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ORIGINAL ARTICLE **OPEN ACCESS**

Social Effects of the Vote of the Majority: An Experiment on the Brexit-Vote

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ABSTRACT

The 2016 EU referendum result was widely perceived as a statement against immigration. This paper investigates whether information about majority voting outcomes influences attitudes towards immigrants. Using a survey experiment conducted in England in 2017–2018, we randomly exposed participants to truthful information indicating that local constituencies had predominantly voted either to Leave or to Remain in the 2016 EU referendum. The intervention substantially shifted beliefs about local Brexit vote shares. Although we find no statistically significant effects on most individual survey questions, exposure to a Leave-majority signal leads to more restrictive immigration policy preferences relative to exposure to a Remain-majority signal. Effects are concentrated among participants for whom the information was politically dissonant or unexpected. Overall, the pattern of results is consistent with a conformity mechanism operating through perceived majority views, suggesting that referenda may continue to shape expressed policy preferences beyond their immediate aftermath.

1 | Introduction

A majority in Britain voted for the UK to leave the European Union in the 2016 EU referendum. The Leave side won with 51.9% of the votes, advocating anti-immigration views with the motto ‘Take back control’. Prominent figures in the Leave campaign, such as then-MP Boris Johnson, stated that ‘The only way to take back control of immigration is to Vote Leave on 23 June’.¹

Following the 2016 referendum, the UK saw a surge in hate crimes targeting immigrants [1], leading to a common perception that the referendum results unearthed prejudice and empowered misconduct towards immigrants [2]. In this paper, we investigate this conjecture with an experiment designed to test whether majority voting outcomes can influence individual attitudes. Rather than studying aggregate post-referendum behaviour, we focus on whether exposure to information about majority voting

outcomes can shift attitudes towards immigrants in a controlled experimental setting.

To explore this, we conducted a survey experiment in England between November 2017 and April 2018. The intervention was an incentivized, computer-based quiz designed to influence participants’ perceptions of local Brexit support. Participants were randomly assigned to one of three groups: a Remain treatment, a Leave treatment, or a control group.

Although the national Brexit result (51.8% voting to leave the EU) was widely known due to extensive media coverage, the referendum results in individual constituencies were less likely to be recognized. Assuming incomplete knowledge about local Brexit votes (discussed further in Section 2.3), we hypothesize that individuals’ perceptions of regional Brexit support could be influenced by information provision. We designed and conducted a quiz with this intention. The central component of our quiz

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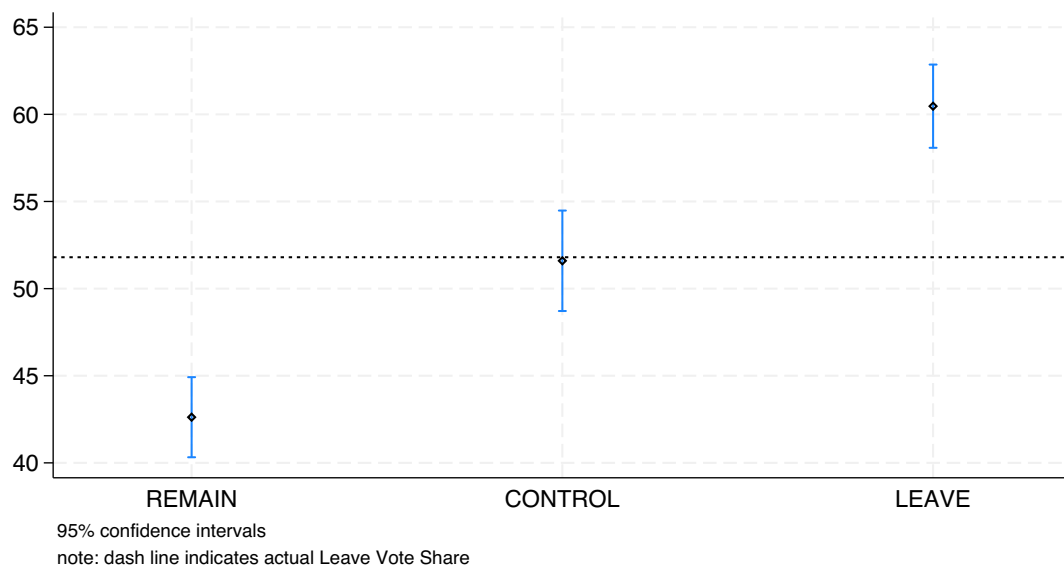


FIGURE 1 | Respondents' guesses of 'Leave' vote share in the South East by experimental treatment.

was to inform participants of the majority voting outcomes in a series of seemingly randomly selected places. We provided truthful information on how the majority had voted in the referendum, but for a pre-selected (not random) set of constituencies.

To present this information, we utilized animated maps that highlighted constituencies in an arbitrary sequence, with blinking locations eventually pausing on a specific constituency (we did not explain how the selection was made). After a place was selected by the computer, participants were asked questions. For those in the Leave and Remain treatments, we asked: 'How did the majority of voters in [place X] vote in the 2016 EU referendum?' with response options of Leave or Remain. Participants received immediate feedback on the correct answer, and this process was repeated three times. Those in the Leave treatment group were presented three times with constituencies where the majority voted to leave the EU, whereas participants in the Remain treatment group were presented only with constituencies where the majority voted to remain. In contrast, the control group was asked and informed about the gender majority within each selected place. This design allowed us to examine how and whether participants' attitudes and Brexit vote beliefs shifted based on quiz information about majority support for Leave or Remain, whereas ensuring all information provided was factually accurate. Figure 1 shows that the intervention effectively shifted individuals' perceptions of the Brexit vote in the South East of England, where participants lived. Participants in the Leave condition estimated the percentage of votes to leave the EU to be 42% higher than those in the Remain condition. Estimates from the control group fell between these two conditions, aligning closely with the actual Brexit outcome.

Our central hypothesis is that individuals adjust their expressed attitudes towards immigration in response to information about majority voting outcomes. In particular, we test whether exposure to a Leave-majority signal leads to more negative

expressed attitudes towards immigrants relative to exposure to a Remain-majority signal. Given our sample size and study design, the analysis is powered to detect differences only for the Leave–Remain comparison.

We collected data on both intrinsic and extrinsic attitudes. As outcomes, we consider reported views about immigrants living in the UK and policy preferences on immigration-related issues. To measure intrinsic attitudes, we conducted an Implicit Association Test (IAT)—a widely used test in social psychology that is based on unconscious behaviour to measure individuals' implicit prejudice [3].

We find no statistically significant effects on most individual survey questions. However, we detect significant effects of the experimental intervention on an aggregated index of attitudes. Individuals assigned to the 'Leave' treatment express preferences for policies less favourable towards immigrants, with index values 0.23 standard deviations higher than those of individuals assigned to the Remain treatment. This result is based on a policy index that combines all policy-related questions included in the survey [4], such as preferences for restricting immigration, access to public health services, and the ending of multilingual formatting in official documents. As discussed later in the paper, these effects do not remain statistically significant after controlling for multiple hypothesis testing using the False Discovery Rate procedure.

Turning to the impact of the Brexit vote share itself, we observe in our OLS regressions that individuals who guessed a higher 'Leave the EU' vote share in their region expressed more negative views about immigrants' contributions to the UK. This association may be influenced by unobserved preferences, correlated with both the individuals' own Brexit votes and those of their social circles. To estimate the causal effects of perceived Leave vote share, we employ instrumental variable (IV) regressions, using assignment to the Leave treatment as an instrument for perceived Leave

vote share. The IV estimates indicate positive effects of perceived Leave support on policy restrictiveness.

A plausible explanation for the effects of majority voting outcomes is that participants infer new information from referendum results, prompting them to update their beliefs about the population's preferences. Consistent with this interpretation, we find that treatment effects are most pronounced when the provided information is either unexpected or novel.

We conducted this experiment in person with a sample of 342 participants. This sample size constitutes a key limitation of the study. As noted above and discussed in Section 3.4, the analysis is underpowered to detect treatment differences relative to the control group or small effect sizes. Consequently, the study is inconclusive with respect to causal effects on intrinsic preferences (measured by the IAT) and generosity (measured using a dictator game).

Our paper contributes to several strands of the literature. First, it adds to research on the social and attitudinal consequences of the Brexit vote. A body of work has examined the impact of the 2016 EU referendum on hate crime in the UK [1, 5–7]. Carr et al. [1] use comprehensive administrative crime data and several empirical approaches (difference-in-differences, event-study, and synthetic control methods) to estimate the impact of the referendum. They document a 15%–25% increase in hate crime following the referendum, with effects concentrated in the first quarter after the referendum. They find that larger increases in hate crimes occurred in areas that voted to Leave than in areas that voted to Remain. Schilter [6] applies machine learning techniques to investigate spatial heterogeneity in changes in hate crime following the referendum. His analysis indicates that areas with higher immigrant shares, higher income levels, and more formally qualified populations experienced greater increases in hate crime after Brexit. On the other hand, Albornoz et al. [5] document that the increase in hate crimes following the EU referendum was more pronounced in Remain areas. They propose that surprise at the referendum outcome in these areas played an important role by revealing new information about societal preferences and allowing individuals to feel more comfortable expressing anti-social behaviour. This paper departs from the literature by adopting an experimental approach, examining more subtle outcomes, and focusing on the mechanism through which majority votes influence behaviour, rather than the broader impacts of the EU referendum.^{2, 3} By doing so, we provide complementary evidence on how majority voting outcomes may continue to shape expressed attitudes beyond the immediate aftermath of a major political shock.

