Renewable Energy and Territory: Perceptions of Wind and Solar Deployment in Spain

Pre-Analysis Plan

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1 Experimental Design

We will conduct an online information-provision experiment on a sample of approximately 2,000 Spanish adults recruited in partnership with the market research company Netquest. The provider complies with the General Data Protection Regulation (GDPR). The survey will be designed to be representative of the Spanish nationals residing in Spain aged 18–64 after applying sampling weights. Quotas will be set by gender, age, and geographical region (*Comunidades Autónomas*). In addition, the sample will oversample rural respondents, defined as individuals living in municipalities with fewer than 5,000 inhabitants, in order to ensure sufficient statistical power for rural-urban comparisons. Survey weights will then be used to restore representativeness with respect to the Spanish population aged 18–64. The survey will be conducted in Spanish, although this pre-analysis plan is presented in English for consistency. A full Spanish version of the questionnaire will be included in the online appendix of the paper.

All panel members will receive an invitation to participate via email and may freely choose whether or not to take part. The aim of this experiment is to understand whether providing information about the unequal territorial distribution of renewable energy facilities and about the availability of compensation schemes for host municipalities can affect support for energy-transition policies.

1.1 Main survey

Before beginning the survey, participants will be informed that the study is conducted by researchers at the Universitat de Barcelona. They will be told that participation is voluntary, that responses are anonymous, and that they may withdraw at any time without consequence. They will also be provided with an email address through which they may contact the research team to ask questions, request clarification, or exercise their data protection rights. The survey will be administered in Spanish. The survey flow is illustrated in Figure 1 and described below.

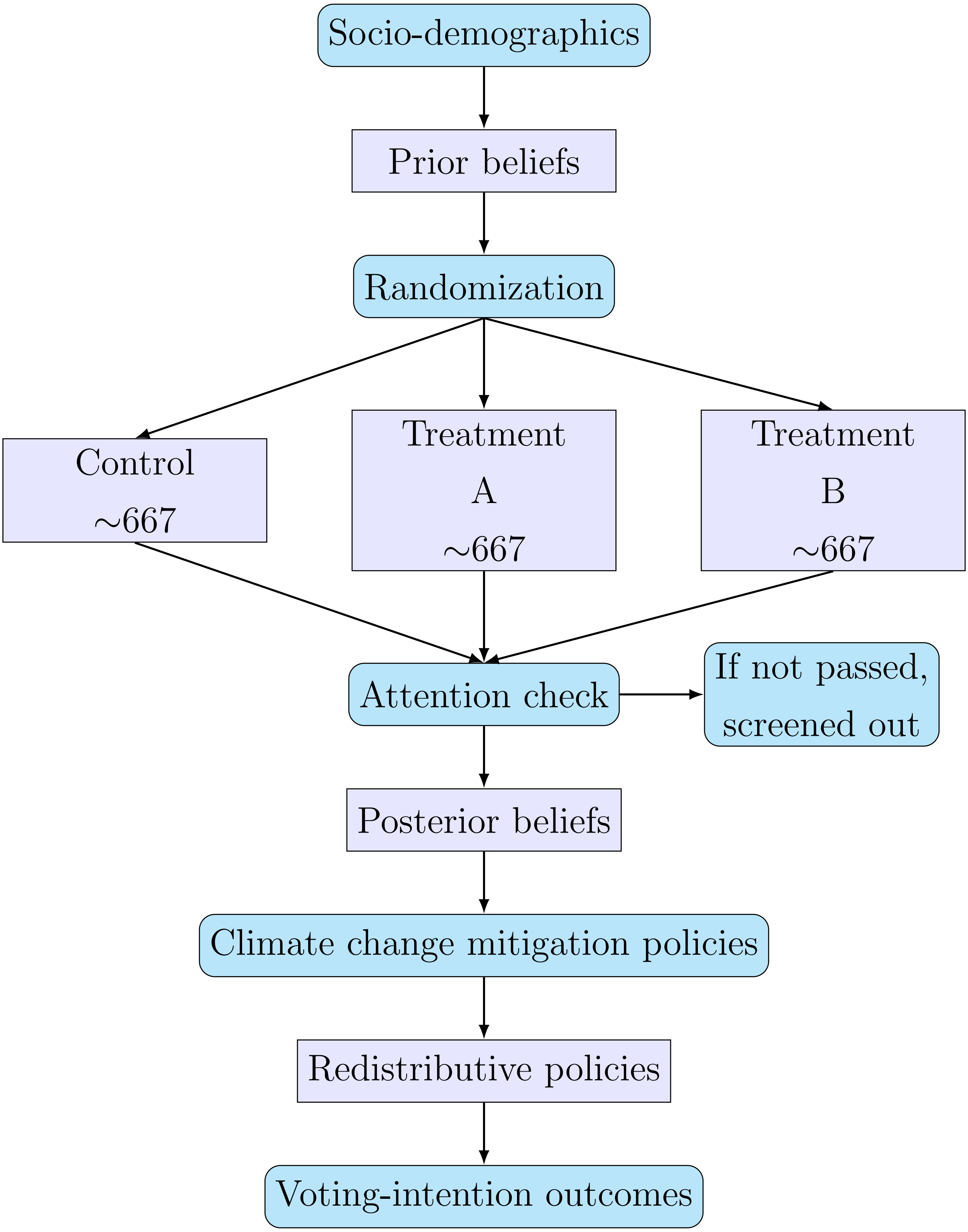

Figure 1: Survey flow

Socio-demographics

After providing informed consent, respondents will answer a battery of socio-demographic questions. We will ask participants their gender, age, highest level of education, current personal situation, whether they have dependent children, current employment status, and total monthly net household income. This information will allow us to describe the sample, check balance across treatment arms, and conduct pre-specified heterogeneity analyses. Addittionally, the survey company will provide us with the municipality of residence and the postal code of each respondent.

We will also ask participants about their political orientation on a standard left-right scale and their vote intention in a hypothetical general election. These variables are central to the research design because preferences over renewable energy deployment and territorial compensation may vary systematically by ideology and partisan alignment. In addition, the municipality of residence will allow us to link individual responses to aggregate contextual information, such as local population size, rurality, previous exposure to renewable energy projects, and electoral characteristics. No exact address or directly identifying geographic information will be collected.

