Drift Begone! Release Policies and Preannouncement Informed Trading*

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Abstract

In 2017 the UK Statistics Authority discontinued the early access of government officials to market-sensitive macroeconomic data. We examine the effect of this policy change on price adjustment in the foreign exchange futures market around major U.K. macroeconomic announcements. Three macroeconomic announcements (consumer price index, industrial production, and retail sales) show strong evidence of informed trading before their public releases until 2017. This preannouncement price drift weakens with the end of the prerelease access. Consistent with less informed trading before the announcements, the market reaction to the announcements at the official release time has become larger, and the speed of adjustment has become slower. Our findings have policy implications for countries that still utilize macroeconomic announcement prereleases to government officials.

Keywords: Macroeconomic news announcements; foreign exchange market; release policies; preannouncement effect; drift; informed trading
JEL classification: G14; E44; F31; D82

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1 Introduction

Macroeconomic news announcements provide crucial public information about the economy and influence asset prices. According to Savor and Wilson (2013), announcement days account for more than one half of the cumulative annual equity risk premium. Given the importance of announcement information, fairness requires that all market participants receive access to the information at the same time. Access to news about macroeconomic announcements is therefore closely guarded until the official release time. Despite this, some macroeconomic announcements show evidence of informed trading before their official release. For example, Bernile, Hu, and Tang (2016) show evidence of informed trading before the U.S. Federal Open Market Committee monetary announcements from 1997 to 2013 and Kurov, Sancetta, Strasser, and Wolfe (2019) find that from 2008 to 2014 nine out of 20 market-moving U.S. macroeconomic announcements show preannouncement price drift, i.e., a drift in the direction of the price move predicted by the announcement surprise. The drift starts about 30 minutes before the release time and on average comprises approximately 40 percent of the total price move.

Informed trading can be attributed to a variety of sources including information leakage, collection of proprietary data proxying the announcement data, and superior ability of individual traders to forecast based on public data. Kurov et al. (2019) provide evidence in support of the leakage hypothesis based on the differences in release policies across macroeconomic announcements. They find that announcements with less secure release policies are associated with a stronger preannouncement drift. However, this result relies on assumptions about comparability across different announcements and could be driven by other unobserved characteristics of the announcements.

In this paper we overcome this limitation by exploiting a change in release procedures for macroeconomic announcements in the U.K. The UK Statistics Authority's Office for National Statistics (ONS) and the Bank of England used to release announcement data before the official release time to individuals such as government ministers and their advisers

(UK Statistics Authority, 2010). The rationale for the prereleases included briefing the ministers before them commenting on the data. Although the National Statistics Code of Practice for releasing announcements stated that the number of individuals receiving early access should be strictly limited (National Statistics, 2002), Bird (2017) reported that over 100 individuals had access to some of the data one day ahead of release. The wide distribution of information prior to its official release sparked concern about information leakage. The concern built up over the years with, for example, the ONS recommending in a special report to the U.K. Government and Parliament (UK Statistics Authority, 2010) that the release policies be changed and repeating the same guidance in its 2014 strategy outline (UK Statistics Authority, 2014). After a prolonged debate,[1] the ONS eliminated the prerelease on July 1, 2017 (Pullinger, 2017) and the Bank of England followed on July 24, 2017 (Data and Statistics Division, 2017). This unique setting – a change in the release policy of many announcements during a short period of time – makes it possible to analyze the effect of data security on the market reaction to announcements holding the announcements' characteristics constant and to infer the role of informed trading before the change in release procedures.

To analyze the market impact of the U.K. macroeconomic announcements, we use data for the British Pound to the U.S. dollar (GBP/USD) exchange rate market. We focus on this foreign exchange market for three reasons. First, it is the most liquid market related to the U.K. economy. Second, previous research on the impact of U.K. macroeconomic announcements such as Dominguez and Panthaki (2006) and Love and Payne (2008) also focus on it. Third, the foreign exchange market is unregulated, making it an attractive vehicle for traders with early access to public information documented by, for example, Wearden (2019) and Aldrick and Ellery (2019) who discuss foreign exchange speculators profiting

[1] An "Independent Review of UK Economic Statistics" mentioned breaches of the prerelease rules and recommended that the list of officials with prerelease access should be as short as possible (Bean, 2016). The Royal Statistical Society issued a manifesto in 2017 (Royal Statistical Society, 2017) and coordinated a letter to *The Times* signed by 114 statisticians and academics advocating for the prerelease to be eliminated completely (Spiegelhalter, 2017). A survey of the public conducted by NatCen Social Research revealed that two thirds of respondents preferred the prerelease be discontinued (Simpson, 2016).

from early access to an audio feed of the Bank of England press conferences; in contrast, the stock and government bond futures markets are regulated, and the U.K. Financial Conduct Authority would pursue insider trading in those markets.

Because our interest focuses on the effect of the policy change in 2017, we use data around this date. Our sample period begins on January 1, 2012 to avoid any lingering effects of the 2008-2009 recession and ends on December 31, 2019. Our results show that three of the four ONS announcements that move the foreign exchange market exhibited a preannouncement price drift before the release policy was changed. The drift started about 30 minutes before the release time and comprised on average about 42 percent of the total price move. Did this preannouncement price drift change after the tightening of the release policies? Did the impact of announcements on the foreign exchange market at release time change? Did the speed of market adjustment change? We find evidence for all these three changes. The preannouncement price drift declined significantly, the market reacted more strongly at release time after the release policy change, and the market takes longer to adjust to the announcement information. Taken together, these findings indicate that the release policy considerably affects the reaction of the foreign exchange market to the U.K. macroeconomic announcements.

We contribute to the literature in several ways. First, previous studies of informed trading before macroeconomic announcements analyzed sample periods that did not contain policy changes. In contrast, we examine a sample period that contains a substantial policy change of eliminating prerelease access, which allows us to analyze the impact of the policy change.

Second, previous studies of informed trading focus on the U.S. macroeconomic announcements and financial markets. Specifically, Bernile et al. (2016) look at the impact of the U.S. macroeconomic and monetary announcements on the U.S. stock market and Kurov et al. (2019) examine the impact of the U.S. macroeconomic announcements on the U.S. stock and bond markets). By examining the impact of the U.K. macroeconomic announcements on the GBP/USD exchange rate, we extend these studies to show that informed trading is not limited to the U.S. but exists in the U.K. as well. This extends previous research on the effect of the U.K. macroeconomic announcements on the foreign exchange market by Dominguez and Panthaki (2006) and Love and Payne (2008) who use a 10-month sample period from 1999 to 2000 and find no evidence of preannouncement price moves. This important topic deserves a reexamination with more recent data because it is quite possible that the market reaction to the announcements has changed as conditions under which the announcements are released have evolved.[2] For example, the number of individuals with prerelease access to the official macroeconomic announcements increased over time. We requested information through the Freedom of Information Act from the ONS and Bank of England to learn how the number of individuals with prerelease access has evolved over time. We were able to obtain this information for the gross domestic product (GDP) from 2006 to 2017: From 2006 to 2009 there were 10 or 11 individuals with the prerelease access, this number doubled to 20 individuals in 2010 and again doubled to 39 in 2011 before increasing to around 50 in 2015-2017.[3]

Third, previous studies of informed trading focus on announcements that are prereleased to media reporters in "lock-in" rooms that enable the reporters to comprehend the announcements before preparing their news stories and therefore provide higher-quality news coverage for the public. This lock-in pre-release period is fairly short, for example, 30 minutes in the U.S. Bureau of Labor Statistics and the U.S. Bureau of Census and 60 minutes for the U.S. Bureau of Economic Analysis announcements. In contrast, our paper focuses on announcements that are prereleased to a much larger number of individuals such as government officials a much longer period (24 hours) before the official release time.

Fourth, our results have practical policy implications for countries that still provide prerelease access to official macroeconomic statistics, which is the case in Australia, Canada,

[2]Methods used to collect data have also changed. For example, Mukherjee, Panayotov, and Shon (2021) show that modern technologies such as satellite imagery are now used to collect proprietary data that proxies for the official macroeconomic announcement data, allowing trading on the information before the release time.

[3]We were not able to obtain the number for individuals with prerelease access for all the years for other macroeconomic announcements.

France, Germany, Israel, New Zealand, South Korea and the U.S. according to Georgiou (2020). In many other countries, prerelease access to such statistics occurs informally in the context of relationships based on power and transactional interactions. Georgiou (2020) provides a number of arguments against prerelease access, including the fact that such access undermines public trust in official statistics and creates opportunities for profiteering by some officials with advance access to market-sensitive data. Our paper provides a first systematic analysis of this important policy issue.

The remainder of this paper is organized as follows. In the next section we describe the data and discuss the role of release policies. Section 3 introduces our methodology and reports the results. Section 4 presents robustness checks. Section 5 briefly concludes.

2 Data

In this section we start by describing the macroeconomic announcement data and discussing their release policies. We then briefly describe the foreign exchange market data.

2.1 Macroeconomic Announcement Data and Release Policies

The complete set of macroeconomic news announcements is large as a casual look into, for example, the Bloomberg database confirms. But most announcements have only a negligible impact on the market and on the profit opportunities for traders because they provide information of only secondary importance (Gilbert, Scotti, Strasser, & Vega, 2017). Because only market-moving announcements are relevant for testing our hypotheses, we focus on announcements that are relevant to the financial markets. We utilize the Bloomberg relevance score ranging from 0 to 100 corresponding to the least and the most consequential announcements, respectively, and analyze only those U.K. macroeconomic announcements that have a score of 75 or higher.

Only the unanticipated component of a news announcement, i.e., the announcement surprise, impacts efficient markets. For a macroeconomic announcement m released at time t we calculate the surprise as the actual announcement, A_{mt}, minus the market's expectation of the announcement before its release, $E_{t-\tau}[A_{mt}]$, where $\tau > 0$, following Balduzzi, Elton, and Green (2001). We standardize this difference by the standard deviation of the announcement, σ_m, to convert all announcements to a common unit of measure. We define the standardized surprise, S_{mt}, as

$$S_{mt} = \frac{A_{mt} - E_{t-\tau}[A_{mt}]}{\sigma_m}, \tag{1}$$

where $\sigma_m = \sqrt{\frac{1}{N_m - 1}\Sigma_{i=1}^{N_m}(S_{im} - \overline{S}_m)^2}$ and $\overline{S}_m$ is the average surprise.

The expectation, $E_{t-\tau}[A_{mt}]$, is proxied by the median forecast of professional forecasters. Forecasts of professional forecasters – in our case obtained from Bloomberg as well – have been shown to outperform forecasts based on historical values of macroeconomic variables (Pearce & Roley, 1985). We assume that the expectation $E_{t-\tau}[A_{mt}]$ is exogenous and not impacted by asset returns in $[t-\tau, t]$. The forecasts are unbiased; the mean forecast error is statistically indistinguishable from zero at a 5% significance level for all of our announcements.

