When Do Electoral Power Grabs Increase Support for Election Monitoring? Evidence from a Survey Experiment in Turkey

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Abstract

How do citizens respond when incumbents manipulate electoral rules before an election? I test this question with a survey experiment in Turkey that compares an institutional electoral intervention, an extra-institutional unilateral intervention, and a procedural reform framed around European Union harmonization. The results reveal a heterogeneous response rather than a general mobilization effect. Opposition supporters exposed to the extra-institutional intervention show greater support for election-monitoring resources than opposition supporters in the EU-framed control condition, while government supporters show no comparable increase. This pattern is concentrated in support for election-monitoring resources rather than broader participatory intentions. The findings suggest that electoral power grabs

can increase opposition support for monitoring-oriented accountability, while leaving broader participation and the precise mechanism more uncertain.

Appendix A. Conceptual Scope and Omitted Theory

This appendix situates the article's distinction between institutional and extra-institutional electoral manipulation within a broader conceptual framework. The main text focuses on the narrower contrast required by the survey experiment: ordinary legislative electoral change versus unilateral extra-institutional intervention. This appendix provides the wider theoretical background for that distinction and clarifies its scope.

A1. Broader Theoretical Motivation

Electoral manipulation plays a central role in democratic backsliding because it allows elected incumbents to reshape the political playing field before competition takes place (Svolik 2018; Haggard and Kaufman 2021; Przeworski 2022). Existing work often emphasizes the demobilizing consequences of electoral unfairness. When citizens perceive the electoral arena as corrupt or rigged, they may lose confidence in electoral procedures and become less willing to participate in costly political action (Birch 2010; Norris 2014; Martinez i Coma and Trinh 2017; Frank and Coma 2017). At the same time, a number of cases suggest that opposition responses are not uniform. In some settings, incumbents' interventions provoke discouragement and withdrawal; in others, they appear to trigger protest, monitoring, or other forms of accountability-oriented action (Cleary and Öztürk 2022; Svolik 2021).

The article builds on this tension by asking whether citizens react differently to pre-election manipulations depending on how incumbents pursue electoral advantage. More specifically, it examines whether a procedurally ordinary electoral intervention elicits a different response than one pursued through extra-institutional, unilateral means.

A2. Relating the Focal Distinction to Wider Typologies

The broader literature often distinguishes between *ex-ante* and *ex-post* electoral manipulation (Little 2015; Rundlett and Svolik 2016; Luo and Rozenas 2018). Ex-ante manipulations occur before election day and alter the political or procedural environment in which competition takes place. Examples include changing electoral rules, restructuring districts, restricting opposition access, or pressuring administrative bodies in advance. Ex-post manipulations, by contrast, occur during or after voting and involve interventions in the recording, counting, certification, or acceptance of electoral outcomes (Little 2015; Luo and Rozenas 2018).

This article examines only a subset of ex-ante manipulation. Within that category, it distinguishes between two modes of enactment. *Institutional* manipulation refers to efforts to secure electoral advantage through recognizable legal-procedural channels, such as parliamentary legislation or publicly ratified reform. *Extra-institutional* manipulation refers to efforts to secure similar advantage by bypassing ordinary channels

through extraordinary decrees, unilateral interventions, or reliance on actors outside normal democratic lawmaking. The experimental design isolates one especially important procedural contrast within that broader conceptual space.

A3. A Broader Typology of Manipulation Types

Table A1 places the article's focal distinction within a broader typology of manipulation types. The main text does not rely on the full typology because the experiment identifies a narrower mode-of-enactment contrast. I include the broader table here to clarify how the article's treatment contrast relates to the wider literature.

Table 1: Broader Typology of Manipulation Types

	Ex-Ante Institutional	**Ex-Ante Extra-Institutional**	**Ex-Post Manipulation**
Target	Electoral process	Electoral process	Electoral outcome
Observability	High	Varies, often lower	Often low
Primary agents	Parliament, voters, public lawmaking bodies	Executive, bureaucracy, judiciary, coercive bodies	Bureaucracy, election authorities, coercive bodies
Illustrative tools	Laws, referenda, formal rule changes, districting reforms	Extraordinary decrees, ultra-vires directives, administrative pressures, selective interventions	Tampering with tabulation, certification, or acceptance of results
Potential implications	Publicly visible but procedurally routinized advantage	Procedurally exceptional advantage, possible signals of bureaucratic compliance or weak popular support	Direct manipulation of outcomes after votes are cast

The article does not compare all institutional manipulations to all extra-institutional manipulations. It compares two stylized pre-election interventions that produce the same substantive electoral advantage but differ in whether that advantage is pursued through ordinary legislative approval or through unilateral decree-law implementation.

A4. Scope Conditions

The informational and procedural meaning of institutional versus extra-institutional manipulation may depend on the surrounding regime context. Where public expectations of legality and representation remain meaningful, citizens may distinguish more sharply between electoral changes pursued through ordinary political channels and those pursued through extraordinary or unilateral means. In such settings, extra-institutional intervention may be more likely to provoke grievance, aversive emotion, or perceptions of heightened threat.

By contrast, in highly repressive or deeply delegitimated settings, the distinction may carry less informational value because citizens may already assume that the regime does not operate within meaningful procedural constraints (Luo and Rozenas 2018; Haggard and Kaufman 2021). At the other extreme, in highly consolidated democracies with strong procedural expectations, even institutional manipulations may trigger backlash. The Turkish case is useful because elections remain meaningful and competitive enough

for procedural distinctions to matter, while democratic backsliding and executive aggrandizement have also made electoral fairness politically salient (Esen and Gumuscu 2016; Esen and Gümüşçü 2017; Öztürk 2020).

A5. Extended Discussion of Possible Pathways

The theoretical framework considers three possible pathways through which extra-institutional intervention may provoke a stronger opposition response. First, such intervention may appear less legitimate and generate stronger grievance than a comparable institutional reform (Birch 2010; Norris 2014; Martinez i Coma and Trinh 2017). Second, it may provoke more aversive emotional reactions, including anger, disgust, and frustration, which can encourage action rather than withdrawal (Brader and Marcus 2013; S. Erdem Aytaç, Rau, and Stokes, n.d.; S. Erdem Aytaç and Stokes 2019; Leach, Iyer, and Pedersen 2006; Pagano and Huo 2007). Third, it may convey information about the incumbent's weakness, heightened bureaucratic compliance, or a greater likelihood of further manipulation before or after election day (Gehlbach and Simpser 2015; Luo and Rozenas 2018; Przeworski, Rivero, and Xi 2015).

The main text treats these pathways cautiously and examines them as exploratory rather than definitive mechanisms. The purpose of this appendix is not to make a stronger causal claim than the article can support, but to show how the experimental contrast fits within a broader theoretical account of citizen responses to electoral manipulation.

