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

Alexander Kurov[y] Alessio Sancetta[z] Georg Strasser[x]

Marketa Halova Wolfe[{]

First Draft: June 15, 2014
This Draft: November 10, 2017

Journal of Financial and Quantitative Analysis, forthcoming

Abstract

We examine stock index futures and Treasury futures around the release time of 30 U.S. macroeconomic announcements. Nine of the 20 announcements that move

1 Introduction

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

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

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

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

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

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

Fourth, we study a recent sample period. Announcement release procedures change over time, and [illegible]

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

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

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

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

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

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

While these ndings are suggestive, one cannot conclude that information leakage causes observed pre-announcement drift because other possible causes of informed trading exist. In particular, we consider information generated by informed investors and impounded into prices through their trading (French & Roll, 1986). Some traders may be able to collect proprietary information or analyze public information in a superior way to forecast an-

nouncements better than other traders. This knowledge could then be utilized to trade in the \correct" direction before announcement releases. We conduct an extensive forecasting exercise with public information (individual analyst forecasts). We also show that proprietary information permits forecasting announcement surprises in some cases.

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

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

2 Data

This section describes the announcements data and markets data.

2.1 Expected and Released Values of Macroeconomic Announcements

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

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

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

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

[4] Andersen et al. (2003) and Andersen et al. (2007) list 24 macroeconomic announcements. We do not report results for Capacity Utilization because it is always released simultaneously with Industrial Production

Table 1: Overview of U.S. Macroeconomic Announcements

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

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

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

standardized surprise, S_{mt}, is then

$$S_{mt} = \frac{A_{mt} \quad E_t \quad [A_{mt}]}{m}. \tag{1}$$

We proxy the expectation, $E_t \quad [A_{mt}]$, by the median response of professional forecasters during the days before the release, $E_t \quad [A_{mt}]$, where $\quad > 0$.[7] We assume that the expectation $E_t \quad [A_{mt}]$ about a macroeconomic announcement is exogenous, in particular not a ected by asset returns during [*t ;td [has.551.9552]. 3.058 0 Td [( )]TJ0447 7.91.95528*

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

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

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

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

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

[11]

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

$$R_t = {}_0 + \sum_{j=1}^{J} {}_j R_{j;t\ 1} + \sum_{m=1}^{M} \sum_{k=\ 1}^{K} {}_{mk} S_{m;t+k} + {}_t. \tag{2}$$

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

with these standardized variables.

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

4 Empirical Results

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

4.1 Pre-Announcement Price Drift

coe cients are signi cant at the 5% level indicating a pre-announcement price drift (column 4). Four of these announcements exhibit signi cant drift in the stock market (column 2), and all nine announcements exhibit signi cant drift in the bond market (column 3).[16] In all nine announcements, the drift is in the \correct" direction, i.e., the direction of the price change predicted by the announcement surprise.

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

The full set of macroeconomic announcements is vast. Most announcements, however, contain information of only secondary importance. These announcements have only a negligible e ect on the market and thus no meaningful pro t potential for informed traders.[17] To limit the analysis to the set of relevant, i.e., market-moving, announcements we use the sum of the coe cients on the lagged, contemporaneous and lead surprises, $\sim_m \sum_{k=1}^{K}$

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

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

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

market-moving announcements by testing the null hypothesis that surprises have no e ect in each market, i.e., $\sim_m = 0$. The two middle columns of Table 3 present { analogous to Table 2 { the results of this t-test on $\sim_m$ separately for the stock and the bond market. The last column tests the hypothesis that the sums in these two markets are jointly di erent from

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

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

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

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

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

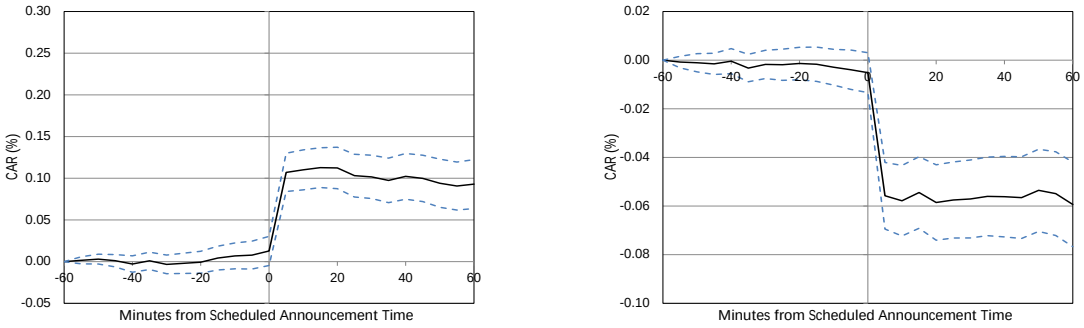
The sample period is from January 1, 2008 through March 31, 2014. Only announcements showing signi cant evidence (at the 5% level) of pre-announcement drift in each market in Table 2 are included. \n.d." denotes no signi cant drift (at the 5% level) in the S&P 500 market.

