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Free riding in climate protests

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Climate protests are an important driver for ambitious climate policies. However, it is still unknown how individual protest participation decisions depend on each other. Exploiting the unique opportunity of the Third Global Climate Strike, we conducted multi-wave population surveys with 1,510 people in the four largest German cities. With a randomized information intervention, we changed turnout expectations of a subgroup of respondents and measured the impact on the probability to join the local protest event. Our findings provide causal evidence for strategic interdependence in protest participation decisions among members of the general public rather than among a movement's core group of supporters. These decisions are found to be strategic substitutes: individuals who expect many other people to participate are less likely to participate themselves. This dynamic has important implications for the movement's future communication and growth perspective.

Protest movements play a key role in the process of social change[1–4]. Contributing to the quest to identify the key drivers that determine the rise and fall of protest movements, we focus on potential participants' expectations about the number of other people attending and how they affect their participation decisions. If such a link exists, individual protest participation decisions are interdependent, and increasing expected protest size makes potential protesters either (1) more motivated to participate (strategic complementarity) or (2) less motivated to participate (strategic substitutability). Researchers conjectured that strategic complementarity holds both in the general population[5] as well as within social networks[6–8]. By contrast, two recent experimental studies found causal evidence for strategic substitutability in protests of the Umbrella Movement in Hong Kong[9] and among right-leaning protesters in Germany[10]. Another study found evidence of strategic complementarity in left-leaning counter-protesters[10], however, highlighting that this crucial aspect of protests is likely to be context-specific. Using a similar experimental design, we empirically scrutinize this in one of the most prominent global-scale movements of our time: climate protest.

Climate protests emerged around United Nations Climate Summits in the 2000s and have gained substantial momentum since the birth of the youth-driven 'Fridays for Future' (FFF) movement in 2018 (refs. 11,12). A plausible argument holds that the success of mass protests depends on the protest size[13]. Yet the youth are a minority in the developed world[14] and lack agency in formal political and governmental institutions[15,16]. It follows that for real impact, the movement must mobilize people beyond established peer groups and convince the general public to join. However, little is known about the mobilization potential among adults.

We exploited a unique opportunity to study this potential rigorously. The third so-called 'Global Climate Strike' on 20 September 2019, a concerted set of more than 6,000 events in 185 countries mobilizing 7.6 million people[4], was the first large-scale climate protest that explicitly addressed adults. We recruited a large number of adults from the general population in the four largest German cities (Berlin, Hamburg, Munich and Cologne) 2 weeks before the announced local protest events. We then conducted randomized controlled trials as part of three successive online survey waves, two before the events and one after, leading to a final sample of 1,510 people.

We designed the trials to empirically assess whether the participation probability of potential protesters depends causally on their beliefs about the participation probability of other potential protesters and to measure the direction and size of the relationship. There are arguments for expecting it to be positive. First, other people's participation plans reveal information about the likelihood of protest 'success' such that signals of high turnout induce an increase in the expected probability of success, which in turn makes one's own participation more likely[5]. Second, there is evidence that image concerns are pivotal in explaining the presence of strategic complementarity in left-leaning

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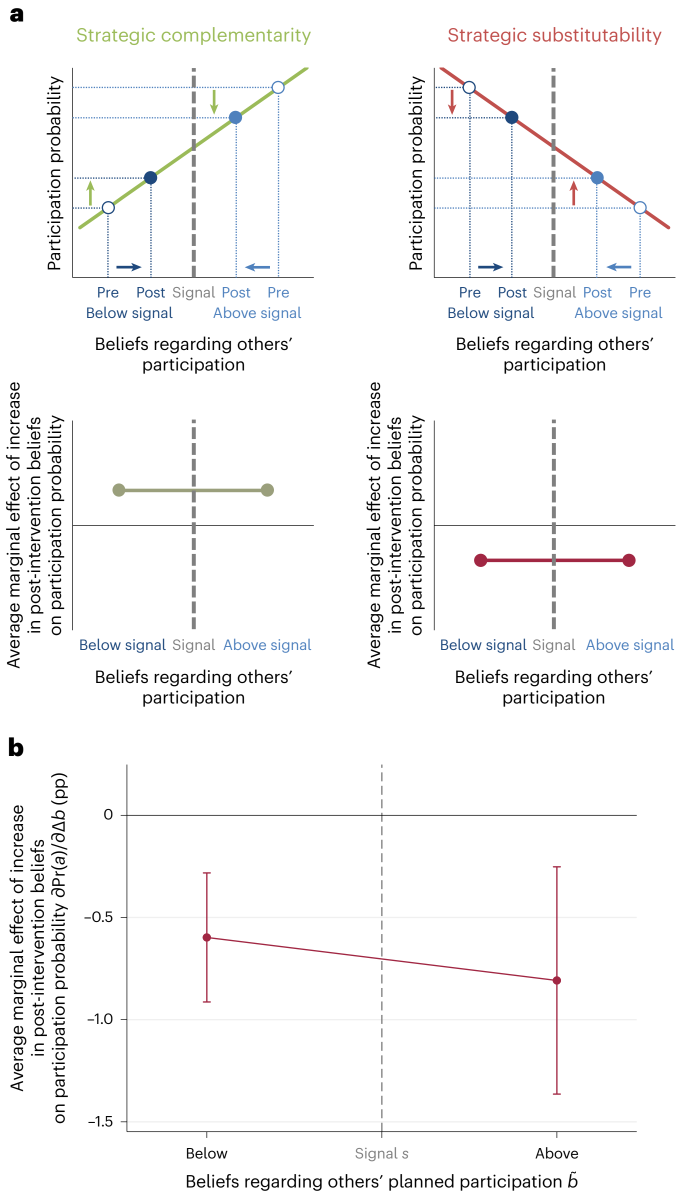

