Drift Begone! Release Policies and Preannouncement Informed Trading*

Alexander Kurov† Alessio Sancetta‡ Marketa Halova Wolfe§

This Draft: December 12, 2019

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. Analogously, the market reaction to the announcements at the official release time has become stronger.

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

*We thank Jonathan Athow, Aaron Henrichsen, Sahn-Wook Huh, participants in the 2019 INFINITI Conference on International Finance and the 2019 Southern Finance Association Conference, and seminar participants at the University of Essex, Swansea University and West Virginia University for helpful comments. Special thanks to Georg Strasser for many suggestions that helped to improve the paper.

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

‡Department of Economics, Royal Holloway, University of London, Egham Hill, Egham, Surrey, TW20 0EX, United Kingdom, Email: Alessio.Sancetta@rhul.ac.uk

§Department of Economics, Skidmore College, Saratoga Springs, NY 12866, Email: mwolfe@skidmore.edu

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 it at the same time. Access to news about macroeconomic announcements is therefore closely guarded until the official release time. Despite this, however, some macroeconomic announcements show evidence of informed trading before their official release (Kurov, Sancetta, Strasser, & Wolfe, 2019).[1]

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.[2] 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. Due to the cross-sectional nature of their study, its results rely on strong assumptions about the comparability of news events across different announcements at different times.

In this paper we overcome this limitation by exploiting a change in release procedures for macroeconomic announcements in the U.K. The U.K. 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).[3] In 2017, the ONS and the Bank of England eliminated the

[1]Kurov et al. (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.

[2]Gu and Kurov (2018) provide evidence that informed trading in natural gas futures before gas inventory announcements is at least partially driven by superior processing of public information.

[3]Among the arguments for the prereleases was briefing the ministers before them commenting on the data.

prerelease access. This unique setting – a change in the release policy of many announcements during a short period of time – allows us to study if and how release procedures affect the market response to the U.K. macroeconomic announcements. Further, the strictness of the new release policy allows us to infer the role of informed trading before the change in release procedures.

Although the National Statistics Code of Practice for releasing announcements states that the number of individuals receiving early access should be strictly limited (National Statistics, 2002), Bird (2017) reports 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,[4] 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). The release policy change makes it possible to analyze the effect of data security on the market reaction to an announcement holding the announcement's characteristics constant.

We analyze the reaction of the of the British Pound to the U.S. dollar (GBP/USD) exchange rate futures to U.K. macroeconomic announcements. With this study we follow the suggestion by Karolyi (2016) and others to strengthen research on non-U.S. financial markets and thereby help interpreting the abundant literature on U.S. markets from a European perspective. Because our interest focuses on the effect of the policy change in 2017, we use data from January 1, 2012 to August 31, 2019.[5] Our study of the effect of the U.K.

[4] An "Independent Review of UK Economic Statistics" mentions breaches of the prerelease rules and recommends that the list of officials with prerelease access should be as short as possible (Bean, 2016). The Royal Statistical Society issued a manifesto in 2015 (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).

[5] We begin with January 1, 2012 to avoid any lingering effects of the 2008-2009 recession.

macroeconomic announcement release policies on the foreign exchange market extends earlier work by Dominguez and Panthaki (2006) and Love and Payne (2008). Both studies consider the possibility of a preannouncement effect but find no evidence of it. A key difference of these papers is the different sample period (1999-2000 in both papers) relative to our paper.

Our results show that three of the four ONS announcements that move markets 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 at release time on the foreign exchange market change? We find evidence for both. The preannouncement price drift declined significantly, and the market reacted more strongly at release time after the release policy change. Taken together, these findings indicate that the release policy considerably affects the reaction of the foreign exchange futures market to U.K. macroeconomic announcements.

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 in our setup only market-moving announcements are relevant, we use from the Bloomberg database only those U.K. macroeconomic announcements with a Bloomberg relevance score above 75.[6]

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.[7] 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 corre-

[6]The Bloomberg relevance score ranges from 0 to 100 corresponding to the least and the most consequential announcements, respectively.

