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

Alexander Kurov† Alessio Sancetta‡ Georg Strasser§

Marketa Halova Wolfe¶

First Draft: June 15, 2014
This Draft: May 1, 2015

Abstract

We examine the price behavior of stock index and Treasury futures around the release of U.S. macroeconomic announcements. Seven out of 18 market-moving announcements show evidence of informed trading before the official release time. For these announcements, prices begin to drift in the "correct" direction about 30 minutes before the announcement time. The pre-announcement drift accounts on average for about half of the total price adjustment. We consider two explanations for this pre-announcement drift: information leakage and superior forecasting. Release procedures that may affect information leakage vary greatly across organizations. We find some evidence of predictability of announcement "surprises" based on reprocessing of public information and on private data collection, which might explain the drift.

Keywords: Macroeconomic news announcements; pre-announcement effect; financial markets; drift; informed trading

JEL classification: E44; G14; G15

*We thank Alan Love, Harry Turtle and seminar participants at the Federal Reserve Bank of St. Louis, Washington State University and West Virginia University for helpful comments. We also thank George Jiranek and Dan Neagu for research assistance. Errors or omissions are our responsibility.

†Associate Professor, Department of Finance, College of Business and Economics, West Virginia University, P.O. Box 6025, Morgantown, WV 26506, Phone: +1-304-293-7892, Email: alkurov@mail.wvu.edu

‡Professor, Department of Economics, Royal Holloway, University of London, Egham Hill, Egham, Surrey, TW20 0EX, United Kingdom, Phone: +44-1784-276394, Email: Alessio.Sancetta@rhul.ac.uk

§Assistant Professor, Department of Economics, Boston College, 140 Commonwealth Avenue, Chestnut Hill, MA 02467-3806, Phone: +1-617-552-1954, Email: Georg.Strasser@bc.edu

¶Assistant Professor, School of Economic Sciences, Washington State University, Pullman, WA 99164, Phone: +1-509-335-8509, Email: marketa.halova@wsu.edu

1 Introduction

Numerous studies, such as Andersen, Bollerslev, Diebold, and Vega (2007), have shown that U.S. macroeconomic news announcements move financial markets. These announcements are considered the quintessential updates to public information on the state of the economy and fundamental inputs to asset pricing. More than half of the cumulative annual equity risk premium is earned on announcement days (Savor & Wilson, 2013). Because of their importance and to ensure fairness, no market participant should have access to this information until the official release time. Once released, the information is almost instantaneously reflected in prices (Hu, Pan, & Wang, 2013). Yet, in this paper we find evidence of informed trading before several key macroeconomic news releases.

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. Twelve out of 18 announcements that move the markets exhibit some pre-announcement price drift, and for seven of these announcements the drift is substantial. Prices start to move about 30 minutes before the announcements and the price move during this pre-announcement time window accounts on average for about one half of the total price adjustment. These results show that informed trading is not limited to corporate announcements documented by, for example, Campbell, Ramadorai, and Schwartz (2009), Sinha and Gadarowski (2010), Agapova and Madura (2011) and Kaniel, Liu, Saar, and Titman (2012), but exists in macroeconomic announcements as well.

Previous studies on macroeconomic announcements can be categorized into two groups with regards 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 announcement release time t, it shows that prices react to macroeconomic news. However, it remains unclear how much of the price move occurs prior to 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 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 18 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 (as it is the case in our data), it will not get detected in such small increments. Our methodology uses a longer pre-announcement interval and uncovers a price drift.

Second, other studies consider short pre-announcement intervals. The study by Andersen et al. (2007) for example includes only ten minutes before the official release time. They find in a sample of 25 U.S. announcements from 1998 to 2002, that global stock, bond and foreign exchange markets react to announcements only after their official release time. We show in this paper that the pre-announcement interval has to be about 30 minutes long to fully 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 of announcements can give the incorrect impression that pre-announcement drift is an exotic outlier.

Fourth, the difference in results may be due to parameter instability. Not only announcement release procedures change over time but also the information collection and computing power increase, which might enable sophisticated market participants to forecast some announcements. Most findings in this paper are based on second-by-second data starting on

January 1, 2008. To compare our results to previous studies using older sample periods, we use a minute-by-minute sample extended back to January 1, 2003. The results, discussed in Section 4.4.4, 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 (2015) 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 the years 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. (2015) suggest these findings are consistent with information leakage.[1]

Our study differs from Hautsch et al. (2011) and Bernile et al. (2015) in two important ways. First, our methodology and a 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 consider other possible sources of private information about public announcements.

The corporate finance literature regards price drift before public guidance issued by company management as de facto evidence of information leakage (see e.g. Sinha and Gadarowski (2010) and Agapova and Madura (2011)). In Section 5.1 we explore the information leakage

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

explanation by examining two aspects of the release process of market-moving announcements: organization type and release procedures.[2]

With respect to organization type we distinguish public from private entities. The U.S. macroeconomic data is generally considered closely guarded. Federal agencies restrict the number of employees with access to the data, implement computer security measures, and take other actions to prevent premature dissemination. But ensuring that all 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. Some data providers have been known to release information to exclusive groups of subscribers before making it available to the general public. For example, Thomson Reuters created a high-speed data feed for paying subscribers where the Consumer Confidence Index prepared by the University of Michigan was released two seconds earlier, and the Manufacturing Index prepared by the Institute of Supply Management (ISM) was released milliseconds, possibly up to 10 seconds, earlier (Rogow, 2012; Javers, 2013c).[3] These seemingly small timing differences create profit opportunities for high-frequency trading (Chang, Liu, Suardi, & Wu, 2014), but might also entail an extremely fast price discovery (Hu et al., 2013). In our cross-regression, announcements released by private organizations have a stronger pre-announcement drift in the 10-year Treasury futures market.

With respect to the release procedures, the data handling, prerelease and lockup room procedures, and the official dissemination channels are of interest. Surprisingly, many organizations do not have this information readily available on their websites. We conducted a phone and email survey of the organizations in our sample. There are two types of release procedures. The first type involves posting the announcement on the organization's website

[2]Macroeconomic announcement leakage has been documented in multiple countries. For example, Andersson, Overby, and Sebestyén (2009) analyze news wires and present evidence of the German employment report being regularly known to investors prior to the official releases. Information leakage has also occurred in other settings, for example, in the London PM gold price fixing (Caminschi & Heaney, 2013).

[3]Although Thomson Reuters argued that it had a 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, 2013b).

that all market participants can access (ideally at the same time). The second type involves prereleasing the information to journalists. Usually, this prerelease occurs in designated "lock-up rooms." 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). For example, initially cell phones were supposed to be stored in a designated container. Despite this, one individual accessed and used his phone during the lock-up. On another occasion, a wire service accidentally transmitted the data during the lock-up period. Two announcements in our dataset are not prereleased in lock-up rooms; instead, they are electronically transmitted to journalists who are asked not to share the information with others ("embargo"). These two announcements are among the seven announcements with drift.

But information leakage is only one possible cause of pre-announcement price drift. In this paper we aim to consider any private information produced by informed investors and impounded into prices through trading (French & Roll, 1986).[4] Some traders may be able to analyze public information in a superior way or to collect information themselves to forecast the announcements better than others. This knowledge can then be utilized to trade in the "correct" direction before the announcements. We conduct extensive forecasting exercises in Section 5.2. We are able to forecast announcement surprises in some announcements but find no relationship between the forecastability of a surprise and pre-announcement drift. Further research is therefore needed to determine the *source* of informed trading.

The rest of this paper is organized as follows: The next two sections describe the methodology and data, and 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.

[4] In the corporate finance literature on trading around company earnings announcements, Campbell et al. (2009) and Kaniel et al. (2012) also remain agnostic about the source of informed trading by institutional and individual investors, respectively.

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 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 the end of the intraday event window $[t - \underline{\tau}, t + \overline{\tau}]$. The reaction of asset returns to the surprise can be captured by the ordinary least squares regression

$$R_t = \gamma_0 + \gamma_m S_{mt} + \varepsilon_t, \tag{1}$$

where 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, $E_{m,t-\Delta}[A_{mt}]$. Specifically,

$$S_{mt} = \frac{A_{mt} - E_{m,t-\Delta}[A_{m,t}]}{\sigma_m}. \tag{2}$$

We proxy the expectation $E_{m,t-\Delta}[A_{mt}]$ by median values of professional forecaster surveys on Bloomberg. Survey-based forecasts have been shown to outperform forecasts using historical values of macroeconomic variables (see e.g. Pearce and Roley (1985)). We verify that the survey-based forecasts are unbiased.[5] We assume that the expectation $E_{m,t-\Delta}[A_{mt}]$ about a macroeconomic announcement is exogenous, in particular not affected by asset returns during $[t - \underline{\tau}, t]$. To obtain comparable units we standardize the surprises by the standard deviation, σ_m, of the respective announcement.

[5] The mean forecast error is statistically indistinguishable from zero at a 10% significance level for all announcements except for the Index of Leading Indicators and Preliminary and Final releases of the 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.

To isolate the pre-announcement effect from the post-announcement effect, we proceed in three steps. First, we estimate Equation (1) using an event window spanning from $\underline{\tau} = -30$ minutes before to $\overline{\tau} = +30$ minutes after the announcement time t.[6] Next, we reestimate (1) using only the pre-announcement window $[t - 30min, t - 5sec]$. Comparing the coefficients of the two regressions yields the pre-announcement effect.

We use five seconds ($\overline{\tau} = -5sec$) before the official release time as the cutoff for the pre-announcement interval for two reasons. First, Thomson Reuters used to prerelease the University of Michigan Consumer Confidence Index two seconds ahead of the official release time to its high-speed data feed clients. We want to capture trading following these prereleases in the post-announcement interval, so that it does not overstate our pre-announcement price drift.[7] Second, there have been instances of inadvertent early release such as Thomson Reuters publishing the ISM Manufacturing Index 15 milliseconds before the scheduled release time on June 3, 2013. Although Scholtus, van Dijk, and Frijns (2014) show that such early releases are rare by comparing the official announcement time and the actual arrival time, we avoid by our interval choice that trading following any accidental early releases falls into our post-announcement interval. Based on Scholtus et al. (2014), using five seconds before the official release time as pre-announcement cutoff suffices to capture such early releases.