Fig. 1 | Illustration of concept and empirical findings. a, Conceptual illustration: strategic complementarity and substitutability in protest participation. **b**, Empirical finding: average marginal effect of increase in post-intervention beliefs on participation probability. Panel **a** is of an exemplary nature to explain the mechanism underlying the experiment (linearity assumed here for simplicity only). Sub-graphs in the upper row depict the effect of adjusting beliefs regarding others' participation due to the signal on the probability to participate in the protest. 'Pre' and 'Post' stand for beliefs regarding others' participation before the intervention of receiving a signal and after the intervention, respectively. Sub-graphs in the second row depict the first derivative of the respective curves in the first row. Panel **b** shows the estimated average marginal effect of an increase in post-intervention beliefs (or equivalently in the difference between post- and pre-intervention beliefs) on participation probability based on actual data from the experiment ($n = 1{,}510$). Data points indicate the average marginal effect in percentage points with 95% confidence intervals, separated by subgroups with pre-intervention beliefs below (left) and above (right) the signal value, respectively. The graph is derived from the results of the instrumental variable probit regression that is shown in Table 2.

protesters but not in their right-leaning counterparts[10]. Climate protests are associated with the left of the political spectrum, and image concerns are likely to matter in participation decisions. Finally, there are plausible arguments that one-time mass events are more prone to strategic complementarity compared with recurring protests[9]. In our context, the one-time element of a globally concerted event was reinforced by the fact that it was publicly known that on the very day of 20 September 2019, the federal government was to meet for its final round of negotiations before announcing a new set of climate policies, the 'Climate Protection Package 2030', in the late afternoon[17]. Against this background, we pre-registered the hypothesis that climate protest participation decisions are strategic complements.

We elicited respondents' beliefs about the other participants' protest participation in the first and second waves and actual participation versus non-participation for each participant in the last survey wave. The empirical challenge to test the hypothesis with this data is that beliefs are influenced by uncontrollable and non-observable factors, such that observed correlation between beliefs and participation choices will generally not indicate causation. We thus injected exogenous variance into beliefs by providing truthful information about other respondents' protest participation plans, which we gathered in the first wave, to a random subset of participants. As expected, participants exposed to this signal (treatment group) adjusted their beliefs regarding turnout more in the direction of the signal value than those who did not receive the information (control group).

If, on average, an increase in beliefs in response to treatment raises (reduces) the probability to attend the protest, the decision to protest is a strategic complement (substitute) to others' participation decisions. The opposite is true for participants responding to treatment with a downward adjustment of beliefs. We estimate the mean marginal change of participation probability in response to a marginal increase in beliefs[18,19], that is, an increase in the delta between post- and pre-intervention beliefs. Technically, this involves an instrumental variable regression in which treatment status serves as an instrument for beliefs. This allows to separate the treatment-induced (exogenous) variance in beliefs from the idiosyncratic (endogenous) variance. The upper row of Fig. 1a depicts the probability of participation as a function of beliefs about others' participation in case of strategic complementarity (left) and substitutability (right). It highlights that (1) the sign of the curve's slope reveals the type of strategic interdependence (second row of Fig. 1a) and (2) that the sign of the econometrically estimated average marginal effect will be identical to the left and to the right of the signal value, although the direction of the immediate response to treatment in the experiment depends on whether a participant's pre-intervention belief has been above or below the signal value. Hence, a positive (negative) marginal effect indicates strategic complementarity (substitutability). In contrast to our pre-registered hypothesis, we find the sign to be clearly negative; that is, participation decisions are strategic substitutes (Fig. 1b).

Our results contribute to several fields of research. Previous evidence on the strategic nature of protests is mixed[5–7,9,10,20–22]. By drawing on a large population sample, we find strategic interdependence in the general public rather than within the social network of a movement's peer groups or groups of supporters. Thereby, we complement the literature on social ties[5], addressing the question of how protests extend beyond small-scale networks of activists[13]. Our study also provides causal evidence for strategic interdependence in climate protests specifically. This adds methodologically and substantively to the extant literature on climate protest mobilization, which is mostly qualitative or non-causal[23–26]. Specifically, it adds to actionable evidence on the structural, tactical and communicational properties of the climate protest movement[27–32]. The findings hence provide both relevant insights for researchers and practical implications for the (climate) protest movements.

Information intervention changes expectations

A key condition of our econometric approach is that the information provision intervention actually influences beliefs, that is, that treated participants' beliefs changed differently in the period between the first

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Table 1 | Key statistics and experimental group sizes

	Berlin	Hamburg	Munich	Cologne	Total
Signal value s	0.325	0.367	0.367	0.366	0.353
Mean belief regarding others' planned participation $\tilde{b}$	0.327	0.327	0.314	0.334	0.326
Mean pre-intervention belief regarding others' actual participation b'	0.260	0.247	0.238	0.248	0.250
Actual participation rate r	0.092	0.113	0.104	0.138	0.110
Control group n	147	134	103	100	484
Treatment group n	343	265	177	241	1,026
Total n	490	399	280	341	1,510

The top part of the table shows mean values of key survey variables by location as well as for the aggregate sample. The signal value s is based on n=2,574 as it was calculated after the first wave, with the remaining three items being based on the final sample of n=1,510. The bottom part of the table shows the number of participants that completed all three survey waves by experimental condition. Further descriptive statistics are reported in Supplementary Tables 1–6.