Appendix B. Case Background and Recruitment Implementation

This appendix provides additional background on the Turkish case and on the implementation of respondent recruitment. The main text includes only the information necessary to understand the logic of the survey experiment and the composition of the sample. This appendix adds fuller detail on why Turkey is a useful setting, how participants were recruited, and what respondents saw during recruitment.

B1. Why Turkey

Turkey provides a useful setting for studying citizen responses to electoral manipulation because elections remain politically meaningful while concerns about electoral fairness and executive aggrandizement have also become highly salient (Esen and Gumuscu 2016; Esen and Gümüşçü 2017; Öztürk 2020; Haggard and Kaufman 2021). Over the last decade, the ruling *Justice and Development Party* (AKP) has weakened checks and balances, concentrated power in the executive, and shaped the electoral environment in ways that have generated controversy over procedural fairness (Esen and Gumuscu 2016; Esen and Gümüşçü 2017). The 2017 constitutional referendum, the transition to executive presidentialism, and subsequent disputes over electoral administration all contributed to a setting in which citizens could plausibly distinguish between electorally consequential changes pursued through ordinary channels and those pursued through more exceptional means (Esen and Gümüşçü 2017).

At the same time, Turkey remains preferable to a fully closed authoritarian setting for the purposes of this design. Elections continue to matter, opposition mobilization remains possible, and electoral interventions remain politically contested. This combination makes Turkey a useful case for examining whether citizens react not only to the substantive consequences of electoral manipulation, but also to the procedures through which incumbents pursue it.

B2. Recruitment Through Meta Advertisements

Participants were recruited through Meta advertisements shown on Facebook and Instagram across Turkey between August 24 and August 30, 2022. The ads targeted Turkish adults age 18 and older and directed respondents to a Qualtrics survey. This approach follows recent work showing that online political recruitment through Meta can efficiently generate large convenience samples while preserving experimental leverage when treatments are randomly assigned (Neundorf and Öztürk 2021).

The recruitment strategy emphasized completion rather than simple click-throughs. Meta's conversion-oriented campaign tools were used so that ad delivery would be optimized toward users who were more likely to begin and complete the survey. In addition, separate advertisement sets were used to improve coverage

across age groups. One set targeted younger respondents and another targeted older respondents. The goal was not to construct a representative sample through quotas, but to reduce the risk that ad delivery would be concentrated too heavily in only one part of the electorate.

B3. Branding and Incentives

To reduce the possibility of politically motivated self-selection tied to foreign institutional sponsorship, the advertisements did not foreground the researcher's home university. Instead, respondents were recruited through a locally branded research account, *Istanbul Social Sciences Research Center (ISBAM)*, created for the purposes of the study. This choice was motivated by concern that explicit association with a foreign academic institution might alter participation patterns in a politically polarized environment.

The advertisements invited users to participate in a survey and informed them that survey completion would qualify them for a raffle involving digital discount coupons. The incentive was designed to encourage survey completion while keeping the recruitment message simple and credible. Respondents were told that they would have a chance to receive either 100 TL or 200 TL in digital discount coupons. In the ad materials, familiar retail logos were included to make the reward structure legible and concrete.

B4. Advertisement Materials

The main recruitment message asked whether users wanted to participate in the survey and informed them that those who completed it would have a chance to win discount coupons. The tone of the advertisements was intentionally straightforward. Rather than emphasizing the article's topic directly, the recruitment materials framed participation in generic survey terms and avoided language that would reveal the treatment content or the paper's broader theoretical interest in electoral manipulation.

Figure 1 reproduces the main advertisement materials used during recruitment.

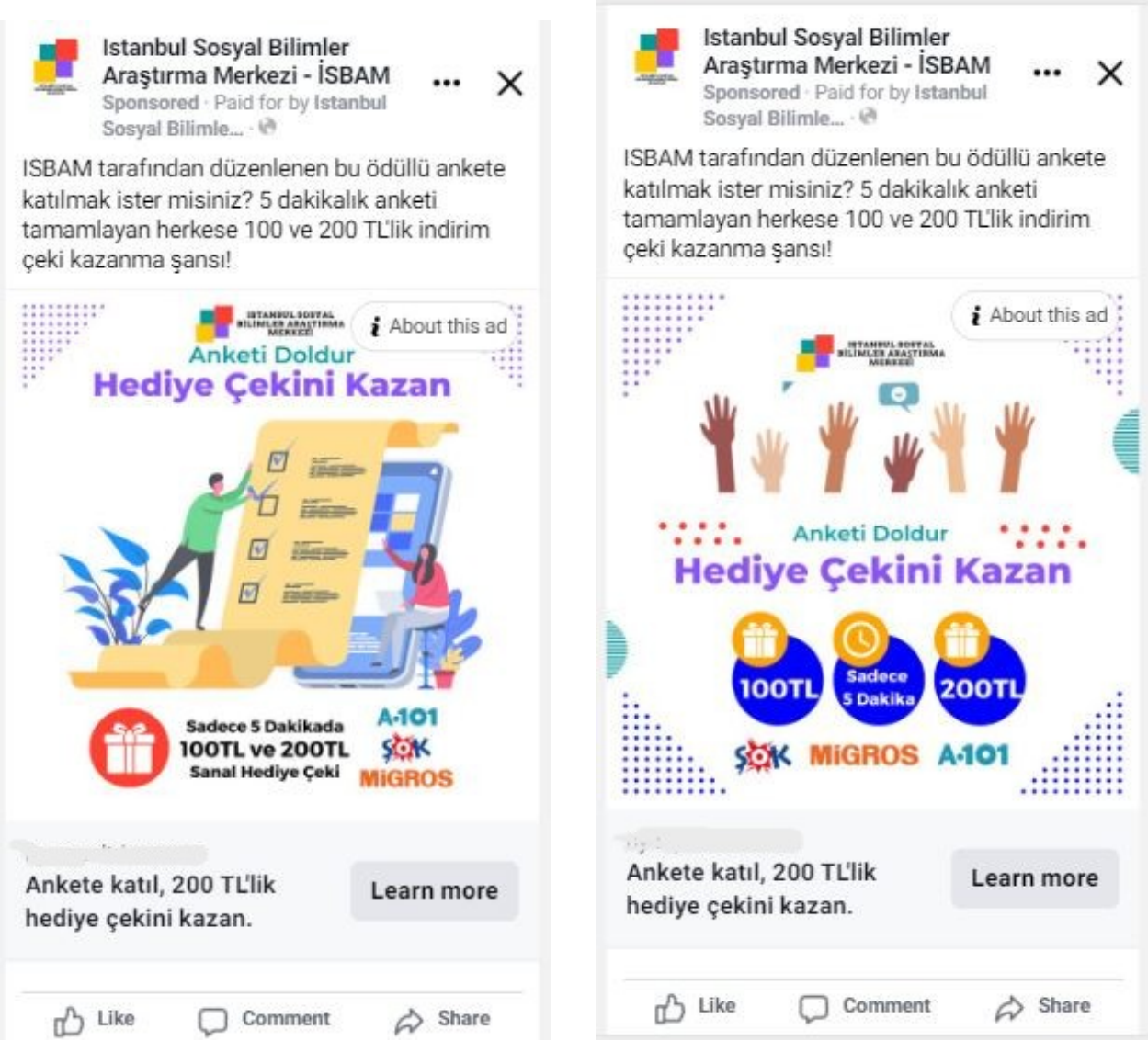

Figure 1: Recruitment Advertisement Materials.

