al., 2013). Although the CAR graphs in Section 4.2 show that information enters the market approximately half an hour before the release of the strong drift announcements and, therefore, the drift is not confined to high-frequency trading, this anecdotal evidence raises the possibility that organization type plays a role in our findings.

We, therefore, examine this possibility. Among our 21 market-moving announcements, there are thirteen PFEI and eight non-PFEI announcements as shown in Table 9. Five of

[31] Although Thomson Reuters argued that it had the right to provide tiered-services, the Security Exchange Commission started an investigation. Thomson Reuters suspended the practice following a probe by the New York Attorney General in July of 2013 (Javers, 2013a).

the eight non-PFEI announcements show a strong evidence of pre-announcement drift.

Table 9: Principal Federal Economic Indicators and Pre-release Procedures

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

[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 and Final 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.

With respect to release procedures, we are interested in the safeguards against premature dissemination. Surprisingly, many organizations do not have this information readily available on their websites. 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 9.

The first type used in five announcements involves posting the announcement on the organization's website that all market participants can access at the same time. The second type of release procedures used in twelve announcements 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 for the public.[32] A testimony in front of the U.S. House of Representatives by the U.S. Department of Labor (DOL) official responsible for lock-up security highlights challenges that new technologies create for preventing premature dissemination from these lock-up rooms (Fillichio, 2012). News media were allowed to install 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. 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 exploited the loose definition of what constitutes a media outlet and obtained access to the lock-up rooms even though the lock-up rooms are designed for media outlets in the journalism business. 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 removed its access to its lock-up room. Attesting to the fact that ensuring a secure 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. Four 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 Consumer Credit and Industrial Production announcements are 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 Consumer Confidence Index to a group of media out-

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

lets that had signed an agreement not to distribute the information prior to the release time; the pre-release was eliminated in June of 2013, and the information is now posted directly on the CB website. We mark these announcements as "embargo only" in Table 9.

We examine the possibility that the release procedures play a role in our findings. We note that three announcements with the least secure release procedure (CB Consumer Confidence Index, Industrial Production and Pending Home Sales) are among our seven strong drift announcements.

We proceed to test the role of organization type and release procedure statistically. We create three indicators: "PFEI" takes on value of 1 if the announcement is released by an organization required to follow PFEI procedures and 0 otherwise, "pre-release procedure" takes on value of 1 if the announcement is pre-released and 0 otherwise[33], and "embargo-only" takes on value of 1 if the announcement is pre-released under "embargo-only" procedures and 0 otherwise.

We interact these indicators with the surprise variable in equation (1) and estimate the regression using the pre-announcement window $[t - 30min, t - 5sec]$. With the exception of the CB Consumer Confidence Index where the release procedure changed during our sample period, the indicator variables are constant for a given announcement, which means that we need to pool all market-moving announcements in the same estimation even though this approach assumes the same coefficients for all announcements:

$$R_{t-\underline{\tau}}^{t+\overline{\tau}} = \gamma_0 + \gamma_1 S_t + \sum_{i=1}^{I} \delta_i X_{i,t} S_t + \varepsilon_t, \tag{6}$$

where γ_0 captures the unconditional price drift around the release time (Lucca & Moench, 2015), γ_1 captures the price drift around the release time conditional on the surprise, δ_i captures the effect of the indicator variables ($I = 3$), and ε_t is an i.i.d. error term reflecting price movements unrelated to the announcements.[34]

[33]Note that the pre-release variable does not capture leakage that might occur 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 time (Javers, 2012) or leakage via information technology systems accessed by hackers (SEC, 2015). 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.

[34]To be able to pool all market-moving announcements in the same estimation, we reverse the signs of

Table 10: Effect of Organization Type and Release Procedures

	E-mini S&P 500 Futures (1)	E-mini S&P 500 Futures (2)	10yr Treasury Note Futures (1)	10yr Treasury Note Futures (2)
Surprise	0.061 (0.018)***	0.036 (0.018)*	-0.022 (0.004)***	-0.019 (0.005)***
Sur.×PFEI	-0.052 (0.020)***	-0.102 (0.037)***	0.016 (0.005)***	0.023 (0.007)***
Sur.×Pre-release	n.a	0.078 (0.040)*	n.a	-0.011 (0.008)
Sur.×Embargo-only	n.a	-0.015 (0.029)	n.a	0.003 (0.006)

The sample period is from January 1, 2008 through March 31, 2014. The number of observations is 1675. The reported coefficients are the ordinary least squares estimates of equation (6) 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.

We present two specifications. The first specification (denoted by (1) in Table 10) includes the Surprise and Surprise×PFEI variables. The second specification (denoted by (2) in Table 10) includes the Surprise and interaction terms between the Surprise and all three indicator variables discussed above. The statistical significance of the drift in pre-released non-PFEI announcements is tested by the Wald test that sums the Surprise and Surprise×Pre-release coefficients. This Wald test is significant in both stock and bond markets at 1% significance level. As indicated in Table 9, all PFEI announcements are pre-released. Therefore, the significance of the drift in PFEI announcements is tested by the Wald test of the sum of the Surprise, Surprise×PFEI and Surprise×Pre-release coefficients. This Wald test is not significant in either market.[35] These results suggest that the pre-announcements drift is stronger in non-PFEI announcements.