Table 2 | Effect of a marginal increase in post-intervention beliefs (or equivalently in the difference between post- and pre-intervention beliefs) on participation probability

	(1)	(2)
	IV probit	2SLS
Coefficients:		
Increase in post-intervention beliefs (Δb)	−3.272***	−0.699***
	(0.803)	(0.212)
Constant	−1.079***	0.117***
	(0.083)	(0.009)
Average marginal effects:		
Increase in post-intervention beliefs (Δb)	−0.679***	−0.699***
	(0.208)	(0.212)
Observations	1,510	1,510
Wald χ^2	16.62	10.89

The first part of the table shows coefficient estimates from instrumental variable regression of participation on the increase in post-intervention beliefs or equivalently in the difference between post- and pre-intervention beliefs $\Delta b=b-b'$ and the instrument of the treatment (z=0 for control, z=1 for treatment), a condition indicator (c=0 for respondents with expectations $\tilde{b}$ below the signal s, c=1 for above or equal to the signal) and the interaction of the two. Standard errors are in parentheses. The second part of the table shows estimated average marginal effects from the instrumental variable regressions described. Delta-method standard errors are in parentheses. Column (1) shows results from IV probit estimation, column (2) from 2SLS regression. For both regressions, the final sample of respondents who completed all three waves (n=1,510) was used. ***Significant at 1%.

and the second survey wave than did the beliefs of participants in the control group. This condition is testable. To do so, we describe the experimental design in a bit more detail first and provide full detail in Extended Data Fig. 1 and Methods.

In the first wave, 2 weeks before protest day, we asked all respondents whether they were planning to participate in the respective local event (own plans $\tilde{r}$, four-point scale). We then calculated the share of respondents that indicated to plan or rather plan to participate (signal s, value between zero and one, first row of Table 1). In the first wave, we also asked respondents about the fraction of all participants they expect planning to participate (others' plans $\tilde{b}$, coded as fraction between zero and one) and the fraction of subjects they expect actually participating (pre-intervention beliefs b', coded as fraction between zero and one). The two beliefs are clearly correlated (Pearson correlation coefficient $\rho = 0.690$).

In the second wave, just before the event, we randomly assigned respondents to experimental groups, treatment ($z = 1$) and control ($z = 0$). Participants in the treatment group got signal s; participants in the control group did not. An individual whose beliefs regarding others' plans $\tilde{b}$ were below the signal s was expected to update beliefs regarding others' actual participation b' upwards and vice versa. To check whether this was indeed the case, we asked subjects again for their expectations regarding others' actual participation (post-intervention beliefs b). Respondents in the control group were asked only for their beliefs again.

We normalize the distribution of beliefs by taking the difference between individual beliefs and the location-specific signal, both pre- and post-intervention ($b' - s$ and $b - s$). This allows us to plot the data from all four cities into a single figure (Fig. 2), with zero representing beliefs that match the city-specific signal value exactly. All distributions are right-skewed; that is, most respondents underestimated the share of other respondents that stated to plan attending the local protest event. In the control group, the distribution of post-intervention beliefs b is not significantly different from the distribution of pre-intervention beliefs b' ($P = 0.954$, exact Wilcoxon signed-rank test). For the treatment group, a shift of the distribution of post-intervention beliefs b relative to the distribution of pre-intervention beliefs b' is evident from the plot. Equality of distributions is clearly rejected ($P = 0.000$, exact Wilcoxon signed-rank test). The same is true for the contrast between treatment and control groups ($P = 0.002$, Mann–Whitney rank-sum test).

Direction and size of the treatment effects on beliefs are confirmed by an ordinary least squares regression of the difference between post- and pre-intervention beliefs ($\Delta b = b - b'$) on treatment assignment z, a dummy c for belief regarding others' plans $\tilde{b}$ below ($c = 0$) or above ($c = 1$), the signal value s and the interaction of the two (see Methods for the detailed statistical analysis). In the 'below group' ($c = 0$), the average treatment effect on beliefs is +4.25 percentage points (pp); that is, participants' expectations about others' participation increase relative to the control group. In the 'above group' ($c = 1$), we find a mean − 5.54 pp decrease under treatment relative to control. Both estimates are statistically highly significant ($P = 0.000$). Results remain within close bounds if respondents with $\tilde{b}$ close to the signal value s are excluded and if the sample is re-weighted to match the sample of the first wave in terms of gender and age groups. If we exclude respondents from the sample that failed on an attention-check question, the change in beliefs is slightly stronger (~1 pp) in the below group. Detailed regression results, including the sensitivity and robustness checks, are shown in Supplementary Table 7.

Wrapping up, the information intervention was successful in shifting participants' beliefs regarding others' participation, such that treatment status z is a viable instrumental variable to estimate the causal belief effect on the probability of protest participation, as it is exogenous (by randomization) and significantly correlated with beliefs.

Participation decisions are strategic substitutes

In a final survey wave done after protest day, we asked participants whether they participated in the event, coded $a = 1$ for 'yes' and $a = 0$ for 'no'. The fraction of protesters among the experimental participants ranged from 9.2% in Berlin to 13.8% in Cologne, averaging 11.0% across locations (10.7% in the control group, 11.1% in the treatment group; Table 1).

