

Quantifying Political Pressure on Central Banks

When Politicians Talk, Do Markets Listen?*

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*Any remaining errors are the authors' own. The views expressed in this paper are those of the authors and do not necessarily reflect the views of the Central Bank of Malta.

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Abstract

We provide the first long-run systematic evidence on whether financial markets price political pressure on the Federal Reserve as a signal of future monetary policy. Using approximately four decades of Reuters newswire articles from 1988 to 2025, classified by an open-weight large language model ensemble into dovish and hawkish pressure, we estimate the daily response of interest rate expectations across the term structure, the US dollar, inflation compensation, and market volatility. Dovish pressure lowers near-term rate expectations by roughly one basis point per event—equivalent to a four-percent increase in the implied probability of a rate cut at the next FOMC meeting—with the effect decaying monotonically and vanishing beyond the three-month horizon. Hawkish pressure has no detectable effect. The response is driven entirely by presidential commentary, accompanied by dollar depreciation, with stable inflation compensation and market volatility. Excluding the Trump presidencies does not weaken the estimate, ruling out the possibility that the result is confined to an idiosyncratic recent episode. We interpret the pattern as markets pricing anticipated monetary accommodation, rather than a credibility tax on the nominal anchor.

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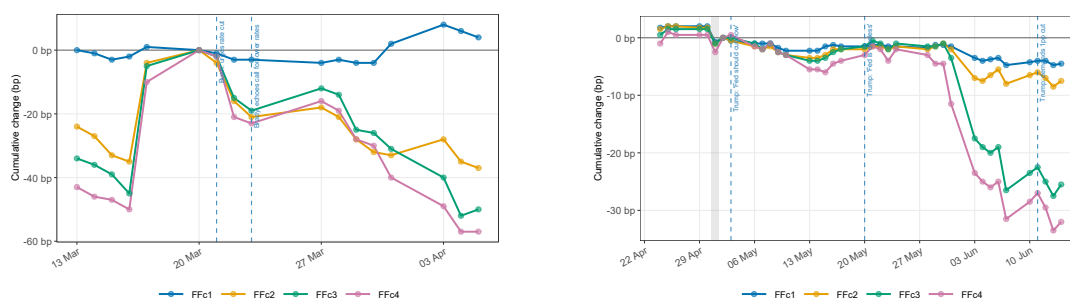
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1 Introduction

Central bank independence is among the most consequential institutional innovations of the past half-century. By delegating monetary decisions to a credible, politically insulated authority, societies can escape the inflationary bias that arises when elected officials directly control the money supply (Barro and Gordon, 1983; Alesina and Summers, 1993). Yet the boundary between politics and monetary policy has never been as clean as the institutional design implies. In the United States, from Reagan-era tensions with Volcker over the severity of disinflation to the Bush administration’s sustained pressure on Greenspan ahead of the 1992 election, elected officials have routinely and publicly attempted to influence the Federal Reserve.¹ The topic has attracted renewed academic and policy interest in the wake of President Trump’s increasingly confrontational public campaign against the Fed and its Chair, Jerome Powell—including explicit threats to remove him before the expiry of his mandate. The phenomenon is not confined to the United States: for example Japan’s incumbent Prime Minister Takaichi has publicly pressured the Bank of Japan to halt its rate-hiking cycle, prompting former IMF Chief Economist Kenneth Rogoff to warn directly that disregarding central bank independence risks driving long-term Japanese government bond yields to fiscally unsustainable levels.²³

Figure 1 provides two illustrative examples.

Figure 1: Event Studies - Federal Funds Futures Response to Political Pressure
(a) Bush Sr.–Greenspan, 1989 **(b)** Trump–Powell, 2019



Note: Each panel shows the cumulative change in the federal funds futures implied rate from a date 0- for current-month to 3-months ahead futures. Date 0 is the first political pressure event in the reference window. Dashed lines indicate political pressure events and shaded area denotes FOMC meetings.

Panel 1a tracks federal funds futures around the March 1989 Bush Sr.–Brady calls for lower

¹See Volcker and Harper (2018) for a thorough discussion.
²Rogoff made these remarks at a meeting of Japan’s Council on Economic and Fiscal Policy on 26 March 2026. See Reuters, “Harvard economist urges Japan PM Takaichi to respect central bank independence,” 8 April 2026.
³See also Schnabel (2026) for an ECB perspective of risks stemming from political pressure.

rates: the President’s statement was accompanied by a pronounced downward adjustment, whereas the Treasury Secretary’s echo the following day drew a more muted reaction. Panel 1b tracks Trump’s sustained pressure on Chair Powell during the trade war escalation of May–June 2019: the response is similar, with the first comment triggering a small revision and the second muted for weeks. Whether markets systematically price political pressure as a signal of future monetary policy is therefore not self-evident from narrative evidence alone. Answering the question systematically requires a measure of political pressure that is consistent in construction, spans multiple presidencies and congressional compositions, and captures the full range of public statements through which elected officials signal their preferences about monetary policy.

We provide such a measure by collecting approximately four decades of Reuters newswire articles from 1988 through 2025, filtered to retain articles that simultaneously mention a U.S. elected or appointed official and the Federal Reserve in the context of monetary policy. Each event is classified using an ensemble of open-weight large language models into dovish pressure and hawkish pressure. The resulting series spans eight U.S. presidential administrations, from Reagan’s final years through Trump’s second term, and constitutes the first long-run measure of public political pressure on the Federal Reserve.⁴

We then exploit daily variation in our measure to estimate the effect of political pressure on interest rate expectations across the full term structure, the US dollar, inflation compensation, and market volatility. Identification rests on daily variation in pressure conditional on scheduled FOMC announcements, slow-moving business-cycle controls, and the Bauer and Swanson (2023) orthogonalisation variables, which together absorb the Fed’s own information set and rule out concurrent macroeconomic news as a confound.

The central finding is that dovish political pressure on the Fed is associated with a significant and robust decline in near-term interest rate expectations, consistent with markets pricing in a more accommodative monetary policy path. Hawkish pressure has no detectable effect. The response is concentrated at the short end of the yield curve: dovish pressure shifts rate expectations significantly at maturities of up to one quarter ahead, with the effect fading and becoming statistically indistinguishable from zero at longer horizons. Crucially, this result is not a Trump-era artifact: the effect is larger in magnitude and more precisely estimated outside the Trump presidencies than within them, establishing political pressure as a persistent feature of the post-1988 U.S. monetary policy landscape rather than the idiosyncrasy of a single

⁴The closest existing measure is that of Drechsel (2026), who constructs an index of *private* political pressure from archival FOMC and White House records. Our measure is complementary as it captures, instead, *public* pressure.

high-intensity administration. Beyond interest rates, dovish pressure days are accompanied by a depreciation of the USD, indicating that markets simultaneously price in near-term monetary easing. Market-based inflation expectations do not respond, consistent with markets retaining confidence in the Fed’s ability to maintain its inflation target even under political pressure.

We further document a pronounced heterogeneity in actor effects: only presidential commentary generates a statistically significant market response, while statements by the Treasury Secretary, executive officials, and congressional actors are uniformly insignificant across all maturities. This pattern is consistent with markets pricing the unique institutional authority of the President—whose public statements carry direct implications for Fed Chair appointments and the political independence of the institution—rather than responding to political rhetoric more broadly.

Our contribution is fourfold. First, we construct the first multi-president, multi-actor, long-run measure of public political pressure on the Federal Reserve, covering 1988–2025 and classified with a fully reproducible, open-weight LLM pipeline. Our methodology is fully extensible to other central banks, opening the door to systematic cross-country comparisons. Second, we characterize the full yield-curve and cross-asset response to political pressure, showing that dovish pressure episodes simultaneously move down rate expectations and depreciate the exchange rate, while inflation expectations remain anchored in a pattern consistent with markets pricing monetary easing. Third, we demonstrate that the market effect of dovish pressure is not a Trump artifact but a persistent feature of U.S. monetary history, with the non-Trump coefficient larger in magnitude and more precisely estimated than the full-sample estimate. Fourth, we document a hierarchy of actor effects consistent with markets pricing institutional authority rather than political rhetoric.

The rest of the paper is organized as follows. Section 2 reviews the related literature. Section 3 describes the construction of our political pressure indicator. Section 5 presents the empirical strategy and reports the main results. Section 7 presents robustness checks. Section 8 concludes.

2 Related Literature

This paper connects to a rich literature on central bank independence and political pressure. The theoretical case for independence runs from the inflation-bias models of Barro and Gordon (1983) and Rogoff (1985) through the cross-country empirical evidence of Alesina and Summers (1993) and subsequent work documenting that statutory independence correlates robustly with lower and less volatile inflation across advanced and developing economies. Testing whether

independence is challenged in practice, however, requires a measure of pressure itself. The pioneering contribution is Havrilesky (1995), who constructs an index of executive-branch signalling toward the Federal Reserve by manually coding *Wall Street Journal* articles over 1964–1991 as ± 1 for pressure toward ease or restraint. Regressing weekly changes in the federal funds rate on cumulated pressure counts, he finds that a one-unit increase in the index is associated with roughly a five-basis-point adjustment in the policy rate. The methodology was subsequently applied to other central banks: comparable indices have been constructed for the Bundesbank (Maier et al., 2002), the Czech National Bank (Geršl, 2006), and the early ECB (Maier and Bezoen, 2004), with broadly similar conclusions that political signalling correlates with subsequent policy moves. In a related line of work, Ehrmann and Fratzscher (2011) assemble a database of politicians’ statements about ECB monetary policy across the twelve founding EMU members, but use it to characterise the *composition* of political preferences rather than their effect on market or policy outcomes. More recently, Binder (2021) extends the measurement exercise to a much broader sample, constructing a monthly dataset of political pressure events on 118 central banks using news archives and documenting that pressure is associated with higher subsequent inflation outcomes.

A parallel high-frequency literature has focused more narrowly on the Trump administrations, shifting the outcome of interest from realised inflation to the response of financial markets. Bianchi et al. (2023) find that markets responded to Trump’s monetary policy tweets during his first term by lowering interest rate expectations alongside rising equity prices and a depreciating dollar, consistent with markets pricing in monetary easing. Liu and Popova (2023) further show that Trump’s tweets raised exchange rate volatility, particularly when accompanied by negative sentiment. For the second administration, preliminary evidence from Frankovic and Karau (2026) documents a qualitatively different pattern: while markets continue to price in lower short-term interest rates following political interventions, these episodes simultaneously trigger a repricing of US risk, reflected in higher implied volatility, rising gold prices, a depreciating dollar, and declining equity and oil valuations.⁵

Despite this progress, two limitations remain. The first, and most consequential, is that the existing high-frequency literature is episodic: studies are confined to specific administrations rather than providing a systematic measure of political pressure over a long historical sample. This is largely a methodological constraint—manual coding of press articles is labour-intensive,

⁵A growing literature documents political pressure on central banks outside the United States, including the Bank of Japan (Chien et al., 2023) and several emerging-market central banks (Gürkaynak et al., 2023; Ioannidou et al., 2025; Bolhuis et al., 2026).

making multi-decade classification infeasible without automation. The second, closely related, is that the pressure measures employed are narrow in scope, typically capturing signals from a single actor (the President), which leaves the broader universe of executive and congressional pressure unexplored. We address both limitations by constructing a long-run measure of public political pressure in the US across multiple actors over 1988–2025, classified by an open-weight LLM ensemble. We then exploit daily variation in this measure to estimate its effect on the full term structure of interest rate expectations, the USD exchange rate, and market-based measures of medium- and long-run inflation expectations. The sample length allows to respond directly to the impact question: the estimated market response averages over episode-specific idiosyncrasies rather than reflecting the peculiarities of any one administration. Because the pipeline extracts the identity of the pressuring actor, the communication channel, and the target, we can also study heterogeneity along each of these dimensions—asking, for instance, whether markets respond differently to presidential pressure than to statements by other executives. The methodology is fully extensible: any central bank subject to documented public pressure can be analysed using the same pipeline, opening the door to systematic cross-country comparison of how political interference with monetary policy is transmitted through financial markets.

3 Measuring Political Pressure

3.1 News Corpus

The primary data source is Reuters newswires, accessed through the Factiva Dow Jones service and covering the period from 1987 through end-2025.⁶ We rely on a single source rather than combining multiple outlets, as in Caldara and Iacoviello (2022), in order to identify unique political pressure events and assess their relevance without conflating event occurrence with media salience. This approach avoids using the volume of news coverage as a proxy for importance—a concern that arises when the same event is counted multiple times across outlets. Reuters is particularly well suited for this purpose: it provides comprehensive, timely, and editorially consistent coverage of major political and economic developments over an extended time horizon, making it a natural textual source for long-run event classification.

The first step in constructing the dataset is to collect all articles containing at least one keyword from each of three categories of interest: political actors, the Federal Reserve, and monetary policy, a standard procedure in textual analysis since the pioneering work of Baker

⁶Reuters newswire coverage on Factiva begins in 1988, which determines the start date of our sample.

et al. (2016)). We adopt a deliberately broad filtering strategy based on expansive keyword classes and retain articles that contain at least one keyword from each of the three categories (Table 1). This approach prioritizes recall over precision, allowing for the inclusion of false positives in order to minimize the risk of excluding relevant articles (false negatives). False positives are subsequently filtered out in the LLM classification pipeline. By construction, our measure captures direct public political pressure on the Fed: every article in the sample explicitly references both a political actor and Federal Reserve monetary policy. We therefore abstract from indirect pressure operating through politicians’ stated preferences over macroeconomic outcomes, which need not invoke the Fed by name.

Table 1: Keyword Categories for Article Filtering

Category	Keywords
Political actors	<i>president, White House, administration, Treasury Secretary, Treasury Department, Senate Banking, Banking Committee, House Banking, economic advisor, Council of Economic Advisors, senator, senators, representative, representatives</i>
Federal Reserve	<i>Federal Reserve, Fed, FOMC, Fed Chair, Fed Chairman, Fed governor, Fed governors, Fed president, Fed presidents, Fed official, Fed officials, Fed policymaker, Fed policymakers</i>
Monetary policy	<i>interest rate, interest rates, monetary policy, rate cut, rate cuts, rate hike, rate hikes, fed policy, federal funds, fed funds rate, inflation, price stability, money supply, money growth</i>

3.2 LLM Classification Pipeline

Starting from a broad collection of articles requires a sequence of refinement steps to isolate the subset of truly relevant observations. To this end, we develop a three-stage LLM pipeline that transforms the raw corpus into a precise set of classified pressure events. The use of LLMs is motivated by the need to automate a classification task that would otherwise require extensive manual labor, making large-scale, multi-decade analysis infeasible. Moreover, LLMs bring two specific advantages over simpler rule-based or keyword approaches: they are particularly suited for inferring the direction of a communication—distinguishing, for instance, a politician pressuring the Fed from the other way around—and for extracting structured metadata from unstructured text, such as the identity of the pressuring actor, the target, and the communication channel, without requiring hand-crafted extraction rules.

The main challenge in using LLMs for replication purposes is ensuring that identical inputs and prompts yield identical outputs. We address this by using open-weight models with temperature set to zero, which allow full control over the inference environment. Open-weight models

may, however, come at some cost in classification accuracy relative to proprietary alternatives. To mitigate this, each stage of the pipeline is run in parallel across three LLMs—Qwen, Llama, and DeepSeek⁷—and the final set of events retains only those observations for which at least two of the three models agree. Beyond improving accuracy, the majority-voting architecture reduces classification variance and partially decorrelates measurement error: because the three models differ in architecture, training data, and alignment procedure, their error patterns are plausibly independent, so idiosyncratic biases of one model are unlikely to be ratified by the other two.⁸

The pipeline proceeds in three stages, illustrated in Figure 2. The first stage consolidates multiple journalistic entries into distinct event units. The unit of analysis is the political pressure event—a discrete act of public interference with the Fed—not the article reporting it. Treating each article as a separate observation would conflate the intensity of media coverage with the frequency of pressure events, reintroducing precisely the problem that motivates relying on a single source. Even within a single source, the same event may be reported multiple times as new information becomes available; it is therefore essential to group articles referring to the same underlying event. This initial step reduces the raw corpus from approximately 11,696 articles to 7,843 unique events.

The second stage identifies whether an event involves a U.S. politician commenting on the Federal Reserve, excluding cases in which the direction of communication is reversed or where the content reflects journalistic interpretation rather than direct political statement. Two exclusion criteria are applied. First, the direction of communication must run from a political actor toward the Fed. Second, the content must reflect an attributable statement by a named political actor rather than editorial framing or analyst commentary embedded in the article. This stage acts as the primary precision filter, reducing the sample to 923 events. In addition to applying these criteria, we extract structured metadata for each event: the name and position of the politician, the object of the statement, and the communication channel through which it is conveyed, as summarized in Figure 14.⁹ These metadata serve two purposes: they enable the heterogeneity analysis in Section D.3, and they feed directly into the quality check below.