Linked administrative and contextual data

In addition to the variables collected directly through the survey, we will link respondents to external administrative and contextual data using their municipality of residence and, where necessary, their postal code. These linked data will be used to construct pre-treatment contextual variables for balance checks, heterogeneity analyses, and robustness specifications. No exact address will be collected, and all linked variables will be aggregated at the municipality or postal-code level. The linkage will therefore not allow the research team to identify respondents personally.

First, we will construct measures of prior local exposure to renewable energy facilities. Using administrative data from the Spanish Ministry for the Ecological Transition and the Demographic Challenge on the location of renewable-energy installations, we will identify whether the respondent lives in, or close to, a municipality hosting wind farms or solar photovoltaic plants. We will construct separate indicators for exposure to wind farms, exposure to solar plants and any renewable-energy exposure according to whether there is an installation built within the same postal code of residence or the adjacents one.

Second, we will link the survey data to municipal socio-demographic characteristics from the Spanish National Statistical Institute (INE). These variables will include population size, population density, rural status, age structure, income-related indicators when available, and other municipal characteristics relevant to renewable-energy deployment and territorial inequality. These variables will be used to describe the sample, classify respondents by municipality type, construct survey weights when appropriate, and assess selection into the survey relative to the general Spanish population.

Third, we will link respondents to electoral and local-political data using official election results from the Spanish Ministry of the Interior and other official sources when applicable. These data will allow us to measure the political context of the respondent's municipality, including past vote shares, turnout, partisan composition, and characteristics of local political authorities. These variables will be used as contextual controls and as pre-specified moderators of treatment effects.

All administrative and contextual variables will be measured prior to treatment assignment. They will therefore be treated as pre-treatment covariates. When we use these variables in the analysis, we will clearly distinguish them from variables elicited directly in the survey.

Prior beliefs

The next block elicits respondents' prior beliefs and attitudes about renewable energy deployment before any experimental manipulation. We first ask them to evaluate the installation of solar and wind plants in Spain in general and in their own municipality or in a nearby municipality. This distinction is important because the literature has repeatedly shown that attitudes towards renewable energy in the abstract may differ from attitudes towards specific local projects.

We then ask respondents to identify their main concerns about the installation of a wind farm and a solar photovoltaic plant in their municipality or in a nearby municipality. The response options distinguish between landscape and local environmental impacts, noise and nuisance, loss of agricultural or livestock land, lack of local economic benefits, and the possibility that the jobs created may not benefit local residents. Finally, we ask participants to identify the main benefit they would expect from each technology, including local employment, municipal tax revenues, climate mitigation, lower electricity prices, or the municipality's sustainable image. This block allows us to measure whether respondents perceive renewable energy projects primarily through a climate-benefit frame, a local-cost frame, or a territorial-justice frame.

Before randomization, we will also elicit respondents' factual beliefs about the territorial distribution of renewable-energy facilities. In particular, respondents will estimate the share of wind-hosting municipalities that have 10,000 inhabitants or fewer and the share of solar-hosting municipalities that are rural. We will also ask them to classify whether renewable-energy facilities are distributed evenly between rural and urban municipalities or are concentrated in one type of municipality. These questions are asked before the treatment in order to measure baseline knowledge and to identify whether the information treatments correct misperceptions about the territorial concentration of renewable infrastructure.

Treatment

Respondents will be randomly assigned in equal proportion to either a *control* group or one of two different *treatment* groups. These three experimental conditions differ in the amount of information received.

- *Treatment A*: Participants will receive information on the uneven territorial distribution of wind and solar facilities in Spain. They will be informed that these facilities are built mostly in rural and sparsely populated municipalities, where land and wind or solar resources are available, while most electricity is consumed elsewhere, especially in urban areas. The treatment emphasizes that a relatively small number of rural areas host the local landscape and land-use impacts of renewable facilities, while the benefits of renewable energy—electricity and lower emissions—are shared by the population as a whole, including areas that do not host such facilities.

- *Treatment B*: Participants will receive the same information as in Treatment A and will additionally be informed that hosting renewable-energy facilities can generate economic resources for host municipalities. The treatment explains that wind and solar projects may increase municipal revenues through taxes and other project-related payments and may contribute to local services, infrastructure, or community investment. It also introduces compensation and benefit-sharing mechanisms through which part of the value generated by renewable-energy projects can be returned to the communities that host them.

- *Control* group: No additional information.

Some details of the design are worth noting. First of all, the information will be provided by means of a short and colorful video (less than 2 minutes), explaining the inequality on the renewable energy facilities' location (*Treatment A*) and, on top of that, informing respondents about local revenues and compensation or benefit-sharing mechanisms for host municipalities (*Treatment B*). The content of the video will be based on Muñoz and Tormos (2025), Fabra, Gutierrez, Lacuesta and Ramos (2024) and Serra-Sala (2024). To enhance engagement and ease the understanding of the treatment, the video will include colorful clear graphs, numerical information, animations, and visual aids (Haaland, Roth and Wohlfart (2023)). Finally, the control group does not receive any additional relevant information on the topic. Thus, we will be able to precisely capture the impact of the information about inequalities on participants' support for renewable-energy policies (by comparing *control* and *treatment A* groups) and the impact of the information about compensation schemes (by comparing *treatment A* and *treatment B* groups). Additionally, given the nature of the design, we will be able to assess the heterogeneity of the different treatments' impact depending on whether the treated individual belongs to a rural municipality or to an urban one.

Posterior beliefs and comprehension

After the treatment or control screen, all respondents will answer posterior-belief questions that mirror, without using identical wording, the pre-treatment belief measures. This will allow us to estimate belief updating by comparing respondents' pre-treatment and post-treatment beliefs about the territorial distribution of renewable-energy facilities.

Outcomes

The primary outcome will be willingness to accept a new wind or solar facility in the respondent's municipality or in a nearby municipality. This item is asked at the end of the posterior beliefs block and right before the mitigation-policy block, because the central political problem is local acceptance rather than abstract support. Additional outcomes include support for a green-infrastructure programme financed with public debt, perceived effectiveness and fairness of that programme, support for public spending on renewable deployment, support for an urban electricity-bill surcharge to compensate rural host municipalities, support for urban self-generation, support for procedural safeguards, and voting intentions under hypothetical party platforms.