By means of maximum likelihood probit regression (instrumental variable (IV) probit) and two-stage least squares regression (2SLS) with treatment assignment z instrumenting for Δb, we estimate the average marginal effect of the treatment-induced change in beliefs on the

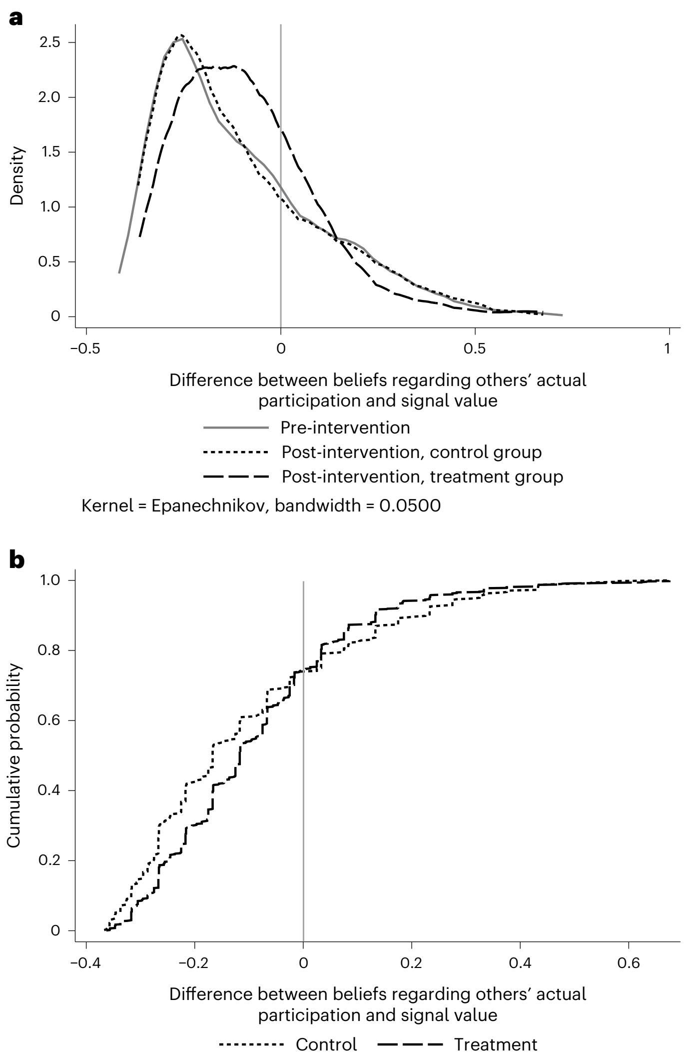

Fig. 2 | Distribution of beliefs. **a**, Probability density functions of pre- and post-intervention differences ($b' - s$ and $b - s$) by experimental group. **b**, Cumulative distribution functions of post-intervention differences $b - s$ by experimental group. Panel **a** shows the kernel density plots (bandwidth equal to 0.05) of the probability density functions of the difference between beliefs regarding others' actual participation and the location-specific signal value s both before the intervention (pre-intervention beliefs b'), $b' - s$, and after the intervention (post-intervention beliefs b), $b - s$, by experimental group. At the vertical solid line, beliefs are equal to the signal value ($b' = s$ or $b = s$). Panel **b** shows plots of the cumulative distribution functions of post-intervention differences $b - s$ by experimental group.

probability of participation. Assuming that the treatment affected only beliefs about the likelihood of participation, the estimate identifies the causal impact of these beliefs on participation decisions.

The estimated average marginal effect is between −0.679 (IV probit) and −0.699 (2SLS) (Table 2); that is, a 1 pp increase in beliefs reduces the probability of participation by about 0.7 pp (or equivalently, a 1 pp decrease in beliefs increases the participation probability by that magnitude). Note that this negative relationship is directionally independent from the location of pre-intervention beliefs relative to the signal value, that is, whether beliefs are updated upwards or downwards (Fig. 1). Thus, our population of respondents displays strategic substitutability in protest mobilization. The hypothesis that Δb is exogenous, that is, that it is sufficient to regress participation on beliefs without the information intervention, is rejected by Wald tests at all conventional levels of significance. Thus, the experimental intervention is in fact crucial for recovering the causal effect of an increase in beliefs on participation. Several further checks of sensitivity and robustness, including the same sample-exclusion tests as in the previous section, are provided in Supplementary Section B. Average marginal effect estimates scatter very little in the range of −0.625 to −0.727, and there is no evidence that key identifying assumptions are violated or that the instrument is weak.

We present effect heterogeneity analysis in Supplementary Section C. We highlight two aspects. First, specific average marginal effects are −0.598 (standard error (s.e.) = 0.161, $P = 0.000$) in the below group and −0.808 (s.e. = 0.284, $P = 0.004$) in the above group, the difference being marginally significant ($P = 0.094$; Fig. 1b). Thus, the belief effect on the probability of protest participation tends to be a bit stronger for downward belief updates compared with upward adjustments.

Second, a random subset of the treated survey participants ($n = 496$ in the final, post-third-wave sample) was exposed to another treatment where, in addition to the main treatment, we asked for beliefs and provided truthful signals about planned participation by the 18–29 yr and 30–69 yr age groups. We used this to test whether participants respond stronger to beliefs about a specific age group or whether strategic interdependence is stronger within age group. Our main finding of strategic substitutability is confirmed for the beliefs about the 30–69 yr age group (average marginal effect −0.493, s.e. = 0.238, $P = 0.038$) while beliefs about the 18–29 yr age group have no significant impact on own participation (average marginal effect −0.235, s.e. = 0.217, $P = 0.278$). This is independent of whether survey participants themselves belong to the older or the younger group ($P > 0.4$). Estimation results are provided in Supplementary Table 10.

Discussion

We find robust evidence of strategic substitutability in climate protest mobilization, which provides empirical backing for assumptions in recent modelling of green transitions[33] and is in line with an influential study in the context of Hong Kong's Umbrella Movement[9].

The results also inform protest and social movement research more generally. One hypothesis to explain strategic substitutability in mobilization is that costs for the individual increase with protest size (for example, by increased risk of violent escalation)[9]. In our context, however, the costs to protest for the individual may increase in protest size for logistic or psychological reasons (for example, fear of large crowds) at the worst, as FFF rallies are usually peaceful, there is little counter-protest and conflicts with the authorities are rare. This is different for protests by more radical movements, such as Last Generation or Extinction Rebellion.