[7]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.

lation coefficient of 0.81. 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 simultaneously with a correlation coefficient of 0.82. We use, for the same reason as before, only the former. 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 the nine macroeconomic announcements listed in Table 1. Our observations are "announcement releases." For example, the gross domestic product (GDP) QoQ announcement has 87 announcement releases including the "preliminary" and "second" estimates. All nine announcements are therefore released at a monthly frequency.

Three of these nine announcements are released by private entities: the Halifax house price index, the purchasing managers' index (PMI) for the manufacturing sector[8] by IHS Markit, and the nationwide house price index by the Nationwide Building Society (NBS). These three announcements are 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 in Section 1, the ONS and Bank of England announcements used to be distributed before the official release time to individuals such as

[8]Bloomberg provides actual released values for the 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.

Table 1: Macroeconomic Announcements

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 August 31, 2019.

[a] The PMI 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.

Announcement	N_m	Source	Unit	Time	Bloomberg Score
GDP QoQ	87	ONS	%	9:30	98
CPI MoM	91	ONS	%	9:30	94
Industrial production MoM	89	ONS	%	9:30	92
PMI manufacturing	90	IHS	Index	9:30[a]	90
Nationwide House Price Index MoM	92	NBS	%	7:00	89
PPI output MoM	91	ONS	%	9:30	85
Mortgage approvals	91	BoE	No. of approvals	9:30	84
Retail sales incl. auto/fuel MoM	90	ONS	%	9:30	81
Halifax house price index MoM	92	IHS	%	8:30[b]	77

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 these release policy changes to identify the effect of the release procedure on the response of financial markets to 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 lockins is to promote fast, orderly, and accurate publication of important economic statistics. Because the information does not leave the lock-in room before the official release time, we treat this release policy in our paper as equivalent to a no-prerelease policy.[9]

2.2 Foreign Exchange Futures Market Data

To analyze the market impact of macroeconomic announcements, we focus on the foreign exchange futures market. Our analysis is closely related to Andersen, Bollerslev, Diebold, and Vega (2003), Dominguez and Panthaki (2006), and Love and Payne (2008). We use second-by-second transaction data for the British pound to U.S. dollar foreign exchange (GBP/USD) futures 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 August 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.[10] This avoids overstating any preannouncement price drift.

We then compute continuously compounded asset returns, R_t, for the entire sample as

[9] Lock-in arrangements are not uncontroversial. They come with the challenge of preventing premature disclosure from the briefing room. Further, such arrangements raise concerns about the equality of access. Specifically, 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 exactly at the official release time, whereas most traders have to rely on the slower publication on the agency website (UK Statistics Authority, 2013).

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

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.[11]

Returns are sampled from 5:00 to 15:00 London Time. Our sample thereby always includes at least 120 minutes before the earliest announcement released at 7:00 London Time and excludes the 60 minutes immediately before the 16:00 London Time fixing on the spot foreign exchange market to avoid any potential confounding effects.[12] 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 57 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 days we search for news about the British pound using the Google search engine. We find one day when the British pound move was due to an event other than macroeconomic announcements: November 15, 2018 when the British pound depreciated due to Brexit concerns. We remove this day from our sample. We then place the announcement surprises of the nine 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.

[11]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.

[12]The sampling end at 15:00 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.

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 prices in the foreign exchange futures market 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 the impact of the change in the announcement release policy.

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 et al. (2003) and Kurov et al. (2019), we use a time series methodology that embeds all announcements in one regression:[13]

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

where β_0 is a constant, the R_{t-1} term accounts for possible autocorrelation of returns, and the $S_{m,t+k}$ term accounts for the impact of the announcement surprises. The sum is over the $M = 9$ 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

[13] 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.

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.[14] 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 the release policy change (June 30, 2017) to estimate this regression.[15]

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 nine 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).[16] An announcement is

[14]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.

[15]The Bank of England changed the mortgage approvals announcement release policy on July 24, 2017 as noted in Section 1; since there were no mortgage approval announcements from July 1, 2017 to July 23, 2017, we use the July 1, 2017 date to be consistent with the ONS announcement release policy change date.

[16]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.

Table 2: Preannouncement Price Drift before Release Policy Change

This table 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.