The "embargo-only" variable is not significant. However, it needs to be noted that the small number of market-moving announcements does not allow designing a rigorous test that would definitively uncover leakage, and caution needs to be exercised in interpreting these results. A thorough analysis of individual trader data would be needed to fully

the stock and bond returns before the Initial Jobless Claims releases as well as the signs of the stock returns before the CPI releases. This model specification does not account for simultaneous announcements. However, if simultaneous announcements are dropped from the sample, the regression results are almost identical.

[35] We also examine whether the following three variables affect the drift: publication lag, number of professional forecasters and standard deviation of individual forecasts. The publication lag might matter if more forecasting effort goes into more up-to-date announcements, given the evidence in Gilbert et al. (2016) that earlier announcements move markets more. A higher average number of professional forecasters might make it more difficult to produce a superior forecast for announcements. The average standard deviation of individual forecasts measures the dispersion of beliefs among professional forecasters. Interaction terms between these variables and the announcement surprise were insignificant.

examine the leakage question.[36]

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 this proprietary information is never published, it remains a noisy private signal of the official announcement and has similar effects as leakage in Brunnermeier (2005). The nature of proprietary information usually makes it impossible for researchers to verify its existence.[37] 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.

Examples of such thorough proprietary data collection are State Street's daily scraping of online prices ("PriceStats") to estimate the U.S. inflation, the State Street Investor Confidence Index measuring confidence based on buying and selling activity of institutional investors, and the Case-Shiller Home Price index by S&P Dow Jones. The automatically collected PriceStats data can be used internally for trading in almost real time, but it is available to the public only with a delay. We test whether information at its collection time (when it was still proprietary) is useful for forecasting related macroeconomic announcement surprises by regressing the announcement surprise, S_{mt}, on the proprietary data.

Indeed, we find predictive power of the PriceStats inflation indicator for the CPI surprise. However, the State Street Investor Confidence Index does not have predictive

[36] This data is available only to the futures exchanges and the Commodity Futures and Trading Commission (CFTC) that oversees the U.S. futures markets.

[37] Examples of proprietary data that does not later become publicly available is data purchased on a subscription basis such as credit-card spending data of MasterCard ("SpendingPulse") and data from flying surveillance helicopters over industrial complexes reported, for example, by Rothfeld and Patterson (2013). Furthermore, trading platforms may gain additional proprietary information by monitoring order flows.

power for the CB Consumer Confidence Index surprise, and the Case-Shiller Home Price index does not have predictive power for the housing sector announcements. Although we cannot perform comprehensive tests of this proprietary information hypothesis for all announcements, the results (available upon request) suggest that early access to proprietary information permits forecasting announcement surprises in some cases.

5.2 Public Information

We now turn to the possibility that published market expectations are mismeasured or not optimal forecasts.

5.2.1 Mismeasurement of Market Expectations

Generating measures of market expectations from surveys faces two difficulties: first, ensuring truthful reporting by participants, and second, summarizing the individual responses in a meaningful aggregate measure. Survey participants with an informational advantage might have no incentive to reveal their information truthfully, and, therefore, the Bloomberg expectations may not give a comprehensive picture of the information in the market. But even if they do, the aggregation of individual responses implemented by Bloomberg might further bias the surprise variable.

The definition of a surprise in equation (2) requires information of market expectations, $E_{t-\underline{\tau}}[A_{m,t}]$, to become operational. Section 4 uses the consensus forecast, a common approach in the literature (Balduzzi et al., 2001). However, the calculation of this consensus forecast by Bloomberg is not innocuous: Bloomberg equal-weights the individual forecasts, which is not optimal in general. Some investment institutions indeed place considerable resources in building models of announcement surprises. We discussed these modelling techniques with several economists who work in these investments institutions. For example, one confirmed that he has a list of professional forecasters he follows for each announcement. The list is based on his experience and transcends the Bloomberg survey. Before an announcement release, he calls the forecasters on his list and updates his forecast accordingly. Although the mechanics of this updating procedure were not disclosed to us, we explore modelling of the announcement surprises. We use the individual forecasts attempting to construct a forecast that outperforms the Bloomberg consensus forecast.[38] If the surprises are predictable with individual forecasts but most traders rely on the consensus forecasts, traders with superior forecasts may trade on these predictions before the announcement, which could explain the price drift.[39]

Here, we build on previous research that uses individual forecasts. Energy markets, for example, react more to inventory forecasts by professional forecasters with a track record of higher forecasting accuracy (C. 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.