The questionnaire measures procedural fairness directly. The redistributive-policy block adds an item asking whether affected communities should be consulted early, receive full information, and have a real right to appeal or negotiate conditions before a facility is built. This responds to the concern that opposition may be driven not only by distributive outcomes, but also by perceived exclusion from siting decisions. A full comparison of compensation, allocation rules, and procedural rights could be developed in a follow-up conjoint design, but the present survey measures procedural demand within the current budget and length constraints.

Conclusion

In the final section, respondents will be invited to share any personal experience related to renewable energy projects, including whether they live near a wind farm or solar plant, know of a specific case, or have observed unequal effects across municipalities or social groups. We will use these open-ended responses to complement the quantitative analysis and better understand the narratives through which citizens interpret renewable energy deployment.

Finally, respondents will report whether they consider the information presented in the survey reliable and whether they perceive it as ideologically biased. These variables will be used as experimental diagnostics. The survey concludes with the experimentally varied concern item described in Section 1.2, which provides a direct diagnostic of sensitivity to perceived researcher expectations.

1.2 Biases

We include a feedback entry field at the end of the survey to test whether respondents perceived the survey as politically inclined.

Bias in answer selection

To assess whether systematic extreme-response behavior affects the results, we will construct an *Extreme Response Sum-Score* (ERS), based on Johnson et al. (2005), using eligible multi-category items measured before treatment assignment. For each item, responses at either endpoint of the response scale will be coded as one and all non-extreme responses as zero; these indicators will then be summed at the respondent level. Restricting the ERS to pre-treatment items ensures that the measure cannot itself be affected by treatment assignment.

The ERS will not enter the preferred specification. As robustness checks, we will re-estimate the main treatment effects after adding the ERS as a covariate and after excluding respondents who select an extreme response on every eligible pre-treatment item. These analyses will be interpreted as sensitivity checks rather than as alternative main specifications.

Experimenter Demand Effect

Experimenter demand effects are a potential concern in information-provision experiments if respondents infer the researchers' objectives from the treatment and adjust their answers accordingly (De Quidt et al., 2018;Haaland et al., 2023). De Quidt et al. (2018) show that such effects arise from participants' beliefs about the experimenter's expectations, although their evidence suggests that demand effects are relatively modest on average. Nevertheless, their importance may vary across settings. Following the recommendations discussed by Haaland et al. (2023), we mitigate this concern by preserving respondent anonymity and using neutral wording throughout the survey and information treatments, thereby limiting explicit cues about the researchers' preferred responses. At the end of the survey, we also ask respondents whether they perceived the information provided as ideologically biased. We will use this item as a diagnostic of respondents' perceptions of the study rather than as a direct measure of experimenter demand.

Social desirability bias

Respondents are assured of complete anonymity in both the survey landing page and the consent form. We also consistently remind participants that we are only interested in their views, and that all responses are strictly confidential and anonymous. The block of questions related to support for renewable energy policies presents arguments both in favor of and against these policies, thereby reducing the likelihood that respondents answer in a socially desirable way.

2 Setting, sample size and restrictions

We will run our experiment through the certified market research company Netquest, which is widely used to conduct experiments. We will only recruit participants who currently live in Spain and have a Spanish nationality. Moreover, panelists must be between 18 and 64 years old. We will apply quotas depending on age group, gender and region of residence. We will also oversample population from rural areas, defined as those individuals living in municipalities with fewer than 5,000 inhabitants. Upon completion of the survey, Netquest pays participants a reward in points, depending on the length of the survey. We apply several rules for inclusion in the survey. In particular, we exclude participants that:

- Do not satisfy the quota requirement;
- Do not pass the attention check;
- Exit and then re-enter the survey as a new subject (as these individuals might see multiple treatments).

In total, we recruit 2,000 Spanish respondents. The sampling plan targets 750 respondents from municipalities below 5,000 inhabitants, corresponding to 37.5% percent of the sample. ith approximately equal assignments across the three experimental arms, the expected number of rural respondents is approximately 250 per arm, although the realized number will vary across arms. The remaining 1,250 respondents will be drawn from larger municipalities according to the fieldwork quota structure. Survey weights will be used to recover nationally representative estimates with respect to the Spanish population aged 18–64.

2.1 Power analysis and implications for the design

We conduct a simulation-based power analysis to assess whether the planned sample provides sufficient statistical power under the treatment-assignment procedure that will be implemented in the field. The analysis follows a simulation approach in which synthetic datasets are repeatedly generated under explicit assumptions about treatment effects and outcome variance, treatment is assigned according to the actual randomization procedure, the pre-specified regressions are estimated by OLS, and the null hypothesis of no treatment effect is tested at the 5 percent two-sided significance level. Headline power calculations are based on 4,000 simulated experiments; implying a maximum Monte Carlo standard error of approximately 0.8 percentage points; effect-size grids and sensitivity analyses use 1,500 simultaions per configuration, for a maximum Monte Carlo standard error of approximately 1.3 percentage points.

The simulations reproduce the assignment mechanism that will be used in the survey. The sample will continue to target 750 respondents living in municipalities with fewer than 5,000 inhabitants, corresponding to 37.5 percent of the total sample. However, rural residence will not be used to block treatment assignment. Respondents will instead be randomly assigned in approximately equal proportions to the control group, Treatment A, or Treatment B within gender-by-age quota cells. Consequently, the number of rural respondents assigned to each experimental arm is random rather than fixed at 250. Simulations of the assignment procedure show that each arm contains 250 rural respondents on average, with a standard deviation of approximately 10 respondents; 90 percent of simulated assignments contain between 233 and 267 rural repsondents per arm.

The power analysis considers four estimands. First, we evaluate the full-sample treatment-control contrasts for each active treatment arm. Second, we estimate treatment effects within the rural subsample. Third, we evaluate rural-by-treatment interactions, which capture differences in treatment effects between rural and urban respondents. Fourth, we consider nationally representative treatment effects obtained using survey weights that restore the rural population share. Outcomes are standardized to have unit variance, so treatment effects and minimum detectable effects are expressed in standard-deviation units.