B5. Additional Implementation Notes

After clicking on the advertisements, respondents were directed to the Qualtrics survey. They first encountered an informed-consent page and then completed a pretreatment questionnaire before random assignment to one of the vignette conditions. The survey used partial disclosure at the recruitment and consent stages so that respondents would not infer the article's specific interest in reactions to electoral manipulation before treatment exposure.

Because the sample was recruited online through Meta rather than drawn from a probability frame, it should be understood as a convenience sample rather than a representative national sample. The value of the design lies in randomized treatment assignment and in the ability to estimate differences across experimental

conditions within the recruited sample. Additional information on exclusions, balance, and comparison to population benchmarks appears in Online Appendix D.

Appendix C. Treatment Materials and Operationalization

This appendix provides the full treatment materials and additional discussion of operationalization. The main text summarizes the experimental contrast in concise form. This appendix gives the fuller design logic, reproduces the vignette texts, and explains the rationale for the procedural control condition.

C1. Design Logic

The experiment is designed to isolate whether citizens respond differently to substantively similar electoral interventions depending on how those interventions are enacted. In all treatment arms, respondents read a short hypothetical news article describing a change in electoral districts that would advantage the incumbent by increasing its expected parliamentary seat share. The crucial difference across conditions is not the substantive electoral consequence, but the procedure through which that consequence is pursued.

The *institutional* condition presents the electoral change as an ordinary legislative proposal requiring parliamentary approval. The *extra-institutional* condition presents the same electoral advantage as a decree-law that would be implemented unilaterally without parliamentary authorization. The *control* condition keeps the topic of electoral reform constant but frames the change as part of a broader European Union harmonization process. This structure makes it possible to compare reactions to different modes of electoral intervention while holding the basic electoral consequence fixed.

The article therefore focuses on a procedural mode-of-enactment distinction. It does not attempt to capture every possible form of institutional or extra-institutional manipulation. Rather, it isolates one especially important contrast: whether electorally consequential change is pursued through ordinary legislative approval or through unilateral extraordinary authority.

C2. Full Vignette Texts

Table 2 below reproduces the treatment texts shown to respondents.

C3. Treatment Presentation

The wording was designed so that all three conditions would remain close in structure, topic, and substantive electoral consequence. Each vignette describes the same broad electoral change and the same expected seat gain for the incumbent. This common structure reduces the risk that respondents are simply reacting to a different policy outcome rather than to the procedural mode through which that outcome is pursued.

The treatment texts were also written in the style of a short news article rather than as an overtly argumentative prompt. This was intended to make the intervention feel politically plausible while avoiding

Table 2: Treatment Materials

Treatment Groups		
Institutional Electoral Intervention	**Extra-Institutional Electoral Intervention**	**Procedural Control**
Attention: Please read the hypothetical short news article below carefully. After you finish, you will be asked several questions about the text.		
"Recently, the government has prepared a new **legislative proposal** for a law change regarding the electoral system. **If this proposal is approved by the parliamentary majority, it will authorize the government to change the laws. If the parliamentary majority supports this proposal,** there will be significant changes in the electoral districts. According to this change, some existing constituencies will be merged, and new electoral districts will be created. Experts argue that with this new regional system, the government can gain 15 more seats in the parliament. Therefore, experts suggest that this change could significantly affect the upcoming general elections."	"Recently, the government has prepared a new **extraordinary decree-law proposal** for a law change regarding the electoral system. **If this decree-law proposal is carried out under extraordinary powers, it will empower the government to change the laws unilaterally. If the government unilaterally implements this decree without the majority of the parliament,** there will be significant changes in the electoral districts. According to this change, some existing constituencies will be merged, and new electoral districts will be created. Experts argue that with this new regional system, the government can gain 15 more seats in the parliament. Therefore, experts suggest that this change could significantly affect the upcoming general elections."	"Recently, the government has prepared a new **legislative proposal** for a law change regarding the electoral system. **This change is one of the prerequisites of the European Union harmonization process and will positively affect Turkey's accession to the European Union. If the parliamentary majority supports this proposal,** there will be significant changes in the electoral districts. According to this change, some existing constituencies will be merged, and new electoral districts will be created. Experts argue that with this new regional system, the government can gain 15 more seats in the parliament. Therefore, experts suggest that this change could significantly affect the upcoming general elections."
Attention: Please read the hypothetical short news article above carefully. After you finish, you will be asked several questions about the text.		

explicit language that would reveal the article's hypotheses to respondents. The same pre-treatment and post-treatment attention language surrounded each vignette to standardize presentation as much as possible across conditions.

C4. Notes on the Procedural Control Condition

The control condition is not a neutral placebo. Instead, it is a procedural baseline that preserves the topic of electoral reform while reducing the degree to which the reform appears to be a direct incumbent power grab. This choice reflects a tradeoff in the design. A purely apolitical placebo would have produced a cleaner contrast between politics and non-politics, but it would not have held constant the article's substantive focus on electoral-rule change. The control condition was therefore designed to remain politically relevant while appearing more procedural and less overtly power-seeking than the two treatment conditions.

The European Union harmonization frame was chosen because it offered a recognizable procedural justification for reform while remaining distinct from the extra-institutional and institutional power-seeking frames. In the Turkish context, EU-related reform language has often been understood as connected to procedural and institutional adjustment rather than to a direct partisan attempt to secure electoral advantage (Aydın-Düzgit 2018). The goal was not to create a politically empty baseline, but to create a baseline in which the same electoral topic could be presented in less confrontational and less obviously unilateral terms.

For that reason, the control condition should be interpreted cautiously. The experiment compares reactions to different modes of electoral intervention, not to politics versus no politics. This is why the article treats

the contrast between extra-institutional manipulation and the procedural control condition as the primary comparison, while remaining more cautious about broader claims.

C5. Additional Notes on Treatment Validity

The treatment operationalization follows the logic that the conceptual distinction of interest lies in the locus and procedure of enactment. Institutional intervention is presented as reform pursued through authorized lawmaking, whereas extra-institutional intervention is presented as reform pursued through unilateral extraordinary authority. The design does not claim that all real-world institutional and extra-institutional manipulations are reducible to this contrast. Instead, it uses a stylized but substantively plausible vignette to isolate a procedurally meaningful distinction that is central to the article's argument.