Announcement	γ_m $[t-90min, t+5min]$	γ_m $[t-90min, t-5sec]$	**Ratio**	**Policy Change**
Retail sales incl. auto/fuel MoM	22.61 (1.42)***	10.48 (1.35)***	46%	Y
CPI MoM	14.84 (1.31)***	7.37 (1.25)***	50%	Y
Industrial production MoM	11.59 (1.37)***	3.50 (1.31)***	30%	Y
PMI manufacturing	11.14 (1.32)***	0.49 (1.26)	n/a	N
GDP QoQ	6.20 (1.38)***	-1.54 (1.31)	n/a	Y
Mortgage approvals	1.23 (1.21)	0.12 (1.16)	n/a	Y
PPI output MoM	-0.47 (1.24)	-0.38 (1.20)	n/a	Y
Halifax house price index MoM	0.39 (1.20)	-0.30 (1.16)	n/a	N
Nationwide house price index MoM	-0.20 (1.00)	-1.07 (0.98)	n/a	N

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 follows the Student's t-distribution.

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 23 basis points. The results show that there are five market-moving announcements. Four announcements are released by the ONS (retail sales, CPI, industrial production, and GDP) and one announcement is released by IHS Markit (PMI manufacturing).[17]

[17] 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 - August 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 annoucements 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 about 10.5 basis points. These results stand in contrast to studies concluding that there is no preannouncement effect in U.K. macroeconomic announcements (Dominguez & Panthaki, 2006; Love & Payne, 2008) and further suggest that preannouncement informed trading is not limited to the U.S. announcements. The announcement released without prerelease (PMI manufacturing), in contrast, does 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 (see, e.g., Kurov et al. (2019)). Interestingly, the magnitude of these ratios is similar to the magnitude in U.S. data, which averages at about 40 percent (Kurov et al., 2019).

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 $[t+5, t+30min]$ 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.[18]

[18] 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.

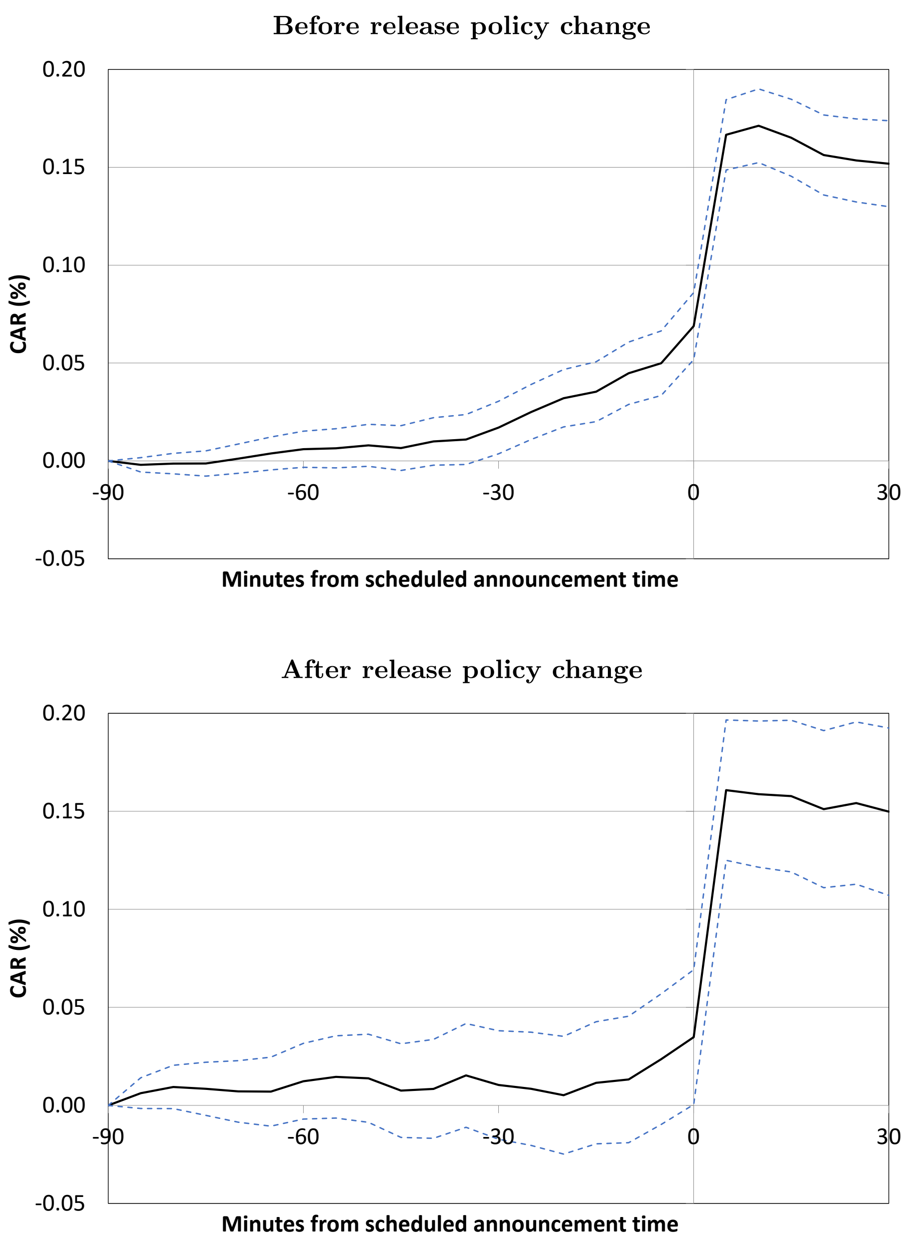
The top panel uses data from the beginning of the sample period (January 1, 2012) to the release policy change (June 30, 2017). There is some evidence of the price moving in the correct direction approximately 90 minutes before the release time, and this move becomes statistically significant approximately 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 mechanisms behind the preannouncement drift appear to be similiar on both sides of the Atlantic.