Second, this paper relates to the literature on randomized information effects on immigrant-related issues. Previous studies examine how correcting misperceptions about objective quantities, such as the size of immigrant populations or the labour-market impacts of immigration, affects attitudes and policy preferences (e.g., [19–22]). These interventions elicit baseline beliefs and then provide factual corrections to a subset of participants. In contrast, our intervention does not aim to correct factual misperceptions. Instead, it is designed to shift individuals' perceptions of the popularity of Brexit in antagonistic ways. This is a novel design that proves effective in changing subjects' beliefs.

However, while belief updating is clearly detected, the design and the generated stimulus do not allow us to draw definitive conclusions about effects on attitudes.

Third, our paper contributes to the literature on social conformity and political behaviour, which emphasizes the role of social interactions, reference groups, and perceived norms in shaping individual attitudes [23–27].⁴ Although much of this literature focuses on conformity within close social networks or in response to highly salient national events, our experiment isolates the effect of localized majority signals on political attitudes. In this respect, our findings suggest that referenda may act as coordination devices that shape social attitudes even outside individuals' immediate social circles.⁵

To better understand how majority influence operates, we estimate treatment effects separately for pro-Remain and pro-Leave participants. We examine whether information about the majority vote reinforces individuals' initial views, potentially giving them 'permission' to express pre-existing attitudes, or instead pressures them to conform to the perceived majority. We find that responses are concentrated in the politically dissonant direction: pro-Leave participants respond primarily to the Remain-majority signal by expressing more positive policy views towards immigrants. This pattern is consistent with conformity rather than permission-giving. At the same time, these heterogeneity results are exploratory in nature, as they were not embedded in the experimental design and were not pre-registered.

Taken together, our results are consistent with information about regional majority voting outcomes influencing expressed attitudes towards immigration and suggest that conformity may extend beyond close social networks. We find statistically significant effects for the comparison between the Leave and Remain treatments on a policy index that aggregates multiple immigration-related policy questions. However, none of the primary outcome estimates remain statistically significant after controlling for multiple hypothesis testing using the False Discovery Rate procedure. These findings should therefore be interpreted in light of both multiple-testing concerns and the study's limited statistical power. Nevertheless, the overall pattern of results is broadly consistent with individuals updating their attitudes in response to perceived majority views. This points to a role for referenda as channels for conformity in society, shaping public opinion on issues advocated by the winning side.

The remainder of the paper is developed as follows. In Section 2, we explain the experiment. In Section 3, we explain the data, present the results in Section 4, and conclude in Section 5.

2 | The Survey Experiment and Context

2.1 | Data Collection

We conducted this survey-experiment among the general public in four public libraries in the South East of England, and at the University of Kent,⁶ among university students between November 2017 and April 2018. This experiment received ethical approval from the institutional review board at the University of Kent.

We used public libraries, as they had space to host our laboratory (with our laptops), and they were easily accessible, facilitating the recruitment of participants among the general public. We contacted all public libraries in the South East of England, and used those in the towns of Maidstone, Tonbridge and Dartford, and in the city of Canterbury. The other libraries did not reply to our requests, were not interested in being involved in a study with a political topic, or did not have sufficient space to accommodate our laboratory.

The survey was conducted on laptops using Qualtrics software. Individuals participated in person so that data were collected in a controlled environment. This was important for two main reasons. First, our intervention intended to change individuals' beliefs about the popularity of Brexit, by giving information on the referendum votes in an incentivized quiz (as will be detailed in Section 2.2). It was key that participants did not have alternative access to the internet to look up correct answers that might undermine the manipulation.⁷ As explained in the information sheet, during the study, communication between participants or internet browsing were not allowed.

Under these controlled conditions, we could ensure respondents' attention was focused selectively on information that the majority voted to Leave or to Remain in the EU. Secondly, one of our key outcomes was the scores from the Implicit Association Tests. The task demanded full attention of respondents to ensure the validity of the IAT results.

A total of 342 individuals participated: 255 British and 87 Europeans. The study was described as 'Citizens' Views on Current Societal Issues,' and participants were informed that they would be quizzed on UK facts, such as the UK's relationship with other countries and immigration. They learned in the information sheet (see Appendix A0 in [Supporting Information](#)) that the survey was anonymous, that all of the provided information was truthful, that the study was conducted by non-partisan researchers,⁸ and that they were free to withdraw at any point or skip questions. Because of the topic, the survey was administered by assistants who were native English speakers.

The survey was advertised via a job-search site, Facebook posts in community groups and participating libraries, tweets from media and local community groups, via volunteer participant mailing lists from the School of Psychology at the University of Kent, and with flyers in relevant city centres on the day of the experiment. Participants signed up in advance and were paid £10 as a show-up fee, but could earn up to £27 for 40–55 min of participation. Individuals had to meet the following criteria to participate: be born in the UK or in a European country, live in the UK, be aged between 18 and 80 years old, and feel comfortable using a laptop keyboard. They were allowed to take the survey only once.

The survey was structured as follows: (i) questions on socio-economic, demographic, and political affiliation characteristics and the first implicit association test (IAT); (ii) a UK-fact quiz, in which we varied questions and the respective feedback on how the local majority voted in the EU referendum, according to the assigned treatment; (iii) a second IAT, questions to assess the effectiveness of the intervention, a dictator game, and questions on views about immigrants, policies, and finally, a third IAT.

In Appendix A1 in [Supporting Information S1](#), we provide the structure.

Possibly because participants came in person to take the survey, most completed the entire survey. We took the standard precautions to avoid raising participants' suspicion about the real purpose of our study, and altogether, our results are not consistent with experimenter demand effects or suggest that respondents guessed the objective and main hypotheses and reacted accordingly [35, 36].^{9,10}

2.2 | Timing Context of the Experiment

We conducted the experiment between November 2017 and April 2018, starting data collection approximately 18 months after the 2016 United Kingdom EU membership referendum. In the period between the referendum and our survey, the UK experienced significant political developments: a new prime minister took office, a snap general election was held in which some parties pledged to implement the referendum result while others advocated remaining in the EU through a second referendum, and there was continuous news coverage of UK-EU negotiations [37]. By January 2017, the UK government had outlined its primary objectives for exiting the EU, including ending free movement of people [38]. During the data collection phase, there were key developments in the Brexit process. In November 2017, the United Kingdom invoked Article 50 of the Treaty on European Union, officially beginning the withdrawal process. In the following month, the UK and the EU reached an agreement on the first phase of Brexit negotiations, and the UK set a deadline for the Brexit transition.

Brexit was a deeply divisive issue in society, yet overall public attitudes towards it remained relatively stable both before and during our data collection period (NatCen Social Research). When asked in retrospect whether they believe the decision to leave the EU was correct, opinion polls indicate the public remained closely divided. Slightly more than half of respondents thought that Britain made the right choice in voting to leave, whereas just under half disagreed. The divide was particularly stark between Remain and Leave supporters, with around 88% of Leave voters affirming that the decision was right, and a similar fraction (88%) of Remain voters asserting it was wrong.¹¹ During our data collection, there was only a small shift in public opinion compared to earlier periods (from August 2016 to September 2017). As we show in Table A1 in [Supporting Information](#), the overall percentage of people who believed Britain made the right choice to leave fell by 2.4 percentage points, and this shift primarily came from those who had originally voted to Leave. The loss in Brexit approval might be related to negative news surrounding the National Health Service (NHS) during this period, including record-low A&E performance and widespread cancellations of non-urgent treatments [39, 40]. Additionally, a series of reports highlighted the potential negative impacts of Brexit, which were covered widely in the media.¹²

The key goal of our quiz intervention was to provide information about the vote of the majority (Leave or Remain) in the EU referendum. This information might have been potentially known to participants. Given the lag between the survey and the referendum, people may have forgotten the specifics of the vote shares

and regional differences. This potential forgetfulness should be evenly distributed across all experimental conditions. However, individuals' recollections of Brexit's popularity may have been skewed by their own political views. For example, pro-Leave individuals may remember Brexit as being more popular in their region, whereas pro-Remain individuals might recall the opposite. If this is the case, the information provided is likely to be more surprising when it contradicts participants' own ideologies, such as when pro-Leave participants are exposed to the Remain treatment or when pro-Remain participants are exposed to the Leave treatment. These impacts are further explored in Section 4.3.

2.3 | The Quiz Intervention

Our aim is to test how individuals react to credible information indicating whether the majority voted for or against Brexit. Although the national referendum result (i.e., 52% of the population voted to leave the EU) was widely known and extensively discussed, we instead presented local referendum results from seemingly random constituencies in the South East of England. We assume that individuals lack complete knowledge of regional or specific constituency voting outcomes, making them likely to be influenced by the information provided.

We changed beliefs by providing truthful information on the EU referendum vote shares for a set of selected constituencies. We used animated maps that appeared to select constituencies in a seemingly random way, blinking over many places before stopping in a constituency, thus giving the impression that the selected places were representative of the East of England. However, constituencies were selected in advance. For the Leave group, we always informed participants about the vote of the majority for areas where the majority voted to Leave. For the Remain group, we repeatedly informed participants about vote results in areas where the majority voted to Remain. For the 'Control' group, we informed participants about the gender majority in the area. Since our goal was to evaluate the impacts of the referendum results, we stressed that the information we provided was impartial: we informed participants that we were not partisan and signed in the Information Sheet that all of the provided information was true (see Appendix A0 in [Supporting Information](#)).

Our intervention ran as follows. We conducted a seven-question quiz and the details of all questions are provided in the [Supporting Information](#). Participants were paid £1 per correct answer, and they were informed of their accuracy immediately after they responded to each question. The first two questions were the same for all participants, whereas the remaining questions varied depending on the assigned experimental condition: Leave, Remain or Control. The third, a four-choice question, was designed to prime Brexit for participants assigned to the two latter conditions. They were asked 'When is the UK scheduled to leave the European Union?', whereas individuals in the control group were asked 'How many times has London hosted the Olympic Games?'