This focus on procedural mode of enactment also helps explain the limits of the design. The experiment does not separately identify whether respondents react to legality, unilateralism, perceived threat, or broader symbolic associations carried by decree-law language. Those possible interpretations are analytically related, and the article therefore treats the treatment as identifying a bundled procedural cue rather than a single purified dimension.

Appendix D. Sample Construction, Balance, and Comparison

This appendix provides additional detail on the construction of the analytic sample, balance across treatment arms, and comparison to population benchmarks. The main text reports only the information necessary to understand the composition of the sample and the logic of the design.

D1. Recruitment, Exclusions, and Final Analytic Sample

Meta advertisements reached 78,500 users, and 1,151 respondents completed the survey. After removing duplicate entries, incomplete submissions, and responses in the top and bottom 1% of the survey-duration distribution, the final analytic sample includes 1,099 participants (please see Table 3, below). Additional details on sample construction, balance, and comparison to population benchmarks appear in the subsequent subsections.

These exclusions were applied to improve data quality while preserving the experimental comparison across conditions. Because treatment assignment occurred within the survey, the core inferential leverage of the design comes from randomization within the recruited sample rather than from any claim of national representativeness.

Table 3: Survey Flow.

Subsamples	Observation_Number
Raw import	1222
After date restriction	1198
After consent == 1	1170
After non-missing consent filter	1151
After Status == 0 (completed / non-preview)	1151
After non-missing incumbent/opposition coding	1139
Unique IP only with No Extreme Duration (Bottom & Top 1%)	1139
Keep first response among repeated IPs	1099
Pass at least one attention check + unique IP only + No Extreme Duration	976
Pass at least one attention check + keep first repeated IP + No Extreme Duration	989
Pass both attention checks + unique IP only + No Extreme Duration	601
Pass both attention checks + keep first repeated IP + No Extreme Duration	607

Notes: Source: Author's original survey experiment.

D2. Summary Statistics

Table 4 reports descriptive statistics for the final analytic sample. The sample is broadly balanced on basic demographic characteristics and includes substantial variation in political affiliation, prior voting behavior, and minority status. As in many Meta-recruited convenience samples, the article does not treat the sample as nationally representative. Instead, the value of the design lies in estimating treatment differences within the recruited respondent pool.

Table 4: Summary Statistics for the Final Analytic Sample.

	Mean	Std.Dev.	Min	Max
Female	0.53	0.50	0	1
College graduate	0.44	0.50	0	1
Age 18–34	0.47	0.50	0	1
Age 35–54	0.40	0.49	0	1
Age 55+	0.13	0.33	0	1
Unemployed	0.17	0.38	0	1
SES (1–4)	1.88	0.66	1	4
Turkish ethnicity	0.85	0.36	0	1
Minority	0.31	0.46	0	1
Political interest (1–4)	2.54	0.98	1	4
Previously voted	0.79	0.40	0	1
Incumbent vote (2018)	0.39	0.49	0	1
Economically dissatisfied	0.86	0.35	0	1
Democratic support index	3.24	0.72	1	4
Extra-institutional group	0.32	0.47	0	1
Institutional group	0.34	0.47	0	1
Control group	0.34	0.47	0	1

Notes: Source: Author's original survey experiment.

D3. Balance Across Treatment Arms

Table 5 reports balance across treatment conditions on key pretreatment characteristics. The treatment groups are similar on the main demographic and political covariates used in the study, including gender, education, age, ethnicity, minority status, prior voting behavior, and government support. This pattern is consistent with successful random assignment.

Table 5: Balance Across Treatment Arms.

	All.sample	Control	Institutional	Extra.institutional	Max.diff.	Omnibus.p
N	1099.00	377.00	369.00	353.00	NA	NA
Female	0.53	0.54	0.53	0.53	0.01	0.893
College graduate	0.44	0.42	0.44	0.46	0.04	0.600
Age 18–34	0.47	0.49	0.45	0.46	0.04	0.584
Age 35–54	0.40	0.38	0.42	0.41	0.04	0.584
Age 55+	0.13	0.12	0.13	0.12	0.01	0.584
Unemployed	0.17	0.16	0.19	0.17	0.03	0.559
SES (1–4)	1.88	1.88	1.90	1.85	0.05	0.539
Turkish ethnicity	0.85	0.84	0.86	0.86	0.02	0.698
Minority	0.31	0.31	0.30	0.34	0.04	0.519
Political interest (1–4)	2.54	2.52	2.57	2.54	0.05	0.722
Previously voted	0.79	0.82	0.79	0.78	0.04	0.310
Incumbent vote (2018)	0.39	0.38	0.41	0.40	0.03	0.676
Economically dissatisfied	0.86	0.85	0.85	0.88	0.03	0.322
Democratic support index	3.24	3.27	3.23	3.23	0.04	0.706

Notes: Source: Author's original survey experiment.

D4. Comparison to Population Benchmarks

Table 6 compares selected characteristics of the convenience sample to available population benchmarks. This comparison is included for descriptive transparency rather than to claim representativeness. As expected for an online Meta-recruited sample, the respondent pool differs from the national population on some margins. These differences do not undermine the experimental design, since the article's causal claims rely on randomized treatment assignment within the recruited sample rather than on descriptive population inference.

Table 6: Comparison of the Sample to Population Benchmarks.

	Sample Proportion	Population Proportion
Woman, 18-34	26.39	17.66
Woman, 35-54	22.47	18.31
Woman, +55	4.55	14.38
Man, 18-34	20.56	18.36
Man, 35-54	17.93	18.58
Man, +55	8.10	12.70

Notes: Source: Address Based Population Registration System, 2021.

D5. Implementation Checks

Table 7 reports implementation checks by treatment arm. These checks are distinct from pretreatment balance because they capture features of survey completion after treatment assignment, including time spent in the survey and attention-check performance. I report them separately to assess whether any condition imposed meaningfully greater cognitive burden or produced systematically different rates of attentive completion.

Table 7: Implementation Checks by Treatment Arm.

Group_label	N	Mean Duration	Median Duration	SD Duration	Seat Attention Pass (%)	Proposal Attention Pass (%)	Passed Both Checks (%)
All Sample	1099	466.2	416	221.3	82.2	61.3	54.7
Control	377	460.9	416	194.3	81.4	60.7	54.6
Extra-Institutional	353	465.0	429	205.8	80.7	60.3	53.8
Institutional	369	472.7	411	258.6	84.3	62.9	55.6

Notes: Duration is measured in seconds. Attention-check rates are reported within the final analytic sample. Source: Author's original survey experiment.