Bloomberg provides a rank for up to ten active professional forecasters who have issued accurate forecasts for previous months. The set of ranked forecasters is a strict subset of all forecasters submitting a forecast for a specific announcement. 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.[40] The Bloomberg ranking is based on information up to the time of the announcement release including the current release. To avoid a forward-looking bias, we use only the professional forecasters ranked *before* the announcement. We use this variable as a predictor of the actual announcement, A_{mt}. Because the surprise appears to explain the pre-announcement price

[38]Bloomberg forecasts are not available to the general public, but they are available to Bloomberg subscribers which comprise major traders in the stock index and Treasury futures markets.

[39]The pre-announcement price drift could also be caused by correlated news received by *all* market participants during the pre-announcement period. However, we are not aware of any such news regularly arriving within 30 minutes before the drift announcements.

[40]Since some individual forecasters submit their forecasts days before the releases as described in Section 3 and Bloomberg equal-weights the forecasts, we also test whether more up-to-date forecasts are better predictors of the surprise. The results (available upon request) show that removing stale forecasts does not improve forecasts of the surprise.

drift documented in Section 4, a good forecast should be highly correlated with it. To avoid estimation of additional parameters, we consider a forecast of the *unstandardized* surprise:

$$\widetilde{S}_{mt} = A_{mt} - E_{t-\underline{\tau}}[A_{mt}] = \sigma_m S_{mt}. \tag{7}$$

Our forecast of the surprise based on the ranked consensus is

$$P_{mt} = E_{t-\underline{\tau}}^{Ranked}[A_{mt}] - E_{t-\underline{\tau}}[A_{mt}], \tag{8}$$

which is the difference between the median values of the professional forecasters ranked by Bloomberg and the whole set of forecasters in the Bloomberg survey. We expect P_{mt} to be a reasonable forecast of $\widetilde{S}_{mt}$. We recursively regress the unstandardized surprise, $\widetilde{S}_{mt}$, on a constant and the prediction, P_{mt}. Nine announcements show significance of the slope coefficient at 10% level.[41]

The forecast error in predicting the next surprise is $\tilde{S}_{mt} - P_{mt}$. We compare this forecast error with a no-surprise benchmark where the forecast error is based on $P_{mt} = 0$. Using the Diebold-Mariano test (Diebold & Mariano, 1995; Diebold, 2015), we test the null hypothesis $H_0 : E\left[\tilde{S}_{mt} - P_{mt}\right]^2 = E\left[\tilde{S}_{mt}\right]^2$ against the alternative hypothesis $H_1 :$ $E\left[\tilde{S}_{mt} - P_{mt}\right]^2 < E\left[\tilde{S}_{mt}\right]^2$.

Table A1 in the Appendix shows the results. The improvement over the zero surprise forecast is significant at 10% level for five of the 21 market-moving announcements. However, these improvements in forecastability of the surprise do not help explain the drift results in Table 4. Two announcements (Existing Home Sales and Industrial Production) show a drift in Table 4 but the other three announcements (CPI, Durable Goods Orders and UM Consumer Sentiment - Final) do not.[42] To test for this relation more formally,

[41]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 UM Consumer Sentiment - Final. Detailed results are reported in the Internet Appendix B.2.

[42]We also conducted the same tests using more complicated methods of combining the individual forecasts similar to Brown et al. (2008), more advanced econometric techniques such as the complete subset regression of Elliott, Gargano, and Timmermann (2013), and insights from S. D. Campbell and Sharpe (2009) who find that announcement surprises can be partially explained by past surprises due to the anchoring bias in consensus forecasts. The results (available upon request) show that we can improve on the Bloomberg consensus forecast in six announcements but the conclusions are not qualitatively different because the improvements in forecastability of the surprise do not help us explain drift results

we analyze correlation between the log of the Wald statistic from Table 3 and the Diebold and Mariano statistic from Table A1. This correlation coefficient is negative (-0.43) at 10% significance level indicating that improved forecastability does not help explain the drift.

5.2.2 Forecasting Surprises with Other Public Information

In this subsection, we conduct a forecasting exercise similar to the one in Section 5.1.2 with various *publicly* available information. Although exhaustive testing of forecasting surprises with any conceivable public information is infeasible, the below anecdotal evidence (details available upon request) suggests that public information does allow forecasting announcement surprises in some cases. However, neither these results nor those in Section 5.1.2 are sufficiently comprehensive to prove that proprietary or public data is indeed used for informed trading around the release time.

Forecasting with Other Announcements In a frictionless market, all public information should be instantaneously reflected in expectations and prices. If instantaneous and complete revision of expectations is costly, publicly available information might allow forecasting the announcement surprises. We use the surprise in one announcement to forecast the surprise in another announcement. For example, we use the UM Consumer Sentiment Preliminary surprise (released on average on the 13^{th} day of each month) to forecast the CB Consumer Confidence Index surprise (released on average on the 27^{th} day of each month) and find predictive power. Similarly, we test whether the CPI surprise forecasts the PPI surprise and vice versa. In about 85% of the months in our sample, the CPI announcement is released one to five days after the PPI announcement. We, therefore, use the PPI surprise to forecast the CPI surprise in these months and find predictive power. In the other months when the CPI is released first, the CPI surprise predicts the subsequent PPI surprise.

in Table 4.