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. Appendix Figure A1 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 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, Hu, & Tang, 2016). The signed trading volume is then aggregated in five-minute intervals. Figure 2 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

Figure 1: Cumulative Average Returns

Figure 1 shows cumulative average returns (CARs) in the British poiund 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 (August 31, 2019). The solid line shows the estimated CAR. Dashed lines indicate two-standard-error bands.



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

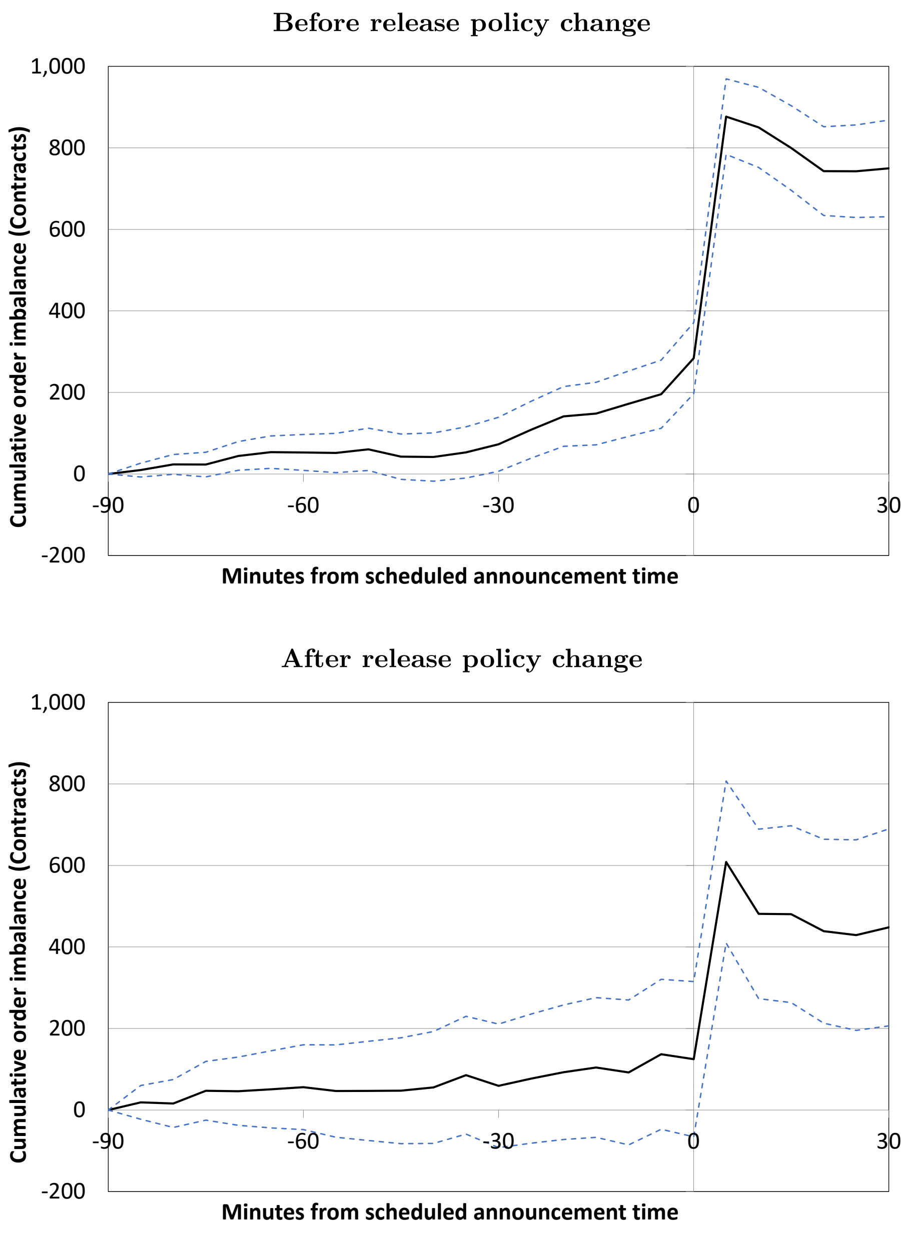
The preceding sections provided evidence that prior to the release policy change the foreign exchange rate begins drifting before the release time of three of the four market-moving announcements that utilized prereleases. We are interested in two questions: First, has tightening of the release policy changed the preannouncement drift? Second, has the tightening of the release policy change the postannouncement impact of an announcement? If the tightening of the release policy reduces informed trading before the announcement, we should expect the preannouncement drift to weaken and and the postannouncement impact to become stronger.

This estimation uses data from the release policy change (July 1, 2017) to the end of our sample period (August 31, 2019). Because this sample period is relatively short (26 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 announcements that do not move the foreign exchange market (pool 3 consisting of Halifax house price index, mortgage approvals, Nationwide house price index, and producer price index).

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 $\hat{X}^{(pre)}_{p,t} = \sum_{m \in \mathcal{G}_p} \left(\sum_{k=0}^{K} \hat{\gamma}_{m,k} S_{m,t+k} \right)$ and $\hat{X}^{(post)}_{p,t} = \sum_{m \in \mathcal{G}_p} \left(\hat{\gamma}_{m,-1} S_{m,t-1} \right)$ that measure the

Figure 2: Cumulative Order Imbalances

Figure 2 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 (August 31, 2019). The solid line shows the mean COI. Dashed lines indicate two-standard-error bands.