To evaluate these differences formally, I conduct omnibus F-tests comparing treatment arms on survey duration and attention-check outcomes. Across all implementation measures, the resulting p-values exceed conventional significance thresholds, indicating no statistically significant differences between the control, institutional, and extra-institutional conditions (please see Table 8). This pattern suggests that the treatments did not differentially affect completion time or basic attentiveness in ways that would complicate interpretation of the main results.

Table 8: Omnibus Tests for Implementation Checks.

Metric	Omnibus p-value
Duration	0.763
Seat Attention	0.416
Proposal Attention	0.752
Passed Both Checks	0.897

Notes: Source: Author's original survey experiment.

Appendix E. Measures and Coding

This appendix documents the article's measurement strategy in greater detail. The main text focuses on the principal donation outcome and summarizes the role of secondary outcomes. This appendix provides fuller information on question wording, derived variables, attention checks, and coding decisions used in the study. The full survey instrument is reproduced at the end of the Online Appendix.

E1. Main Outcome

The article's principal dependent variable is the amount allocated to the independent election-monitoring organization in the post-treatment donation task. Respondents were asked to allocate a hypothetical 500 Turkish Lira across four nongovernmental organizations. The amount allocated to the election-monitoring organization serves as the main outcome because it captures support for accountability-oriented action without relying exclusively on direct self-reports of political participation.

The donation task also generated three additional allocation variables for organizations working on violence against women, protection of stray animals, and poverty. These outcomes are not central to the article's main claim, but they are useful for assessing whether treatment effects are specific to election-monitoring action rather than reflecting a general shift in allocation behavior.

E2. Secondary Outcomes

The article also measures several secondary outcomes. These include self-reported willingness to engage in different political actions, post-treatment evaluations of the proposed electoral reform, and self-reported emotional reactions to the vignette. The self-reported political participation items are treated as secondary because they are more vulnerable to overstatement and social desirability bias than the donation outcome. The post-treatment attitudinal and emotional measures are used primarily for exploratory analysis of possible pathways rather than as principal outcomes.

E3. Attention and Manipulation Checks

Two post-treatment attention checks were included to assess whether respondents retained the basic content of the vignette. One item asked respondents what type of proposal had been described, and the other asked how many additional parliamentary seats experts said the government could gain under the proposed reform. These items are useful for assessing respondent attentiveness, but because they are downstream of treatment exposure, the main specifications do not define the principal analytic sample by conditioning on passing them.

The survey also included post-treatment evaluative items on policy approval and perceived democratic

consistency. These are reported as exploratory measures rather than as part of the article's main identification strategy.

E4. Derived Variables and Coding

Several pretreatment and post-treatment variables were recoded for analysis. The most important derived variables are the treatment indicators, the government-support indicator, the age-category variables, the minority-status indicator, and the binary versions of the donation and self-reported participation outcomes. Table 9 summarizes the principal derived variables used in the article. The full wording of the pretreatment items, post-treatment items, and donation task appears in the Full Survey Instrument at the end of the Online Appendix.

Table 9: Principal Derived Variables.

Variable	Definition
T_Parli	Indicator equal to 1 for the institutional treatment condition and 0 otherwise.
T_Decre	Indicator equal to 1 for the extra-institutional treatment condition and 0 otherwise.
dem_Incumbent	Indicator equal to 1 for respondents aligned with the incumbent coalition before treatment and 0 otherwise.
dem_Age	Three-category age variable recoded from pretreatment age responses.
dem_Age1	Indicator for age 18–34.
dem_Age2	Indicator for age 35–54.
dem_Age3	Indicator for age 55+.
dem_Fem	Indicator equal to 1 for female respondents.
dem_Grad	Indicator equal to 1 for college or higher education.
dem_minority	Indicator equal to 1 for respondents who are not both ethnically Turkish and Sunni Muslim.
dem_EconPerf	Indicator equal to 1 for respondents dissatisfied with the current economic situation.
Don_Monit_Dum	Indicator equal to 1 if the respondent allocated any positive amount to the election-monitoring organization.
PolPart_Prot_Dum	Binary recoding of protest participation intention.
Policy_Approval_Dum	Binary recoding of post-treatment policy approval.
Policy_Consistency_Dum	Binary recoding of post-treatment perceived democratic consistency.

Notes: Source: Author's original survey experiment.

Appendix F. Estimation Details and Additional Robustness

This appendix reports additional estimation details and robustness analyses for the revised article. The principal dependent variable is the amount allocated to the independent election-monitoring organization in the donation task. The main text presents the core treatment contrasts for that outcome and summarizes the broad pattern of secondary results. This appendix provides fuller information on model specification, self-reported political behavior outcomes, full regression tables, randomization inference, covariate-adjusted models, and alternative outcome specifications.

The principal specifications are estimated on the duplicate-handled analytic sample described in Online Appendix D. Because the post-treatment attention items are themselves downstream of treatment exposure, the main specifications do not condition sample inclusion on passing those items. All principal estimates are reported using conventional two-sided inference and heteroskedasticity-robust standard errors. The primary inferential focus remains the contrast between the extra-institutional treatment and the procedural control condition, with the contrast between extra-institutional and institutional treatment treated as secondary. Because the self-reported participation outcomes and exploratory post-treatment measures are reported for completeness and interpretation rather than as a single family of confirmatory tests, I do not apply multiple-comparison corrections to these analyses. Isolated significant coefficients should therefore be interpreted cautiously and in light of the broader pattern across outcomes.

F1. Main Estimating Strategy

For the continuous donation outcome, the principal specifications use ordinary least squares models in which the outcome is regressed on indicators for the institutional and extra-institutional treatment conditions, an indicator for government support measured before treatment, and the corresponding treatment-by-partisanship interactions. This pooled specification allows partisan heterogeneity to be estimated directly within a single model rather than through separate regressions for partisan subsamples.

The principal pooled specification takes the form

$$Y_i = \alpha + \beta_1 T_i^{Inst} + \beta_2 T_i^{Extra} + \gamma Inc_i + \delta_1 (T_i^{Inst} \times Inc_i) + \delta_2 (T_i^{Extra} \times Inc_i) + \varepsilon_i,$$

where Y_i is the outcome of interest, T_i^{Inst} indicates assignment to the institutional treatment, T_i^{Extra} indicates assignment to the extra-institutional treatment, and Inc_i indicates government alignment measured before treatment. This specification makes it possible to recover both the average treatment contrasts and their partisan heterogeneity within a common framework.

I report two versions of the principal specification. The first is an unadjusted model that includes

only treatment indicators, government support, and treatment-by-partisanship interactions. The second adds pretreatment covariates, including age, gender, education, minority status, prior voting, economic dissatisfaction, and political interest. These covariate-adjusted models are included to assess precision and robustness rather than to change identification, which comes from random assignment.