The benchmark data-generating process allows rural and urban respondents to differ in their outcome levels even in the absence of treatment. This is important because the theoretical framework predicts lower baseline support for nearby renewable-energy facilities among rural respondents. The benchmark assumes a baseline rural-urban difference of 0.35 standard deviations, an urban treatment effect of 0.15 standard deviations, a rural treatment effect of 0.30 standard deviations, and therefore a rural-urban treatment-effect differential of 0.15 standard deviations. Rural residence and the remaining pre-treatment covariates are assumed jointly to explain 20 percent of outcome variance. The population rural share used for the nationally weighted estimand is 20 percent.

The simulations use the following benchmark assumptions.

Table 1: Benchmark simulation assumptions

Parameter	Value
Total sample size N	2,000
Experimental arms	3 (Control, T_A, T_B)
Assignment	Randomization within gender-by-age quota cells
Rural respondents (sample share)	750 (37.5%)
Rural respondents per arm	Random; Expected value $\approx$ 250
Rural population share (for weights)	0.20
Urban effect size d_{urban} (benchmark)	0.15 SD
Rural effect size d_{rural} (benchmark)	0.30 SD
Implied interaction $d_{\text{rural}} - d_{\text{urban}}$	0.15 SD
Baseline rural-urban difference	0.35 SD
Covariate explanatory power R^2	0.20
Significance level	$\alpha = 0.05$, two-sided
Headline simulations	4,000

Note: Effect sizes are in standard-deviation units of the standardized outcome. Reported power for treatment effects is the average rejection rate across the two arms; arm-specific power is essentially identical by symmetry.

Because treatment is not blocked by rural residence, chance differences in the rural composition of the experimental arms can modestly reduce precision when rural residence predicts the outcome. We therefore pre-specify an adjusted estimating specification for the full-sample and nationally weighted estimands that includes the two treatment indicators, rural residence, gender-by-age quota-cells fixed effects,and pre-treatment covariates:

$$Y_i = \alpha + \beta_A T_i^A + \beta_B T_i^B +_i + \sum_c \lambda_c Q_{ci} + \gamma' X_i + \varepsilon_i, \tag{1}$$

where R_i indicates rural residence and Q_{ci} denotes the gender-by-age quota cells usd in the assignment procedure. The corresponding within rural specification conditions on the rural subsample, while the heterogeneity specification includes interactions between rural residence and the treatment indicators.

The simulations indicate that the statistical cost of not blocking on rural residence is small. Under the benchmark assumptions, moving from rural-blocked to quota-cell randomization reduces simulated power by approximately halg a percentage point for the full-sample and nationally weighted estimands when the unadjusted estimator is used. In terms of precision, the standard deviation of the estimated full-sample treatment effect increases by approximately .26 percent. Including rural residence and the assignment-cell fixed effects recovers essentially all of this precision loss. Within-rural treatment effects and rural-by-treatment interactions are essentially unaffected by the absence of rural blocking because the former condition on rural residence by construction and the latter explicitly model it.

At the benchmark effect sizes, the adjusted specification achieves approximately 99 percent power for the full-sample treatment-control contrasts, 96 percent for the within-rural treatment effects, and 94–95 percent for the nationally weighted treatment effects. By contrast, power for the rural-by-treatment interaction is only approximately 32 percent because the assumed rural-urban differential is substantially smaller than the treatment effects within either group.

Table 2: Simulated power by assumed effect size (N = 2,000, 750 rural, R^2 = 0.20, $\alpha = 0.05$)

Assumed effects (SD units)			Simulated power			
d_{urban}	d_{rural}	Interaction	Full sample	Within-rural	Interaction	National (wtd.)
0.05	0.10	0.05	0.27	0.23	0.08	0.24
0.10	0.20	0.10	0.79	0.70	0.17	0.66
0.15	0.30	0.15	0.99	0.96	0.32	0.95
0.20	0.40	0.20	1.00	1.00	0.50	0.99
0.25	0.50	0.25	1.00	1.00	0.69	1.00
0.30	0.60	0.30	1.00	1.00	0.83	1.00
0.35	0.70	0.35	1.00	1.00	0.93	1.00
0.40	0.80	0.40	1.00	1.00	0.97	1.00
0.45	0.90	0.45	1.00	1.00	0.99	1.00

Note: 1,500 simulations per grid point: Monte Carlo SE $\leq$ 1.33 p.p. The benchmark configuration is the row $d_{urb} = 0.15$. "Within-rural" power is indexed by the row's d_{rur}; not d_{urb}.

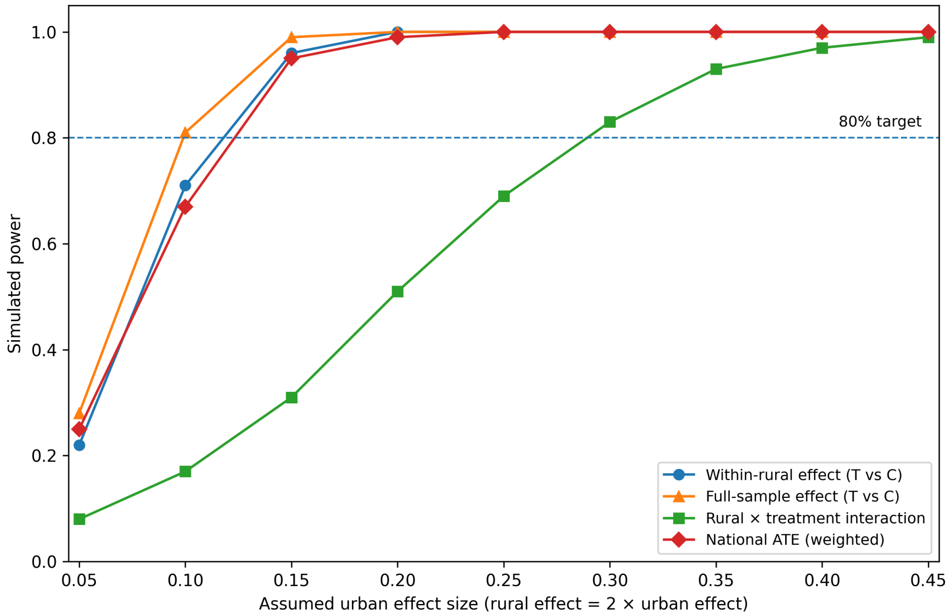

Figure 2: Simulated power as a function of the assumed effect size at the design point. The horizontal axis reports the urban effect size; the rural effect is assumed to be twice as large. The dashed line marks 80% power.