[6] We vary the event window length as a robustness check. Shortening the pre-announcement interval (for example, to $\underline{\tau} = 25min$, $20min$ or $15min$) generally results in lower coefficients and lower standard errors than those reported in Table 2, which 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. We use a 30-minute length for the post-announcement interval. Although previous studies such as Balduzzi et al. (2001) report that markets absorb macroeconomic news within a few minutes, a joint test of significance of price moves in the $[t + 10min, t + 30min]$ window for all 30 announcements shows some evidence of continuing adjustment. Using $\overline{\tau} = +30min$ also accounts for possible overshooting and subsequent reversal of prices.

[7] 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. Because Thomson Reuters stated that the ISM Manufacturing Index could possibly be released to the high-speed data feed clients up to ten seconds earlier (Rogow, 2012; Javers, 2013c), we check robustness for both ISM indices with the $[t - 30min, t - 10sec]$ window. The results do not differ.

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.[8] We augment this set by seven announcements, which 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 releases of the University of Michigan (UM) Consumer Sentiment Index. Our updated set of announcements is larger than the set in previous studies, 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.

We use the Bloomberg consensus forecast to proxy for the market expectations, $E_{m,t-\Delta}[A_{mt}]$. Bloomberg collects forecasts from analysts during about a two-week period preceding the announcements. For example, for our 30 announcements in November of 2014, the first analysts posted their forecasts on Bloomberg five to 14 days before the announcements. A forecast can be posted until two hours before the announcement. On average, the forecasts are five days old as of the announcement time. They can be updated although this appears to be done infrequently.[9] Bloomberg calculates the consensus forecast as the median of the analyst forecasts and continuously updates the consensus forecast as additional individual forecasts are posted. The expected value of absolute standardized surprises in the last column of Table 1 is slightly smaller than the expected value of the absolute value of a standard normal random variable. $E\left(|S_{mt}|\right) < \sqrt{\frac{2}{\pi}} \approx 0.80$ indicates that market expectations go on

[8]We omit the four monetary announcements because these policy variables differ from macroeconomic announcements by long preparatory discussions. 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.

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

Table 1: Overview of U.S. Macroeconomic Announcements

Category	Announcement	Frequency	Obs.	Source[a]	Units	Time	Fcts.	$E(\lvert S_{mt}\rvert)$
Income	GDP advance	Quarterly	25	BEA	%	8:30	82	0.707
	GDP preliminary	Quarterly	25	BEA	%	8:30	78	0.755
	GDP final	Quarterly	25	BEA	%	8:30	76	0.743
	Personal income	Monthly	74	BEA	%	8:30	70	0.584
Employment	ADP employment	Monthly	75	ADP	Number of jobs	8:15	34	0.713
	Initial jobless claims	Weekly	326	ETA	Number of claims	8:30	44	0.733
	Non-farm employment	Monthly	75	BLS	Number of jobs	8:30	84	0.796
Industrial Activity	Factory orders	Monthly	74	BC	%	10:00	62	0.779
	Industrial production	Monthly	75	FRB	%	9:15	78	0.735
Investment	Construction spending	Monthly	74	BC	%	10:00	48	0.749
	Durable goods orders	Monthly	75	BC	%	8:30	76	0.749
	Wholesale inventories	Monthly	75	BC	%	10:00	31	0.784
Consumption	Advance retail sales	Monthly	75	BC	%	8:30	79	0.718
	Consumer credit	Monthly	74	FRB	USD	15:00	33	0.786
	Personal consumption	Monthly	74	BEA	%	8:30	74	0.768
Housing Sector	Building permits	Monthly	74	BC	Number of permits	8:30	52	0.787
	Existing home sales	Monthly	75	NAR	Number of homes	10:00	73	0.703
	Housing starts	Monthly	73	BC	Number of homes	8:30	76	0.756
	New home sales	Monthly	74	BC	Number of homes	10:00	73	0.722
	Pending home sales	Monthly	76	NAR	%	10:00	36	0.738
Government	Government budget	Monthly	74	USDT	USD	14:00	27	0.603
Net Exports	Trade balance	Monthly	75	BEA	USD	8:30	73	0.780
Inflation	Consumer price index	Monthly	75	BLS	%	8:30	80	0.700
	Producer price index	Monthly	73	BLS	%	8:30	74	0.775
Forward-looking indices	CB Consumer confidence index	Monthly	75	CB	Index	10:00	71	0.809
	Index of leading indicators	Monthly	75	CB	%	10:00	53	0.796
	ISM Manufacturing index	Monthly	75	ISM	Index	10:00	76	0.799
	ISM Non-manufacturing index	Monthly	75	ISM	Index	10:00	71	0.749
	UM Consumer sentiment - Prel	Monthly	75	TRUM	Index	9:55	67	0.824
	UM Consumer sentiment - Final	Monthly	74	TRUM	Index	9:55	61	0.817

The sample period covers January 1, 2008 to March 31, 2014. The announcement time is stated in Eastern Time. The "Fcts." column shows the average number of analysts that submitted a forecast to Bloomberg. The $E(|S_{mt}|)$ column shows absolute value of the standardized surprise defined in Equation (2).

[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 (TRUM), and U.S Department of the Treasury (USDT).

average in the correct direction so that $Var(S_{mt}) < \sigma_m^2$.

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 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). 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 announcement.

4 Empirical Results

This section presents graphical and regression evidence of the pre-announcement price drift. We start with an event study regression, present 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 in the three steps outlined in Section 2. We begin by identifying market-moving announcements among our set of 30 announcements using regression (1). The focus of markets on a subset of announcements can be a direct consequence of their intrinsic value (Gilbert, Scotti, Strasser, & Vega, 2015), but also be an optimal information acquisition strategy in presence of private information (Hirshleifer, Subrahmanyam, & Titman, 1994).

We examine the event window ranging from 30 minutes before to 30 minutes after the official announcement time t. Analogously, the dependent variable R_t is the continuously compounded futures return over the $[t - 30min, t + 30min]$ interval.

Our sample contains 18 market-moving announcements, which we identify in Table 2 based on the p-values of the joint test of both stock and bond market. The coefficients have the expected sign: 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 0.239 percent and its surprises explain 24 percent of the price variation within the announcement window. The magnitude of the coefficients is sizable because, for example, one standard deviation of 30-minute returns during our entire sample period for the stock and bond markets is 0.18 and 0.06 percent, respectively. All subsequent analysis is based on these 18 market-moving announcements.

Next, we focus on the pre-announcement period, to find out which of the 18 market-moving announcements exhibit pre-announcement price drift. We re-estimate (1) using an event window ranging from 30 minutes prior to five seconds prior to the scheduled announcement time. Accordingly, we use now the continuously compounded futures return over the window $[t - 30min, t - 5sec]$. Table 3, sorted by the p-value of the joint test for the stock and bond market, shows that there are seven such announcements at 5% significance level.[10] About half of these announcements move both markets already before the announcement. A joint test of the 18 hypotheses overwhelmingly confirms the overall statistical significance of the pre-announcement price drift.[11] These results stand in contrast to previous studies concluding that the pre-announcement effect is small or insignificant.

To account for the turbulent financial crisis, we re-estimate (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

[10] As a robustness check, we estimate the model with the seemingly unrelated regression to allow for the covariance between parameters γ_m in the stock and bond market to be used in the joint Wald test. The results (available upon request) confirm those reported in Table 3.

[11] Assuming the t-statistics in Table 3 are independent and standard normal, squaring and summing them gives a χ^2- statistic with 18 degrees of freedom. The computed values of this statistic for the E-mini S&P 500 and 10-year Treasury note futures are 60.4 and 77.5, respectively. This result confirms statistical significance of the pre-announcement drift at 1% significance level.

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

Announcement	E-mini S&P 500 Futures γ_m	R^2	10-year Treasury Note Futures γ_m	R^2	Joint Test p-value
GDP advance	0.239 (0.096)**	0.24	-0.063 (0.041)	0.08	0.014
GDP preliminary	0.219 (0.072)***	0.13	-0.082 (0.021)***	0.32	<0.001
GDP final	0.074 (0.050)	0.05	-0.015 (0.027)	0.01	0.289
Personal income	0.011 (0.029)	0.00	-0.006 (0.015)	0.00	0.858
ADP employment	0.235 (0.056)***	0.32	-0.102 (0.023)***	0.32	<0.001
Initial jobless claims	-0.120 (0.021)***	0.10	0.059 (0.011)***	0.11	<0.001
Non-farm employment	0.433 (0.058)***	0.40	-0.249 (0.055)***	0.33	<0.001
Factory orders	-0.057 (0.060)	0.02	0.019 (0.018)	0.02	0.364
Industrial production	0.091 (0.049)*	0.08	-0.012 (0.011)	0.01	0.089
Construction spending	0.073 (0.067)	0.01	-0.008 (0.022)	0.00	0.516
Durable goods orders	0.067 (0.032)**	0.06	-0.028 (0.015)*	0.03	0.019
Wholesale inventories	0.008 (0.048)	0.00	-0.013 (0.018)	0.01	0.754
Advance retail sales	0.188 (0.031)***	0.36	-0.110 (0.020)***	0.36	<0.001
Consumer credit	-0.104 (0.081)	0.03	0.008 (0.014)	0.01	0.372
Personal consumption	0.014 (0.024)	0.00	0.007 (0.017)	0.00	0.777
Building permits	0.028 (0.046)	0.01	-0.041 (0.022)*	0.06	0.156
Existing home sales	0.206 (0.062)***	0.13	-0.055 (0.017)***	0.12	<0.001
Housing starts	0.049 (0.042)	0.02	-0.067 (0.019)***	0.17	0.001
New home sales	0.082 (0.054)***	0.02	-0.057 (0.016)***	0.15	0.001
Pending home sales	0.218 (0.065)***	0.17	-0.064 (0.013)***	0.26	<0.001
Government budget	-0.249 (0.159)	0.19	0.042 (0.032)	0.06	0.125
Trade balance	0.042 (0.066)	0.01	-0.020 (0.015)	0.01	0.342
Consumer price index	-0.131 (0.058)**	0.10	-0.008 (0.024)	0.00	0.076
Producer price index	-0.001 (0.051)	0.00	-0.054 (0.018)***	0.12	0.008
CB Consumer confidence index	0.245 (0.080)***	0.22	-0.098 (0.019)***	0.33	<0.001
Index of leading indicators	0.049 (0.080)	0.01	-0.011 (0.020)	0.01	0.712
ISM Manufacturing index	0.329 (0.058)***	0.24	-0.147 (0.022)***	0.39	<0.001
ISM Non-manufacturing index	0.097 (0.084)***	0.04	-0.091 (0.014)***	0.32	<0.001
UM Consumer sent. - Final	-0.066 (0.063)	0.02	-0.010 (0.020)	0.00	0.512
UM Consumer sentiment - Prel	0.082 (0.073)	0.02	-0.037 (0.019)*	0.05	0.081