All principal estimates are reported with heteroskedasticity-robust standard errors. For the coefficient plots reported in the appendix, each outcome is standardized using the full-sample distribution of that outcome so that estimates remain directly comparable across displayed subgroups and treatment contrasts.

The appendix also presents coefficient plots separately for the full sample, opposition supporters, and government supporters. These figures are intended as transparent descriptive summaries of the treatment pattern across subsamples. The article's main inferential claims, however, rely on the pooled interaction specifications rather than on visual comparison of separately estimated subgroup regressions alone.

For the comparison between extra-institutional and institutional intervention, I estimate models on the subset of respondents assigned to those two treatment arms. This contrast is treated as secondary throughout the article, since both treatment arms describe electorally advantageous interventions by the incumbent, and the article's principal focus remains the comparison between extra-institutional intervention and the procedural control condition.

F2. Self-Reported Political Behavior Outcomes

The article also collected a set of self-reported political behavior outcomes, including willingness to contact politicians, cast a protest vote, switch vote choice, incur costs in order to vote, join protests, and volunteer as an election observer. These outcomes are substantively relevant, but they are treated as secondary because they rely on self-reports and are therefore more vulnerable than the donation task to overstatement, expressive responding, and social desirability bias.

I report these models as supplementary evidence rather than as co-equal tests of the article's main claim. Their purpose is to assess whether the broader pattern of responses is directionally consistent with the main allocation result, not to replace the donation outcome as the article's principal evidentiary basis.

Figure 2 summarizes the treatment pattern across these self-reported outcomes. Overall, the estimates are generally smaller and less consistent than for the election-monitoring donation measure. Most 95% confidence intervals include zero, and the self-reported outcomes do not reveal a clear or uniform pattern of increased oppositional action under extra-institutional intervention. A few coefficients are suggestive in direction, but the broader picture is one of substantial imprecision and limited consistency across measures. For that reason, these outcomes are interpreted cautiously and are reported primarily for completeness and transparency. Full regression tables for these outcomes appear in Appendix F3.

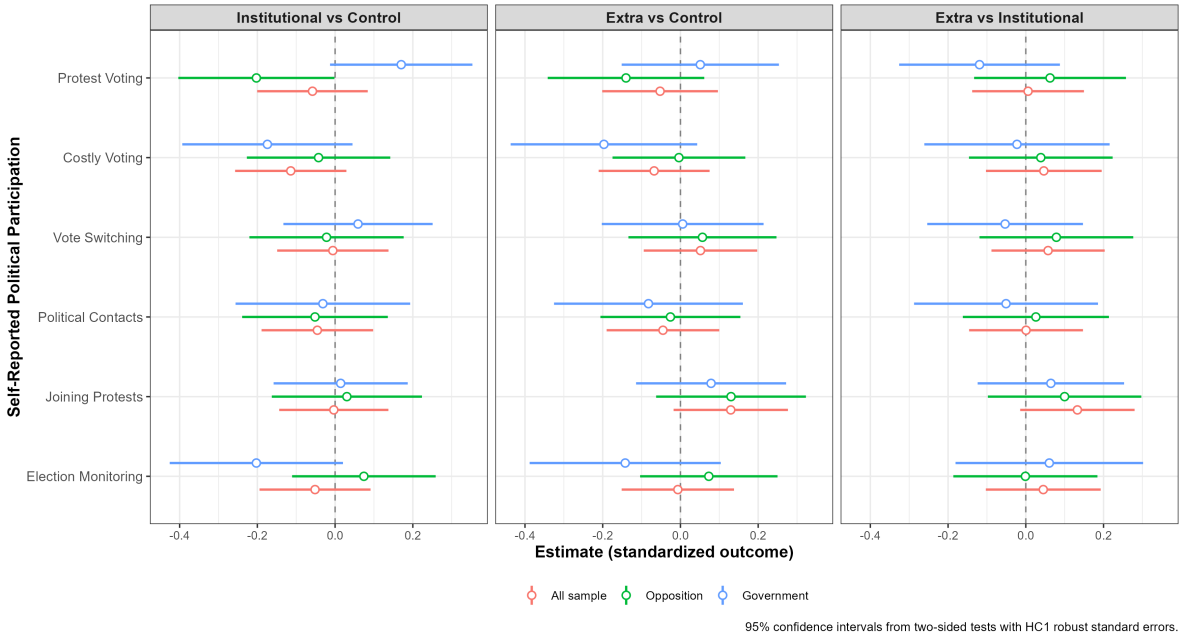

Figure 2: Self-reported Political Behavior Outcomes.

F3. Full Regression Tables

This section reports the full regression tables corresponding to the specifications discussed in the main text and in the preceding appendix sections. These tables are included for completeness and transparency. They provide the full coefficient estimates, robust standard errors, and sample sizes underlying the principal donation models and the supplementary self-reported political behavior analyses.

The tables in this section are organized to mirror the revised empirical structure of the article. The principal donation outcome remains the main allocation test, while the self-reported political behavior outcomes are reported as secondary and interpreted more cautiously. Because the donation task requires respondents to allocate a fixed total amount across categories, the resulting donation outcomes should be understood as jointly determined shares of a common allocation decision rather than as fully separable outcomes. Alternative functional forms and additional robustness checks are reported separately in later sections of this appendix rather than being folded into the main regression-table presentation here.

All tables in this section use the revised inferential conventions adopted throughout the manuscript: heteroskedasticity-robust standard errors, conventional two-sided inference, and no multiple-testing adjustment.

Table 10: Donation Allocation Outcomes, Unadjusted Pooled Interaction OLS Models.

term_label	Election Monitoring	Violence Against Women	Stray Animals	Poverty
Constant	-0.036 (0.066)	0.005 (0.065)	0.115 (0.073)	-0.075 (0.069)
Extra-institutional	0.311*** (0.105)	-0.027 (0.095)	-0.162 (0.100)	-0.023 (0.094)
Institutional	0.183* (0.107)	0.145 (0.097)	-0.094 (0.101)	-0.143 (0.094)
Government supporter	-0.079 (0.088)	-0.187** (0.088)	-0.174* (0.100)	0.330*** (0.103)
Institutional x Government	-0.320** (0.132)	0.049 (0.142)	0.071 (0.143)	0.084 (0.146)
Extra-institutional x Government	-0.375*** (0.139)	0.198 (0.146)	0.244 (0.151)	-0.140 (0.153)
N	1,099	1,099	1,099	1,099
R-squared	0.035	0.009	0.005	0.028

Table 11: Donation Allocation Outcomes, Covariate-Adjusted Pooled Interaction OLS Models.