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):[19]

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

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).[20]

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. 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. We then follow up with a null hypothesis $H_0 : \delta_p^{(pre)} = 1$ that we test against the alternative hypothesis $H_1 : \delta_p^{(pre)} \neq 1$ for the market-moving announcements that were subject to the release policy change (Pool 1). The null hypothesis is rejected at the 5% significance level. This indicates that the preannouncement price drift has changed after

[19]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 4.3 as a robustness check.

[20]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 separate 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.

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

Table 3 uses data from the release policy change (July 1, 2017) to the end of our sample period (August 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.

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)	0.081 (0.138)
Market-moving not subject to policy change (Pool 2)	-0.215 (0.395)
Non-market moving (Pool 3)	0.166 (0.234)
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)	0.427 (0.054)***
Market-moving not subject to policy change (Pool 2)	-0.487 (0.072)***
Non-market moving (Pool 3)	-0.875 (0.355)**

the release policy change. Specifically, the estimate of $\delta_p^{(pre)} - 1$ is -0.919 and significant at the 1% level, which indicates that the preannouncement drift declined on average by about 92% 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 Figure 2 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.[21]

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

[21] The confidence intervals in the bottom panels of Figures 1 and Figure 2 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.

becomes available at release time. Because the preannouncement price drift has decreased following the release policy change (as indicated by the above analysis), we would 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 0.427 corresponds to an increase in the market reaction at release time by 43%.