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.

towards higher efficiency. Table 4 shows that all seven announcements significant in Table 3 remain significant. We label them as "strong drift" announcements. Six announcements display significant drift for stock or bond markets neither in the robust regression nor in the Table 3 joint test. We label them as "no drift" announcements. Five announcements

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

Announcement	E-mini S&P 500 Futures γ_m	R^2	10-year Treasury Note Futures γ_m	R^2	Joint Test p-value
ISM Non-manufacturing index	0.139 (0.030)***	0.19	-0.058 (0.011)***	0.30	<0.0001
Pending home sales	0.154 (0.083)*	0.09	-0.035 (0.010)***	0.16	0.001
ISM Manufacturing index	0.091 (0.036)**	0.06	-0.027 (0.009)***	0.10	0.001
Existing home sales	0.113 (0.040)***	0.10	-0.019 (0.009)**	0.04	0.002
CB Consumer confidence index	0.035 (0.052)	0.01	-0.031 (0.010)***	0.12	0.007
GDP preliminary	0.146 (0.068)**	0.15	-0.022 (0.011)*	0.08	0.013
Industrial production	0.066 (0.023)***	0.15	-0.007 (0.008)	0.01	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
ADP employment	0.035 (0.027)	0.03	-0.006 (0.007)	0.01	0.291
Initial jobless claims	-0.009 (0.012)	0.00	0.007 (0.006)	0.01	0.369
Producer price index	-0.043 (0.036)	0.05	-0.004 (0.010)	0.00	0.442
New home sales	0.030 (0.033)	0.01	-0.005 (0.009)	0.01	0.539
GDP advance	0.024 (0.044)	0.01	-0.023 (0.027)	0.06	0.608
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 that have significant effect on 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.

with significant coefficients in the robust regression (mainly in the bond market) are not significant in the joint test of Table 3. We label them as "some drift" announcements.

To quantify the magnitude of the pre-announcement price drift as a proportion of the total price adjustment, we divide the γ_m coefficients in Table 3 by the corresponding coefficients in Table 2, i.e., $\Gamma_m = \gamma_m^{\overline{\tau}=-5sec}/\gamma_m^{\overline{\tau}=+30min}$. Table 5 shows these ratios sorted by the proportion obtained for the stock market. The ratio Γ_m ranges from 14 percent in the CB Consumer Confidence Index up to 143 percent in the ISM Non-Manufacturing Index. The ratio exceeding 100 percent in the ISM Non-Manufacturing Index is due to a partial reversal of the

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	0.138 (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
Housing starts	-0.007 (0.016)	0.00	-0.018 (0.009)*	0.03
Initial jobless claims	-0.009 (0.007)	0.00	0.013 (0.005)***	0.01
No Evidence of Pre-Announcement Drift				
ADP employment	0.009 (0.013)	0.01	-0.006 (0.008)	0.01
Durable goods orders	0.005 (0.015)	0.00	-0.007 (0.006)	0.01
New home sales	0.041 (0.031)	0.01	-0.006 (0.001)	0.00
Non-farm employment	0.018 (0.016)	0.00	-0.000 (0.009)	0.00
Producer price index	0.011 (0.018)	0.00	0.000 (0.009)	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 significant effect on 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 MM weighted least squares (Yohai, 1987). Standard errors are shown in parentheses. *, **, and *** indicate statistical significance at 10%, 5%, and 1% levels, respectively. The announcements are classified based on these results jointly with the evidence from Table 3.

pre-announcement drift after the announcement. The mean ratio across all announcements and both markets is 53 percent. Therefore, failing to account for the pre-announcement effect substantially underestimates the total influence that these macroeconomic announcements exert in markets.

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

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 $[t-30min, t+30min]$	γ_m $[t-30min, t-5sec]$	Γ_m	γ_m $[t-30min, t+30min]$	γ_m $[t-30min, t-5sec]$	Γ_m
ISM Non-manufacturing index	0.097	0.139	143%	-0.091	-0.058	64%
Industrial production	0.091	0.066	73%	-0.012	-0.007	58%
Pending home sales	0.218	0.154	71%	-0.064	-0.035	55%
GDP preliminary	0.219	0.146	67%	-0.082	-0.022	27%
Existing home sales	0.206	0.113	55%	-0.055	-0.019	35%
ISM Manufacturing index	0.329	0.091	28%	-0.147	-0.027	18%
CB Consumer confidence index	0.245	0.035	14%	-0.098	-0.031	32%
Mean			64%			41%

The sample period is from January 1, 2008 through March 31, 2014. Only the announcements that show significant pre-announcement price drift in stock index or Treasury security futures prices (based on the joint test in Table 3) are included.

or negative effect on the stock and bond markets using the coefficients in Table 2. Following Bernile et al. (2015) we invert the sign of returns for negative surprises.[12] CARs are then calculated in the window $[t - 60min, t + 60min]$ for each of the three categories "drift", "some drift", and "no drift" defined in Table 4[13] The CARS in Figure 1 reveal what happens around the announcements.

For the no-drift announcements in Panel a), significant price adjustment does not occur in the stock market until after the announcement time, but even there the overall price change correctly anticipates the announcement impact. For the strong-drift announcements in Panel c), the price begins moving in the correct direction about 30 minutes before the announcement, and in contrast to Panel a) these price changes are significant. For the intermediate group in Panel b), there is a somewhat less pronounced price adjustment before

[12]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 in inflation is often considered as bad news for stocks.

[13]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 subperiod $[t-60min, t-30min]$ in Figure 1.

the announcement. The bond market in Panel c) shows the same pattern as the stock market with price starting to drift about 30 minutes before the official announcement time.[14,15]

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 Table 5. Calculated as the CAR during the $[t - 30min, t - 5sec]$ window divided by the CAR during the $[t - 30min, t + 30min]$ window, these results (in the Appendix Table A1) confirm substantial pre-announcement price drift in both stock and bond markets.[16]

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. (2015), we classify trading volume as buyer- or seller-initiated using the tick rule. Specifically, the trade volume in a 1-second bar is classified as buyer-initiated (seller-initiated) if the price for that bar is higher (lower) than the last different price.[17] We plot cumulative order imbalances in Figure 2 for the same time window as in Figure 1. Similar to price drifts, order flow imbalances start building up about 30 minutes prior to the announcement, pointing towards informed trading during the pre-announcement interval. The pre-announcement imbalances are particularly pronounced for strong (price)

[14] For the bond market Panels b) and c) look similar. This is because the classification of announcements as "some evidence of drift" is mainly influenced 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. We, therefore, estimate the regression in Equation (1) for the $[t - 60min, t - 30min]$ window; only the ADP Employment announcement is significant. Figure A1 in the Appendix shows CARs for the individual announcements.

[15] The drift in both the stock and bond markets is particularly pronounced before large surprises. See Appendix Figure A2 for more detail.

[16] The methodology using CARs to calculate the proportions follows Sinha and Gadarowski (2010) and Agapova and Madura (2011) in the corporate finance literature. 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.

[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. The correlation of 5-minute order imbalances constructed using the tick rule with those computed from the order book data ranges from 0.7 to 0.8. We also find that the tick rule performs better than the bulk volume classification method of Easley, Lopez de Prado, and O'Hara (2012).

drift announcements. Interestingly, all announcements show some pre-announcement order imbalance on the Treasury note futures market.

The magnitude of the drift is economically significant. To gauge the magnitude of the total profits in the E-mini S&P 500 futures market earned by market participants trading in the correct direction ahead of the announcements during our sample period, we compute cumulative order imbalances over 30-minute pre-announcement windows for all announcements and average them to arrive at an average cumulative order imbalance of 3,327 contracts. We then multiply by the average price per contract (1,285) and the average profit of 15.7 basis points per contract (assuming that the average informed trader establishes a position 15 minutes before the announcement and closes it five minutes after the announcement). Since the contract size of the E-mini S&P 500 futures contract is 50 USD times the index, we multiply by 50. For the seven "drift" announcements, this yields an average profit *per announcement release* of about 336,000 USD. Multiplying by the number of observations for each of the seven drift announcements, we approximate the total profit at 154 million USD during a little more than six years.[18] That corresponds to a profit of about 25 million USD per year in the E-mini S&P 500 futures market alone. Profits in the 10-year Treasury note futures market over our sample period amount to 52 million USD and profits in other stock and bond markets can be calculated similarly.

4.4 Robustness Checks

We have already verified robustness with respect to the event window length and outliers in Sections 2 and 4.1, respectively. In this subsection we test whether our results are robust to the business cycle, (potential) effects stemming from other announcements, data snooping, asymmetries, and choice of the asset market. Further, we compare our results with previous studies that analyze older sample periods. All tests in this subsection confirm robustness of our results and tabulated details are available upon request.

[18] We have 476 observations for the announcements in the "drift" category. We multiply by 458 because 18 observations had zero surprises.

4.4.1 Business Cycle Effect

Some studies indicate that the impact of macroeconomic announcements differs between recessions and expansions. For example, Boyd, Hu, and Jagannathan (2005) show that during the years 1957 to 2000 higher unemployment drives stock markets up during expansions but pushes them down during recessions. We analyze whether this is the case in our sample period. Recession dating by the National Bureau of Economic Research identifies June 2009 as the recession end. We do not detect a sign switch at this date. Better than expected news boosts prices in the stock market and lowers the prices in the bond market throughout the sample period independently of the state of the business cycle. Overall, our results do not appear to be driven by the crisis vs. non-crisis periods.[19]

4.4.2 Effect of Other Recent Announcements

On some days the markets receive news about multiple announcements. Six out of the seven strong drift announcements follow 8:30 announcements on some days (Industrial 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 event-study Equation (1) that measures the cumulative return from 90 minutes before the announcement time to 30 minutes before the announcement time. For example, for 10:00 o'clock announcements this corresponds to the window from 8:30 to 9:30. This control variable is usually insignificant and the results from Section

[19] This robustness check suggests that there is no systematic time-variation across the business cycle. This does *not* mean there is no time-variation. A notable example of parameter instability is the Nonfarm Employment announcement. Known as the "king of announcements" for moving both stock and bond markets more than any other announcement, it is significant in Table 3 only at the 10% significance level in the stock market. However, the significance increases when we use only 2008, which is when concerns about data leakage in the DOL lock-up room emerged and the lock-up participant "Need to Know News" was found in violation of rules in four consecutive lock-ups (Mullins & Patterson, 2013).