Forecasting with Internet Activity Data Here, we use internet search engine activity data. This data reflects interest in acquiring information, and several recent studies have shown that it is useful for forecasting numerous variables (for example, Choi and Varian (2012) for unemployment claims, consumer confidence and automobile sales, and Da, Engelberg, and Gao (2011) for stock prices). The data is publicly available from Google via the Google Trends service since January 2004. Google Trends groups search terms into numerous categories. We use search activity in the "Jobs" category to forecast announcement surprises because it is particularly relevant for the macroeconomy. For example, we find predictive power for the Initial Jobless Claims surprise but not for the CB Consumer Confidence Index surprise.

Bandwagon Effect A possibility arises that uninformed speculators are able to "jump on the bandwagon" with informed traders by observing the trading activity and returns before the announcement.[43] However, it is important to recognize that 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 177,000 and 62,000 contracts in the E-mini S&P 500 and 10-year Treasury note futures, respectively. This high level of trading activity likely allows informed traders to camouflage their information and trade profitably before announcement releases.[44]

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 $[t - 15min, t - 5sec]$ window. Such correlations are not significant, suggesting that simply observing price movements cannot be easily used to trade profitably ahead of announcements.

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

[44] See, for example, Kyle (1985) and Admati and Pfleiderer (1988) for a theoretical exposition of how informed speculators trade strategically to avoid revealing their information in the price.

6 Conclusion

We find evidence of substantial pre-announcement informed trading in equity index and Treasury futures markets for seven out of 21 market-moving U.S. macroeconomic announcements. 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 39 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. Failing to account for the pre-announcement effect substantially underestimates the total influence that these macroeconomic announcements exert in the financial markets. Importantly, we also show that the price drift has increased since 2007.

We examine two possible sources of informed trading: information leakage and superior forecasting. Some of the superior forecasting ability may be based on smart reprocessing of publicly available data. Superior forecasts of the announcement surprises may also be generated by "digging deeper" into pre-packaged information products, for example, by using forecasts by individual professional forecasters instead of the Bloomberg consensus forecast. Further improvements in forecasting may be due to resource-intensive legwork creating original proprietary datasets that proxy the data underlying public announcements.

The small number of market-moving announcements makes it difficult to definitively rule out either information leakage or superior forecasting. Despite this limitation, our evidence suggests that organizations that are not subject to the Principal Federal Economic Indicator guidelines are prone to pre-announcement drift. We also note that three of our drift announcements are released under rather lax procedures. Whether the drift in announcements with seemingly stronger safeguarding of data is also due to leakage or massive data collection and forecasting power of some market participants remains an open question. It is also conceivable that several factors combine to cause the drift.

Considering the public and regulatory attention that leakage has received especially due to the recent hacking scandal, the *source* of informed trading merits more research in view of the public interest in the safeguarding of macroeconomic data. Of particular interest will be the effect of proprietary realtime 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

The pre-announcement price drift of almost 50% of the total announcement impact shown in Table 5 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 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

$$E[X|A_1] \equiv \mu_{X1} = \rho_{X1}^{-1}(\rho_{X0}\mu_{X0} + \rho_{A1}A_1) \tag{9}$$

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

$$E[X|A_1, A_2] \equiv \mu_{X2} = \rho_{X2}^{-1}(\rho_{X1}\mu_{X1} + \rho_{A2}A_2) \tag{10}$$

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 (9) and (10), 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)

$$p_1 - p_0 > p_2 - p_1 \Leftrightarrow \frac{\rho_{A1}}{\rho_{A2}} + \frac{\rho_{A1}}{\rho_{X0} + \rho_{A1}} > \frac{S_2}{S_1} \tag{11}$$

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.

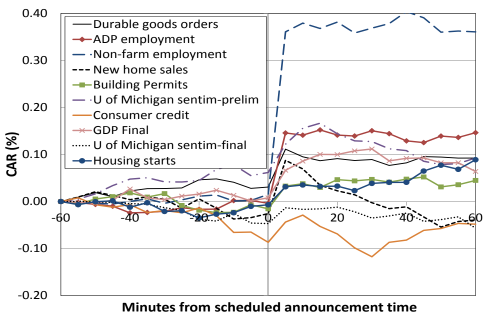
The proposition shows that even vague proprietary information can have a large price impact. To see this in a specific example, suppose that there is no prior public information ($\rho_{X0} \to 0$), and that the pre-release information is less precise and less surprising than the official release later on ($\rho_{A2} = 2\rho_{A1}$, $S_2 = 1.5 S_1$). Substituting into condition (11), 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.