Since we are interested in the market effects of pressure exerted by actors currently holding positions of institutional authority, we restrict the sample to statements made by incumbent

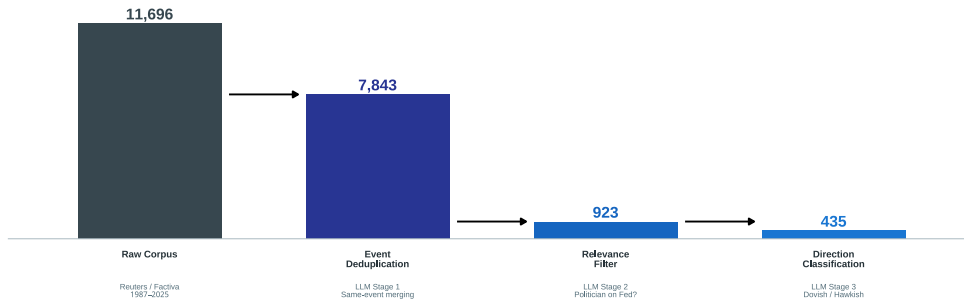
⁷Specifically, Qwen (qwen3-235b-a22b-2507), Meta Llama (llama-4-maverick), and DeepSeek (deepseek-v3-0324).

⁸Reducing the systematic component of measurement error is consequential for inference: non-classical measurement error of the kind that LLM misclassification can generate does not attenuate the OLS estimator in a simple, predictable way (Battaglia et al., 2024), and majority voting mitigates this concern by pushing the noise-to-signal ratio down. The residual caveat is that all three models may share common failure modes if their training corpora overlap sufficiently; we return to this in the validation section.

⁹The extraction of these features is performed using Qwen.

officeholders, excluding candidates and former officials. Markets are more likely to reprice assets in response to pressure from actors with current decision-making power over fiscal policy, legislative agenda, or executive appointments. We verify Reuters’ incumbent classification by cross-checking extracted politician names and positions against official records from the U.S. Congress, the White House, and the Treasury.¹⁰

Figure 2: Three-Stage LLM Classification Pipeline



Note: The figure illustrates the three-stage LLM pipeline used to classify political pressure events. Each stage is run in parallel across Qwen (qwen3-235b-a22b-2507), Meta Llama (llama-4-maverick), and DeepSeek (deepseek-chat-v3-0324), and only observations for which at least two of the three models agree are retained.

In the final stage, the three LLMs classify each remaining event along two dimensions. First, they determine whether the statement constitutes genuine political pressure—that is, whether it goes beyond mere commentary and represents an attempt to influence the conduct of monetary policy. Second, conditional on being classified as pressure, they determine its direction: dovish, if the politician advocates for easier monetary conditions such as rate cuts or more stimulus; or hawkish, if the politician pushes for tighter policy or more aggressive action against inflation.¹¹¹² This step further reduces the sample to 579 events, of which 508 are classified as dovish and 71 as hawkish—a pronounced asymmetry reflecting the well-documented bias of political actors toward easier monetary conditions, consistent with Barro and Gordon (1983).¹³

¹⁰This procedure confirms the reliability of Reuters’ labeling in the vast majority of cases. Across the full sample, we identify only three instances of misclassification, all involving Treasury Secretary nominees reported as incumbents prior to Senate confirmation.

¹¹The classification does not require an explicit directional statement. When pressure is implicit, the model infers direction from the economic context embedded in the article—the prevailing inflation or unemployment environment and the recent stance of monetary policy. Explicit statements take precedence over contextual inference whenever both are present.

¹²This context-sensitive inference illustrates a key advantage of LLMs over bag-of-words or dictionary-based classifiers. A tweet calling for the Fed Chair to be fired contains no rate-relevant keywords, yet an LLM correctly infers dovish pressure by recognising that removing the FOMC Chair during a tightening cycle is an implicit demand for easier policy.

¹³Full text of the three prompts in Appendix B.

3.3 Political Pressure Measure

Classified articles are aggregated to the daily frequency to construct the main treatment variables used in the regression analysis. For each trading day t , we count the number of articles classified as dovish and hawkish political pressure, yielding $n_{\text{dovish},t}$ and $n_{\text{hawkish},t}$, respectively. Both variables are non-negative integers that equal zero on days with no qualifying articles. The choice of daily rather than intraday frequency is motivated by data availability: precise publication timestamps are available in the newswires only from mid-1997 onward, so moving to higher frequencies would sacrifice the first decade of the sample—a period that includes some of the most intense pressure episodes in our data. Figure 3 shows the temporal evolution of our indicators.

Several patterns stand out. First, dovish pressure is the dominant form throughout the sample, consistent with the 508-to-71 split documented in the previous section. Second, pressure is episodic rather than continuous, clustering around specific confrontations between the executive and the Fed. The most prominent dovish episodes are the Trump–Fed conflict during the first, and even more intensely, the second Trump administration. The Bush Sr.–Greenspan tensions of the early 1990s are also clearly visible, with President Bush publicly and repeatedly criticizing Greenspan for keeping rates too high ahead of the 1992 election—a conflict widely seen as having contributed to Bush’s electoral defeat and to a lasting norm against overt presidential commentary on monetary policy. Third, hawkish pressure is rare but notable: the most intense hawkish episode coincides with the 2022 inflation surge, when CPI reached 9% and several legislators publicly pushed for more aggressive tightening. A secondary cluster appears around the 2010 QE2 announcement, when a group of Republican lawmakers wrote an open letter urging the Fed to abandon its asset purchase program.¹⁴

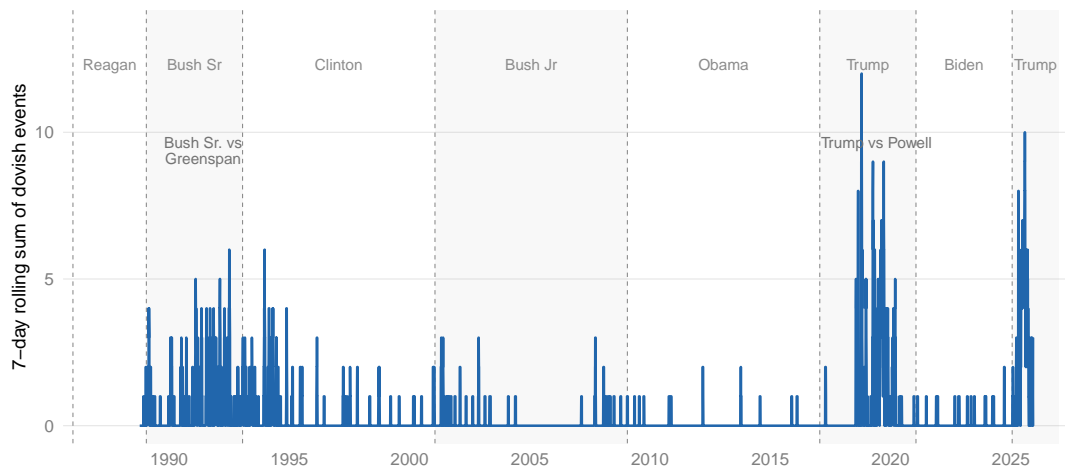
3.4 Validation

We validate our measure against two existing high-frequency benchmarks for the Trump administrations: Bianchi et al. (2023), who construct a daily pressure measure based on Trump’s monetary policy tweets combined with Bloomberg news coverage during the first administration, and Frankovic and Karau (2026), who use Truth Social posts during the second term. Figure 4 reports the comparison at the daily frequency, smoothed by a five-day centred rolling mean.

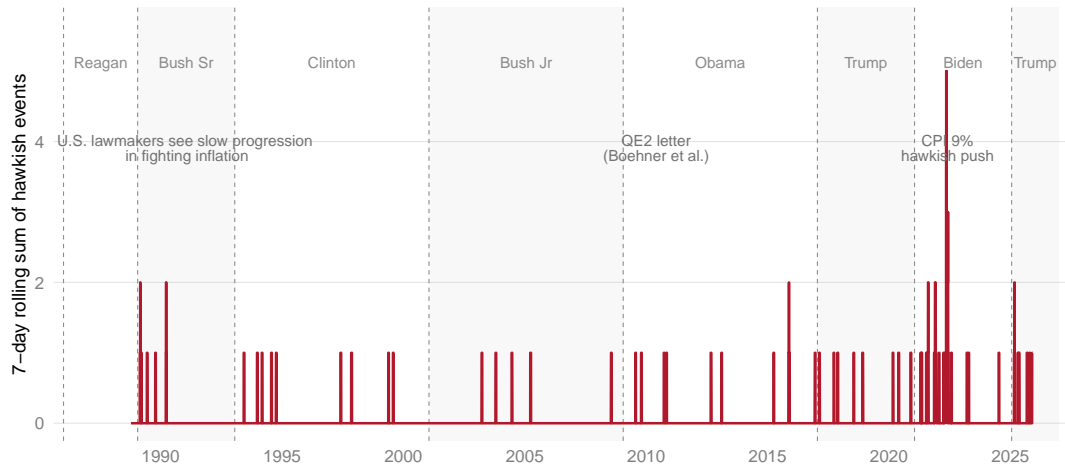
The two measures co-move broadly, correctly identifying the same major pressure episodes:

¹⁴In November 2010, Speaker Boehner, Majority Leader Cantor, Senate Majority Leader McConnell, and Whip Kyl sent an open letter to Chairman Bernanke urging the Fed to “resist further extraordinary intervention in the U.S. economy”.

Figure 3: Political Pressure over Time
(a) Dovish pressure



(b) Hawkish pressure

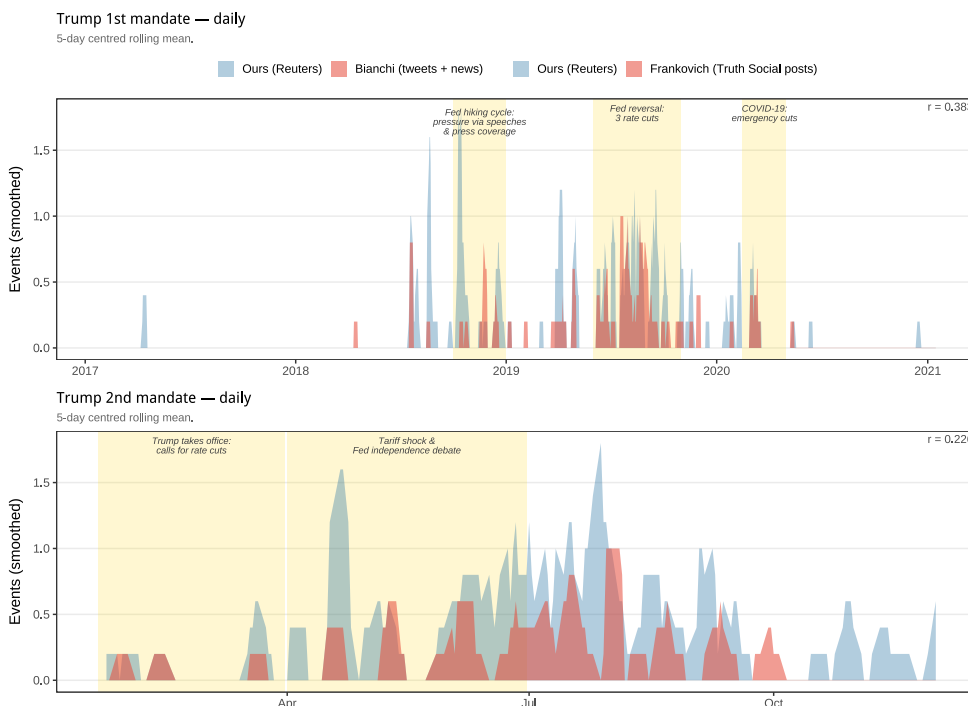


Note: Dashed lines indicate change in presidency.

the rate-hiking cycle of 2018–2019, the Fed reversal and subsequent rate cuts, the COVID-19 emergency cuts, and the tariff shock and Fed independence debate of 2025. The daily correlation between our measure and Bianchi et al. (2023) is 0.40, and 0.23 against Frankovic and Karau (2026). These moderate correlations are expected: Bianchi et al. (2023) and Frankovic and Karau (2026) restrict attention to a single actor and a single channel—presidential social media—whereas our measure aggregates pressure from the full spectrum of political actors and communication channels.

Tables 2a and 2b formalize the overlap analysis. Against Bianchi et al. (2023), our measure identifies pressure on 106 trading days versus 57 for the tweet-based measure, with 36 days in common. Against Frankovic and Karau (2026), we identify 70 days versus 31, agreeing on

Figure 4: Comparison with Existing Benchmarks



Note: All series are smoothed using a five-day centred rolling mean. Yellow shading denotes periods of heightened pressure identified in the narrative record.

22. In both cases, the dominant off-diagonal cell consists of days flagged by us but not by the benchmark, confirming that the gap reflects the broader scope of our source. The small number of days captured by the benchmarks but not by us—21 and 9 respectively—are consistent with social media posts that did not generate Reuters newswire coverage, plausibly because they were deemed insufficiently newsworthy.

Table 2: Confusion matrices across validation samples
(a) Ours vs. Bianchi et al. **(b) Ours vs. Frankovic et al.**

	Bianchi et al. (Tweets + news)			Frankovic et al. (Truth Social posts)	
Ours (Reuters)	Event	No event	Ours (Reuters)	Event	No event
Event	36 (2.6%)	70 (5.0%)	Event	22 (10.0%)	48 (21.8%)
No event	21 (1.5%)	1284 (91.0%)	No event	9 (4.1%)	141 (64.1%)

Note: Each cell reports count of trading days and percentage of total in the mandate sample. *Event* = at least one dovish pressure observation on that day. Mandate periods: Bianchi, 16 Jun 2015–20 Jan 2021; Frankovich, 20 Jan 2025–20 Jan 2026.

Taken together, our measure advances the existing literature along two dimensions: it extends

the time horizon to a systematic four-decade record, and it captures the full universe of actors and channels through which political pressure on the Fed is publicly exerted—recovering a substantially larger set of pressure events while remaining well-anchored to the same historical episodes identified by existing measures.

4 Data

4.1 Dependent Variables

4.1.1 Federal Funds Futures

The primary dependent variables in this study are federal funds futures (hereafter, *ffc*) traded on the Chicago Mercantile Exchange (CME), at different maturities. *Ffc* are contracts that settle on the average effective federal funds rate over a given expiration month, quoted as 100 minus that expected rate. For an expiring contract, the last trading day corresponds to the last business day in the delivery month of the futures contract. The corresponding daily federal funds overnight rate is provided by the Federal Reserve Bank of New York. For each contract maturing i months ahead, the futures rate can be decomposed into the expected federal funds target, a term capturing the discrepancy between the target and the effective rate, and a bias term reflecting risk premia. Under the standard assumption that political pressure events do not systematically affect these latter two components within a short window— an assumption shared by the high-frequency monetary policy literature since Gürkaynak et al. (2005)— changes in futures prices over a narrow window identify the market’s revision in interest rate expectations. Contracts at successively longer maturities allow us to trace this revision across the term structure, from the current month out to the twelve consecutive contract, providing a comprehensive picture of how political pressure shifts expectations not only about the immediate policy rate but also about the medium-run path of monetary policy.

In the LSGE dataset, continuous federal funds futures series are available only for a limited historical window. To extend the series backward, we roll individual futures according to the following procedure: for each maturity N , we select the contract expiring exactly $N + 1$ months ahead and stitch them together over time. Under this convention, *ffc1* corresponds to the current-month contract, *ffc2* to the one-month-ahead contract, and so on.¹⁵ In addition, since

¹⁵Because these contracts reflect the average effective federal funds rate over the delivery month, price discontinuities arise when transitioning from one contract to the next. We account for these mechanical jumps by including a dummy variable for the beginning of month in our successive estimations to ensure that they do not affect the results.

the front-month futures price represents a monthly average of the federal funds rate over the entire settlement month, a raw change in the futures price understates the true policy surprise when the pressure event occurs mid-month. To see this, consider a month with 30 days and a pressure event on day 20: the futures price only reflects the expected rate for the remaining 10 days of the month, so a one-basis-point revision in the expected overnight rate translates into only a 10/30 basis point move in the futures price. Scaling by the inverse of this fraction recovers the true revision in the expected rate level. Following Kuttner (2001), we implement this adjustment formally as:

$$\text{surprise}_t = \frac{D}{D-d} \cdot \Delta ffc_t^1, \quad (1)$$

where D denotes the total number of days in the settlement month and d is the current calendar day. To avoid the scaling factor becoming unstable as the settlement date approaches—when the denominator $D - d$ shrinks toward zero—we cap the adjustment for the final three calendar days of each settlement period.¹⁶

4.1.2 Treasury Futures

In general, moving to longer maturities, trading volumes in federal funds futures, however, become increasingly thin at the beginning of the sample, implying a progressive shortening of the usable time span for a given data quality threshold. To maintain a comparable number of observations across maturities and, crucially, to extend the analysis to the long end of the yield curve—where inflation expectations and term premium channels of political pressure could arise—we complement the federal funds futures with Treasury-based instruments. Specifically, we include the 3-month Treasury Bill yield from FRED as a benchmark for the short end of the curve, and add Treasury futures contracts for the 2-year (TU), 5-year (FV), 10-year (TY), and 30-year (US) maturities, sourced from the CME via LSEG Workspace.