4.1 maintain, which is consistent with the CARs in Figures 1 and 2 remaining near zero until 30 minutes before announcement time.

Second, we employ a time-series approach (following, e.g., 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 values of each announcement surprise 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 here 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:

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

We use 15-minute returns.[20] To measure the pre-announcement price drift, we use $M = 2$ leads of surprises. Their coefficients capture the effect in the $[t - 30min, t - 15min]$ and $[t - 15min, t - 5sec]$ windows, which are the ones 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 six lags of returns, so $I = 6$. Similarly, there is one contemporaneous and five lagged terms of each announcement surprise. To reduce the number of estimated parameters, we try a specification with only one contemporaneous and one lagged term of the surprise as well, and test if the sum of surprise coefficients on lags 2 through 5 representing the $[t - 30min, t - 90min]$ window is different from zero.[21] Since the pre-announcement drift

[20] Ideally, we would use 5-minute returns to separate the effects of all announcement 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 UM Consumer Sentiment Index announcements released at 9:55, so $K = 28$.

[21] 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.

coefficients did not differ when the number of lags was reduced, Table 6 reports results for this specification with $J = 1$.[22]

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 a 5% significance level for the Industrial Production announcement and at a 1% significance level for the other six drift announcements. These results are consistent with the event study methodology results confirming that seven of the 18 market-moving announcements exhibit pre-announcement price drift and suggesting that the drift is not driven by forecast updating based on earlier announcements.

Table 6: Cumulative Return Impact of Future Announcements (Time-Series Regression)

Announcement	E-mini S&P 500 Futures $[t - 30min, t - 5sec]$	10-year Treasury Note Futures $[t - 30min, t - 5sec]$	Joint Test p-value
CB Consumer confidence index	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 index	0.084 (0.034)**	-0.023 (0.010)**	0.003
ISM Non-manufacturing index	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 coefficients of announcement surprises for the E-mini S&P 500 and 10-year Treasury note futures are equal to zero.

4.4.3 Other Robustness Checks

Testing multiple hypotheses might lead to unintended data-snooping. To rule out this possibility in our joint tests for 18 announcements we use Holm's step-down procedure. This

[22] 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. There are 2,190 observations with non-missing surprises per Table 1.

procedure adjusts the hypothesis rejection criteria to control the probability of encountering one or more type I errors ("familywise error rate"). Based on this conservative approach, four announcements ranked at the top of Table 3 show significant drift (ISM Manufacturing, ISM Non-manufacturing and Pending Home Sales at 1% significance level and Existing Home Sales at 5% significance level).

We test for asymmetries between positive and negative surprises as a robustness check. The difference between the coefficients for positive and negative surprises is not statistically significant.

Finally, we conduct robustness checks based on other stock index and bond futures markets (E-mini Dow and 30-year Treasury bonds). The results are similar, which confirms the findings of e.g. Baum, Kurov, and Wolfe (2014) that results do not differ much across markets given any asset category.

4.4.4 Extended Sample

Our second-by-second data starts on January 1, 2008. The studies referenced in Section 1 use older sample periods, for which second-by-second data is not available. We obtain minute-by-minute data instead, which allows us to extend the sample period back to January 1, 2003. There are 22 market-moving announcements in this longer sample period. Only two of them exhibit a pre-announcement price drift. This suggests that the pre-announcement effect was indeed weak or non-existent in the pre-2008 period. A variety of factors may have contributed to this change. Not only do the procedures for releasing the announcements change over time but the information collection and computing power also increases, which might enable sophisticated market participants to forecast some announcements. We discuss such possible explanations in the following section.

5 Causes of Pre-Announcement Price Drift

The corporate finance literature (e.g. Sinha and Gadarowski (2010) and Agapova and Madura (2011)) considers price drift before public guidance issued by company management as de facto evidence of informed trading that follows from information leakage. Likewise, Bernile et al. (2015) suggest that the pre-FOMC drift discovered in their analysis is caused by information leakage. However, at least one other explanation for informed trading exists: some traders may be able to analyze public information in a superior way or collect proprietary information to forecast the announcements better than others. We discuss the information leakage explanation first and then turn to the superior forecasting explanation.[23]

5.1 Information Leakage

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). Javers (2012) reports that the DOL asked Sandia National Laboratories to review the security protocol for releasing its monthly employment report. The last documented case of a U.S. government employee fired for leakage dates far back: in 1986 one employee of the Commerce Department was terminated for leaking Gross National Product data (Wall Street Journal, 1986). However, the possibility of leakage in more recent times cannot be ruled out.

Insider trading based on leaked information can seriously impair markets. It typically reduces risk sharing and the informational efficiency of prices in the long run (Brunnermeier, 2005). Our data does not allow designing a test that would definitively uncover leakage, but it allows an indirect approach to identifying systematic circumstances that lead to drift and that might capture leakage. We regress the share of preannouncement price drift, Γ_m,

[23]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.

defined in Section 4.1 on various properties of the release process, X_m, for the 18 market-moving announcements. We use the Tobit model because the dependent variable is left-censored at zero in the subset of announcements not exhibiting any drift. Therefore, $\Gamma_m^* = \beta_0 + \beta_m X_m + \varepsilon_m$, with $\Gamma_m = \Gamma_m^*$ for $\Gamma_m^* > 0$, $\Gamma_m = 0$ otherwise, and error term ε_m.

The first regressor in this small cross-sectional regression captures the type of organization releasing the announcement. The Office of Management and Budget provides guidance to federal statistical agencies on releasing their data. Key economic indicators are designated as principal federal economic indicators (PFEIs) and the agencies are required to follow strict security procedures to ensure fairness in the markets (Office of Management and Budget, 1985). This includes government agencies as well as the Federal Reserve Board. In contrast, some private entities were found releasing information to a group of subscribers before making the data available to other market participants as discussed in Section 1. We use an indicator variable taking on values of 1 and 0 based on whether the announcement is a PFEI (11 announcements) or non-PFEI (7 announcements) per Table 7. This variable is significant at 5% level in the 10-year Treasury market but not in the E-mini S&P 500 market (p-value of 0.16). The coefficient in the 10-year Treasury market is -0.41, suggesting that PFEI announcements exhibit less drift than non-PFEI announcements. However, caution needs to be exercised in interpreting these results because of the small sample size in our cross-regression.

The second regressor captures the release procedures. Surprisingly, this information is not readily available for many organizations on their websites. We conducted a phone and email survey of the organizations and summarize the stated release procedures in the columns "Prerelease" and "Safeguarding" of Table 7. Two types of procedures are used for releasing announcements. The first type used in four announcements involves posting the announcement on the organization's website that all market participants can access at the same time. The second type used in fourteen announcements involves prereleasing the information to journalists. The purpose of the preview is to allow the journalists to understand

Table 7: Principal Federal Economic Indicators and Prerelease Procedures

Announcement	Source	Drift	PFEI	Prerelease	Safeguarding
CB Consumer confidence index	CB	Drift	N	Y[b]	Lockup room
Existing home sales	NAR	Drift	N	Y	Lockup room
GDP preliminary	BEA	Drift	Y	Y	Lockup room
Industrial production	FRB	Drift	Y	Y	Embargo only
ISM Non-manufacturing index	ISM	Drift	N	N	–
ISM Manufacturing index	ISM	Drift	N	N	–
Pending home sales	NAR	Drift	N	Y	Embargo only
Advance retail sales	BC	Some drift	Y	Y	Lockup room
Consumer price index	BLS	Some drift	Y	Y	Lockup room
GDP advance	BEA	Some drift	Y	Y	Lockup room
Housing starts	BC	Some drift	Y	Y	Lockup room
Initial jobless claims	ETA	Some drift	Y[a]	Y	Lockup room
ADP employment	ADP	No drift	N	N	–
Durable goods orders	BC	No drift	Y	Y	Lockup room
New home sales	BC	No drift	Y	Y	Lockup room
Non-farm employment	BLS	No drift	Y	Y	Lockup room
Producer price index	BLS	No drift	Y	Y	Lockup room
UM Consumer sentiment - Prel	TRUM	No drift	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 DOL PFEIs such as Non-farm Employment.
[b] The Conference Board eliminated the prerelease during our sample period. It did not respond to any of our requests to provide or discuss the release policy.

the data before writing their news stories and thus provide more informed news coverage for the public.[24] Usually, this prerelease occurs in designated "lock-up rooms" although in two announcements the procedure differs ("embargo only"). The Pending Home Sales announcement released by the National Association of Realtors is transmitted to journalists who are asked not to share the information with individuals other than those working on the news story. Similarly, the Industrial Production announcement is released by the Federal Reserve Board through an electronic system to selected reporters at credentialed news organizations that have written agreements governing this prerelease access (Federal Reserve Board, 2014).

Our second regressor is an indicator taking on values of 0 and 1 depending on whether the

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

announcement is prereleased or not. Although the two announcements with the least secure release procedure (Pending Home Sales and Industrial Production) are among the seven drift announcements, the prerelease regressor is not significant in our cross-regression perhaps because some organizations go to great length to ensure that information does not leak out of the lock-up rooms. This is a challenging task in the modern communication technologies era. For example, news media were allowed to install their own computer equipment in the DOL's lock-up rooms without the organization staff being able to verify what exactly the equipment does (Fillichio, 2012; Hall, 2012). In addition, although the lock-up rooms were designed for media outlets that are in the journalism business such as newspapers, other entities have exploited the loose definition of what constitutes a media outlet and obtained access to the lock-up rooms. 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 the lock-up room. Citing similar challenges with monitoring the lock-up room, the Conference Board has decided to eliminate its lock-up room and post information directly on its website (Javers, 2013a). The DOL is considering the same procedure change (Mullins, 2014) attesting to the fact that ensuring a secure prerelease is a formidable task.[25]

Finally, we include a regressor that attempts to capture how easy it is to trade throughout the day based on private information. We hypothesize that it is easier when trading volume is high because it is more likely that informed trades will go unnoticed (Kyle, 1985). Although there is electronic trading throughout day and night, trading activity spikes when the open outcry starts at 8:20 in the 10-year Treasury note futures market and 9:30 in the S&P 500 futures market as shown in the Appendix Figure A3. To capture the two different volume regimes, we use indicator variables taking on values of 1 for announcements released within

[25]The prerelease 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). Other factors that might affect the likelihood of leakage include the number of individuals involved in the release process and the length of time from data collection to release. However, this information is not publicly available and we were unable to obtain it from all organizations.