Our raw dataset contains several pairs of closely related announcements, which are released simultaneously.[4] Because their surprise components are highly correlated and would thus introduce multicollinearity to our estimation, we include only the one with the higher Bloomberg relevance score. Specifically, the consumer price index (CPI) and the CPI core index are released simultaneously. Their surprise components are correlated with a correlation coefficient of 0.78. We include only CPI because of its higher Bloomberg relevance score. Similarly, two measures of retail sales are released simultaneously: one including and one excluding auto and fuel purchases with a correlation coefficient of 0.96. We use retail sales including auto and fuel purchases, again because of its higher relevance score. Finally, two measures of production, industrial production and manufacturing production, are released

[4]When an announcement is stated in both month-on-month (MoM) (or quarter-on-quarter, QoQ) and year-on-year (YoY) comparison formats, we use change over the most recent horizon. For example, we use CPI MoM rather than CPI YoY.

simultaneously with a correlation coefficient of 0.82. We use industrial production again because it has a higher relevance score. We omit the Bank of England bank rate because it shows almost no variation during our sample period. We also omit jobless claims and the claimant count rate because Bloomberg forecast data has been unavailable for these announcements since January 2017. Unfortunately these two announcements share their release time with the unemployment rate. Omitted variable bias due to the exclusion of the other two employment-related announcements would overstate the impact of the unemployment rate, which we avoid by excluding it as well.

These conventions give us ten macroeconomic announcements listed in Table 1. Our observations are "announcement releases." For example, the GDP QoQ announcement has 90 announcement releases[5] and the CPI MoM announcement has 95 announcement releases. All ten announcements are released at a monthly frequency.

Four of these ten announcements are released by private entities: the Halifax house price index, the purchasing managers' index (PMI) for the manufacturing sector and the PMI for the construction sector by IHS Markit, and the Nationwide house price index by the Nationwide Building Society (NBS).[6] These four announcements were not released to any individuals prior to the official release time. Five announcements are released by the ONS: the CPI, GDP, industrial production, the producer price index (PPI), and retail sales. One announcement (mortgage approvals) is released by the Bank of England. As explained

[5]Before the second quarter of 2018, there were Advance, Preliminary and Final GDP announcements. Beginning with the second quarter of 2018, there were only Preliminary and Final GDP announcements. We consider the possibility that the first GDP announcement in any given month may affect the markets more strongly than the subsequent GDP announcements in that month. Therefore, as a robustness check we separate the GDP announcements into two groups: the first announcement (Advance before the second quarter of 2018 and then Preliminary from the second quarter of 2018 until the end of our sample period) and the subsequent announcements (Preliminary and Final before the second quarter of 2018 and then Final from the second quarter of 2018 until the end of our sample period). The results of this robustness check, available upon request, agree with the results for the combined GDP announcements, so we report the combined GDP announcement results for simplicity.

[6]Bloomberg provides actual released values for the manufacturing sector PMI and the construction sector PMI only since November of 2015. We obtain the values for January of 2012 to October of 2015 from www.investing.com/economic-calendar/manufacturing-pmi-204 and https://www.investing.com/economic-calendar/construction-pmi-44, respectively. The Halifax house price index actual and survey values are not available on Bloomberg for September through December 2019.

in Section 1, the ONS and the Bank of England announcements used to be distributed before the official release time to individuals such as ministers and other government officials (UK Statistics Authority, 2010). The prerelease was eliminated as of July 1, 2017 by the ONS (Pullinger, 2017) and July 24, 2017 by the Bank of England (Data and Statistics Division, 2017). We take advantage of this release policy change to identify the effect of the release procedure on the response of the foreign exchange market to the U.K. macroeconomic announcements.

The new policy allows for prerelease only in exceptional cases when someone needs the statistics "to act or make a decision in the public interest" (Athow, 2018; Pullinger, 2017). Such exceptions have been applied, for example, to a small number of individuals in the Bank of England when inflation and labor market statistics were scheduled to be released shortly before a monetary policy decision (Athow, 2018; Broadbent, 2018). In addition to these prereleases, the ONS and the Bank of England provide some macroeconomic announcement data classified as market-sensitive before public release to selected media reporters in secure briefing rooms (UK Statistics Authority, 2013). These "lock-in" arrangements use procedures comparable to, for example, those used by the U.S. Department of Labor for announcements classified as Principal Federal Economic Indicators (Fillichio, 2012). The purpose of the lock-ins is to promote fast, orderly, and accurate publication of important macroeconomic statistics. Because the information does not leave the lock-in rooms before the official release time, we treat this release policy in our paper as equivalent to a no-prerelease policy.[7]

[7]Lock-in arrangements are not uncontroversial. They come with the challenge of preventing premature disclosure from the lock-in room. For example, journalists in the U.S. Department of Labor (DOL) lock-in room were required to place their cell phones in a designated container, but one individual used his phone during the lock-in (Fillichio, 2012). The media organizations were supposed to use the DOL computer equipment in the lock-in room, but they installed their own equipment, and the DOL staff was not able to verify what the equipment did (Fillichio, 2012; Hall, 2012). Furthermore, such arrangements raise concerns about the equality of access because newswire services such as Bloomberg, Thomson Reuters, and Dow Jones are able to provide the announcement information to high-frequency traders directly from the lock-in room exactly at the official release time, whereas most traders have to rely on the slower publication on the agency website (UK Statistics Authority, 2013). The U.S. Department of Labor therefore eliminated the lock-ins in May of 2020 (Lanman, 2020).

2.2 Foreign Exchange Market Data

To analyze the market impact of macroeconomic announcements, we focus on the foreign exchange market because it is the most liquid market related to the U.K. economy, it is unregulated, and previous studies of the impact of U.K. macroeconomic announcements (Dominguez & Panthaki, 2006; Love & Payne, 2008) also focus on it as discussed in Section 1.[8]

We use second-by-second transaction data for the British pound to the U.S. dollar foreign exchange (GBP/USD) futures market. We use the foreign exchange futures market as a proxy for the more liquid foreign exchange spot market because the main electronic communication networks such as Reuters and EBS do not report trade size data for the spot market. More specifically, we obtain the prices of GBP/USD foreign exchange futures traded on the Chicago Mercantile Exchange (Globex ticker symbol 6B) from Genesis Financial Technologies. Our sample period begins on January 1, 2012 and ends on December 31, 2019. We sample the price data every five minutes. Liquidity of the nearby futures contract diminishes as its expiration date nears; therefore we switch to the subsequent maturity contract when its daily trading volume surpasses the nearby contract volume.

Our identification relies on accurately assigning prices to the pre- or post-announcement intervals. To ensure that trading following any potential inadvertent early release is captured in the postannouncement interval, we follow Kurov et al. (2019) in replacing prices prevailing at announcement release time with prices five seconds before.[9] This avoids biasing any

[8]We also considered analyzing the U.K. government bond (Gilt) futures and the FTSE-100 stock index futures. The Gilt futures start trading at 8:00 London Time. Since our preannouncement interval begins 90 minutes before the release as explained in Section 3.1, we would not be able to include the two announcements listed in Table 1 released before 9:30 London Time (the Nationwide house price index and the Halifax house price index). The FTSE-100 index futures trade before 8:00 London Time, although they are relatively illiquid before 8:00. Based on event study regressions of the FTSE-100 index futures returns in the interval from 90 minutes before to five minutes after the release on the announcement surprises, none of the 10 announcements listed in Table 1 is classified as market-moving at the 5% significance level. This may be due to good economic news leading to expectations of tighter monetary policy, which increases the discount rate for future corporate cash flows (for example, Kurov and Stan (2018)) and mutes the stock market reaction to the announcements. In contrast, the foreign exchange market is highly liquid essentially around the clock and is significantly affected by most of the announcements included in our analysis.

[9]In the U.S., inadvertent early releases have occurred. For example, on June 3, 2013 Thomson Reuters

preannouncement price drift due to inadvertent early release.

We then compute continuously compounded asset returns, R_t, for the entire sample as the first difference between adjacent log prices in this modified time grid. For example, for an announcement released at time t, the return R_t spans the $[t - 5min, t - 5sec]$ interval, i.e., the five minutes before the announcement release excluding the five seconds immediately before the release, the return R_{t-1} spans the $[t - 10min, t - 5min]$ interval, and the return R_{t+1} spans the $[t - 5sec, t + 5min]$ interval, which captures the announcement impact at release time.[10]

Returns are sampled from 5:00 to 15:30 London Time. Our sample therefore always includes at least 120 minutes before the earliest announcement released at 7:00 London Time and excludes the 30 minutes immediately before the 16:00 London Time fixing on the spot foreign exchange market to avoid any potential confounding effects.[11] If any sum of twelve subsequent five-minute returns equals zero, i.e., if the price effectively does not change for one hour, we exclude that day from the sample. This removes 62 days corresponding to holidays.

We also examine outliers defined as returns below the 1st or above the 99th percentiles in the $[t - 90min, t - 5sec]$ window. There are twelve days with such returns. For these twelve days we search for news about the British pound using the Google search engine. For one day we find that the British pound move was due to an event other than macroeconomic announcements: On November 15, 2018 the British pound depreciated due to Brexit concerns. We therefore remove this day from our sample to make sure that this outlier is not affecting our results. We leave the remaining eleven days in our sample.

inadvertently published the Institute for Supply Management Manufacturing Index 15 milliseconds before the release time (Javers, 2013). Scholtus, van Dijk, and Frijns (2014) conclude that such inadvertent early releases are rare, and we are not aware of similar early releases in the U.K.

[10]In this modified time grid, no-release intervals are exactly five minutes, the last prerelease intervals are five seconds shorter, and the first postrelease intervals are five seconds longer. Because the PMI announcement was released to a selected group of traders at 9:28 London Time before April 1, 2014, before this date we change the time of this announcement to 9:25 London Time to ensure that trading following the release to these traders is captured in the postrelease interval.

[11]The sampling end at 15:30 London Time eliminates one release of the mortgage approvals announcement on November 28, 2012 because the Bloomberg data shows 16:57 release time instead of the usual 9:30 time.

We then place the announcement surprises of the ten announcements shown in Table 1 in the same time grid as the returns; the surprise equals zero when there is no announcement release during a time interval.

3 Methodology and Empirical Results

In this section we describe our methodology and empirical results. Section 3.1 presents the methodology for analyzing the effect of macroeconomic announcements on asset returns and selects market-moving announcements. Section 3.2 provides evidence that prior to the release policy change the foreign exchange futures price began to move in the direction predicted by the subsequent announcement "surprise" before some announcements were officially released. We illustrate this with cumulative average return and cumulative order imbalance graphs in Section 3.3. Section 3.4 analyzes how the change in the announcement release policy impacted returns. Section 3.5 shows that after the release policy the market adjusts to the announcements more slowly.