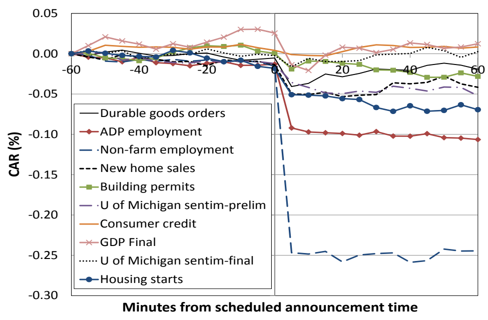
A.2 Additional Figures and Tables

Figure A1: Cumulative Average Returns for Individual Announcements

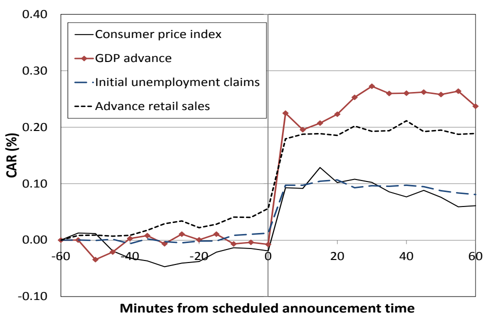
E-mini S&P 500 Futures | **10-year Treasury Note Futures**

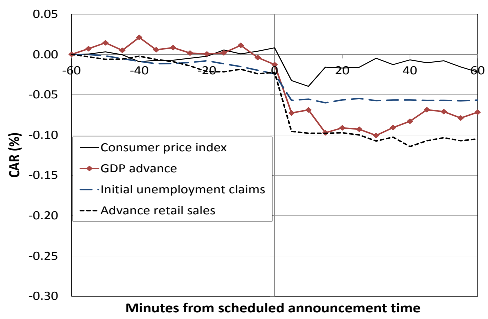
(a) Announcements with no evidence of drift



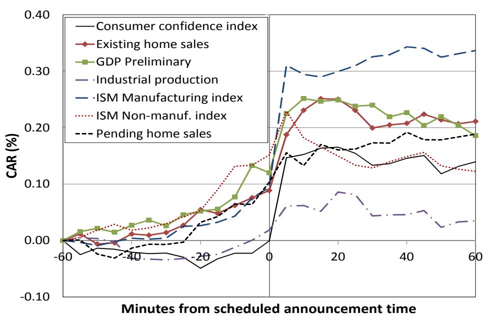


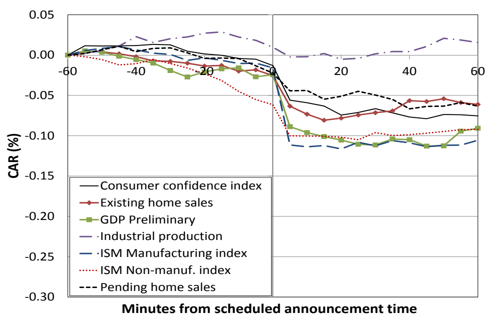
(b) Announcements with some evidence of drift





(c) Announcements with strong evidence of drift

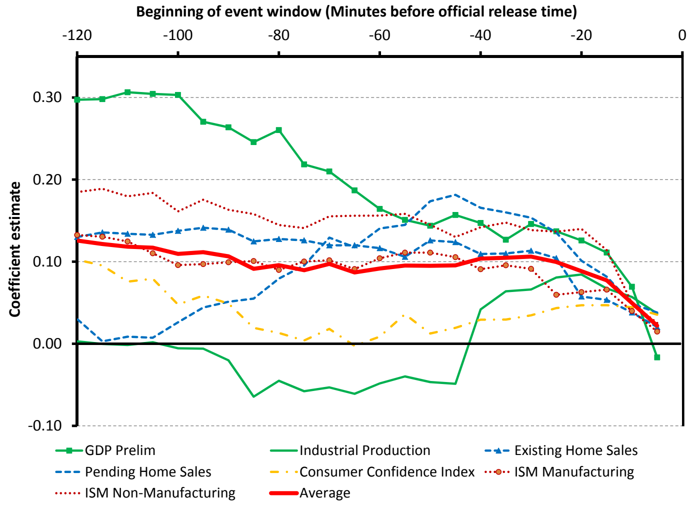




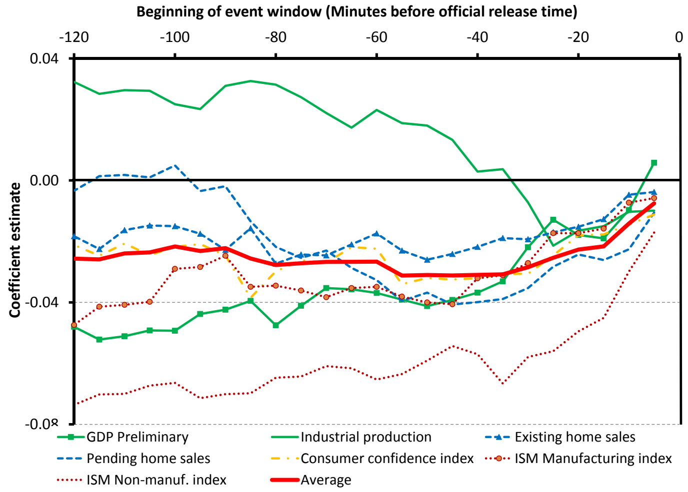
Announcements are categorized as no drift, some drift or strong drift using the classification in Table 4. For each category we compute mean cumulative average returns in the event window from 60 minutes before the release time to 60 minutes after the release time. The sample period is from January 1, 2008 through March 31, 2014.