45 minutes of the market opening when the trading volume is higher and 0 otherwise.[26] This regressor is also not significant in either market.

While we are not able to conclusively detect evidence of information leakage, we cannot conclude that it does not exist.[27] A thorough analysis of individual trader data would be needed to answer the leakage question definitively.[28]

5.2 Superior Forecasting

In addition to leakage, the pre-announcement drift could result from some traders forecasting the announcements better than others and using their superior forecasts to trade in the correct direction before the announcements.

5.2.1 Mismeasurement of Market Expectations

In Section 4 we have shown that the drift can be explained by the surprise variable. Thus it is possible that market participants use forecasts of the surprise, S_{mt}, to trade before the announcement release. In some investment houses considerable resources are placed in forecasting models of announcement surprises. If the surprises are predictable 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.

The definition of a surprise in Equation (2) requires information of market expectations $E_{m,t-\Delta}[A_{m,t}]$ to become operational. Section 4 uses the analyst consensus forecast, a common

[26] In the 10-year Treasury note futures, electronic trading takes place from 18:00 o'clock on Sundays through 17:00 o'clock on Fridays with 1-hour breaks starting at 17:00 in addition to the open outcry from 8:20 to 15:00 o'clock. In the E-mini S&P 500 futures market, electronic trading takes place from 18:00 o'clock on Sundays through 17:15 o'clock on Fridays with 45-minute breaks starting at 17:15 and 15-minute breaks starting at 16:15. Trading activity is also affected by the open outcry period in the S&P 500 from 9:30 to 16:15. All times are stated in Eastern Time.

[27] We also estimated this model controlling for forecastability of the surprise. We used three variables. First, we use the publication lag. It is possible that more forecasting effort goes into more up-to-date announcements because Gilbert et al. (2015) show that earlier announcements move markets more. Second, we use the average number of analysts because it might be easier to produce a superior forecast for announcements followed by fewer analysts. Third, we use the average standard deviation of individual forecasts as a measure of belief dispersion among analysts. None of these variables is significant in our cross-sectional regression.

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

approach in the literature (Balduzzi et al., 2001). The calculation of this consensus forecast by Bloomberg, however, is not innocuous: Bloomberg equal-weights its analysts, which is not optimal in general. We, therefore, use individual analyst forecasts (available to Bloomberg subscribers) attempting to construct a forecast that outperforms the Bloomberg consensus forecast. Here, we build on previous research that uses individual analyst forecasts. For example, Chang, Daouk, and Wang (2009) show that the crude oil market reacts more to crude oil inventory forecasts by analysts that have made more accurate forecasts in the past, and Gay, Simkins, and Turac (2009) show that the natural gas market also puts more emphasis on inventory forecasts by analysts with higher forecasting precision. 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 horizons and target variables.

Bloomberg provides a rank for active analysts who have issued accurate forecasts. The set of ranked analysts is a strict subset of all analysts who submitting a forecast for a specific announcement. We compute the median consensus for the ranked analyst subset, $E_{m,t-\Delta}^{Ranked}[A_{mt}]$, using only forecasts submitted no more than seven days before the announcement date. 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 analysts that have been ranked *before* the announcement. We use this variable as a predictor of the actual announcement, A_{mt}. Given that the surprise appears to reasonably explain the pre-announcement price drift documented in Section 4, a good forecast should be a variable that highly correlates with it. To avoid estimation of additional parameters, we consider a forecast of the *unstandardized* surprise:

$$\widetilde{S}_{mt} = A_{mt} - E_{m,t-\Delta}[A_{mt}] = \sigma_m S_{mt}.$$

Our forecast of the surprise based on the ranked consensus is

$$P_{mt} = E^{Ranked}_{mt-\Delta}[A_{mt}] - E_{mt-\Delta}[A_{mt}], \tag{4}$$

which is the difference between the median value of the professional forecasters that have been ranked by Bloomberg and the whole set of forecasters in the survey. We expect P_{mt} to be a reasonable forecast for $\widetilde{S}_{mt}$.

The forecast error in predicting the next surprise is $\tilde{S}_{mt} - P_{mt}$. We compare the forecast error based on (4) with a no-surprise benchmark, whose forecast errors are 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 B1 in Appendix B shows the results. The improvement over the zero surprise forecast is significant at 5% level for four of the 18 market-moving announcements (CPI, Durable Goods Orders, Industrial Production and PPI). However, there is no relationship between forecastibility of the surprise and drift results in Table 4.[29,30]

We also regress the unstandardized surprise, $\widetilde{S}_{mt}$, on a constant and the prediction, P_{mt}. The results for the regression are reported in Table B2, where the p-values are for a two-

[29] We also conducted the same tests using more complicated methods of combining the individual analysts similar to Brown et al. (2008) and more advanced econometric techniques such as the complete subset regression of Elliott, Gargano, and Timmermann (2013). The results (available upon request) show that we can improve on the Bloomberg consensus forecast in six announcements (CB Consumer Confidence Index, CPI, Durable Goods Orders, Industrial Production, PPI and UM Consumer Sentiment Index Preliminary) but the conclusions are not qualitatively different: There is no relationship between forecastability of the surprise and drift results in Table 4.

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

sided test. No intercept is significant indicating that our forecast for the surprise is unbiased. Eight announcements show slope coefficient significance at the 5% level (CB Consumer Confidence Index, CPI, Durable Goods Orders, Existing Home Sales, GDP Advance, Industrial Production, Pending Home Sales and PPI).

The results from Table B1 show that there is a significant linear relation between the predictions and the 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 S_{mt}. Table B3 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 B3 Panel b) reports the results for the $[t - 30min, t + 30min]$ window. Again, returns can be forecasted 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.

We discussed these results with several economists who work in investments houses. One confirmed that he has a list of analysts he follows for each announcement. The list is based on his experience and transcends the Bloomberg individual analysts survey. Before an announcement, he calls the analysts on his list and updates his forecasts accordingly. The mechanics of this updating procedure were not disclosed to us.

5.2.2 Proprietary Information

In addition to refining the Bloomberg consensus forecasts discussed in Section 5.2.1, some market participants generate their own *proprietary* information by continuously 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 private information is never published, it remains a noisy 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. However, initially proprietary data that is released to researchers or the public at some later point in time 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.[31] 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 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. These results (available upon request) suggest that early access to proprietary information permits forecasting announcement surprises in some cases.

5.2.3 Forecasting Surprises with Public Information

To further explore predictability of announcement surprises, we consider the possibility that *publicly* available information can be used to forecast the surprises. We conduct a forecasting exercise similar to Section 5.2.2 with various publicly available information. We use the surprise in one announcement to forecast the surprise in another announcement. For example, we use the Preliminary UM Consumer Sentiment surprise (released on average on the 13^{th}

[31] An example of proprietary data that is available only on a subscription basis without being released to the public later is credit-card spending data ("SpendingPulse") of MasterCard.

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.

We also use internet search engine activity data. This data reflects interest in acquiring information and several recent studies 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 categorizes search terms into numerous groups. We use the search activity in "Jobs" category to forecast the announcement surprises because it is particularly relevant for the macroeconomy. We find predictive power for the Initial Jobless Claims surprise but not for the CB Consumer Confidence Index surprise. Again, these results (available upon request) suggest that public information allows forecasting announcement surprises in some cases. However, they are too sketchy to prove that proprietary or public data is in fact used for informed trading around announcement time.

5.2.4 Bandwagon Effect

Leakage makes prices before the news announcement more informative (Brunnermeier, 2005). 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. It is important to recognize, however, that the markets that we examine are very liquid. The average trading volume in the 30-minute window before drift announcements, for example, is about 177,000 and 62,000 contracts in the E-mini S&P 500 and 10-year Treasury note futures, respectively. The order imbalances we observe before these announcements are sizable but represent only a small fraction of the overall trading activity. This high level of trading activity might allow informed traders to camouflage their information and trade profitably before the announcements.[32] We consider uninformed traders observing price movements at

[32] 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.

the beginning of the drift and trading accordingly. For example, we analyze correlations of returns in the $[t - 30min, t - 15min]$ window with returns in the $[t - 15min, t]$ window. Such correlations were not significant, suggesting that simply observing price movements cannot be easily used to trade profitably ahead of announcements.

6 Conclusion

We document pre-announcement price drift in equity index and Treasury futures markets for seven out of 18 market-moving U.S. macroeconomic announcements. About 30 minutes before the announcement time, prices begin to drift in the same direction as the market subsequently reacts to the news. This drift accounts for about 64 percent and 41 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 emphasizes the economic significance of these price moves.

We examine two possible sources of private information about public announcements: information leakage and superior forecasting. Some of the improved forecasting ability may be based on smart reprocessing of publicly available data. Other components of the improved forecasting ability may be based on "digging deeper" into pre-packaged information products, for example, using individual analyst forecasts 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.

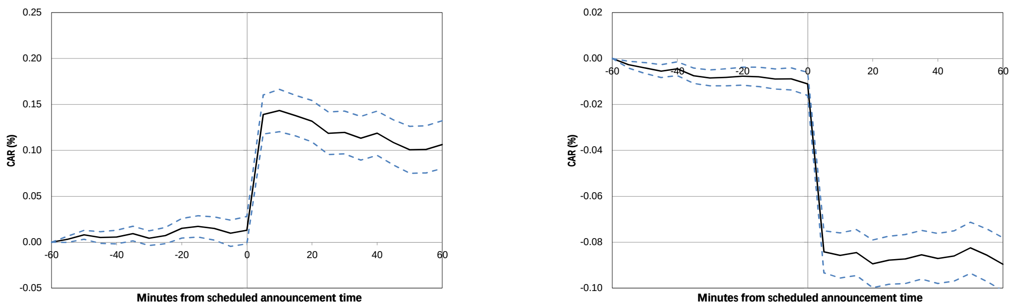
Our tests are not able to rule out either information leakage or superior forecasting. Considering the public and regulatory attention that leakage has received, the *source* of informed trading merits more research in view of the public interest in safeguarding of macroeconomic data. Of particular interest 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 regulation.

