Biased Forecasts and Voting*

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Abstract

We introduce macroeconomic forecasters as strategic agents who may bias their reports to influence their audience. We test the motives of forecasters using monthly forecaster-level data around the Brexit referendum. To guide our analysis, we model the way in which a referendum voter forms expectations based on the reports of a politically motivated forecaster. The forecaster trades off its reputation for honesty against the opportunity to influence the vote. The model predicts that the forecaster lies selectively and continues to do so even after the referendum. Our analysis finds that forecasters exposed to financial losses from Brexit issued significantly less accurate GDP growth forecasts than others. The findings suggest that influential forecasters may strategically manipulate information when the stakes are high.

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1 Introduction

Referendums often address issues of major economic relevance. Recent examples include the United Kingdom's referendum on European Union membership, the Greek referendum on the measures proposed to tackle the debt crisis, and the Catalan independence referendum. In the weeks leading up to these votes, public debate frequently centers on their economic consequences. Estimates released by professional macroeconomic forecasters are often an important input into that debate. Forecasters' estimates are typically treated as neutral assessments, with limited attention paid to the incentives faced by the experts producing them. We challenge this view and investigate whether macroeconomic forecasters bias their forecasts to influence voting outcomes. Forecasters are economic agents with preferences over the outcomes of these votes, and referendums represent moments in which forecasters exert unusually high influence on the legislative process. It is therefore natural to ask whether their reports are based solely on available information or are instead shaped by their preferences and influence.[1]

To guide the empirical analysis, we develop a theoretical framework to study the behavior of a forecaster during a referendum. The referendum allows a voter to choose between two policies. The forecaster publishes macroeconomic forecasts associated with both policies, and the voter relies on this information to evaluate each alternative. The forecaster has a vested interest in the outcome of the referendum. In the tradition of Kreps and Wilson (1982) and Milgrom and Roberts (1982), deviating from honest behavior—i.e., biasing the forecasts—trades off a short-term gain (the opportunity to influence the vote) against a long-term cost in terms of reputation for honesty. While the forecaster publishes macroeconomic forecasts associated with both policies, the realized economic outcome is observable ex post only for the policy that the referendum selects. This has relevant implications. First, only one of the two forecasts will eventually be compared to a realized outcome. The reputation of the forecaster hinges primarily on the accuracy of this forecast. Second, the probability that each forecast will be associated with a realized outcome is endogenous. That is, releasing a biased forecast may simultaneously influence the likelihood that the voter chooses the forecaster's preferred outcome and the likelihood that bias in a forecast is detected.[2]

The theoretical framework yields three testable predictions. First, before the referendum, the forecaster is more likely to lie about the consequences of the alternative that is (i) less likely to be selected in the referendum and (ii) associated with greater macroeco-

[1] The same logic may apply more generally to other expert intermediaries—such as policy analysts, legal experts, or consultants—whose public assessments may shape collective decisions while not being fully insulated from institutional, financial, or ideological incentives.

[2] Our model also departs from the aforementioned models in that the state of the world can change over time, and the forecaster can therefore amend the initial forecast. This feature yields the second testable prediction of our theoretical framework (see next paragraph).

nomic uncertainty than about the consequences of the other policy. Second, a forecaster that published biased forecasts before the referendum will adhere to these forecasts, at least for a short while, after the referendum. Third, a stronger influence of the forecaster on the voter is associated with a greater bias in the forecasts.

We test these predictions in the context of the 2016 EU membership referendum, also known as the Brexit referendum. The referendum pitted the status quo (Remain) against a new policy (Leave). The application is appropriate for several reasons. First and foremost, we present evidence that macroeconomic forecasts influenced voters' opinions in the weeks leading up to the referendum. Second, one of the policies, namely Leave, satisfied the two properties that, according to our first prediction, lead to a more likely bias in the forecasts. A victory for Leave was perceived as the least likely outcome (Property (i)), and the consequences of leaving the EU were more difficult to predict than the consequences of remaining (Property (ii)), no country having ever left the EU. Finally, crucially for our empirical strategy, some, but not all, forecasters had their interests threatened by Brexit.

Our data consist of a sample of forecasters that publish short-term GDP growth forecasts in the United Kingdom on a monthly basis. We observe forecasts published before the announcement of the referendum, between the announcement and the vote, and after the vote. For the period before the vote, we observe individual forecasts associated with Remain (we refer to those as "Remain forecasts"). Leave forecasts before the vote are instead available only at an aggregate level for the entire sample, and at the individual level for a handful of forecasters. To address this limitation, we rely on individual forecasts collected a few days after the vote as proxies for the individual Leave forecasts published right before.[3]

The broad consensus before the referendum was that Leave would be worse than Remain for the UK economy as a whole and for individual economic actors. The ideal metric for a forecaster's vested interest in the referendum is then the extent of its expected losses in the event of Brexit. We rely on four proxies for expected losses: (i) being a bank—our preferred measure; (ii) being located in the City of London's financial district; (iii) being exposed to stock market declines in connection with the referendum result; and (iv) having a large fraction of capital held by UK-based shareholders. We divide forecasters between those with large expected losses (*partisan* forecasters) and those with little or no expected losses (*non-partisan* forecasters). We compare forecasts released by partisan and non-partisan forecasters. The idea is straightforward: the latter should reflect the forecasts that partisan forecasters would have released without the incentive to influence voters.

[3] Our theoretical framework supports this approach, as it predicts that forecasts adjust with a lag after the referendum. Additionally, we observe in the data that, at an aggregate level, Leave forecasts published right before and right after the referendum were identical.

Our first prediction states that partisan forecasters would be more likely to bias Leave forecasts than Remain forecasts. We observe in the data that partisan forecasters released less accurate and more pessimistic Leave forecasts compared to their non-partisan counterparts, while the two groups released identical Remain forecasts. The bias associated with Leave was substantial. On average, banks estimated the negative impact of Brexit on short-term GDP growth to be 0.73 percentage points higher than other forecasters. This result is robust to the alternative measures of partisanship.

The second prediction concerns the persistence of bias after the referendum. We show that the bias was indeed strong immediately after the referendum, in July 2016. At the aggregate level as well as at the individual level when feasible, we observe that Leave forecasts published right before the referendum were identical to those published right after. Consistent with the prediction, we also show that the bias gradually disappeared: by the end of 2016, the bias was essentially gone.

Next, we consider the role of the strength of a forecaster's influence on voters. Consistent with our third prediction, we observe that the bias is significantly heterogeneous across forecasters with varying amounts of coverage in UK media outlets, with more covered forecasters publishing more biased forecasts. As further evidence, we show that forecasters frequently mentioned in newspapers consumed by a relatively large fraction of undecided voters estimated the negative impact of Brexit to be 0.6 percentage points higher than forecasters seldom mentioned in such outlets.

The match between our second and third predictions and our empirical observations is important, as both predictions are difficult to reconcile with —or are plainly at odds with— alternative explanations for the observed biases in Leave forecasts.

One might conjecture, for instance, that forecasters more exposed to Brexit used data, or adopted statistical models, that yielded peculiarly pessimistic views on Brexit. The most natural "microfoundation" for this alternative conjecture would involve forecasters with high stakes "panicking" during a period of turbulence and thus producing unreliable forecasts. This conjecture would yield a prediction similar to our first prediction, but does not provide a rationale for the correlation between influence and bias nor does it help rationalize the gradual disappearance of the bias in the Leave forecasts after the vote. Furthermore, we provide additional evidence against this interpretation by showing that similar results cannot be obtained in periods of high economic uncertainty or economic downturns not connected with a popular vote in the UK. Specifically, we do not find any evidence that banks published estimates significantly different from those of other forecasters at the beginning of the 2008 financial crisis, during the referendum on the EU Constitution that French voters did not approve in 2005, or after the terrorist attack on the World Trade Center in 2001. Moreover, when we look beyond the period right after the referendum, we do not find any evidence that banks and other forecasters published significantly different estimates of the effect of Brexit on GDP growth. Taken together, this evidence makes panic an unlikely explanation for the divergent forecasts.

One might also be concerned that influential forecasters published pessimistic views due to demand-driven market incentives—i.e., because consumers of media outlets in which these forecasters were mentioned wanted to read negative news about Brexit. However, we provide evidence of the opposite pattern: GDP growth in the event of Brexit was underestimated more by forecasters who were relatively more often mentioned in newspapers catering to Leave voters compared to forecasters mostly mentioned in newspapers catering to Remain voters.

A substantial body of literature has demonstrated that special interest groups (see, e.g., Baron, 1994; Grossman and Helpman, 1996; Besley and Coate, 2001; Sadler, 2023) and the media (see, e.g., Enikolopov, Petrova, and Zhuravskaya, 2011; DellaVigna, Enikolopov, Mironova, Petrova, and Zhuravskaya, 2014; Qin, Strömberg, and Wu, 2018) are key players in political economy and often release biased information to influence individual beliefs and, consequently, voting behavior (Martin and Yurukoglu, 2017; Durante, Pinotti, and Tesei, 2019). The key novelty of our paper is to document that non-political actors that provide the voters with essential information on the expected economic impact of their vote may be motivated by similar "influence motives." Although we focus on macroeconomic forecasters, we highlight a mechanism that may extend to other environments in which publicly released assessments can affect collective decisions.

These "influence motives" result in biased forecasts. The literature has identified several other sources of bias in macroeconomic forecasts, including "non-strategic" sources such as overconfidence (Broer and Kohlhas, 2024), inattention (Giacomini, Skreta, and Turen, 2020), and irrational optimism/pessimism (Ashiya, 2009; Batchelor, 2007). Our work is more closely aligned with the strand of literature suggesting that forecasters strategically bias their forecasts. For instance, Laster, Bennett, and Geoum (1999) show that forecasters aim to be perceived as accurate while simultaneously attempting to generate publicity for their forecasts. Ottaviani and Sørensen (2006) compare two theories of professional forecasters' strategic behavior: one in which forecasters strive to be perceived as accurate, and another in which they participate in forecasting contests. Similarly, Capistran and Timmermann (2009) find that inflation forecasters bias their predictions in response to asymmetric costs of over- and under-predicting inflation. In these studies, forecasters manipulate their predictions to influence perceptions of their ability or credibility. In contrast, our work explores an additional motive for manipulation: influencing voters' beliefs.

Beyond the voting context, a significant body of literature has investigated how experts bias their assessments to influence decision-makers. This includes research on credit rating agencies (e.g., Bolton, Freixas, and Shapiro, 2012), analysts specializing in earnings forecasts (e.g., Hong and Kacperczyk, 2010), and financial advisors (e.g., Inderst and

Ottaviani, 2012). Part of this literature focuses on advisors with reputational concerns (see, e.g., Sobel, 1985; Benabou and Laroque, 1992; Durbin and Iyer, 2009). We focus on macroeconomic forecasters and verify whether, at least in special times such as the onset of an important referendum, forecasters behave like other motivated experts and strategically misreport their findings so as to influence the actions of their audience.

The next section presents a number of stylized facts about the Brexit referendum. The theoretical framework is presented in Section 3. The empirical strategy and the results can be found, respectively, in Sections 4 and 5. Section 6 discusses alternative mechanisms, while Section 7 presents additional robustness checks. Section 8 concludes.

2 The Brexit Referendum

In February 2016, the UK Prime Minister announced a referendum on EU membership to be held on June 23 of the same year. Voters were asked to choose between two options: Leave (the EU) or Remain.[4] On the day of the referendum, a majority (51.9%) of voters chose Leave.[5]

We focus on the Brexit referendum for three reasons. First, as we argue below, macroeconomic forecasts influenced voters' opinions in the weeks leading up to the referendum. Without influence on the public, macroeconomic forecasters would have no way to influence voting outcomes. Second, Leave was perceived as less likely to be selected by voters and associated with more macroeconomic uncertainty than Remain (see Appendix A for evidence supporting this claim); our theoretical framework has sharp predictions exactly when one alternative in the ballot satisfies both the aforementioned properties. Third, some forecasters had stronger reasons than others to favor Remain: this feature will be key to our empirical exercise (see Section 4).

2.1 Forecaster Influence

Forecasters require coverage in news outlets to influence voters. The coverage of macroeconomic forecasters was extensive in the United Kingdom even during "normal" times. We measure the number of times online media sources published in the UK and indexed in Google News mentioned the forecasters in our sample (see Section 4 for a description of our sample) in 2015, before the referendum was announced. The median number of mentions was 7,000, while the most covered forecaster was mentioned 185,000 times. We also focus on five national newspapers: *The Times*, *The Financial Times*, *The Guardian*, *The Daily Mail*, and *The Telegraph*. In 2015, the median number of mentions in those

[4] The referendum was nonbinding. Nevertheless, before the vote, the government indicated its willingness to commit to voters' preferences.

[5] See Table D1 for a summary of the relevant dates.

newspapers amounted to one per week, while the most covered forecaster was mentioned daily.

Coverage alone is not sufficient to influence voters; voters also need to pay attention. According to the *British Election Study* panel, prior to the referendum, the economy was the most important issue for the relative majority of voters. The panel is a monthly survey representative of the UK voting-age population; it gathers information about the political views of the UK electorate. We conjecture that voters must have found it challenging to anticipate the economic consequences of Leave, since no country had previously withdrawn from the EU. In this context, we should expect experts' opinions to have played a role.

We indeed find evidence that forecasts influenced voters' beliefs. We focus on a negative forecast of the immediate economic impact of Brexit released by Her Majesty's (HM) Treasury on May 23 and show that the forecast had a sizable and statistically significant impact on voters' beliefs.[6] We measure the impact of the forecast using Waves 7 and 8 of the *British Election Study* panel. Interviews for Wave 7 took place before the release of the forecast, with the wave 7 completed on May 4. Interviews for Wave 8 occurred before the release of the forecast for some individuals and after the release for others. Importantly for our purposes, the exact date on which each participant was interviewed was randomized. For each individual, we measure the change in beliefs between Wave 7 and Wave 8. We let this change be a function of whether the individual was interviewed for Wave 8 before or after the forecast release. We estimate

$$\Delta y_i = \alpha + \beta \times PostRelease_i + \varepsilon_i, \tag{1}$$

where the dependent variable Δy_i measures the change in whether the surveyed individual reports understanding the issues at stake in the referendum, to be willing to vote, and to be interested in the outcome of the referendum, respectively.

Estimating equation (1) allows us to measure whether voters interviewed for Wave 8 after the publication of the HM Treasury forecast changed their views compared to the previous wave more than individuals whose responses to Wave 8 were collected before the forecast release. A concern is that, as the referendum drew closer, individuals became more interested and reported a higher understanding of the issues at stake. To ensure the credibility of our results, we limit our attention to individuals interviewed for Wave 8 within a window of five calendar days before or after the forecast release.[7]

Figure 1 documents that respondents surveyed within a window of five days *after* the

[6] According to the report (HM Treasury, 2016), leaving the EU would reduce GDP by between 3.6 and 6 per cent within two years, with higher inflation and unemployment, and a depreciation of house prices and the pound sterling.

[7] Figure D1 replicates this exercise for different time windows between one and twenty days around the HM Treasury forecast release. The conclusions are the same.

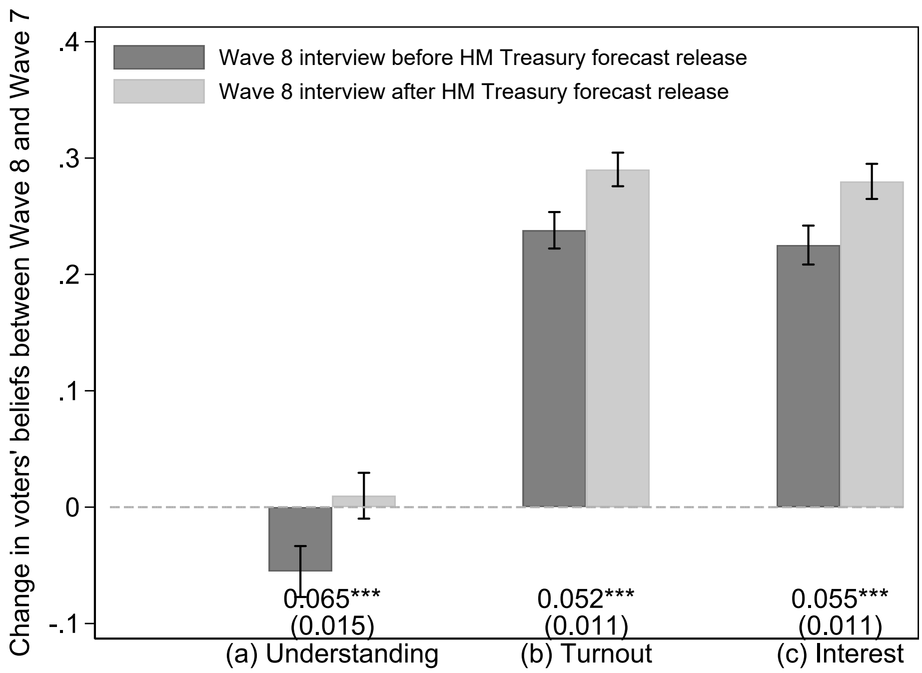

Figure 1: Voters' Beliefs and Forecast Release

Notes: The figure illustrates how respondents in the British Election Study panel changed their views between Wave 7 (April 14–May 4, 2016) and Wave 8 (May 6–June 22, 2016). Dark gray bars refer to respondents surveyed for Wave 8 before the release of HM Treasury forecasts on May 23, whereas light gray bars refer to respondents surveyed for Wave 8 after the release. The sample includes only individuals surveyed in a window of five calendar days around the forecast's release. Panel (a) reports the change in the probability of agreeing or strongly agreeing with the following statement: *I have a good understanding of the important issues at stake in the EU referendum.* Panel (b) shows the change in the probability that the respondent reports that s/he will very likely or likely vote. Panel (c) shows the change in the probability that the respondent reports being very or somewhat interested in the referendum. In all panels, we report coefficients and confidence intervals for β in equation (1). Standard errors and 95% confidence intervals are robust to heteroskedasticity. Labels *, ** and *** represent, respectively, 10%, 5% and 1% significance levels.

release of the survey were more likely to understand the relevant issues, report their intention to participate in the referendum, and be interested in the debates than respondents surveyed in the five days *before*. The formal estimates are reported in Table D2.

Having argued that forecasts influenced voters in the Brexit referendum, we present next a simple model of a voter that relies on forecasts to make up his mind prior to a referendum.

3 Theoretical Framework

To guide the empirical analysis, we develop a theoretical model of a biased macroeconomic forecaster during a referendum in the tradition of reputation models (Kreps and Wilson (1982) and Milgrom and Roberts (1982)). In our model, the forecaster trades a short-term gain from dishonest reports – better odds for her favorite referendum outcome – with a long-term cost to reputation. Our model adds to the reputational cheap talk models a few elements that make for a better fit to the Brexit referendum and help test our proposed model against alternative explanations for forecast mistakes. First, the forecaster publishes not one, but two forecasts before the referendum - one for each referendum outcome. Importantly, the consequence of each forecast on the forecaster reputation depends on the outcome of the referendum, and the odds of each outcome realizing are endogenously determined by the forecasts, by way of their influence on the vote. Finally, we let the forecaster update their forecast after the referendum. As we will show, the forecasts of a strategic forecaster that lied before the referendum and needs to hide the lie as much as possible after the referendum evolve in a way inconsistent with different explanations for forecast mistakes.

Players: A macroeconomic forecaster (she) publishes forecasts to influence the choice of a voter (he) in a referendum. The referendum involves the voter choosing between two policies: Leave (ℓ) and Remain (r). After the referendum, a passive observer (it), referred to as "the market", updates its belief about the type of the forecaster based on the forecasts and the macroeconomic outcome.