Another explanation for substitutability is that the larger the protest, the less an individual can express their self-image of being particularly progressive or active because it blends in the masses. It appears plausible in climate protesters, especially in our context in which the youth-driven movement aimed to mobilize people beyond their 'traditional' age group. With their participation, adults could signal that they care for the youth and their future as well as position themselves as progressive and open-minded. The information provided as part of the experiment referred to the expected participation by other adults. The more other adults turn out, the less special the participation of one individual adult and hence the smaller the value of the signal sent by attending. This effect would be particularly relevant if participation in climate protests attracted non-conformist adults that like to set themselves apart. Given that the third Global Climate Strike was the first adults have been explicitly invited to, attending might still carry such a non-conformist flavour. Indeed, we find that protesters differ markedly from non-protesters in our sample in terms of age (below age 50 cohorts over-represented), education (academic background over-represented) and income (high incomes over-represented), and they lean severely towards stronger environmental preferences and attitudes (Supplementary Table 6).

A third hypothesis to explain strategic substitutability is that a protest's likelihood of 'success' increases sharply around a certain turnout threshold (critical mass)[5,9]. Our finding is consistent with the idea of there being a target number of adults in what had been a youth-driven movement. Such a target could be motivated both by enough adults showing support for the youth and by not diluting the narrative of the youth fighting for their future. This is in line with the finding that substitutability is driven by expected participation of the age group 30–69 yr.

A final model-backed hypothesis from the literature is when a protest movement's goal is 'modest' (a specific policy goal but not systemic revolution), people rather free ride, that is, exhibit strategic substitutability, whereas when the aim is to overthrow the entire status quo, coordination becomes more important and people act in strategic complements[34]. There might be some controversy within the climate protest movement about the boldness of its aims, but climate protest is defined by a specific policy goal in the first place, and modest goals are probably dominant in our population sample of adults. Moreover, respondents in our population sample are strangers to each other, and no social ties exist. Social connection probably plays an important role for strategic complementarity. Hence, the lack of connection to the other respondents could explain why we do not find complementarity, as social norms (for example, participation in such protests) are more likely to form within a specific milieu rather than for the general population.

An important lesson for the climate protest movement is that its current structure, a large number of local groups organizing local events at coordinated dates, substantially reduces the hazards of strategic substitutability in participation decisions. Compared with a small number or even a single national event at a central location, the decentralized approach reduces logistical hurdles and allows strategic substitutability to work in favour of the protest. Smaller local turnouts increase the willingness to participate. Moreover, social ties and networks that counter strategic substitutability are easier to establish and create more traction at the local level. In terms of communication strategies, it seems advisable to communicate conservative estimates to the media in the run-up to another Global Climate Strike.

Online content

Any methods, additional references, Nature Portfolio reporting summaries, source data, extended data, supplementary information, acknowledgements, peer review information; details of author contributions and competing interests; and statements of data and code availability are available at https://doi.org/10.1038/s41558-023-01833-y.

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Methods

We developed and implemented a sequence of three online survey waves containing a randomized controlled trial in the four most populous German cities (Berlin, Hamburg, Munich and Cologne) to detect the causal effect of a marginal change in beliefs on the probability of participation in the climate protest described in the main text. The intervention design is adapted from ref. 9. The study was pre-registered in the Randomized Controlled Trial Registry of the American Economic Association under code AEARCTR-0004583.

Experimental design

Extended Data Fig. 1 depicts the experimental design. The longitudinal study consisted of three sequential survey waves, with two of them taking place before the global strike on 20 September 2019 and the last one being issued after the event. Respondents could participate in the second (third) wave only if they had completed the first (second) one. The key variables sampled in the three waves are described in more detail in the following. We additionally collected sociodemographic variables, opinions and preferences. We devoted great care in asking the questions in a sequence that does not pollute our key instruments. The questionnaires in German (original) and English (transcript) are available online at the Open Science Framework (OSF)[35].

Wave 1. In the first wave, respondents' intentions to participate in the protest were elicited. From 6 September (Friday) to 11 September (Wednesday) 2019, respondents could participate in the first wave. Respondents were briefly introduced to the subject matter in objective and neutral language and informed about the public call applicable to their city of residence, that is, when and where the rally would take place as announced by local FFF organizers weeks in advance via various media such as flyers, stickers, social media, conventional media and so on (see questionnaire for exact time and place for each location). It was also explained that people in the same city between 18 and 69 yr of age were surveyed and that this group approximates the structure of the local population.

In the first key question, we asked respondents about their intention to participate in the rally in their respective city on a scale with four answer options (variable $\tilde{r}$, Q9 in the questionnaire). The fraction of participants that selected response 1 (planning to participate) or 2 (rather planning to participate) was calculated for each city after the first wave was completed. This is the signal value s used in the intervention in the second wave.

The second instrument elicited respondents' location-specific beliefs about other participants' planned participation (variable $\tilde{b}$, Q10 in the questionnaire), that is, the share of other participants in their respective city that selected responses 1 or 2 in the question about intended participation, either planned or rather planned to participate in the rally. Respondents could type in a number between 0 and 100 at a resolution of a single decimal digit.

The third key question in this first wave was on respondents' beliefs regarding others' actual participation (variable b', Q13 in the questionnaire), that is, the fraction of other participants in their respective city that will actually attend the local event. Respondents could again type in a number between 0 and 100 at a resolution of a single decimal digit. This variable is the pre-intervention belief that helps in controlling for endogeneity in the post-intervention belief measured in the second wave (see the following).

Before the two belief-eliciting questions, we informed participants that they are part of a sample that approximately matches the actual population structure of their respective city of residence. While not strictly representative in a probabilistic-sampling sense (we used quota sampling, explained later), the sample comes very close to official population statistics (Supplementary Table 4).