We classify each event based on whether the surprise has a positive or negative e ect on the stock and bond markets using the coe cients in Table 3. Following Bernile et al. (2016), we invert the sign of returns for negative surprises.[20] CARs are then calculated in the [t − 60min; t + 60min] window for each of the \drift" and \no drift" categories based on Tables 2 and 3: In the stock market, there are four drift and sixteen no-drift announcements, and in the bond market, there are nine drift and eleven no-drift announcements.[21] The CARs in Figure 1 reveal what happens around these announcements. In the no-drift announcements in Panel (a), a signi cant price adjustment does not occur until after the

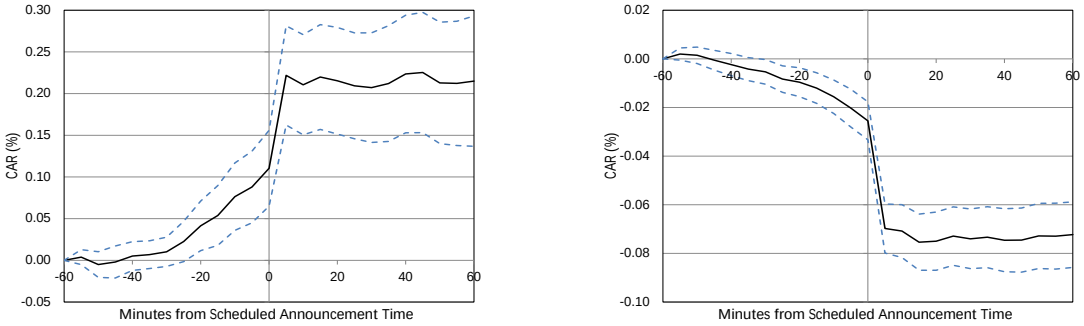
Figure 1: Cumulative Average Returns

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

(a) Announcements without evidence of drift



(b) Announcements with evidence of drift



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

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

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

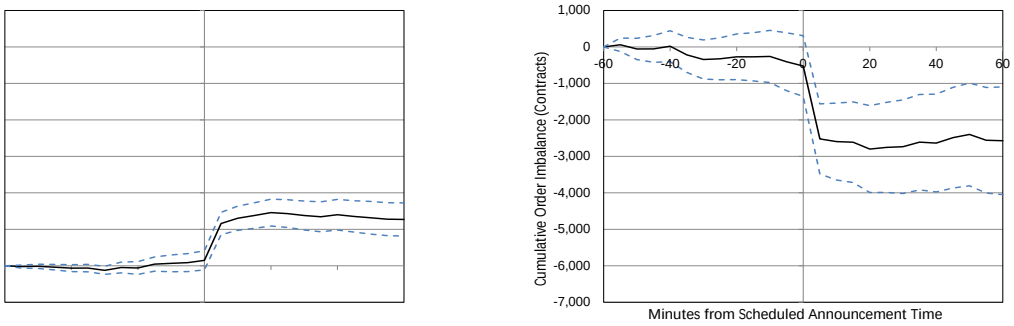
Second, traders may choose to execute trades close to the release time instead of during the preceding hours in order to minimize exposure to risks that are unrelated to the macroeconomic announcement but are driven by other unpredictable economic or geopolitical events.