Actions: Each policy $p \in \{\ell, r\}$ is associated with a macroeconomic outcome. At the onset, Nature draws a pair of outcome realizations, (y_ℓ, y_r). The outcomes are independently distributed on $\{\underline{\ell}, \overline{\ell}\} \times \{\underline{r}, \overline{r}\}$, where $\underline{\ell} < \overline{\ell}$ and $\underline{r} < \overline{r}$ as follows:

$$Pr(y_\ell = \underline{\ell}) = Pr(y_\ell = \overline{\ell}) = Pr(y_r = \underline{r}) = Pr(y_r = \overline{r}) = 1/2.$$

We assume that $\frac{1}{2} > \overline{r} - \underline{\ell}$ and $\underline{r} - \overline{\ell} > -\frac{1}{2}$: as it will become clear, this amounts to assuming that the referendum outcome is ex-ante uncertain (see footnote 8). The forecaster privately observes a signal perfectly correlated with these draws and publishes a pair of *initial forecasts* $(f_\ell, f_r) \in \{\overline{\ell}, \underline{\ell}\} \times \{\overline{r}, \underline{r}\}$.

With some probability $\chi \in (0, 1)$ the voter observes the forecasts, while with probability $1 - \chi$ he does not. This probability measures the influence of the forecaster on the voter. In practice, influence depends on media coverage as well as the attention paid by voters (see Section 2.1). Here we do not distinguish between the two factors: we model influence as the probability that the voter observes the forecast, and assume that a voter who observes the forecast pays attention to it.

After (potentially) observing the forecasts, the voter selects one of the two policies. We refer to the policy selected as p°. We allow for the macroeconomic outcome to change after the referendum: Nature draws a new outcome realization y°, associated with policy p°. If $p^\circ = \ell$, then y° is drawn from $\{\underline{\ell}, \overline{\ell}\}$; if instead $p^\circ = r$, then y° is drawn from

$\{\underline{r}, \overline{r}\}$. With some probability $\eta \in [1/2, 1]$, new and old draw coincide: $y^\circ = y_{p^\circ}$; with probability $1-\eta$, the outcome changes: $y^\circ \neq y_{p^\circ}$. Having observed the new draw, the forecaster publishes a single *final forecast* f°. If $p^\circ = \ell$, then $f^\circ \in \{\underline{\ell}, \overline{\ell}\}$; if instead $p^\circ = r$ then $f^\circ \in \{\underline{r}, \overline{r}\}$.

With probability $\psi \in (0,1)$ the market observes both y_{p° and y°, while with probability $\psi^\circ \in (0, 1-\psi)$ it observes only the new draw y°. With probability $1-\psi-\psi^\circ$, the market does not observe any outcome draw. Intuitively, external observers (the market) need to establish what the forecaster knew at the time of publishing the forecasts to infer her type (more on this below). The probabilities ψ and ψ° capture, in a simple way, the fact that external observers have partial knowledge about the forecaster's information.

Strategies: The strategy of the voter includes – for the realization of the voter's ideological bias (see below) – a mapping from the initial forecasts to a policy and a rule for selecting a policy in case the initial forecasts are not observed. The strategy of the forecaster is described by two conditional probability distributions. The first distribution assigns probabilities to initial forecasts conditional on the outcome draws observed before the referendum. The second distribution assigns probabilities to final forecasts, conditional on the draws observed before the referendum, the initial forecasts, the policy selected by the voter and the outcome drawn after the referendum. The third player, the market, is passive.

Reputation: At the onset, the voter and the market believe the forecaster to be *sincere* with probability $\mu_0 \in (0,1)$ and *strategic* otherwise. The voter and the market expect the forecaster, if sincere, to publish forecasts equal to the outcomes observed and, if strategic, to choose forecasts according to some strategy. After observing the initial forecasts, the final forecast and, possibly, one or two outcome draws, the market updates the probability assigned to the forecaster being sincere to some μ°, according to Bayes rule whenever possible. We refer to μ° as the *reputation* of the forecaster.

Payoffs and Equilibrium: The voter's payoff is

$$y^\circ + \sigma_{p^\circ},$$

where the term σ_{p° captures the voter's ideological bias. We set $\sigma_\ell = 0$, and let σ_r be drawn from a uniform distribution on the interval $[-\frac{1}{2}, \frac{1}{2}]$.[8] Thus, the voter is biased in favor of Remain if $\sigma_r > 0$ and in favor of Leave if $\sigma_r < 0$. The realization of σ_r is private

[8] The size of the support of the distribution of the voter's bias, combined with the assumption that $1/2 > \overline{r} - \underline{\ell}$ and $\underline{r} - \overline{\ell} > -1/2$, implies that the referendum outcome is ex-ante uncertain. Adding stochastic bias so as to model uncertainty is a standard feature of probabilistic voting models (see Section 3.4 in Persson and Tabellini (2000)).

information of the voter.

The forecaster's payoff is:

$$\mu^\circ + \rho_{p^\circ},$$

where the term ρ_{p° captures the forecaster's bias. We normalize $\rho_\ell = 0$ and let ρ_r be a commonly known parameter of the game. We will refer to ρ_r simply as ρ. The forecaster's payoff captures the trade off between reputation (μ°) and desire to influence the referendum (ρ_{p°).

We focus on Perfect Bayesian Equilibria, or just equilibria, of the game.

Parameter Restrictions: To match the setting of the Brexit referendum, we restrict attention to parameter values that satisfy:

(i) $\overline{\ell} - \underline{\ell} \geq \overline{r} - \underline{r}$;

(ii) $\underline{r} > \overline{\ell}$;

(iii) $\rho \geq 0$.

In words, Leave is associated with at least as much macroeconomic uncertainty as Remain (Assumption (i)), and Remain is associated with better macroeconomic outcomes (Assumption (ii)) and thus enjoys better odds in the referendum. In Appendix A we present evidence that these assumptions indeed fit the Brexit referendum. Finally, the forecaster is either impartial between the policies, or she favors Remain (Assumption (iii)). In Section 4.1, we will discuss the validity of this last assumption in the context of the Brexit referendum.

Definitions: We say a strategy of the forecaster is *honest* if it calls for initial forecasts identical to the outcome draws observed, that is, $(f_\ell, f_r) = (y_\ell, y_r)$. A strategy is *selectively dishonest* if:

- for $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$, the strategy requires $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ with some probability $x > 0$ and $(f_\ell, f_r) = (\overline{\ell}, \overline{r})$ with probability $1 - x$;
- for $(y_\ell, y_r) \neq (\overline{\ell}, \overline{r})$, the strategy requires $(f_\ell, f_r) = (y_\ell, y_r)$.

An equilibrium is *honest* if the forecaster adopts an honest strategy, and it is *selectively dishonest* if the forecaster adopts a selectively dishonest strategy.

3.1 Analysis

We now present three propositions and map them to predictions that will guide the empirical analysis. The proofs of all the results are in Appendix B. First, we establish that before the referendum, honesty is optimal for a forecaster with little or no bias, while lying selectively is optimal for a forecaster with a stronger – but not too strong – bias.

Proposition 1. *There exist two thresholds ρ_1 and ρ_2 such that $\rho_2 > \rho_1 > 0$ and:*

1. *an honest equilibrium exists for $\rho < \rho_1$;*
2. *a selectively dishonest equilibrium exists for $\rho \in (\rho_1, \rho_2)$.*

The intuition is that lying damages the forecaster's reputation, but a forecaster with a sufficiently large bias may choose to do so anyway to improve the odds of her preferred policy. However, there are various ways of lying, and a forecaster with moderate bias does so selectively. The reasoning is as follows. The forecaster can improve the odds of Remain by exaggerating the demerits of Leave (i.e., publishing $\underline{\ell}$ upon observing $\overline{\ell}$), by exaggerating the merits of Remain (i.e., publishing $\overline{r}$ upon observing $\underline{r}$), or by doing both. The former option is more effective and less risky than the latter. A lie about Leave is more effective because Leave is associated with at least as much macroeconomic uncertainty as Remain (Assumption (i)). To see this, note that if a policy p is associated with no uncertainty at all, meaning $\underline{p} = \overline{p}$, then the macroeconomic forecast has no influence on the referendum. A lie about Leave is less risky because the voter is more likely to select Remain over Leave (Assumption (ii)). The key observation is that the market can determine whether the forecaster lied about a policy only if the associated outcome is observed, which requires the voter to have selected that policy.[9]

In Section 4.1 we argue that the forecasters in our dataset could be divided between those with strong motives to favor Remain (dubbed "partisan forecasters") and those with weak or no motives to favor Remain (i.e. "non-partisan forecasters"). In the context of our empirical exercise, Proposition 1 is consistent with partisan forecasters adopting a selective dishonest strategy and non-partisan forecasters adopting an honest strategy, as described in the following observation.[10]

Prediction 1 (Selective Lies)**.** *Before the Brexit referendum, partisan and non-partisan forecasters published, on average, identical Remain forecasts, but the former published more pessimistic Leave forecasts than the latter. Moreover, partisan Leave forecasts were more incorrect than non-partisan ones.*

[9] This argument does not hinge on the forecaster having a preference for Remain: a forecaster with a preference for Leave would also find it optimal to lie selectively about Leave (though the direction of the lie would evidently be the opposite).

[10] The proposition is also consistent with both types of forecasters adopting an honest strategy or both adopting a selective dishonest one. The proposition is instead not consistent with partisan forecasters adopting an honest strategy while non-partisans adopt a selectively dishonest one. The proposition is also inconsistent with forecasters adopting any of the many strategies that are neither honest nor selective dishonest, according to our definitions.

In Section 5.1, we take Prediction 1 to the data and show that, regardless of the measure of partisanship adopted, our findings are consistent with it. Our empirical results are therefore consistent with the theoretical framework and with the suggested mapping of partisan forecasters to intermediate values of ρ and non-partisan forecasters to low values of ρ. This mapping is somewhat arbitrary: mapping partisan forecasters to higher values of ρ (not discussed in Proposition 1 nor in Prediction 1) would be consistent with less selective dishonest reporting, so that the forecaster would also report dishonestly regarding the Remain scenario. This should not be interpreted as suggesting that "anything goes", i.e., that our model can rationalize any observed behavior by selecting the appropriate value of ρ. For instance, it is never an equilibrium to misreport only in the Remain scenario, to misreport "more" in the Remain scenario that in the Leave one, or to report $\underline{r}$ upon observing $\overline{r}$.[11]

We turn next to the final forecast. The definitions of honest and selectively dishonest equilibria are silent regarding the final forecast. It is straightforward to verify that in an honest equilibrium the final forecast is unbiased: $f^\circ = y^\circ$. Our second proposition focuses on selectively dishonest equilibria and establishes that, if the voter selects Leave, a forecaster who lied about Leave before the referendum may lie again after it. The likelihood of a post-referendum lie depends on the correlation between the outcomes drawn before and after the referendum.

Proposition 2. *In a selectively dishonest equilibrium, for η sufficiently large, following a lie $(f_\ell, f_r) \neq (y_\ell, y_r)$, the forecaster keeps lying: for $y^\circ = \overline{\ell}$ the forecaster publishes $f^\circ = \underline{\ell}$ with some probability. Following $(f_\ell, f_r) = (y_\ell, y_r)$ instead, the forecaster publishes $f^\circ = y^\circ$.*

The intuition behind the first part of the proposition is clearest for extreme values of η. Suppose $\eta = 1$, so that for the policy selected by the voter, the draw after the referendum, y°, is identical to the draw before the referendum, y_{p°. In a selectively dishonest equilibrium, suppose the forecaster observes $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$ and lies by publishing $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$. In equilibrium, if the voter selects Leave, the forecaster continues lying after the referendum and publishes $f^\circ = \underline{\ell}$: publishing $f^\circ = \overline{\ell}$ instead would reveal to the market that the forecaster lied either before or after the referendum. In either case, a detected lie proves to the market that the forecaster is strategic.

Suppose instead that $\eta = 1/2$, meaning the draws before and after the referendum are independently distributed. In equilibrium, the forecaster publishes $f^\circ = y^\circ$ and the market disregards the final forecast when inferring the forecaster's type.

The correlation between outcome draws will depend on the timing of those draws. Forecasts released immediately after the referendum are likely to be based on essentially

[11] Reporting $\underline{r}$ upon observing $\overline{r}$ would yield a negative coefficient β_1 in (2). See the discussion in Footnote 23 on how $\beta_1 < 0$ in (2) would contradict our theoretical model.

the same information that was available beforehand, whereas forecasts released later are likely to incorporate new information. Proposition 2 is therefore consistent with the following prediction.

Prediction 2 (Persistent Lies)**.** *After the Brexit referendum, partisan forecasters published for some time more pessimistic Leave forecasts than non-partisan forecasters. This difference eventually vanished.*

We take Prediction 2 to the data in Section 5.1 where we show that the pattern of forecasts before and after the referendum is in line with our model prediction.

Our final proposition is a comparative statics exercise. It states that the likelihood of a forecaster lying increases with her influence on the voter, as measured by the probability that the voter observes the forecast.

Proposition 3. *Suppose a selectively dishonest equilibrium exists for $\chi = \chi'$. Then, for any $\chi < \chi'$, there exists either a selectively dishonest equilibrium with a lower probability of lying, or an honest equilibrium.*

Proposition 3 is consistent with our third and final prediction.

Prediction 3 (Influential Lies)**.** *Before the Brexit referendum, forecasters with more influence on the voters were more likely to publish excessively pessimistic Leave forecasts.*

We take Prediction 3 to the data in Section 5.2, where we find a strong correlation between media coverage of a forecaster and the magnitude of bias, in line with our prediction.

4 Empirical Strategy

To test the predictions discussed above, we exploit the heterogeneity in stakes—our measure of partisanship—across forecasters who released forecasts around the time of the referendum. Specifically, we compare forecasts for the Remain and Leave scenarios published by forecasters who faced the risk of substantial financial losses in the event of Brexit ("partisan forecasters") with forecasts released by other forecasters, who were arguably more likely to have been indifferent between the two alternatives ("non-partisan forecasters"). The intuition behind our empirical strategy is that forecasts produced by forecasters without stakes in the referendum should serve as proxies for the most accurate estimates that partisan forecasters would have published (i) given the information available at the time and (ii) absent the incentive to influence voters.[12]

[12] This approach enables us to compare the ex-post performance of different forecasters while holding constant the availability of ex-ante information and the forecast horizon, thus circumventing the limitations of ex-post performance comparisons highlighted by Millner and Heyen (2021).

We rely on professional forecasters' surveys to compare estimates across forecasters. In these surveys, all forecasters report their estimates for the same target year at the same horizon, given a common set of assumptions about what to forecast. Our main outcome variable is the forecast for GDP growth in target year $t + 1$.

We use the *Forecasts for the UK Economy* survey, a panel of non-anonymous forecasts collected and released monthly by HM Treasury. The dataset is a monthly, publicly available survey of independent forecasters. Our main sample covers 44 forecasters from January 2012 to April 2018.[13]

4.1 Measures of Partisanship

Government agencies, forecasters, media outlets, and European and international public institutions warned British citizens of the prospect of a substantial economic downturn associated with a victory for Leave, driven in particular by a decline in investment (Dhingra, Ottaviano, Sampson, and Van Reenen, 2016a) and exports (Dhingra, Ottaviano, Sampson, and Van Reenen, 2016b). It stands to reason that any forecaster whose fortunes were tied to the performance of the UK economy would have preferred a victory for Remain. Accordingly, we define the partisanship of forecasters as proportional to their stakes in the performance of the UK economy. Our preferred measure of partisanship is an indicator variable (Bank) that takes the value of 1 for (investment and commercial) banks and 0 otherwise. The choice of this proxy is motivated by the observation that banks were more exposed to the performance of the UK economy than other forecasters in our sample.[14]

For robustness, we additionally propose three alternative measures of partisanship. The first measure is an indicator variable (City) taking the value 1 for forecasters in the City of London's financial district and 0 otherwise. The second measure is an indicator (Stock Price) equal to 1 for forecasters in the top quartile of the percentage decline in the forecaster's stock prices between the day of the referendum and the second market day after the vote. The third measure is an indicator (UK Holder) equal to 1 for forecasters in the top quartile of the distribution of the share of each forecaster's capital held by UK-based investors immediately prior to the referendum.[15] We dichotomize the percentage decline in the forecaster's stock price and the fraction of shares owned by investors based

[13] Figure D2 shows the distribution of forecasters by industry, whereas Table D4 offers a comparison of the forecasts released before the announcement of the EU membership referendum.

[14] Although some forecasters active at the time were explicitly pro-Leave (e.g., the think tank *Economists for Brexit*), none of them appeared in *Forecasts for the UK Economy*. Dividing forecasters between neutral and pro-Remain aligns well with the focus of our theoretical framework (recall that Assumption (iii) required $\rho \geq 0$).

[15] The City indicator proves to be the least restrictive one, as all forecasters flagged as partisan according to any of the other measures are also flagged according to the City indicator. The Stock Price and UK Holder indicators are the most restrictive, as each flags a distinct subsample of banks.

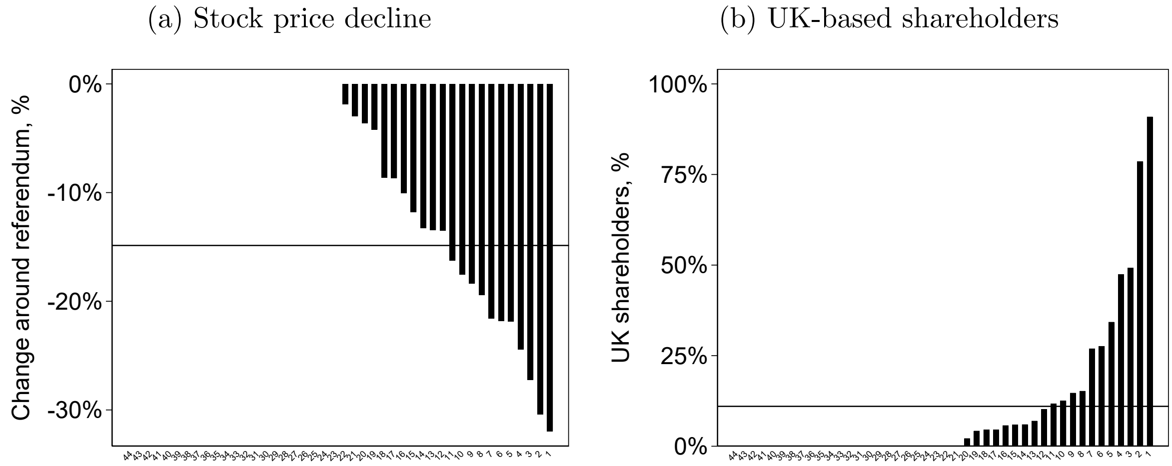

Figure 2: Decline in Forecasters' Stock Price and share of UK holders around the Referendum

Notes: Panel (a) shows the percentage variation in each forecaster's stock market price between the referendum date and the second market day after the vote. Panel (b) shows the share of each forecaster's stocks owned by UK-based holders at the end of 2015. See Appendix C for details.

in the United Kingdom for consistency with the theoretical model and to allow direct comparisons with the other two binary measures. This choice comes at a cost of not using all the available variation in the data but, at the same time, it allows us to obtain results that are not contingent on a specific functional-form assumption for the underlying relationship between magnitude of the bias and stock price decline (or, fraction of UK-based shareholders).

Figure 2 shows the substantial heterogeneity across forecasters in the latter measures. Panel (a) shows that the most affected forecasters lost up to 30% of their value around the referendum, whereas other forecasters were unaffected by Brexit.[16] Panel (b) documents the variation in the proportion of shareholders in the UK across forecasters. Most forecasters were owned only by non-UK individuals or companies, whereas for some forecasters a significant proportion of shares was in the hands of UK investors.[17]

4.2 Identifying Assumptions

Our empirical analysis, as explained in detail in the next subsection, builds on a difference-in-differences model. We assume that, in the absence of the referendum, forecasts released by partisan and non-partisan forecasters would have evolved following parallel trends.

Data constraints require an additional identifying assumption. Before the referendum, forecasters published forecasts contingent on both Leave and Remain in their own reports

[16] The panel also shows that none of the forecasters experienced an increase in stock price upon the realization of the referendum outcome.

[17] See Appendix C for details about the measures. Table D3 shows that these measures are positively correlated; however, the correlation coefficients are far from 1.