Treasury futures are standardized exchange-traded contracts that obligate the holder to buy or sell a notional basket of U.S. Treasury securities at a predetermined price on a future delivery date. Rather than referencing a single bond, each contract specifies an eligible basket of Treasury securities within a given maturity range—for instance, the 10-year contract (TY) accepts delivery of Treasury notes with remaining maturities between 6.5 and 10 years—and uses a conversion factor system to make bonds of different coupons and maturities economically equivalent for

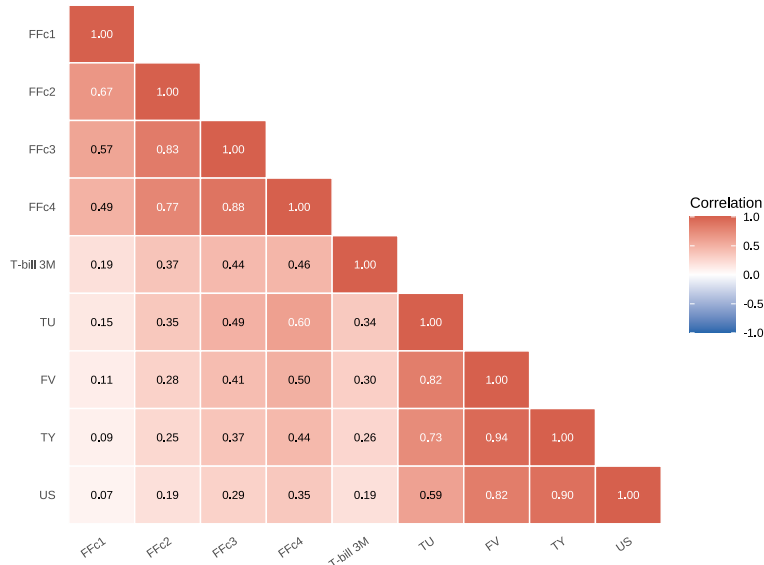
¹⁶This correction applies exclusively to the front-month contract (FFc1), whose settlement price averages the federal funds rate over the current calendar month. For contracts at longer maturities, the settlement month lies entirely in the future and the futures price already reflects the full expected rate for that month, so no timing adjustment is required.

delivery purposes.¹⁷

For these contracts, we construct continuous series by splicing quarterly-expiring contracts. Trading volumes decline near expiry as market participants roll into the next contract; rather than dropping these observations, we include a dummy variable in the regression that equals one on days for which $\log(\text{volume})$ falls more than one rolling standard deviation below its 63-day rolling mean, absorbing any expiry-related noise without reducing the sample (Figure 17). Price returns on Treasury futures are expressed in basis points, with the sign convention that a positive return corresponds to a price increase and therefore a *decline* in the underlying yield.

Figure 5 reports the pairwise correlations among all dependent variables.

Figure 5: Correlation Structure of Dependent Variables



Note: FFC1 refers to the current-month contract.

The high correlation within the federal funds futures strip—and within Treasury futures at adjacent maturities—is expected, as instruments at nearby horizons reflect overlapping information about the expected path of monetary policy. The lower correlation between the short end (FFc1–FFc4) and the long end (TY, US) is consistent with the view that long-term yields embed an additional term premium component that is largely orthogonal to near-term rate expectations.

¹⁷We prefer Treasury futures over on-the-run Treasury yields for two reasons. First, futures markets are among the most liquid financial markets in the world: the 10-year Treasury futures contract alone regularly trades several hundred thousand contracts per day, with bid-ask spreads of a single tick (1/64 of a point), making them less susceptible to microstructure noise than cash bond markets. Second, futures prices are not contaminated by the specialness premium that affects on-the-run Treasury yields in the repo market—whereby a bond in high demand as collateral trades at an artificially depressed yield relative to its fair value—providing a cleaner measure of interest rate expectations at standardized maturities along the yield curve (Duffie, 1996; Krishnamurthy, 2002).

This cross-instrument coverage allows us to characterize the complete yield-curve response to political pressure on the Federal Reserve, from the near-term policy rate embedded in the front-month federal funds futures contract to the very long-run expectations captured by the 30-year Treasury futures.

5 Empirical Strategy and Results

Building on the data and classification pipeline described in Section 3 and 4, this section formalises the regression framework used to estimate the effect of political pressure on rate expectations. The core specification regresses the daily change in each rate instrument on the contemporaneous count of political-pressure articles, two lags of the dependent variable, a vector of dummies, and a vector of controls. Formally:

$$\Delta r_t^m = \alpha + \beta \text{PolPres}_t + \sum_{k=1}^2 \gamma_k \Delta r_{t-k}^m + \mathbf{D}'_t \delta + \mathbf{Z}'_t \lambda + \varepsilon_t \quad (2)$$

where Δr_t^m is the daily change in the rate instrument at maturity m , the variable PolPres_t denotes the daily count of news articles classified as dovish or hawkish political pressure, and the coefficient β therefore measures the Conditional Average Treatment Effect (CATE) in the rate instrument associated with one additional political-pressure article on day t . The two lags $\Delta r_{t-1}^{(m)}$ and $\Delta r_{t-2}^{(m)}$ absorb short-run autocorrelation that is common in daily financial series, and ε_t is the regression residual. The regressions are estimated at the daily frequency, which implies that rate fluctuations on a given day may reflect a combination of political pressure and other contemporaneous factors. Isolating the effect of political pressure from these confounders requires a rich set of controls, which we organize into two groups.

The first group (\mathbf{D}_t) consists of indicator variables. We include a beginning-of-month dummy (BOM_t) to absorb the mechanical price discontinuities that arise when rolling federal funds futures contracts at month turns, and an FOMC announcement dummy to capture the large movements in rate expectations that occur on policy decision days and that are by construction unrelated to political pressure. For Treasury futures, we substitute the BOM_t dummy with the low-volume dummy described in section 4.1.2.

The second group collects time-varying covariates (\mathbf{Z}_t) that proxy for the macroeconomic and financial environment in which pressure occurs. As discussed above, the core concern is that political pressure and futures returns both correlate with the state of the economy. To absorb business-cycle variation and control for the state of the Federal Reserve’s dual mandate, we in-

clude the inflation gap—defined as PCE in annual percentage changes minus the 2% target—and the unemployment gap relative to the CBO estimate of the natural rate¹⁸. These are slow-moving variables that capture the macroeconomic environment in which political pressure occurs, and that directly correspond to the two objectives the Fed is mandated to pursue. Including them ensures that any estimated effect of political pressure on rate expectations is not simply picking up the mechanical response of markets to a deteriorating economic outlook that politicians and investors observe simultaneously. To further control for higher-frequency predictable variation in rate expectations, we include a subset of the controls proposed by Bauer and Swanson (2023), who show that a range of publicly observable variables 3-month oil returns, 3-month S&P 500 returns, nonfarm payroll release surprises, and Treasury yield skewness—can predict monetary policy surprises at FOMC meetings, implying that naive high-frequency regressions may confound genuine shocks with forecastable variation in the policy rate.¹⁹

The equation is estimated by OLS, and all standard errors are computed using Newey-West HAC corrections. Regarding the sample period, we consider two specifications: *full sample* and *excl. Trump*. The *full sample* covers all trading days over 1987–2025. The *Excl. Trump* specification excludes the first Trump term (20 January 2017 to 20 January 2021) and the post-inauguration period of the second term (from 20 January 2025 onwards). This split allows us to assess whether the unusually intense political pressure during these periods drives the results relative to more typical episodes.

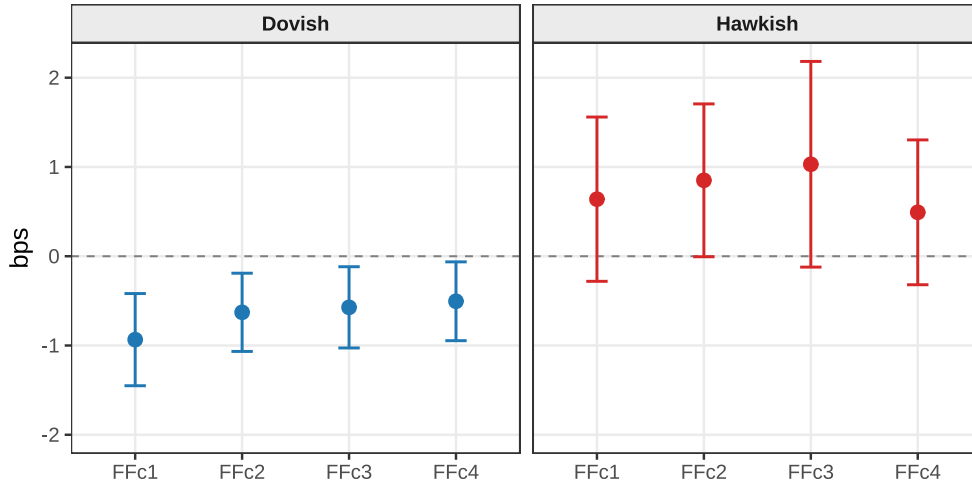
5.1 Baseline Results

We first estimate the baseline specification on the full sample to assess whether the impact of political pressure on rate expectations varies by direction and maturity. The central finding is that dovish political pressure is associated with a statistically significant decline in near-term federal funds rate expectations, whereas hawkish pressure yields no comparable effect, as shown in Figure 6.

¹⁸Both inflation and unemployment data were downloaded from FRED.

¹⁹We retain only those Bauer–Swanson controls available at daily frequency over our full 1988–2025 sample

Figure 6: Impact of dovish and hawkish pressure on federal funds futures



Note: The figure shows OLS estimates of $\hat{\beta}$ from equation (2) estimated separately for dovish and hawkish pressure across federal funds futures contracts. The dependent variable is the daily change in the implied rate (in basis points). Confidence intervals are constructed at the 90% level using Newey–West HAC standard errors.

For dovish pressure, the estimated effect is largest at the front of the curve: an additional pressure event is associated with a 0.93 bps decline in the front-month contract.²⁰ The effect decays monotonically along the near-term segment of the term structure, falling by 0.63 bps for one-month ahead, 0.57 bps for two-months ahead, and 0.50 bps for three-months ahead. This attenuation pattern is consistent with a market revision of expectations that only affects short horizons. To interpret the economic magnitude of the estimated effects, note that the standard Federal Reserve policy move is 25 bps. Under a simple two-outcome scenario in which the Fed either holds rates unchanged or cuts by 25 bps, a futures price decline of 0.25 bps corresponds to a 1 percentage point increase in the implied probability of a cut. On this metric, each additional dovish political pressure raises the probability of a near-term cut by approximately 3.7 percentage points at the front-month horizon, declining to 2.5, 2.3, and 2.0 percentage points for the subsequent maturities. While modest relative to the usual tightening and loosening cycles (± 200 – 400 bp), these effects are per-event averages; therefore, the cumulative signal embedded in sustained political pressure campaigns can be meaningfully larger.²¹

Estimates for hawkish pressure are positive across all maturities and borderline significant

²⁰We focus initially on *ffc1–ffc4* as these are the most liquid contracts throughout the sample period; trading volume drops substantially beyond the four-month horizon in the earlier portion of the sample.

²¹The regressions include two lags of the dependent variable (Equation 2), so the long-run multiplier is $1/(1 - \hat{\gamma}_1 - \hat{\gamma}_2)$. Across contracts, the sum of AR coefficients ranges from 0.07 to 0.11, implying cumulative effects 8–12% larger than the contemporaneous estimates reported in the text.

at conventional levels for the second and third contract, though the wide confidence intervals — reflecting the relative scarcity of hawkish episodes, which account for roughly one-sixth of observed dovish pressure days — preclude sharp inference. The point estimates suggest a symmetric market response in direction, but the asymmetry in episode frequency prevents a definitive comparison of magnitudes.

The baseline coefficients are accompanied by statistically significant estimates on most of the controls, confirming that the specification captures the daily determinants of futures-price variation. The political pressure coefficient is estimated against, rather than in place of, this conditional information set (Table 3). The inflation gap enters positively and the unemployment gap negatively across all four maturities, each significant at conventional levels, reproducing the sign pattern of a standard Taylor-type response: futures price in a higher near-term policy rate when inflation runs above target and a lower rate when unemployment runs above its natural level. The FOMC announcement dummy is negative and significant throughout, with magnitudes between -0.010 and -0.013 basis points, consistent with the average FOMC meeting in our sample being dovish relative to prior expectations. The Nonfarm Payroll release dummy is also negative and strongly significant at the front of the curve— indicating that scheduled labour-market releases in the sample coincided on average with downward revisions in the expected policy path. The yield-curve skewness measure (ISK) enters positively and is highly significant at every maturity, with magnitudes rising from roughly 0.005 at FFc1 to 0.009 at FFc4, which is consistent with a standard term-premium channel in which richer convexity in the yield distribution translates into higher forward rates further out the curve. Analogous results hold for the hawkish regressions (Table 8),

Table 3: Dovish Political Pressure and Fed Funds Futures

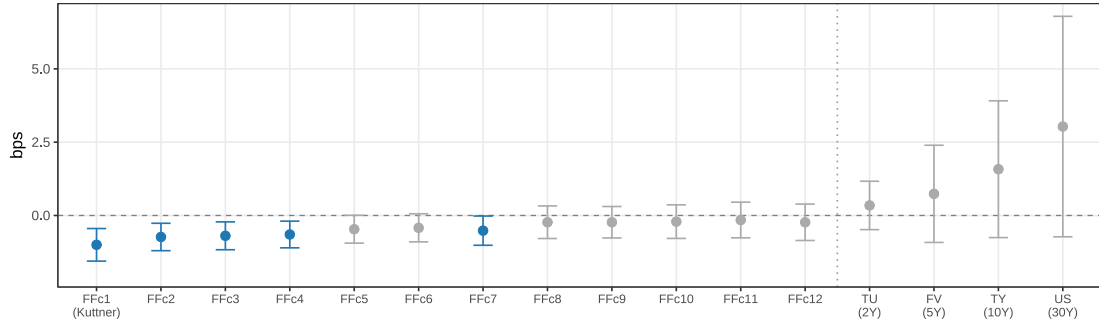
Dependent Variables:	FFc1 (Kuttner)	FFc2	FFc3	FFc4
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Constant	-9.28×10^{-5} (0.0006)	-0.0006 (0.0004)	-0.0007 (0.0005)	-0.0007 (0.0005)
Dovish pressure (n_{dovish})	-0.0093*** (0.0031)	-0.0063** (0.0027)	-0.0057** (0.0028)	-0.0050* (0.0027)
Oil price growth (QoQ, %)	-2.22×10^{-5} (2.63×10^{-5})	-3×10^{-5} (2.33×10^{-5})	$-4.05 \times 10^{-5*}$ (2.46×10^{-5})	$-4.53 \times 10^{-5**}$ (2.29×10^{-5})
S&P 500 return (QoQ, %)	0.0001** (7.19×10^{-5})	5.74×10^{-5} (5.76×10^{-5})	5.44×10^{-5} (5.94×10^{-5})	4.93×10^{-5} (5.76×10^{-5})
NFP announcement	-0.0084*** (0.0032)	-0.0051** (0.0023)	-0.0063** (0.0025)	-0.0047 (0.0029)
Inflation gap	0.0011** (0.0005)	0.0008** (0.0004)	0.0008* (0.0005)	0.0008* (0.0005)
Unemployment gap	-0.0008** (0.0004)	-0.0006*** (0.0002)	-0.0006*** (0.0002)	-0.0007*** (0.0003)
FOMC announcement	-0.0103** (0.0048)	-0.0113*** (0.0038)	-0.0119*** (0.0040)	-0.0124*** (0.0038)
Yield skewness (ISK)	0.0048*** (0.0018)	0.0059*** (0.0017)	0.0072*** (0.0017)	0.0085*** (0.0018)
<i>Fit statistics</i>				
Observations	9,004	9,019	9,015	9,015
R ²	0.016	0.032	0.039	0.032

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

All columns use the full sample. Coefficients on FF futures variables multiplied by 100 are expressed in basis points. All regressions include: beginning-of-month dummy and two lags of the dependent variable (omitted from display). HAC (Newey-West) standard errors in parentheses. *, **, *** denote significance at the 10%, 5%, 1% level.