Figure A2: Sensitivity of Coefficients to Event Window Length

(a) E-mini S&P 500 Futures



(b) 10-year Treasury Note Futures

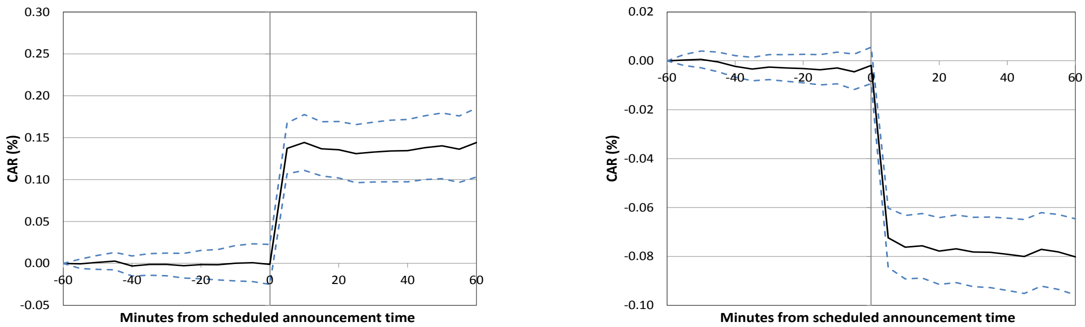


The sample period is from January 1, 2008 through March 31, 2014. The figure plots response coefficients, γ_m, based on the ordinary least squares estimates of equation (1) against $\underline{\tau}$, the beginning of the pre-announcement window $[t - \underline{\tau}, t - 5sec]$, for seven strong drift announcements identified in Table 4.

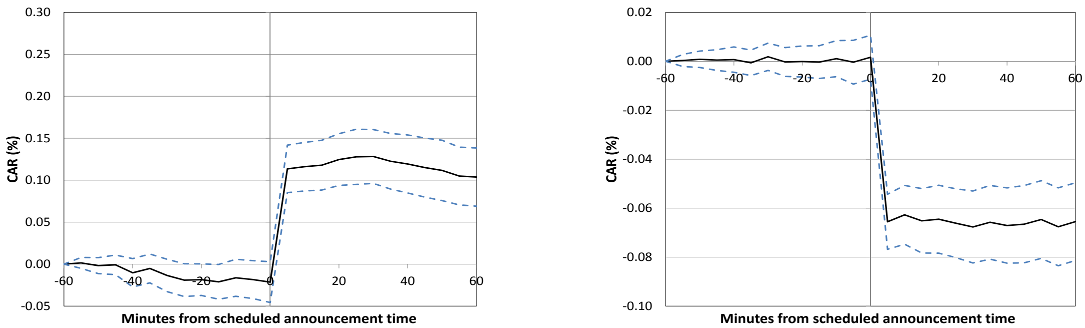
Figure A3: Cumulative Average Returns Conditional on Sign of Return in $[-5sec, 1min]$ Window

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

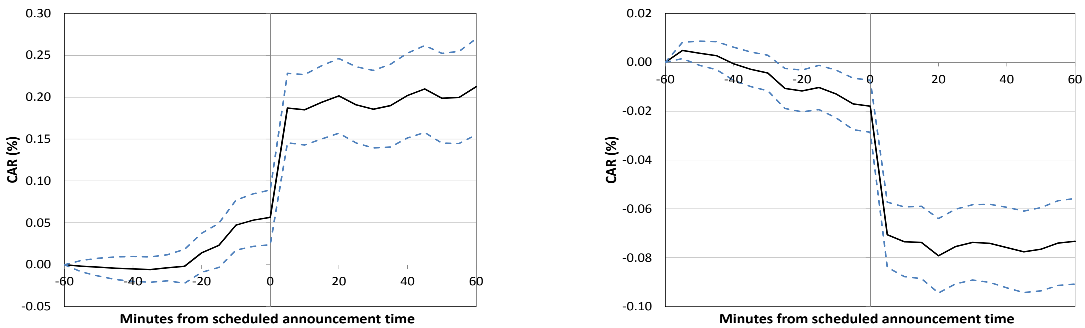
(a) Announcements with no evidence of drift



(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 multiply all 5-minute returns by the sign of the return in the $[t - 5sec, t + 1min]$ window. 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 4. For each category the solid line shows the mean CAR. Dashed lines mark two-standard-error bands (standard error of the mean).