This result is also qualitatively similar to the cumulative average returns in Figure 1. Before the release policy change the impact in the five minutes following the announcement is approximately 10 basis points (shown in the top panel). After the release policy change the impact has increased to approximately 13 basis points (shown in the bottom panel). Overall, the price response at release time now includes also the price response that until the policy change occurred already before the official release time. The total price impact of news contained in an announcement appears to be largely unchanged. Overall, our 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.

Taken together, the findings in the top and bottom panels of Tables 3 supported by Figures 1 and 2 indicate that the tightening of the release policy considerably changed the reaction of the foreign exchange futures market to the U.K. macroeconomic announcements.

4 Robustness Checks

We already 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), 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 shows that our results hold for different post-policy sample periods. Section 4.3 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 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 UK 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:

$$R_t = \alpha_0 + \alpha_1 R_{t-1} + \sum_{p=1}^{3} \Big[\lambda_p^{preAnn}(S_t' \hat{b}_p^{preAnn}) + \lambda_p^{postAnn}(S_t' \hat{b}_p^{postAnn}) + \lambda_p^{preAnn,postBrexit}(1_{\{t \geq \bar{t}\}} S_t' \hat{b}_p^{preAnn}) + \lambda_p^{postAnn,postBrexit}(1_{\{t \geq \bar{t}\}} S_t' \hat{b}_p^{postAnn}) \Big] + \zeta_t, \quad (4)$$

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 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 (August 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 two other sample period end dates: June 30, 2018 and December 31, 2018. Table 4 reports these results in the second and third columns, respectively. For comparison, the fourth column shows the results from Table 3 that uses the sample period through August 31, 2019. The results are qualitatively similar in all three sample periods: the preannouncement price drift is no longer present, and the

Table 4: Different Post-Policy Sample Periods

Table 4 uses data from the release policy change (July 1, 2017) to June 30, 2018, December 31, 2018, and August 31, 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.

Announcement Pool	**07/01/17 - 06/30/18**	**07/01/17 - 12/31/18**	**07/01/17 - 08/31/19**
		$\boldsymbol{\delta_p^{(pre)}}$	
(a) $\boldsymbol{H_0 : \delta_p^{(pre)} = 0}$**, i.e., Is the preannouncement price drift present?**			
Market-moving subject to policy change (Pool 1)	0.221	0.262	0.081
	(0.230)	(0.183)	(0.138)
Market-moving not subject to policy change (Pool 2)	-0.522	-0.687	-0.215
	(0.675)	(0.570)	(0.395)
Non-market moving (Pool 3)	0.587	0.363	0.166
	(0.393)	(0.332)	(0.234)
		$\boldsymbol{\delta_p^{(post)} - 1}$	
(b) $\boldsymbol{H_0 : \delta_p^{(post)} = 1}$**, i.e., Has the postannouncement price impact changed?**			
Market-moving subject to policy change (Pool 1)	1.545***	1.149***	0.427***
	(0.086)	(0.071)	(0.054)
Market-moving not subject to policy change (Pool 2)	-0.341***	-0.449***	-0.487***
	(0.122)	(0.103)	(0.072)
Non-market moving (Pool 3)	-0.061	-0.630	-0.875**
	(0.593)	(0.500)	(0.355)

postannouncement price impact has increased.