In terms of trading strategies, it is interesting to note that the significant pre-announcement price drift occurs only about 30 minutes before announcement time. If the drift is indeed caused by trading based on superior forecasts, this timing suggests that market participants trade on their superior knowledge only shortly before the announcements – potentially to minimize exposure to non-announcement risks.

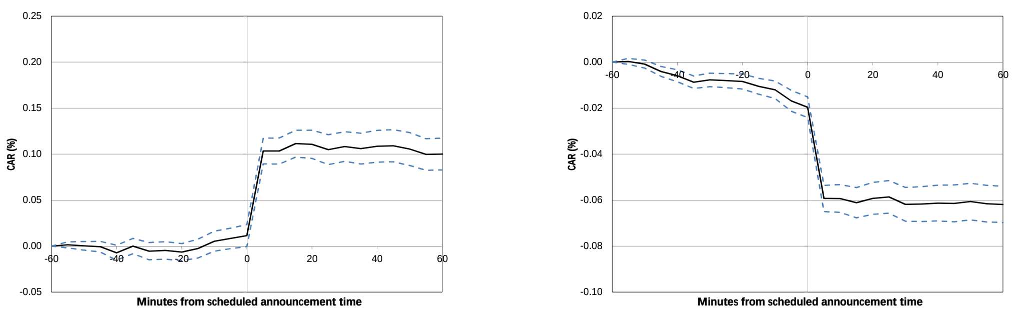
Figure 1: Cumulative Average Returns

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

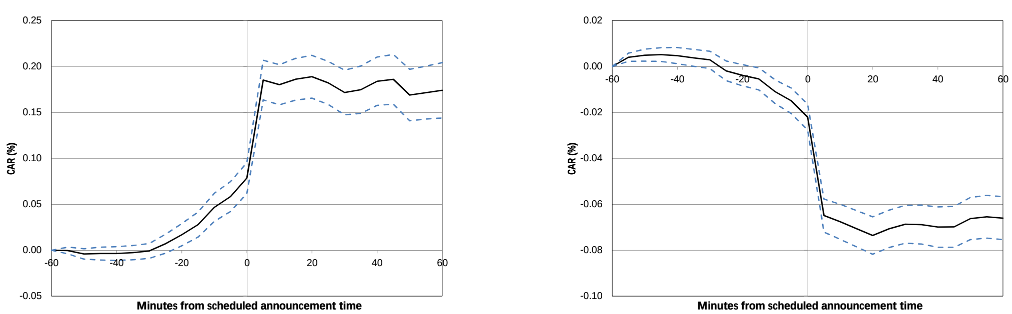
Panel a): Announcements with no evidence of drift



Panel b): Announcements with some evidence of drift



Panel c): Announcements with strong evidence of drift

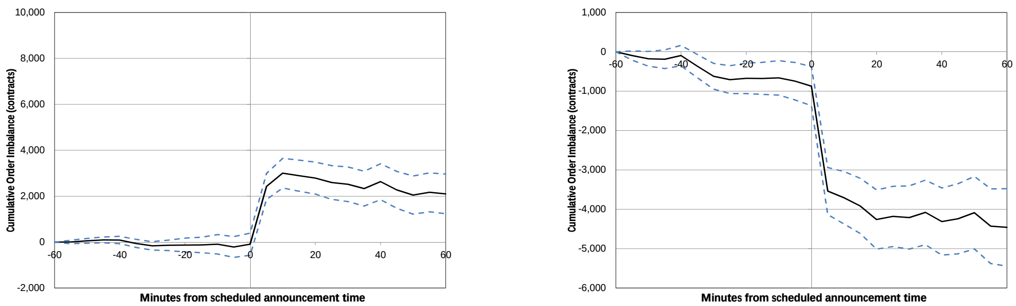


Announcements are categorized as strong drift, no drift and some evidence of drift using the classification of Table 4. For each category the solid line shows the mean cumulative average returns since sixty minutes before the announcement time. Dashed lines mark average one-standard-errors bands of the mean.

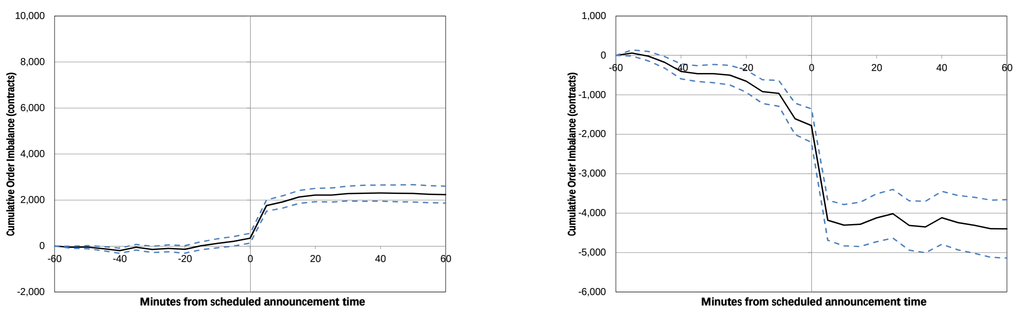
Figure 2: Cumulative Order Imbalances

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

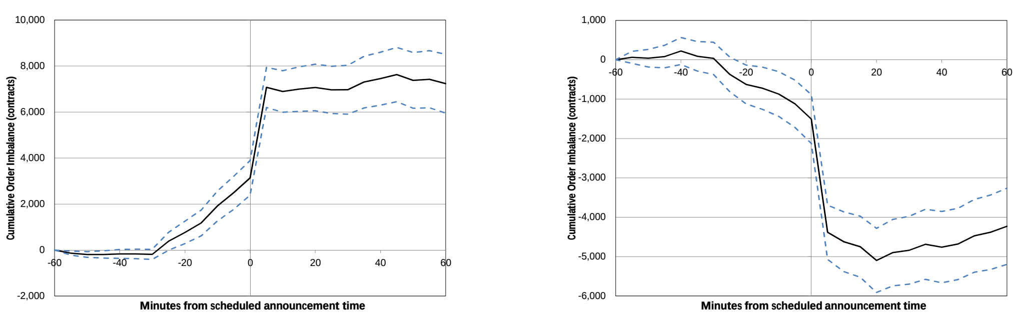
Panel a): Announcements with no evidence of drift



Panel b): Announcements with some evidence of drift



Panel c): Announcements with strong evidence of drift



Announcements are categorized as drift, no drift and some evidence of drift using the classification in Table 4. For each category, we compute cumulative order imbalances in the event window from sixty minutes before the announcement time to sixty minutes after the announcement time. To reduce the influence of extreme order imbalance observations, the order imbalances are winsorized at the 1^{st} and 99^{th} percentiles.

References

Admati, A. R., & Pfleiderer, P. (1988). A theory of intraday patterns: Volume and price variability. *Review of Financial Studies*, *1*(1), 3–40.

Agapova, A., & Madura, J. (2011). Information leakage prior to company issued guidance. *Financial Management*, *40*, 623–646.

Andersen, T. G., Bollerslev, T., Diebold, F. X., & Vega, C. (2003). Micro effects of macro announcements: Real-time price discovery in foreign exchange. *American Economic Review*, *93*(1), 38–62.

Andersen, T. G., Bollerslev, T., Diebold, F. X., & Vega, C. (2007). Real-time price discovery in global stock, bond and foreign exchange markets. *Journal of International Economics*, *73*(2), 251–277.

Andersson, M., Overby, L. J., & Sebestyén, S. (2009). Which news moves the Euro area bond market? *German Economic Review*, *10*, 1–31.

Balduzzi, P., Elton, E. J., & Green, T. C. (2001). Economic news and bond prices: Evidence from the U.S. Treasury market. *Journal of Financial and Quantitative Analysis*, *36*, 523–543.

Baum, C. F., Kurov, A., & Wolfe, M. H. (2014). What do Chinese macro announcements tells us about the world economy? *Boston College Working Paper Series No. 834*.

Bernile, G., Hu, J., & Tang, Y. (2015). Can information be locked-up? Informed trading ahead of macro-news announcements. *Manuscript. Singapore Management University*.

Boyd, J. H., Hu, J., & Jagannathan, R. (2005). The stock market's reaction to unemployment news: Why bad news is usually good for stocks. *Journal of Finance*, *60*, 649–672.

Brown, L. D., Gay, G. D., & Turac, M. (2008). Creating a "smart" conditional consensus forecast. *Financial Analysts Journal*, *64*, 74–86.

Brunnermeier, M. K. (2005). Information leakage and market efficiency. *Review of Financial Studies*, *18*(2), 417–457.

Caminschi, A., & Heaney, R. (2013). Fixing a leaky fixing: Short-term market reactions to the London PM gold price fixing. *Journal of Futures Markets*, *34*, 1003–1039.

Campbell, J. Y., Ramadorai, T., & Schwartz, A. (2009). Caught on tape: Institutional trading, stock returns, and earnings announcements. *Journal of Financial Economics*, *92*, 66–91.

Chang, Daouk, H., & Wang, A. (2009). Do investors learn about analyst accuracy? A study of the oil futures market. *Journal of Futures Markets*, *29*, 414–429.

Chang, Liu, W., Suardi, S., & Wu, W. (2014). Informed high frequency trading with advance peek on index of consumer sentiment. *Manuscript*.

Choi, H., & Varian, H. (2012). Predicting the present with Google Trends. *The Economic Record*, *88*, 2–9.

Da, Z., Engelberg, J., & Gao, P. (2011). In search of attention. *Journal of Finance*, *56*, 1461–1499.

Diebold, F. X. (2015). Comparing predictive accuracy, twenty years later: A personal perspective on the use and abuse of Diebold-Mariano tests. *Journal of Business & Economic Statistics*, *33*(1), 1–9.

Diebold, F. X., & Mariano, R. S. (1995). Comparing predictive accuracy. *Journal of Business & Economic Statistics*, *13*(3), 253–263.

Easley, D., Lopez de Prado, M., & O'Hara, M. (2012). Flow toxicity and liquidity in a high frequency world. *Review of Financial Studies*, *25*(5), 1457–1493.

Ederington, L. H., & Lee, J. H. (1995). The short-run dynamics of the price adjustment to new information. *Journal of Financial and Quantitative Analysis*, *30*, 117–134.

Elliott, G., Gargano, A., & Timmermann, A. (2013). Complete subset regressions. *Journal of Econometrics*, *177*(2), 357–373.

Federal Reserve Board. (2014). Performance evaluation of the Federal Reserve G.17 (419) statistical release.