3.1 Methodology

This section provides evidence that prior to the release policy change the foreign exchange market price begins to move in the "correct" direction before some announcements are released. Following Andersen, Bollerslev, Diebold, and Vega (2003) and Kurov et al. (2019), we use a time series methodology that embeds all announcements in one regression:[12]

$$R_t = \beta_0 + \beta_1 R_{t-1} + \sum_{m=1}^{M} \sum_{k=-1}^{K} \gamma_{m,k} S_{m,t+k} + \epsilon_t, \tag{2}$$

where β_0 is a constant, the R_{t-1} term accounts for possible autocorrelation of returns, and the

[12] The number of return lags in chosen by the Bayesian information criterion. We assume that the surprise is exogenous and not affected by previous asset returns. As a robustness check, we use an event study methodology following Balduzzi et al. (2001) analyzing the impact of the announcements one at a time. The results, available upon request, agree with the reported time series results.

$S_{m,t+k}$ term accounts for the impact of the announcement surprises. The sum is over the $M = 10$ announcements listed in Table 1. The lagged surprise corresponding to $k = -1$ captures the impact that an announcement has in the five-minute interval after the announcement. The contemporaneous and lead surprises capture the preannouncement drift. We use the contemporaneous surprise and $K = 17$ leads of the surprise which together correspond to the $[t - 90min, t - 5sec]$ window. We use 90 minutes before the releases as the beginning of this window consistent with Figure 1 presented below, and as a robustness check we repeat the analysis with a shorter $[t - 60min, t - 5sec]$ window corresponding to $K = 11$ leads; the results, available upon request, agree with the $[t - 90min, t - 5sec]$ results.

To account for heteroskedasticity in the error term ϵ_t we estimate equation (2) by a two-step weighted least squares procedure. The first step estimates equation (2) with ordinary least squares (OLS). The second step derives an estimate of the time-varying volatility using the residuals, e_t, (estimates of ϵ_t) from this OLS regression and applies the weighted least squares estimation (Andersen et al., 2003). The weight w_t is an estimate of volatility calculated as an exponential moving average $w_t = \alpha w_{t-1} + (1 - \alpha)|e_t|$, where α is a smoothing parameter. Standardizing the residuals by w_t eliminates almost all heteroskedasticity and outperforms other methods such as regressing $|e_1|$ on seasonal hourly dummies.[13] The dependent and explanatory variables are then standardized by w_t, and the OLS regression is estimated with these standardized variables.

We use data from the beginning of the sample period (January 1, 2012) to June 30, 2017 to estimate this regression because the ONS changed the release policy on July 1, 2017. The Bank of England changed the mortgage approvals release policy on July 24, 2017 but there were no mortgage approval announcements from July 1, 2017 to July 23, 2017, so we use July 1, 2017 as the release policy change date to be consistent with the ONS release policy change date.

[13] We use $\alpha = 0.95$ and verify that the results are robust to other values such as 0.9. $w_1 = |e_1|$ in the first period. Since the estimator is volatile in the initial periods of our sample, we omit the first 50 observations which discards the morning of January 4, 2012.

As discussed in Section 2.1, most announcements provide information of only secondary importance and consequently have a negligible impact on the market and profit opportunities for traders (Gilbert et al., 2017). Because this might apply even to announcements with a high Bloomberg relevance score, we first check which of the ten announcements indeed impact the foreign exchange market. The statistical test of whether an announcement m moves the market is based on the sum of coefficients on the lagged, contemporaneous and lead surprises corresponding to the $[t - 90min, t + 5min]$ window, following Kurov et al. (2019) because previous papers such as Hu, Pan, and Wang (2017) have shown that announcements are almost instantaneously reflected in prices once released, so a five-minute postannouncement interval suffices to capture the announcement impact. An announcement is market-moving if the null hypothesis $H_0 : \sum_{k=-1}^{K} \gamma_{m,k} = 0$ is rejected in favor of the alternative hypothesis $H_1 : \sum_{k=-1}^{K} \gamma_{m,k} \neq 0$. Under standard assumptions, the resulting test statistic is asymptotically normal.

The second column of Table 2 presents results of this estimation. The reported values sum the coefficients corresponding to the $[t-90min, t+5min]$ window multiplied by one hundred, which allows interpreting the results as basis point changes. For example, a one-standard-deviation positive surprise in the retail sales announcement is associated with the foreign exchange futures price moving on average by approximately 22 basis points. The results show that there are six market-moving announcements. Four announcements are released by the ONS (retail sales, CPI, industrial production, and GDP) and two announcements are released by IHS Markit (PMI manufacturing and PMI construction).[14]

[14] Since this analysis uses data from from the beginning of our sample period (January 1, 2012) to the release policy change (June 30, 2017), as a robustness check we verify that the set of market-moving announcements is identical when the entire sample period (January 1, 2012 - December 31, 2019) is used. These results are available upon request.

3.2 Preannouncement Price Drift

Next, we ask whether the announcements impact the market before the release time. The statistical test of whether an announcement m has preannouncement price drift is based on the sum of coefficients on contemporaneous and lead surprises corresponding to the $[t - 90min, t - 5sec]$ window. Under the null hypothesis of no drift, $H_0 : \sum_{k=0}^{K} \gamma_{m,k} = 0$, and again under standard assumptions, the resulting test statistic follows the Student's t-distribution. The third column of Table 2 presents results of this estimation. The reported values sum the coefficients corresponding to the $[t-90min, t-5sec]$ window, again multiplied by one hundred. Three of the four market-moving announcements that utilized prereleases during the January 1, 2012 to June 30, 2017 period show drift coefficient sums significant at the 5% level indicating a preannouncement price drift in the correct direction. The three anoucements are retail sales, CPI, and industrial production. Before a one-standard-deviation positive surprise in the retail sales announcement, for example, the foreign exchange futures price increases on average by approximately 10 basis points. The magnitude of the preannouncement drift is substantial. For comparison, the standard deviation of 5-minute returns during our entire sample period (5:00 to 15:30 London Time from January 1, 2012 to December 31, 2019) is approximately 0.038% and the standard deviation of daily returns is about 0.55%. In contrast, the announcements released without prerelease (PMI manufacturing and PMI construction) do not impact the foreign exchange market before the release time at all.

To quantify the importance of the preannouncement price drift, we relate its magnitude to the total price impact of each announcement. In the fourth column we show the ratio of the coefficients from the third column to the corresponding coefficients in the second column. For the three announcements that exhibit preannouncement price drift, the ratios are positive and below 100 percent, which indicates that the prerelease signal is informative but noisy; it is either imperfect or absent in some releases. The ratio ranges from 30 percent in the industrial production announcement to 50 percent in the CPI announcement, giving a mean ratio of 42 percent. These preannouncement price moves amount to a substantial proportion of the total price move. Such large preannouncement drifts can originate under Bayesian learning from relatively little information before the release time (for example, Kurov et al. (2019)). Interestingly, the magnitude of these ratios is similar to the magnitude in U.S. data, which is on average approximately 40 percent (Kurov et al., 2019).

As Kurov et al. (2019) discuss, informed trading before the official announcement release time can be attributed to a variety of sources. Kurov et al. (2019) provide evidence in support of the information leakage hypothesis based on the differences in release policies across U.S. macroeconomic announcements: They find that announcements with less secure release policies are associated with a stronger preannouncement drift. In addition to information leakage, superior ability of individual traders to forecast based on public data might be another source of informed trading. For example, Kalamara, Turrell, Redl, Kapetanios, and Kapadia (2020) show that it is possible to extract economic signals from newspapers, which can improve forecasts of macroeconomic variables such as GDP, inflation, and unemployment, and Gu and Kurov (2018) provide evidence that superior processing of public information at least partially drives informed trading in natural gas futures before gas inventory announcements. Collection of proprietary data proxying the announcement data using technologies such as satellite imagery might be another source of informed trading as discussed in Section 1. We come back to this discussion of sources of informed trading in Section 3.4.

3.3 Cumulative Average Returns and Order Flow Imbalances

This section illustrates our findings graphically. We begin with the cumulative average return analysis in Figure 1. We focus on the four announcements that were subject to the release policy change and are market-moving per the second column of Table 2: CPI, GDP, industrial production, and retail sales. To construct the cumulative average return figures, we estimate a regression similar to that in equation (2) with two modifications. First, while equation (2) is estimated for the $[t-90min, t+5min]$ window, in the cumulative average return analysis we are interested in a longer postannouncement interval to graphically illustrate what happens after the announcements are released and examine whether any overshooting occurs. We therefore include five additional lags of the surprise variables that together correspond to the $[t + 5min, t + 30min]$ interval, so that we capture the $[t - 90min, t + 30min]$ window around the announcement. Second, because we are interested in price adjustment around an average announcement (rather than around a one-standard-deviation surprise), we use signs of the surprises instead of the surprises computed in equation (1). We therefore set S_{mt} equal to -1 (1) if the surprise for announcement m released at time t is negative (positive). Heteroskedasticity is modelled as described in Section 3.1. After estimating the gamma coefficients, we average them across the four announcements and cumulate them within the event window. The resulting estimates with associated confidence intervals describe price adjustment around the average market-moving announcement affected by the pre-release policy change. As explained in Section 2.1, the sample period contains instances when several macroeconomic announcements are released at the same time. Estimating cumulative average returns using this approach controls for the effects of such simultaneous announcements.

The top panel uses data from the beginning of the sample period (January 1, 2012) to the release policy change (June 30, 2017). There is some evidence of the price moving in the correct direction approximately 90 minutes before the release time, and this move becomes statistically significant approximately 30 minutes before the release time. This timing resembles the timing of the preannouncement price drift in the U.S. in Kurov et al. (2019), where the drift becomes statistically significant approximately 30 minutes before release time as well. The timing of the preannouncement drift appears to be similar on both sides of the Atlantic. We show in the Appendix that this timing is optimal for an informed trader.

Kurov et al. (2019) discuss possible rationales for this timing including traders finding relevant information only shortly before the release time, entering into trades close to the release time to minimize exposure to risks driven by unpredictable economic or geopolitical events, and attempting to strategically "hide" their trades by trading when liquidity is high and trades are likelier to go unnoticed (Admati & Pfleiderer, 1988; Kyle, 1985). Because we do not have limit order data to measure the bid-ask spread, we exploit the insight that the bid-ask spread has an inverse relation with trading volume (Wang & Yau, 2000). All three announcements that exhibit preannouncement price drift in Table 2 are released at 9:30 London Time. Figure 2 shows that there indeed is a substantial increase in trading volume before this time.

Evidence of informed trading exists not only in prices but also in order flow imbalances. This order flow analysis uses data at one-second intervals. Total trading volume in each one-second interval is classified as buyer- or seller-initiated depending on whether the last trade price in the interval is higher or lower than the last different price (Bernile et al., 2016). The signed trading volume is then aggregated in five-minute intervals. Figure 3 shows cumulative order imbalances for the same $[t - 90min, t + 30min]$ time window used in Figure 1. These cumulative order imbalances are estimated using the same regression model as the one used to estimate the cumulative average returns in Figure 1. In the top panel for the period before the release policy change, there is some evidence of the order flow imbalances beginning to build up approximately 90 minutes before the release time, and this increase becomes statistically significant approximately 30 minutes before the release time, which agrees with the cumulative average returns in Figure 1.