To assess whether the effect extends beyond the near-term horizon, we estimate the baseline specification across the full term structure of federal funds futures contracts, supplementing them with Treasury futures to gauge the medium- and longer-term impact. Figure 7 shows the results.

Figure 7: Impact of dovish political pressure across maturities



Note: The figure shows OLS estimates of $\hat{\beta}$ from equation (2) for dovish pressure across the full maturity spectrum, from near-term federal funds futures (*ffc1*–*ffc12*) to Treasury futures (TU, FV, TY, US). Blue markers denote estimates significant at the 10% level; grey markers denote insignificant estimates. The dependent variable is the daily change in the implied rate or yield (in basis points). A dotted vertical line separates federal funds futures from Treasury futures. Confidence intervals are constructed at the 90% level using Newey–West HAC standard errors.

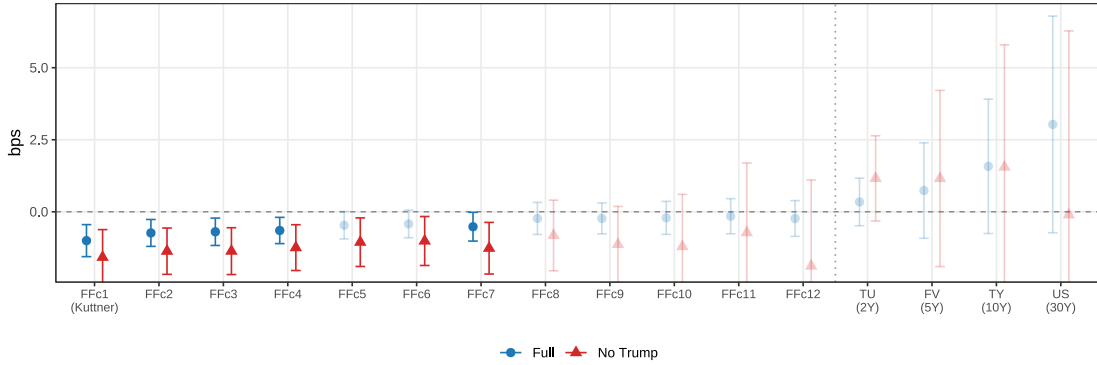
The statistically significant estimates cluster at the near end of the curve — *ffc1* through *ffc4* — consistent with dovish pressure primarily shifting rate expectations over the next three months. The effect remains borderline significant through *ffc7*, with point estimates consistently negative and confidence intervals that only marginally encompass zero, before dissipating entirely at longer horizons.

For Treasury futures, point estimates are near zero or positive across all four contracts, with confidence intervals that widen substantially with maturity. This indicates that dovish political pressure does not lead markets to revise longer-term rate expectations, consistent with the view that political interference is perceived as a transitory phenomenon that leaves the medium- and long-run policy stance unaffected. Moreover, the absence of any upward pressure on longer-term yields argues against a risk-premium channel in which political interference de-anchors inflation expectations and pushes up nominal yields.

5.2 A Trump story?

A key question is whether the baseline results are driven solely by Trump presidencies. As shown in Figure 3, the intensity of political pressure increased markedly during both mandates, with the pattern shifting from isolated episodes to a more persistent and continuous process. To answer this question, we re-estimate the baseline specification on a restricted sample excluding the Trump periods. Results are reported in Figure 8; estimates for hawkish pressure remain statistically insignificant throughout, hence we exclude them.

Figure 8: Dovish political pressure: full sample vs. excluding Trump periods



Note: The figure shows OLS estimates of $\hat{\beta}$ from equation (2) for dovish pressure, estimated on the full sample and on the subsample excluding the Trump presidencies. The dependent variable is the daily change in the implied rate (in basis points). Confidence intervals are constructed at the 90% level using Newey–West HAC standard errors.

The sign of the effect is stable across samples, while its magnitude is approximately 50% larger outside the Trump period at all maturities. More specifically, in the non-Trump subsample, near-term rate expectations for the front-month contract decline by 1.58 bps per additional pressure event, with the effect attenuating gradually to 1.37 bps for *ffc2*, 1.37 bps for *ffc3*, and -1.24 bps for *ffc4*.

These findings allow us to strongly reject the argument that the baseline results are driven by the Trump presidencies. If anything, the effect is larger and more consistently significant outside that period, confirming that the underlying mechanism operates throughout the full 1988–2025 sample and is not attributable to a single high-intensity episode. Notably, despite the reduction in sample size and the associated loss of precision, the non-Trump estimates remain statistically significant up to the six-month horizon.

One natural interpretation of the attenuated Trump-period estimates is that markets progressively discount political pressure as its frequency rises. When pressure is a rare signal, each additional article carries substantial informational weight and is more likely to be perceived as a credible cue. When it becomes a near-daily occurrence, however, the marginal impact on rate expectations diminishes: markets may come to treat persistent pressure as background noise rather than as new information about the future path of monetary policy. An alternative interpretation is that the true effects differs across presidential administrations, reflecting heterogeneity in the perceived credibility of the threat or in the Fed’s willingness to accommodate. Under this view, the attenuation is not a function of frequency but of the political context itself.

Distinguishing between these two mechanisms —saturation versus structural heterogeneity — is not straightforward in a reduced-form framework, and we treat the split-sample evidence as suggestive rather than conclusive. However, we provide evidence consistent with the saturation mechanism in Appendix D.2. Regardless of which interpretation one favors, the key takeaway is unambiguous: the baseline results are not driven by the Trump presidencies, as the effect is stronger, more precisely estimated, and significant across a wider range of maturities precisely in the sample that excludes them.

5.3 Beyond Interest Rates

The evidence thus far establishes that dovish political pressure shifts near-term interest rate expectations, with the effect concentrated at the short-end of the yield curve. We now ask whether the same pressure episodes move other financial markets, and what the joint cross-asset response reveals about the transmission channel.

Two mechanisms can rationalise the baseline rate response. The first is a *monetary channel*: pressure is read as news about the near-term path of monetary policy, prompting markets to reprice short-term rate expectations without any implication for central bank credibility. The second is a *credibility channel*: persistent or intense pressure raises the probability, in the eyes of market participants, that the Fed’s independence is compromised, with implications that extend beyond the short end to longer-horizon inflation expectations and to risk premia more broadly. The two channels are observationally equivalent at short maturities—both predict a dovish repricing of FFc1 through FFc4—but they diverge sharply across other instruments. Under the information channel, longer-horizon inflation expectations remain anchored and financial market volatility does not respond; under the credibility channel, long-run inflation compensation rises, implied volatility increases in both equity (VIX) and bond (MOVE) markets, and the dollar depreciates on a broader basis than can be accounted for by the rate shift alone. We examine each of these implications in turn.

Credibility measures. Tables 5 and 4 report the estimated response of two volatility indices (VIX, MOVE) and two inflation compensation measures (2-year and 10-year inflation swap rates) to dovish political pressure, both with and without the Trump presidencies. None of the eight dovish coefficients is statistically distinguishable from zero, and the pattern is informative beyond statistical insignificance.

Table 4: Dovish Political Pressure and Inflation Expectations

Dependent Variables:	2Y Infl. Swap (Δ , pp)		10Y Infl. Swap (Δ , pp)	
	Full	No Trump	Full	No Trump
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Constant	-0.0010 (0.0016)	-0.0006 (0.0020)	-0.0001 (0.0008)	-0.0003 (0.0010)
Dovish pressure (n_{dovish})	-0.0014 (0.0029)	-0.0111 (0.0114)	-0.0005 (0.0015)	-0.0046 (0.0073)
Oil price growth (QoQ, %)	-0.0002*** (7.62×10^{-5})	-0.0002* (0.0001)	-8.38×10^{-5} ** (3.81×10^{-5})	-0.0001** (4.78×10^{-5})
S&P 500 return (QoQ, %)	0.0002 (0.0002)	0.0004 (0.0003)	3.7×10^{-5} (0.0001)	7.69×10^{-5} (0.0001)
NFP announcement	0.0119** (0.0059)	0.0163** (0.0076)	0.0076*** (0.0028)	0.0096*** (0.0035)
Inflation gap	-0.0007 (0.0011)	-0.0009 (0.0012)	-0.0004 (0.0005)	-0.0004 (0.0005)
Unemployment gap	0.0001 (0.0007)	-0.0009 (0.0011)	-0.0005 (0.0004)	-0.0010** (0.0005)
FOMC announcement	-0.0010 (0.0062)	-0.0024 (0.0071)	-0.0002 (0.0033)	0.0025 (0.0042)
Yield skewness (ISK)	0.0105*** (0.0039)	0.0099* (0.0050)	0.0090*** (0.0021)	0.0089*** (0.0023)
<i>Fit statistics</i>				
Observations	4,644	3,419	4,644	3,419
R ²	0.038	0.045	0.016	0.015

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Dependent variables: daily change in 2-year and 10-year USD inflation swap rates (percentage points; $\times 100 =$ basis points). Drivers specification: inflation gap, unemployment gap, QoQ oil price growth (LOCF), QoQ S&P 500 return (LOCF), NFP announcement dummy, FOMC announcement dummy, and treasury yield skewness (ISK). Beginning-of-month dummy and two lags of the dependent variable included but omitted from display. “No Trump” drops 20 Jan 2017–20 Jan 2021 and post-20 Jan 2025. HAC (Newey-West) standard errors in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

The point estimates on inflation compensation are, if anything, mildly *negative* across all four specifications—the opposite of the sign the credibility channel would predict. A deterioration in perceived Fed independence should raise long-run inflation expectations as markets price a higher probability of accommodation beyond what the Fed’s mandate would imply; we see the reverse, with small negative point estimates at both the two- and ten-year horizons, in both the full sample and the subsample excluding the Trump presidencies. The market-implied inflation

anchor is therefore robust to dovish pressure: markets read these episodes as shifting the near-term policy path without revising their assessment of the Fed’s long-run commitment to price stability.

Table 5: Dovish Political Pressure and Financial Volatility

Dependent Variables:	VIX (Δ , pts)		MOVE (Δ , pts)	
	Full (1)	No Trump (2)	Full (3)	No Trump (4)
<i>Variables</i>				
Constant	-0.0152 (0.0188)	-0.0056 (0.0210)	0.1476** (0.0728)	0.1794** (0.0871)
Dovish pressure (n_{dovish})	0.0688 (0.0842)	-0.0041 (0.2466)	-0.2648 (0.2281)	1.219 (0.9104)
Oil price growth (QoQ, %)	0.0009 (0.0010)	4.2×10^{-5} (0.0010)	0.0035 (0.0032)	0.0037 (0.0047)
S&P 500 return (QoQ, %)	0.0046* (0.0024)	0.0028 (0.0025)	0.0066 (0.0098)	0.0049 (0.0110)
NFP announcement	-0.1026 (0.0757)	-0.0661 (0.0749)	-2.778*** (0.3111)	-3.372*** (0.3392)
Inflation gap	0.0197* (0.0119)	0.0227 (0.0140)	0.0739 (0.0533)	0.0713 (0.0564)
Unemployment gap	0.0157 (0.0151)	0.0260* (0.0155)	0.0368 (0.0363)	0.0555 (0.0496)
FOMC announcement	-0.2043* (0.1080)	-0.2519** (0.1284)	-2.782*** (0.3741)	-3.107*** (0.4538)
Yield skewness (ISK)	-0.2688*** (0.1011)	-0.2393*** (0.0721)	-0.6298*** (0.2361)	-0.5861** (0.2940)
<i>Fit statistics</i>				
Observations	5,412	4,187	5,444	4,302
R ²	0.019	0.014	0.046	0.052

*Signif. Codes: ***, 0.01, **, 0.05, *, 0.1*

Dependent variables: first difference of VIX and MOVE index levels (index points). Drivers specification: inflation gap, unemployment gap, QoQ oil price growth (LOCF), QoQ S&P 500 return (LOCF), NFP announcement dummy, FOMC announcement dummy, and treasury yield skewness (ISK). Beginning-of-month dummy and two lags of the dependent variable included but omitted from display. “No Trump” drops 20 Jan 2017–20 Jan 2021 and post-20 Jan 2025. HAC (Newey-West) standard errors in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

The volatility estimates tell a consistent story. Dovish pressure does not generate a systematic increase in implied volatility across equity or bond markets: the coefficients are small in magnitude and inconsistent in sign across the full and no-Trump subsamples, a pattern more consistent with noise around zero than with a repricing of institutional risk. If markets interpreted political pressure as a credibility-undermining event, implied volatility should rise across the board—in equities as investors reprice policy uncertainty, and in bonds as the distribution of future rates widens around its conditional mean. We find no such evidence.²²

²²The inflation swap and volatility regressions are estimated on shorter samples than the federal funds futures regressions (roughly 4,600–5,400 observations versus 9,000), reflecting the later inception of these markets: the VIX began trading in 1990, inflation swaps in the early 2000s, and the MOVE index from 2002. Statistical power is correspondingly lower. The point estimates reported here are, however, economically small relative to the unconditional daily variation in each series: the dovish coefficient in the 2-year inflation swap regression implies a response of less than one-twentieth of a standard deviation per additional pressure article, an order of magnitude smaller than the corresponding FFC1 effect.

Exchange rates. While the credibility channel appears inactive, the short-rate expectations channel still predicts a concrete implication for foreign exchange: under uncovered interest parity, a decline in near-term US rate expectations should—holding foreign rates constant—generate a depreciation of the US dollar. To test this, we re-estimate the baseline with the dependent variable replaced by the daily log return on the Dollar Index (DXY) and the EUR/USD spot rate. Because the EUR/USD is a bilateral exchange rate, identifying the US leg of the response requires conditioning on contemporaneous euro-area monetary developments: a daily movement in EUR/USD reflects both US and euro-area interest rate expectations, and without a euro-leg control the estimated coefficient would conflate the two. We therefore augment the baseline with the daily change in the two-year German Bund yield, $\Delta r_t^{\text{Bund},2Y}$, which absorbs high-frequency movements in euro-area short-rate expectations.²³ Formally, for $y_t \in \{\Delta \log \text{DXY}_t, \Delta \log \text{EUR/USD}_t\}$ we estimate

$$y_t = \alpha + \beta n_{\text{dovish},t} + \phi \Delta r_t^{\text{Bund},2Y} + \sum_{l=1}^2 \delta_l y_{t-l} + \gamma' \mathbf{X}_t + \varepsilon_t, \quad (3)$$

where \mathbf{X}_t collects the baseline controls. Under the UIP prediction, conditioning on the Bund yield isolates the dollar-leg response to US political pressure: $\hat{\beta} < 0$ for the DXY and $\hat{\beta} > 0$ for the EUR/USD would indicate that dovish pressure on the Fed depreciates the dollar relative to a foreign currency whose own short-rate expectations are held fixed by the control.

Table 6 confirms the prediction. Dovish pressure depreciates the dollar on both measures, with the effect stronger and more precisely estimated in the sample excluding the Trump administrations. In the full sample, the EUR/USD appreciates by roughly 4.5 basis points per additional dovish article (significant at 10%) and the DXY declines by 2.9 basis points (not significant); excluding the Trump windows, the effects rise to 11.1 and -7.7 basis points respectively, both significant at 5%.²⁴

²³The Bund is available throughout our 1987–2025 sample, whereas ESTR OIS is available only from 2019 and EONIA OIS from 1999; using a spliced control would introduce a structural break in the euro-leg series that the Bund avoids.

²⁴The full-sample DXY coefficient is less precisely estimated than its EUR/USD counterpart. This is consistent with the composition of the DXY, which weights five non-euro currencies—JPY, GBP, CAD, SEK, and CHF—whose own short-rate expectations are not controlled for in Equation (3). The EUR/USD specification is, by construction, the cleaner test: it is bilateral, and the Bund control isolates the dollar-leg response by absorbing contemporaneous euro-leg movements. The non-Trump subsample, in which pressure events are more isolated and more informative at the margin, is precise enough to deliver significance at 5% on both measures despite the DXY’s noisier construction.