The minimum detectable effects confirm this distinction. With 80 percent power, the design can detect full-sample effects of approximately 0.10 standard deviations and within-rural effects of approximately 0.24 standard deviations. The nationally weighted minimum detectable effect is approximately 0.12 standard deviations. In contrast, the rural-urban difference in treatment effects would need to be approximately 0.29 standard deviations to reach conventional 80 percent power.

Table 3: Simulated power by rural share of the sample (benchmark effects: $d_{\text{urban}} = 0.15$, $d_{\text{rural}} = 0.30$)

Rural share	Rural N	Rural per arm	Full sample	Within-rural	Interaction	National (wtd.)
0.200	400	133	0.96	0.78	0.24	0.96
0.300	600	200	0.98	0.93	0.30	0.95
0.375	**750**	**250**	**0.99**	**0.97**	**0.31**	**0.95**
0.450	900	300	1.00	0.99	0.34	0.93
0.500	1,000	333	1.00	0.99	0.34	0.92

Note: The row in bold is the chosen design, corresponding to the 37.5% rural quota. The first row corresponds to a purely proportional sample with no oversampling. Power is the average rejection rate across the two treatment arms over 1,000 simulations per row (Monte Carlo SE $\leq$ 1.6 p.p.).

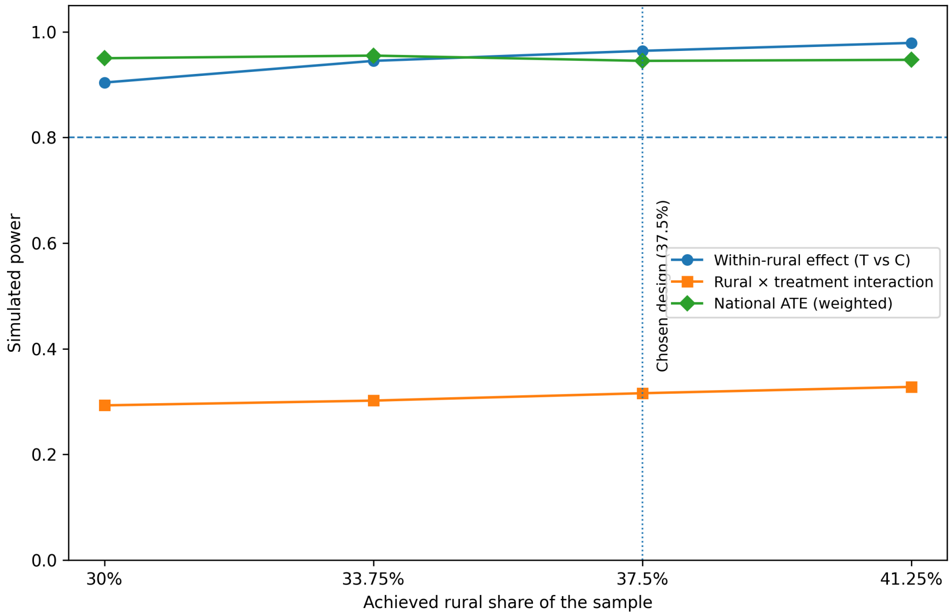

Figure 3: Simulated power as a function of the rural share of the sample, holding benchmark effects fixed. The dotted vertical line marks the chosen design with 37.5% rural respondents

These calculations imply that the full-sample and within-rural treatment effects are appropriately powered to serve as primary confirmatory estimands. Rural-urban differences in treatment effects will instead be treated as exploratory heterogeneity analyses. We will report rural and urban subgroup estimates and the corresponding interaction coefficients with confidence intervals, but a statistically insignificant interaction will be interpreted cautiously.

The achieved rural recruitment target remains more important for the within-rural analysis than the absence of rural blocking. Holding total sample size fixed at 2,000, simulations indicate that reducing the rural subsample from 750 to 675 respondents lowers within-rural power from approximately 96 to 95 percent, while a reduction to 600 respondents lowers it to approximately 93 percent. We therefore maintain the target of 750 rural respondents despite not using rural residence as a treatment-assignment stratum.

Finally, simulations under the null confirm that the loss of rural blocking does not distort the size of the conventional tests for the full-sample, within-rural, or interaction estimands. The weighted national specification, however, displays mild over-rejection when conventional homoskedastic standard errors are used. We will therefore use heteroskedasticity-robust HC2 standard errors for the weighted national estimates.

3 Hypotheses

The information collected through the survey by exposing respondents to two differentiated treatment groups will allow us to test the following set of hypotheses. First, regarding prior beliefs and baseline attitudes, we expect:

1. Rural respondents to report lower support for nearby renewable installations than urban respondents;

2. Rural respondents to be more aware of the local costs and benefits associated with wind farms and solar plants;

3. Respondents with higher education are associated with more accurate beliefs about the territorial concentration of renewables;

With regard to the experimental treatments, we hypothesize that:

4. Both treatments should move posterior wind/solar beliefs toward the factual benchmarks; updating should be greater among prior underestimators. Additionally, the compensation information treatment should increase beliefs that host municipalities may receive revenues/compensation relative to A and control.

5. The inequality-information treatment should reduce unconditional willingness to accept nearby facilities among respondents.

6. The inequality-information treatment should increase perceived unfairness of the current distribution and increase demand for compensation and procedural remedies;

7. Adding compensation information should increase willingness to accept nearby facilities relative to the inequality-only arm, indicating that compensation can offset the burden effect;

8. Compensation information should have stronger effects among respondents who, before treatment, already considered local fiscal revenues, job creation or electricity-price reductions to be credible benefits of renewable-energy facilities.