4.3 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:

$$
\begin{aligned}
R_t = 1_{\{t<\bar{t}\}} \left(\beta_0^{(1)} + \beta_1^{(1)} R_{t-1}\right) + 1_{\{t\geq\bar{t}\}} \left(\beta_0^{(2)} + \beta_1^{(2)} R_{t-1}\right) \\
+ \sum_{m=1}^{M} \sum_{k=-1}^{K} \left(\gamma_{m,k}^{(1)} 1_{\{t<\bar{t}\}} + \gamma_{m,k}^{(2)} 1_{\{t\geq\bar{t}\}}\right) S_{m,t+k} + \epsilon_t, \quad (5)
\end{aligned}
$$

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 (5) 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 (5) 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

$$
\begin{aligned}
R_t = 1_{\{t<\bar{t}\}} \left(\theta_0^{(1)} + \theta_1^{(1)} R_{t-1}\right) + 1_{\{t\geq\bar{t}\}} \left(\theta_0^{(2)} + \theta_1^{(2)} R_{t-1}\right) \\
+ \sum_{p=1}^{3} 1_{\{t<\bar{t}\}} \left[\delta_p^{(1)(pre)} \hat{X}_{p,t}^{(pre)} + \delta_p^{(1)(post)} \hat{X}_{p,t}^{(post)}\right] \\
+ \sum_{p=1}^{3} 1_{\{t<\bar{t}\}} \left[\delta_p^{(2)(pre)} \hat{X}_{p,t}^{(pre)} + \delta_p^{(2)(post)} \hat{X}_{p,t}^{(post)}\right] + \varepsilon_t, \quad (6)
\end{aligned}
$$

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 (6) 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 (6) 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 then compute

$$\frac{\left(RSS^R - RSS^U\right)}{RSS^U} = \exp\left\{\ln\left(\frac{RSS^R}{RSS^U}\right)\right\} - 1, \tag{7}$$

which is a continuous monotonic transformation of the likelihood ratio test. We use the bootstrap to compute the critical values of this test statistic. 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 UK macroeconomic announcements were tightened considerably. 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 nine 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 price drift becomes significantly weaker. Moreover, the average market reaction at release time increases. The news that used to diffuse into the market before release time is now processed at release time. The stronger 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. Aware of private information in the market, traders previously might have 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, i.e., traders without private information, the confidence to trade more aggressively in response to macroeconomic news right at its release.

References

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

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

Athow, J. (2018). Letter from Jonathan Athow, the Deputy National Statistician and Director General, Economic Statistics, Office for National Statistics to Ben Broadbent, Deputy Governor Monetary Policy, Bank of England, February 28, 2018. Retrieved on February 2, 2019, from `www.ons.gov.uk/aboutus/transparencyandgovernance/prereleaseaccess`.

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*(4), 523–543.

Bean, S. C. (2016). Independent review of UK economic statistics, March 2016. Retrieved on February 2, 2019, from `www.gov.uk/government/publications/independent-review-of-uk-economic-statistics-final-report`.

Bernile, G., Hu, J., & Tang, Y. (2016). Can information be locked-up? Informed trading ahead of macro-news announcements. *Journal of Financial Economics*, *121*(3), 496–520.

Bird, M. (2017). U.K. government's controversial early peeks at economic stats to end. *Wall Street Journal*, June 15, 2017. Retrieved on June 16, 2017, from `www.wsj.com`.

Brennan, M. J., Huh, S.-W., & Subrahmanyam, A. (2018). High-frequency measures of informed trading and corporate announcements. *Review of Financial Studies*, *31*(6), 2326–2376.

Broadbent, B. (2018). Letter from Ben Broadbent, Deputy Governor Monetary Policy, Bank of England to Jonathan Athow, the Deputy National Statistician and Director General, Economic Statistics, Office for National Statistics, February 16, 2018. Retrieved on February 2, 2019, from `www.ons.gov.uk/aboutus/transparencyandgovernance/prereleaseaccess`.

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

Data and Statistics Division. (2017). Ending pre-release access to Bank of England statistics and publications, Letter from the Data and Statistics Division of Bank of England as of July 18, 2017. Retrieved on February 2, 2019, from `webarchive.nationalarchives.gov.uk/20170823154153/http://www.bankofengland.co.uk/statistics/Documents/endingprereleaseaccess.pdf`.

Dominguez, K. M. E., & Panthaki, F. (2006). What defines 'news' in foreign exchange markets? *Journal of International Money and Finance*, *25*(1), 168–198.

Fillichio, C. (2012, June). Statement of Carl Fillichio, Testimony before the Committee on Oversight and Government Reform, United States House of Representatives. U.S. Department of Labor. Retrieved on February 2, 2019, from `www.dol.gov/newsroom/lockups/testimony`.