term_label	Election Monitoring	Violence Against Women	Stray Animals	Poverty
Constant	-0.111 (0.123)	-0.218 (0.150)	0.157 (0.154)	0.110 (0.141)
Extra-institutional	0.304*** (0.102)	-0.015 (0.094)	-0.155 (0.100)	-0.035 (0.093)
Institutional	0.174 (0.106)	0.160* (0.096)	-0.087 (0.101)	-0.154* (0.091)
Government supporter	-0.015 (0.092)	-0.182* (0.094)	-0.178* (0.107)	0.293*** (0.106)
Age 35-54	-0.019 (0.061)	-0.008 (0.069)	-0.154** (0.069)	0.139* (0.071)
Age 55+	0.307** (0.127)	-0.037 (0.109)	0.039 (0.121)	-0.174* (0.104)
Minority	0.027 (0.069)	0.063 (0.064)	0.056 (0.068)	-0.110* (0.065)
Previously voted	-0.016 (0.078)	0.032 (0.084)	0.017 (0.082)	-0.029 (0.084)
Economic dissatisfaction	0.010 (0.067)	0.029 (0.090)	-0.095 (0.094)	0.048 (0.091)
Political interest	0.098 (0.060)	-0.022 (0.064)	-0.028 (0.063)	-0.016 (0.063)
Institutional x Government	-0.313** (0.132)	0.034 (0.139)	0.070 (0.142)	0.092 (0.142)
Extra-institutional x Government	-0.365*** (0.139)	0.186 (0.143)	0.241 (0.151)	-0.136 (0.151)
N	1,099	1,099	1,099	1,099
R-squared	0.057	0.050	0.016	0.062

Table 12: Self-Reported Political Outcomes, Unadjusted Pooled Interaction OLS Models.

term_label	Protest Voting	Costly Voting	Vote Switching	Political Contacts	Joining Protests	Election Monitoring
Constant	0.256*** (0.075)	0.185*** (0.062)	0.182*** (0.069)	0.059 (0.065)	0.220*** (0.068)	0.039 (0.064)
Extra-institutional	-0.140 (0.103)	-0.004 (0.087)	0.057 (0.097)	-0.026 (0.092)	0.130 (0.098)	0.073 (0.090)
Institutional	-0.202** (0.102)	-0.043 (0.094)	-0.022 (0.101)	-0.052 (0.096)	0.030 (0.099)	0.074 (0.094)
Government supporter	-0.540*** (0.098)	-0.307*** (0.101)	-0.484*** (0.100)	-0.072 (0.106)	-0.641*** (0.093)	-0.048 (0.104)
Institutional x Government	0.373*** (0.139)	-0.132 (0.146)	0.081 (0.141)	0.020 (0.149)	-0.016 (0.132)	-0.277* (0.148)
Extra-institutional x Government	0.191 (0.146)	-0.193 (0.150)	-0.051 (0.144)	-0.057 (0.154)	-0.051 (0.139)	-0.215 (0.154)
N	1,099	1,099	1,099	1,099	1,099	1,099
R-squared	0.036	0.045	0.055	0.002	0.110	0.015

Table 13: Self-Reported Political Outcomes, Covariate-Adjusted Pooled Interaction OLS Models.

term_label	Protest Voting	Costly Voting	Vote Switching	Political Contacts	Joining Protests	Election Monitoring
Constant	0.424*** (0.143)	-0.001 (0.142)	0.069 (0.132)	0.275* (0.147)	-0.070 (0.131)	-0.130 (0.142)
Extra-institutional	-0.150 (0.102)	-0.015 (0.083)	0.039 (0.097)	-0.021 (0.092)	0.117 (0.095)	0.080 (0.087)
Institutional	-0.212** (0.102)	-0.054 (0.091)	-0.028 (0.101)	-0.048 (0.096)	0.023 (0.093)	0.082 (0.091)
Government supporter	-0.582*** (0.106)	-0.264** (0.104)	-0.413*** (0.105)	-0.166 (0.108)	-0.526*** (0.097)	-0.099 (0.106)
Age 35-54	-0.024 (0.069)	0.075 (0.070)	-0.081 (0.068)	-0.098 (0.070)	-0.040 (0.064)	0.013 (0.068)
Age 55+	-0.172 (0.110)	0.354*** (0.094)	-0.184* (0.111)	-0.108 (0.107)	0.357*** (0.101)	0.138 (0.099)
Minority	-0.186*** (0.066)	0.061 (0.063)	-0.079 (0.066)	-0.034 (0.066)	0.061 (0.061)	-0.019 (0.066)
Previously voted	-0.209** (0.081)	0.033 (0.082)	-0.188** (0.079)	0.063 (0.085)	-0.007 (0.074)	0.321*** (0.081)
Economic dissatisfaction	-0.021 (0.083)	-0.170* (0.097)	0.278*** (0.081)	-0.350*** (0.098)	0.033 (0.083)	-0.329*** (0.095)
Political interest	0.158*** (0.061)	0.319*** (0.064)	0.074 (0.061)	0.248*** (0.064)	0.372*** (0.057)	0.366*** (0.064)
Institutional x Government	0.387*** (0.139)	-0.117 (0.143)	0.078 (0.140)	0.036 (0.147)	-0.010 (0.128)	-0.264* (0.141)
Extra-institutional x Government	0.211 (0.145)	-0.157 (0.148)	-0.049 (0.143)	-0.022 (0.151)	-0.020 (0.139)	-0.160 (0.150)
N	1,099	1,099	1,099	1,099	1,099	1,099
R-squared	0.056	0.096	0.082	0.032	0.174	0.090

Notes: Entries are OLS coefficients with HC1 robust standard errors in parentheses. Outcomes are standardized using the full-sample distribution of each outcome. Significance stars reflect conventional two-sided p-values: * p<0.10, ** p<0.05, *** p<0.01. No multiple-testing adjustment is applied.

F4. Randomization Inference

Because the donation outcome is bounded and somewhat skewed, I supplement the regression-based estimates with randomization inference. These analyses assess whether the main treatment contrasts remain unusual relative to the distribution of estimates obtained under repeated reassignment of treatment labels.

The randomization-inference exercise focuses on the article's principal donation outcome, namely the amount allocated to the independent election-monitoring organization. For each pairwise comparison, I repeatedly reassign treatment labels while holding fixed the observed treatment-group sizes within the relevant subsample. I report two-sided randomization-inference p-values based on 10,000 simulated reassignments.

Table 14 below reports the results for the main pairwise contrasts in the full sample and among opposition supporters. The randomization-inference results are consistent with the paper's main regression-based conclusions. The strongest evidence continues to concern the extra-institutional versus control contrast among opposition supporters, while the remaining contrasts are generally weaker and less precisely distinguished from the null.

Table 14: Randomization-inference results for the election-monitoring donation outcome.