The remaining questions were designed to inform participants about how the majority voted in their local constituencies, forming the key part of the intervention. We utilized animated maps of the UK, as explained above. Participants were instructed to wait for the selection of an area without any explanation of the computer's selection process.

For those in the Leave or Remain treatment groups, the fourth question was: 'How did the majority of voters in (constituency X) vote in the 2016 EU referendum?' Participants had two response options: 'The majority voted to LEAVE the EU' or 'The majority voted to REMAIN in the EU'. After another apparent random draw by the computer, the fifth question mirrored the fourth: 'How did the majority of voters in (constituency Y) vote in the 2016 EU referendum?' The subsequent question repeated this format for a third constituency, asking: 'How did the majority of voters in (constituency W) vote in the 2016 EU referendum?'

In the Remain condition, constituencies X, Y and W were Norwich South, Tunbridge Wells and Winchester, where the majority voted to remain in the EU. In the Leave condition, these constituencies were Mid Norfolk, Sevenoaks, and Gosport, where the majority voted to leave.¹³ The 'Control' group was presented with the same animated UK maps, but their fourth through sixth questions were modified to: 'Is the majority of the population in the constituency of (constituency X, Y, W) male or female?'¹⁴

The final question asked participants to estimate a vote share, with individuals receiving £1 if their answer was within two percentage points of the correct figure. Participants in the Remain (Leave) treatment were posed the question: 'Out of every 100 voters in constituency W, how many do you think voted for the UK to remain in (to leave) the EU in the 2016 referendum?' In Winchester (for the Remain treatment), 60.3% of voters opted to remain, whereas in Gosport (for the Leave treatment), 62% voted to leave. These figures reinforced the information provided in the previous questions, emphasizing that the local majority voted either for or against Brexit, depending on the assigned treatment. For the control group, the final question was slightly different: 'Out of every 100 people living in the South East of England, how many do you think reside in (constituency W)?'

We carefully selected the constituencies that we showed in the maps to ensure symmetry across Remain and Leave conditions. This included geographical proximity (e.g., Mid Norfolk and Norwich South) and comparable vote shares for the winning side (e.g., Gosport with a 62% leave vote share and Winchester with a 60.2% remain vote share). Moreover, we avoided selecting locations where we conducted the survey, as participants, likely local residents, would be more familiar with the local Brexit results in these places. In the [Supporting Information](#), we provide details on all questions and feedback provided in the quiz.

In summary, individuals in the Remain condition were informed that the majority voted to remain in the EU across three constituencies that appeared to be randomly selected, whereas those in the Leave treatment were informed of a Leave-majority vote in the same constituencies. The control group received no information about others' voting behaviour and did not hear any mention

TABLE 1 | Participants' predictions of 'Leave the EU' vote shares in queried locations.

	South East of England	London	Tunbridge Wells	Sevenoaks
REMAIN	42.6	35.9	39.1	45.1
CONTROL	51.6	43.5	54.3	54.7
LEAVE	60.5	52.5	59.1	61.2
Actual 'LEAVE' vote-share	51.8	40.1	45.1	54.4
<i>Differences across pairwise treatments in percentage points</i>				
REMAIN—CONTROL	−9.0	−7.6	−15.2	−9.7
LEAVE—CONTROL	8.9	9.0	4.8	6.5
LEAVE—REMAIN	17.8	16.6	20.1	16.2

Note: These predictions were accessed after the quiz and refer to different places than the ones asked in the quiz. All differences are significant at the 5% level.

of Brexit at all, which implies that their perceptions of Brexit support remained unaffected by the intervention.

After the quiz, we asked participants to estimate the proportion of votes favouring Brexit across several locations (distinct from those in the quiz).¹⁵ Table 1 shows the average estimates by treatment group. Participants in the Leave condition estimated significantly higher Brexit vote shares, 16–20 percentage points higher (or between 36% and 51%), than those in the Remain condition. The control group, uninfluenced by information, provided intermediate estimates that were closer to the actual Brexit vote share. These results suggest that participants adjusted their beliefs about the Brexit vote in response to the experimental information.

We did not collect information on pre-intervention Brexit vote priors for the same locations (which we avoided to prevent potential priming), so we cannot verify the conjecture of prior updating with certainty or identify individuals who may have changed their priors as a result of the information we provided. However, we collected information on demographics and other characteristics, including pre-intervention political questions, which are balanced across treatments, as shown in Tables 3 and Table A5 in Supporting Information, indicating that the randomization was successful. Presumably, it was similarly effective in balancing participants' awareness of the Brexit vote across groups.

Our assumption was that individuals lacked complete knowledge of regional Brexit outcomes, making them susceptible to influence from the information provided. If participants had perfect knowledge of local voting outcomes, it would be unlikely that we would have observed the differences across treatment groups or the shifts in responses aligned with the feedback provided during the quiz. Furthermore, the quiz questions were incentivized, yet only 20% of participants answered all three map questions correctly.

However, our data indicate that participants were not entirely uninformed. Table 1 shows that across all conditions, participants consistently guessed lower Leave vote shares for London than for other locations in the South East, such as Tunbridge Wells or Sevenoaks, suggesting some awareness of regional voting patterns.

At the national level, the Brexit vote was split almost evenly (51.9% for Leave, with 51.8% in the South East), but a majority of counties leaned Leave; of 359 voting areas, 246 (68%) favoured Leave (UK Electoral Commission).¹⁶ Even if participants were unaware of the exact Brexit results in a particular location, they may have assumed that most constituencies leaned towards Leave. To explore this assumption, that a Leave guess was perceived to be more likely, we examined responses to the first map question, asked before any feedback was given (see Table A2 in Supporting Information). In this question, 80% of participants in the Leave condition and 64% in the Remain condition guessed that the constituency had voted Leave, indicating a prior belief skewed towards Leave.¹⁷

Given the geography of Brexit votes, the Remain treatment likely provided the most surprising information. As shown in Table A2 in Supporting Information, the pattern of guesses varied between treatments. In the Remain condition, the feedback prompted participants to increase the probability of guessing Remain for the next constituency, indicating learning about constituencies' Brexit votes based on the information provided in the quiz. In contrast, participants in the Leave condition initially had a higher likelihood of guessing correctly than those in the Remain condition (80% vs. 35%). However, after receiving feedback that the previous constituency had voted Leave, they consistently decreased the probability of guessing Leave for the next constituency, seemingly expecting that a Remain constituency would eventually be selected.

3 | Data

3.1 | Outcomes

We study the effects of the intervention on implicit and explicit measures of discrimination,¹⁸ grouped into four outcome domains: (i) Implicit Association Test (IAT) scores; (ii) views on immigrants; (iii) views on policies; and (iv) giving in a dictator game. Views and policy preferences (items (ii) and (iii)) are measured using multiple survey questions, which we aggregate into summary indexes following Anderson [4].

A key advantage of using indexes is that they allow us to assess whether the experimental intervention had an overall effect by

combining information from multiple related outcomes. Indexes also tend to be more statistically powerful than tests of individual items, as they place less weight on highly correlated outcomes and greater weight on less correlated ones, which provide additional information [4, 43]. All indexes are constructed so that higher values correspond to more negative attitudes towards immigrants.

Our primary analysis focuses on six outcomes: the second and third IAT scores, the standardized views index, the standardized policy index, the donation amount in the dictator game, and an indicator for donating less than half of the endowment. Together, these outcomes capture the four conceptual domains described above. All individual survey items underlying the summary indexes are treated as secondary outcomes and are reported in the [Supporting Information](#). To address concerns related to multiple hypothesis testing, we apply false discovery rate adjustments to these six primary outcomes across the three pairwise treatment comparisons.

3.1.1 | Implicit Association Test

We assessed implicit attitudes towards British and non-British nationals by applying the Implicit Association Test (IAT) developed by [3]. This psychological test is a computer-based double-characterization task that has been utilized extensively to study racial prejudice in Social Psychology [44–46] and in Economics [47–50].

Our test intended to measure the strength of respondents' associations between UK and non-UK names with positive and negative concepts by comparing the fluency (measured in reaction time) with which the individual matched in-group or out-of-group names, such as Henry or Hans, with words that carry a positive or negative meaning, such as 'peace' or 'anger'.¹⁹

The idea is that the faster one assigns the name to a category (positive or negative), the easier is the characterization, and hence, the stronger the association becomes. The IAT result we use is the D-measure [52]. This score was calculated based on the differences between the average response times from the combined critical contrasting blocks (i.e., blocks 4 and 7), divided by the standard deviation of all latencies for both blocks (as in [52]).²⁰ Within this measure, an increase (decrease) in reaction times to match non-UK names with unpleasant (pleasant) words indicates implicit prejudice towards non-UK individuals. Higher values of the IAT indicate stronger implicit prejudice towards non-UK individuals.

We conducted the IAT three times over the survey. The first time was before the intervention to measure participants' pre-existing implicit views. The second time was just after the quiz to isolate the effect of the Brexit vote-share information itself, and the third time was by the end of the survey, after participants might have acted on the vote-information, which could have changed them. It is worth mentioning that the IAT is used in studies using priming interventions, indicating that this measure is somewhat malleable [46, 53].