Fillichio, C. (2012). Statement of Carl Fillichio, Testimony before the Committee on Oversight and Government Reform, United States House of Representatives. U.S. Department of Labor.

French, K. R., & Roll, R. (1986). Stock return variances: The arrival of information and the reaction of traders. *Journal of Financial Economics*, *17*, 5–26.

Gay, G. D., Simkins, B. J., & Turac, M. (2009). Analyst forecasts and price discovery in futures markets: The case of natural gas storage. *Journal of Futures Markets*, *29*, 451–477.

Genre, V., Kenny, G., Meyler, A., & Timmermann, A. (2013). Combining expert forecasts: Can anything beat the simple average? *International Journal of Forecasting*, *29*, 108–121.

Gilbert, T., Scotti, C., Strasser, G., & Vega, C. (2015). *Is the Intrinsic Value of Macroeconomic News Announcements Related to their Asset Price Impact?* (Working Paper No. 874). Boston College: Department of Economics.

Hall, K. (2012, June). Addressing concerns about the integrity of the U.S. Department of Labor's jobs reporting, testimony before the Committee on Oversight and Government Reform, United States House of Representatives. U.S. Department of Labor.

Hautsch, N., Hess, D., & Veredas, D. (2011). The impact of macroeconomic news on quote adjustments, noise, and informational volatility. *Journal of Banking & Finance*, *35*, 2733–2746.

Hirshleifer, D., Subrahmanyam, A., & Titman, S. (1994). Security analysis and trading patterns when some investors receive information before others. *Journal of Finance*, *49*(5), 1665–1698.

Hu, G. X., Pan, J., & Wang, J. (2013, 11). *Early peek advantage?* (MIT Sloan School of Management)

Javers, E. (2012). Labor dept. asks nuclear guardians for help keeping jobs data secret. Retrieved on March 7, 2012, from `www.cnbc.com`.

Javers, E. (2013a). Conference Board ends embargoes after leaks. Retrieved on June 13, 2013, from `www.cnbc.com`.

Javers, E. (2013b). NY AG's early data probe goes beyond Reuters. Retrieved on July 8, 2013, from `www.cnbc.com`.

Javers, E. (2013c). Thomson Reuters gives elite traders early advantage. Retrieved on June 12, 2013, from `www.cnbc.com`.

Kaniel, R., Liu, S., Saar, G., & Titman, S. (2012). Individual investor trading and return patterns around earnings announcements. *Journal of Finance*, *67*, 639–680.

Kim, O., & Verrecchia, R. E. (1997). Pre-announcement and event-period private information. *Journal of Accounting and Economics*, *24*(3), 395–419.

Kyle, A. S. (1985). Continuous auctions and insider trading. *Econometrica*, *53*, 1315–1335.

Lucca, D. O., & Mönch, E. (2015). The pre-FOMC announcement drift. *Journal of Finance*, *70*(1), 329–371.

Mullins, B. (2014). Labor Department panel calls for ending lockup for jobs data. *Wall Street Journal*. Retrieved on January 2, 2014, from `www.wsj.com`.

Mullins, B., & Patterson, S. (2013). Deutsche Börse's news service for traders draws scrutiny of investigators. *Wall Street Journal*. Retrieved on August 12, 2013, from `www.wsj.com`.

Office of Management and Budget. (1985). Statistical policy directive on compilation, release, and evaluation of principal federal economic indicators. *Federal Register*, *50*(186).

Pearce, D. K., & Roley, V. V. (1985). Stock prices and economic news. *Journal of Business*, *58*, 49-67.

Rogow, G. (2012). High-frequency traders flat-out buying data ahead of you. *Wall Street Journal*. Retrieved on August 28, 2012, from `www.wsj.com`.

Savor, P., & Wilson, M. (2013). How much do investors care about macroeconomic risk? Evidence from scheduled economic announcements. *Journal of Financial and Quantitative Analysis*, *48*(02), 343–375.

Scholtus, M., van Dijk, D., & Frijns, B. (2014). Speed, algorithmic trading, and market quality around macroeconomic news announcements. *Journal of Banking & Finance*, *38*, 89–105.

Sinha, P., & Gadarowski, C. (2010). The efficacy of regulation fair disclosure. *Financial Review*, *45*, 331–354.

Wall Street Journal. (1986). U.S. agency fires employee accused of leaking data. *Wall Street Journal*, August 22, 1986.

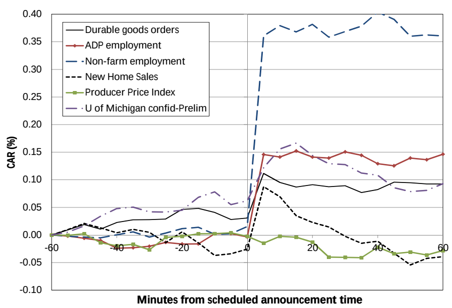
Yohai, V. J. (1987). High breakdown-point and high efficiency robust estimates for regression. *Annals of Statistics*, *15*, 642–656.

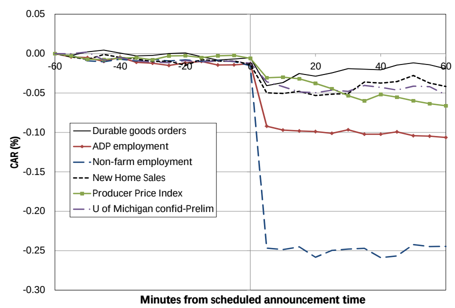
A Appendix: Additional Figures and Tables

Figure A1: Cumulative Average Returns for Individual Announcements

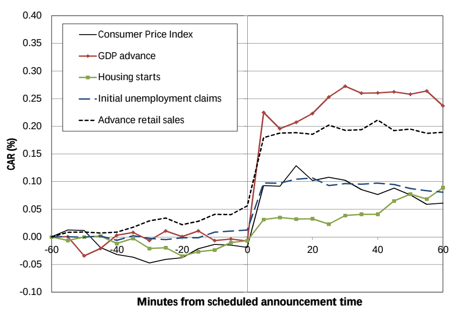
E-mini S&P 500 Futures | **10-year Treasury Note Futures**

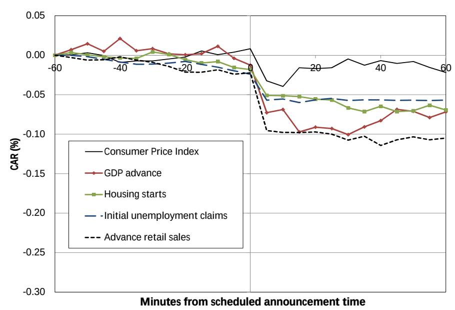
Panel a): Announcements with no evidence of drift



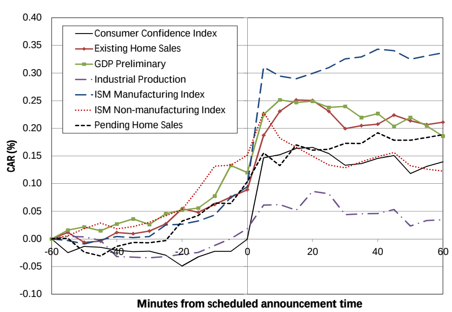


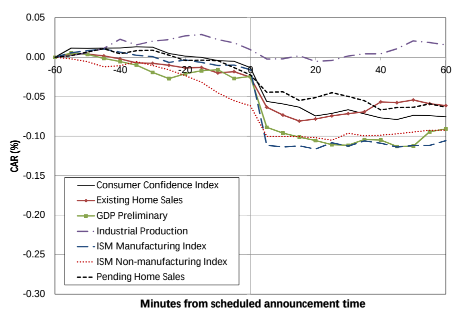
Panel b): Announcements with some evidence of drift





Panel c): Announcements with strong evidence of drift





Announcements are categorized as drift, no drift and some evidence of drift using the classification in Table 4. For each category, we compute mean cumulative average returns in the event window from sixty minutes before the announcement time to sixty minutes after the announcement time.

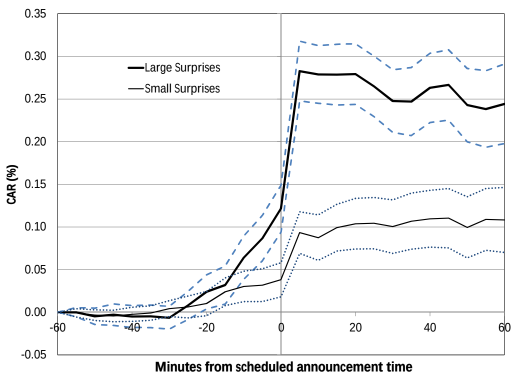
Table A1: Pre-announcement Price Drift as a Proportion of the Total Price Change using CARs

	E-mini S&P 500 Futures			10-year Treasury Note Futures		
	CAR $[t-30min, t+30min]$	CAR $[t-30min, t-5sec]$	Γ_m	CAR $[t-30min, t+30min]$	CAR $[t-30min, t-5sec]$	Γ_m
ISM Non-manufacturing index	0.099	0.121	122%	-0.086	-0.051	59%
Industrial production	0.078	0.053	68%	-0.019	-0.011	58%
Pending home sales	0.180	0.110	61%	-0.058	-0.031	53%
GDP preliminary	0.214	0.094	44%	-0.092	-0.005	5%
Existing home sales	0.185	0.075	41%	-0.064	-0.018	28%
ISM Manufacturing index	0.321	0.090	28%	-0.113	-0.017	15%
CB Consumer confidence index	0.155	0.022	14%	-0.079	-0.026	33%
Mean			54%			36%

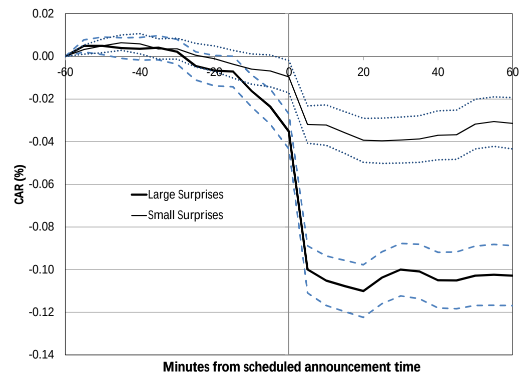
The sample period is from January 1, 2008 through March 31, 2014. Only the announcements that show significant pre-announcement price drift in stock index or Treasury note futures prices (based on the joint test in Table 3) are included. Proportion values are calculated as CARs in the 30 minutes before the announcement to five seconds before the announcement window divided by the CARs in the 30 minutes before the announcement to 30 minutes after the announcement window.