3.4 The Role of Release Policies

The preceding Sections 3.2 and 3.3 provided evidence that prior to the release policy change the foreign exchange rate begins drifting before the release time of three of the four market-moving announcements that utilized prereleases. We are interested in two questions: First, has the tightening of the release policy changed the preannouncement drift? Second, has the tightening of the release policy changed the postannouncement impact of an announcement?

If the tightening of the release policy reduces informed trading before the announcement, we expect the preannouncement drift to weaken and the postannouncement impact to become larger.

This estimation uses data from the release policy change (July 1, 2017) to the end of our sample period (December 31, 2019). Because this sample period is relatively short (30 observations for each announcement), instead of estimating coefficients for each announcement separately as in Table 2, we pool announcements together based on the results of estimating equation (2) reported in Table 2. We split announcements into three pools: market-moving announcements that were subject to the policy change (pool 1 consisting of CPI, GDP, industrial production, and retail sales), market-moving announcements that were not subject to the policy change (pool 2 consisting of PMI manufacturing and PMI construction), and announcements that do not move the foreign exchange market (pool 3 consisting of Halifax house price index, mortgage approvals, Nationwide house price index, and PPI).

The pooling is carried out using a weighted sum of the announcements with their equation (2) coefficients, $\hat{\gamma}_{m,k}$, as weights. Denote by $\mathcal{G}_p$ the set of indexes of announcements belonging to these three pools $p \in \{1,2,3\}$. We define the following pooled variables $\hat{X}_{p,t}^{(pre)} = \sum_{m \in \mathcal{G}_p} \left(\sum_{k=0}^{K} \hat{\gamma}_{m,k} S_{m,t+k} \right)$ and $\hat{X}_{p,t}^{(post)} = \sum_{m \in \mathcal{G}_p} \left(\hat{\gamma}_{m,-1} S_{m,t-1} \right)$ that measure the preannouncement and postannouncement impacts, respectively. For example, $\hat{X}_{1,t}^{(pre)}$ measures the preannouncement impact of the pool of market-moving announcements that were subject to the policy change. Similarly, $\hat{X}_{1,t}^{(post)}$ measures the postannouncement impact of this pool. We then estimate the following regression based on Swanson and Williams (2014):[15]

$$R_t = \theta_0 + \theta_1 R_{t-1} + \sum_{p=1}^{3} \delta_p^{(pre)} \hat{X}_{p,t}^{(pre)} + \sum_{p=1}^{3} \delta_p^{(post)} \hat{X}_{p,t}^{(post)} + \varepsilon_t. \tag{3}$$

[15]This regression imposes the following restriction: The coefficient estimates are fixed thoroughout the sample, but the preannouncement and postannouncement coefficients are scaled differently after the release policy change. This means that the relative values of the coefficients do not change before and after the release policy change but the scale does. We test this restriction in Section 4.4 as a robustness check.

Similarly to equation (2), we use the weighted least squares procedure to account for heteroskedasticity. Given that the pooled variables depend on the coefficients previously estimated in equation (2), $\hat{\gamma}_{m,k}$, we compute adjusted standard errors according to Murphy and Topel (1985).[16]

First, we analyze the preannouncement drift to find out whether the drift is still present in the sample period after the release policy change. We test the null hypothesis $H_0 : \delta_p^{(pre)} = 0$ against the alternative hypothesis $H_1 : \delta_p^{(pre)} \neq 0$. The top panel of Table 3 reports results of this estimation. Because this analysis is applicable only to market-moving announcements (i.e., Pools 1 and 2), we do not report results for non-market-moving announcements (Pool 3), although we do control for them when estimating equation (3).

The null hypothesis is not rejected at the 5% significance level for the market-moving announcements that were subject to the policy change (Pool 1). This indicates that these announcements no longer exhibit the strong preannouncement price drift that existed before the policy change. To quantify the size of this change, we conduct an additional test (not reported in Table 3) for the market-moving announcements that were subject to the release policy change (Pool 1). We test the null hypothesis $H_0 : \delta_p^{(pre)} = 1$ against the alternative hypothesis $H_1 : \delta_p^{(pre)} \neq 1$. This null hypothesis is rejected at the 5% significance level, indicating that the preannouncement price drift has changed after the release policy change. The estimate of $\delta_p^{(pre)} - 1$ is -0.944 and significant at the 1% level, which indicates that the preannouncement drift declined on average by about 94% after the release policy change.

These results are consistent with the cumulative average return and cumulative order imbalance figures in Section 3.3. In Figure 1, the cumulative average returns in the bottom panel show a noticeably lower preannouncement price drift after the release policy change in comparison to the top panel before the policy change. Similarly, the bottom panel of

[16] Assuming that the observations are independent across days, the coefficients $\delta_p^{(pre)}$ and $\delta_p^{(post)}$ are independent of the coefficients $\hat{\gamma}_{m,k}$ because they are estimated over non-overlapping samples. However, we do need to account for the added variability in the regressors when we use $\hat{\gamma}_{m,k}$ rather than the true coefficients. Moreover, given that the sample size prior to the policy change, n_0, and the sample size following the policy change, n_1, are different, we also need to pre-multiply the second term in equation (15) of Murphy and Topel (1985) by n_1/n_0 to derive correct standard errors.

Figure 3 for the period after the release policy change does not show statistically significant cumulative order imbalances before the release time, suggesting that the preannouncement informed trading that existed before the release policy change has dissipated.[17]

Second, we analyze the impact that the announcements have after the release time. If no information has entered the market before the release time, then more information becomes available at release time. Because our above analysis indicates that the preannouncement price drift has decreased following the release policy change, we expect the impact of the announcements in the postannouncement interval to increase. We test the null hypothesis $H_0 : \delta_p^{(post)} = 1$ against the alternative hypothesis $H_1 : \delta_p^{(post)} \neq 1$. The bottom panel of Table 3 shows that the null hypothesis is rejected for the market-moving announcements that were subject to the release policy change. This indicates that the postannouncement price impact has changed after the release policy change. Specifically, because $(\delta_p^{(post)} - 1)$ can be interpreted as a percentage change in the market reaction after the release policy change relative to the market reaction before the release policy change, the estimate of 0.317 corresponds to an increase in the market reaction at release time by 32%.

We also note that the second pool for market-moving announcements not subject to policy change shows a statistically significant decline in the price impact of the news. Although this pool contains only two announcements (PMI construction and PMI manufacturing), this finding provides additional evidence that the strengthening of the market reaction to the U.K. announcements in Pool 1 is related to the release policy change. This finding also agrees with the below Table 7 that includes U.S. announcements as a robustness check because the average market reaction to these U.S. macroeconomic announcements declined after July 1, 2017 as well.

Based on the cumulative average returns shown in Figure 1, the total price impact of news contained in an announcement appears to be largely unchanged. The price response

[17]The confidence intervals in the bottom panels of Figures 1 and Figure 3 are wider than the confidence intervals in the top panels because the number of observations in the sample period after the policy change is lower than in the sample period before the policy change.

at release time now includes also the price response that until the policy change occurred already before the official release time. These results are consistent with the "attenuation hypothesis" of Brennan, Huh, and Subrahmanyam (2018): informed trading before public announcements attenuates the market response to the announcement that occurs after the official release time.[18] Taken together, the findings in the top and bottom panels of Table 3 supported by Figures 1 and 3 indicate that the tightening of the release policy considerably changed the reaction of the foreign exchange futures market to the U.K. macroeconomic announcements.

While our analysis does not allow us to conclusively determine the source of the informed trading evidence in Table 2, the results in Table 3 are suggestive of the information leakage hypothesis because it is not clear why the drift and the market impact would change after the policy change if the source of informed trading were superior processing of public information or collection of proprietary data proxying the announcement data.

3.5 Speed of Adjustment

Section 3.4 suggests that after the elimination of prereleases, the news that used to diffuse into the market before the release time is now processed at the release time. Therefore, the post-announcement price adjustment might be slower. We use methodology similar to Hu et al. (2017) to analyze the speed of price adjustment to the U.K. macroeconomic announcements before and after the prerelease policy change. We estimate the following regression that uses second-by-second data:

$$\begin{aligned} R_{t+\underline{\tau}}^{t+\overline{\tau}} =& a_0 + a_1 1_{\{t\in\mathcal{T}\}} + a_2 1_{\{t\geq\bar{t}\}} + a_3 1_{\{t\in\mathcal{T}\}} 1_{\{t\geq\bar{t}\}} + b_0 \bar{S}_t \\ &+ b_1 \bar{S}_t 1_{\{t\in\mathcal{T}\}} + b_2 \bar{S}_t 1_{\{t\geq\bar{t}\}} + b_3 \bar{S}_t 1_{\{t\in\mathcal{T}\}} 1_{\{t\geq\bar{t}\}} + \varepsilon_t, \end{aligned} \tag{4}$$

where the times t are the times when there is a market-moving news announcement and $R_{t+\underline{\tau}}^{t+\overline{\tau}}$ is the return in the event window $[t+\underline{\tau}, t+\overline{\tau}]$. We estimate this regression separately

[18] Similarly, Michaelides, Milidonis, and Nishiotis (2019) show that trading on private information before changes in sovereign debt ratings weakens the reaction of currency markets to the rating changes.

for return in the first second after the announcement ($\underline{\tau} = 0$ and $\overline{\tau} = 1$ second) and for return in the four-second window that begins one second after the announcement and ends five seconds after the announcement ($\underline{\tau} = 1$ second and $\overline{\tau} = 5$ seconds) following Hu et al. (2017). The indicator $1_{\{.\}}$ is the indicator function: it is equal one if the argument is true and zero otherwise. The set $\mathcal{T}$ comprises the release times of announcements subject to prerelease policy change (i.e., those announcements in set $\mathcal{G}_1$ using the notation in Section 3.4). Here, the time $\bar{t}$ equals the policy change date, i.e., July 1, 2017.

The variable $\bar{S}_t$ is the aggregate standardised surprise constructed as follows: $\bar{S}_t = \sum_{m \in \mathcal{G}_1 \cap \mathcal{G}_2} S_{m,t}/n_t$ where $n_t = \max\left\{1, \sum_{m \in \mathcal{G}_1 \cap \mathcal{G}_2} |S_{m,t}| > 0\right\}$ so that the summation is over all market moving announcements, using the notation in Section 3.4. Here,n_t is the number of nonzero surprises at time t, with the minimum value set to one to avoid division by zero.

The regression in equation (4) estimates a difference-in-difference test. Market-moving announcements not subject to the policy change (PMI manufacturing and PMI construction) are used as the cross-sectional control group. The main interest is the significance of the estimated coefficient for b_3. A positive b_3 means that after the policy change, a positive surprise for the announcements affected by the policy change has a higher positive impact on price relative to the announcements in the control group. Hence, b_3 represents the change in the speed of adjustment after the policy change for market-moving announcements affected by the policy change. All regressors in the model other than $\bar{S}_t 1_{\{t \in \mathcal{T}\}} 1_{\{t > \bar{t}\}}$ are controls.