3.1.2 | Views on Immigrants

On a 5-point scale, we asked the extent to which participants agree with the following statements: 'The South East of England is made a better place by people coming to live in the UK from Europe' and 'London is made a better place by people coming to live in the UK from Europe'. We also asked 'Most people who come to live in the South East of England pay taxes. They also use health and welfare services. On balance, do you think Europeans who come to the South East of England take out more than they put in, or put in more than they take out?'. The answers varied on a five-point scale from 'Take out much more than they put in' to 'Put in much more than they take out'. We repeated this question, asking instead about people that come to live in London. The remaining two questions were, on a five-point scale, the extent to which the participant agrees with the following statements: 'Most foreign people living here who receive support from the state could get along without it if they tried' and 'It makes sense for foreign people to live in their own neighbourhoods because they share more and get along better than they would if they mixed with British people'.

3.1.3 | Policy Preferences

To assess attitudes about immigration, participants were asked, using a five-point scale, the extent to which they agree with the establishment of targets to reduce immigration, quotas for non-Europeans and quotas for Europeans to settle in the UK, in three separate questions. We asked what Britain's long-term policy should be using the same wording as in the British Social Attitudes Survey. This was a five-choice question varying along the scale of complete integration to separation from Europe, including options such as 'Work for the formation of a single European government' to 'Leave the European Union'.²¹ We created an indicator for whether the participant answered that the UK should leave the European Union.

Related to access to public resources, we asked whether non-UK citizens living in the UK should pay a charge for their use of the NHS,²² and we created an indicator for whether the respondent answered that the NHS should be free for all those living in the UK.

Lastly, we considered a nationalistic proposal by a right-wing party, the UK Independence Party (UKIP), in their 2017 Manifesto. We asked the extent to which, on a five-point scale, participants agree with the statement that 'Britain should implement the end of multi-lingual formatting on official documents. Official documents should be published only in English and, where appropriate, Welsh and Gaelic'.

3.1.4 | Giving

Generosity in our study is measured by the amount of money an individual allocates to another participant in a dictator game. The donor (comprising half of our sample) was always a British national and was asked to divide £10 between themselves and an

anonymous participant. The recipient's identity was manipulated by assigning either a European-sounding name (Pawel or Hans) or a British-sounding name (Henry).

Our initial hypothesis was that participants in the Leave condition would exhibit less generosity towards non-British recipients than towards British recipients, whereas those in the Remain condition would show greater generosity. We did not observe significant differences in generosity based on recipient names, as shown in Figure A2 in [Supporting Information](#), contrary to our initial conjecture, but this may be due to sample size limitations. To enhance statistical power, we aggregated the data across all recipients in the main analysis and tested whether generosity varies according to participants' assignment to the Leave or Remain condition.²³

It is plausible that individuals' generosity responds to these treatments if the perceived outcome of the Brexit vote signals broader societal attitudes towards generosity. Given the strong anti-immigration rhetoric associated with the Leave campaign [1], we hypothesized that participants in the Leave condition would donate less compared to those in the Remain condition.

The majority of participants (73.5%) gave £5, which is the most equitable split. We examine both the amount given and whether the amount given was less than £5, considered an unfair donation.²⁴

3.2 | Sample

In total, 342 individuals participated, comprising 169 members of the general public and 173 university students. The sample is composed of 75% British nationals, 61% females, 53% college graduates, and 59% individuals younger than 25 years old. Most British participants are from the general public (62%), with a larger participation rate among the older population (49% are older than 25, and 22% are older than 55). The sample is not representative of the population and better illustrates the views and demographics of university students or individuals who use public libraries and were willing to attend sessions during the day. We do not have a strong prior reason to believe this sample would react differently, but this is a limitation of our study.

In Table A3 in [Supporting Information](#), we show the summary statistics for all outcomes for the overall sample and by experimental condition. Among the control group, 23.2% of individuals stated that the UK should leave the EU in the long run (this proportion was 29.7% for participants who are British nationals). This fraction is lower than in the British Social Survey (41% in 2016), suggesting that the sample used in this study has a higher proportion of Remainers than the UK population.

Possibly due to the in-person nature of the survey, attrition was low. Response rates exceed 90% for all survey questions across treatment arms. For a small subset of items, respondents assigned to the control group are more likely to provide an answer, and there are no differences between the Remain and Leave conditions (Table A3 in [Supporting Information](#)). Outcome-specific sample sizes are reported throughout the results tables.

TABLE 2 | Number of observations by treatment.

	Dictator [I]	Recipient [II]	Total [I] + [II]
REMAIN	58	56	114
CONTROL	57	58	115
LEAVE	56	57	113
Total	171	171	342

3.3 | Experimental Design and Randomization

Table 2 provides the number of participants by treatment.²⁵

Randomization was conducted at the individual level using Qualtrics software. Most participant characteristics did not predict assignment to the quiz treatment, with averages across the three conditions being similar, as shown in Table 3. In Table A5 of the [Supporting Information](#), we present *p* values from *t*-tests assessing the null hypothesis of mean equality across pairwise treatments for all characteristics listed in Table 3. Of the 57 pairwise mean comparisons, only 3 resulted in *p* values below 0.10. These significant differences suggest that participants in the control group were less likely to express a preference for the Labour Party and were more likely to be married.

To improve precision and account for any differences in observable characteristics across conditions, the main regression includes covariates for party preference and demographic variables.

3.4 | Statistical Power

A limitation of this study is the relatively small number of participants in each treatment arm (114 per group), which constrains the magnitude of effects that can be reliably detected. To assess the study's ability to identify treatment effects, we conducted a simulation-based power analysis using the same empirical specification employed in Section 4.1, including all covariates, the three treatment indicators, and clustering of standard errors.

For each simulation, we generated synthetic outcomes based on the residual distribution of the baseline specification, imposed a pre-specified effect size on the relevant treatment condition, re-estimated our regression model, and recorded whether the estimated treatment effect was statistically significant at the 5% level. We repeated this procedure 1000 times for each candidate effect size to infer the statistical power of the design. The simulations show that:

- For Remain–Leave comparisons, where both groups receive politically salient but contrasting information, the design achieves approximately 80% power to detect effects of around 0.21–0.23 standard deviations for continuous outcomes observed for all participants (e.g., the policy and views indexes and the IAT measures). In contrast, Remain–Control and Leave–Control comparisons reach 80% power for substantially larger effects (above 0.6 standard deviations). Small effects (below 0.15 standard

TABLE 3 | Participant demographics and characteristics: Overall and by treatment group.

	ALL	REMAIN	CONTROL	LEAVE
British	0.746	0.754	0.739	0.743
Female	0.611	0.571	0.596	0.664
< 25 years old	0.587	0.616	0.535	0.611
25–54 years old	0.239	0.223	0.246	0.248
> 55 years old	0.174	0.161	0.219	0.142
Married	0.180	0.188	0.219	0.133
Living with children	0.139	0.152	0.140	0.124
University degree	0.530	0.523	0.555	0.514
Employed (full + half time)	0.192	0.161	0.184	0.230
Unemployed	0.080	0.080	0.070	0.088
Voted last election	0.640	0.643	0.640	0.637
Close to political party	0.644	0.614	0.630	0.691
Labour Party preference	0.324	0.393	0.211	0.372
% Leave-vote in own constituency	0.496	0.498	0.505	0.487
IAT (pre-determined)	0.699	0.681	0.726	0.688
General public sample	0.494	0.526	0.478	0.478
Political knowledge score (0–2)	1.229	1.195	1.296	1.196
Non-British university student	0.222	0.219	0.217	0.230
Familiarity with places shown in the quiz (0–3)	1.061	1.079	1.096	1.009
<i>n</i>	342	114	115	113

Note: Political knowledge score is defined as the number of correct answers to the two questions asked prior to the map intervention, which were consistent across all treatments. Familiarity with the places shown in the quiz is measured by the number of locations the participant had previously visited among those presented in the quiz intervention.

deviations) cannot be detected with high probability in any comparison, which helps explain the imprecision observed for individual survey items. Consistent with these power calculations, the few individual survey-item estimates that are statistically significant exceed the corresponding detectable thresholds, whereas non-significant estimates are uniformly smaller than the minimum detectable effects and are therefore uninformative about the presence of small treatment effects.

- ii. For donation outcomes observed in only half of the sample ($N = 171$), statistical power is lower; only effects in the range of 0.30–0.35 standard deviations are detectable with high probability.

Tables A6 and A7 in [Supporting Information](#) report the full simulation results, including power estimates and minimum detectable effects. In sum, the experiment is sufficiently powered to detect moderate differences between the Remain and Leave treatments, particularly when using the policy and views indices that aggregate multiple related items. However, it is underpowered to detect small effects or differences between either political treatment and the control group.

Given these constraints, non-significant differences involving the control group should not be interpreted as evidence of no effect. Consistent with this, the analysis in Section 4 shows that most statistically significant findings arise from comparisons between the Leave and Remain groups, where the experimental interventions are most sharply differentiated.