Table 6: Dovish Political Pressure and Spot Exchange Rates

Dependent Variables:	EUR/USD (%)		DXY (%)	
	Full	No Trump	Full	No Trump
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Constant	-0.0046 (0.0082)	-0.0098 (0.0096)	0.0056 (0.0070)	0.0104 (0.0079)
Dovish pressure (n_{dovish})	0.0449* (0.0273)	0.1106** (0.0496)	-0.0260 (0.0231)	-0.0765* (0.0419)
Oil price growth (QoQ, %)	0.0009** (0.0004)	0.0010* (0.0005)	-0.0009*** (0.0003)	-0.0010** (0.0004)
S&P 500 return (QoQ, %)	-0.0015 (0.0009)	-0.0014 (0.0011)	0.0013* (0.0008)	0.0012 (0.0009)
NFP announcement	-0.0244 (0.0357)	-0.0158 (0.0403)	0.0268 (0.0310)	0.0238 (0.0349)
Inflation gap	-0.0102* (0.0058)	-0.0109* (0.0060)	0.0091* (0.0049)	0.0096* (0.0051)
Unemployment gap	0.0010 (0.0047)	-0.0017 (0.0062)	-0.0022 (0.0038)	-0.0002 (0.0049)
FOMC announcement	0.1133*** (0.0434)	0.1242** (0.0483)	-0.1195*** (0.0341)	-0.1308*** (0.0374)
Yield skewness (ISK)	-0.0004 (0.0204)	0.0148 (0.0284)	0.0074 (0.0172)	-0.0057 (0.0237)
Δ Bund 2Y (pp)	0.5096** (0.2225)	0.4902** (0.2317)	-0.1525 (0.2001)	-0.1521 (0.1846)
<i>Fit statistics</i>				
Observations	8,486	7,288	8,477	7,279
R ²	0.005	0.006	0.004	0.005

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Dependent variables: log return \times 100 (percentage points). Drivers specification: inflation gap, unemployment gap, QoQ oil price growth (LOCF), QoQ S&P 500 return (LOCF), NFP announcement dummy, FOMC announcement dummy, treasury yield skewness (ISK), and Δ Bund 2Y (daily change in the German 2-year Bund yield, pp) to partial out euro-area rate news (UIP logic). Beginning-of-month dummy and two lags of the dependent variable included but omitted from display. “No Trump” drops 20 Jan 2017–20 Jan 2021 and post-20 Jan 2025. HAC (Newey-West) standard errors in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

The Bund control is itself quantitatively large and highly significant in the EUR/USD regression—a daily rise of 1 percentage point in the 2Y Bund yield is associated with a 0.51% appreciation of the euro against the dollar, exactly the sign predicted by UIP—validating the identification strategy: the dovish coefficient is estimated on variation in EUR/USD that re-

mains after euro-area short-rate news is absorbed. The non-Trump amplification mirrors the pattern documented for the FFC coefficient in Section 5.2: dovish pressure in the non-Trump sample is less clustered, more informative at the margin, and therefore more fully priced in both the rate and FX margins.

6 Pressure and the Actual Policy Rate

The main results establish that dovish political pressure shifts market-implied rate expectations. A distinct question is whether those shifts are eventually validated by actual Fed policy. Following Bianchi et al. (2023), we construct the ex-post pricing error of federal funds futures:

$$\text{FE}_{jt} = r_{jt} - \text{FEDFUNDS}_{d(j,t)}, \quad (4)$$

where r_{jt} is the FFC j implied rate on day t and $\text{FEDFUNDS}_{d(j,t)}$ is the monthly average effective FFR for the delivery month, the contract’s settlement value. We then estimate, for each maturity j ,

$$\Delta|\text{FE}|_{jt} = \gamma n_{\text{dovish},t} + \beta' \mathbf{X}_t, \quad (5)$$

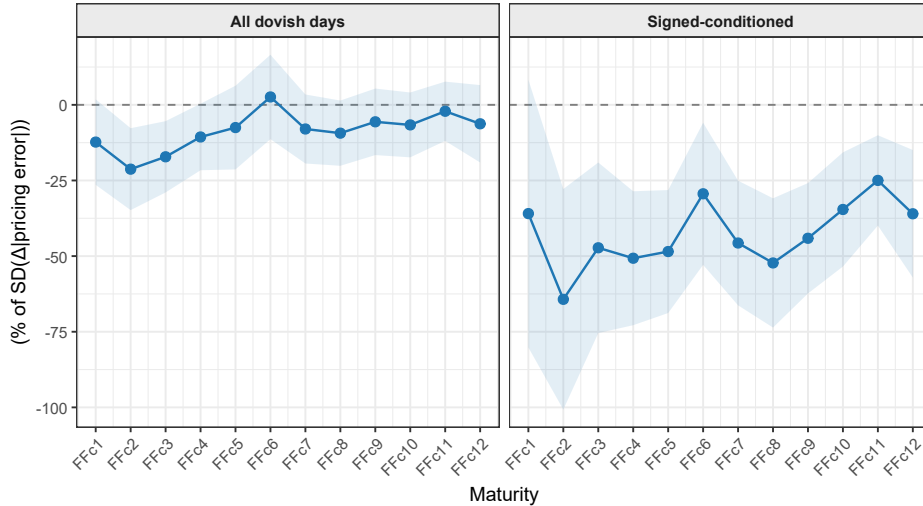
where $\Delta|\text{FE}|_{jt} = |\text{FE}_{jt}| - |\text{FE}_{j,t-1}|$ and \mathbf{X}_t contains the same controls of Section 5. $\gamma < 0$ implies that futures moved closer to the eventual policy outturn on days with more dovish pressure — consistent with the Fed eventually moving in the direction of political pressure. Standard errors are clustered by date.

We estimate two variants that answer distinct economic questions. The *unconditional* specification uses $n_{\text{dovish},t}$ on all days and asks: when politicians press for easier policy, does the Fed eventually cut by more than macro conditions alone would predict? The *signed-conditioned* specification asks, instead, a narrower question: among days when political pressure is accompanied by a market reaction — that is, when investors respond by pricing in a higher probability of a rate cut ($\Delta r_{jt} < 0$) — does the Fed then follow through with actual policy easing? Formally, we replace $n_{\text{dovish},t}$ with $n_{\text{dovish},t}^{\text{eff}} \equiv n_{\text{dovish},t} \cdot \mathbf{1}[\Delta r_{jt} < 0]$. The two estimands thus differ in their implicit model of the transmission mechanism. A negative $\hat{\gamma}$ in the unconditional specification is consistent with a direct channel from political pressure to Fed behaviour. A negative $\hat{\gamma}$ in the signed-conditioned specification additionally requires that the Fed respond to the market signal generated by political pressure, rather than to the political statement itself — a pattern related to the market-dependent monetary policy in the spirit of Cieslak and Vissing-Jorgensen (2021),

whereby the Fed incorporates financial market information into its policy deliberations.

Figure 9 reports the results.

Figure 9: Pressure and Changes in Forecasting Error



Note: The figure plots $\hat{\gamma}$ from equation (5) across maturities FFc1–FFc12, normalised by the within-maturity standard deviation of $\Delta|FE|_{jt}$. The left panel uses $n_{\text{dovish},t}$ on all days (unconditional); the right panel uses $n_{\text{dovish},t}^{\text{eff}} \equiv n_{\text{dovish},t} \cdot \mathbf{1}[\Delta r_{jt} < 0]$, which retains the pressure count only on days when the futures rate also fell (signed-conditioned). Controls follow the drivers specification in Section 5. Standard errors are clustered by date; shaded bands denote 95% confidence intervals.

The unconditional estimates are negative and significant across all maturities, amounting to 0.5–0.8 basis points per additional dovish article and flat across the maturity spectrum — suggesting that political pressure shifts the entire expected rate path, not just the near-term horizon. Normalised by the within-maturity standard deviation of $\Delta|FE|_{jt}$, this corresponds to 15–25% of a typical daily fluctuation in the pricing error. The signed-conditioned estimates are roughly three times larger, reaching 35–65% of the daily standard deviation. The three-fold amplification between the two specifications implies that the observable convergence between market expectations and eventual policy is concentrated on days when political pressure and market co-movement coincide, consistent with the Fed responding to the market signal generated by pressure rather than to the political statement itself.

Two caveats apply. First, politicians tend to call for rate cuts precisely when the economy is weak — the same conditions that independently lead the Fed to ease. Even with our extensive set of controls, some of the estimated $\hat{\gamma}$ may reflect this common driver rather than a causal effect of pressure on Fed behaviour; the results should be read as suggestive evidence of accom-

modation rather than as a causal claim. Second, the signed-conditioned estimand selects days on which the market reacted to political pressure by pricing in a higher probability of a cut. Since the main results show that $n_{\text{dovish},t}$ itself moves futures prices, this conditioning tends to retain the largest and most market-relevant pressure events, occurring in environments already tilted toward easing. The signed-conditioned estimate therefore captures the upper end of the distribution of pressure episodes rather than the average effect.

7 Robustness

This section subjects the baseline estimates of Figure 7 to two sets of robustness checks. The motivation is that the daily frequency, while unusually high by macroeconomic standards, remains coarse relative to the speed at which financial markets incorporate information: a trading day contains scheduled macro releases, FOMC communication, and non-political news, any of which may co-move with political pressure and contaminate the baseline coefficient. First, we augment the control set to reduce the scope for omitted variables that simultaneously drive interest rate expectations and political pressure. Second, we implement a placebo test that replaces the dovish pressure count with the daily count of articles the classifier flagged as mentioning the Federal Reserve without exerting directional pressure—a null result would indicate that the baseline estimate reflects the directional pressure signal rather than mechanical features of the classification pipeline or of Fed-related news flow more broadly.

7.1 Alternative Specifications and Samples

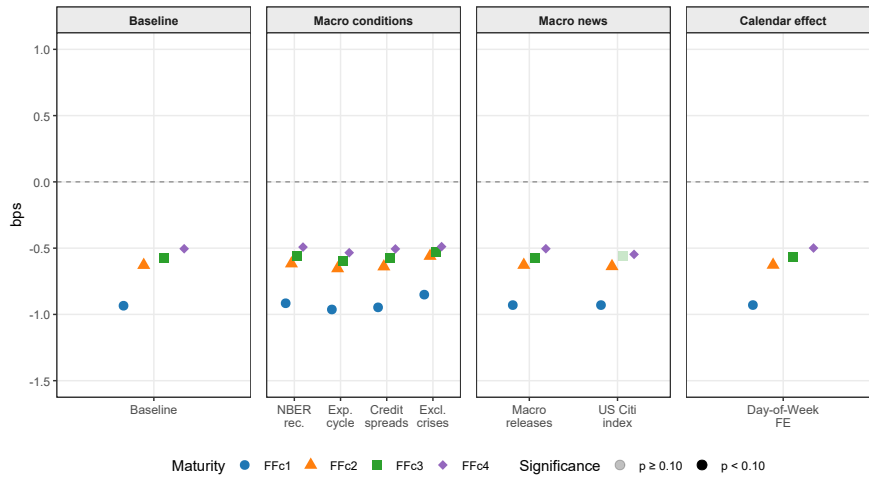
We begin by assessing the robustness of the results to alternative sets of control variables. The baseline specification (equation 2) includes a beginning-of-month dummy, an FOMC announcement-day dummy, and a set of controls capturing the macro-financial environment, following Bauer and Swanson (2023).

We augment the control set along three dimensions, each addressing a distinct channel through which political pressure and interest rate expectations may be jointly determined. The first is the macroeconomic state. Political pressure intensifies in recessions, in credit-market stress, and in financial crises. We therefore add, in turn, an NBER recession indicator, substitute the inflation gap with a forward-looking indicator, introduce a daily corporate credit spread, and re-estimate the baseline excluding crises windows. The second is the flow of macroeconomic news. Scheduled data releases move rates for reasons unrelated to political pressure, and if the

two are correlated in time, the baseline coefficient would partly absorb the release response. We address this directly by conditioning on the most important release-day indicators- CPI, advance GDP, PPI, and retail sales- and, in a separate specification, on the daily US Citi Economic Surprise Index (CESI)—a continuous measure of how macro data has been landing relative to consensus.²⁵ The third is systematic day-of-week variation in financial returns, which we absorb with day-of-week fixed effects.

The baseline finding is robust to the choice of the different control sets. Figure 10 report the impact of a political pressure event across the baseline specification and the seven aforementioned ones for futures up to three-months ahead. The point estimates are remarkably stable and significant at the 10% level for every maturity-control pair but one.

Figure 10: Stability of the Political Pressure Effect Across Alternative Specifications



Note: Each point is the effect of political pressure on federal funds futures returns (in basis points) for contracts up to three-months ahead. The baseline specification is equation 2. Robustness variants are grouped by the potential concern they address. *Macro conditions*: (i) adds the NBER recession indicator; (ii) replaces the inflation gap with the one-year Michigan Survey expected inflation gap; (iii) adds the daily change in the BAA–AAA corporate bond spread; (iv) excludes calendar years 2009 (Global Financial Crisis) and 2020 (Covid-19). *Macro news*: (v) adds announcement dummies for CPI, advance GDP, PPI, and retail sales releases from ALFRED; (vi) replaces the NFP releases dummy with the first difference of the US Citigroup Economic Surprise Index. *Calendar*: (vii) adds day-of-week fixed effects. HAC (Newey-West) standard errors.

The tightness of this range, across specifications that differ materially in the confounds they absorb, is the central robustness finding: if the baseline coefficient reflected an omitted macroeconomic factor, it should load into at least one of these controls and move accordingly,

²⁵The CESI specification, which controls most directly for real-time data surprises, is available only from January 2005, shortening the estimation sample by roughly twenty years relative to the full 1987–2025 baseline. We addressed this by replacing the Citigroup Economic Surprise Index with direct dummies for the five major scheduled US macroeconomic releases.

however it does not.

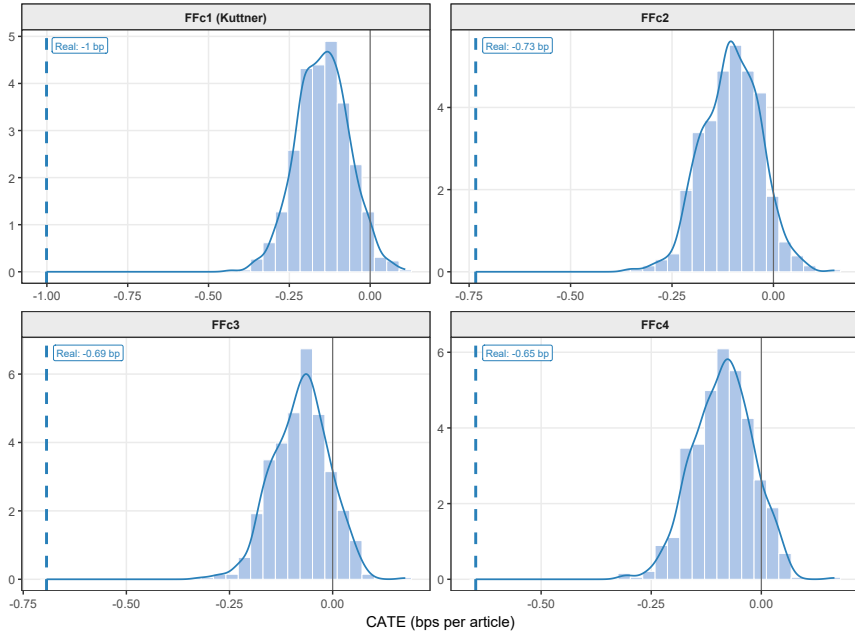
7.2 Placebo Test

A further identification concern is that our variable of interest, $n_{\text{dovish},t}$ is a count of news articles, and the coefficient could in principle load on Fed-related news attention generally rather than on directional pressure specifically—for instance, if markets respond to heightened political scrutiny of the Federal Reserve regardless of the scrutiny’s content. To address this concern, we construct a within-sample placebo using the pool of articles that passed the politician-and-Fed filter—a US elected official talking about the Federal Reserve—but that the LLM classified as not containing directional political pressure. This pool shares the upstream selection process with the treatment series (the same politicians, the same Fed mentions, the same newswire, the same daily arrival rate) and differs from it only in the classifier’s judgment that the article carries no pressure signal. A null coefficient on this placebo would indicate that the baseline estimate reflects the directional content captured by the classifier rather than mechanical features of Fed-related news flow.

Formally, we draw 1,000 placebo samples from the pool of non-pressure articles. Each draw samples, without replacement, a number of articles equal to 678, the total number of days in the sample on which at least one dovish pressure article appears; aggregates the sampled articles to daily counts; and re-estimates the baseline specification for FFC1 through FFC4 using this placebo count in place of $n_{\text{dovish},t}$. The procedure generates a distribution of 1,000 placebo coefficients under the null that the baseline estimate reflects only the volume of Fed-related news flow rather than its directional content. Figure 11 plots this distribution for each outcome alongside the baseline coefficient, shown as a vertical line.

The placebo distribution is tightly centred around zero, consistent with the null hypothesis that randomly selected Fed-related political articles have no systematic effect on rate expectations. The baseline coefficient lies in the extreme left tail of this distribution— corresponding to a one-sided p -value of 0.001. The baseline effect is therefore not a mechanical artefact of article volume or of Fed-related news coverage more broadly; it is specific to articles the classifier identifies as carrying dovish pressure content, which in turn validates the directional content of the classification pipeline.

Figure 11: Placebo Test: Is the Content of Political Pressure Articles Informative?



Note: Each panel shows the distribution of the coefficient on a placebo pressure variable across 678 simulation runs. In each run, a number of articles equal to the actual count of dovish pressure events is drawn at random from the pool of articles classified as NOT_RELEVANT by the LLM majority vote; these are aggregated to daily counts and entered in place of $n_{\text{dovish},t}$ in equation 2. The dashed vertical line marks the real coefficient from the same specification.