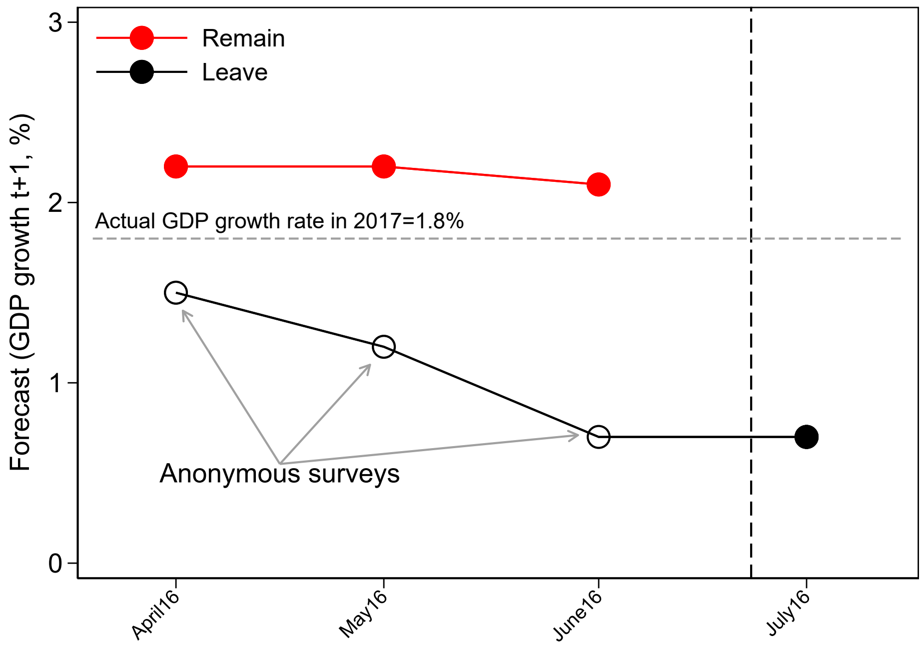

Figure 3: Average Forecasts around the Time of the Referendum

Notes: The chart shows the average forecast for the 2017 GDP growth rate conditional on Remain (red) and Leave (black) outcomes published by *Consensus Economics* in June and July 2016 (Consensus Economics, 2016a,b). The vertical line represents the referendum date, whereas the dashed horizontal line represents the actual GDP growth rate in 2017.

and in the media. The formats of these "raw" forecasts are too heterogeneous to allow comparisons across forecasters.[18] We therefore follow a standard approach (see, e.g., Ramey, 2011; Rossi and Sekhposyan, 2015; Bordalo, Gennaioli, Ma, and Shleifer, 2020) and rely on professional forecasters' surveys to compare estimates across forecasters. Surveys solve the issue of cross-forecaster comparison, as they require all forecasters to report estimates for the same target year at the same horizon, given a common set of assumptions. Yet, surveys are not perfect for our purpose. At each point in time, surveys only ask surveyed forecasters for the central (modal) forecast, i.e. a forecast contingent on the outcome considered the most likely at the time. As the victory for Leave was unexpected (see Appendix A), surveys only started reporting individual forecasts

[18] First, many forecasters provided bounds for the effect of the withdrawal on economic growth subject to different assumptions on the future agreements between the UK and the European Union. Second, many forecasters reported estimates of the net effect of the Brexit (i.e., they published the difference between the GDP in the event of the Brexit and the GDP in the Remain scenario) without providing additional information on their baseline forecast. Third, forecasts included in the reports or on the media are released at different points in time, implying that they did not rely on the same set of available information. In Figure D3 in the Appendix, we report a snapshot of the cases mentioned just above. For example, forecasts of Mansfield (2014), Bertelsmann Stiftung (2015) and Open Europe (2015) were released before 2016, while the others were released during 2016. Additionally, the forecast of Oxford Economics (2016) was published in March 2016, OECD (2016) in April and Ebell and Warren (2016) in May.

conditional on Leave after the referendum.[19] We address this limitation by using the forecasts included in the first survey after the referendum as a proxy for the forecasts published immediately before the vote. In the next paragraphs, we provide evidence to support this assumption and discuss how our empirical results might change if the assumption were not satisfied.

First, the average forecast conditional on Leave was the same immediately before and immediately after the referendum. We compare average forecasts conditional on Leave before and immediately after the referendum as published in the *Consensus Economics* report.[20] In the April, May, and June 2016 releases of the report (Consensus Economics, 2016a), surveyed forecasters were asked to anonymously report their forecasts conditional on Leave. Figure 3 shows that the average forecast of GDP growth released in the last release before the vote (the June release) was equal to the average forecast according to the first non-anonymous release published after the vote (the July one).

Second, all forecasters for whom we retrieved a pre-referendum forecast did not revise the forecast around the date of the referendum. For example, the forecast of GDP growth for 2017 released by the Economist Intelligence Unit in June 2016 was the same as the forecast included in the July 2016 release of the *Forecasts for the UK Economy*.

Finally, the identifying assumption is supported by our theoretical framework: Proposition 2 predicts that a selectively dishonest forecaster will keep lying after the vote, at least for some time. Indeed, HM Treasury began collecting forecasts for the first post-referendum release of the *Forecasts for the UK Economy* survey only seven days after the referendum.[21]

What can we learn from the empirical results if the forecasts included in the first survey after the referendum do not match the forecasts available to voters before the vote? On the one hand, our theoretical framework indicates that measuring bias using forecasts released after the vote may induce an underestimation of political forecast bias since forecasters may have already started to converge towards unbiased estimates. On the other hand, if forecasters with high stakes in the referendum systematically reacted irrationally to the unexpected referendum result shock, our results would overestimate the strategic bias. In light of the discussion above, the "shock" interpretation appears unlikely. Nonetheless, in Section 6, we provide additional evidence against this interpretation.

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[19] Importantly, the forecasts contingent on a Leave victory account for the fact that an actual withdrawal from the EU would take at least two years, according to the Treaty of the European Union (Art. 50). Therefore, the forecasts reflect the referendum outcome and not the actual withdrawal.

[20] The *Consensus Economics* report is largely equivalent to our primary data, although it reports information from a slightly smaller number of forecasters. Six institutions included in our primary data have not been surveyed by *Consensus Economics*, and *Consensus Economics* surveyed three institutions not included in our primary data.

[21] The first post-referendum release was the July 2016 edition of *Forecasts for the UK Economy*. The release contained information from forecasters surveyed between July 1 and July 13.

4.3 Specification

Our data allow us to identify three well-specified periods. Forecasts included in *Forecasts for the UK economy* between March 2016 and June 2016 reflect forecasters' views about Remain in the pre-referendum period (these correspond to f_r in the theoretical framework). According to our identifying assumption, forecasts in the July 2016 issue of *Forecasts for the UK economy* reflect forecasters' views about Leave published immediately before the referendum (f_ℓ in the theoretical framework). Forecasts included in *Forecasts for the UK economy* between August 2016 and November 2016 reflect forecasters' views about Leave published in the post-referendum period (f° in the theoretical framework). We estimate the difference-in-differences model

$$
\begin{aligned}
f_{j,m}^{t+1} = {} & \beta_1 Partisan_j \times \underbrace{\mathbb{1}(k_m \in [-4,-1])}_{f_r} + \beta_2 Partisan_j \times \underbrace{\mathbb{1}(k_m = 0)}_{f_\ell} \\
& + \beta_3 Partisan_j \times \underbrace{\mathbb{1}(k_m \in [1,4])}_{f^\circ} + \theta_j + \delta_m + \varepsilon_{j,m},
\end{aligned} \tag{2}
$$

where k_m measures the distance (in months) of each survey release m from the first survey after the vote, θ_j represents forecaster fixed effects, and δ_m represents survey month effects. The dependent variable $f_{j,m}^{t+1}$ is the central forecast of GDP growth for year $t+1$ released by forecaster j in survey month m.[22]

The data support Prediction 1 if the coefficient β_2 is negative and statistically significant and if the coefficient β_1 is not significantly different from zero.[23] The data support Prediction 2 if the coefficient β_3 is also negative and statistically significant, with $|\beta_2| > |\beta_3|$.

We test the validity of the parallel trends assumption by estimating a dynamic version of equation (2) in which we interact the $Partisan_j$ indicator with monthly dummies before and after the announcement of the referendum. Detecting insignificant coefficients for β_{-7} and β_{-6} (i.e., the first two occurrences in which forecasters in our data are asked about their predictions for yearly GDP growth for 2017) would reassure us about the

[22] We focus on short-term forecasts because only a subset of forecasters releases, on a quarterly basis, GDP growth forecasts for the medium term. Short-term forecasts were very informative about the potential effects of Brexit in subsequent years, as short- and long-term forecasts were highly correlated. Figure D4 shows the correlation between GDP growth forecasts in year $t+1$ and $t+3$ among forecasters who release medium-term forecasts on a quarterly basis. Note also that our theoretical framework considers forecasts before and after the referendum that focus on the same outcome. This feature of the theoretical framework finds an exact empirical counterpart: all forecasts published around the Brexit referendum target 2017 yearly GDP growth.

[23] In the context of our theoretical framework, for extreme parameter values—for which we do not show the equilibrium characterization for brevity—in equilibrium the forecaster could both release too pessimistic forecasts for Leave and too optimistic forecasts for Remain. Hence, the broader lessons from the theoretical model would be consistent with any $\beta_1 \geq 0$. Conversely, the theoretical model would not find empirical support if β_1 is negative.

validity of the parallel trends assumption. Formally, we estimate

$$f_{j,m}^{t+1} = Partisan_j \times \sum_{k_m \in \{-7,+4\}; k_m \neq -5} \beta_k \mathbb{1}(m = k_m) + \theta_j + \delta_m + \varepsilon_{j,m}, \tag{3}$$

5 Results

5.1 Selective and Persistent Lies

Figure 4 illustrates the main empirical results of the paper. The figure shows the average forecasts of the GDP growth rate for year $t + 1$ (calendar year 2017) released by partisan and non-partisan forecasters. The visual evidence supports the overall validity of the research design and strongly aligns with Predictions 1 and 2. The parallel trends assumption is likely satisfied in this context since we do not find evidence that the two groups of forecasters released different forecasts before the announcement of the referendum. The figure also shows that, in line with Prediction 1, before the referendum, partisan forecasters released more pessimistic Leave forecasts f_ℓ than non-partisan forecasters (recall that we proxy Leave forecasts right before the referendum with the forecasts right after the referendum, as discussed in Section 4.2); in contrast, we do not find any evidence that the two groups released different Remain forecasts f_r.[24] In line with Prediction 2, partisan forecasters continued to release more pessimistic estimates than other forecasters for at least four months after the vote and the forecasts of the two groups slowly converged toward each other.

Table 1 reports the results of the formal estimation of equation (2). In column (1), we compare forecasts released by banks with other forecasts. In column (2), we compare forecasts released by institutions located in the City of London's financial district with forecasts developed by other institutions. In columns (3) and (4), we measure partisanship using the percentage decline in the forecaster's stock price associated with the referendum and the share of capital owned by shareholders based in the UK, respectively.

All specifications are consistent with Predictions 1 and 2. We estimate that banks published a GDP growth rate forecast for the Leave scenario that was 0.73 percentage point lower than that of other institutions (see Column 1). The estimated partisan bias is substantial compared to the average GDP growth forecasts in the event of Brexit as

[24] We treat pre-referendum forecasts as Remain forecasts, based on HM Treasury's survey requiring forecasters to report their modal forecast, which — given the consensus that Remain was the most likely outcome — corresponds to the forecast conditional on Remain. Should forecasters have nevertheless reported a weighted average of Leave and Remain forecasts, the empirical evidence would also be consistent with an overestimation of Remain forecasts paired with an underestimation of Leave forecasts. The key empirical results in the rest of this section — the existence of bias in Leave forecasts, its persistence, and the relationship between bias and influence — do not hinge on whether the pre-referendum forecasts reflect a Remain forecast or a weighted average of Leave and Remain forecasts.

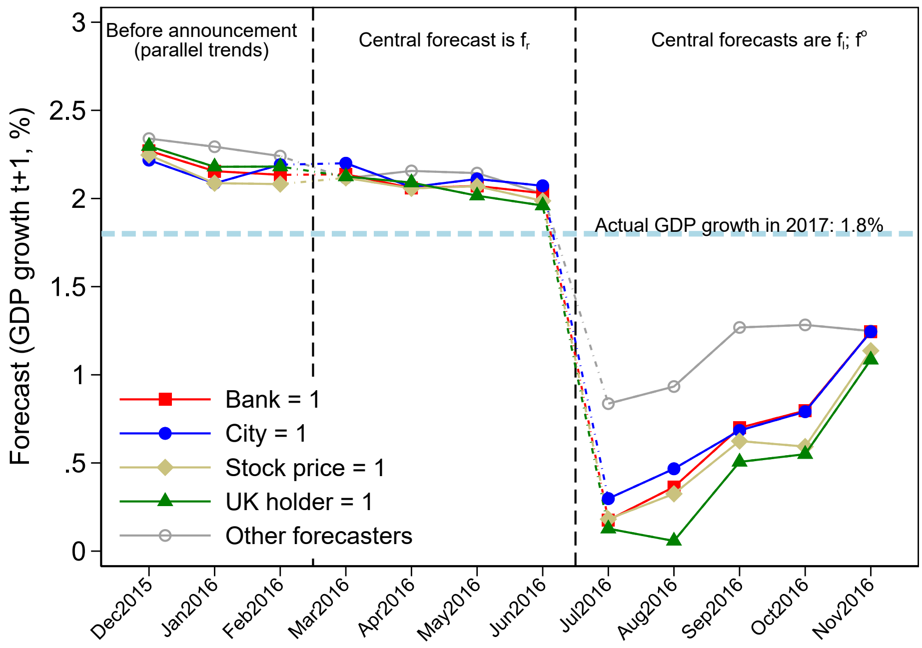

Figure 4: Pre- and Post-Referendum Trends

Notes: The lines represent the average forecast of the GDP growth rate in period $t+1$ released by forecasters with high stakes—i.e., with indicators Bank=1 (red); City=1 (blue); Stock Price=1 (sand); UK Holder=1 (green)—whereas the gray line represents the respective average forecast released by other forecasters—i.e., with Bank=0, City=0, Stock Price=0, and UK Holder = 0.

published in July 2016 (equal to 0.48%) and to the actual GDP growth for 2017 (equal to 1.8%).[25] Column (2) reflects our estimate that forecasters in the City of London's financial district overestimated the negative impact of a victory for Leave on GDP by 0.52 percentage point more than other forecasters.

In columns (3) and (4), forecasters exposed to a sizable loss in stock market capitalization and forecasters with a higher exposure to UK investors overestimated the economic downturn associated with the Brexit referendum outcome by 0.47–0.51 percentage point more than their competitors. In all columns, we also observe that forecasters with high and low stakes did not publish significantly different forecasts for Remain. Moreover, in line with Prediction 2, forecasts for Leave converged slowly in subsequent surveys, as the point estimates for the interaction term $Partisan_j \times f^{\circ}$ are consistently negative, smaller in magnitude than the coefficients on $Partisan_j \times f_{\ell}$ and statistically significant at the 5 or 10 percent levels.[26]

[25] Figure D5 shows the dispersion of forecasts published in June 2016 and in July 2016, respectively. The figure shows a clear cluster of partisan forecasters at the bottom of the distribution in the July survey, but not in the June survey.

[26] In Table D6, we estimate the partisan bias in each component of GDP growth. The results show that biased GDP growth forecasts arose from substantially biased investment and trade exposure growth forecasts. More specifically, banks overestimated the negative impact of Brexit on investments by 2.3 percentage points and on exports by 1.2 percentage points.

Table 1: Selective and Persistent Lies

	(1)	(2)	(3)	(4)
	Dep. var.: Forecast (GDP growth t+1, %)			
Partisan $\times f_r$	–0.059	0.022	–0.054	–0.028
	(0.084)	(0.088)	(0.086)	(0.085)
Partisan $\times f_\ell$	–0.731***	–0.518***	–0.468***	–0.511***
	(0.148)	(0.175)	(0.167)	(0.145)
Partisan $\times f^\circ$	–0.362**	–0.333*	–0.364*	–0.415**
	(0.178)	(0.182)	(0.189)	(0.194)
Observations	1,662	1,662	1,662	1,662
R^2	0.776	0.774	0.773	0.774
Fixed Effects	✓	✓	✓	✓
Survey Month Effects	✓	✓	✓	✓
Measure of Partisanship	Bank	City	Stock price	UK holders

Notes: The estimated equation is (2). Standard errors robust to two-way clustering at the forecaster and survey levels are shown in parentheses. Labels *, ** and *** represent significance levels of 10%, 5% and 1%.

Figure 5 provides further evidence of the validity of the parallel trends assumption. By focusing on our preferred measure of partisanship (Bank), we plot the estimated coefficients obtained from estimating equation (3). The results show that banks and other forecasters published comparable estimates before the announcement of the referendum. After the announcement, we observe no statistically significant differences between the two groups of forecasters when the central forecast is f_r (consistently with a selectively dishonest equilibrium) and, instead, a sizable difference when the forecast is f_ℓ, that slowly disappears over time when the forecast is f°.

5.2 Influential Lies

Constructing a proxy for influence is challenging because influence depends both on the frequency with which a forecaster's activity is covered in the mass media and on the extent to which voters are persuaded by the forecaster's assessments. Both dimensions are potentially endogenous to the positions the forecaster takes on the Brexit referendum. To test Prediction 3 of our theoretical framework, we use the coverage a forecaster received on all online media outlets and in five major UK newspapers in the year 2015 (i.e., before the announcement of the referendum) as a proxy for her influence. This approach mitigates endogeneity concerns for two reasons. First, the measure captures whether information produced by each forecaster is available to voters, independently of whether voters *choose* to consume that particular information. Second, the measure cannot be shaped by the position the forecaster takes on the Brexit referendum. Nonetheless, the results presented in this section should be interpreted with the caveat that media coverage of a forecaster

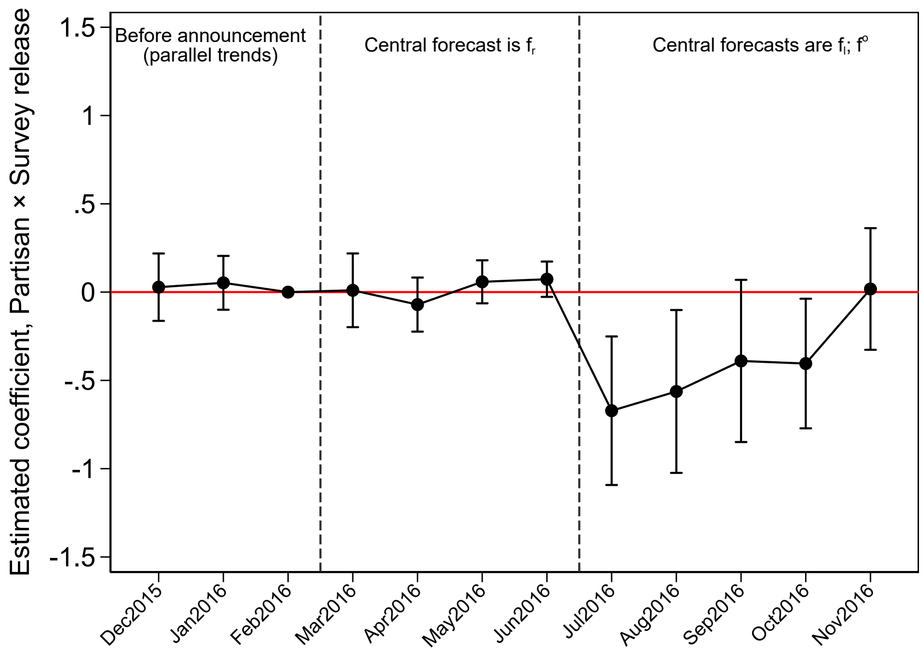

Figure 5: Dynamic Specification

Notes: The estimated equation is (3) and 95% confidence intervals are based on standard errors robust to two-way clustering at the forecaster and survey levels. The measure of partisanship is the binary indicator Bank.

is likely to correlate with other forecaster characteristics, such as size.