The estimation results are shown in Table 4. For the regression where the dependent variable is the return in the four-second window that begins one second after the announcement and ends five seconds after the announcement, the estimate of b_3 is about 3 basis points. This estimate is statistically significant at the 1% level. This shows that for the market-moving announcements affected by the prerelease policy change the price adjustment to the news after the policy change takes longer than it did when the announcements were prereleased to government officials. In other words, the elimination of the prereleases leads to a slower price discovery. Specifically, a significant part of the overall price adjustment to the announcement surprise that used to occur before the announcement now occurs in seconds $2-5$ after the release. This is a substantial lengthening of the market response given how fast markets typically respond to news: for example, Hu et al. (2017) show that when the University of Michigan Index of Consumer Sentiment was provided to fee-paying high-frequency traders two seconds before its broader release the market price fully incorporated information contained in the announcement within one second (in about 200 milliseconds).

4 Additional Analysis and Robustness Checks

We already discussed a robustness check with separating the GDP announcements into two groups (rather than combining them into one announcement) in Section 2.1. In Section 3, we discussed robustness checks with an event study methodology (rather than the time series methodology), a shorter $[t-60min, t-5sec]$ window (rather than the $[t-90min, t-5sec]$ window), a smoothing parameter $\alpha = 0.9$ (rather than $\alpha = 0.95$) in the two-step weighted least squares procedure, and a selection of market-moving announcements based on the entire sample period (rather than the period from the beginning of our sample period to the release policy change). This section presents additional robustness checks. Section 4.1 shows that our results are not driven by the Brexit referendum. Section 4.2 includes U.S. macroeconomic announcements in the analysis to show that the impact of the policy change on announcements that were subject to the change is specific to these announcements. Section 4.3 shows that our results hold for different post-policy sample periods. Section 4.4 tests parameter restrictions used in Section 3.4.

4.1 Brexit

Our analysis attributes the decrease in the preannouncement drift and the increase in the postannouncement price impact to the release policy change implemented by the ONS and the Bank of England in July of 2017. However, a possibility arises that the results are due to unrelated geopolitical events rather than the macroeconomic announcements policy change. The most consequential event in the U.K. during our sample period is the Brexit referendum that took place on June 23, 2016 where the majority voted to leave the European Union. Therefore, we conduct a robustness check to verify that our results are not driven by the Brexit referendum.

Recall that $\gamma_{m,k}$ in equation (2) stands for the coefficient corresponding to the kth surprise lead or lag of the mth announcement where the estimation of the coefficients uses data from the beginning of the sample period (January 1, 2012) to the release policy change (June 30, 2017). Define $\hat{b}$ as the vector consisting of the estimated $\gamma_{m,k}$ coefficients. Then, define $\hat{b}^{preAnn}$ as the vector of the same size as $\hat{b}$ with zeros everywhere except for the entries corresponding to the contemporaneous surprise and 17 leads of the surprise, which together correspond to the $[t - 90min, t - 5sec]$ window. Similarly, define $\hat{b}^{postAnn}$ as the vector of the same size as $\hat{b}$ with zeros everywhere except for the entries corresponding to the lag of the surprise, which corresponds to the $[t - 5sec, t + 5min]$ window. As in Section 3.4, we split the announcements into three pools: market-moving announcements that were subject to the policy change (pool 1), market-moving announcements that were not subject to the policy change (pool 2), and announcements that do not move the foreign exchange market (pool 3), and we use $p \in \{1, 2, 3\}$ to denote these three pools of announcements.

We then estimate the following equation using data from the beginning of the sample period to the release policy change:

$$R_t = \alpha_0 + \alpha_1 R_{t-1} + \sum_{p=1}^{3} \Big[\lambda_p^{preAnn} (S_t' \hat{b}_p^{preAnn}) + \lambda_p^{postAnn} (S_t' \hat{b}_p^{postAnn}) + \lambda_p^{preAnn,postBrexit} (1_{\{t \geq \bar{t}\}} S_t' \hat{b}_p^{preAnn}) + \lambda_p^{postAnn,postBrexit} (1_{\{t \geq \bar{t}\}} S_t' \hat{b}_p^{postAnn}) \Big] + \zeta_t, \quad (5)$$

where S_t' is the set of lead and lag surprises and $\bar{t}$ is the date of the Brexit referendum.

We test the null hypotheses that $\lambda_p^{preAnn,postBrexit} = 0$ and $\lambda_p^{postAnn,postBrexit} = 0$. These null hypotheses cannot be rejected at any conventional level of significance. These results (available upon request) show that the Brexit referendum had no significant effect on the preannouncement drift or the postannouncement price impact.

4.2 U.S. Macroeconomic Announcements

Our analysis attributes the decrease in the preannouncement drift and the increase in the postannouncement price impact to the release policy change, and Section 4.1 shows that the results are not driven by Brexit. A possibility arises, however, that our results are driven by other factors that affected the market reaction to not only the U.K. announcements but also announcements from other countries. Since we are analyzing the British Pound to the U.S. dollar exchange rate futures, in this section we repeat the analysis of Section 3 while including U.S. announcements. Following the data procedure described in Section 2, we include announcements with Bloomberg relevance score greater than or equal to 75. Table 5 lists these 29 announcements.

We begin by estimating equation (2) where $M = 39$ because there are ten U.K. and 29 U.S. announcements. Table 6 presents the results that are analogous to those in Table 2. The second column shows the impact during the $[t - 90min, t + 5min]$ window. The results for the U.K. announcements agree with Table 2: there are six market-moving announcements and their magnitude and statistical significance are essentially unchanged. In addition, there are eleven U.S. market-moving announcements.

The third column shows the impact during the $[t - 90min, t - 5sec]$ window. The results for the U.K. announcements again agree with Table 2: there are three U.K. announcements that exhibit preanouncement drift. In contrast, there are no U.S. announcements showing evidence of preannouncement drift at a 5% significance level.

We then estimate equation (3) with four pools: (1) U.K. market-moving announcements subject to the policy change, (2) U.K. market-moving announcements not subject to the policy change, (3) U.S. market-moving announcements, and (4) U.K. and U.S. non-market-moving announcements. Table 7 shows results analogous to results in Table 3. As in Table 3, we do not report results for the non-market-moving announcements (Pool 4) because this analysis is applicable only to market-moving announcements, although we do control for the non-market-moving announcements when estimating equation (3).

The results for the U.K. market-moving announcements that were subject to the policy change (Pool 1) confirm the results in Table 3: the price drift became significantly weaker and the average market reaction at release time increased for announcements that were subject to the policy change. The estimates in Table 7 also show that the average market reaction to the U.S. macroeconomic announcements (Pool 3) declined on average by approximately 60% after July 1, 2017. This finding agrees with Section 3.4 where the U.K. market-moving announcements that were not subject to the policy change (Pool 2) also experienced a decline in the market reaction. These estimates therefore provide additional evidence that the strengthening of the market reaction to the U.K. announcements in Pool 1 is related to the release policy change.

4.3 Post-Policy Sample Periods

The analysis of the effect of release policies in Section 3.4 uses data from the release policy change (July 1, 2017) to the end of our sample period (December 31, 2019). In this section, we verify that our results are not qualitatively affected by the choice of the end of the sample period.

We repeat the analysis of Section 3.4 with three other sample period end dates: June 30, 2018, December 31, 2018 and June 30, 2019. Table 8 reports these results. The results are qualitatively similar in all three sample periods and similar to the the results in Table 3 that uses the sample period through December 31, 2019: the preannouncement price drift is no longer present, and the postannouncement price impact has increased.

4.4 Test of Parameter Restrictions

The estimation in Section 3.4 relies on parameters estimated in Section 3.2 that uses data from the beginning of the sample period (January 1, 2012) to the release policy change (June 30, 2017). As explained in Section 3.4, this imposes the restriction that the coefficient estimates do not differ before and after the policy change and only their scale changes. In this section, we test this restriction.

We estimate the following model:

$$\begin{aligned} R_t = 1_{\{t<\bar{t}\}} \left(\beta_0^{(1)} + \beta_1^{(1)} R_{t-1}\right) + 1_{\{t\geq\bar{t}\}} \left(\beta_0^{(2)} + \beta_1^{(2)} R_{t-1}\right) \\ + \sum_{m=1}^{M} \sum_{k=-1}^{K} \left(\gamma_{m,k}^{(1)} 1_{\{t<\bar{t}\}} + \gamma_{m,k}^{(2)} 1_{\{t\geq\bar{t}\}}\right) S_{m,t+k} + \epsilon_t, \quad (6) \end{aligned}$$

where time $\bar{t}$ equals the policy change date, i.e., July 1, 2017, and therefore the superscripts (1) and (2) stand for the periods before and after the policy change, respectively. In contrast to equation (2) that uses data from the beginning of the sample period to the release policy change, equation (6) is estimated using data for the whole sample period and allows for the possibility of a structural change after the policy change. As in equation (2), the coefficients $\gamma_{m,k}^{(1)}$ and $\gamma_{m,k}^{(2)}$ in equation (6) are not restricted. Therefore, we refer to this model as the unrestricted model and denote the residual sum of the squares from this unrestricted model by RSS^U.

We then estimate a restricted model. We begin by estimating equation (2) for the entire sample period and use a hat to denote the estimated coefficients. We follow by estimating

$$\begin{aligned} R_t = 1_{\{t<\bar{t}\}} \left(\theta_0^{(1)} + \theta_1^{(1)} R_{t-1}\right) + 1_{\{t\geq\bar{t}\}} \left(\theta_0^{(2)} + \theta_1^{(2)} R_{t-1}\right) \\ + \sum_{p=1}^{3} 1_{\{t<\bar{t}\}} \left[\delta_p^{(1)(pre)} \hat{X}_{p,t}^{(pre)} + \delta_p^{(1)(post)} \hat{X}_{p,t}^{(post)}\right] \\ + \sum_{p=1}^{3} 1_{\{t>\bar{t}\}} \left[\delta_p^{(2)(pre)} \hat{X}_{p,t}^{(pre)} + \delta_p^{(2)(post)} \hat{X}_{p,t}^{(post)}\right] + \varepsilon_t, \quad (7) \end{aligned}$$

where time $\bar{t}$ again equals the policy change date, i.e., July 1, 2017, and the superscripts (1) and (2) stand for the periods before and after the policy change, respectively. In contrast to equation (3) in Section 3.4 that uses data from the release policy change to the end of the sample period, equation (7) is estimated using data for the whole sample period and allows for the possibility of a structural change after the discontinuation of the prerelease because the coefficients $\delta_p^{(pre)}$ and $\delta_p^{(post)}$ (as well as other coefficients) are allowed to vary before and after the release policy change. The coefficients in equation (7) are restricted in the sense that we only allow a change in the magnitude of the sum of the coefficients but not in the relative weights $\gamma_{m,k}$. Therefore, we refer to this model as the restricted model and denote the residual sum of the squares from this restricted model by RSS^R. We compute a monotonic transformation of the likelihood ratio test and compute its critical values by the bootstrap. We have two reasons for using the bootstrap. First, we have a large number of nuisance parameters and despite the fact that the sample size is large, this can lead to poor asymptotic approximations. The bootstrap allows us to account for this. Second, the restrictions are nonlinear, which makes asymptotic arguments based on the likelihood ratio statistic more difficult to derive.