The power analysis leads us to distinguish between primary confirmatory estimands and secondary heterogeneity analyses. Our primary outcome is willingness to accept a new wind or solar facility in the respondent's municipality or in a nearby municipality. For this outcome, the main confirmatory experimental contrasts are Treatment A versus Control, Treatment B versus Control, and Treatment B versus Treatment A. The first two contrasts identify the effects of the two information treatments relative to receiving no relevant information, while the third identifies the incremental effect of adding information about local revenues, compensation, and benefit-sharing to the territorial-inequality information.

Treatment-control effects within the rural subsample are also pre-specified confirmatory subgroup estimands. By contrast, rural–urban differences in treatment effects are treated as secondary heterogeneity analyses because the design has conventional power only for relatively large interaction effects. We will therefore report rural and urban subgroup estimates together with the corresponding interaction coefficients and confidence intervals, and will not interpret a statistically insignificant interaction as evidence of homogeneous treatment effects.

Finally, we will examine heterogeneous treatment effects. We expect the treatment effect to vary by:

9. Rural residence;
10. Prior overestimation/underestimation of the territorial concentration of renewable infrastructure;
11. Prior exposure to wind farms or solar plants;
12. Ideological orientation;
13. Age
14. Educational attainment;

4 Analysis

4.1 Main analysis

As described in the section on linked administrative and contextual data, we will merge the survey data with external municipal- and postal-code-level information. These sources will provide measures of exposure to renewable-energy facilities, socio-demographic characteristics of municipalities, and electoral and political-context variables. These linked variables will be used as pre-treatment contextual controls and as moderators in the heterogeneity analysis.

For descriptive analyses, we distinguish between variables measured before and after treatment assignment. Pre-treatment beliefs and attitudes will be described using the full sample. Descriptive analyses of variables elicited after treatment assignment will use the control group as the untreated benchmark. In both cases, we will report descriptive statistics both without and with survey weights. The unweighted statistics will characterize the deliberately rural-heavy experimental sample, while the weighted statistics will characterize the target Spanish population aged 18–64, allowing readers to assess how weighting changes the reported levels.

4.1.1 Baseline balance

We will assess baseline balance across the three experimental arms using the following pre-treatment characteristics:

- gender and age;
- region of residence;
- educational attainment;
- log household income;
- employment-status indicators;
- presence of dependent children;
- rural residence;
- political ideology;
- baseline support for local renewable-energy deployment;
- prior belief about wind concentration;
- prior belief about solar rurality;
- prior geographical exposure to any renewable installation, and separately to wind and solar installations.

For each characteristic, we will report descriptive statistics by experimental arm and estimate differences between Treatment A and Control and between Treatment B and Control. We will additionally conduct an omnibus test of whether the full set of pre-treatment characteristics jointly predicts experimental assignment. Detailed balance results, including additional political and socio-demographic variables collected in the questionnaire, will be reported in the Appendix. Lastly, using population benchmarks from the Spanish National Statistical Institute (INE), we will compare the final survey sample with the general Spanish population aged 18–64. This comparison will be based on the variables available both in the survey and in the external benchmark data, such as age, gender, region, municipality size, and other socio-demographic characteristics where available. We will use these comparisons to assess selection into the survey and, where appropriate, to inform the construction or validation of survey weights.

4.1.2 Differential Attrition

We will test whether survey completion after treatment assignment differs across experimental conditions by estimating

$$Completion_i = \alpha + \delta_A T_i^A + \delta_B T_i^B + \eta' X_i + \varepsilon_i,$$

where $Completion_i$ equals one when respondent i completes the survey and T_i^A and T_i^B denote assignment to the two active treatment arms. X_i contains pre-treatment characteristics available for both completers and non-completers.

We will report δ_A and δ_B separately and conduct a joint test of $H_0 : \delta_A = \delta_B = 0$. We will also test whether attrition differs between Treatment A and Treatment B. If completion is systematically related to treatment assignment, we will assess the sensitivity of the main treatment effects to selective attrition and, where the required assumptions are plausible, report Lee bounds (Lee, 2005) for the affected treatment contrasts.

4.1.3 Attention check

Halfway through the survey, we present respondents with a simple attention check designed to assess whether they are reading the survey instructions carefully: they are asked to select the second option(''Brown") as their favorite color, even if it is not true. Respondents who do not select the instructed response will fail the validation check and will be screened out of the survey.

4.1.4 Prior beliefs

We measure respondents' prior beliefs about the territorial distribution of renewable energy facilities in Spain before the experimental manipulation. The purpose of this block is to assess whether respondents are aware that wind and solar facilities are disproportionately located in rural and sparsely populated municipalities, and whether these prior beliefs moderate the effect of the information treatments.

We elicit prior beliefs using quantitative questions. Respondents are asked to estimate, on a 0–100 scale, the share of wind-hosting municipalities that have 10,000 inhabitants or fewer, and the share of solar-hosting municipalities that are rural. These two questions correspond to the core factual content of the information treatment. The true values used in the treatment are 87 percent for wind-hosting municipalities with 10,000 inhabitants or fewer [1].

We will analyze prior beliefs in terms of gaps between respondents' guesses and the true values. In the paper, we will plot the distribution of each belief variable, including

[1] This 10,000 threshold is the factual quantity elicited for wind, which differs from the definition used in the analysis to classify survey respondents as rural, i.e. municipalities with $\leq 5,000$ inhabitants.

a vertical line for the true value. We will also create indicators for whether respondents under- or over-estimate the true value within several symmetric intervals. In particular, we will use a benchmark threshold of ± 10 percentage points around the true value and further ± 2, ± 5pp as robustness checks.

These variables are elicited before randomization and before exposure to the informational videos. Similar, but not identical, questions are asked again after the treatment in order to measure belief updating and treatment comprehension.