We construct three measures of influence. The first measure is an indicator equal to 1 if the number of mentions of a forecaster on any media source indexed in Google News was above the median.[27] Indeed, this measure attempts to capture the exposure of each forecaster to any online media available in the UK. The second and the third indicators focus on four major UK general-interest daily newspapers (*The Times*, *The Daily Mail*, *The Guardian*, and *The Telegraph*), and a daily newspaper specializing in the financial sector (*Financial Times*).[28] In turn, the second indicator is assigned a value of 1 if the sum of mentions in any of the five reference newspapers exceeds the median. The third indicator takes the value of 1 if the sum of mentions of a forecaster across the five reference newspapers—weighted by the share of readers of each newspaper in the UK population according to Wave 8 of the *British Election Study*—was above the median.[29]

[27] Panel (a) of Figure D6 shows the log of the number of articles mentioning each forecaster on any online media outlet covered by Google News.

[28] Panel (b) of Figure D6 shows the total coverage of each forecaster across the five reference newspapers while Panels (c)–(g) document each forecaster's coverage in each of the five above-mentioned newspapers. The most covered forecaster was mentioned 42 times in the Daily Mail (i.e., almost once per week) and 101 times in the Financial Times (i.e., twice per week). Table D5 reports the correlation between the number of articles that mentioned a forecaster in each media outlet.

[29] See Appendix C for additional details about the measures.

Formally, we construct the variable

$$Coverage_j^w = \sum_k \iota_k \times Mentions_{j,k}, \tag{4}$$

where ι_k denotes the readership share of newspaper k, and $Mentions_{j,k}$ denotes the historical coverage of forecaster j in newspaper k. Then, the binary indicator for influence is constructed by assigning the value of 1 to forecasters above the median value of $Coverage_j^w$.

We then estimate the following regression models:

$$\begin{aligned} f_{j,m}^{t+1} = {} & \beta_1 Influential_j \times \underbrace{\mathbb{1}(k_m \in [-4,-1])}_{f_r} + \beta_2 Influential_j \times \underbrace{\mathbb{1}(k_m = 0)}_{f_\ell} \\ & + \beta_3 Influential_j \times \underbrace{\mathbb{1}(k_m \in [1,5])}_{f^\circ} + \theta_j + \delta_m + \varepsilon_{j,m}, \end{aligned} \tag{5}$$

and

$$\begin{aligned} f_{j,m}^{t+1} = {} & \beta_1 Partisan_j \times \underbrace{\mathbb{1}(k_m \in [-4,-1])}_{f_r} + \beta_2 Influential_j \times \underbrace{\mathbb{1}(k_m \in [-4,-1])}_{f_r} \\ & + \beta_3 Partisan_j \times \underbrace{\mathbb{1}(k_m = 0)}_{f_\ell} + \beta_4 Influential_j \times \underbrace{\mathbb{1}(k_m = 0)}_{f_\ell} \\ & + \beta_5 Partisan_j \times \underbrace{\mathbb{1}(k_m \in [1,5])}_{f^\circ} + \beta_6 Influential_j \times \underbrace{\mathbb{1}(k_m \in [1,5])}_{f^\circ} + \theta_j + \delta_m + \varepsilon_{j,m}, \end{aligned} \tag{6}$$

where $Influential_j$ is defined using each of the three indicators defined above.[30]

We detect a strong correlation between influence and the magnitude of bias estimated in connection with the Brexit referendum. In columns (1) and (2) of Table 2, we measure influence using the number of mentions of each forecaster on the universe of media outlets indexed in Google News. Consistently with Prediction 3, we find a strong and negative coefficient on $Influential_j \times f_\ell$ and $Influential_j \times f^\circ$—which holds also when controlling for the effect of partisanship. Specifically, we estimate that more influential forecasters released, on average, more pessimistic estimates for GDP growth in the event of Leave. The estimated effect is sizable and amounts to 0.76 percentage points. The results remain qualitatively unaltered when we use the second or the third measures of influence. Importantly, the coefficient on $Partisan_j \times f_\ell$ and the coefficient on $Partisan_j \times f^\circ$ remain negative and statistically significant also when accounting for the impact of influence.

[30] Notice that Prediction 3 of the theoretical framework is about the marginal effect of influence on the forecast bias and not on the interaction between partisanship and influence.

Table 2: Influential Lies

	(1)	(2)	(3)	(4)	(5)	(6)
	Dep. var.: Forecast (GDP growth t+1, %)					
Influential $\times f_r$	0.064	0.130*	0.240***	0.252***	0.179**	0.185**
	(0.081)	(0.076)	(0.072)	(0.072)	(0.075)	(0.075)
Partisan $\times f_r$		-0.123		-0.084		-0.064
		(0.077)		(0.071)		(0.076)
Influential $\times f_\ell$	-0.935***	-0.761***	-0.421**	-0.263*	-0.375**	-0.259**
	(0.147)	(0.128)	(0.160)	(0.133)	(0.162)	(0.128)
Partisan $\times f_\ell$		-0.321***		-0.670***		-0.688***
		(0.115)		(0.136)		(0.134)
Influential $\times f^\circ$	-0.441***	-0.348***	-0.392***	-0.351***	-0.389***	-0.351***
	(0.137)	(0.114)	(0.142)	(0.113)	(0.143)	(0.115)
Partisan $\times f^\circ$		-0.198		-0.319**		-0.321**
		(0.153)		(0.150)		(0.149)
Observations	1,662	1,662	1,662	1,662	1,662	1,662
R^2	0.780	0.782	0.776	0.781	0.775	0.781
Fixed Effects	✓	✓	✓	✓	✓	✓
Survey Month Effects	✓	✓	✓	✓	✓	✓
Measure of Influence	GNews	GNews	Newsp. sum	Newsp. sum	Newsp. w. sum	Newsp. w. sum

Notes: In columns (1), (3), and (5), the estimated equation is (5). In columns (2), (4), and (6), the estimated equation is (6). The measure of partisanship is the binary indicator Bank. Standard errors robust to two-way clustering at the forecaster and survey levels are shown in parentheses. Labels *, ** and *** represent significance levels of 10%, 5% and 1%.

6 Distinguishing Strategic Manipulation from Alternative Mechanisms

Our empirical findings are consistent with the three predictions of our theoretical framework. Some of these predictions, however, are also consistent with alternative explanations for the observed dispersion in forecasts. In this section, we argue that these alternative accounts are unlikely to be consistent with *all* the predictions of our framework and *all* the empirical evidence we have presented. Where relevant, we perform additional tests to further reassure about our interpretation.

6.1 Demand-Driven Catering

Influential forecasters may have published pessimistic Leave forecasts in response to demand-side market incentives—that is, because readers of the media outlets that cited them wanted negative news about Brexit. This mechanism can account for some of the patterns predicted by our model. In particular, demand-driven incentives may generate selectively pessimistic Leave forecasts if consumers favoring Remain demand information that confirms their prior views. They may also generate a relationship between media exposure and forecast bias, since forecasters with greater media exposure have stronger

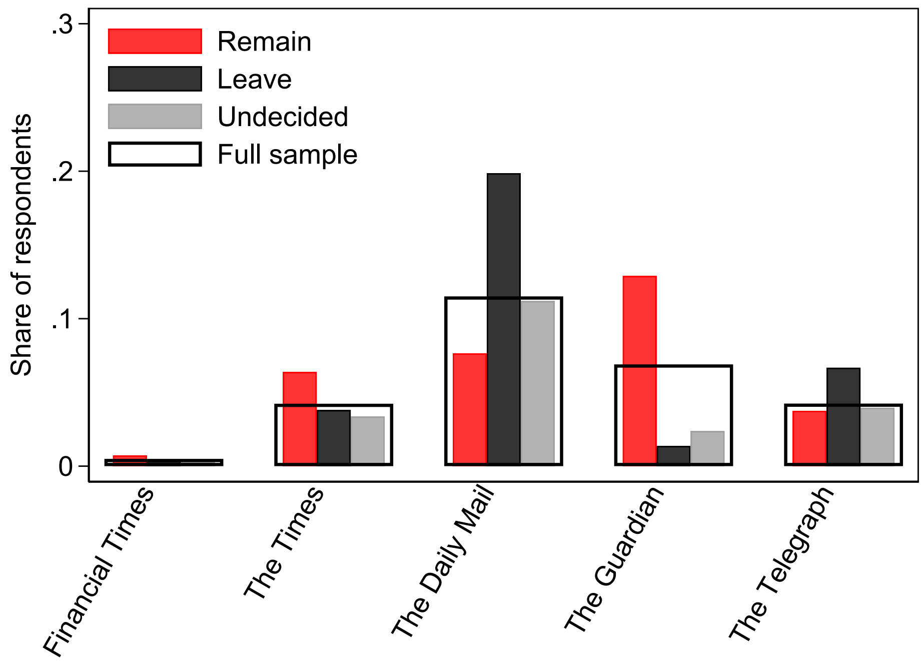

Figure 6: Newspaper Consumption by Voting Intention

Notes: The chart shows the share of respondents in Wave 8 of the *British Election Study* panel who mentioned each newspaper as the one they read most often. The sample is divided based on the self-reported voting intention in the EU Membership referendum.

incentives to cater to their audience. Hence, the relationship between influence and bias documented in Table 2 does not, by itself, distinguish demand-driven catering from our influence mechanism.

The two mechanisms have, however, different implications for the type of audience associated with more biased forecasts. Under demand-driven catering, pessimistic Leave forecasts should be particularly pronounced among forecasters exposed to Remain-oriented readers. By contrast, if forecasters bias their reports to influence the referendum, reaching voters whose decision can potentially be changed—such as undecided or Leave-leaning voters—should be particularly valuable. We investigate this distinction by decomposing our measure of influence according to the voting intentions of the audience reached by each forecaster.

Our data allow us to leverage the difference in the exposure of each forecaster to voters with different views on Brexit. To this end, we combine our measures of a forecaster's coverage on each of the five newspapers mentioned in Table 2 with the newspaper's diffusion among voters with specific voting intentions. Figure 6 reports the readership shares in the full voting-age population and by self-reported voting intention. The figure documents that readership shares in the general population were heterogeneous across voting intentions.

The predictions of the model would find empirical support if the effects estimated in

Table 3: Influential Lies: Decomposing Influence

	(1)	(2)	(3)	(4)
	Dep. var.: Forecast (GDP growth t+1, %)			
Leave-oriented × Influential $\times f_r$			-0.172*	-0.164*
			(0.087)	(0.093)
Influential $\times f_r$	0.201**	0.203**	0.269***	0.268***
	(0.078)	(0.078)	(0.082)	(0.087)
Partisan $\times f_r$		-0.053		-0.026
		(0.076)		(0.079)
Leave-oriented × Influential $\times f_\ell$			-0.793***	-0.544***
			(0.175)	(0.173)
Influential $\times f_\ell$	-0.404**	-0.335**	-0.010	-0.073
	(0.158)	(0.130)	(0.200)	(0.180)
Partisan $\times f_\ell$		-0.700***		-0.614***
		(0.140)		(0.150)
Leave-oriented × Influential $\times f^\circ$			-0.235	-0.147
			(0.183)	(0.153)
Influential $\times f^\circ$	-0.384***	-0.394***	-0.277	-0.326**
	(0.140)	(0.118)	(0.177)	(0.159)
Partisan $\times f^\circ$		-0.374**		-0.357**
		(0.154)		(0.155)
Observations	1,662	1,662	1,662	1,662
R^2	0.775	0.782	0.778	0.783
Fixed Effects	✓	✓	✓	✓
Survey Month Effects	✓	✓	✓	✓

Notes: In column (1), the estimated equation is (5). In column (2), the estimated equation is an augmented version of (5) in which we add all interaction terms between the $Influential_j$ dummy and the dummy $LeaveOriented_j$. In column (3), the estimated equation is (6). In column (4), the estimated equation is an augmented version of (6) in which we add all interaction terms between the $Influential_j$ dummy and the dummy $LeaveOriented_j$. In all columns, the measure of partisanship is the binary indicator Bank, while the measure of influence is based on the number of mentions on the top 5 UK-based newspapers, weighted by their readership shares among undecided voters. Standard errors robust to two-way clustering at the forecaster and survey levels are shown in parentheses. Labels *, ** and *** represent significance levels of 10%, 5% and 1%.

Table 2 were present among forecasters that have a historical record of significant coverage in newspapers consumed by many undecided voters (i.e., voters that self-declare themselves as being on the fence between choosing Remain or Leave). Formally, we construct an indicator equal to 1 for forecasters whose number of mentions in the five reference newspapers, weighted by the share of readers of each newspaper among undecided voters according to Wave 8 of the *British Election Study*, was above the median number. More precisely, we measure $Coverage_j^{wu}$ by replacing ι_k in (4) with the readership share of newspaper k among undecided voters ι_k^u, and we create an indicator equal to 1 for forecasters above the median value of $Coverage_j^{wu}$. In Table 3, we report the coefficients obtained estimating a version of equation (5) and equation (6) in which the $Influential_j$ indicator is constructed as specified just above. In columns (1) and (2), we find that forecasters that were more influential among undecided voters tend to publish more biased forecasts. This result also holds when controlling for the forecaster's partisanship.

Figure 6 also documents that the readership shares among pro-Remain and pro-Leave voters are substantially different across newspapers. This evidence allows us to disentangle the key mechanism underlined by the theoretical framework from an alternative story. To this end, we estimate versions of equations 5 and 6 in which $Coverage_j^{wu}$ is used to construct the binary measure of influence and to which we add an interaction term between each $Influential_j$ term and the indicator $LeaveOriented_j$. The indicator $LeaveOriented_j$ is equal to 1 if the number of mentions in any of the five newspapers weighted by the share of Leave-leaning readers of each newspaper exceeds the number of mentions weighted by the share of Remain-leaning readers.[31]

Columns (3) and (4) of Table 3 report that, among the forecasters most read by undecided voters, those with a historical record of coverage among Leave-oriented voters published the most pessimistic Leave forecasts.

This pattern is the opposite of what demand-driven catering to prior-confirming information would predict. If forecasters catered to their audience, pessimistic assessments of Brexit should be particularly pronounced among forecasters exposed to Remain-oriented readers, who were more likely to demand information consistent with their prior views. Instead, the largest biases are associated with forecasters reaching undecided or Leave-oriented voters—precisely the audiences for which pessimistic forecasts had greater scope to affect voting decisions. We therefore interpret the decomposition of influence as evidence against demand-driven catering and consistent with strategic voter manipulation.

6.2 Biased Beliefs: Panic and Irrational Pessimism

Differences in the way forecasters interpret the available data could all generate dispersion in forecasts, whether these differences stem from biased beliefs (for instance, overconfidence), from differing degrees of pessimism, or from differing speeds at which new data are incorporated into forecasting models. In other contexts, forecaster bias has been linked to overconfidence (e.g., Broer and Kohlhas, 2024), inattention (e.g., Giacomini et al., 2020), and irrational pessimism (Batchelor, 2007). Also, forecaster stakes in the referendum might correlate with political leanings, and there is evidence that experts and consumers tend to interpret information in ways that align with their political orientation (Kempf and Tsoutsoura, 2021; Gerber and Huber, 2009).[32]

[31] Formally, we replace ι_k in (4) with the readership shares of newspaper k among Remain-leaning voters ι_k^r and the readership shares of newspaper k among Leave-leaning voters ι_k^ℓ. Then, $LeaveOriented_j$ takes the value of 1 if $Coverage_j^{w\ell} > Coverage_j^{wr}$.

[32] Kempf and Tsoutsoura (2021) compare the behavior of credit rating analysts that are politically aligned (vs. not aligned) with the party of the incumbent U.S. President. They find that non-aligned analysts adjust downwards the rating of firms more frequently. Gerber and Huber (2009) compare aligned and not aligned U.S. consumers: relative to the latter, politically aligned consumers report more optimistic views about the economy and tend to consume more. Our work is agnostic about whether ideology

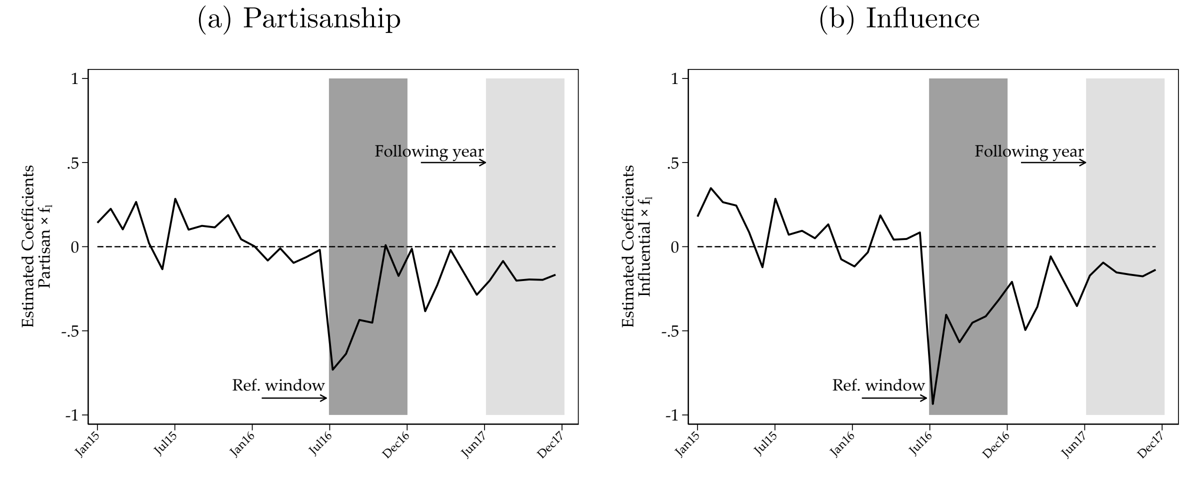

Figure 7: Estimated Bias at Different Points in Time

Notes: In panel (a), we estimate (2) by assuming that a placebo referendum occurs in every month between January 2015 and April 2018. In panel (b), we estimate (5) by assuming that a placebo referendum occurs in every month between January 2015 and April 2018. In both panels, the solid line represents the estimated coefficient of β_2 obtained at each iteration. The dark gray area includes the coefficients between July and November 2016. The light gray area includes the estimates for surveys released between July and November 2017. The measure of partisanship is the binary indicator Bank. The measure of influence is the binary indicator GNews.

Each of these biased-belief explanations of forecast dispersion could be consistent with our first prediction, that partisan forecasters exhibit a larger Brexit-related bias, since a correlation between partisanship and a forecaster's interpretation of the data cannot be excluded *a priori*.[33]

The second prediction, that misreporting persists for a time and eventually disappears, sits less comfortably with biased-belief explanations, as differences in the interpretation of data should persist beyond the referendum. We cannot rule out, however, that the information arriving after the referendum clarified the consequences of Brexit and thereby narrowed the scope for divergent interpretations.

The third prediction is the hardest to reconcile with biased-belief explanations. We cannot rule out a correlation between influence and interpretation of the data, but such a correlation seems unlikely: unlike partisanship, influence varies considerably even within a given class of forecasters (e.g., banks, think tanks, international organizations). Nevertheless, the evidence that forecast bias is increasing in influence is difficult to reconcile

matters also among macroeconomic forecasters; yet, the fact that the estimated forecast bias increases in a forecaster's influence allows us to conclude that ideological bias cannot be the key driver of our estimates.

[33] A subtle point deserves emphasis. Biased-belief explanations can accommodate a stronger bias for the forecasts associated with greater macroeconomic uncertainty, but not a stronger bias for those associated with the outcome less likely to prevail. Our strategic mechanism, by contrast, predicts stronger bias along both dimensions. The distinction is largely moot in our setting, however, since we argue that Leave was simultaneously the more uncertain outcome and the one expected to lose.

with the interpretation that our results reflect forecasters' biased beliefs about the impact of Brexit, and instead points toward intentional manipulation.

A particularly relevant form of biased beliefs in our setting is an irrationally pessimistic reaction to the uncertainty surrounding Brexit. The most natural "microfoundation" for this alternative conjecture would involve forecasters with high stakes "panicking" during a period of turbulence and thus producing unreliable forecasts. This conjecture would yield a prediction similar to our first prediction, but does not provide a rationale for the correlation between influence and bias nor does it help rationalize the gradual disappearance of the bias in the Leave forecasts after the vote.