8 Conclusion

This paper examines whether politically motivated pressure on the Federal Reserve by elected officials shifts near-term interest rate expectations as priced in financial markets. Using approximately forty years of Reuters newswire data from 1988 to 2025 and a majority-vote LLM classification pipeline to create a new political pressure measure, we document a robust negative association between dovish pressure and futures rates: the response of current-month futures to an additional dovish event is roughly 1bps coefficients, equivalent to a 4% additional probability of a rate cut in the next FOMC. The effect is statistically significant up to the 3-months ahead tenor, but decreases monotonically and disappears at the medium and long-term horizon. Hawkish pressure yields positive but uniformly insignificant coefficients across the same specifications.

A natural concern is that the estimated association reflects the unusually salient and sustained pressure campaigns of the two Trump administrations rather than a structural feature of the pressure–markets relationship. The evidence does not support this reading. Re-estimating the

baseline specification on the subsample that excludes the windows of 20 January 2017 to 20 January 2021 and 20 January 2025 onward yields an effect on the current-month futures of 1.58 bps, greater than the full-sample specification. The monotonic relationship also remains intact. We interpret this pattern as evidence that the mechanism is a general feature of the post-Volcker US monetary framework rather than an artefact of an idiosyncratic episode.

The joint pattern across instruments—short-end rally, muted long-end response, stable inflation compensation and market volatility, and dollar depreciation—is consistent with markets pricing anticipated monetary accommodation rather than a credibility tax in the sense of Rogoff (1985) and Alesina and Summers (1993), which would require the nominal anchor to move. Market participants appear to price the presidential principal–agent relationship specifically—consistent with the President’s unique hold on the appointment and reappointment channel over the Chair and Board of Governors—rather than a diffuse political-pressure effect operating through rhetoric at large. The absence of a congressional response is consistent with Binder (2018), who documents the limited traction of legislative oversight on the conduct of monetary policy.

Three caveats qualify the interpretation. First, the reduced-form coefficient cannot separate completely a compliance channel, through which markets update about the Fed’s reaction function, from an information channel, through which pressure co-occurs with a deteriorating macroeconomic outlook that would be priced independently. We address this to the extent that daily identification permits—nevertheless we acknowledge that a residual information component cannot be ruled out within a reduced-form design, and flag its clean identification as an open question. Second, the LLM classification is imperfect; the resulting measurement error biases estimates toward zero, so the reported coefficients should be read as a lower bound on the true market response. Human annotation of a stratified subsample would permit a formal correction. Third, the regressor counts Reuters articles classified as exerting pressure, and therefore reflects both the underlying intensity of political pressure and the newswire’s editorial decision to cover it. This is a feature shared with essentially all newspaper-based index constructions in the macro-finance literature—from the Baker et al. (2016) policy uncertainty index onward—and one the literature has generally treated as a second-order concern relative to the informational content that media coverage conveys.

Three directions follow naturally. First, the methodology is portable: the same LLM classification pipeline, applied to newswire corpora for the ECB, Bank of England, and Bank of Japan, would test whether anticipated accommodation is a general feature of advanced-economy cen-

tral banking or specific to the US institutional framework. Second, a natural next step is to ask whether the realised policy path tracks the expectational shift documented here. We document that federal funds futures track the settlement rate more tightly around pressure events than on average, on the order of 25 percent of one standard deviation, consistent with markets correctly anticipating the subsequent path. A sharper and more complete answer is nonetheless warranted. Third, and conditional on compliance, the estimates invite a welfare accounting. Does sustained pressure move realised inflation above target by a quantitatively meaningful margin, and at what cost in terms of output variability? That is the question that decides whether political pressure on the Federal Reserve is a first-order concern for monetary policy or a second-order feature of the communication environment.

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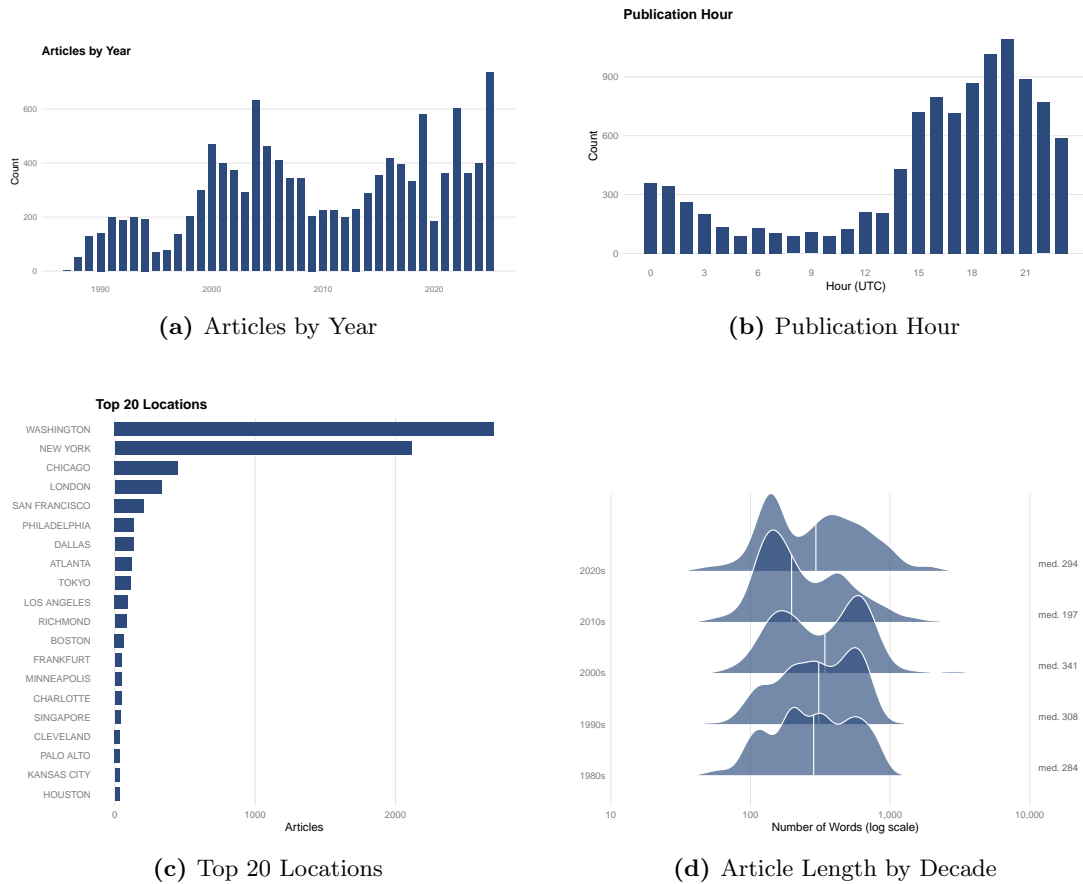
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Appendix

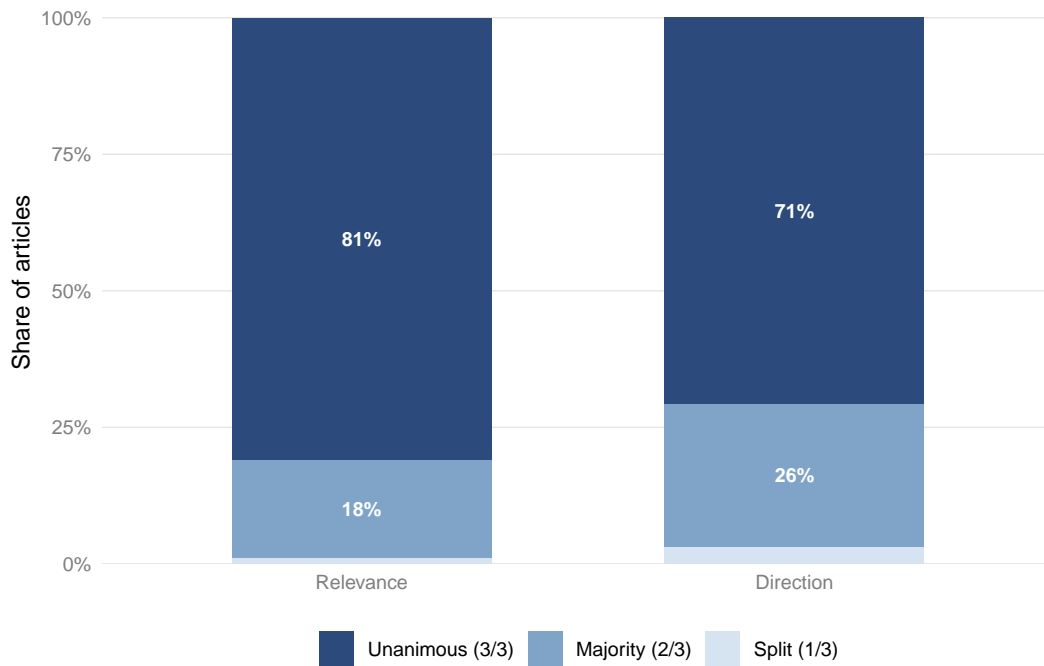
A Corpus and Political Pressure Measurement

Figure 12: Reuters Corpus: Descriptive Statistics



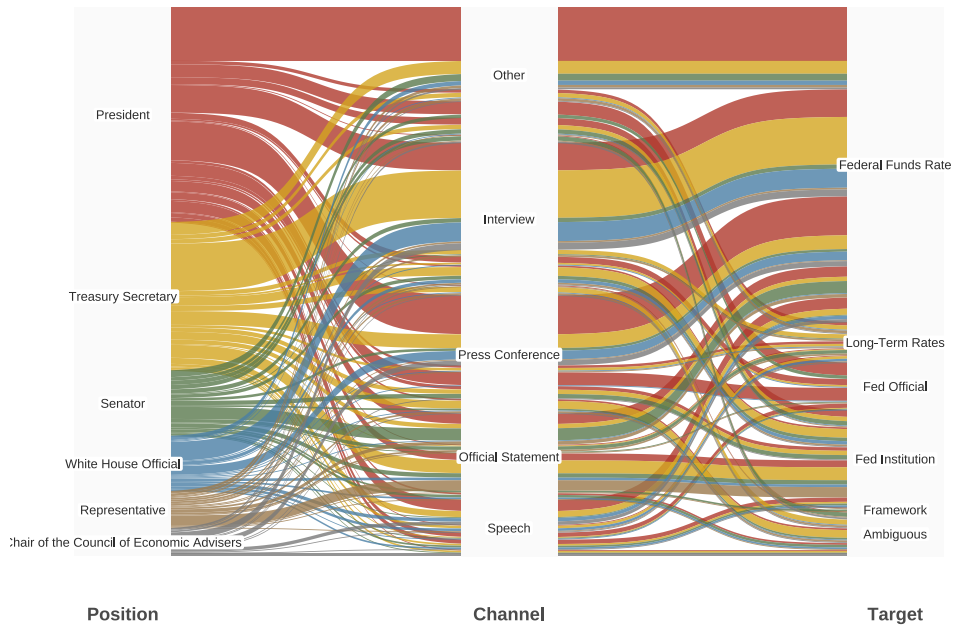
Note: The figure reports descriptive statistics for the raw Reuters corpus retrieved from Factiva over the period 1988–2025. Panel (a) shows the number of articles per year. Panel (b) shows the distribution of publication hours reported in the header. Panel (c) reports the top 20 locations reported in the header. Panel (d) shows the evolution of article length by decade with the median number words noted on the right.

Figure 13: Inter-model agreement in the LLM classification ensemble



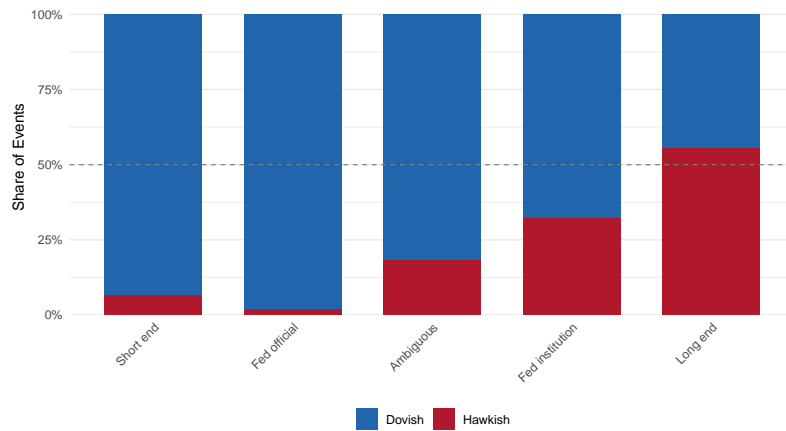
Note: The figure reports the degree of agreement across the three open-weight LLMs used in the classification ensemble—Qwen (qwen3-235b-a22b-2507), Meta Llama (llama-4-maverick), and DeepSeek (deepseek-chat-v3-0324)—shown separately for the relevance judgment (left, whether an article contains a US politician talking about the FED) and the direction judgment (right, whether the event is classified as dovish, hawkish, or not relevant). *Unanimous* denotes articles on which all three models agree; *Majority* denotes articles on which exactly two of three agree; *Split* denotes articles on which no majority is available, either because one of the three models returned a malformed response or because all three models assign a different label.

Figure 14: Characterizing pressure: from who to whom using what



Note: The diagram traces each political pressure event along three dimensions: the political actor's institutional role (left), the communication channel through which the statement was made (centre), and the target of the statement (right). Flows are coloured by actor role. The six most frequent actor roles are shown individually; remaining roles are excluded.

Figure 15: Direction of Political Pressure by Target



Note: The figure reports the share of dovish and hawkish events within each target category. *Short end* denotes pressure aimed at the near-term policy rate; *Fed official* denotes pressure directed at a specific person on FOMC Board; *Fed institution* denotes pressure aimed at the Federal Reserve as an institution, including its mandate, or structure; *Long end* denotes pressure aimed at the longer-run policy stance or long-term interest rates; *Ambiguous* groups articles for which the target could not be determined with confidence.

B LLM Classification Prompts

Prompt 1 — Event Deduplication

You are an expert system for classifying whether news articles describe the same real-world event.

Your task:

For each pair of articles below, determine if they refer to the SAME underlying event or to DISTINCT events.

Definition of "same event":

- Both articles describe the same real-world occurrence.
- The actors, action, and timing match.
- One article is an update or expanded version of the other.
- Both texts reference the same speech, interview, press release, announcement, meeting, data release, incident, or physical occurrence.
- Differences in wording, detail, or angle do not matter if the underlying event is the same.

Definition of "different event":

- The actions occurred at different times.
- They describe different statements, decisions, speeches, or occurrences -- even if the topic is related.
- Commentary, reaction, or analysis triggered by another event counts as a separate event.
- Similar topic or subject matter does NOT imply the same event.

```

Comparative criteria -- evaluate alignment of:
- WHO acted or spoke
- WHAT was said or done
- WHEN the event occurred (explicitly or implicitly)
- WHERE it happened (if relevant)
- Whether one article directly updates or refers to the same occurrence

Rules:
1. Ignore generic topic similarity.
2. Focus strictly on whether both texts describe the same specific
   occurrence in reality.
3. Compare ALL pairs of articles ([N] comparisons needed for [N] articles).
4. If there is insufficient evidence that both articles describe the same
   specific real-world occurrence, classify as "different_event".
5. Multiple actions or statements by the same actor on different dates or
   occasions are separate events unless explicitly described as the same
   occurrence.

ARTICLES (doc_id: [doc_id]):
[formatted article texts with separators]

Return ONLY a JSON array with one object per pair:
[
  {
    "doc_id": "[doc_id]",
    "group_seq": 1,
    "article_1": 1,
    "article_2": 2,
    "classification": "same_event" or "different_event"
  },
  ...
]

Return ONLY the JSON array, no other text.

```

Prompt 2 — Relevance Classification

```

You are assisting me in analyzing news articles about US politicians and
the Federal Reserve.

## Definitions ##

A "politician commenting on Fed policy" means a US politician (President,
Vice President, Senator, Representative, Treasury Secretary, cabinet member,
White House official, or presidential candidate) makes a statement --
directly quoted or paraphrased -- about:
- The Federal Reserve or Fed policy
- Fed officials (Chair, Vice Chair, Governors, regional bank presidents)
- Interest rates or monetary policy
- Quantitative easing or the Fed balance sheet

## What DOES count ##
- Direct quotes from politicians about the Fed
- Paraphrased statements attributed to politicians about Fed policy
- Reported remarks from speeches, interviews, tweets, press conferences

## What does NOT count ##

```

- Journalist analysis or opinion about Fed-politician relations
- Articles that mention both a politician and the Fed without the politician commenting
- Historical background or context about past Fed decisions
- Fed officials commenting on politicians (reverse direction)
- General economic commentary not specifically about Fed policy
- Foreign politicians commenting on US monetary policy
- Private sector economists or analysts commenting

Response Instructions

CRITICAL: Do NOT explain your reasoning. Do NOT use numbered steps.