The main prior-belief variables are defined as follows:

- *Prior belief: wind concentration* = respondent's estimate of the percentage of municipalities hosting wind farms that have 10,000 inhabitants or fewer.
- *Gap in wind concentration beliefs* =

  $$\textit{Prior belief: wind concentration}_i - 87.$$

  Positive values indicate that the respondent overestimates the concentration of wind farms in small municipalities, while negative values indicate underestimation.
- *Prior belief: solar rurality* = respondent's estimate of the percentage of municipalities hosting solar plants that are rural.
- *Gap in solar rurality beliefs* =

  $$\textit{Prior belief: solar rurality}_i - 70.$$

  Positive values indicate that the respondent overestimates the rural concentration of solar plants, while negative values indicate underestimation.
- *Accurate wind belief* = 1 if the respondent's estimate of wind concentration lies within a ± 10 percentage points interval around the true value, and 0 otherwise. Although, the ± 10 will be the benchmark, we will also use alternatibve thresholds of ± 2, and ± 5 percentage points as robustness checks.
- *Accurate solar belief* = 1 if the respondent's estimate of solar rurality lies within a given interval around the true value, and 0 otherwise. We will construct this variable using the same thresholds.
- *Underestimation of wind concentration* = 1 if the respondent's estimate is below the lower bound of the corresponding accuracy interval, and 0 otherwise.
- *Overestimation of wind concentration* = 1 if the respondent's estimate is above the upper bound of the corresponding accuracy interval, and 0 otherwise.

- *Underestimation of solar rurality* = 1 if the respondent's estimate is below the lower bound of the corresponding accuracy interval, and 0 otherwise.
- *Overestimation of solar rurality* = 1 if the respondent's estimate is above the upper bound of the corresponding accuracy interval, and 0 otherwise.

We will also construct a summary measure of prior awareness of territorial concentration. The main version of this measure will be an accuracy index based on the absolute distance between each respondent's estimate and the corresponding factual benchmark. Absolute deviations from a factual benchmark provide a natural measure of belief accuracy, abstracting from whether the respondent over- or underestimates the true value. Related standardized measures of absolute error have been used to compare the accuracy of beliefs across different dimensions (Cullen and Perez-Truglia, 2023). More generally, standardization allows conceptually related measures expressed on different scales to be compared and combined into summary indices (Kling et al. , 2007).

For each technology k, we first define raw accuracy as the negative absolute belief error,

$$A_i^k = -\left|Belief_i^k - Truth^k\right|,$$

and standardize it using the survey-weighted mean and standard deviation:

$$Accuracy_i^k = \frac{A_i^k - \mu_{A^k,w}}{\sigma_{A^k,w}}.$$

Higher values of $Accuracy_i^k$ therefore indicate more accurate beliefs, while lower values indicate larger deviations from the factual benchmark. By construction, the survey-weighted mean of each technology-specific accuracy measure is zero and its standard deviation is one. Thus, for example, a value of one indicates accuracy one standard deviation above the population mean, rather than an error of one percentage point.

In addition to factual beliefs, we elicit respondents' views about the fairest criterion for deciding where wind and solar facilities should be located. This item captures respondents' prior fairness denominator before treatment assignment. Respondents are asked whether facilities should be allocated according to electricity consumption, population, available land, technical suitability, financial compensation for host areas, local consultation and negotiation, or another criterion. We will use this variable descriptively and as a pre-specified moderator of treatment effects.

For the fairness-denominator item, we will create the following variables:

- *Consumption-based criterion* = 1 if the respondent states that facilities should be located in proportion to how much electricity each area consumes.

- *Population-based criterion* = 1 if the respondent states that facilities should be located in proportion to population.
- *Land-based criterion* = 1 if the respondent states that facilities should be located in proportion to available land or surface area.
- *Technical-suitability criterion* = 1 if the respondent states that facilities should be located wherever they are technically most suitable, considering wind, sun, grid connection, and protected areas.
- *Compensation criterion* = 1 if the respondent states that areas hosting more facilities should be financially compensated by others.
- *Procedural criterion* = 1 if the respondent states that affected communities should decide through consultation and negotiation.

We will examine whether prior beliefs are balanced across the control group and the two treatment groups. Prior beliefs will also enter the pre-specified heterogeneous treatment- effect analysis. In particular, we expect the information treatments to generate stronger belief updating among respondents who initially underestimate the territorial concentration of renewable facilities.

The preferred-siting-criterion item will be used primarily for descriptive analysis of baseline preferences. Exploratory analyses may additionally examine whether treatment responses differ across broad categories of siting principles, but these comparisons will not be treated as confirmatory heterogeneity tests.

4.1.5 Treatment

We will create two dummy variables, T_i^A and T_i^B, indicating assignment to Treatment A and Treatment B, respectively. The control group is the omitted category. Respondents are required to visualize a short video and paying attention to it. They are instructed to continue with the questions by clicking "NEXT", once they have finished. It is not possible to proceed to the next section if they have not finished the video. At the end of the video, respondents may re-watch the information another time. Thus, we will measure the time spent on the video section and generate an indicator for having re-watched the video.

4.2 Outcomes

The estimation strategy will be mainly based on OLS regressions. Since the experiment includes two treatment arms and one control group, our main specification will estimate the effect of each treatment relative to the control group. We will estimate the following equation:

$$Y_i = \alpha + \beta_A T_i^A + \beta_B T_i^B + \delta R_i + \sum_c \lambda_c Q_{ci} + \gamma' X_i + \varepsilon_i,$$

where Y_i is the outcome of interest for respondent i, T_i^A and T_i^B indicate assignment to Treatment A and Treatment B respectively, and the control groupp is the omitted category. R_i is an indicator for residence in a rural municipality, defined as a municipality with fewer than 5,000 inhabitants, and Q_{ci} denotes fixed effects for the gender-by-age quota cells within which treatment assignment is conducted. Therefore, β_A captures the causal effect of receiving information on the unequal territorial distribution of renewable energy facilities relative to receiving no information, while β_B captures the causal effect of receiving both the territorial-distribution information and the additional information on local revenues, compensation, and benefit-sharing mechanisms.

The vector X_i contains a pre-specified set of individual-level variables measured before treatment assignment and selected to improve precision. Specifically, we will include as control variables the personal characteristics drawn from questions in the sociodemographics' section of the survey and the municipal characteristics that we import from the external administrative datasets. We will sequentially implement the regression without control, with individual characteristics as the only control, and finally adding municipal characteristics as further controls. We will further add province and day-of-interview FE.