If this interpretation were driving our results, one would expect similar differences between forecasters with and without stakes—and, where measurable, influence—during other periods of unusually high uncertainty or around major unexpected events. We examine this implication using two additional exercises. First, in Figure 7, we present estimates from the same regression models as in (2) and (5) at different points in time and show that forecasters with and without stakes or influence published very similar estimates throughout 2017, especially in the releases published exactly one year after this paper's main focus—and, hence, at a constant forecast horizon.[34,35]

Second, in Figure 8, we present estimates from the same regression model as in (2) before and after a host of economy-wide shocks. We consider the unexpected onset of the 2008 financial crisis, the 2005 referendum in France that halted the approval process of the EU Constitution and the 9/11 terrorist attacks in the US.[36,37] We do not find evidence of differences in the behavior of forecasters with and without stakes in the UK economy: the coefficients are not statistically distinguishable from zero in the surveys of professional forecasters after each event.

Taken together, these exercises provide little evidence that high-stakes forecasters systematically become relatively more pessimistic during periods of high uncertainty or following major unexpected events. Combined with the temporary nature of the Brexit-

[34] As documented in panel (c) of Figure A1, economic policy uncertainty remained at an all-time high throughout 2017, as debates on Brexit policies dragged on.

[35] Figure 7 also documents—consistent with Figure 5—that estimates obtained prior to the referendum are centered around zero and negligible in magnitude.

[36] We identify the beginning of the financial crisis with the bankruptcy of Lehman Brothers Holdings Inc. on September 15, 2008.

[37] We do not find any adverse events comparable to the withdrawal from the European Union during the period of our primary data (from January 2012 to April 2018). We therefore extend the sample back to 1998 by digitizing older publications of the *Forecasts for the UK Economy* collection from the UK National Archives. We investigate forecasters' behavior around the time of the 2001 terrorist attack by estimating equation (2) for a sample between 1998 and 2003, whereas we explore the reaction to the result of the French referendum on the EU Constitution by restricting the sample to observations between 2000 and 2007 and the reaction to the unexpected beginning of the financial crisis by restricting the sample to observations between 2004 and 2010. The set of forecasters surveyed changes slightly across the different samples. Notice that we cannot estimate versions of equation (5) in this extended sample.

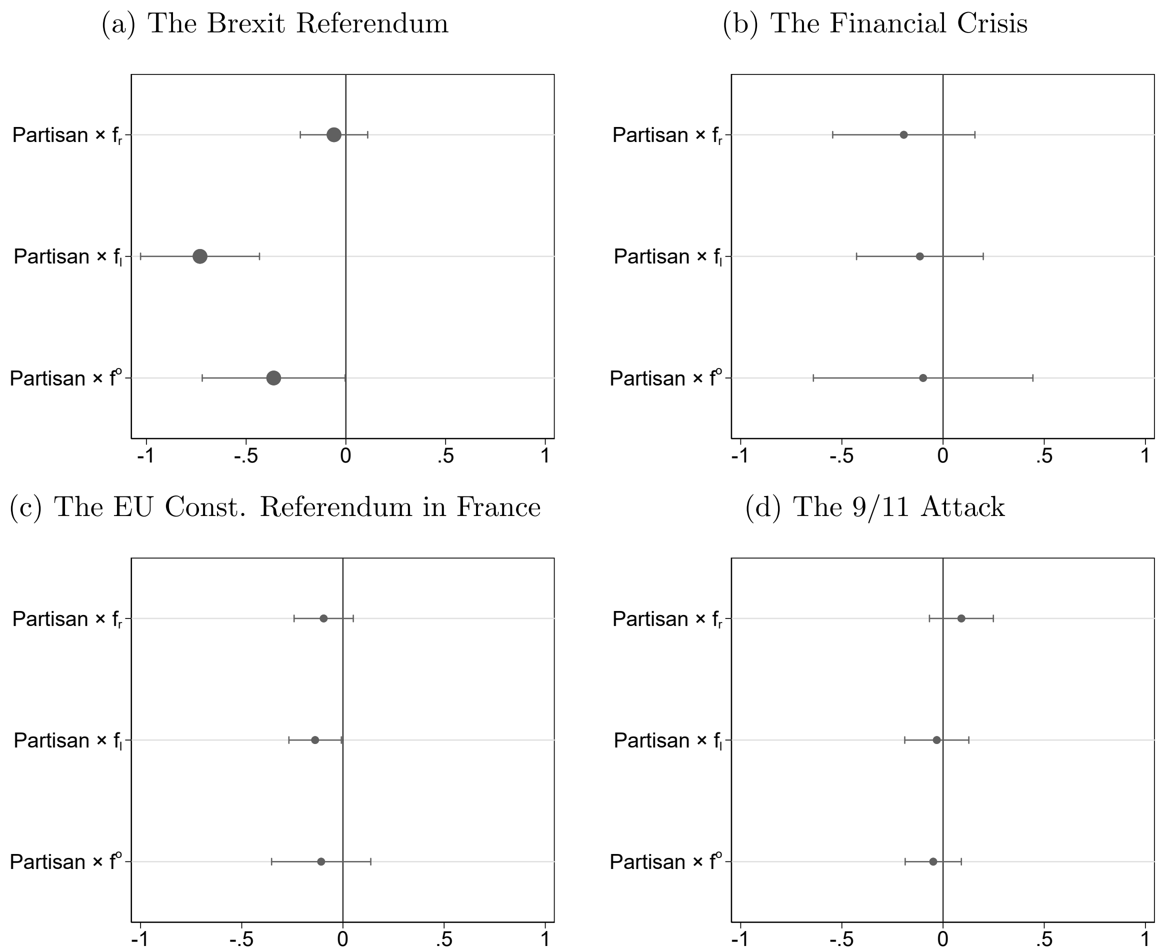

Figure 8: Effects during the EU Membership Referendum and in Connection with other Events

Notes: The figure reports estimated coefficients and 95% confidence intervals resulting from estimating (2) in connection with the Brexit referendum, the bankruptcy of Lehman Brothers, the EU Constitution referendum in France and the 9/11 terrorist attacks. The measure of partisanship is the binary indicator Bank. Estimates of 95% confidence intervals are based on standard errors robust to two-way clustering at the forecaster and survey levels.

related bias and its relationship with forecaster influence, these results make panic or irrational pessimism an unlikely explanation for the overall pattern of evidence.

6.3 Differences in Forecasting Ability

A separate possible explanation for the observed forecast dispersion is that forecasters with high stakes or influence systematically differ from other forecasters in their forecasting ability. Rather than reflecting different beliefs about Brexit, the differences documented above could then reflect systematic differences in forecast performance across groups. This explanation could account for poorer forecasts among partisan or influential forecasters, but it would also imply that these groups perform systematically differently outside the specific context of the Brexit referendum. We therefore examine whether the same forecasters were systematically less accurate before the referendum.

To assess this explanation, we regress the absolute forecast error on survey fixed effects and the $Partisan_j$ (resp. $Influential_j$) indicator (see Table D7 and Table D8) and find no evidence of differences in forecast performance between forecasters. The results show that the $Partisan_j$ and the $Influential_j$ coefficients are indistinguishable from zero between 2012 and 2015 and that the $Partisan_j$ indicator is statistically indistinguishable from zero in an extended sample that includes all monthly forecasts released between 1998 and 2015. Evidence speaks against the possibility that high-stakes or high-influence forecasters have lower forecasting ability than their competitors.

Thus, the differences observed around the Brexit referendum do not appear to reflect systematic differences in forecasting performance across the groups of forecasters that underpin our tests of Predictions 1 and 3.

7 Robustness Checks

Figure 4 shows that the parallel trends assumption is likely satisfied in our context. Moreover, forecasts reported in Consensus Economics (2016b) reflect, on average, the views expressed before the referendum (see Figure 3). In this section, we perform several additional robustness checks to validate our empirical strategy.

Our results do not depend on the number of surveys included in the analysis. Figures D7 and D8 report the estimated coefficients and confidence intervals obtained estimating equations (2) and (5) using several different temporal windows. The estimated coefficients are stable for all specifications and are not sensitive to the data's sample period.

Our results are also robust to alternative criteria to select non-partisan forecasters. In Table D9, we exclude from the sample other forecasters that might have had high stakes in the referendum outcomes, such as the confederation of UK firms and international institutions. Reassuringly, the results are similar and stronger than those in our main specification.

Finally, the statistical significance of our estimates does not depend on our specific choice to impose standard errors that are robust to two-way clustering at the forecaster and survey month levels. We show in Figures D9 and D10 that imposing other assumptions on standard errors yields conclusions analogous to our main results.

8 Concluding Remarks

The political economy literature has devoted a lot of attention to special interest groups and the media. The consensus is that these actors sometimes feed voters biased information and ultimately influence their vote. We suggest that macroeconomic forecasters might behave in the same way.

We develop a theoretical framework to analyze the incentives of a biased forecaster in the period leading up to a referendum. The forecaster is better informed than the voter about the future state of the economy under each of the two policies available to the voter. The forecaster can manipulate forecasts to influence the voter's beliefs, but issuing biased forecasts is costly in terms of reputation. Under the assumptions of our model, it is optimal for the forecaster to selectively bias the forecast associated with one of the two policies. We test the model's predictions in the context of the Brexit referendum.

The model is purposely simple. In particular, in each scenario only two outcomes are possible, and the forecaster observes a signal perfectly correlated with the outcomes. Relaxing either of these assumptions would render the analysis much less tractable. Here we simply note that in any generalized version of our model, the property that reports are at least as dishonest for the scenario that (i) is the least likely and (ii) is associated with more macroeconomic uncertainty than the other scenario would hold "in the limit": in equilibrium, a partisan forecaster cannot report honestly on a scenario that never occurs and lie on a scenario that occurs with certainty, and has no reason to lie on a scenario associated with infinitesimal macroeconomic uncertainty.

The empirical results are consistent with the predictions of the theoretical framework. First, forecasters with an interest in supporting Remain released GDP growth forecasts contingent on Leave that were more pessimistic than those released by neutral forecasters. Second, the two groups of forecasters published on average identical forecasts contingent on Remain. Third, after the referendum, the bias slowly disappeared over a period of a few months. Fourth, the forecasters' degree of influence on voters, as measured by their coverage in the media, was an important determinant of the bias in their forecasts. Alternative explanations, such as non-strategic bias or demand-driven bias arising from voters consuming news aligned with their priors, are difficult to reconcile with the data.

A key identifying assumption underlying our empirical design is that the first post-referendum survey provides a reasonable proxy for the Leave-contingent forecast that each forecaster made available to voters immediately prior to the vote. To assess its validity, we show that the assumption holds in the aggregate: the average Leave-contingent forecast did not change between the periods immediately before and after the referendum. We further document that the assumption holds for the limited subset of forecasters for whom we were able to retrieve pre-referendum Leave-contingent estimates comparable to our post-referendum data. This evidence is reassuring but does not entirely rule out potential violations, such as the unlikely scenario in which one group of forecasters revised their estimates upward while another revised them downward. A failure of the assumption would lead us to underestimate strategic bias if forecasters had already begun to converge back toward unbiased estimates, or, conversely, to overestimate it if partisan forecasters irrationally overreacted to the shock.

Biased forecasts might affect the welfare of both voters and forecasters. In the Brexit referendum, voters did not face any welfare loss compared to a counterfactual in which all forecasts were unbiased, as the referendum selected the forecasters' least favorite outcome - Leave. In contrast, forecasters with higher stakes and influence arguably paid a reputation cost. Additional welfare losses might have occurred if consumers and investors based their consumption and investment decisions on biased forecasts.

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A Leave: Winning Odds and Uncertainty

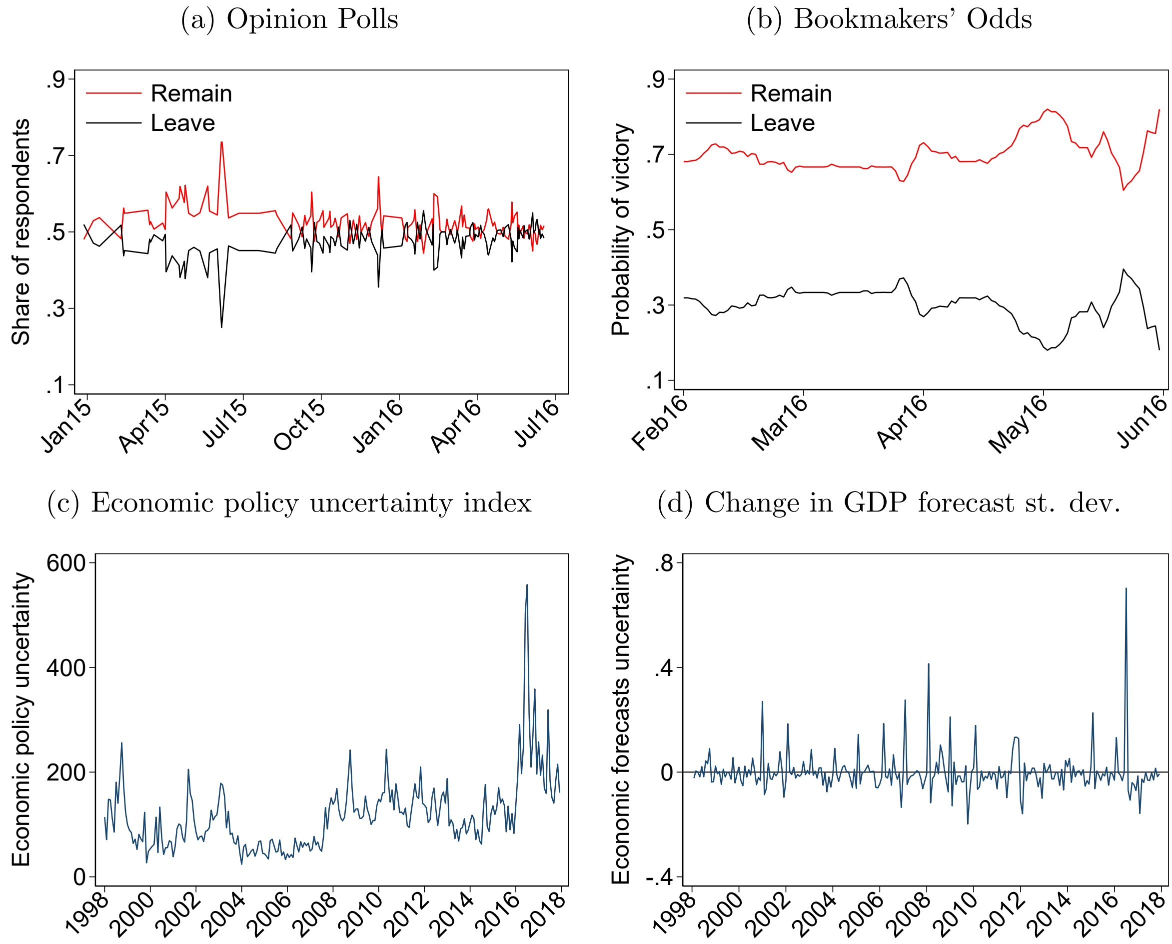


Figure A1: Opinion Polls and Bookmakers' Odds Approaching the Referendum

Notes: Panel (a) reports the daily averages of all opinion polls recorded by Financial Times Research between January 2015 and June 22, 2016. Panel (b) documents the daily average of the odds released by all bookmakers recorded by the portal Betdata.io between the announcement of the referendum date and June 22, 2016. Panel (c) shows the United Kingdom's *Economic Policy Uncertainty Index* measured monthly according to PolicyUncertainty.com. Panel (d) plots monthly changes in the standard deviation of GDP growth forecasts for the next period among forecasters surveyed by HM Treasury. Both Panels (c) and (d) refer to the period between January 1998 and December 2017.

In Panels (a) and (b) of Figure A1, we document that Remain was considered the most likely outcome before the referendum. According to 66% of the opinion polls, Remain led, often with a predicted winning margin of at least 5 percentage points (Figure A1, panel (a)). In the final days before the vote, bookmakers assigned a probability of approximately 85% to a victory for Remain (Figure A1, panel (b)). Macroeconomic forecasters too predicted a victory for Remain: Consensus Economics (2016a) reports that forecasters assigned a probability of 63% to a victory for Remain.

In panels (c) and (d) of Figure A1, we provide evidence that the economic consequences of leaving the EU were perceived as more uncertain than the consequences of remaining. Panel (c) plots the *Economic Policy Uncertainty Index* of the United Kingdom over the period 1998-2018. Economic policy uncertainty was historically relatively low until early 2016, when, in connection with the announcement of the referendum, it jumped above 300 points for the first time in recent history. The index peaked right after the referendum at approximately 500 points and remained at values higher than the historical trends thereafter. Panel (d) reports the monthly variation in the standard deviation of forecasts published by independent forecasters, as included in the collection *Forecasts for the UK economy* collected and released by HM Treasury over the period 1998-2018. Also this measure peaked precisely at the time of the referendum.

B Proofs

B.1 Proof of Proposition 1

We prove Proposition 1 by way of a series of intermediate results. We focus throughout on equilibria such that, if the market observes an outcome draw that does not match the forecast (either $f_p \neq y_p$ for some p, or $f^\circ \neq y^\circ$), and this observation is outside of the equilibrium path, then $\mu^\circ = 0$.

The first two remarks describe the voter's choice in an honest equilibrium. Let

$$\overline{\phi} := \frac{1}{2} - \frac{\overline{\ell} + \underline{\ell}}{2} + \frac{\overline{r} + \underline{r}}{2};$$
$$\phi(f_\ell, f_r) := \chi \left(\frac{1}{2} - f_\ell + f_r \right) + (1 - \chi)\overline{\phi}.$$

Remark 1. *In equilibrium, if the voter does not observe the initial forecasts, then he selects Remain with probability $\overline{\phi}$.*

Remark 2. *In an honest equilibrium, upon observing forecasts (f_ℓ, f_r), the voter selects Remain with probability $\phi(f_\ell, f_r)$.*

The first lemma describes the choice of final forecasts in an honest equilibrium.

Lemma B1. *In an honest equilibrium, $f^\circ = y^\circ$.*

Proof. The proof of the lemma follows from standard arguments. □

The following lemma establishes the first half of Proposition 1.

Lemma B2. *An honest equilibrium exists if and only if $\rho < \rho_1$, where*

$$\rho_1 := \frac{(1 - \phi(\underline{\ell}, \overline{r}))\psi}{(\overline{\ell} - \underline{\ell})\chi} \mu.$$

Proof. If $\rho = 0$, the argument is immediate. We focus in the rest of the proof on the case $\rho \in (0, \rho_1)$. To show that an honest equilibrium exists, we show that if market and voter expect the forecaster to adopt an honest strategy and to publish $f^\circ = y^\circ$, then it is optimal for the forecaster to do so.

It is easy to verify that, as long as the market holds the beliefs we just described, it is optimal for the forecaster to publish $f^\circ = y^\circ$, regardless of whether the forecaster published $(f_\ell, f_r) = (y_\ell, y_r)$ or not. So the only possible profitable deviations from the aforementioned strategy involve publishing $(f_\ell, f_r) \neq (y_\ell, y_r)$, while publishing $f^\circ = y^\circ$.

Let strategy $s((f'_\ell, f'_r), (y'_\ell, y'_r))$ of the forecaster require:

- $f^\circ = y^\circ$ regardless of the initial forecasts and of the policy selected by the voter;

- $(f_\ell, f_r) = (y_\ell, y_r)$ upon observing $(y_\ell, y_r) \neq (y'_\ell, y'_r)$;

- $(f_\ell, f_r) = (f'_\ell, f'_r)$ upon observing $(y_\ell, y_r) = (y'_\ell, y'_r)$.

Let $\Delta((f'_\ell, f'_r), (y'_\ell, y'_r))$ denote the difference in forecaster's expected payoff between strategy $s((f'_\ell, f'_r), (y'_\ell, y'_r))$ and the honest strategy, conditional on voter and market anticipating the honest strategy, that is:

$$\Delta((f'_\ell, f'_r), (y'_\ell, y'_r)) = \frac{1}{4}\left(\chi\rho(y'_\ell - f'_\ell + f'_r - y'_r) - \left(\mathbf{1}_{f'_r}\phi(f'_\ell, f'_r) + \mathbf{1}_{f'_\ell}(1 - \phi(f'_\ell, f'_r))\right)\psi \cdot \mu\right),$$

where $\mathbf{1}_{f'_p}$ is an index function that takes value 1 if $f'_p \neq y'_p$.