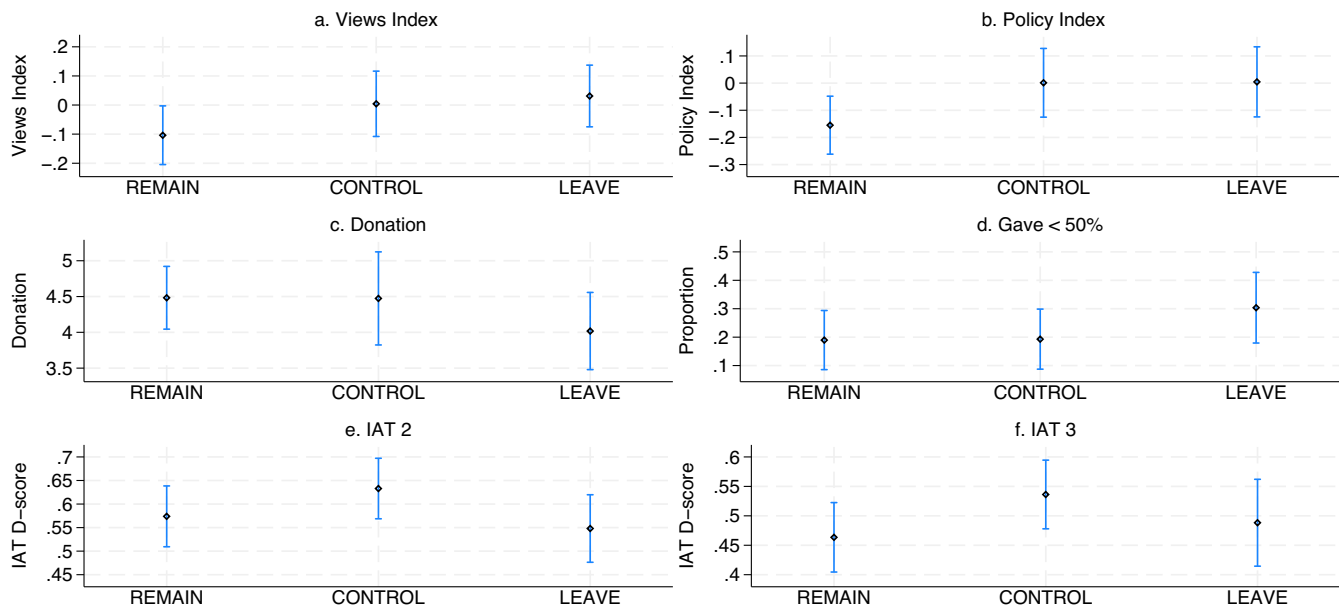
At the time of our experiment, Brexit remained a divisive and highly salient topic, with views largely stable since the aftermath of the EU referendum. We hypothesized that individuals could still be influenced by information about the regional majority voting outcomes, although the magnitude of any such effects is ultimately an empirical question.

Our intervention differs from much of the closest literature, which examines responses to large real-world political shocks (such as the referendum itself) rather than to relatively subtle experimental information treatments. To our knowledge, there are no other interventions that are directly comparable to ours in terms of experimentally manipulating perceptions of local voting outcomes. However, studies that are more closely comparable in design, such as Grigorieff et al. [20], which examine information interventions correcting misperceptions about immigration, report detectable effects on policy-view indices in the range of 0.13–0.25 standard deviations. These magnitudes are consistent with the minimum detectable effects for our Leave–Remain comparison.²⁶

4 | Results

4.1 | Main Effects

We examine treatment effects both visually and through regression analysis. Figure 2 presents the main results, displaying



Notes: Each panel reports mean outcomes by experimental treatment with 95% confidence intervals. Views index aggregates eight survey items capturing respondents' views on immigrants (Section 3.1.2), following Anderson (2008). Policy index aggregates six survey items capturing respondents' policy preferences toward immigration (Section 3.1.3), following Anderson (2008). Donation is the dictator-game amount given to a hypothetical participant, varying from 0?10 (Section 3.1.4). Proportion gave < 50% is an indicator equals one if the dictator-game donation is below 50 percent, and zero otherwise (Section 3.1.4). IAT 2 is the standardized D-score IAT administered immediately after the quiz (Section 3.1.1); higher values indicate stronger implicit bias. IAT 3 is the standardized D-score from the end-of-survey IAT (Section 3.1.1); higher values indicate stronger implicit bias.

FIGURE 2 | Outcomes by experimental treatment.

unconditional averages by experimental condition with 95% confidence intervals. For all outcomes in Figure 2, the confidence intervals overlap across treatments, indicating no clear differences across experimental conditions. Additionally, Table A8 in Supporting Information provides treatment averages for all outcomes, along with p values for pairwise treatment comparisons.²⁷

Figure 2a,b show average values of the views and policy indexes by treatment group. Participants in the Remain treatment tend to hold more positive views towards immigrants and to favour less restrictive policies compared to those in the control group and the Leave treatment, with the largest differences observed between the Leave and Remain groups. Although these differences are not statistically distinguishable in the unconditional comparisons, the direction of the estimated differences is consistent with the paper's main hypothesis. The descriptive patterns also suggest that the Remain treatment, which introduced unexpected information, may have had a greater influence on attitudes than the Leave treatment.

Regarding giving, participants in the Leave treatment donated smaller amounts on average than those in the control or Remain groups (4.01 vs. 4.47 and 4.48, respectively) and were more likely to donate less than half of their endowment. These differences are directionally consistent with our expectations, although they are estimated imprecisely.

For intrinsic measures (IAT2 and IAT3), average scores are very similar across treatment groups, indicating that exposure to the information treatments does not substantially shift implicit

attitudes towards immigrants, with any effects appearing to be either null or very small.

We then proceed to the regression analysis to assess statistically significant impacts in a more controlled manner. We estimate Equation (1), in which $Leave_i$ and $Remain_i$ are indicators for the respective treatments that individual i is assigned to. $\mathbf{X}_i$ is a vector of covariates including gender, age, marital status, British nationality, 11 political party preference indicators and a dummy for whether the respondent was recruited from the general public, and ε_i is a random disturbance term. Robust standard errors are clustered at the level of day-place the survey was conducted.²⁸

$$outcome_i = \alpha + \gamma Leave_i + \delta Remain_i + \theta \mathbf{X}_i + \varepsilon_i \quad (1)$$

The estimated coefficients in (1) are impacts, measured as the difference in average outcomes with respect to the control group. To test for different impacts between the Leave and Remain treatments, we report the p value for a two-tailed equality test between γ and δ .²⁹

To facilitate interpretation, we standardize the policy and view indexes. Thus, estimates for these outcomes represent changes in standard deviations associated with each treatment condition.

Table 4 reports treatment effects for the six primary outcomes. For the policy index, participants assigned to the Leave condition score higher, indicating more restrictive preferences than those in the Remain condition. The estimated difference between the Leave and Remain treatments is approximately 0.23 standard deviations.

TABLE 4 | Impact of treatments on extrinsic and intrinsic outcome measures.

	Views index	Policy index	Amount given	Gave less than £5 (50%)	Second IAT	Third IAT
Control mean	0.048	0.078	4.474	0.193	0.633	0.536
Leave	0.0981 [0.102]	0.120 [0.142]	−0.245 [0.398]	0.0619 [0.0637]	−0.0621 [0.0637]	−0.0334 [0.0298]
Remain	−0.119 [0.137]	−0.103 [0.146]	0.178 [0.400]	−0.0724 [0.0720]	−0.0166 [0.0265]	−0.0446 [0.0323]
<i>p</i> value (Remain = Leave)	0.12	0.03**	0.15	0.08*	0.41	0.75
Observations	329	317	171	171	253	280

Note: The second IAT_D refers to the score of the IAT administered just after the experimental intervention. The third IAT_D refers to the IAT administered at the end of the survey. Each column in each panel presents estimates from a separate regression. All regressions include controls for gender, age, marital status, political party preference indicators, and dummies for whether the participant is British and whether the participant was recruited from the general public. Regressions using IAT as the dependent variable also include a control for the pre-determined IAT conducted before the quiz. Robust standard errors are clustered at the session-day level and are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In terms of views about immigrants, the pattern observed in Figure 2a suggests that negative views are more prevalent among individuals assigned to the Leave condition than to the ‘Control’ or to the Remain treatment, though these differences are not statistically significant for the overall views index. In terms of giving, the regression results detect directionally consistent but statistically imprecise differences between the Remain and Leave conditions. Specifically, participants assigned to the Leave treatment are 11 p.p. (or 37%) more likely to give less than half of the endowment (p value, Leave vs. Remain = 0.083). We find no statistically significant effects of the political treatments on implicit attitudes as measured by the second or third IAT. Estimated coefficients are small and imprecise.³⁰

In Table A10 in Supporting Information, we report results for the individual survey statements that compose the summary indices. Out of 12 items, 4 exhibit statistically significant Leave–Remain differences at the 6% level or better, whereas the remaining 8 are not statistically significant. These significant item-level estimates are directionally consistent with the main hypothesis, particularly for outcomes related to access to public resources and nationalist policy preferences. Individuals assigned to the Leave treatment are 10.6 percentage points (p.p.), or 25%, less likely than those in the Remain treatment to state that the NHS should be free for all (p value, Leave vs. Remain = 0.045). They are also 9.3 p.p., or 45%, more likely to agree with putting an end to multilingual official documentation (p value, Leave vs. Remain = 0.033). In addition, assignment to the Leave treatment is associated with stronger agreement, relative to the Remain treatment, with statements such as ‘Europeans who come to London take out more than they put in’ (p value, Leave vs. Remain = 0.028) and ‘Foreign people could get along without support if they tried harder’ (p value, Leave vs. Remain = 0.058). Some individual outcomes also display similar movements in both the Leave and Remain treatments relative to the control group. We do not interpret these hill-shaped patterns as evidence of treatment effects and discuss them further below.³¹

Despite the presence of statistically significant results under conventional inference, none of the main effect estimates in Table 4 remain statistically significant after false discovery rate

adjustment (Table A11 in Supporting Information). At the same time, consistent with the simulation-based power analysis in Table A7 in Supporting Information, the magnitudes of estimates that are statistically significant under conventional inference are at or above the minimum detectable effect sizes implied by the design, whereas non-significant estimates are smaller than these thresholds. We therefore interpret statistically significant estimates as being consistent with the detectability of the design, whereas viewing non-significant results as inconclusive rather than as evidence of no effect.