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

	DM-Stat	p-value
ADP employment	-1.062	0.856
Advance retail sales	0.687	0.246
Building permits	-7.764	1.000
CB Consumer confidence index	1.010	0.156
Consumer credit	-5.110	1.000
Consumer price index	2.813	0.002
Durable goods orders	2.555	0.005
Existing home sales	1.316	0.094
GDP advance	0.996	0.160
GDP final	-3.195	0.999
GDP preliminary	-0.747	0.772
Housing starts	-0.827	0.796
Industrial production	1.806	0.035
Initial jobless claims	-0.414	0.660
ISM Manufacturing index	0.709	0.239
ISM Non-manufacturing index	-0.701	0.758
New home sales	-0.507	0.694
Non-farm employment	-1.612	0.946
Pending home sales	0.683	0.247
UM Consumer sentiment - Final	1.465	0.071
UM Consumer sentiment - Prel	0.373	0.355

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

B Internet Appendix (Not for Publication)

B.1 Additional Robustness Checks

B.1.1 Holm's Step-down Procedure

The Holm (1979) step-down procedure adjusts the hypothesis rejection criteria to control the probability of encountering one or more type I errors. Denote the hypotheses by $H_1, ..., H_m$ where $m = 21$ because there are 21 market-moving announcements in Table 3. Denote the corresponding p-values by $p_1, ..., p_m$. Consider the significance level of 0.05. The procedure orders the Table 3 joint test p-values from the lowest to the highest. Denoting the ordered hypotheses by $k = 1 \dots 21$, it computes $\frac{0.05}{m+1-k}$ for each k, and compares this computed value to the Table 3 p-value. The null hypothesis of no drift is rejected if $\frac{0.05}{m+1-k} >$ Table 3 p-value.

Table B1: Holm's Step-down Procedure

Announcement	Table 3 Joint Test p-value	$\frac{0.05}{m+1-k}$	Null Hypothesis of No Drift Rejected
ISM Non-manufacturing index	0.0001	0.0024	Yes
Pending home sales	0.0005	0.0025	Yes
ISM Manufacturing index	0.0006	0.0026	Yes
Existing home sales	0.002	0.0028	Yes
CB Consumer confidence index	0.007	0.0029	No
GDP preliminary	0.013	0.0031	No
Industrial production	0.013	0.0033	No
Housing starts	0.112	0.0036	No
Non-farm employment	0.123	0.0038	No
Advance retail sales	0.190	0.0042	No
Consumer credit	0.271	0.0045	No
ADP employment	0.291	0.0050	No
UM Consumer sentiment - Final	0.361	0.0056	No
Initial jobless claims	0.369	0.0063	No
New home sales	0.539	0.0071	No
Building permits	0.567	0.0083	No
GDP advance	0.608	0.0100	No
GDP final	0.739	0.0125	No
UM Consumer sentiment - Prel	0.845	0.0167	No
Durable goods orders	0.852	0.0250	No
Consumer price index	0.981	0.0500	No

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 2) are included.

B.2 Forecasting the Announcement Surprise Using Individual Forecasts

As described in Section 5.2.1, we regress the unstandardized surprise, $\widetilde{S}_{mt}$, on a constant and the prediction, P_{mt}. The results for this regression are reported in Table B3 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. Eight announcements show significance of the slope coefficient at 10% level (Advance Retail Sales, CB Consumer Confidence Index, CPI, Durable Goods Orders, Existing Home Sales, GDP Advance, Industrial Production and Pending Home Sales).

Table B2: Robustness Check with Other Markets: Announcement Surprise Impact During $[t - 30min, t - 5sec]$ for E-mini Dow and 30-year Treasury Bond Futures

Announcement	E-mini Dow Futures γ_m	E-mini Dow Futures R^2	30yr Treasury Bond Futures γ_m	30yr Treasury Bond Futures R^2	Joint Test p-value
ISM Non-manufacturing	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	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	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 sent. - 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 sent. - 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 2) 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 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.

The results from Table B3 show that there is a significant linear relation between the predictions and surprises, but they do not necessarily imply that the forecasts have superior predictive power for *futures returns*. To explore this, we estimate equation (1) using the prediction, P_{mt}, instead of the surprise, S_{mt}. Table B4 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 B4 Panel b) reports the results for the $[t - 5sec, t + 5min]$ window. Again, returns can be forecast using the prediction, P_{mt}, only in a handful of announcements, and there does not appear to be any relation between these results and drift results in Table 4.