The results (available upon request) show that we cannot reject the null hypothesis of the restricted model being correct at any conventional level of significance. This means that the parameter restrictions used in Section 3.4 do not drive our results.

5 Conclusion

In 2017 the release procedures of several U.K. macroeconomic announcements were considerably tightened. Prior to 2017 important macroeconomic announcements were distributed to many government officials 24 hours before their release to the public. This practice led to concerns that the information provided by early access might leak, giving some traders an unfair advantage. The ONS and the Bank of England consequently ended such prereleases in

July 2017. We examine the price adjustment in the foreign exchange futures market around the release time of ten U.K. macroeconomic announcements before and after this prerelease policy change.

Four of the announcements subject to the change in the release policy significantly impact the foreign exchange market, and three of them (CPI, industrial production, and retail sales) display a significant price drift in the "correct" direction about 30 minutes before the official announcement release until July 2017. The preannouncement drift accounts on average for about 40 percent of the total price adjustment to these three announcements. These results are consistent with information in these announcements being known to some traders in advance.

After the tightening of release policies, in particular the elimination of prereleases, the market reaction changes in three ways: the price drift becomes significantly weaker, the average market reaction at release time increases, and the market takes longer to absorb the announcement information. These three changes indicate that the news that used to diffuse into the market before release time is now processed at release time. The larger and slower response at release time reflects the larger surprise at release time and might also indicate that the announcements have become more valuable for ordinary traders, i.e., traders without private information. Aware of the existence of private information in the market, these traders might have previously been hesitant to respond to a news release. This can occur because informed traders benefit from their private information also at the moment when the announcement is officially released. Only they know the extent to which the news has been already reflected by the preannouncement price (Brunnermeier, 2005) and trade accordingly. The creation of comparable information sets across market participants might give ordinary traders the confidence to trade more actively in response to macroeconomic news right at its release.

Appendix: Optimal Trading Time

Figures 1 and 3 showed that there is some evidence of the foreign exchange futures price moving in the correct direction approximately 90 minutes before the announcement release time, and this move becomes statistically significant approximately 30 minutes before the release time. This appendix illustrates that this timing is optimal for an informed trader.

Suppose that for a given security the expected value of trading the macroeconomic news announcement is μ. Denote by σ the volatility of the security and let $t \geq 0$ be the difference between the time of trading and the time when the announcement was released. Consider a market where only one agent is informed. We proceed in two steps. First, we show that it is suboptimal to trade too early. Second, we show that it is suboptimal to trade too close to the announcement release time.

To show that it is suboptimal to trade too early, we consider the case of infinite (relatively large) liquidity. Consider an agent that maximizes a mean variance utility function with risk aversion parameter ρ. The utility is then given by $\mu - \frac{1}{2}\rho\sigma^2 t$. It can be seen from this expression that the agent will choose $t \to 0$. This is because an informed trader will trade as close as possible to the announcement release time to minimize risk since trading early involves a higher risk as the market can move in the opposite direction due to other, unpredictable economic or geopolitical events. This is captured by ρ. The volatility provides a bound for trading too early.

Next, we show that it is suboptimal to trade too close to the announcement release time. We lift the assumption of infinite liquidity and allow for market impact. The trading involves cost because the trading reveals information by making an impact on the market.

We model this cost, $\kappa(t)$, as $\kappa(t) = \mu e^{-\alpha t}$, where $\alpha > 0$ is a parameter that is inversely proportional to the market impact. The earlier trading occurs, the larger the time t and hence the lower the impact of trading. In contrast, if trading occurs at the announcement release time (as $t \to 0$), we obtain $\kappa(t) = \mu$, which means that there is no benefit in holding the information when the announcement is released. The agent therefore maximizes a utility function adjusted for the market impact: $\mu - \frac{1}{2}\rho\sigma^2 t - \kappa(t) = \mu(1 - e^{-\alpha t}) - \frac{1}{2}\rho\sigma^2 t$. The first-order conditions show that this utility is maximized at $t^* = 24 * 60\frac{1}{\alpha}\ln[\frac{2\alpha\mu}{\rho\sigma^2}]$ minutes. The impact function thus provides a bound for trading too close to the announcement.

We assume the risk aversion parameter $\rho = 2$ following previous literature such as Ang (2014). We match the CAR in Figure 1, so that $\mu = 0.16$ (corresponding to a reward of 16 basis points) and $\sigma = 0.55$ (corresponding to the standard deviation of daily returns of approximately 0.55% noted in Section 3.2). We measure time t in days. To find α, we fit the preannouncement price drift in Figure 1 to $\kappa(t)$ using the nonlinear least square regression. The dependent variable is $E[P(0-) - P(-90)]$ where $P(0)$ is the prevailing price just before the announcement release, $P(90)$ is the price ninety minutes before the announcement release, and E is the expectation. Using these values, we obtain $t^* = 24 * 60 * \ln((123 * 0.16)/(0.55^2))/123 = 49$ minutes. A value of $\alpha = 100$ results in $t^* = 57$ minutes. Hence, it is optimal to trade approximately one hour before the announcement.

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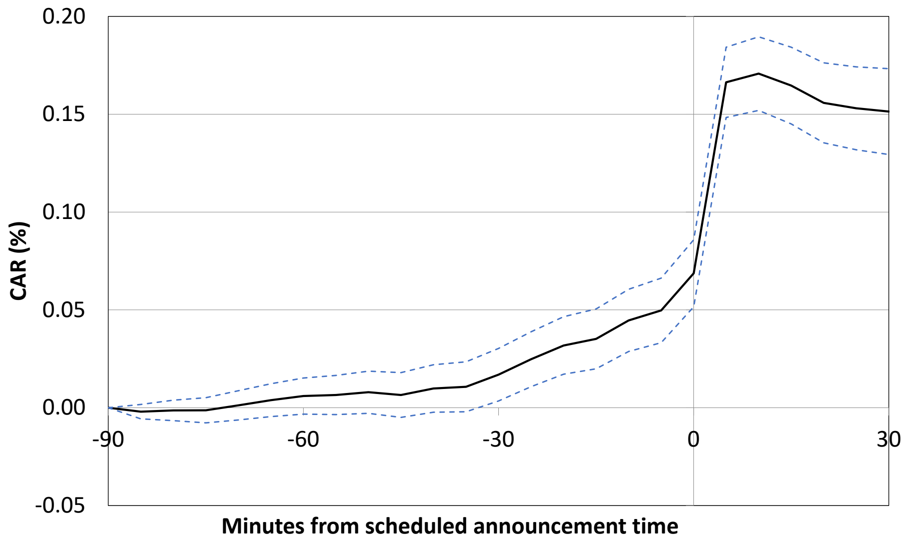
Figure 1: Cumulative Average Returns

Figure 1 shows cumulative average returns (CARs) in the British pound to U.S. dollar exchange rate (GBP/USD) futures market around the four announcements that were subject to the release policy change and are market-moving per the second column of Table 2: Consumer Price Index, gross domestic product, industrial production, and retail sales. We estimate a regression similar to that in equation (2) with two modifications. First, we include five additional lags of the surprise variables. Second, instead of the announcement surprises we use signs of the surprises. We average the estimated gamma coefficients across the four announcements mentioned above and cumulate them within the $[t - 90min, t + 30min]$ window. The top panel uses data from the beginning of the sample period (January 1, 2012) to the release policy change (June 30, 2017). The bottom panel uses data from the release policy change (July 1, 2017) to the the end of our sample period (December 31, 2019). The solid line shows the estimated CAR. Dashed lines indicate two-standard-error bands.

Before release policy change



After release policy change

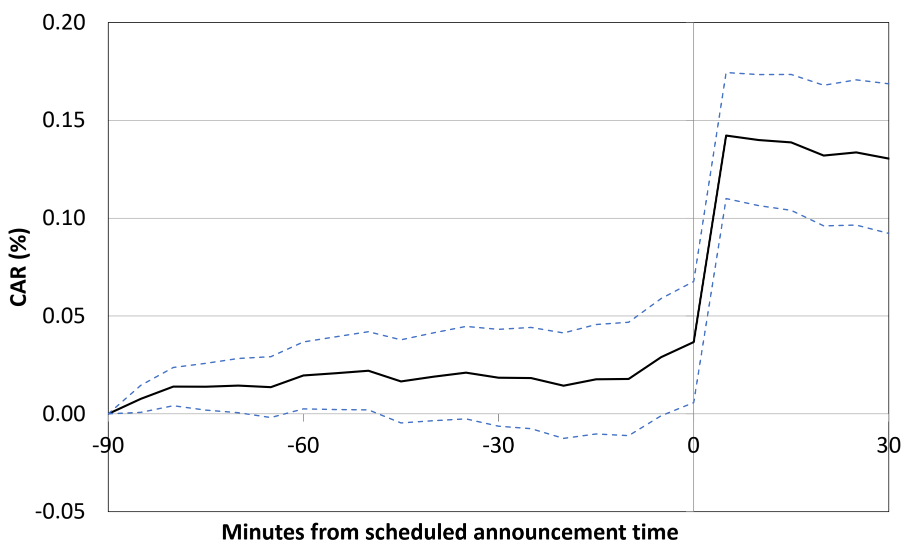


Figure 2: Trading Volume

In Figure 2, the sample period is from January 1, 2012 to December 31, 2019. The figure shows the average trading volume in the GBP/USD futures market measured as the number of contracts per minute. The time is stated in London Time.