We repeated the belief elicitation for the share of other participants' planned participation for age groups 18–29 yr and 30–69 yr separately (Q11 and Q12 in the questionnaire).

Wave 2. In the second wave, the experimental intervention was implemented to induce exogenous variation in beliefs. The wave was open to participants between 16 September (Monday) and 20 September (Friday) 2019, at noon, local time. At the beginning of the second wave, participants were randomly assigned to either the control condition or one of two treatment conditions, which are pooled in the main analysis into a single treatment. Independent from the experimental group, all respondents were reminded of their own belief regarding others' planned participation $\tilde{b}$ that they stated in question Q10 in the first wave. Participants in treatment groups 1 and 2 were informed about their location-specific signal value on others' planned participation s, calculated on the basis of answers to question Q9 in the first wave. Participants in treatment group 2 were additionally informed about their location-specific signal value on planned participation of two age subgroups, also calculated on the basis of answers to question Q9.

The signal values s provided per city are shown in the first row of Table 1. Participants in the control group did not receive this information. Otherwise the experimental conditions were identical. The treatment in combination with the reference belief on planned participation induces an informational stimulus that gives reason to adjust the post-intervention belief on others' actual participation relative to the control. All respondents were then asked for their post-intervention belief regarding others' actual participation (variable b, Q19 in the questionnaire). This question was essentially identical to question Q13 in the first wave. The only difference was an introductory sentence that reminded participants of their own previous responses. Respondents could again type in a number between 0 and 100 with a resolution of a single decimal digit.

Wave 3. In the third wave, respondents' actual participation in the event a was elicited. The third wave was fielded between 5 December (Thursday) and 16 December (Monday) 2019. We note that the third wave had been originally planned for the period between 21 September and 1 October but had to be postponed due to technical problems of the data collection contractor Kantar. Since the key instrument in this wave was a simple fact question about participation in the local protest event, it is unlikely that the delay caused any kind of problem.

Participation was measured with the following five-point nominal-scale instrument (Q25 in the questionnaire): (1) Yes, I was there as a participant; (2) Yes, I was there as an observer; (3) Yes, I was there as a counter-demonstrator; (4) No, I was not there, but at a different 'Fridays for Future' event that day; (5) No, I did not participate in any 'Fridays for Future' event that day. We used the five-point distinction instead of asking a binary question to avoid misunderstandings of what 'participating' means. We wanted to be able to distinguish between actual participants (response 1) and people that happened to be there for other reasons (responses 2 and 3). Since there were events at many locations on the same day, we also wanted to distinguish participants in the local event (response 1) and other events (response 4). We used the opportunity that the wave was postponed to also elicit participation decisions for the so-called Fourth Global Climate Strike (28 November 2019) for exploratory purposes.

Sample

Data were collected in collaboration with the Munich office of The Kantar Group Ltd under commercial contract. The company maintains local opt-in online panels of volunteers (typically used for market research) in each of the four subject cities. Quota samples of these panels constituted the gross sample for the present study. All participants were informed about and consented to their answers being used for scientific purposes only.

Invitations to participate in a scientific study on 'environmental and climate protection matters' involving three sequential survey waves were sent to registered panelists aged 18 to 69 by e-mail on 6 September 2019. They were informed that they would be compensated financially for each completed wave according to Kantar's default lump-sum rates, plus a bonus of €2 for completing all three waves. The data collection contractor Kantar informed us that they have established a 'code of honour' for truthfulness with their respondents, and response-conditioned incentives would undermine this code and make non-truthfulness salient in the first place. This is supported by evidence[36,37]. There is also evidence that scoring rules for belief elicitation can adversely affect accuracy and induce hedging, although the problem appears to hinge on specific details of implementation[38–41].

The first wave was completed by 2,574 participants, the second by 1,879. A total of 1,510 respondents completed the third wave. This was our final sample that entered the data analysis. A breakdown of the sampling process by location is shown in Supplementary Table 1. The attrition rate from the first to the second wave is 27%, and the one from the second to the third wave equals 19.6%. The first round attrition is comparable to the 25% in ref. 9, whose experimental design we adapted. The second attrition rate is higher than the 5% rate in ref. 9, which we attribute to the postponement of our third wave to December as explained. While attrition is expected in a panel survey, we check whether it is selective. Most important, attrition was not different across treatments. Random treatment assignment took place at the beginning of wave 2. Hence, attrition from wave 1 to wave 2 cannot be influenced by treatment assignment by construction. Attrition from wave 2 to wave 3 was 19.9% in the control and 19.5% in the treatment group. A probit regression confirms that treatment assignment does not predict attrition ($P = 0.86$). Supplementary Table 2 shows results from tests for selective attrition from the first to the third wave based on demographics, preferences and beliefs elicited in the first wave. We find selective attrition for the lowest and the two highest age groups; that is, the sample is getting older, on average, in the treatment group. For the other descriptive variables, we do not find significant differences between the samples in the first and in the third waves. To account for the selective attrition, we estimated the main regression models with a re-weighted sample as a robustness check (see main text and Statistical analysis). Supplementary Table 3 checks for selective attrition in each experimental group.

Supplementary Table 4 compares demographics gender and age in the city subsamples with the actual city population. We emphasize that we did not draw probabilistic samples from the populations, but we aimed for approximate matches with the respective quotas.

Supplementary Table 5 provides sample characteristics by experimental group and balance checks. We find that control and treatment groups do not differ in the listed descriptive statistics at the 5% significance level.

For informational purposes, Supplementary Table 6 provides an overview on characteristics of the group of participants, that is, subjects who indicated that they participated as a participant in the protest ($n = 166$), and compares them with those who did not participate in the protest ($n = 1,344$).