Table B3: Regression of Unstandardized Surprise, $\widetilde{S}_{mt}$, 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
Building permits	-0.013	0.030	0.669	0.02
CB Consumer confidence index	1.188	0.586	0.021	0.06
Consumer credit	-0.086	0.099	0.806	0.02
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
UM Consumer sentiment - Final	0.555	0.496	0.132	0.03
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 $\widetilde{S}_{mt} = A_{mt} - E_{t-\underline{\tau}}[A_{mt}] = \sigma_m S_{mt}$. 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 B4: Regression of Returns on Prediction

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

	E-mini S&P 500 Futures			10yr Treasury Note Futures			Wald Test	
	γ_m	s.e.	R^2	γ_m	s.e.	R^2	stat.	p-val.
ADP employment	0.030	0.015	0.03	-0.019	0.007	0.09	11.11	<0.01
Advance retail sales	0.002	0.019	0.01	-0.009	0.010	0.02	0.78	0.68
Building permits	-0.008	0.020	0.02	0.002	0.011	0.01	0.20	0.92
CB Consumer confidence	-0.004	0.039	0.01	-0.019	0.007	0.06	7.79	0.02
Consumer credit	-0.011	0.032	0.01	0.010	0.006	0.03	2.59	0.27
Consumer price index	0.001	0.022	0.01	-0.002	0.009	0.01	0.05	0.98
Durable goods orders	0.019	0.013	0.03	-0.007	0.007	0.03	3.33	0.19
Existing home sales	0.014	0.065	0.01	-0.021	0.018	0.05	1.42	0.49
GDP advance	0.087	0.055	0.19	-0.016	0.016	0.07	3.50	0.17
GDP preliminary	0.005	0.044	0.04	-0.007	0.013	0.05	0.28	0.87
GDP Final	-0.001	0.028	0.04	-0.022	0.013	0.12	3.09	0.21
Housing starts	0.006	0.016	0.01	-0.015	0.006	0.04	6.96	0.03
Industrial production	0.012	0.020	0.02	-0.002	0.005	0.07	19.14	<0.01
Initial jobless claims	-0.025	0.010	0.02	0.006	0.005	0.01	7.34	0.03
ISM Manufacturing	-0.010	0.070	0.01	0.004	0.014	0.02	0.11	0.95
ISM Non-manufacturing	0.012	0.032	0.01	-0.009	0.017	0.02	0.38	0.83
New home sales	-0.015	0.030	0.02	-0.008	0.006	0.03	2.17	0.34
Non-farm employment	0.009	0.019	0.02	-0.006	0.011	0.02	0.51	0.77
Pending home sales	-0.023	0.032	0.02	-0.012	0.007	0.03	3.65	0.16
UM Consumer sent. -Final	0.041	0.022	0.04	-0.015	0.010	0.03	5.56	0.06
UM Consumer sent. - Prel	-0.076	0.036	0.04	0.001	0.009	0.01	4.56	0.10

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

	E-mini S&P 500 Futures			10-year Treasury Note Futures			Wald Test	
	γ_m	s.e.	R^2	γ_m	s.e.	R^2	stat.	p-val.
ADP employment	-0.001	0.023	0.01	0.018	0.013	0.03	2.03	0.36
Advance retail sales	0.043	0.031	0.04	-0.020	0.014	0.03	3.94	0.14
Building permits	0.035	0.021	0.05	-0.005	0.013	0.02	2.93	0.23
CB Consumer confidence	0.016	0.037	0.02	0.001	0.010	0.01	0.21	0.90
Consumer credit	-0.008	0.013	0.02	-0.001	0.003	0.02	0.58	0.75
Consumer price index	-0.040	0.035	0.03	-0.006	0.012	0.02	1.54	0.46
Durable goods orders	0.046	0.020	0.07	-0.027	0.011	0.08	11.14	<0.01
Existing home sales	-0.039	0.031	0.03	-0.009	0.013	0.02	2.09	0.35
GDP advance	-0.015	0.089	0.04	0.035	0.023	0.09	2.27	0.32
GDP Final	0.069	0.047	0.13	0.006	0.012	0.04	2.46	0.29
GDP preliminary	-0.055	0.037	0.07	0.040	0.021	0.17	5.88	0.05
Housing starts	0.021	0.019	0.03	-0.005	0.008	0.02	1.69	0.43
Industrial production	0.000	0.014	0.01	0.003	0.004	0.02	0.60	0.74
Initial jobless claims	-0.018	0.013	0.00	0.004	0.005	0.00	0.87	0.65
ISM Manufacturing	0.004	0.040	0.01	-0.001	0.017	0.01	0.02	0.99
ISM Non-manufacturing	0.022	0.033	0.02	-0.005	0.008	0.02	0.89	0.64
New home sales	0.020	0.022	0.02	0.005	0.009	0.02	1.21	0.55
Non-farm employment	-0.066	0.076	0.03	0.020	0.043	0.02	0.96	0.62
Pending home sales	-0.016	0.038	0.02	0.016	0.006	0.06	8.11	0.02
UM Consumer sent. - Final	-0.014	0.018	0.02	0.003	0.005	0.02	0.90	0.64
UM Consumer sent. - Prel	0.019	0.020	0.02	0.002	0.006	0.01	0.95	0.62

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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