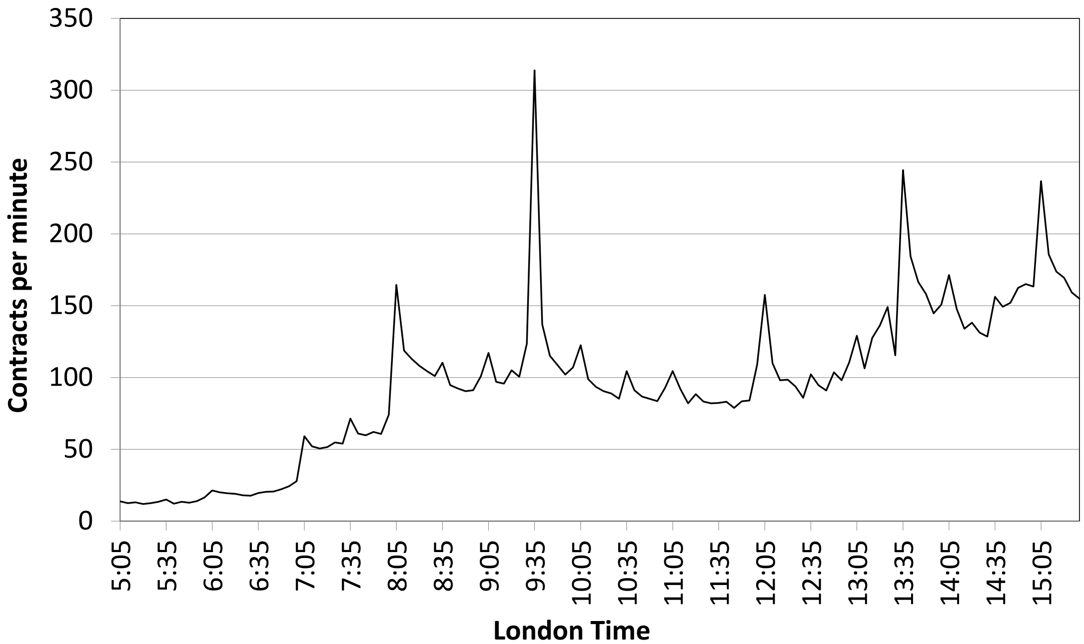
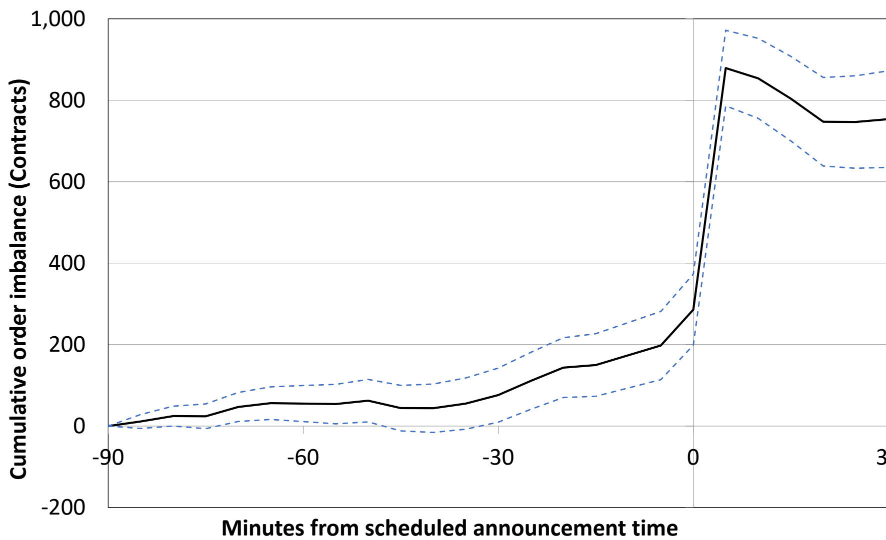


Figure 3: Cumulative Order Imbalances

Figure 3 shows cumulative order imbalances (COIs) in the the British pound to U.S. dollar exchange rate (GBP/USD) futures market around the four announcements that were subject to the release policy change and are market-moving per the second column of Table 2: Consumer Price Index, gross domestic product, industrial production, and retail sales. We estimate a regression similar to that in equation (2) with three modifications. First, we use the order imbalance as the dependent variable. Second, we include five additional lags of the surprise variables. Third, instead of the announcement surprises we use signs of the surprises. We average the estimated gamma coefficients across the four announcements mentioned above and cumulate them within the $[t-90min, t+30min]$ window. The top panel uses data from the beginning of the sample period (January 1, 2012) to the release policy change (June 30, 2017). The bottom panel uses data from the release policy change (July 1, 2017) to the the end of our sample period (December 31, 2019). The solid line shows the mean COI. Dashed lines indicate two-standard-error bands.

Before release policy change



After release policy change

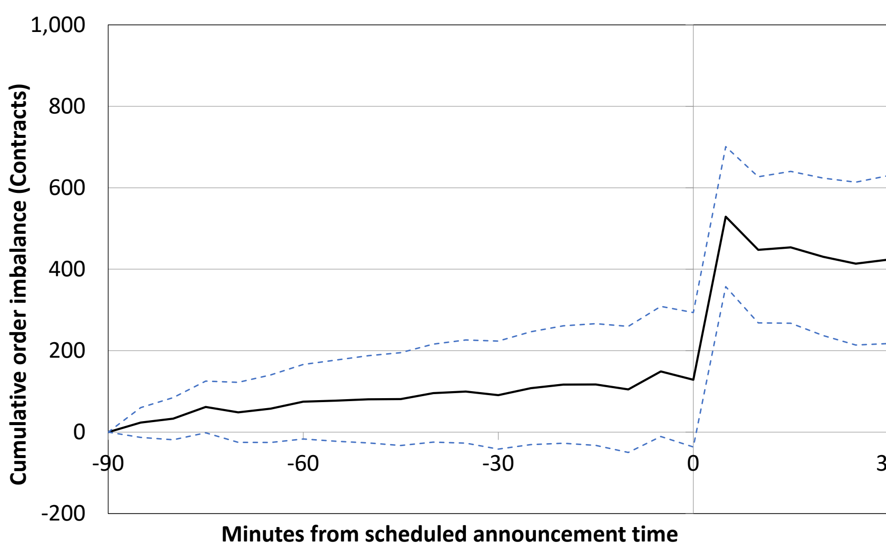


Table 1: Macroeconomic Announcements

Announcement	N_m	**Source**	**Unit**	**Time**	**Bloomberg Score**
GDP QoQ	90	ONS	%	9:30	98
CPI MoM	95	ONS	%	9:30	94
Industrial production MoM	93	ONS	%	9:30	92
PMI manufacturing	94	IHS	Index	9:30[a]	90
Nationwide house price index MoM	95	NBS	%	7:00	89
PPI output MoM	95	ONS	%	9:30	85
Mortgage approvals	94	BoE	No. of approvals	9:30	84
Retail sales incl. auto/fuel MoM	94	ONS	%	9:30	80
Halifax house price index MoM	92	IHS	%	8:30[b]	77
PMI construction	93	IHS	Index	9:30	75

Table 1 lists the U.K. macroeconomic announcements used in our analysis. N_m shows the number of releases for each announcement. BoE, IHS, NBS, and ONS stand for the Bank of England, IHS Markit, Nationwide Building Society, and the Office for National Statistics, respectively. The Unit column lists the units in which the data is shown in the Bloomberg database. The release time is stated in London Time. The sample period is from January 1, 2012 to December 31, 2019.

[a] The PMI manufacturing was released at 9:28 London Time before April 1, 2014 to a selected group of traders.

[b] The Halifax house price index was released at 8:00 London Time before January 8, 2016.

Table 2: Preannouncement Price Drift before Release Policy Change

Announcement	γ_m $[t-90min, t+5min]$	γ_m $[t-90min, t-5sec]$	**Ratio**	**Policy Change**
Retail sales incl. auto/fuel MoM	22.36 (1.41)***	10.38 (1.34)***	46%	Y
CPI MoM	14.58 (1.29)***	7.23 (1.23)***	50%	Y
Industrial production MoM	11.47 (1.36)***	3.48 (1.30)***	30%	Y
PMI manufacturing	11.07 (1.32)***	0.54 (1.26)	n/a	N
GDP QoQ	6.18 (1.37)***	-1.49 (1.31)	n/a	Y
PMI construction	4.00 (1.50)***	-2.15 (1.45)	n/a	N
Mortgage approvals	1.27 (1.19)	0.19 (1.19)	n/a	Y
PPI output MoM	-0.50 (1.25)	-0.39 (1.18)	n/a	Y
Halifax house price index MoM	0.41 (1.24)	-0.28 (1.22)	n/a	N
Nationwide house price index MoM	0.03 (1.00)	-0.85 (1.02)	n/a	N

Table 2 uses data only from the beginning of our sample period (January 1, 2012) to the release policy change (June 30, 2017). The second and third columns estimate equation (2) with the weighted least squares procedure. The reported results, γ_m, are sums of coefficients corresponding to the $[t-90min, t+5min]$ and $[t-90min, t-5sec]$ windows, respectively. Standard errors are in parentheses. *, **, and *** show statistical significance at 10%, 5%, and 1% levels, respectively. The fourth column computes the ratio of the coefficients in the second and third columns; "n/a" stands for "not applicable" indicating that the ratio is not computed because the announcement does not exhibit a preannouncement price drift in the third column. The fifth column indicates whether the release policy of the announcement has changed during the sample period.

Table 3: Preannouncement Price Drift and Postannouncement Price Impact after Release Policy Change

Announcement Pool	$\boldsymbol{\delta_p^{(pre)}}$
(a) $\boldsymbol{H_0 : \delta_p^{(pre)} = 0}$, i.e., Is the preannouncement price drift present?	
Market-moving subject to policy change (Pool 1)	0.057 (0.133)
Market-moving not subject to policy change (Pool 2)	0.077 (0.189)
Announcement Pool	$\boldsymbol{\delta_p^{(post)} - 1}$
(b) $\boldsymbol{H_0 : \delta_p^{(post)} = 1}$, i.e., Has the postannouncement price impact changed?	
Market-moving subject to policy change (Pool 1)	0.317 (0.051)***
Market-moving not subject to policy change (Pool 2)	-0.474 (0.056)***

Table 3 uses data from the release policy change (July 1, 2017) to the end of our sample period (December 31, 2019). The table reports results of estimating equation (3) with the weighted least squares procedure. Panel (a) tests the null hypothesis $H_0 : \delta_p^{(pre)} = 0$ against the alternative hypothesis $H_1 : \delta_p^{(pre)} \neq 0$. Panel (b) tests the null hypothesis $H_0 : \delta_p^{(post)} = 1$ against the alternative hypothesis $H_1 : \delta_p^{(post)} \neq 1$. Standard errors are in parentheses. *, **, and *** show statistical significance at 10%, 5%, and 1% levels, respectively.

Table 4: Speed of Adjustment Regression Results

	$[t, t + 1sec]$	$[t + 1sec, t + 5sec]$
a_0	-0.03 (0.51)	-0.13 (0.46)
a_1	-0.46 (0.62)	0.09 (0.56)
a_2	0.06 (0.89)	0.74 (0.81)
a_3	1.21 (1.11)	-1.31 (1.00)
b_0	5.12 (0.50)***	0.07 (0.45)
b_1	1.33 (0.59)**	0.91 (0.54)
b_2	-0.81 (0.94)	-0.29 (0.85)
b_3 (Diff-in-diff)	0.44 (1.21)	2.97 (1.10)***
R^2	0.53	0.08

Table 4 shows OLS coefficient estimates for equation (4). The reported results are for the $[t, t + 1sec]$ and $[t + 1sec, t + 5sec]$ windows in the first and second columns, respectively, where t is the time of the market-moving announcement. The regression uses data for our full sample period (January 2012 through December 2019). Standard errors are in parentheses. *, **, and *** show statistical significance at 10%, 5%, and 1% levels, respectively.