Output ONLY these two blocks with NOTHING before or after:

<ANALYSIS>...</ANALYSIS>

<JSON>...</JSON>

Part 1: <ANALYSIS> (100 words max)

- Is a politician quoted or paraphrased making a statement about the Fed?
- If so: Who is the politician? What is the target? Through what channel?
- If not: Why does this article not qualify?

Part 2: <JSON>

```
{
  "unique_article_id": "[unique_article_id]",
  "politician_comments_on_fed": 0 or 1,
  "politician_name": "string or null",
  "politician_position": "string or null",
  "statement_type": "direct_quote" or "paraphrased" or "none",
  "communication_channel": "speech" or "press_conference" or "interview"
                          or "tweet" or "congressional_hearing"
                          or "official_statement" or "other" or "unknown",
  "target_type": "fed_official" or "fed_institution" or "policy" or "none",
  "target_name": "string or null"
}
```

Article to analyze

[article_text]

Return ONLY the <ANALYSIS> and <JSON> blocks.

Prompt 3 — Direction Classification

You are classifying the DIRECTION of political pressure on Federal Reserve monetary policy.

Context

This article has been identified as containing a US politician commenting on the Federal Reserve.

Politician identified: [politician_name]

Definitions

DOVISH -- The politician wants EASIER monetary policy:

- Lower interest rates / rate cuts
- More economic stimulus / looser monetary conditions
- Concerns about: jobs, growth, recession, credit availability, strong dollar hurting exports

- Criticism that current policy is "too tight" or rates are "too high"

HAWKISH -- The politician wants TIGHTER monetary policy:

- Higher interest rates / rate hikes
- Fighting inflation / tighter monetary conditions
- Concerns about: inflation, rising prices, currency debasement, asset bubbles
- Criticism that current policy is "too loose" or rates are "too low"

NOT_RELEVANT -- No pressure on monetary policy direction:

- General economic commentary without Fed pressure
- Factual reporting without politician advocacy
- Discussion of Fed appointments/personnel without policy implications
- Praise for Fed actions without directional preference

Classification Rules

1. Explicit statements override context: if the politician explicitly mentions rates or policy direction, use that.
2. Use context for implicit pressure: if the politician criticizes the Fed without stating a direction, infer from the economic context.
3. Default logic when context is unclear:
 - Criticism of rate HIKES -> dovish
 - Criticism of rate CUTS -> hawkish
 - Criticism during HIGH INFLATION -> hawkish
 - Criticism during RECESSION/HIGH UNEMPLOYMENT -> dovish
 - Calls for "action" during inflation -> hawkish
 - Calls for "action" during recession -> dovish
4. Personnel attacks without policy content: use economic context to infer direction; if truly impossible to determine, classify as not_relevant.

Response Format

Part 1: <ANALYSIS> (100 words max)

- What is the politician's statement?
- Is the direction explicit or inferred from context?
- What economic context helped determine direction (if applicable)?