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

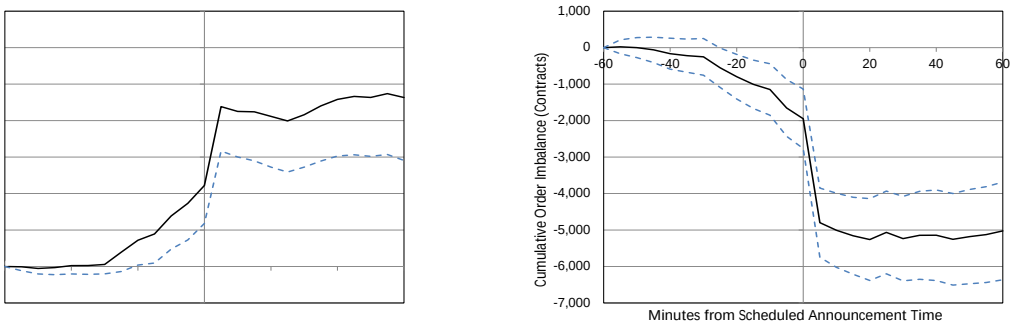
Figure 2: Cumulative Order Imbalances

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

(a) Announcements without evidence of drift



(b) Announcements with evidence of drift



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

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

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

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

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

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

4.3 Increase in Drift After 2007

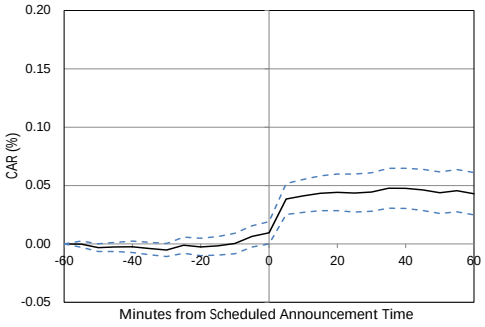
Our *second-by-second* data starts on January 1, 2008. The existing literature referenced in Section 1 analyzes earlier sample periods, for which we do not have such high-frequency data. However, we have *minute-by-minute* data for the sample period from August 1, 2003 to December 31, 2007. Therefore,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

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

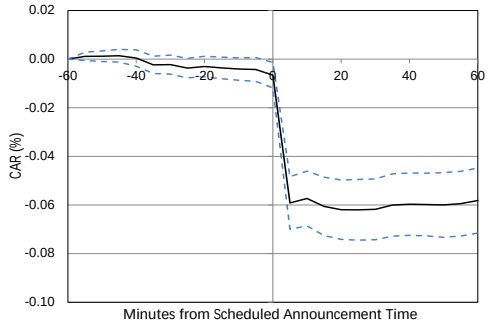
A variety of factors may have contributed to this change. One contributing factor may be a differential impact of macroeconomic announcements on financial markets between recessions and expansions as shown by, for example, Boyd, Hu, and Jagannathan (2005) and Andersen et al. (2007). This state-dependence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

Figure 3: Cumulative Average Returns, 2003{2007

E-mini S&P 500 Futures



10-year Treasury Note Futures



The sample period is from August 1, 2003 through December 31, 2007. We classify each event as \good" or \bad" news based on whether the announcement surprise has a positive or negative e ect on the stock

quantitative easing and communication with the markets muted expectations of notable tightening of monetary policy until the spring of 2013. Absent any imminent tightening, good economic news continued to be good news for stocks until the end of our sample period. The strong response of the stock market to macroeconomic announcements increased the rewards

perfections. First, the calculation of the consensus forecast by Bloomberg is a plausible summary statistic of the forecasters' responses but not necessarily the best one. Second, the forecasters' responses might not re ect an optimal forecast, which creates room for some traders to analyze public information in a superior way. Third, if the sampling of expectations precedes the beginning of the event window, i.e., if > , market expectations might change by time *t* . We discuss these possible explanations in Section 5.2.1. Section 5.2.2 discusses the possibility of uninformed traders \jumping on the bandwagon" with informed traders.

5.1 Private Information

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

5.1.1 Information Leakage

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

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

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

However, ensuring that market participants receive all market-moving macroeconomic data at the same time is complicated by the fact that some important data is collected

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

Announcement	Source

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

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

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

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

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

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

Only for the CB Consumer Con dence Index the release procedure changed during our sample period. Otherwise, the indicator variables for a given announcement are constant over time. The identi cation of the e ect of release procedures must, therefore, rely on cross-sectional variation. To allow the pooling of announcements, we adjust the sign of the surprises such that a positive surprise increases stock and lowers bond prices based on the sign of the sum of γ_m coe cients in Table 3.[30]

Let us denote the sign-adjusted surprise by $S_{m;t}$. We de ne S_t as the cross-sectional average of all non-zero surprises at time t:

$$S_t = \Big(\sum_{m=1}^{M} S_{m;t}\Big) \Big/ \Big(\sum_{m=1}^{M} \mathbb{1}(|S_{m;t}| > 0)\Big).$$

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

$$X_{i;t} = \Big(\sum_{m=1}^{M} \big[S_{m;t}\mathbb{1}(X^i_{m;t} = 1)\big]\Big) \Big/ \Big(\sum_{m=1}^{M} \mathbb{1}(|S_{m;t}| > 0)\Big).$$

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

$$R_t = \alpha_0 + \sum_{j=1}^{P} \beta_j R_{j;t-1} + \sum_{k=-1}^{K} \gamma_k S_{t+k} + \sum_{i=1}^{I} \delta_{ik} X_{i;t+k}$$