Table 5: U.S. Macroeconomic Announcements

Announcement	N_m	**Source**	**Unit**	**Time**	**Bloomberg Score**
Non-farm employment	93	BLS	Number of jobs	8:30	99
Initial jobless claims	407	ETA	Number of claims	8:30	98
GDP	93	BEA	%	8:30	98
Consumer price index	95	BLS	%	8:30	96
ISM Manufacturing index	93	ISM	Index	10:00	95
UM Consumer sentiment (final)	93	TR/UM	Index	9:55	94
UM Consumer sentiment (prel.)	94	TR/UM	Index	9:55	94
CB Consumer confidence	94	CB	Index	10:00	94
Durable goods orders	96	BC	%	8:30	93
Advance retail sales	93	BC	%	8:30	92
Industrial production	93	FRB	%	9:15	92
New home sales	94	BC	Number of homes	10:00	91
Housing starts	93	BC	Number of homes	8:30	90
Unemployment rate	93	BLS	%	8:30	89
Existing home sales	96	NAR	Number of homes	10:00	88
ADP employment	92	ADP	Number of jobs	8:15	87
Factory orders	91	BC	%	10:00	86
Personal consumption	92	BEA	%	8:30	85
Personal income	93	BEA	%	8:30	85
Trade balance	94	BEA	USD	8:30	84
Index of leading indicators	94	CB	%	10:00	83
Empire State manufacturing	94	FRBNY	Index	8:30	83
Wholesale inventories	93	BC	%	10:00	81
Construction spending	92	BC	%	10:00	80
Philadelphia Fed Business Outlook	94	FRBP	Index	10:00	80
ISM Non-manufacturing index	92	ISM	Index	10:00	79
Import price index	94	BLS	%	8:30	78
GDP price index	93	BEA	%	8:30	77
Pending home sales	93	NAR	%	10:00	77

Table 5 lists the U.S. macroeconomic announcements used in our analysis. N_m shows the number of releases for each announcement. ADP, BC, BLS, CB, ETA, FRB, FRBNY, FRBP, ISM, NAR, and TR/UM stand for Automatic Data Processing, Bureau of the Census, Bureau of Labor Statistics, Conference Board, Employment and Training Administration, Federal Reserve Board, Federal Reserve Bank of New York, Federal Reserve Bank of Philadelphia, Institute of Supply Management, National Association of Realtors, and Thomson Reuters/University of Michigan respectively. The Unit column lists the units in which the data is shown in the Bloomberg database. The release time is stated in Eastern Time. The sample period is from January 1, 2012 to December 31, 2019.

Table 6: Preannouncement Price Drift before Release Policy Change: With U.S. Macroeconomic Announcements

Announcement	γ_m $[t-90min, t+5min]$	γ_m $[t-90min, t-5sec]$	Ratio	Policy Change
U.K. Retail sales incl. auto/fuel MoM	22.37 (1.41)***	10.39 (1.34)***	46%	Y
U.K. CPI MoM	14.58 (1.28)***	7.23 (1.23)***	50%	Y
U.K. Industrial production MoM	11.45 (1.36)***	3.45 (1.30)***	30%	Y
U.K. PMI manufacturing	11.08 (1.31)***	0.54 (1.26)	n/a	N
U.K. GDP QoQ	6.18 (1.36)***	-1.49 (1.30)	n/a	Y
U.K. PMI construction	4.06 (1.49)***	-2.08 (1.44)	n/a	N
U.K. Mortgage approvals	1.26 (1.18)	0.17 (1.14)	n/a	Y
U.K. PPI output MoM	-0.52 (1.25)	-0.41 (1.20)	n/a	Y
U.K. Halifax house price index MoM	0.40 (1.24)	-0.29 (1.20)	n/a	N
U.K. Nationwide house price index MoM	0.06 (1.04)	-0.82 (1.02)	n/a	N
U.S. Non-farm employment	19.01 (1.56)***	0.42 (1.48)	n/a	N
U.S. Advance retail sales	8.93 (1.83)***	-0.77 (1.77)	n/a	N
U.S. ISM Non-manufacturing index	7.01 (1.91)***	2.76 (1.85)	n/a	N
U.S. GDP	6.70 (1.25)***	2.24 (1.21)*	n/a	N
U.S. ISM Manufacturing index	6.06 (1.62)***	1.92 (1.56)	n/a	N
U.S. Consumer price index	4.10 (1.54)***	0.96 (1.49)	n/a	N
U.S. ADP employment	4.04 (1.69)**	-0.05 (1.63)	n/a	N
U.S. Industrial production	3.82 (1.57)**	2.75 (1.52)*	n/a	N
U.S. Unemployment rate	3.78 (1.24)***	0.33 (1.19)	n/a	N
U.S. Trade balance	3.73 (1.45)**	2.22 (1.40)	n/a	N
U.S. Initial jobless claims	2.00 (0.71)***	0.82 (0.69)	n/a	N
U.S. Housing starts	2.78 (1.54)*	-0.01 (1.49)	n/a	N
U.S. UM Consumer sentiment (prel.)	3.06 (1.74)*	1.77 (1.69)	n/a	N
U.S. Factory orders	2.85 (1.84)	1.84 (1.79)	n/a	N
U.S. Personal income	-2.25 (1.55)	-2.50 (1.50)*	n/a	N
U.S. Pending home sales	2.15 (1.48)	1.70 (1.44)	n/a	N
U.S. Durable goods orders	1.37 (1.06)	0.46 (1.00)	n/a	N
U.S. Wholesale inventories	1.81 (1.55)	1.48 (1.51)	n/a	N
U.S. GDP price index	-1.72 (1.48)	-0.90 (1.44)	n/a	N
U.S. Personal consumption	1.41 (1.30)	0.15 (1.26)	n/a	N
U.S. Empire State manufacturing	1.38 (1.37)	-0.74 (1.33)	n/a	N
U.S. CB Consumer confidence	1.46 (1.66)	-0.40 (1.61)	n/a	N
U.S. Philadelphia Fed Business Outlook	-1.17 (1.77)	-3.28 (1.72)*	n/a	N
U.S. UM Consumer sentiment (final)	0.82 (1.55)	0.59 (1.50)	n/a	N
U.S. New home sales	0.67 (1.66)	-1.26 (1.61)	n/a	N
U.S. Index of leading indicators	0.66 (1.71)	0.23 (1.66)	n/a	N
U.S. Existing home sales	-0.57 (1.61)	-0.75 (1.56)	n/a	N
U.S. Construction spending	0.07 (1.50)	-0.89 (1.46)	n/a	N
U.S. Import price index	-0.03 (1.52)	0.91 (1.48)	n/a	N

Table 6 uses data only from the beginning of our sample period (January 1, 2012) to the release policy change (June 30, 2017). The second and third columns estimate equation (2) with the weighted least squares procedure. The reported results, γ_m, are sums of coefficients corresponding to the $[t-90min, t+5min]$ and $[t-90min, t-5sec]$ windows, respectively. Standard errors are in parentheses. *, **, and *** show statistical significance at 10%, 5%, and 1% levels, respectively. The fourth column computes the ratio of the coefficients in the second and third columns; "n/a" stands for "not applicable" indicating that the ratio is not computed because the announcement does not exhibit a preannouncement price drift at a 5% significance level in the third column. The fifth column indicates whether the release policy of the announcement has changed during the sample period.

Table 7: Preannouncement Price Drift and Postannouncement Price Impact after Release Policy Change: With U.S. Announcements

Announcement Pool	$\boldsymbol{\delta_p^{(pre)}}$
(a) $\boldsymbol{H_0 : \delta_p^{(pre)} = 0}$, i.e., Is the preannouncement price drift present?	
U.K. Market-moving subject to policy change (Pool 1)	0.054 (0.133)
U.K. Market-moving not subject to policy change (Pool 2)	0.077 (0.190)
U.S. Market-moving (Pool 3)	-0.081 (0.112)
Announcement Pool	$\boldsymbol{\delta_p^{(post)} - 1}$
(b) $\boldsymbol{H_0 : \delta_p^{(post)} = 1}$, i.e., Has the postannouncement price impact changed?	
U.K. Market-moving subject to policy change (Pool 1)	0.315 (0.051)***
U.K. Market-moving not subject to policy change (Pool 2)	-0.478 (0.056)***
U.S. Market-moving (Pool 3)	-0.605 (0.025)***

Table 7 uses data from the release policy change (July 1, 2017) to the end of our sample period (December 31, 2019). The table reports results of estimating equation (3) with the weighted least squares procedure. Panel (a) tests the null hypothesis $H_0 : \delta_p^{(pre)} = 0$ against the alternative hypothesis $H_1 : \delta_p^{(pre)} \neq 0$. Panel (b) tests the null hypothesis $H_0 : \delta_p^{(post)} = 1$ against the alternative hypothesis $H_1 : \delta_p^{(post)} \neq 1$. Standard errors are in parentheses. *, **, and *** show statistical significance at 10%, 5%, and 1% levels, respectively.

Table 8: Different Post-Policy Sample Periods

Announcement Pool	07/01/17 - 06/30/18	07/01/17 - 12/31/18	07/01/17 - 06/30/19
		$\boldsymbol{\delta_p^{(pre)}}$	
(a) $\boldsymbol{H_0 : \delta_p^{(pre)} = 0}$, i.e., Is the preannouncement price drift present?			
Market-moving subject to policy change (Pool 1)	0.219	0.259	0.047
	(0.230)	(0.183)	(0.144)
Market-moving not subject to policy change (Pool 2)	-0.019	0.161	0.209
	(0.315)	(0.271)	(0.236)
		$\boldsymbol{\delta_p^{(post)} - 1}$	
(b) $\boldsymbol{H_0 : \delta_p^{(post)} = 1}$, i.e., Has the postannouncement price impact changed?			
Market-moving subject to policy change (Pool 1)	1.544***	1.147***	0.459***
	(0.086)	(0.071)	(0.057)
Market-moving not subject to policy change (Pool 2)	-0.266***	-0.378***	-0.487***
	(0.099)	(0.085)	(0.065)

Table 8 uses data from the release policy change (July 1, 2017) to June 30, 2018, December 31, 2018, and June 30, 2019 in the second, third, and fourth columns, respectively. The table reports results of estimating equation (3) with the weighted least squares procedure. Panel (a) tests the null hypothesis $H_0 : \delta_p^{(pre)} = 0$ against the alternative hypothesis $H_1 : \delta_p^{(pre)} \neq 0$. Panel (b) tests the null hypothesis $H_0 : \delta_p^{(post)} = 1$ against the alternative hypothesis $H_1 : \delta_p^{(post)} \neq 1$. Standard errors are in parentheses. *, **, and *** show statistical significance at 10%, 5%, and 1% levels, respectively.