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.

Gu, C., & Kurov, A. (2018). What drives informed trading before public releases? Evidence from natural gas inventory announcements. *Journal of Futures Markets*, *38*(9), 1079–1096.

Hu, G. X., Pan, J., & Wang, J. (2017). Early peek advantage? Efficient price discovery with tiered information disclosure. *Journal of Financial Economics*, *126*(2), 399–421.

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

Karolyi, G. A. (2016). Home bias, an academic puzzle. *Review of Finance*, *20*(6), 2049–2078.

Kurov, A., Sancetta, A., Strasser, G., & Wolfe, M. H. (2019). Price drift before U.S. macroeconomic news: Private information about public announcements? *Journal of Financial and Quantitative Analysis*, *54*(1), 449–479.

Kyle, A. S. (1985). Continuous auctions and insider trading. *Econometrica*, *53*(6), 1315–1335.

Love, R., & Payne, R. (2008). Macroeconomic news, order flows, and exchange rates. *Journal of Financial and Quantitative Analysis*, *43*(2), 467–488.

Murphy, K. M., & Topel, R. H. (1985). Estimation and inference in two-step econometric models. *Journal of Business & Economic Statistics*, *3*(4), 370–379.

National Statistics. (2002). National statistics code of practice: Protocol on release practices. Retrieved on February 1, 2019, from `www.ons.gov.uk/ons/guide-method/the-national-statistics-standard/code-of-practice/protocols/index.html`.

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

Pullinger, J. (2017). Ending pre-release access to official statistics produced by ONS, Letter from the National Statistician to the Chair of the UK Statistics Authority Board, June 15, 2017. Retrieved on February 1, 2019, from `www.ons.gov.uk/aboutus/transparencyandgovernance/prereleaseaccess`.

Royal Statistical Society. (2017). Public trust and pre-release access to statistics. *Data Manifesto Briefing Note 4*, *May*. Retrieved on February 2, 2019, from `www.rss.org.uk/RSS/Influencing_Change/Data_manifesto/Public_trust_and_pre-release_access_of_statistics/RSS/Influencing_Change/Data_democracy_sub/Public_trust_and_pre-release_access_of_statistics.aspx?hkey=e890776e-aa19-4da3-bae6-def16454c9c7`.

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

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

Simpson, I. (2016). Public confidence in official statistics - 2016. *NatCen Social Research*. Retrieved on February 2, 2019, from natcen.ac.uk/media/1361381/natcen_public-confidence-in-official-statistics_web_v2.pdf.

Spiegelhalter, S. D. (2017). Letter to the editor "Statistical abuse". *The Times*, *May 8*.

Swanson, E. T., & Williams, J. C. (2014). Measuring the effect of the zero lower bound on interest rates. *American Economic Review*, *104*(10), 3154–3185.

UK Statistics Authority. (2010). Pre-release access to official statistics: A review of the statutory arrangements, Monitoring Report 6, March 2010. Retrieved on February 2, 2019, from www.statisticsauthority.gov.uk/wp-content/uploads/2015/12/images-prerelease-access-to-official-statistics-a-review-of-the-statutory-arrangements_tcm97-29772.pdf.

UK Statistics Authority. (2013). Issuing ONS market sensitive statistics at 09:30, Monitoring Review 7/13, October 2013. Retrieved on February 2, 2019, from www.statisticsauthority.gov.uk/archive/assessment/monitoring/monitoring-reviews/monitoring-review-7-2013---issuing-ons-market-sensitive-statistics-at-09-30.pdf.

UK Statistics Authority. (2014). Strategy for UK statistics, 2015 to 2020. Retrieved on February 2, 2019, from gss.civilservice.gov.uk/wp-content/uploads/2012/12/Better-Statistics-Better-Decisions.pdf

Wang, G. H. K., & Yau, J. (2000). Trading volume, bid-ask spread, and price volatility in futures markets. *Journal of Futures Markets*, *20*(10), 943-970.

A Appendix: Additional Figures

Figure A1: Trading Volume

In Figure A1, the sample period is from January 1, 2012 to August 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.