Table 6: E ect of Organization Type and Release Procedures

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

The sample period is from January 1, 2008 through March 31, 2014. The results show weighted least squares estimates of equation (3) for each market as described in Section 3. Reported coe cients in the rst row are the sum $\sum_{k=0}^{5} \_{k}$ and in the bottom three rows the sum $\sum_{k=0}^{5} \_{ik}$. Standard errors are shown in parentheses. *, **, and *** indicate statistical signi cance at 10%, 5% and 1% levels, respectiv254tiv indicate =0=0

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

5.2 Public Information

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

5.2.1 Individual Analyst Forecasts

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

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

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

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

$$P_{mt} = E_t^{Ranked}[A_{mt}] \quad E_t \quad [A_{mt}]: \tag{4}$$

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

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

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

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

[38] We use a forecast of the *unstandardized* surprise $\hat{S}_{mt} = A_{mt} \quad E_t \quad [A_{mt}] = \quad _m S_{mt}$ to avoid the estimation of additional parameters.

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

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

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

5.2.2 Bandwagon Effect

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

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

[40] Appendix Table A1 shows the results.

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

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

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

6 Conclusion

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

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

The de nite source of the drift remains an open question. In view of the public interest in the safeguarding of macroeconomic data and considering the public and regulatory attention that leakage has received, for example, in the recent hacking scandal (SEC, 2015), the source of informed trading merits more research. Of particular interest is the e ect of proprietary

real-time data collection on announcement surprises and prices, and a comparison of pre-announcement e ects across countries with di erent regulations and supervisory structures.

References

Admati, A. R., & P eiderer, P. (1988). A theory of intraday patterns: Volume and price variability. *Review of Financial Studies*, *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 e ects of macro announcements: Real-time price discovery in foreign exchange. *American Economic Review*, *93*, 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*, 251{277.

Andersson, M., Overby, L. J., & Sebestyen, 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.

Bernile, G., Hu, J., & Tang, Y. (2016). Can information be locked-up? Informed trading ahead of macro-news announcements. *Journal of Financial Economics*, *121*, 496{520.

Boyd, J. H., Hu, J., & Jagannathan, R. (2005). The stock market's reaction to unemployment

trading, stock returns, and earnings announcements. *Journal of Financial Economics*, *92*, 66{91.

Chang, C., 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.

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 StatistF1711.9552Tf140.460911.9552 StatistF1711.97eF033.1540Td[(29)]TJ/F1711.9552T*

108{121.

Gilbert, T., Scotti, C., Strasser, G., & Vega, C. (2017). Is the intrinsic value of a macroeconomic news announcement related to its asset price impact? *Journal of Monetary Economics*, *92*, 78{95.

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. (2017). Early peek advantage? E cient price discovery with tiered information disclosure. *Journal of Financial Economics*. (forthcoming)

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

Javers, E. (2013b). 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*(2), 639{680.

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

28, 871-888.

Press Release. Retrieved on August 11, 2015, from www.sec.org.

A Appendix

A.1 Impact of Early Signals

mation, changes their expectation of X to

$$E[X|A_1] \quad _{X1} = \quad ^{1}_{X1}(\ _{X0} \ \ _{X0} + \ _{A1}A_1) \tag{5}$$

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

$$E[X|A_1; A_2] \quad _{X2} = \quad ^{1}_{X2}(\ _{X1} \ \ _{X1} + \ _{A2}A_2) \tag{6}$$

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

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

$$E[U(W)] = E[\ \ exp(\ \ DX)];$$

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

$$p_1 \quad p_0 = \frac{_{A1}}{_{X1}}(A_1 \quad _{X0}):$$

At the o cial release time, the price changes again, now by

$$p_2 \quad p_1 = \frac{_{A2}}{_{X2}}(A_2 \quad _{X1}):$$

For concise notation, we write for each surprise $S_i \quad A_i \quad _{X\,i\ 1}$. The following proposition provides a condition for the price change in the pre-release period exceeding the price change at the o cial release time.

Proposition (Impact of Early News)

$$p_1 - p_0 > p_2 - p_1 \quad , \quad \frac{\sigma_{A1}}{\sigma_{A2}} + \frac{\sigma_{A1}}{\sigma_{X0} + \sigma_{A1}} > \frac{S_2}{S_1} \tag{7}$$

Proof:

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

q.e.d.

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

A.2 Additional Figures and Tables

Figure A1: Trading Volumes

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

	DM	*p*-value
ADP employment	-1.06	0.86
Advance retail sales	0.69	0.25
CB Consumer con dence index	1.01	0.16
Construction spending	-4.42	1.00
Consumer price index	2.81	0.00
Durable goods orders	2.56	0.01
Existing home sales	1.32	0.09
GDP advance	1.00	0.16
GDP nal	-3.20	1.00
GDP preliminary	-0.75	0.77
Housing starts	-0.83	0.80
Industrial production	1.81	0.04