As $\rho > 0$, then

$$\Delta((\overline{\ell}, r'), (\underline{\ell}, r'')) < \Delta((\underline{\ell}, r'), (\underline{\ell}, r'')); \text{ and } \Delta((\ell', \underline{r}), (\ell'', \overline{r})) < \Delta((\ell', \overline{r}), (\ell'', \overline{r}))$$

for any $r', r'' \in \{\underline{r}, \overline{r}\}$ and $\ell', \ell'' \in \{\underline{\ell}, \overline{\ell}\}$. We can thus restrict attention to 5 deviations:

i. $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ for $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$;

ii. $(f_\ell, f_r) = (\underline{\ell}, \underline{r})$ for $(y_\ell, y_r) = (\overline{\ell}, \underline{r})$;

iii. $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ for $(y_\ell, y_r) = (\underline{\ell}, \underline{r})$;

iv. $(f_\ell, f_r) = (\overline{\ell}, \overline{r})$ for $(y_\ell, y_r) = (\overline{\ell}, \underline{r})$;

v. $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ for $(y_\ell, y_r) = (\overline{\ell}, \underline{r})$.

Note that:

$$\Delta((\underline{\ell}, \overline{r}), (\overline{\ell}, \overline{r})) = \Delta((\underline{\ell}, \underline{r}), (\overline{\ell}, \underline{r})) + \frac{1}{4}(\phi(\underline{\ell}, \overline{r}) - \phi(\underline{\ell}, \underline{r}))\psi \cdot \mu; \tag{B1}$$

$$= \Delta((\underline{\ell}, \overline{r}), (\underline{\ell}, \underline{r})) + \frac{1}{4}\left(\left((\overline{\ell} - \underline{\ell}) - (\overline{r} - \underline{r})\right)\chi \cdot \rho + (2\phi(\underline{\ell}, \overline{r}) - 1)\psi \cdot \mu\right); \tag{B2}$$

$$= \Delta((\overline{\ell}, \overline{r}), (\overline{\ell}, \underline{r})) + \frac{1}{4}\left(\left((\overline{\ell} - \underline{\ell}) - (\overline{r} - \underline{r})\right)\chi \cdot \rho + (\phi(\underline{\ell}, \overline{r}) - (1 - \phi(\overline{\ell}, \overline{r})))\psi \cdot \mu\right). \tag{B3}$$

As $\phi(\underline{\ell}, \overline{r}) > \phi(\underline{\ell}, \underline{r})$, then (B1) implies that deviation (ii) is not profitable whenever deviation (i) is not. As $\overline{\ell} - \underline{\ell} > \overline{r} - \underline{r}$ (Assumption (i)) and $\phi(f_\ell, f_r) > 1/2$ for any (f_ℓ, f_r) (immediate from Assumption (ii)) then (B2) and (B3) imply, respectively, that deviation (iii) and deviation (iv) are not profitable whenever deviation (i) is not. Finally,

$$\Delta((\underline{\ell}, \overline{r}), (\overline{\ell}, \underline{r})) - \Delta((\underline{\ell}, \overline{r}), (\overline{\ell}, \overline{r})) = \frac{1}{4}\left((\overline{r} - \underline{r})\chi \cdot \rho - \phi(\underline{\ell}, \overline{r})\psi \cdot \mu\right) < 0, \tag{B4}$$

where the inequality follows from $\rho \leq \rho_1$. Hence deviation (v) is not profitable whenever deviation (i) is not. To conclude the proof it is sufficient to show that deviation (i) is not profitable. This is the case as:

$$\Delta((\underline{\ell}, \overline{r}), (\overline{\ell}, \overline{r})) = \frac{1}{4}\left(\chi(\overline{\ell} - \underline{\ell})\rho - (1 - \phi(\underline{\ell}, \overline{r}))\psi \cdot \mu\right) \leq 0 \Leftrightarrow \rho \leq \rho_1.$$

$\square$

Let

$$\phi_{(\underline{\ell}, \overline{r})}(x) := \chi\left(\frac{1}{2} - \frac{\underline{\ell} + (1-\mu)\cdot x \cdot \overline{\ell}}{1 + (1-\mu)x} + \overline{r}\right) + (1-\chi)\overline{\phi}.$$

Remark 3. *In a selectively dishonest equilibrium, upon observing forecasts $(\underline{\ell}, \overline{r})$, the voter selects Remain with probability $\phi_{(\underline{\ell}, \overline{r})}(x)$. Upon observing forecasts $(f_\ell, f_r) \neq (\underline{\ell}, \overline{r})$ instead, the probability is $\phi(f_\ell, f_r)$.*

Remark 1 and Remark 3 together describe the voter's choice in a selectively dishonest equilibrium. Let

$$\begin{aligned}
\mu_{\overline{\ell}}(x) &:= \frac{\mu}{1 - (1-\mu)x}; \\
\mu_{\underline{\ell}}(x) &:= \frac{\mu}{1 + (1-\mu)x}; \\
\mu_{\underline{\ell}, \overline{\ell}}(x) &:= \frac{\mu(1-\eta)}{(1-\eta) + (1-\mu)x \cdot \eta}; \\
\mu_{\underline{\ell}, \underline{\ell}}(x) &:= \frac{\mu \cdot \eta}{\eta + (1-\mu)x(1-\eta)}.
\end{aligned}$$

In a selectively dishonest equilibrium, market and voter, upon observing forecasts $(f_\ell, f_r) = (\overline{\ell}, \overline{r})$ revise the probability that the forecaster is honest to $\mu_{\overline{\ell}}(x)$. In case $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$, they instead revise to $\mu_{\underline{\ell}}(x)$. If the market expects $f^\circ = y^\circ$, upon observing $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ and $f^\circ = \underline{\ell}$ and not observing (y_ℓ, y°), the market assigns probability $\mu_{\underline{\ell}, \underline{\ell}}(x)$ to the forecaster being honest. If instead $f^\circ = \overline{\ell}$, the probability is $\mu_{\underline{\ell}, \overline{\ell}}(x)$.

We now consider the choice of final forecast in a selectively dishonest equilibrium. We refer to a selectively dishonest equilibrium in which the equilibrium strategy of the forecaster requires to publish $f^\circ = y^\circ$ regardless of initial forecasts, choice of the voter and realization of y° as an *ex-post honest* equilibrium. We refer to a selectively dishonest equilibrium in which the equilibrium strategy of the forecaster requires to publish $f^\circ = \underline{\ell}$ with some probability $y \in (0, 1)$ upon observing $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$, publishing $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ and then observing $y^\circ = \overline{\ell}$, and to publish $f^\circ = y^\circ$ in any other case as an *ex-post dishonest* equilibrium.

The following function θ will play an important role. The function measures the expected gain from publishing $f^\circ = \underline{\ell}$ instead of $f^\circ = \overline{\ell}$ after having observed $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$ and $y^\circ = \overline{\ell}$ and having published $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$. The gain is conditional on the market anticipating (i) a selectively dishonest strategy; (ii) $f^\circ = \underline{\ell}$ after $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ and $y^\circ = \overline{\ell}$ with some probability $\overline{y}$ if $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$ and with some probability $\underline{y}$ if $(y_\ell, y_r) = (\underline{\ell}, \overline{r})$. So:

$$\theta(\underline{y}, \overline{y}, x) := \frac{(1-\psi-\psi^\circ)\eta \cdot \mu}{\eta + (1-\eta)(1-\mu)\underline{y} + (1-\mu)x\,(1-\eta+\eta\overline{y})} - \frac{(1-\psi)(1-\eta)\mu}{(1-\eta)(\mu + (1-\mu)(1-\underline{y})) + \eta x(1-\mu)(1-\overline{y})}.$$

Lemma B3. *A selectively dishonest equilibrium is either ex-post honest or ex-post dishonest.*

Proof. The same standard arguments that prove Lemma B1 also prove that in a selectively dishonest equilibrium, if either the forecaster publishes $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ and the voter selects Remain, or if the forecaster publishes $(f_\ell, f_r) \neq (\underline{\ell}, \overline{r})$, then the forecaster publishes $f^\circ = y^\circ$.

Consider, for a selectively dishonest equilibrium, the continuation following $(y_\ell, y_r) \in \{(\underline{\ell}, \overline{r}), (\overline{\ell}, \overline{r})\}$, $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$, and the voter selecting Leave.

First, it cannot be the case that the forecaster publishes $f^\circ = \overline{\ell}$ with some probability upon observing $y^\circ = \underline{\ell}$ while publishing $f^\circ = \underline{\ell}$ conditional on observing $y^\circ = \underline{\ell}$. In this case, publishing $f^\circ = \underline{\ell}$ instead of $f^\circ = \overline{\ell}$ upon observing $y^\circ = \underline{\ell}$ would be profitable, yielding a contradiction. Second, it cannot be the case that the forecaster publishes $f^\circ = \overline{\ell}$ with some probability upon observing $y^\circ = \underline{\ell}$ and publishes $f^\circ = \underline{\ell}$ with some probability upon observing $y^\circ = \overline{\ell}$. In this case, publishing $f^\circ = y^\circ$ would be profitable. Hence, if $y^\circ = \underline{\ell}$ then $f^\circ = \underline{\ell}$.

Consider now the expected gain from publishing $f^\circ = \underline{\ell}$ instead of $f^\circ = \overline{\ell}$. The gain is conditional on the market anticipating (i) a selectively dishonest strategy; (ii) $f^\circ = \underline{\ell}$ after $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ and $y^\circ = \overline{\ell}$ with some probability $\overline{y}$ if $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$ and with some probability $\underline{y}$ if $(y_\ell, y_r) = (\underline{\ell}, \overline{r})$. If $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$, by definition the expected gain is $\theta(\underline{y}, \overline{y}, x)$. If instead $(y_\ell, y_r) = (\underline{\ell}, \overline{r})$, the expected gain is:

$$\theta(\underline{y}, \overline{y}, x) - \psi\frac{\mu}{\mu + (1-\mu)(1-\underline{y})}.$$

So the expected gain is strictly larger for $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$ than for $(y_\ell, y_r) = (\underline{\ell}, \overline{r})$ (call this Prediction 1). Note that θ is decreasing in its first two arguments and $\theta(0, 1, x) < 0$ for any $x > 0$ (call this Prediction 2). These two observations together imply that in any selectively dishonest equilibrium $f^\circ = \underline{\ell}$ with some probability $y < 1$ for $y^\circ = \overline{\ell}$ following $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$ and $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$, while following $(y_\ell, y_r) = (\underline{\ell}, \overline{r})$, the forecaster publishes $f^\circ = y^\circ$. This observation concludes the proof of the lemma. □

The following lemma characterizes the forecaster's strategy in a selectively dishonest equilibrium.

Lemma B4. *Let $\rho > \rho_1$. In a selectively dishonest equilibrium, x is an increasing function of ρ. If $\theta(0,0,1) \geq 0$, there exists a ρ^* such that $\rho^* \geq \rho_1$ and:*

- *for $\rho < \rho^*$ any selectively dishonest equilibrium is ex-post honest;*
- *for $\rho > \rho^*$ any selectively dishonest equilibrium is ex-post dishonest.*

If instead $\theta(0,0,1) < 0$, every selectively dishonest equilibrium is ex-post honest.

Proof. Let

$$\begin{aligned} &f(x) := \phi(\bar{\ell},\bar{r})\rho + \mu_{\bar{\ell}}(x), \\ &g(x) := \phi_{(\underline{\ell},\bar{r})}(x)\left(\rho + \mu_{\underline{\ell}}(x)\right) + (1-\phi_{(\underline{\ell},\bar{r})}(x))(1-\psi)\left(\eta \cdot \mu_{\underline{\ell},\bar{\ell}}(x) + (1-\eta)\mu_{\underline{\ell},\underline{\ell}}(x)\right). \end{aligned}$$

In a selectively dishonest equilibrium, the forecaster's expected payoff associated with publishing $(f_\ell, f_r) = (\bar{\ell},\bar{r})$ upon observing $(y_\ell, y_r) = (\bar{\ell},\bar{r})$ is given by $f(x)$. If the equilibrium is ex-post honest, the expected payoff associated with publishing $(f_\ell, f_r) = (\underline{\ell},\bar{r})$ is given by $g(x)$.

Functions f and g are both differentiable, and $f_x > 0 > g_x$. For $\rho > \rho_1$, it is the case that $f(0) < g(0)$, hence either $f(1) \geq g(1)$, in which case there exists a $\tilde{x} \in (0,1]$ such that $f(\tilde{x}) = g(\tilde{x})$, or else $f(1) < g(1)$, in which case $f(x) < g(x)$ for all $x \in (0,1]$. Thus in every ex-post honest equilibrium $x = x_1(\rho)$ where:

$$x_1(\rho) := \begin{cases} \tilde{x} & \text{if } f(1) \geq g(1); \\ 1 & \text{if } f(1) < g(1). \end{cases}$$

As long as $f(1) \geq g(1)$, then $\tilde{x}$ is well defined and is a strictly increasing function of ρ. Hence x_1 is an increasing function of ρ.

Function θ is decreasing in its first two arguments and increasing in its third argument. Hence if $\theta(0,0,1) \leq 0$ every selectively dishonest equilibrium is ex-post honest.

Suppose for the rest of the proof that $\theta(0,0,1) > 0$. A unique $x^* \in (0,1]$ thus exists such that $\theta(0,0,x^*) = 0$. Let ρ^* be implicitly defined by $\tilde{x}(\rho^*) = x^*$ (such ρ^* exists as for ρ sufficiently large $\tilde{x}(\rho) > 1$).

For $x \in [x^*, 1]$, let $y(x)$ be defined implicitly by

$$\theta(0, y(x), x) = 0.$$

Let

$$h(x) := \phi_{(\underline{\ell},\bar{r})}(x)\left(\rho + \mu_{\underline{\ell}}(x)\right) + (1-\phi_{(\underline{\ell},\bar{r})}(x))\left(\psi^\circ(1-\eta)\mu_{\underline{\ell},\underline{\ell}}(x) + \frac{(1-\psi-\psi^\circ)\mu \cdot \eta}{\eta + (1-\mu)x(1-\eta+\eta \cdot y(x))}\right).$$

In an ex-post dishonest equilibrium, the expected payoff associated with publishing $(f_\ell, f_r) = (\underline{\ell}, \overline{r})$ upon observing $(y_\ell, y_r) = (\overline{\ell}, \overline{r})$ is given by $h(x)$.

Function h is differentiable and decreasing. For $\rho > \rho^*$, it is the case that $f(x^*) < h(x^*)$, hence either $f(1) \geq h(1)$, in which case there exists a $\hat{x} \in (x^*, 1]$, such that $f(\hat{x}) = h(\hat{x})$, or $f(1) < h(1)$, in which case $f(x) < h(x)$ for all $x \in (x^*, 1]$. Thus in every ex-post dishonest equilibrium $x = x_2(\rho)$ where:

$$x_2(\rho) := \begin{cases} \hat{x} & \text{if } f(1) \geq h(1); \\ 1 & \text{if } f(1) < h(1). \end{cases}$$

As long as $f(1) \geq h(1)$, then $\hat{x}$ is well defined and is a strictly increasing function of ρ. Hence x_2 is an increasing function of ρ.

As x_1 and x_2 are both increasing functions, $y(x^*) = 0$ and therefore $x_1(\rho^*) = x_2(\rho^*)$, we conclude that for $\rho \in (\rho_1, \rho^*)$ any selectively dishonest equilibrium is ex-post honest, while for $\rho > \rho^*$ any selectively dishonest equilibrium is ex-post dishonest. $\square$

Proof of Proposition 1. The first half of the proposition follows from Lemma B2. We prove here the second half. Let $\Delta^{SD}((f'_\ell, f'_r), (y'_\ell, y'_r))$ denote the gain for the forecaster from deviating from a selectively dishonest strategy by publishing $(f_\ell, f_r) = (f'_\ell, f'_r)$ instead of $(f_\ell, f_r) = (y'_\ell, y'_r)$ upon observing $(y_\ell, y_r) = (y'_\ell, y'_r)$. Note that:

$$\Delta^{SD}((\underline{\ell}, \underline{r}), (\overline{\ell}, \underline{r})) \leq 0 \Leftrightarrow \rho \leq \rho' := \frac{(1 - \phi(\underline{\ell}, \underline{r}))\psi}{(\overline{\ell} - \underline{\ell})\chi}\mu.$$

Note that $\rho' > \rho_1$.

Let

$$x^\dagger(\rho) = \begin{cases} x_1(\rho) & \text{if } \theta(0,0,1) < 1, \text{ or if } \theta(0,0,1) \geq 1 \text{ and } \rho < \rho^*; \\ x_2(\rho) & \text{if } \theta(0,0,1) \geq 1 \text{ and } \rho > \rho^*. \end{cases}$$

Let ρ'' be defined implicitly by:

$$\Delta^{SD}((\overline{\ell}, \overline{r}), (\overline{\ell}, \underline{r})) = 0,$$

which amounts to:

$$\chi \cdot \rho'' \cdot (\overline{r} - \underline{r}) - \phi(\overline{\ell}, \overline{r}) \cdot \psi \cdot \mu + (1 - \phi(\overline{\ell}, \overline{r}))(\mu_{\overline{\ell}}(x^\dagger(\rho'')) - \mu) = 0$$

Note that ρ'' is well defined as: (i) for $\rho = \rho_1$ we have $\Delta^{SD}((\overline{\ell}, \overline{r}), (\overline{\ell}, \underline{r})) < 0$; (ii) $\Delta^{SD}((\overline{\ell}, \overline{r}), (\overline{\ell}, \underline{r}))$ is a continuous and increasing function of ρ, and (iii)$\Delta^{SD}((\overline{\ell}, \overline{r}), (\overline{\ell}, \underline{r})) > 0$ for sufficiently large ρ.

So far we have shown that for $\rho \in [\rho_1, \min\{\rho', \rho''\}]$ there exists a selectively dishonest strategy such that, if voter and market anticipate that the forecaster adopts the strategy, the forecaster does not gain from deviating by (i) publishing $(f_\ell, f_r) = (\underline{\ell}, \underline{r})$ upon observing $(y_\ell, y_r) = (\bar{\ell}, \underline{r})$ or (ii) publishing $(f_\ell, f_r) = (\bar{\ell}, \bar{r})$ upon observing $(y_\ell, y_r) = (\bar{\ell}, \underline{r})$. All the other possible deviations can be easily ruled out as unprofitable using arguments akin those presented in the proof of Lemma B2 for the honest equilibrium.

The second half of the proposition thus holds for $\rho_2 = \min\{\rho', \rho''\}$. □

B.2 Proof of Proposition 2

Proof of Proposition 2. The second half of the proposition follows from standard arguments. To prove the first half, note that for $\eta = 1$ we have $x^* = 0$ (see the proof of Lemma B4 for a definition of x^*), so any selectively dishonest equilibrium is ex-post dishonest. □

B.3 Proof of Proposition 3

Proof of Proposition 3. First, ρ_1 (defined in Lemma B2) is a decreasing function of χ. Hence if a selectively dishonest equilibrium exists for $\chi = \chi'$, then for any $\chi = \chi'' < \chi'$ there exists either an honest equilibrium, or a selectively dishonest one.

Suppose that for $\chi = \chi''$ there exists a selectively dishonest equilibrium, with probability of lie $x = x''$. We show next that $x'' < x'$, where $x = x'$ in the selectively dishonest equilibrium associated with $\chi = \chi'$.

Note that $f(x) - g(x)$ is a continuous and decreasing function of χ, hence $x_1(\rho)$ is an increasing function of χ. Moreover, $f(x) - h(x)$ is also a continuous and decreasing function of χ, hence also $x_2(\rho)$ is an increasing function of χ. Hence indeed $x'' < x'$. Hence if $\theta(0, 0, 1) < 0$ (note that whether this condition holds does not depend on the size of χ), then $x = x_1(\rho)$, and as $x_1(\rho)$ is increasing in χ, we get $x' > x''$.