4.2 | Instrumental Variable Regressions

To further examine the role of participants’ perceived local Brexit support, we implement an instrumental variable strategy in which participants’ guessed Leave vote share in the South East is instrumented by random assignment to the Leave versus Remain treatment. As shown in Table 1, treatment assignment strongly predicts perceived vote shares. In the first stage reported in column 1 of Table 5, the coefficient on the Leave indicator is statistically significant and the F -statistic of 15 indicates that the instrument is relevant.

A potential concern is that the treatments may affect participants through channels other than information about the majority vote. Both the Leave and Remain treatments prime participants to think about Brexit, whereas the control group receives no such mention (Appendix A2 in Supporting Information). Because both treatment groups are exposed to identical Brexit-related framing, general priming effects are likely to be similar across these two conditions, with the main variation arising from the direction of the majority-vote information. We therefore conduct the IV analysis on the subsample of individuals assigned to the Leave and Remain treatments only, excluding the control group. This restriction is intended to strengthen the plausibility of the exclusion restriction by ensuring that all observations in the IV sample are exposed to a comparable level of Brexit salience, so that the instrument primarily captures variation in the perceived direction of the majority signal rather than variation in whether Brexit is mentioned at all. Although this restriction reduces concerns

TABLE 5 | Effect of perceived Brexit vote shares on participant attitudes.

	Independent variable			<i>n</i>
	[1]	[2]	[3]	
	Coefficient			
	LEAVE indicator First stage (OLS)	Guess on % Brexit-vote in the South East		
		OLS	IV	
Outcomes				
Guess on % Brexit-vote in the South East	18.13*** [2.169]			218
Views index		0.0108** [0.00482]	0.0121 [0.00686]	217
Policy index		0.00878** [0.00335]	0.0119** [0.00517]	206
Amount given		0.00327 [0.0125]	−0.0270 [0.0185]	111
Gave less than 5 (out of 10)		−0.00056 [0.00297]	0.00781* [0.00431]	111
Second IAT_D		−0.000996 [0.00171]	−0.00264 [0.00306]	169
Third IAT_D		−0.000660 [0.000694]	0.000467 [0.00193]	185

Note: Each entry presents estimates from a separate regression. The key independent variable in regression in column 1 is an indicator the Leave treatment. All regressions include covariates for gender, age, marital status, political party preference indicators, and dummies for whether the participant is British and whether the participant was recruited from the general public. Regressions using IAT as the dependent variable also include a control for the pre-determined IAT conducted before the quiz. Robust standard errors are clustered at the session-day level. ** $p < 0.05$, * $p < 0.1$. Each entry in columns [1] and [2] presents estimates for a separate regression. All regressions include covariates for gender, marital status, political party preference indicators and dummies for whether the participant is British and if the participant was recruited from the general public. Robust standard errors are clustered at the session-day level. ** Statistically significant at the 5% level, * Statistically significant at the 10% level.

that the instrument captures general Brexit priming, it does not fully eliminate the possibility that the Leave and Remain treatments differ in the salience of the information provided. We discuss this limitation further below.^{32, 33}

Column 2 of Table 5 reports OLS correlations between perceived Brexit support and outcomes. Higher perceived Leave vote shares are associated with more negative views towards immigrants and more restrictive policy preferences, though these correlations may reflect personal political views, social networks, and media exposure. Column 3 reports the corresponding IV estimates. The point estimates indicate that perceiving a larger share of the Brexit vote increases the policy index (p value = 0.038). Specifically, a 10-percentage-point increase in the perceived Brexit vote share leads to a 0.119 standard deviation rise in the policy index.³⁴

We do not find statistically significant evidence of an effect on views (p value = 0.101), though the estimated coefficient points in the expected direction.

The IV estimates also reveal a small, negative impact of the perceived Brexit vote on generosity, measured by the likelihood of donating an unfair amount. This effect is only marginally significant (p value = 0.095).

4.3 | Heterogeneous Treatment Effects

This section explores heterogeneous treatment effects across selected subgroups to provide descriptive evidence on potential mechanisms underlying the main results. These analyses are explicitly exploratory in nature. They were not pre-registered, involve a large number of pairwise comparisons, and are limited by reduced statistical power within subgroups. In Table A14 in Supporting Information, we provide results with standard errors corrected for multiple hypothesis testing in regressions.³⁵

In Table 6, we examine effects by subgroups to better understand whether the patterns observed in the main analysis are concentrated among participants for whom the information may have been more novel. One conjecture is that the majority signal may be more influential when it conveys information that is unexpected relative to participants' priors. We attempted to mimic this in our data by identifying participants exposed to relatively more surprising information. To test the hypothesis that the treatment effects were driven by 'learning' about population preferences, we decomposed the data based on participants' performance on the map quiz. We divided the sample into two groups based on whether the information was likely 'expected' or 'unexpected'

TABLE 6 | Heterogeneous treatment effects by knowledge and political views.

Panel A: By performance on the referendum results questions										
Non-surprising information (mostly correct answers)						Surprising information (mostly incorrect answers)				
Index			Index							
Views [1]	Policy [2]	Amount given [3]	Gave less than 5 [4]	Third IAT_D [5]	Views [6]	Policy [7]	Amount given [8]	Gave less than 5 [9]	Third IAT_D [10]	
Control mean	−0.057	−0.012	0.212	0.531	0.176	0.185	4.375	0.167	0.544	
Leave	0.0974 [0.194]	0.144 [0.195]	−0.244 [0.639]	−0.0349 [0.0461]	0.403* [0.188]	0.175 [0.247]	−0.746 [0.588]	0.304* [0.155]	−0.00525 [0.0754]	
Remain	−0.0668 [0.197]	−0.0782 [0.198]	−0.0442 [0.726]	−0.0104 [0.0586]	−0.188 [0.207]	−0.128 [0.316]	0.659 [0.442]	−0.0751 [0.120]	−0.0241 [0.0390]	
<i>p</i> value (Remain = Leave)	0.451	0.201	0.747	0.665	0.001***	0.280	0.028**	0.048**	0.779	
Observations	198	189	105	174	131	128	66	66	106	

Panel B: By political views										
Pro-Remain					Pro-Leave					
Index			Index							
Views [1]	Policy [2]	Amount given [3]	Gave less than 5 [4]	Third IAT_D [5]	Views [6]	Policy [7]	Amount given [8]	Gave less than 5 [9]	Third IAT_D [10]	
Control mean	−0.268	−0.330	0.167	0.516	0.382	0.515	4.513	0.205	0.561	
Leave	0.178 [0.211]	0.117 [0.171]	−0.212 [0.462]	−0.0583 [0.0389]	−0.0532 [0.254]	−0.0116 [0.154]	−0.00142 [0.516]	−0.00762 [0.0941]	0.0134 [0.0583]	
Remain	−0.0963 [0.121]	0.0614 [0.163]	0.157 [0.370]	−0.0519 [0.0391]	−0.266 [0.221]	−0.569*** [0.180]	0.475 [0.632]	−0.175 [0.102]	−0.0350 [0.0691]	
<i>p</i> value (Remain = Leave)	0.099*	0.672	0.336	0.856	0.486	0.015**	0.385	0.143	0.457	
Observations	186	180	74	164	143	137	97	97	116	

Note: The third IAT_D refers to the IAT administered at the end of the survey. Each column in each panel presents estimates from a separate regression. All regressions in Panel A include covariates for gender, age, marital status, political party preference indicators, dummies for whether the participant is British and whether the participant was recruited from the general public, and the score for the first two quiz questions (the same for all). All regressions in Panel B include covariates for gender, age, marital status, and a dummy for whether the participant was recruited from the general public. The regressions in columns 5 and 10 also control for the pre-determined IAT. Robust standard errors are clustered at the session-day level and are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

and novel. The first group includes individuals who scored 2 or 3 correct answers out of 3, whereas the second group consists of those who scored 0 or 1 correct answer. To account for prior political knowledge, we used the baseline specification and controlled for the score on the first two quiz questions, which were identical across all treatments (Table 3).³⁶ The results of this analysis are presented in Table 6, Panel A.

Among individuals who performed well on the quiz (Columns 1–5), treatment effects are generally small and imprecisely estimated. In contrast, among individuals who performed poorly on the quiz (Columns 6–10), treatment effects are larger for several outcomes. Noteworthy, within this latter low-performing group, individuals assigned to the Leave treatment express significantly more negative views towards immigrants than those in the Remain treatment. The estimated Leave–Remain difference in the views index is 0.591 standard deviations and remains statistically significant after correction for multiple hypothesis testing (Table A14A in [Supporting Information](#)). Point estimates for giving behaviour (Panel A, Columns 8–9) are also larger in magnitude and significant in this subgroup, although these effects are not robust to multiple-testing adjustment. Taken together, these results suggest that the strongest impact of the Leave–Remain contrast is concentrated among participants who were provided with more surprising information about Brexit support.

Next, we divided the sample based on individual characteristics known to predict alignment with Brexit. To avoid general priming of Brexit, contamination across treatments, or suspicions about the study's aim, we did not ask participants about their voting choice in the EU referendum. Instead, we focused on their views regarding the long-term relationship between the UK and Europe (as noted earlier, this was asked after the experimental intervention). Based on participants' current views on the EU–UK relationship, we classified them as either 'Pro-Remain' or 'Pro-Leave'.^{37,38} Panel B reports heterogeneous treatment effects by this classification.

The most pronounced and statistically robust effects are concentrated among likely Pro-Leave participants. For this group, exposure to the Remain treatment reduces the policy index by approximately 0.56–0.58 standard deviations relative to the Leave and Control treatments, and these differences remain statistically significant after multiple-testing correction (Table A14A in [Supporting Information](#)). Among Pro-Remain participants, differences across treatments are smaller and not statistically significant, although they are directionally consistent for the view index.