Figure A2: Cumulative Average Returns for Large and Small Surprises

E-mini S&P 500 Futures



10-year Treasury Note Futures

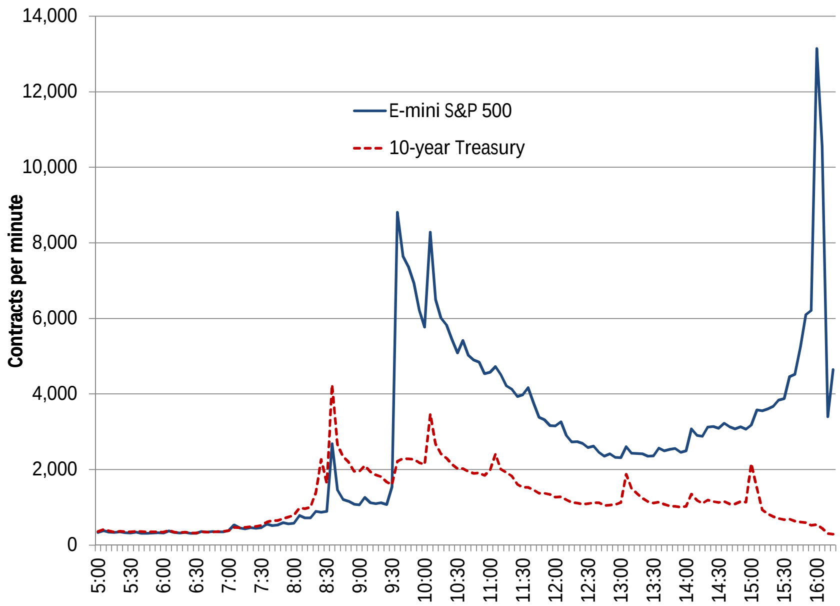


We classify surprises in the 1st and 4th quartiles as large and the remaining surprises as small for the seven announcements exhibiting drift in Table 4. The figure shows mean cumulative average returns in the event window from 60 minutes before the announcement time to 60 minutes after the announcement time. Dashed lines are mean one-standard-error bands. Although small surprises are also associated with drift, it is the large surprises that drive the results, reflecting the fact that small surprises do not offer much room for informed traders to make profits before the announcements.

We also estimate Equation (1) separately for large and small surprises. These results (not tabulated to save space) again indicate that the preannouncement drift is mainly driven by the large surprises.

Note that Bernile et al. (2015) classify surprises as large using a variety of methods such as the 10th and 90th percentiles of individual analyst forecasts, minimum and maximum forecasts, and comparing surprises standardized by their rolling-window standard deviation against some threshold. They show that the results do not differ across the different methods.

Figure A3: Trading Volume



This figure shows the average trading volume in number of contracts per minute. Only the period from 5:00 to 16:00 Eastern Time is shown because no announcements are made at nighttime as indicated in Table 1.

B Appendix: Results for Section 5.2

Table B1: Results of Forecasting the Surprise Using Individual Analyst Forecasts

	DM-Stat	p-value
ADP employment	-1.062	0.856
Advance retail sales	0.687	0.246
CB Consumer confidence index	1.010	0.156
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 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
Producer price index	1.758	0.039
UM Consumer sentiment index - Prel	0.373	0.355

The sample period is from January 1, 2008 through March 31, 2014. The value of 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 analysts 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 analysts, $H_1 : E\left[\tilde{S}_{mt} - P_{mt}\right]^2 < E\left[\tilde{S}_{mt}\right]^2$.

Table B2: Regression of Unstandardized Surprise, $\widetilde{S}_{mt}$, on a Constant and Prediction, P_{mt}

	Intercept	s.e.	p-value	Slope Coefficient	s.e.	p-value	R^2
ADP employment	4,672.400	6,962.300	0.251	0.173	0.371	0.320	0.02
Advance retail sales	-0.0004	0.001	0.771	1.096	0.724	0.065	0.07
CB Consumer confidence index	-0.358	0.619	0.719	1.188	0.586	0.021	0.06
Consumer price index	-0.0001	0.0001	0.839	0.961	0.113	<0.001	0.36
Durable goods orders	-0.001	0.002	0.709	1.946	0.468	<0.001	0.17
Existing home sales	-0.013	0.025	0.698	1.621	0.767	0.017	0.09
GDP advance	-0.0003	0.001	0.592	1.371	0.784	0.040	0.17
GDP preliminary	-0.0005	0.001	0.767	0.118	0.593	0.421	0.04
Housing starts	-2,926.000	6,527.300	0.673	0.039	0.453	0.466	0.01
Industrial production	-0.001	0.0005	0.951	1.026	0.318	0.001	0.22
Initial jobless claims	1,278.200	1,098.800	0.122	0.360	0.289	0.106	0.01
ISM Manufacturing index	0.216	0.268	0.210	0.580	0.540	0.141	0.03
ISM Non-manufacturing index	0.033	0.235	0.444	-0.149	0.782	0.575	0.01
New home sales	-4,596.600	4,301.500	0.857	-0.324	1.157	0.610	0.01
Non-farm employment	-11,156.000	7,567.300	0.930	-0.052	0.332	0.562	0.01
Pending home sales	0.003	0.005	0.293	0.762	0.405	0.030	0.08
Producer price index	0.0002	0.0004	0.349	1.206	0.397	0.001	0.15
UM Consumer sent. - Prel	-0.928	0.450	0.980	0.608	0.821	0.229	0.02

The sample period is from January 1, 2008 through March 31, 2014. The prediction, P_{mt}, is based on the consensus of the ranked analysts. Results are from the Ordinary Least Squares regression, where the standard errors are based on a heteroskedasticity consistent covariance matrix.

Table B3: Regression of Returns on Prediction

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

	E-mini S&P 500 Futures			10-year Treasury Note Futures			Wald	
	γ_m	s.e.	R^2	γ_m	s.e.	R^2	Test	p-value
ADP employment	1.83E-06	9.30E-07	0.03	-1.14E-06	4.22E-07	0.09	11.108	0.004
Advance retail sales	1.849	16.343	0.01	-7.427	8.471	0.02	0.781	0.677
CB Consumer confidence idx	-0.005	0.041	0.01	-0.020	0.007	0.06	7.788	0.020
Consumer price index	0.665	26.592	0.01	-2.495	11.262	0.01	0.050	0.975
Durable goods orders	4.205	2.801	0.03	-1.627	1.565	0.03	3.334	0.189
Existing home sales	0.380	1.723	0.02	-0.555	0.473	0.06	1.427	0.490
GDP advance	50.802	31.724	0.22	-9.644	9.110	0.08	3.685	0.158
GDP preliminary	4.852	46.964	0.04	-6.953	14.221	0.05	0.226	0.893
Housing starts	4.83E-07	1.23E-06	0.01	-1.16E-06	4.45E-07	0.04	6.959	0.031
Industrial production	6.070	9.757	0.02	-10.651	2.460	0.07	19.136	<0.001
Initial jobless claims	-5.02E-06	2.03E-06	0.02	1.09E-06	9.48E-07	0.01	7.340	0.025
ISM Manufacturing index	-0.025	0.184	0.01	0.013	0.038	0.02	0.127	0.938
ISM Non-manufacturing index	0.019	0.077	0.01	-0.018	0.041	0.02	0.249	0.883
New home sales	-4.34E-06	8.81E-06	0.02	-2.39E-06	1.72E-06	0.03	2.167	0.338
Non-farm employment	4.61E-07	1.03E-06	0.02	-3.14E-07	5.61E-07	0.02	0.513	0.774
Pending home sales	-1.395	1.938	0.02	-0.728	0.411	0.03	3.649	0.161
Producer price index	-21.071	16.729	0.03	10.178	7.037	0.04	3.679	0.159
UM Consumer sent. - Prel	-0.151	0.071	0.04	0.003	0.017	0.01	4.561	0.102

b) $[t - 30min, t + 30min]$ **Window**

	E-mini S&P 500 Futures			10-year Treasury Note Futures			Wald	
	γ_m	s.e.	R^2	γ_m	s.e.	R^2	Test	p-value
ADP employment	1.32E-06	2.33E-06	0.02	2.27E-09	1.31E-06	0.01	0.318	0.853
Advance retail sales	32.521	27.708	0.03	-14.612	16.598	0.02	2.153	0.341
CB Consumer confidence idx	0.026	0.050	0.02	-0.037	0.029	0.06	1.847	0.397
Consumer price index	-23.258	64.274	0.02	-21.909	20.316	0.03	1.294	0.524
Durable goods orders	6.643	5.771	0.02	-7.909	3.787	0.07	5.688	0.058
Existing home sales	-1.436	1.592	0.02	-0.617	0.565	0.03	2.005	0.367
GDP advance	37.948	56.477	0.06	18.257	21.276	0.07	1.188	0.552
GDP preliminary	0.94	77.338	0.04	11.259	27.121	0.02	0.173	0.917
Housing starts	2.73E-07	2.01E-06	0.01	-7.56E-07	9.59E-07	0.02	0.640	0.726
Industrial production	-28.921	19.685	0.05	-0.494	4.975	0.01	2.168	0.338
Initial jobless claims	-1.03E-05	3.84E-06	0.02	1.32E-06	1.98E-06	0.00	7.629	0.022
ISM Manufacturing index	-0.166	0.261	0.03	0.021	0.066	0.02	0.508	0.776
ISM Non-manufacturing index	0.327	0.172	0.09	-0.043	0.037	0.02	4.972	0.083
New home sales	8.98E-07	1.15E-05	0.01	-3.31E-06	4.20E-06	0.02	0.629	0.730
Non-farm employment	-1.31E-06	5.11E-06	0.02	1.25E-06	2.45E-06	0.02	0.328	0.849
Pending home sales	2.143	4.021	0.02	-0.776	0.957	0.02	0.942	0.624
Producer price index	-36.143	21.339	0.03	-3.958	16.001	0.02	2.930	0.231
UM Consumer sent. - Prel	-0.037	0.105	0.01	-0.026	0.032	0.02	0.803	0.669

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 unstandardised surprise, $\tilde{S}_{mt}$, instead of the actual surprise. The prediction, P_{mt}, is based on the consensus of the ranked analysts. The standard errors are based on a heteroskedasticity consistent covariance matrix.