Statistical analysis

We base our analysis on a micro-founded parametric model of political protest mobilization, which explains the mean participation probability in a population of potential protesters as a function of preferences and beliefs. In the analysis, we want to examine whether the participation decision depends on beliefs, and if yes, in which direction. The basic statistical approach is to use the treatment assignment as an instrumental variable for the potentially endogenous belief variable[18,19,42]. By randomization, treatment status is credibly exogenous. First, we test whether the randomly assigned information intervention had an effect on beliefs and compute the average treatment effect. Second, we run an IV probit regression to test whether the treatment-induced change in beliefs impacts participation decisions and compute the average marginal effect of a marginal change of beliefs on the probability of participation.

We estimate ordinary least squares linear regression models to determine the (first-stage) effect of the treatment on beliefs. The dependent variable is the difference between post-intervention beliefs and pre-intervention beliefs ($\Delta b = b - b'$). The independent variables are the binary treatment indicator z (0 for control, 1 for treatment group), the binary condition indicator c (0 for respondents with beliefs regarding others' plans $\tilde{b}$ below the signal s, 1 for above or equal to the signal) and an interaction between the two. Results are shown in Supplementary Table 7. Column 1 lists the main results. In columns 2–4, sensitivity and robustness checks are shown: in column 2, respondents who answered the attention question Q17.f incorrectly are excluded ($n = 1,326$); in column 3, respondents with beliefs regarding others' planned participation $\tilde{b}$ close to the signal value s (within 2 pp, that is, $-0.02 \le \tilde{b} - s \le 0.02$) are excluded ($n = 1,465$); and in column 4, the final sample is re-weighted to match the quotas of the first-wave sample in terms of gender and age groups ($n = 1,510$).

For the average marginal effect, we estimate an IV probit regression given that the dependent variable of participation a is binary (0 for non-participation, 1 for participation), but 2SLS estimates are also given for reference. The endogenous regressor is $\Delta b = b - b'$, the difference between post-intervention beliefs and pre-intervention beliefs. It is instrumented using the treatment indicator z, the condition indicator c and the interaction between the two. From the IV probit regression, we obtain the coefficient for a marginal change (increase or decrease; the two are symmetric) in Δb. It indicates whether there is strategic interdependence (coefficient different from zero) and its direction (coefficient greater than zero in case of strategic complementarity and smaller than zero in case of strategic substitutability). From the estimates, we can also compute the average marginal effect in percentage-point terms, that is, the effect of a 1 pp increase in post-intervention beliefs or equivalent increase in the difference between post- and pre-intervention beliefs (Δb) on the participation probability. Results are shown in Table 2. Under certain assumptions, which are discussed in Supplementary Section B, this effect identifies the so-called local average treatment effect, or 'LATE'[18,19], the effect of treatment on the class of participants that respond to treatment by updating their beliefs. Sensitivity and robustness checks via regressions on the restricted and re-weighted samples described in the previous paragraph are also provided in Supplementary Section B (Supplementary Table 8).

Ethical statement

The study was ethically approved by the Dean's Office of the Faculty of Business, Economics and Social Sciences at Universität Hamburg on the basis of the code of ethics and corresponding terms set up by WISO Research Lab at Universität Hamburg.

Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Data availability

Data and survey questionnaire are freely available online at the Open Science Framework (OSF) via https://doi.org/10.17605/OSF.IO/Z2EWS (https://osf.io/z2ews).

Code availability

The statistical analysis codes for replicating the results presented in the figures, tables and Supplementary Information are freely available online at the Open Science Framework (OSF) under https://doi.org/10.17605/OSF.IO/Z2EWS (https://osf.io/z2ews).

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Author contributions

All authors contributed equally to the research. H.S. developed the idea. J.J.-N., G.P. and H.S. designed the study. J.J.-N. implemented the survey and managed data collection. H.S. and J.J.-N. conducted the statistical analyses. J.J.-N., G.P. and H.S. wrote the manuscript and contributed to the discussions and paper revision.

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Competing interests

The authors declare no competing interests.

Additional information

Extended data is available for this paper at https://doi.org/10.1038/s41558-023-01833-y.

Supplementary information The online version contains supplementary material available at https://doi.org/10.1038/s41558-023-01833-y.

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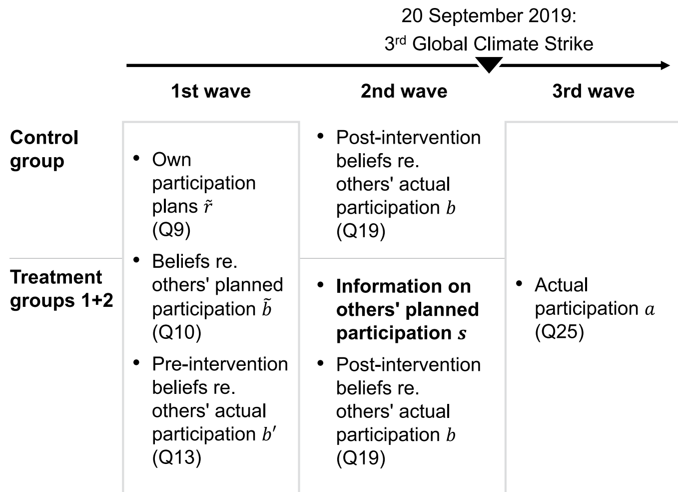

Extended Data Fig. 1 | Experimental design. The figure depicts a schematic overview of the three survey waves conducted and the key instruments of each wave. In parentheses, the corresponding questions in the questionnaire are indicated.