No significant treatment effects are detected for the IAT outcomes in either subgroup, suggesting that the treatment effects arise primarily for explicit attitudes, noticed on the policy and views indexes.

In Tables A16 and A17, we report the main results separately by nationality (British vs. non-British nationals). Among non-British nationals (mostly university students), point estimates indicate more negative policy views in both treatment conditions relative to the control group for some outcomes. However, these patterns are imprecisely estimated and do not

survive multiple-testing adjustment. On the other hand, the estimated effects for the British sample are qualitatively similar to those obtained when including all participants. The heterogeneous patterns by knowledge and political views, reported in Table A18, are also qualitatively similar to those in the main sample when the sample is restricted to British participants only, although the estimates become less precise due to the smaller sample size. Therefore, neither the main results (Table 4) nor the core heterogeneity patterns (Table 6) are driven by the inclusion of non-British students in the sample.

We further investigate how conformism might operate by examining additional subgroups. In Table A19 in [Supporting Information](#), we present results for subgroups based on political engagement (whether participants indicated a party preference) and familiarity with the locations shown on the maps (measured by whether they had visited at least one of the three places depicted). In general, the estimated coefficients do not indicate significant treatment effects. For participants who expressed a party preference, some coefficients associated with the Remain treatment are consistent with weaker anti-immigrant views. These results should be interpreted with caution, as they are not robust to multiple-hypothesis testing (Table A14B in [Supporting Information](#)).

5 | Discussion and Concluding Remarks

In this paper, we conducted a survey experiment in England to examine how information about local support for Brexit, as revealed by referendum results, affects attitudes towards immigrants in the UK. We show that exposure to information indicating that the local majority voted to Leave the EU is likely to cause more negative views towards immigrants and more restrictive preferences regarding immigration-related policies than exposure to information indicating that the local majority voted to Remain.

This paper contributes to the literature on the social consequences of Brexit. Previous studies [1, 6, 59–61] document a short-lived spike in hate crime following the 2016 EU referendum. Hate crimes represent extreme forms of negative behaviour and are committed by a small fraction of the population. Our focus is instead on more subtle and widespread forms of attitudinal and policy-based responses that may persist over a longer horizon. Our experiment was conducted 17–22 months after the referendum, suggesting that majority information may continue to shape expressed attitudes well beyond the immediate post-referendum period.

The heterogeneity analysis, which is exploratory in nature, reveals that larger effects are concentrated in specific subgroups. Individuals exposed to more surprising or novel information exhibit substantial shifts in explicit views and policy preferences when exposed to the political-majority signal. Similarly, individuals predicted to be Pro-Leave exhibit shifts in policy preferences when exposed to the Remain-majority signal, which constrains their inherent inclination. These results suggest that majority-vote information can have meaningful effects when it is either unexpected or politically dissonant.

It is important to note that our findings do not necessarily imply that individuals perceive majority opinion as an emerging ‘new social norm’ to which they mechanically conform. Conformist responses may arise from a variety of intrinsic motivations, including the desire to fit in, to align with prevailing views, or from informational updating in the presence of uncertainty [33].³⁹ A related concern is that the estimated effects could, in part, reflect experimenter-demand responses or social desirability bias, since the intervention repeatedly presented the same majority choice three times. Although we cannot entirely rule this out, the observed pattern of heterogeneous responses is difficult to reconcile with a simple demand interpretation. If respondents were merely inferring and complying with the perceived experimental hypothesis, the strongest effects would be expected when the treatment information aligned with their political orientation, rather than when it conflicted with it (Table 6, Panel B). Moreover, if respondents reacted to the information because they felt intimidated or socially pressured by it, this mechanism closely resembles the way similar information is encountered through social interactions in real-world settings.

Our results are subject to several limitations. First, the sample size is small (342 participants), resulting in limited statistical power to detect small or subtle effects, particularly at the level of individual outcomes and within subgroups. Consequently, the analysis is underpowered to detect effects on some outcomes, such as donations or intrinsic preferences. Results for these outcomes are therefore inconclusive.

Second, the sample consists of residents from the South East of England, where the Brexit vote closely mirrored the national average. Caution is therefore warranted when generalizing these findings to the UK as a whole. Regional variation in political norms and migration exposure may shape how majority information is interpreted and acted upon. In more homogeneous or politically polarized areas, majority signals may generate different behavioural responses.

Despite these caveats, the results have several implications. First, responses in national surveys may be influenced by conformity or peer effects following salient electoral outcomes, and such effects should be considered when interpreting post-election attitudinal data. Second, the findings underscore the potential importance of majority voting outcomes in shaping individuals’ expressed preferences towards immigration.

The extent to which our experimental results map to observational settings likely depends on which layer of information is most salient to individuals, national vote shares or more localized voting patterns. If the national outcome is the dominant signal, one would expect stronger attitudinal shifts in Remain areas following a national Leave victory, consistent with the evidence in Alborno et al. [5] showing larger post-referendum hate-crime responses in Remain regions. Alternatively, if more local information dominates, individuals in Leave-voting areas embedded within broader Remain regions may respond differently from those in national Leave strongholds.

Overall, this article sheds light on the medium-run social effects of Brexit. The experiment was conducted 17–22 months after the 2016 EU referendum, a period in which views may already

have partially adjusted. Whether Brexit will lead to permanent changes in immigration attitudes in the UK remains an open question and an important avenue for future research.

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Ethics Statement

It obtained Institutional Review Board approval from the University of Kent. This experiment is at the AEA RCT Registry (AEARCTR-0002576).

Endnotes

¹http://www.voteleavetakecontrol.org/briefing_immigration.html, http://www.voteleavetakecontrol.org/why_vote_leave.html.

²For example, the articles above are less suited to drawing conclusions about the impacts of perceived Brexit votes on attitudes.

³Other work on Brexit has focused on evaluating its macroeconomic impacts [8–11] or the determinants of the Brexit vote [12–17]. Fetzer [18] shows that post-2010 austerity policies in the UK contributed to the rise of an extreme right-wing populist party (UKIP) and likely influenced the Leave victory.

⁴The concept of social conformity has been formalized by economists, in models that the peer influence are driven by asymmetry of information ([28]) or identity reasons [29]. An alternative explanation for conformity is the theory of information cascades [30]. Social conformity has been documented across various domains [31–33]; for instance, in political behaviour, Perez-Truglia [26] found that individuals in predominantly Democratic areas contributed more to Barack Obama’s presidential campaign.

⁵One potential mechanism emphasized in this literature is that election or referendum outcomes may shift perceptions of socially acceptable behaviour, for instance by lowering the stigma associated with expressing anti-immigrant views. Bursztyn et al. [23] test this mechanism in an online experiment in the United States, measuring individuals’ willingness to express xenophobic views publicly versus privately in the days surrounding the 2016 U.S. presidential election. They find that following Donald Trump’s victory, participants became more likely to make public, but not private, donations to an anti-immigration organization. Our survey, by contrast, was fully anonymous, and participants were explicitly informed that their responses could not be linked to their identities. As a result, our design is not suited to identifying effects driven by social image concerns, as in Bursztyn et al. [23].

⁶Including both university students and members of the general public in our sample allowed us to capture a broader range of perspectives. The roughly even mix—50% from each group (Table 3) was also crucial for increasing our sample size. We faced higher-than-expected costs associated with surveying in public libraries, due to a high no-show rate among registered participants. Given these logistical challenges and funding constraints, incorporating university students was essential to bolster our sample size.

⁷The evidence in Hugh-Jones [34] hints to this possibility. He conducted a monetary-incentivized quiz among online participants, and

concluded that participants looked up for answers. This happened despite the fact that participants were requested not to search for the correct answers and they ticked a box confirming they had answered on their own.