Suppose instead that $\theta(0, 0, 1) \geq 0$, hence there exists a x^* (independent of χ) such that $\theta(0, 0, x^*) = 0$. Suppose $x' \leq x^*$. As $x_1(\rho)$ is increasing in χ, then x'' satisfies $x'' \leq x^*$ and moreover $x'' < x'$. Suppose $x' > x^*$. If $x'' \leq x^*$, by transitivity $x'' < x'$. If instead $x'' > x^*$ then we have $x'' < x'$ as $x_2(\rho)$ is increasing in χ. This observation concludes the proof.

□

C Data

C.1 Measures of Partisanship

Bank. We determine whether a forecaster is a bank by referring to the forecaster's official web page and relying on the institution self-description. We label using the indicator Bank those that can be best described as banks. We also confirm that all institution-labeled banks are quoted on international financial markets.

City. We use the City/Non-City group assignment made by HM Treasury in its *Forecasts for the UK Economy* data collection.

Stock Price. We compute for each forecaster listed on a stock market the percentage decline in the stock market price after the referendum –specifically, between the referendum date (both the London and New York stock markets closed before the announcement of the referendum results) and the second banking day after the referendum results. We include the second banking day as the decline in market prices was continuous on not only the very first day after the vote (Friday, June 24) but also the subsequent Monday. For one forecaster (IHS Global Insight), we did not retrieve data as its parent company (IHS) was involved in a merger during the referendum period. The data source for this was Eikon (2018).

C.2 Measures of Influence

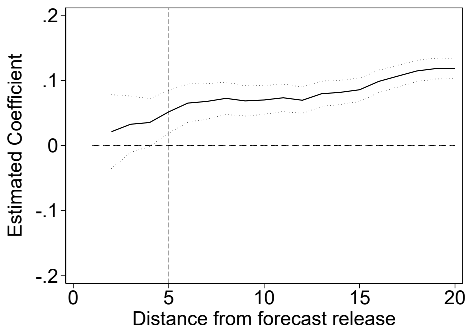
Google News. The number of search results on Google News provides an indication of a forecaster's influence. We recorded the number of mentions in a Google News search in the United Kingdom during 2015 and constructed a binary measure of influence based on a threshold of 7,000 citations. The threshold is such that half of the forecasters are above it.

Newspaper-based measures. For each forecaster, we search for its name associated with the words "GDP" and "forecast" on the Dow Jones Factiva search engine and export the number of articles reporting this textual content that were published during 2015 for the *Financial Times*, *The Times*, *The Daily Mail*, *The Guardian*, and *The Telegraph*. Then, we calculate readership shares of each newspaper according to Wave 8 of the British Election Study Panel and define the *Coverage* variables as described in Section 5.2.

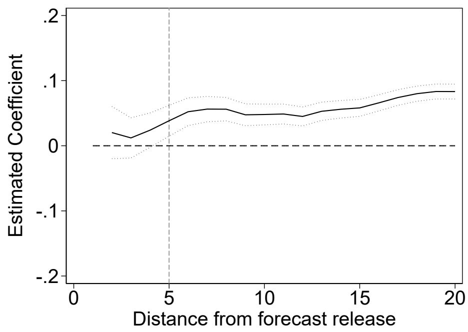
D Additional Empirical Results

D.1 Figures

(a) Δ Understanding



(b) Δ Turnout



(c) Δ Interest

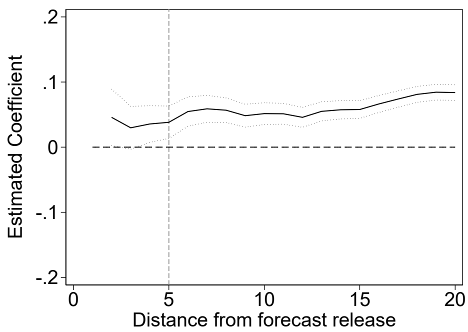


Figure D1: Voters' Beliefs and Forecast Release—Varying Time Window

Notes: The figure reports how respondents in the British Election Study panel changed their beliefs about the Brexit referendum between Wave 7 and Wave 8 by estimating equation (1) for varying time windows of days around the forecast's release, as specified on the horizontal axis. In Panel (a), the dependent variable is the change in the probability of agreeing or strongly agreeing with the following statement: *I have a good understanding of the important issues at stake in the EU referendum.* In Panel (b), the dependent variable is the change in the probability that the respondent announces that he or she will very likely or likely vote in the EU membership referendum. In Panel (c), the dependent variable is the change in the probability that the respondent reports being very interested or somewhat interested in the EU referendum. 95% confidence intervals are robust to heteroskedasticity. The vertical dashed line reports the results presented in Figure 1 and in Table D2.

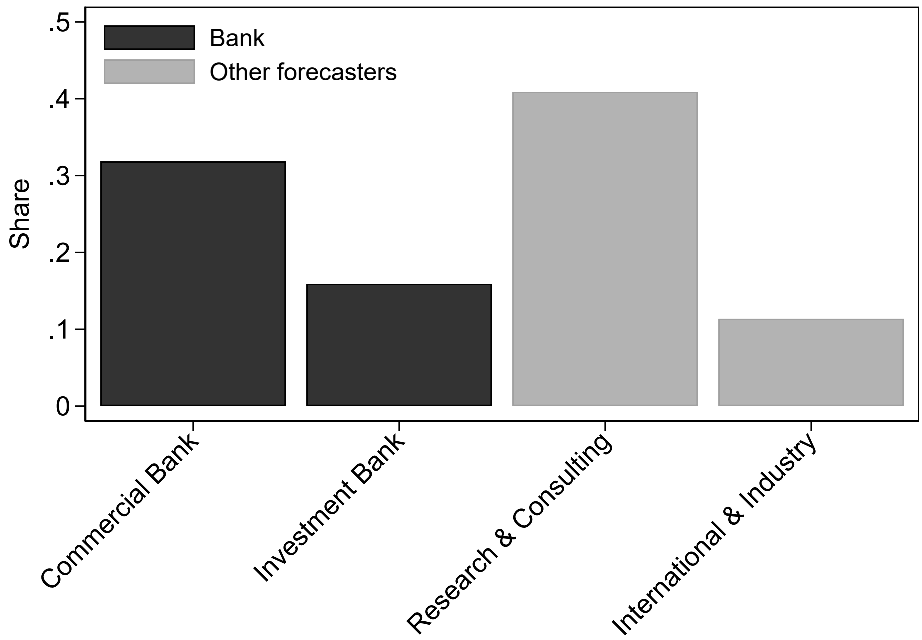


Figure D2: Distribution of Sampled Forecasters by Industry

Notes: The figure reports the proportion of forecasters in the sample by type. International & Industry refer to industry and trade associations and international organizations.

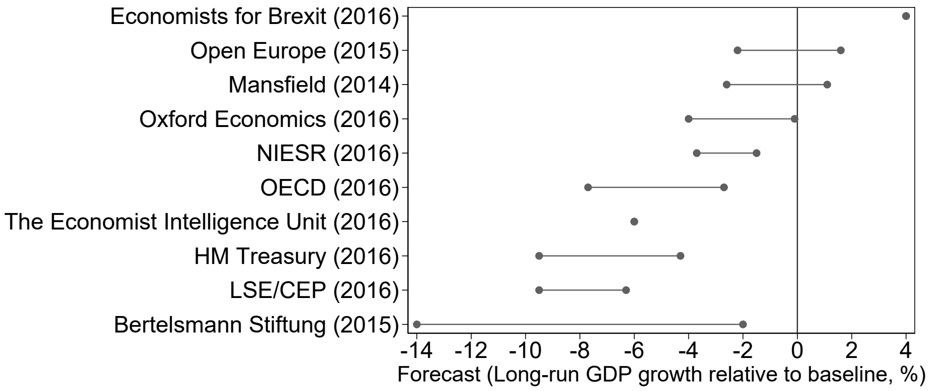


Figure D3: Forecasts contingent on Leave available prior to the referendum

Notes: See cited papers for the respective sources. Economists for Brexit are cited as Minford (2016), NIESR as Ebell and Warren (2016), and LSE/CEP as Dhingra et al. (2016a).

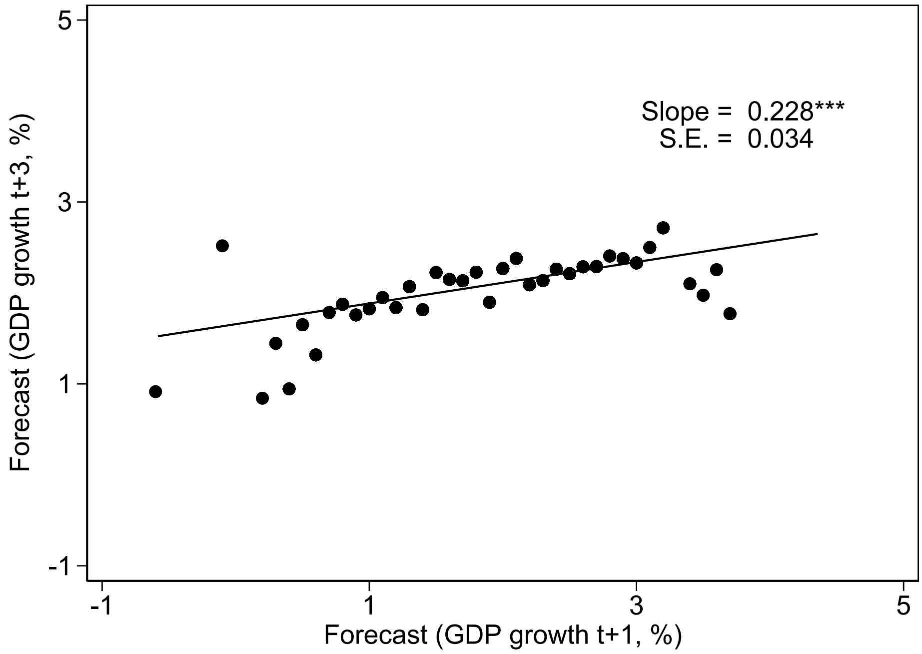


Figure D4: Correlation between Short- and Medium-term Forecasts

Notes: We consider all the forecasters surveyed by HM Treasury between January 2012 and June 2016 who announced within the same release both short- and medium-term forecasts. The figure shows the results of a bivariate regression $F_{j,t}^{t+3} = \alpha + \beta F_{j,t}^{t+1} + \varepsilon_{j,t}$. Heteroskedasticity-robust standard errors are reported. Labels *, **, and *** represent, respectively, 10%, 5% and 1% significance levels. Markers represent the sample average of $F_{j,t}^{t+3}$ within bins of 0.1 percentage point of $F_{j,t}^{t+1}$.

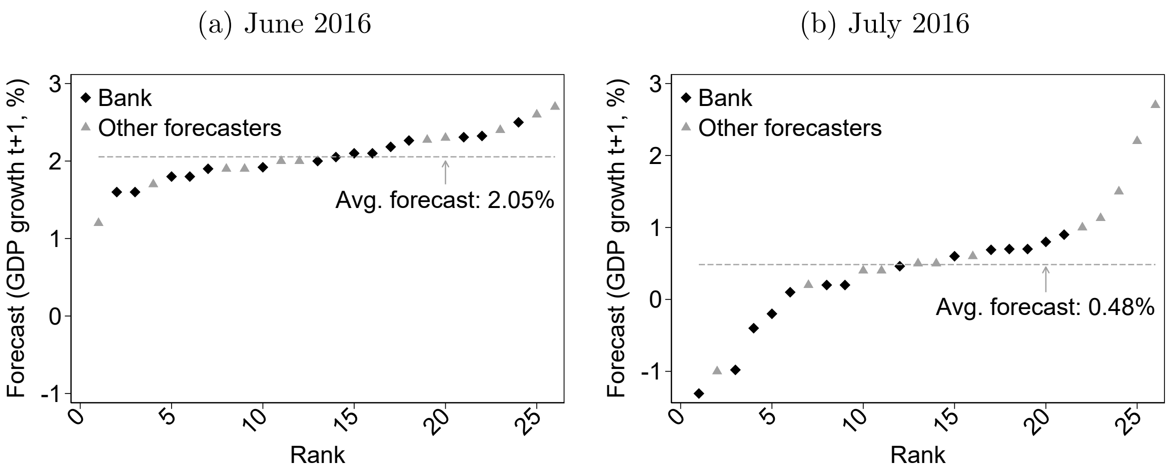
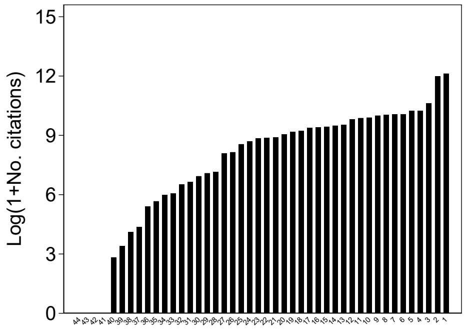


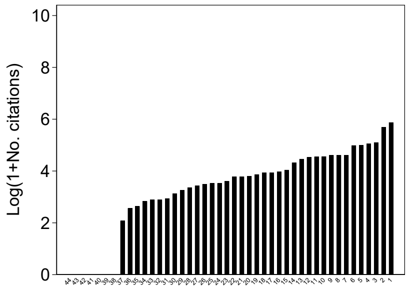
Figure D5: Forecast for the 2017 GDP Growth Rate Before and After the Referendum

Notes: All forecasters surveyed by HM Treasury in June and July 2016 are considered. Each marker represents an individual GDP growth forecast for period $t+1$. Black diamonds represent forecasts made by partisan forecasters, while gray triangles represent forecasts released by other forecasters. The horizontal line represents the average forecast.

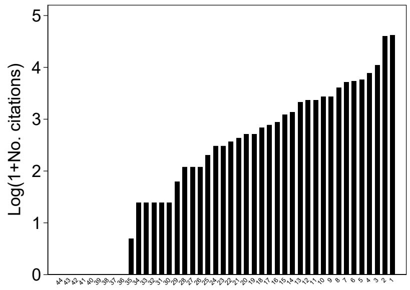
(a) Google News



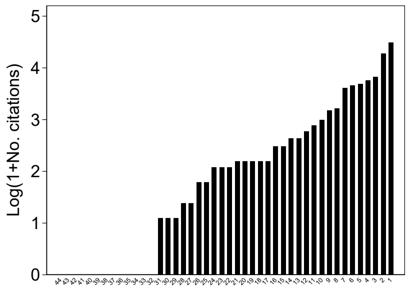
(b) Top newspapers



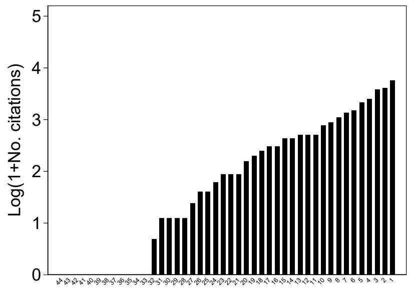
(c) Financial Times



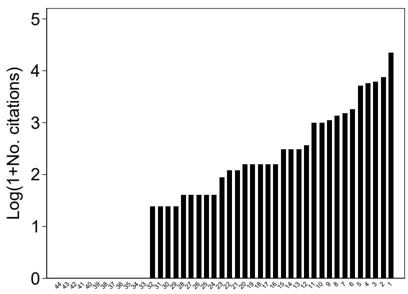
(d) The Times



(e) The Daily Mail



(f) The Guardian



(g) The Telegraph

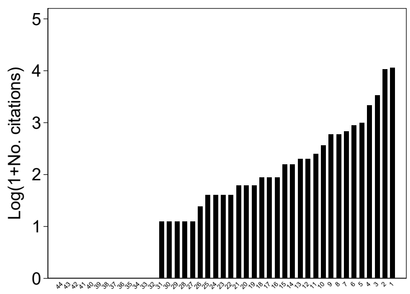
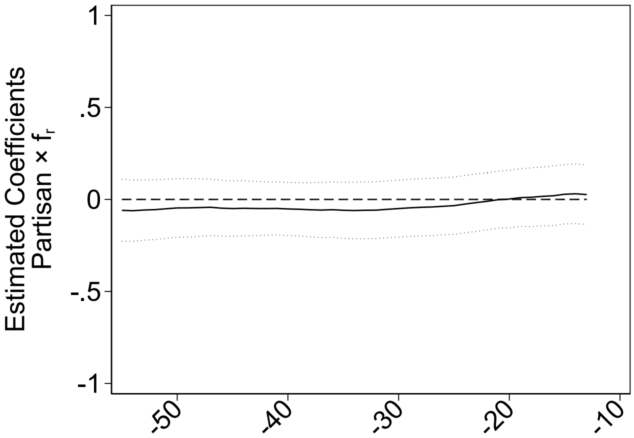


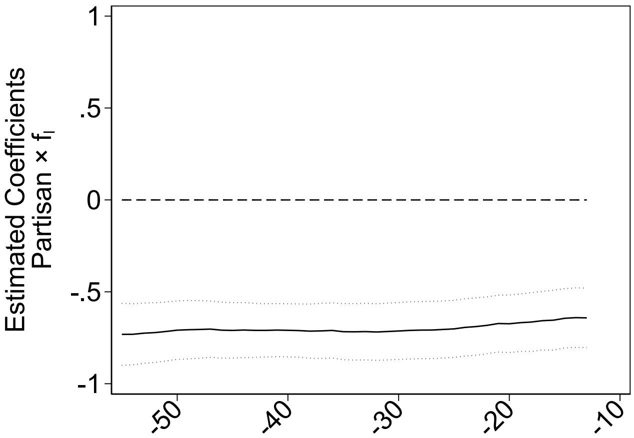
Figure D6: Measures of Influence

Notes: The charts show values by forecaster. Panels (a)-(g) plot the logarithm of the number of citations of each forecaster in Google News, all top newspapers in the United Kingdom (Financial Times, The Times, The Daily Mail, The Guardian and The Telegraph), and in each of the five aforementioned newspapers, respectively.

(a) f_r



(b) f_ℓ



(c) f°

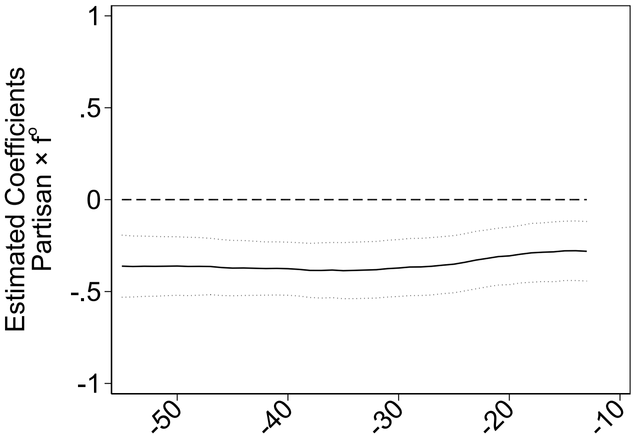
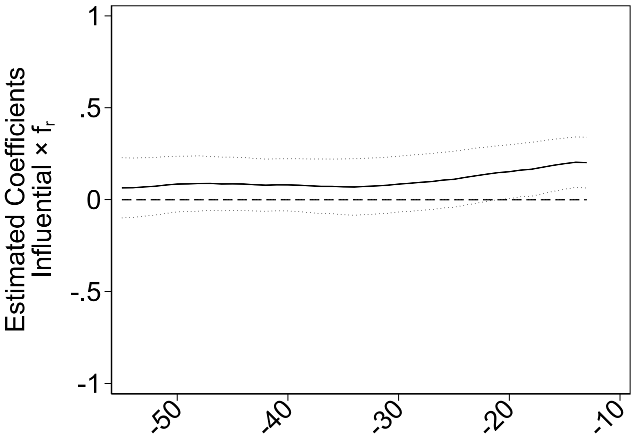


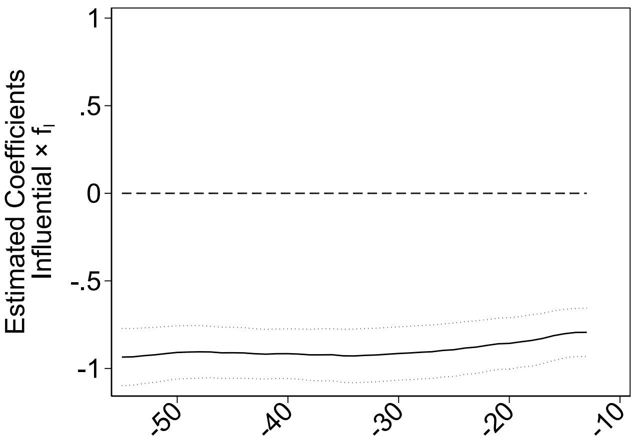
Figure D7: Sensitivity to Changes in the Time Span – Partisanship

Notes: In each graph, we estimate (2) by restricting the sample in terms of months before the referendum, as specified on the horizontal axis. For instance, at -54 we consider all data starting from January 2012. The solid black line represents estimated coefficients, whereas dotted lines represent 95% confidence intervals. All specifications include forecasters' fixed effects and survey fixed effects.