This adjusted specification is pre-specified as the main estimating equation. Although random assignment ensures that covariate adjustment is not required for identification, the power analysis shows that including rural residence and the gender-by-age assignment-cell fixed effects recovers essentially all of the precision loss associated with not blocking treatment assignment on rural status. As a robustness check, we will also report an unadjusted specification containing only the two treatment indicators.

Our preferred unweighted estimates characterize causal effects in the experimental sample. Because respondents from municipalities with fewer than 5,000 inhabitants are deliberately oversampled, we will additionally estimate the same specification using survey weights that restore the population distribution of Spanish residents aged 18–64. These weighted estimates represent nationally representative population-level estimands rather than alternative identification strategies or conventional robustness checks. For the primary outcome, we will report both the unweighted experimental-sample estimates and the nationally representative weighted estimates in the main analysis. Complete weighted counterparts for secondary outcomes will also be reported. Standard errors robust to heteroskedasticity will be used throughout.

Our main analysis will report the following specifications:

1. a specification without control variables;

2. a specification including individual-level pre-treatment controls;
3. a specification including municipality-level controls;
4. a specification including both individual-level and municipality-level controls.

We will also include geographical fixed effects and, when relevant, day-of-interview fixed effects to increase precision.

The outcomes of the experiment are organized as follows:

- **First-stage outcomes.** Posterior beliefs about the territorial concentration of wind and solar installations constitute the main first-stage outcomes. We additionally examine respondents' beliefs about whether renewable-energy projects can generate additional revenues, compensation, or other resources for host municipalities.
- **Primary outcome.** Our primary outcome is respondents' willingness to accept a new wind or solar facility in their municipality or in a nearby municipality.
- **Territorial-justice and policy outcomes.** These include perceived unfairness of the current territorial distribution of renewable-energy installations; perceived effectiveness and fairness of, and support for, the green-infrastructure programme; support for additional public spending on renewable-energy deployment; support for an electricity-bill surcharge on urban consumers to compensate rural host municipalities; support for greater renewable generation in urban areas; and support for procedural safeguards for communities affected by new installations.
- **Political outcomes.** Respondents evaluate three hypothetical party platforms: an ambitious expansion of renewable energy combined with rural compensation; a slowdown in deployment accompanied by greater local decision-making authority; and a territorial- justice platform combining compensation, a more equitable distribution of benefits, and greater local participation.

Unless otherwise specified, treatment effects will be estimated using the original response scales. Outcomes will be oriented so that higher values have a substantively consistent interpretation whenever possible. In particular, local-acceptance and policy-support outcomes will be coded so that higher values indicate greater acceptance or support. The item measuring fairness of the current territorial distribution will be reverse-coded so that higher values indicate greater perceived unfairness. The hypothetical voting outcomes will be analyzed on their original five-point scales.

As an alternative outcome definition, we will also construct binary versions of the ordered outcomes whenever the response scale permits a substantively meaningful distinction between favorable and non-favorable responses. These indicators will equal one when the respondent expresses acceptance, support, perceived unfairness, or a positive voting intention, as appropriate, and zero otherwise. For five-point scales with a neutral midpoint, responses on the two favorable categories will be coded as one, while the neutral category and the two unfavorable categories will be coded as zero. We will estimate the same experimental specifications using these binary outcomes, so that treatment coefficients can be interpreted as percentage-point changes in the probability of expressing a favorable response. The original-scale outcomes constitute our main outcome definitions, while the binary transformations will be reported as complementary analyses.

For each outcome, we will estimate the effect of Treatment A and Treatment B relative to the control group. We will also test whether the two treatment effects differ from each other. In particular, we will test:

$$H_0 : \beta_A = \beta_B.$$

This comparison allows us to assess whether adding information on municipal revenues, compensation, and benefit-sharing mechanisms changes respondents' attitudes relative to providing only information on the unequal territorial distribution of renewable facilities.

As a complementary analysis, we will also consider a pooled treatment indicator,

$$T_i^{Any} = T_i^A + T_i^B,$$

which equals one for respondents assigned to either active treatment and zero for respondents assigned to the control group. The corresponding coefficient captures the average effect of assignment to either information treatment relative to the control condition. Because Treatment B contains additional information on local revenues, compensation, and benefit-sharing that is not included in Treatment A, this pooled coefficient will not be interpreted as identifying the effect of a single common informational component. It is therefore treated as a complementary rather than a primary confirmatory estimand.

A central contrast in the experiment is the incremental effect of the information specific to Treatment B. We estimate this effect directly from the main three-arm specification as

$$\Delta^{Comp} = \beta_B - \beta_A.$$

We will test the null hypothesis $H_0 : \beta_A = \beta_B$ using the corresponding linear restriction. This contrast measures the effect of adding information about municipal revenues, compensation, and benefit-sharing to the territorial-inequality information received by both active treatment groups. A positive value of Δ^{Comp} for the primary outcome indicates that the additional compensation information increases local acceptance relative to the inequality-only treatment.

As a robustness check, we may replicate this comparison in the subsample containing only Treatment A and Treatment B respondents.

Finally, we will examine heterogeneous treatment effects using the following specification:

$$Y_i = \alpha + \beta_A T_i^A + \beta_B T_i^B + \delta Z_i + \theta_A (T_i^A \times Z_i) + \theta_B (T_i^B \times Z_i) + \gamma' X_i + \varepsilon_i,$$

where Z_i denotes a pre-treatment characteristic. Our primary heterogeneity analyses focus on three characteristics: (i) rural residence; (ii) prior geographical exposure to renewable- energy infrastructure; and (iii) prior beliefs about the territorial concentration of renewable installations. For prior beliefs, particular attention will be paid to respondents who underestimate the corresponding factual benchmark using the pre-specified ± 10 percentage-point classification. We additionally examine heterogeneity by political ideology, age, and educational attainment. Thus, the six pre-specified moderators are rural residence, prior renewable exposure, prior beliefs, ideology, age, and education.

Consistent with the power analysis, within-rural treatment effects are confirmatory subgroup estimands, whereas rural–urban interaction coefficients are secondary heterogeneity estimands. For rural residence and prior exposure, we will report subgroup treatment estimates with confidence intervals alongside the corresponding interaction coefficients.

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