- ⁸We informed the participants that the researchers were affiliated with the University of Kent in the consent form, but aside from that, there was no other mention that this was a university-related study. We proceed this way because, at that time, there were rumours in society that universities were taking a side and teaching about the adverse possible impacts of Brexit.
- ⁹As explained above, in the instruction sheet we stated that the study was non-partisan and framed it as an investigation of citizens' views on current societal issues. We informed participants that they would be quizzed on UK facts, but only half of the quiz items related to Brexit. Moreover, participants were inexperienced subjects, unrelated to the research team, and had no clear incentive to infer or attend to any hidden hypotheses. Even if this were not the case, the appearance that locations were selected at random helps dispel the perception that we were promoting or supporting either side of the referendum.
- ¹⁰If participants were guessing the study's hypotheses and answering accordingly, then respondents whose political views more closely aligned with the information provided in the quiz, would be more likely to provide answers that are consistent with the study's hypotheses. This was not the case. As shown in Table 6, there is no evidence that 'Brexiters' ('Remainers') were affected by the Leave (Remain) conditions. In contrast, we find that 'Brexiters' were affected by the Remain treatment and that 'Remainers' were affected by the Leave treatment. This pattern of responses in the politically dissonant direction is difficult to reconcile with a simple experimenter-demand interpretation, which would instead predict the strongest responses when treatment information aligns with respondents' prior political views.
- ¹¹NatCen Social Research. "The Brexit Vote: Right or Wrong?" What UK Thinks: EU. Accessed [14/11/2024]. <https://www.whatukthinks.org/uk/explore-by-topic/the-brexit-vote-right-or-wrong/>.
- ¹²For example, a leaked government report suggested that Brexit could reduce the UK's GDP growth by up to 8% over the next 15 years [41]. Other announcements, such as the relocation of the European Banking Authority from London to Paris and the UK's exclusion from the European Capital of Culture program, likely contributed to the decline in public support.
- ¹³We used results at the constituency level, as estimated by Hanretty [42].
- ¹⁴For 50% of the participants, the X, Y, W constituencies were Norwich South, Tunbridge Wells, and Winchester. For the other half, Mid Norfolk, Sevenoaks and Gosport were selected.
- ¹⁵The wording of the question was: Out of every 100 voters in the [place M], how many do you think voted for the UK to leave the European Union in the 2016 EU referendum?
- ¹⁶For the South East, 64% of areas had the majority voting to Leave.
- ¹⁷While these response frequencies suggest that participants had a Leave-leaning bias, this perception might be similar across treatment groups. Most participants (65%) had visited at most one location among the three asked in the quiz, with familiarity (i.e., number of places visited) evenly balanced across conditions.
- ¹⁸The terms—implicit and explicit views—are commonly used in psychology. The main distinction in our case is that the intrinsic measure (IAT) captures subconscious views and responses are not easily manipulated by respondents. On the other hand, the extrinsic measures can be manipulated by respondents and responses can be affected by external factors, such as the expectation of punishment or reward.
- ¹⁹Participants were asked to pair UK/non-UK names with positive/negative attributes as rapidly as possible by sorting a series of UK/non-UK names and good and bad adjectives (that appear in the middle of the screen) into left or right columns with the move of two fingers. The left and right columns sometimes are explained respectively as UK/pleasant and non-UK/unpleasant, and later as UK/unpleasant and non-UK/pleasant. The task was comprised of seven blocks. In Block 1, observers were presented with words, which had to be classified as positive or negative by pressing one of two buttons on a standard computer keyboard. In Block 2, observers were asked to classify names as UK or non-UK names with the same two keys. In Block 3, observers performed the combined categorization of words and names, by requiring pressing of one key if the stimulus was a UK name or a positive word and the other key if the stimulus was a non-UK name or a negative word. This was followed by Block 4, which was identical to Block 3. Block 5 was identical to Block 1 but with reversed keys. In Block 6, observers then performed the combined categorization, but in this case the name-word relation was reverted compared to Blocks 3 and 4. Thus, if Blocks 3 and 4 combined UK with positive and non-UK with negative, then Block 6 combined UK with negative and non-UK with positive. Finally, Block 7 was identical to Block 6. In line with previous work (e.g., [51]), Blocks 1, 2 and 5 were practise trials, whereas Blocks 3, 4, 6 and 7 were the critical blocks that were used to calculate a measure of prejudice. This prejudice measure was based on the reaction times with which observers matched stimuli that correspond either to ingroup or outgroup categories (i.e., UK and non-UK names) when the same response keys are also used to classify words that carry a positive or negative meaning (i.e., peace and anger).
- ²⁰In other words, the IAT D score was calculated by taking the difference in average response times between two contrasting associative blocks. This difference was then divided by the standard deviation, calculated from the pooled trials of both blocks. In the first contrasting block, participants paired UK names with positive words (assigned to the left response key) and non-UK names with negative words (assigned to the right response key). In the second block, the associations were reversed: non-UK names with positive words (left response key) and UK names with negative words (right response key).
- ²¹The remaining and intermediate alternatives were: Stay in the EU but reduce its power, Leave things as they are, and Stay in EU and increase its powers.
- ²²Participants chose from three possible alternatives: (i) Yes, definitely; (ii) Yes, non-UK citizens should pay a charge, unless they are paying taxes in the UK; and (iii) No, the NHS should be free for all those living in the UK.
- ²³All participants (donors and recipients) were paid in cash at the end of the survey, in accordance with the donor's decision. The specific wording for the task was: "You have been paired with another survey participant. He or she is a real person, who also lives in the UK. We will refer to this person as (name). Your team—youself and (name)—have been selected to receive an additional payment of £10 for your joint participation in this survey. However, you will be the one to decide how to split the £10 between yourself and (name)." We assigned the hypothetical name to the recipient. This could have been Henry, Hans or Pawel, with equal probability. As mentioned above, we observed large differences across recipient nationality, as we show in Figure A2 in [Supporting Information](#). The absence of differences across British and non-British names may suggest that out-group biases in giving did not occur more frequently under the Leave treatment compared to the Remain treatment, contrary to our initial hypothesis.
- ²⁴In a meta-analysis of dictator games, Engel [54] found that participants (dictators) typically donate less than half of the endowment. We believe the framing of our endowment, described as a bonus for joint participation, was suggestive and influenced participants to donate £5, as shown in Figure A1 in [Supporting Information](#). This framing was selected after pilot testing and incorporating feedback to ensure it sounded as natural as possible. While we developed our own framing, we follow other work in redistributing the dictator donations within our sample.
- ²⁵As explained above, all donors were British, and the paired-recipient was British or European, resulting in a one-to-one pairing between

donors and recipients. In Table A4 in [Supporting Information](#), a breakdown of Table 2 by nationality is provided.

²⁶The magnitudes reported in Grigorieff et al. [20] are at the upper bound of the literature on correcting misperceptions. Other studies report mixed results [55], with some finding null effects [22, 56], while others find that correcting misperceptions improves perceptions of immigration [57, 58]. As such, the moderate-sized impacts that this study is powered to detect are relatively large relative to this literature. On the other hand, studies of the Brexit referendum itself, which examine hate crime in the immediate aftermath of the vote, document large effects. For example, Devine [59] and Carr et al. [1] find increases in hate crimes following the referendum of approximately 15%–25%, while Alborno et al. [5] report effects of around 13%, with larger impacts in areas that voted to Remain.

²⁷Note that in Table A8, each of the individual pairwise impacts is estimated using two-thirds of the data, as opposed to the analysis in Table 4, which includes the entire dataset considering all three treatments together.

²⁸We also run regressions including covariates for IAT pre-determined score and p-values for the estimated impacts do not increase. These results are not described in the paper, but available under request.

²⁹Because the experiment is primarily powered to compare Leave versus Remain exposure, we additionally estimate a re-parameterized version of (1) with Remain as the omitted category to facilitate this comparison. These results are reported in Table A9 in [Supporting Information](#).

³⁰When we run the regressions using standardized measures of the IAT variables (not shown, but available upon request), the point estimates for the Leave–Remain comparison are 0.05 standard deviations for IAT2 and 0.08 standard deviations for IAT3.

³¹For some outcomes, such as the Implicit Association Tests (IATs), views on whether foreign people should live in their own neighbourhoods, and preferences for immigration quotas or immigration reduction, both the Leave and Remain treatments move estimated outcomes in a similar direction relative to the control group. These patterns do not correspond to the core Leave–Remain contrast of interest. Given the limited statistical power of the study and the number of individual outcomes examined, these “hill-shaped” patterns are consistent with sampling variability rather than with systematic treatment effects. We therefore refrain from assigning a causal interpretation to these results.

³²This strategy does not fully eliminate the possibility that salience itself differs across the two treatment conditions. In particular, information that a local majority voted to Leave or to Remain may generate different degrees of psychological activation. As a result, the IV estimates should be interpreted as identifying the effect of perceived majority-vote direction conditional on Brexit being salient, rather than as isolating a fully salience-free belief-updating channel.

³³As a robustness check, we also estimate the IV specification on the full sample including the control group; these results are reported in Table A12 in [Supporting Information](#) and are qualitatively similar in directional terms.

³⁴This effect is driven primarily by a greater proportion of participants expressing less support for free access to public resources for non-UK citizens (such as NHS services) and stronger backing for nationalist policies like ending multilingual formatting in official documents (as proposed by UKIP) (see Table A13 in [Supporting Information](#)).

³⁵We correct for multiple hypothesis testing using the mhtexp procedure, which jointly adjusts p-values across all pairwise treatment comparisons within each subgroup. Specifically, for each outcome we treat all pairwise contrasts across the relevant subgroups as a single hypothesis family and obtain multiplicity-adjusted p-values.

³⁶As explained earlier, we do not have other measures of political knowledge.

³⁷To classify participants, we estimated an OLS regression, using as the dependent variable an indicator for whether a participant believed the UK’s future relationship with the EU should be to ‘Leave the European Union’ or to ‘Stay in the EU but reduce its powers.’ (We chose this specific cut-off because it is commonly used in the British Social Survey as a proxy for Brexit approval and is highly correlated with the Brexit vote.) The analysis focused on the control group. Explanatory variables included British nationality, age, and party preference, which are well-established predictors of Brexit voting behaviour (Alabrese et al. [13]). The regression results are in Table A15 in [Supporting Information](#). Using the estimated coefficients, we predicted the probability for each individual and classified those with probabilities above the sample median as Pro-Leave, while those below were classified as Pro-Remain.

³⁸Our classification may not align with their views at the time of the referendum, as some individuals may have since changed their minds. However, as shown in Table A1 in [Supporting Information](#), the shift in Brexit favorability between August 2016 and the time of our survey was small, suggesting that Brexit preferences remained relatively stable over that period.

³⁹Conformism may occur irrespective of whether participants interpret the majority opinion as indicative of an emerging ‘new social norm’. Social norms, in contrast, are established patterns of behaviour or beliefs that individuals often follow because they feel a social obligation to do so, based on the expectation that others also believe in and reinforce these norms [62]. While our experiment points to conformist responses to perceived majority views, it does not assess whether participants are responding to what they may see as a ‘new’ social norm. Indeed, participants may adjust their responses without necessarily interpreting the majority view as a socially endorsed norm that carries an expectation of compliance. This test is beyond the scope of this paper.

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Supporting Information

Additional supporting information can be found online in the Supporting Information section. **Appendix A1:** obes70109-sup-0001-Appendix.zip.