(a) f_r



(b) f_ℓ



(c) f°

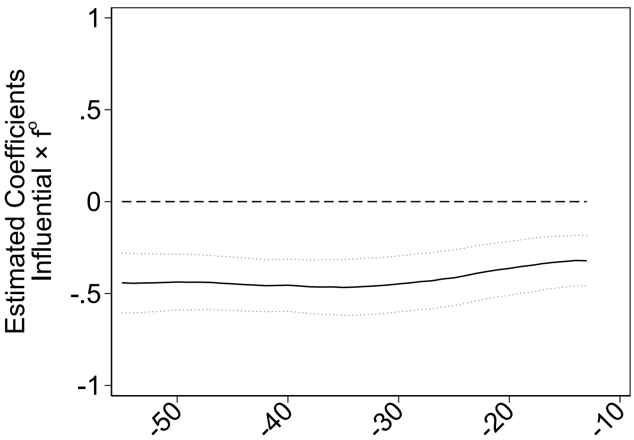


Figure D8: Sensitivity to Changes in the Time Span – Influence

Notes: In each graph, we estimate (5) by restricting the sample in terms of months before the referendum, as specified on the horizontal axis. For instance, at -54 we consider all data starting from January 2012. The solid black line represents estimated coefficients, whereas dotted lines represent 95% confidence intervals. All specifications include forecasters' fixed effects and survey fixed effects.

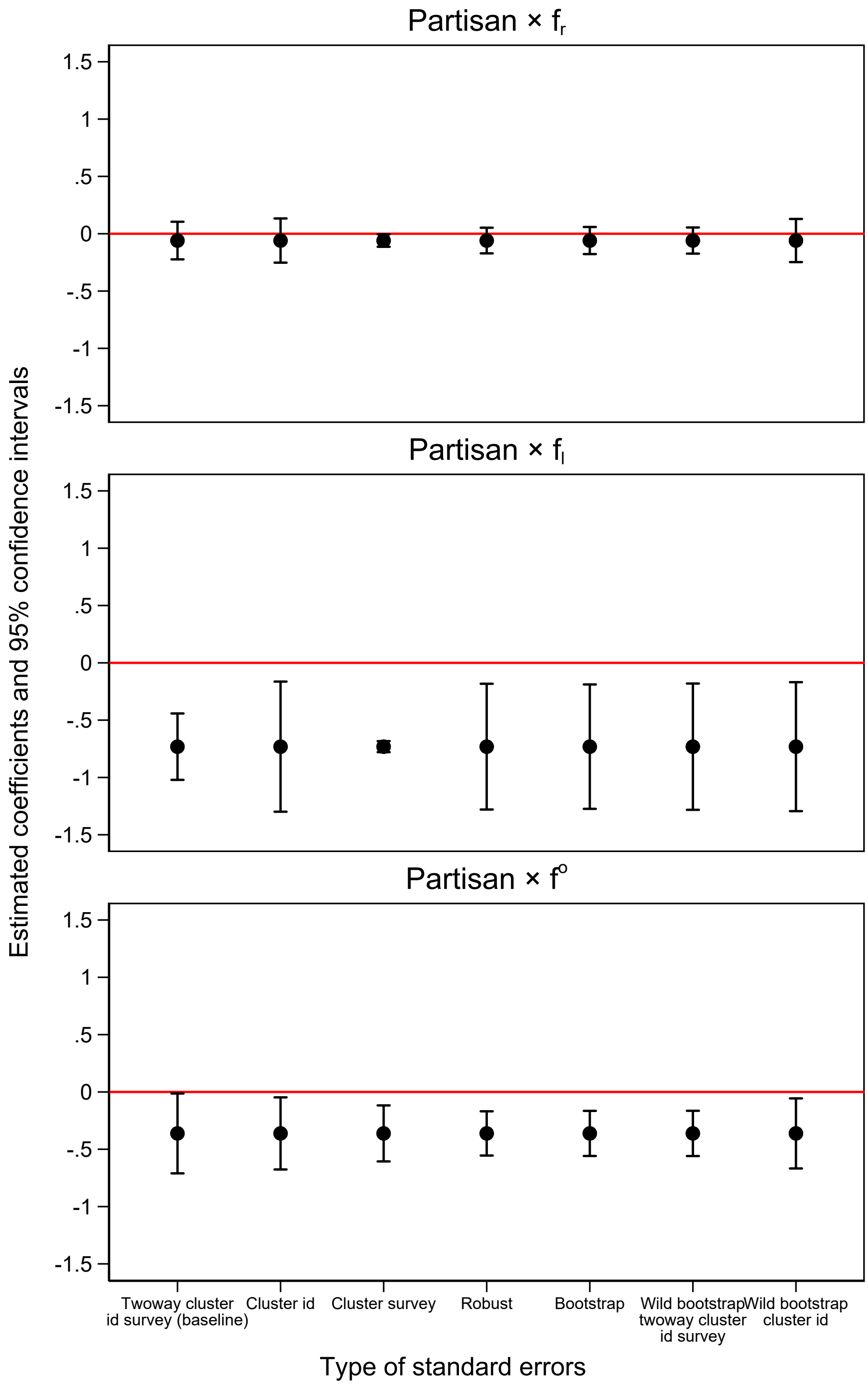


Figure D9: Alternative Inference Approaches – Partisanship

Notes: The estimated equation is (2). The figure reports estimated coefficients and 95% confidence intervals obtained using alternative inference approaches as specified in the horizontal axis. Bootstrap and wild-clustered bootstrap confidence intervals are based on 1,000 replications. The measure of partisanship is the binary indicator Bank.

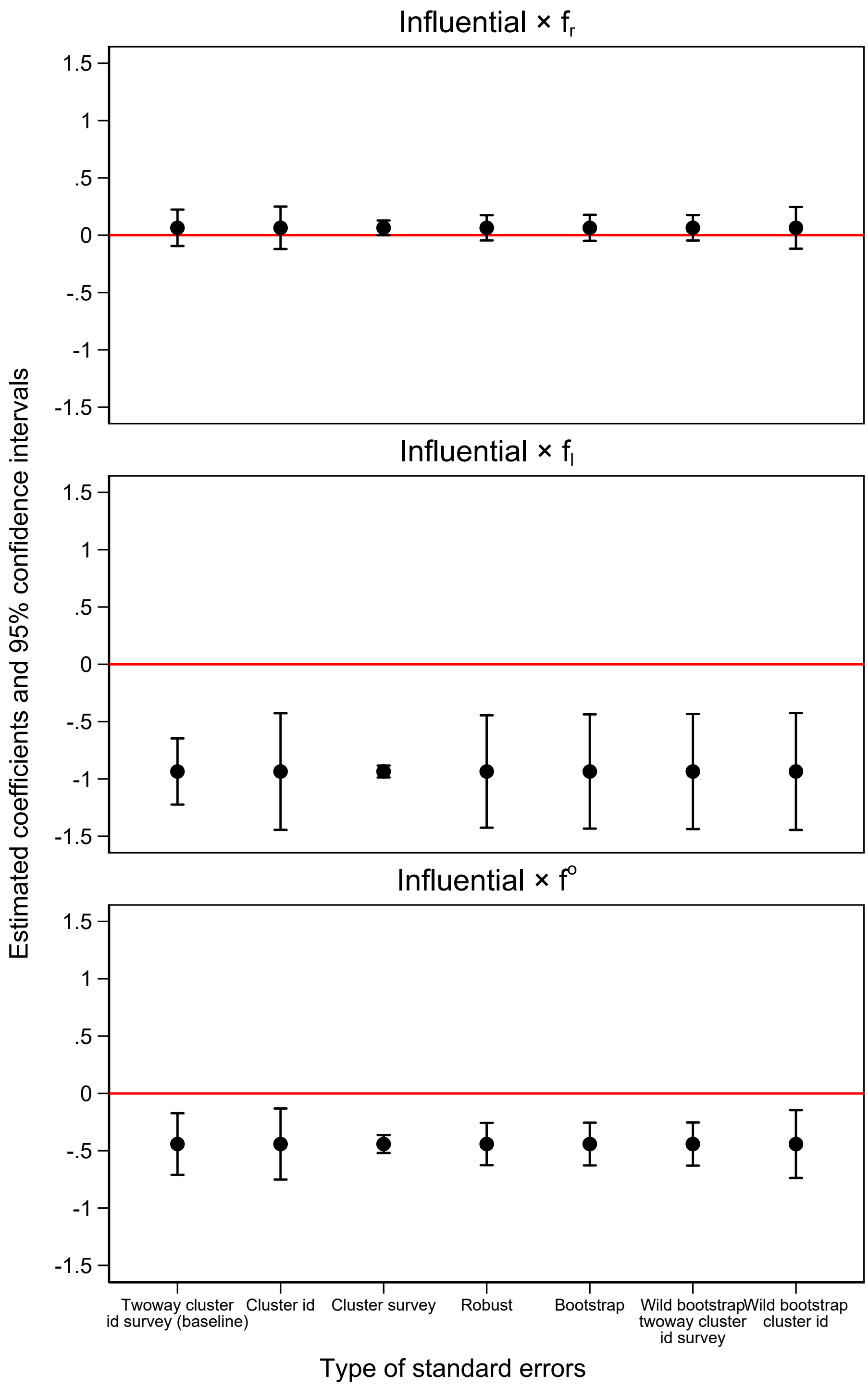


Figure D10: Alternative Inference Approaches – Influence

Notes: The estimated equation is (5). The figure reports estimated coefficients and 95% confidence intervals obtained using alternative inference approaches as specified in the horizontal axis. Bootstrap and wild-clustered bootstrap confidence intervals are based on 1,000 replications. The measure of influence is the binary indicator GNews.

D.2 Tables

Table D1: Timeline of the United Kingdom's European Union Membership Referendum

Date	Event
Jan. 22, 2013	Incumbent Prime Minister David Cameron commits to holding a referendum on the EU membership if he is reelected in 2015.
May 22, 2014	EU-sceptical UKIP receives 26% of votes in European elections and becomes the largest UK party in the EU parliament.
May 7, 2015	Conservative Party wins the majority in the 2015 general election.
May 27, 2015	The queen unveils the European Union Referendum Act 2015 (c. 36).
Dec. 17, 2015	The Act is given Royal Assent.
Jan. 5, 2016	PM Cameron says ministers are free to campaign on either side.
Feb. 20, 2016	PM Cameron announces the referendum date (June 23, 2016).
Apr. 15, 2016	Official start of the referendum campaign.
May 6, 2016	Start of the British Election Study panel (Wave 8).
May 23, 2016	HM Treasury analysis: the immediate economic impact of leaving the EU.
June 22, 2016	End of the British Election Study panel (Wave 8).
June 23, 2016	The United Kingdom's European Union membership referendum.
June 24, 2016	PM Cameron resigns after the Brexit victory.
July 9, 2016	The government rejects the petition for a second referendum.
July 11, 2016	Theresa May is elected as the new leader of the Conservative Party.
July 13, 2016	Theresa May is appointed Prime Minister by the Queen.
Mar. 29, 2017	PM May triggers Article 50, officially starting the process of withdrawal.
Jan. 31, 2020	The United Kingdom leaves the EU.

Notes: The table reports the dates of the key events related to the EU membership referendum. Source: Authors' analysis of information from https://www.bbc.com/news/politics.

Table D2: Voters' Beliefs and Forecast Release

	(1) Δ Understanding	(2) Δ Turnout	(3) Δ Interest
Subject to Wave 8 Post HM Release	0.065***	0.052***	0.055***
	(0.015)	(0.011)	(0.011)
Observations	7,358	7,332	7,398
R^2	0.003	0.003	0.003

Notes: The table reports how respondents in the British Election Study panel changed their beliefs about the Brexit referendum between Wave 7 and Wave 8 by estimating equation (1). The sample was restricted to include only individuals surveyed in a window of five calendar days around the forecast's release. In Column (1), the dependent variable is the change in the probability of agreeing or strongly agreeing with the following statement: *I have a good understanding of the important issues at stake in the EU referendum*. In Column (2), the dependent variable is the change in the probability that the respondent announces that he or she will very likely or likely vote in the EU membership referendum. In Column (3), the dependent variable is the change in the probability that the respondent announces being very interested or somewhat interested in the EU referendum. Standard errors and 95% confidence intervals are robust to heteroskedasticity. Labels *, ** and *** represent significance levels of 10%, 5% and 1%.

Table D3: Correlation Matrix—Partisanship

	Banks	City	Stock price	UK holders
Banks	1.00	0.83	0.57	0.50
City	0.83	1.00	0.47	0.40
Stock price	0.57	0.47	1.00	0.44
UK holders	0.50	0.40	0.44	1.00

Notes: The table reports the correlation between the measures of partisanship introduced in Section 4. All forecasters surveyed by HM Treasury between January 2012 and April 2018 are considered.

Table D4: Forecasts before the Referendum Announcement by Forecaster Type

	Forecast (GDP growth t+1, %)		
Type	Mean	SD	N
Commercial Bank	2.04	0.73	350
Investment Bank	1.86	0.76	163
Research & Consulting	1.96	0.68	457
International & Industry	2.19	0.55	56

Notes: All forecasts collected by HM Treasury between January 2012 and December 2015 are considered by forecaster type.

Table D5: Correlation Matrix—Influence

	Google News	Financial Times	The Times	The Daily Mail	The Guardian	The Telegraph
Google News	1.00	0.17	0.14	–0.15	0.13	0.10
Financial Times	0.17	1.00	0.50	0.51	0.46	0.41
The Times	0.14	0.50	1.00	0.37	0.68	0.73
The Daily Mail	–0.15	0.51	0.37	1.00	0.50	0.37
The Guardian	0.13	0.46	0.68	0.50	1.00	0.59
The Telegraph	0.10	0.41	0.73	0.37	0.59	1.00

Notes: The table reports the correlation between the number of times each forecaster was mentioned in Google News, the Financial Times, The Times, The Daily Mail, The Guardian, and The Telegraph in 2015. All forecasters surveyed by HM Treasury between January 2012 and April 2018 are considered.

Table D6: Estimation of Propaganda Bias in GDP Components' Growth Forecasts

	(1) Private consum.	(2) Fixed investment	(3) Government consum.	(4) Total exports	(5) Total imports
Partisan $\times f_r$	–0.082	–0.701*	0.126	–0.583	–0.725***
	(0.102)	(0.394)	(0.155)	(0.370)	(0.268)
Partisan $\times f_\ell$	–0.285	–2.307***	0.473***	–1.123**	–0.472
	(0.259)	(0.781)	(0.131)	(0.526)	(0.642)
Partisan $\times f^\circ$	0.035	–1.731*	0.246	–0.546	0.682
	(0.234)	(0.870)	(0.222)	(0.376)	(0.590)
Observations	1,636	1,642	1,634	1,534	1,532
R^2	0.720	0.692	0.719	0.574	0.667
Fixed Effects	✓	✓	✓	✓	✓
Survey Month Effects	✓	✓	✓	✓	✓

Notes: The estimated equation is (2). In column (1), the dependent variable is the forecast of the growth in household consumption in year $t + 1$. In column (2), the dependent variable is the forecast of the growth in fixed investments in year $t + 1$. In column (3), the dependent variable is the forecast of the growth in government consumption in year $t + 1$. In column (4), the dependent variable is the forecast of the growth in total exports in year $t + 1$. In column (5), the dependent variable is the forecast of the growth in total import in year $t+1$. The measure of partisanship is the binary indicator Bank. Standard errors robust to two-way clustering at the forecaster and survey levels are shown in parentheses. Labels *, **, and *** represent significance levels of 10%, 5% and 1%.

Table D7: Forecast Performance by Group – Partisanship

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dep. var.: Absolute Forecast Error (GDP growth t+1, %)							
Bank	0.032				–0.022			
	(0.070)				(0.047)			
City		0.093				–0.033		
		(0.064)				(0.051)		
Stock price			–0.082				0.044	
			(0.053)				(0.065)	
UK holders				–0.086				0.052
				(0.056)				(0.064)
Observations	1,038	1,038	1,038	1,038	4,886	4,886	4,886	4,886
R^2	0.476	0.486	0.482	0.481	0.884	0.884	0.884	0.884
Survey Month Effects	✓	✓	✓	✓	✓	✓	✓	✓
Start of sample period	2012	2012	2012	2012	1998	1998	1998	1998

Notes: The estimated equation is: $|E^{t+1}_{j,m}| = \alpha + \beta Partisan_j + \delta_m + \varepsilon_{j,m}$, where $|E^{t+1}_{j,m}|$ is the absolute value of the forecast error produced by forecaster j in release m targeting year $t+1$; δ_m is a set of survey fixed effects. In columns (1)–(4), the sample includes forecasts published during 2012–2015. In columns (5)–(8), the sample includes forecasts published during 1998–2015. Standard errors robust to two-way clustering at the forecaster and survey levels are shown in parentheses. Labels *, **, and *** represent significance levels of 10%, 5% and 1%.

Table D8: Forecast Performance by Group – Influence

	(1)	(2)	(3)
	Dep. var.: Absolute Forecast Error (GDP growth t+1, %)		
GNews	0.050		
	(0.068)		
Newsp. sum		–0.034	
		(0.071)	
Newsp. w. sum			–0.044
			(0.068)
Observations	1,038	1,038	1,038
R^2	0.478	0.477	0.478
Survey Month Effects	✓	✓	✓
Start of sample period	2012	2012	2012

Notes: The estimated equation is: $|E^{t+1}_{j,m}| = \alpha + \beta Influential_j + \delta_m + \varepsilon_{j,m}$, where $|E^{t+1}_{j,m}|$ is the absolute value of the forecast error produced by forecaster j in release m targeting year $t+1$; δ_m is a set of survey fixed effects. In all specifications, the sample includes forecasts published during 2012–2015. Standard errors robust to two-way clustering at the forecaster and survey levels are shown in parentheses. Labels *, **, and *** represent significance levels of 10%, 5% and 1%.

Table D9: Results excluding forecasts from the *International & Industry* Sectors

	(1)	(2)	(3)	(4)
	Dep. var.: Forecast (GDP growth t+1, %)			
Partisan $\times f_r$	–0.093	–0.005	–0.074	–0.048
	(0.097)	(0.103)	(0.091)	(0.092)
Partisan $\times f_\ell$	–0.735***	–0.521***	–0.472***	–0.515***
	(0.161)	(0.190)	(0.170)	(0.150)
Partisan $\times f^\circ$	–0.401**	–0.374*	–0.377*	–0.434**
	(0.184)	(0.193)	(0.190)	(0.193)
Observations	1,554	1,554	1,554	1,554
R^2	0.769	0.766	0.765	0.766
Fixed Effects	✓	✓	✓	✓
Survey Month Effects	✓	✓	✓	✓
Measure of Partisanship	Bank	City	Stock price	UK holders

Notes: Forecasters labelled *International & Industry* have been removed from the sample. The estimated equation is (2). Standard errors robust to two-way clustering at the forecaster and survey levels are shown in parentheses. Labels *, **, and *** represent significance levels of 10%, 5% and 1%.