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Population characteristics	*See Section "Behavioural & social sciences study design - Research sample"*
Recruitment	*Respondents were recruited by the Munich office of the professional panel provider and data collection contractor The Kantar Group Ltd. (London, UK). The panel is an actively managed opt-in online panel of volunteers. Self-selection bias exists to the extent that participants could voluntarily decide to participate in the surveys or not when receiving the link by Kantar.*
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Sampling strategy	*As sampling strategy, non-probability quota-based sampling was applied to the first wave. In the following two waves, only respondents who completed the previous wave, were invited to participate. The final sample consists of 1,510 respondents. Sample size was determined based on the maximum number of respondents Kantar could recruit in each of the four cities.*
Data collection	*The data was collected online by Kantar via the computer, laptop, or other mobile devices with access to the Internet. We had no direct contact to respondents and could only link their responses from the three different survey waves via a randomly-generated ID. It is impossible for us to connect the data to the individuals. Respondents were randomly assigned to experimental intervention.*
Timing	*Data was collected between 6 September 2019 and 16 December 2019. The first wave of the survey ran from 6 to 11 September 2019, the second wave from 16 to 20 September 2019 (until 12 noon), the third wave from 5 to 16 December 2019.*
Data exclusions	*No respondents, who completed the third wave, were excluded from the final sample.*
Non-participation	*2,574 respondents completed the first wave. Of those, 1,879 respondents completed the second wave. Of those, 1,510 completed the final third wave. Reasons for the drop-outs are not available.*
Randomization	*Participants were randomly allocated to the experimental intervention .*

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☐ The axis scales are clearly visible. Include numbers along axes only for bottom left plot of group (a 'group' is an analysis of identical markers).

☐ All plots are contour plots with outliers or pseudocolor plots.

☐ A numerical value for number of cells or percentage (with statistics) is provided.

Methodology

Sample preparation	*Describe the sample preparation, detailing the biological source of the cells and any tissue processing steps used.*
Instrument	*Identify the instrument used for data collection, specifying make and model number.*

Software	*Describe the software used to collect and analyze the flow cytometry data. For custom code that has been deposited into a community repository, provide accession details.*
Cell population abundance	*Describe the abundance of the relevant cell populations within post-sort fractions, providing details on the purity of the samples and how it was determined.*
Gating strategy	*Describe the gating strategy used for all relevant experiments, specifying the preliminary FSC/SSC gates of the starting cell population, indicating where boundaries between "positive" and "negative" staining cell populations are defined.*

☐ Tick this box to confirm that a figure exemplifying the gating strategy is provided in the Supplementary Information.

Magnetic resonance imaging

Experimental design

Design type	*Indicate task or resting state; event-related or block design.*
Design specifications	*Specify the number of blocks, trials or experimental units per session and/or subject, and specify the length of each trial or block (if trials are blocked) and interval between trials.*
Behavioral performance measures	*State number and/or type of variables recorded (e.g. correct button press, response time) and what statistics were used to establish that the subjects were performing the task as expected (e.g. mean, range, and/or standard deviation across subjects).*

Acquisition

Imaging type(s)	*Specify: functional, structural, diffusion, perfusion.*
Field strength	*Specify in Tesla*
Sequence & imaging parameters	*Specify the pulse sequence type (gradient echo, spin echo, etc.), imaging type (EPI, spiral, etc.), field of view, matrix size, slice thickness, orientation and TE/TR/flip angle.*
Area of acquisition	*State whether a whole brain scan was used OR define the area of acquisition, describing how the region was determined.*

Diffusion MRI ☐ Used ☐ Not used

Preprocessing

Preprocessing software	*Provide detail on software version and revision number and on specific parameters (model/functions, brain extraction, segmentation, smoothing kernel size, etc.).*
Normalization	*If data were normalized/standardized, describe the approach(es): specify linear or non-linear and define image types used for transformation OR indicate that data were not normalized and explain rationale for lack of normalization.*
Normalization template	*Describe the template used for normalization/transformation, specifying subject space or group standardized space (e.g. original Talairach, MNI305, ICBM152) OR indicate that the data were not normalized.*
Noise and artifact removal	*Describe your procedure(s) for artifact and structured noise removal, specifying motion parameters, tissue signals and physiological signals (heart rate, respiration).*
Volume censoring	*Define your software and/or method and criteria for volume censoring, and state the extent of such censoring.*

Statistical modeling & inference

Model type and settings	*Specify type (mass univariate, multivariate, RSA, predictive, etc.) and describe essential details of the model at the first and second levels (e.g. fixed, random or mixed effects; drift or auto-correlation).*
Effect(s) tested	*Define precise effect in terms of the task or stimulus conditions instead of psychological concepts and indicate whether ANOVA or factorial designs were used.*

Specify type of analysis: ☐ Whole brain ☐ ROI-based ☐ Both

Statistic type for inference (See Eklund et al. 2016)	*Specify voxel-wise or cluster-wise and report all relevant parameters for cluster-wise methods.*
Correction	*Describe the type of correction and how it is obtained for multiple comparisons (e.g. FWE, FDR, permutation or Monte Carlo).*

Models & analysis

n/a	Involved in the study
☐	☐ Functional and/or effective connectivity
☐	☐ Graph analysis
☐	☐ Multivariate modeling or predictive analysis

Functional and/or effective connectivity	*Report the measures of dependence used and the model details (e.g. Pearson correlation, partial correlation, mutual information).*
Graph analysis	*Report the dependent variable and connectivity measure, specifying weighted graph or binarized graph, subject- or group-level, and the global and/or node summaries used (e.g. clustering coefficient, efficiency, etc.).*
Multivariate modeling and predictive analysis	*Specify independent variables, features extraction and dimension reduction, model, training and evaluation metrics.*