Part 2: <JSON>

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{  
  "unique_article_id": "[unique_article_id]",  
  "pressure_direction": "dovish" or "hawkish" or "not_relevant"  
}
```

Article to analyze

[article_text]

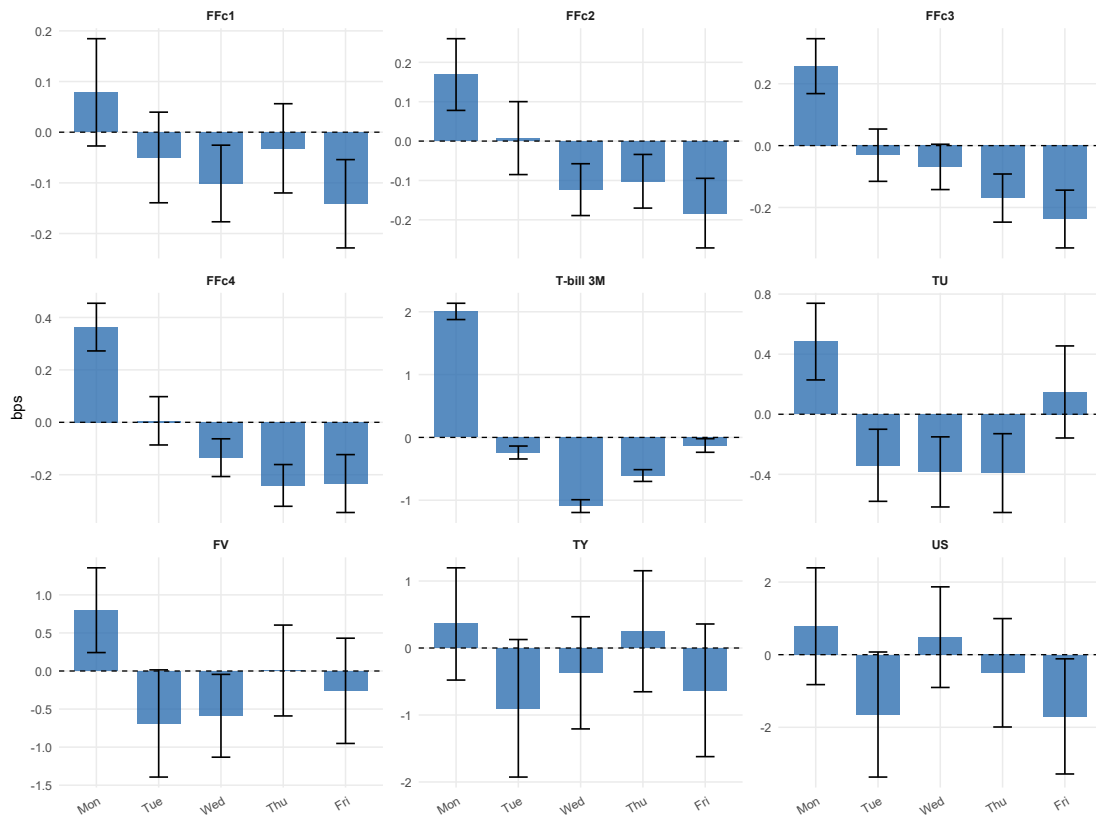
C Additional Data Statistics

Table 7: Summary Statistics

Variable	Unit	N	Mean	SD	Min	Max
<i>Pressure measure</i>						
Dovish pressure (n_{dovish})	count	9,361	0.054	0.262	0.000	4.000
Hawkish pressure (n_{hawkish})	count	9,361	0.008	0.090	0.000	3.000
<i>Panel A: FF futures</i>						
FFc1 (Kuttner surprise)	bps	9,338	-0.043	4.963	-98.500	86.800
FFc2 rate change	bps	9,352	-0.050	3.539	-52.000	63.000
FFc3 rate change	bps	9,349	-0.055	3.624	-52.000	46.500
FFc4 rate change	bps	9,348	-0.055	3.893	-52.500	40.000
<i>Panel B: Treasury futures</i>						
TU (2Y Treasury)	bps	8,882	0.151	9.780	-33.323	29.947
FV (5Y Treasury)	bps	9,346	0.281	24.244	-75.332	67.437
TY (10Y Treasury)	bps	9,354	0.404	36.425	-107.479	99.110
US (30Y Treasury)	bps	9,352	0.702	60.319	-165.595	158.369
<i>Panel C: Volatility</i>						
VIX (CBOE)	pts	5,441	-0.000	1.346	-12.075	19.200
MOVE Index	pts	5,616	-0.021	4.315	-40.500	41.500
<i>Panel D: FX & commodities</i>						
DXY (USD Index)	%	9,353	0.000	0.505	-3.056	2.853
EUR/USD	%	9,358	0.001	0.607	-3.669	3.719
<i>Panel E: Inflation expectations</i>						
2Y inflation swap	bps	4,651	-0.000	7.601	-153.100	121.000
10Y inflation swap	bps	4,651	-0.009	4.225	-63.000	29.000

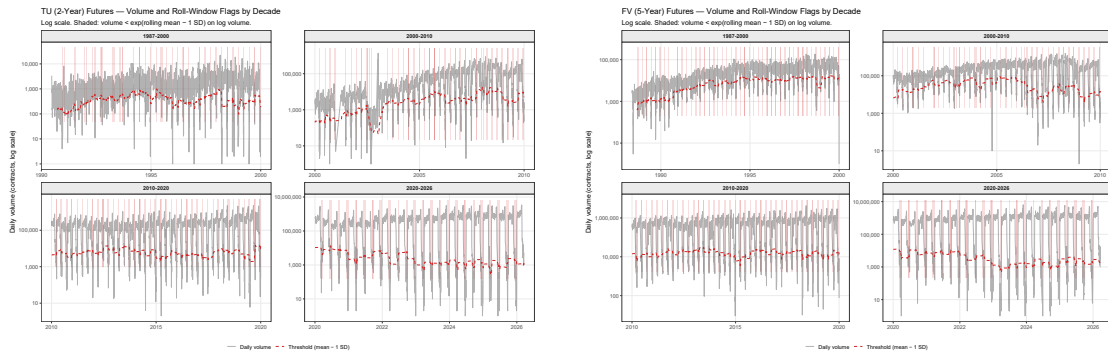
Notes: FFc1 is the Kuttner (2001) surprise scaled to basis points; its extreme values (-98.5 and $+86.8$ bps) correspond to the 2008 crisis and COVID-19 emergency cuts. FFc2–FFc4 are daily changes in implied Fed Funds rates ($\text{pp} \times 100 = \text{bps}$). TU, FV, TY, and US are daily price returns on 2Y, 5Y, 10Y, and 30Y Treasury futures ($\text{price return} \times 100 = \text{bps}$), winsorised at the 1st and 99th percentiles to remove contract-roll and data-error artifacts. FX and commodity series are daily log returns ($\times 100, \%$). Inflation swap rates are daily first differences ($\times 100 = \text{bps}$); coverage begins in 2004, yielding approximately half the full sample. VIX and MOVE are daily first differences (index points); both series begin in the mid-1990s, accounting for their lower observation counts. n_{dovish} and n_{hawkish} are daily counts of classified news events.

Figure 16: Dependent variables and day-of-week seasonality



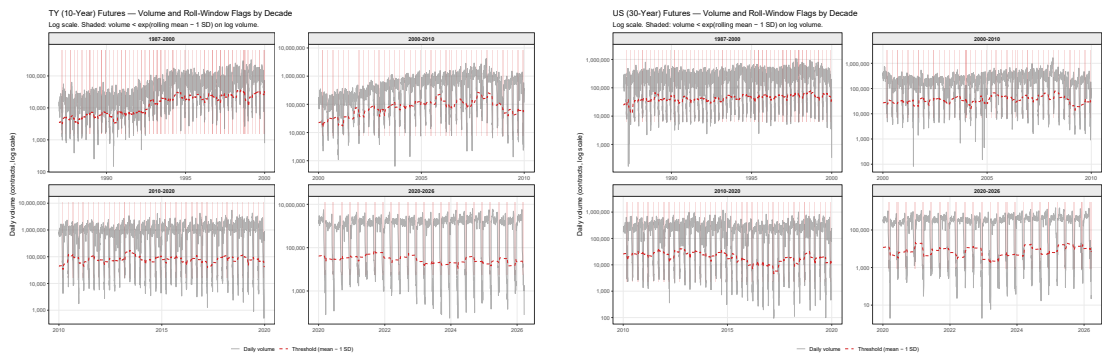
Note:

Figure 17: Treasury Futures: Rolling Volume by Decade



(a) 2-Year (TU)

(b) 5-Year (FV)



(c) 10-Year (TY)

(d) 30-Year (US)

Note: Each panel shows the distribution of daily log-volume for the corresponding Treasury futures contract, disaggregated by decade. Observations flagged as low-volume near contract expiry and excluded from the analysis are shown in a lighter shade.

D Additional Results

D.1 Hawkish Pressure

Table 8: Hawkish Political Pressure and Fed Funds Futures

Dependent Variables:	FFc1 (Kuttner)	FFc2	FFc3	FFc4
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Constant	-0.0006 (0.0006)	-0.0010** (0.0004)	-0.0010** (0.0005)	-0.0010** (0.0005)
Hawkish pressure (n_{hawkish})	0.0064 (0.0056)	0.0085 (0.0052)	0.0103 (0.0070)	0.0049 (0.0049)
Oil price growth (QoQ, %)	-2.04×10^{-5} (2.56×10^{-5})	-2.89×10^{-5} (2.35×10^{-5})	-3.95×10^{-5} (2.48×10^{-5})	-4.43×10^{-5} (2.33×10^{-5})
S&P 500 return (QoQ, %)	0.0001* (7.18×10^{-5})	4.73×10^{-5} (5.8×10^{-5})	4.53×10^{-5} (5.99×10^{-5})	4.13×10^{-5} (5.81×10^{-5})
NFP announcement	-0.0090*** (0.0033)	-0.0055** (0.0023)	-0.0066** (0.0026)	-0.0050* (0.0029)
Inflation gap	0.0010** (0.0005)	0.0007* (0.0004)	0.0007 (0.0005)	0.0008 (0.0005)
Unemployment gap	-0.0009** (0.0004)	-0.0006*** (0.0002)	-0.0006*** (0.0003)	-0.0007*** (0.0003)
FOMC announcement	-0.0113** (0.0050)	-0.0120*** (0.0040)	-0.0125*** (0.0042)	-0.0130*** (0.0039)
Yield skewness (ISK)	0.0057*** (0.0019)	0.0065*** (0.0018)	0.0078*** (0.0017)	0.0090*** (0.0018)
<i>Fit statistics</i>				
Observations	9,004	9,019	9,015	9,015
R ²	0.014	0.031	0.038	0.031

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

All columns use the full sample. Coefficients on FF futures variables multiplied by 100 are expressed in basis points. All regressions include: beginning-of-month dummy and two lags of the dependent variable (omitted from display). HAC (Newey-West) standard errors in parentheses. *, **, *** denote significance at the 10%, 5%, 1% level.

Table 9: Hawkish Political Pressure and Exchange Rates

Dependent Variables:	EUR/USD (%)		DXY (%)	
	Full	No Trump	Full	No Trump
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Constant	-0.0024 (0.0082)	-0.0068 (0.0096)	0.0043 (0.0068)	0.0083 (0.0079)
Hawkish pressure (n_{hawkish})	0.0109 (0.0713)	-0.0770 (0.0761)	-0.0028 (0.0607)	0.0519 (0.0639)
Oil price growth (QoQ, %)	0.0009** (0.0004)	0.0009* (0.0005)	-0.0009*** (0.0003)	-0.0009** (0.0004)
S&P 500 return (QoQ, %)	-0.0014 (0.0009)	-0.0015 (0.0011)	0.0012 (0.0008)	0.0012 (0.0009)
NFP announcement	-0.0216 (0.0357)	-0.0123 (0.0404)	0.0251 (0.0309)	0.0214 (0.0348)
Inflation gap	-0.0102* (0.0058)	-0.0099 (0.0060)	0.0090* (0.0049)	0.0089* (0.0052)
Unemployment gap	0.0010 (0.0047)	-0.0013 (0.0062)	-0.0022 (0.0038)	-0.0005 (0.0049)
FOMC announcement	0.1181*** (0.0433)	0.1322*** (0.0482)	-0.1223*** (0.0341)	-0.1364*** (0.0375)
Yield skewness (ISK)	-0.0051 (0.0202)	0.0138 (0.0285)	0.0102 (0.0171)	-0.0050 (0.0238)
Δ Bund 2Y (pp)	0.5074** (0.2225)	0.4817** (0.2319)	-0.1513 (0.2000)	-0.1465 (0.1808)
<i>Fit statistics</i>				
Observations	8,486	7,288	8,477	7,279
R ²	0.004	0.005	0.004	0.005

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent variables: log return \times 100 (percentage points). Drivers specification: inflation gap, unemployment gap, QoQ oil price growth (LOCF), QoQ S&P 500 return (LOCF), NFP announcement dummy, FOMC announcement dummy, treasury yield skewness (ISK), and Δ Bund 2Y (daily change in the German 2-year Bund yield, pp) to partial out euro-area rate news (UIP logic). Beginning-of-month dummy and two lags of the dependent variable included but omitted from display. “No Trump” drops 20 Jan 2017–20 Jan 2021 and post-20 Jan 2025. HAC (Newey-West) standard errors in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

Table 10: Hawkish Political Pressure and Inflation Expectations

Dependent Variables:	2Y Infl. Swap (Δ , pp)		10Y Infl. Swap (Δ , pp)	
	Full	No Trump	Full	No Trump
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Constant	-0.0009 (0.0016)	-0.0005 (0.0020)	-7.09×10^{-5} (0.0008)	-0.0003 (0.0010)
Hawkish pressure (n_{hawkish})	-0.0189 (0.0129)	-0.0291* (0.0154)	-0.0062 (0.0068)	-0.0092 (0.0080)
Oil price growth (QoQ, %)	-0.0002*** (7.62×10^{-5})	-0.0002* (0.0001)	-8.28×10^{-5} ** (3.81×10^{-5})	-0.0001** (4.81×10^{-5})
S&P 500 return (QoQ, %)	0.0002 (0.0002)	0.0003 (0.0003)	3.39×10^{-5} (0.0001)	7.42×10^{-5} (0.0001)
NFP announcement	0.0118** (0.0059)	0.0162** (0.0076)	0.0075*** (0.0028)	0.0095*** (0.0035)
Inflation gap	-0.0005 (0.0011)	-0.0007 (0.0012)	-0.0004 (0.0005)	-0.0004 (0.0005)
Unemployment gap	0.0001 (0.0007)	-0.0009 (0.0011)	-0.0005 (0.0004)	-0.0009** (0.0005)
FOMC announcement	-0.0013 (0.0062)	-0.0031 (0.0073)	-0.0003 (0.0033)	0.0022 (0.0042)
Yield skewness (ISK)	0.0106*** (0.0039)	0.0100** (0.0051)	0.0090*** (0.0021)	0.0090*** (0.0023)
<i>Fit statistics</i>				
Observations	4,644	3,419	4,644	3,419
R ²	0.038	0.046	0.016	0.015

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent variables: daily change in 2-year and 10-year USD inflation swap rates (percentage points; $\times 100 =$ basis points). Drivers specification: inflation gap, unemployment gap, QoQ oil price growth (LOCF), QoQ S&P 500 return (LOCF), NFP announcement dummy, FOMC announcement dummy, and treasury yield skewness (ISK). Beginning-of-month dummy and two lags of the dependent variable included but omitted from display. “No Trump” drops 20 Jan 2017–20 Jan 2021 and post-20 Jan 2025. HAC (Newey-West) standard errors in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

Table 11: Hawkish Political Pressure and Financial Volatility

Dependent Variables:	VIX (Δ , pts)		MOVE (Δ , pts)	
	Full	No Trump	Full	No Trump
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Constant	-0.0114 (0.0193)	-0.0045 (0.0213)	0.1399* (0.0719)	0.1976** (0.0876)
Hawkish pressure (n_{hawkish})	-0.0167 (0.1922)	-0.1968 (0.1894)	-0.8875 (0.7419)	-1.006 (0.8911)
Oil price growth (QoQ, %)	0.0009 (0.0010)	7.57×10^{-5} (0.0010)	0.0036 (0.0032)	0.0040 (0.0046)
S&P 500 return (QoQ, %)	0.0047* (0.0025)	0.0028 (0.0025)	0.0059 (0.0096)	0.0046 (0.0109)
NFP announcement	-0.0983 (0.0751)	-0.0669 (0.0748)	-2.796*** (0.3108)	-3.376*** (0.3384)
Inflation gap	0.0193 (0.0121)	0.0239* (0.0141)	0.0817 (0.0531)	0.0788 (0.0573)
Unemployment gap	0.0151 (0.0151)	0.0262* (0.0154)	0.0401 (0.0357)	0.0562 (0.0499)
FOMC announcement	-0.1969* (0.1083)	-0.2525* (0.1309)	-2.814*** (0.3741)	-3.050*** (0.4652)
Yield skewness (ISK)	-0.2765*** (0.1042)	-0.2393*** (0.0725)	-0.6037** (0.2346)	-0.5983** (0.2949)
<i>Fit statistics</i>				
Observations	5,412	4,187	5,444	4,302
R ²	0.019	0.015	0.046	0.052

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent variables: first difference of VIX and MOVE index levels (index points). Drivers specification: inflation gap, unemployment gap, QoQ oil price growth (LOCF), QoQ S&P 500 return (LOCF), NFP announcement dummy, FOMC announcement dummy, and treasury yield skewness (ISK). Beginning-of-month dummy and two lags of the dependent variable included but omitted from display. “No Trump” drops 20 Jan 2017–20 Jan 2021 and post-20 Jan 2025. HAC (Newey-West) standard errors in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

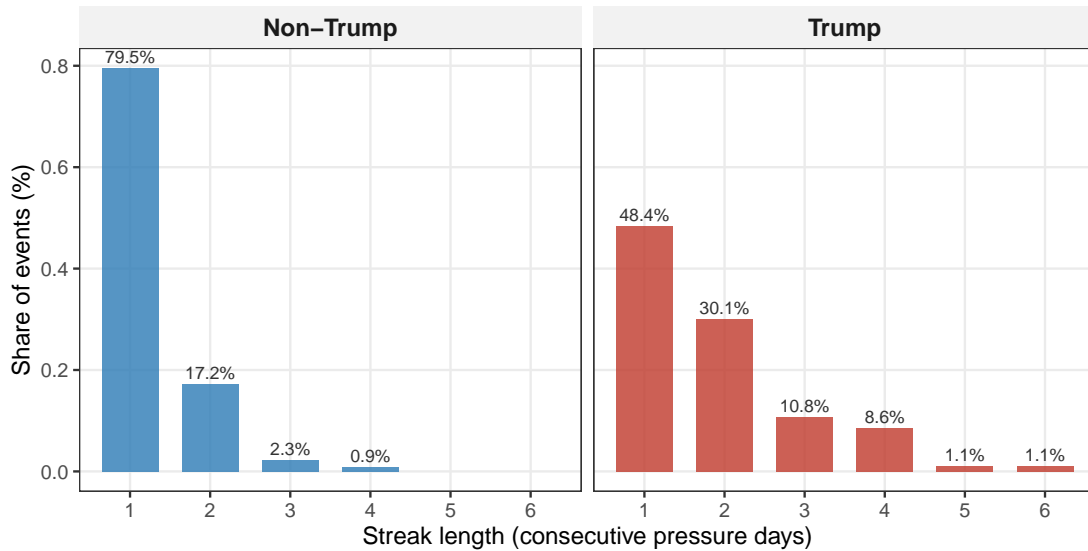
D.2 Pressure Intensity and the Trump Period

The result that the dovish coefficient is larger in the non-Trump subsample than in the full sample—documented in Section 5.2—invites a natural question. If Trump-era pressure is by any conventional measure *more* intense than pressure under previous administrations, why does the per-event coefficient attenuate when the Trump windows are included? This subsection offers

a characterisation of the difference in pressure intensity between the two periods and uses it to rationalise the coefficient pattern.

Figure 18 documents the first piece of evidence. We define a pressure streak as a sequence of consecutive days on which at least one dovish pressure article appears, and plot the distribution of streak lengths separately for the pre-Trump and Trump periods. The two distributions differ markedly. Pre-Trump streaks are overwhelmingly short, with isolated single-day events dominating the mass of the distribution. Trump-period streaks exhibit a substantially heavier right tail, reflecting the sustained multi-day and multi-week campaigns of public pressure against the Chair and the FOMC that characterise both Trump administrations.

Figure 18: Political pressure streaks: pre-Trump vs. Trump periods



Note: The figure shows the distribution of pressure streak lengths separately for the pre-Trump and Trump periods. A streak is defined as a sequence of consecutive days with at least one dovish pressure article.

The streak distribution suggests that the two periods differ not only in the overall frequency of pressure events but in their temporal clustering. A coefficient estimated on the pooled sample weighs each event equally, implicitly assuming a constant marginal response regardless of the intensity of recent pressure flow—an assumption the Trump-era streak distribution makes difficult to sustain. We test it directly by augmenting the baseline specification with an interaction between $n_{\text{dovish},t}$ and the rolling sum of dovish articles over the preceding three days.

Formally, for each maturity $m \in \{1, 2, 3, 4\}$ we estimate

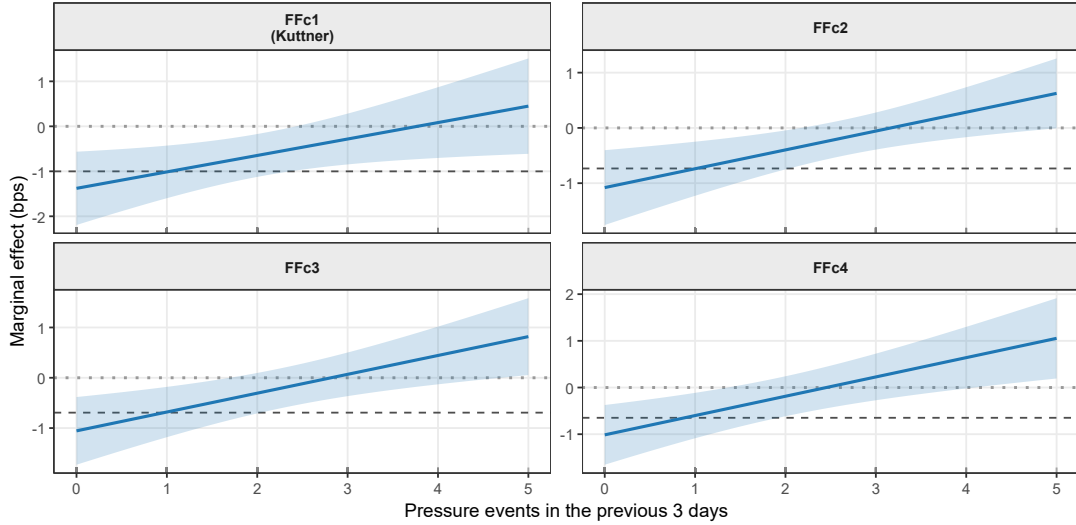
$$\Delta r_t^m = \alpha_m + \beta_{1,m} n_{\text{dovish},t} + \beta_{2,m} S_{t-1}^{(3)} + \beta_{3,m} (n_{\text{dovish},t} \times S_{t-1}^{(3)}) + \sum_{l=1}^2 \delta_{l,m} \Delta r_{t-l}^m + \gamma'_m \mathbf{X}_t + \varepsilon_{m,t}, \quad (6)$$

where $\Delta y_{m,t}$ is the daily change in the FFC m implied rate, $n_{\text{dovish},t}$ is the count of dovish pressure articles on day t , and $S_{t-1}^{(3)} = \sum_{k=1}^3 n_{\text{dovish},t-k}$ is the rolling three-day sum of dovish articles over the preceding window. The vector \mathbf{X}_t collects the baseline controls; $\varepsilon_{m,t}$ is an error term and standard errors are HAC (Newey–West). The marginal effect of an additional dovish article on day t , evaluated at recent pressure intensity s , is

$$\frac{\partial \mathbb{E}[\Delta r_t^m \mid \cdot]}{\partial n_{\text{dovish},t}} = \beta_{1,m} + \beta_{3,m} s, \quad (7)$$

which is the object plotted in Figure 19 for each maturity m .

Figure 19: Marginal effect of dovish pressure as a function of recent pressure intensity



Note: The figure plots the marginal effect of political pressure events on the daily change in federal funds futures implied rates (in basis points) as a function of recent pressure intensity, measured as the sum of dovish articles over the preceding three days. Shaded bands denote 90% confidence intervals constructed via the delta method. The dashed horizontal line indicates the unconditional baseline estimate.

The marginal effect declines monotonically in recent pressure intensity. An isolated dovish event—one that arrives after a quiet three-day window—moves near-term rate expectations by substantially more than the unconditional baseline. As recent intensity rises, the marginal effect attenuates, and at the highest observed intensities it is statistically indistinguishable from zero.

The pattern is consistent with market habituation: the first dovish article in a campaign carries information about the executive’s stance and the likely path of the Fed’s reaction function, but successive articles in a sustained campaign convey progressively less incremental information, because the stance has already been priced.

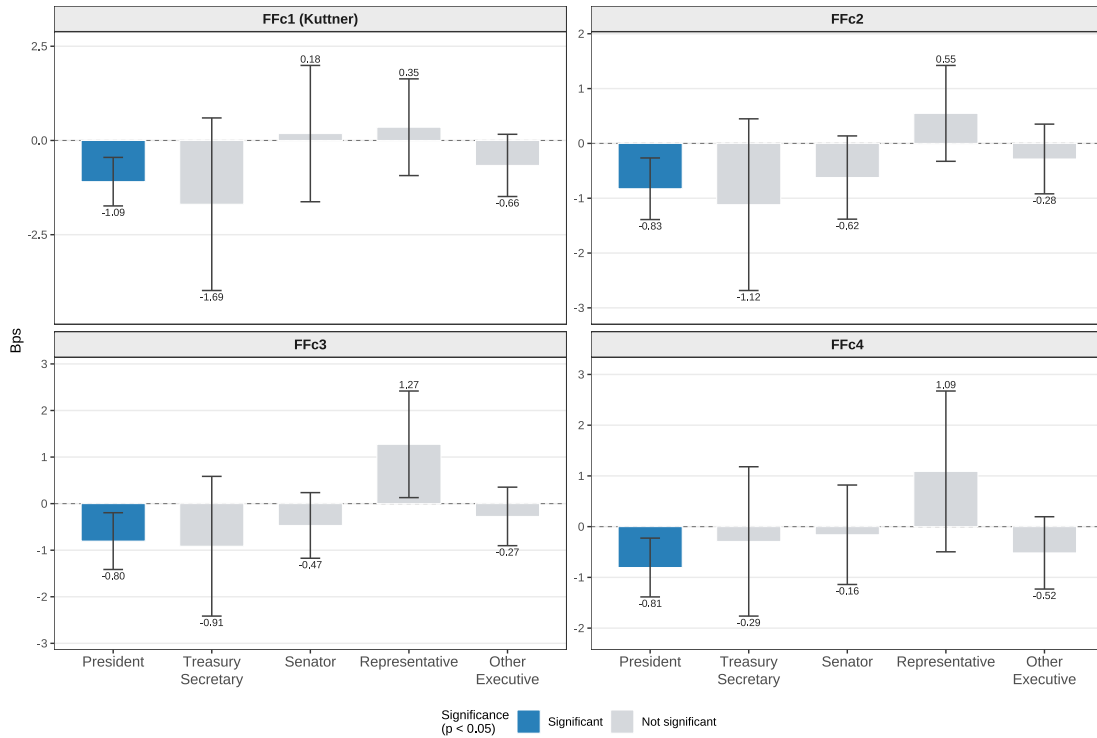
This interaction rationalises the non-Trump coefficient pattern. The Trump periods are disproportionately populated by high-intensity streaks, and within those streaks the marginal event carries a smaller-than-average effect. Pooling across the full sample therefore weights the estimate toward the low-marginal-effect region of the distribution, attenuating the pooled coefficient relative to the non-Trump subsample—where pressure events are more often isolated and closer to the high-marginal-effect region. The non-Trump estimate of -1.58 basis points is not, on this reading, evidence that markets responded more to Bush or Obama-era pressure than to Trump-era pressure; rather, it reflects the fact that Bush and Obama-era pressure arrived in the information-rich, isolated form that markets price most sharply. Habituation at the per-event margin does not, however, imply a small cumulative effect: a sustained campaign with many events can move expectations substantially even as each additional event contributes less, and the welfare-relevant object is the cumulative shift in the expected policy path rather than the marginal response to any single article. The substantive conclusion is unchanged—political pressure shifts rate expectations throughout the post-1987 sample—but the mechanism is sharpened: what markets price is new information about the executive’s stance, and sustained repetition of the same stance delivers diminishing returns.

D.3 Heterogeneity by Actor

The baseline specification pools all political pressure events, implicitly treating a presidential statement and a congressional speech as equivalent signals. We relax this assumption by estimating the baseline separately for five actor categories: President, Treasury Secretary, Senator, Representative, and other executive officials. Presidential pressure dominates the Reuters sample in frequency, so cross-actor comparisons are affected by differential precision: non-presidential categories have smaller samples and wider confidence intervals.

Figure 20 shows that the baseline effect is driven entirely by presidential pressure. The presidential coefficient is negative and significant at the 5% level across all four maturities, ranging from -1.09 basis points at FFc1 to -0.80 at FFc3. All four remaining actor categories—Treasury Secretary, Senator, Representative, and other executive officials—enter with coefficients that are statistically indistinguishable from zero at every maturity. Market participants appear

Figure 20: Heterogeneity by source of political pressure



Note: The figure reports the coefficient on $n_{\text{dovish},t}$ from the baseline specification, estimated separately for five actor categories, across FFc1 through FFc4. Bars denote point estimates; whiskers denote 95% confidence intervals. Blue bars indicate significance at the 5% level; grey bars are not significant.

to price the unique institutional authority of the President specifically, rather than political rhetoric more broadly; neither opposition statements nor pressure from within the governing party have detectable market effects unless the signal originates from the President himself. This concentration of the effect, however, can only be documented *ex post* and with a pressure measure that preserves the identity of each actor—prior aggregate indices, which collapse all political signalling into a single count, would not have revealed it. The finding thus also illustrates the value of actor-level resolution in the construction of the pressure measure itself.