Subsample	Contrast	Observed difference	Two-sided RI p-value	N	Treated	Control
All sample	Institutional vs Control	2.715	0.593	746	369	377
All sample	Extra-institutional vs Control	12.659	0.018	730	353	377
All sample	Extra-institutional vs Institutional	9.943	0.089	722	353	369
Opposition	Institutional vs Control	13.482	0.087	428	204	224
Opposition	Extra-institutional vs Control	22.887	0.002	444	220	224
Opposition	Extra-institutional vs Institutional	9.405	0.279	424	220	204

Notes: Entries report observed pairwise differences in mean donation to the independent election-monitoring organization and two-sided randomization-inference p-values based on 10,000 simulated reassignments. Within each comparison, the number of treated and control observations is held fixed at its observed value.

F5. Covariate-Adjusted Models

This section summarizes the effect of adding pretreatment covariates to the principal pooled interaction specification. Because the full adjusted regression tables are already reported in Appendix F3, I do not repeat the entire set of adjusted models here. Instead, I present a compact comparison of the principal donation specification with and without pretreatment covariates.

The purpose of this comparison is not to change identification, which comes from random assignment, but to assess whether the article's main conclusions depend heavily on adjustment. The covariates include pretreatment measures of age, gender, education, minority status, prior voting, economic dissatisfaction, and political interest.

Table 15 reports the main pooled interaction model for the election-monitoring donation outcome with and without covariate adjustment. The substantive pattern remains similar across the two specifications, indicating that the principal result does not depend on the inclusion of pretreatment controls.

Table 15: Principal donation model with and without covariate adjustment.

term_label	Unadjusted	Adjusted
Extra-institutional	0.311*** (0.105)	0.304*** (0.102)
Institutional	0.183* (0.107)	0.174 (0.106)
Government supporter	-0.079 (0.088)	-0.015 (0.092)
Institutional x Government	-0.320** (0.132)	-0.313** (0.132)
Extra-institutional x Government	-0.375*** (0.139)	-0.365*** (0.139)
N	1,099	1,099
R-squared	0.035	0.057
Pretreatment covariates included	No	Yes

Notes: Entries are OLS coefficients with HC1 robust standard errors in parentheses. The outcome is standardized using the full-sample distribution of donation to the independent election-monitoring organization. Significance stars reflect conventional two-sided p-values: * p<0.10, ** p<0.05, *** p<0.01. No multiple-testing adjustment is applied. Full adjusted regression tables for donation and self-reported outcomes appear in Appendix F3.

F6. Alternative Outcome Specifications

This section reports alternative specifications for outcomes analyzed in the article, including a binary version of the principal donation outcome and ordered-response models for the self-reported political behavior items. These models are useful for assessing whether the substantive pattern depends heavily on the linear probability or linear OLS form used elsewhere in the appendix.

The purpose of these models is not to replace the article's principal specification, but to verify that the main substantive conclusions do not hinge on a particular functional-form choice. In keeping with the broader pattern reported in the manuscript, the alternative specifications continue to suggest that the clearest and most stable treatment effect concerns support for election-monitoring resources, while the self-reported outcomes remain weaker and less consistently estimated.

Table 16 reports pooled interaction logit models for the binary donation outcome. Table 17 reports pooled interaction ordered-response models for the self-reported political behavior outcomes. Taken together, these alternative specifications do not materially alter the article's main interpretation.

Table 16: Binary donation outcome: pooled interaction logit models.

term_label	Unadjusted logit	Adjusted logit
Extra-institutional	0.559*** (0.197)	0.575*** (0.201)
Institutional	0.075 (0.195)	0.087 (0.197)
Government supporter	-0.017 (0.211)	-0.049 (0.228)
Institutional x Government	-0.231 (0.298)	-0.220 (0.301)
Extra-institutional x Government	-0.839*** (0.309)	-0.803** (0.316)
N	1,099	1,099
Pretreatment covariates included	No	Yes

Notes for Table: Entries are logit coefficients with robust standard errors in parentheses. The dependent variable equals 1 if the respondent allocates a positive amount to the independent election-monitoring organization and 0 otherwise. Significance stars reflect conventional two-sided p-values: * p<0.10, ** p<0.05, *** p<0.01. No multiple-testing adjustment is applied.

Table 17: Self-reported political outcomes: pooled interaction ordered-response models.

term_label	Protest Voting	Costly Voting	Vote Switching	Political Contacts	Joining Protests	Election Monitoring
Extra-institutional	-0.231 (0.178)	-0.001 (0.172)	0.104 (0.173)	-0.034 (0.170)	0.230 (0.172)	0.135 (0.168)
Institutional	-0.326* (0.181)	-0.010 (0.179)	-0.037 (0.178)	-0.110 (0.174)	0.056 (0.174)	0.158 (0.173)
Government supporter	-1.006*** (0.209)	-0.546*** (0.188)	-0.866*** (0.191)	-0.158 (0.190)	-1.351*** (0.209)	-0.064 (0.188)
Institutional x Government	0.735** (0.288)	-0.295 (0.268)	0.173 (0.267)	0.068 (0.270)	0.004 (0.291)	-0.531** (0.267)
Extra-institutional x Government	0.203 (0.306)	-0.346 (0.275)	-0.104 (0.275)	-0.146 (0.278)	-0.079 (0.300)	-0.382 (0.276)
N	1,099	1,099	1,099	1,099	1,099	1,099

Notes for Table: Entries are ordered-logit coefficients with robust standard errors in parentheses. Threshold parameters are omitted for readability. Significance stars reflect conventional two-sided p-values: * p<0.10, ** p<0.05, *** p<0.01. No multiple-testing adjustment is applied.

F7. Robustness to Alternative Partisan Coding

Partisan heterogeneity is central to the article's argument, and the main specification classifies respondents using a broad measure of coalition alignment based on the pretreatment affiliation items. In the baseline

coding, respondents are classified as government-aligned if they either directly identify with the ruling coalition or initially select "other" but subsequently indicate that they are closer to the ruling coalition. This approach preserves sample size and captures a broad notion of pre-treatment coalition alignment, but it may also include respondents whose partisan attachment is weaker or less direct.

To assess whether the main heterogeneous treatment pattern depends on this coding choice, I re-estimate the principal donation models using two narrower alternatives. The first alternative uses only respondents who directly identify with either the ruling or opposition coalition in the pretreatment affiliation item. The second uses reported closeness and retains only respondents who report relatively strong attachment to the ruling or opposition coalition. These alternative codings provide stricter definitions of partisan alignment at the cost of reduced sample size.

This check is intended to assess whether the main opposition-specific response to extra-institutional manipulation is robust to stricter definitions of coalition alignment rather than to introduce a new preferred specification. Because the narrower codings reduce the effective sample size, they are best understood as robustness checks on the pattern of heterogeneity rather than as precision-maximizing alternatives. The baseline coding remains the main specification in the article because it preserves statistical power and best matches the article's broader conception of partisan alignment prior to treatment exposure.

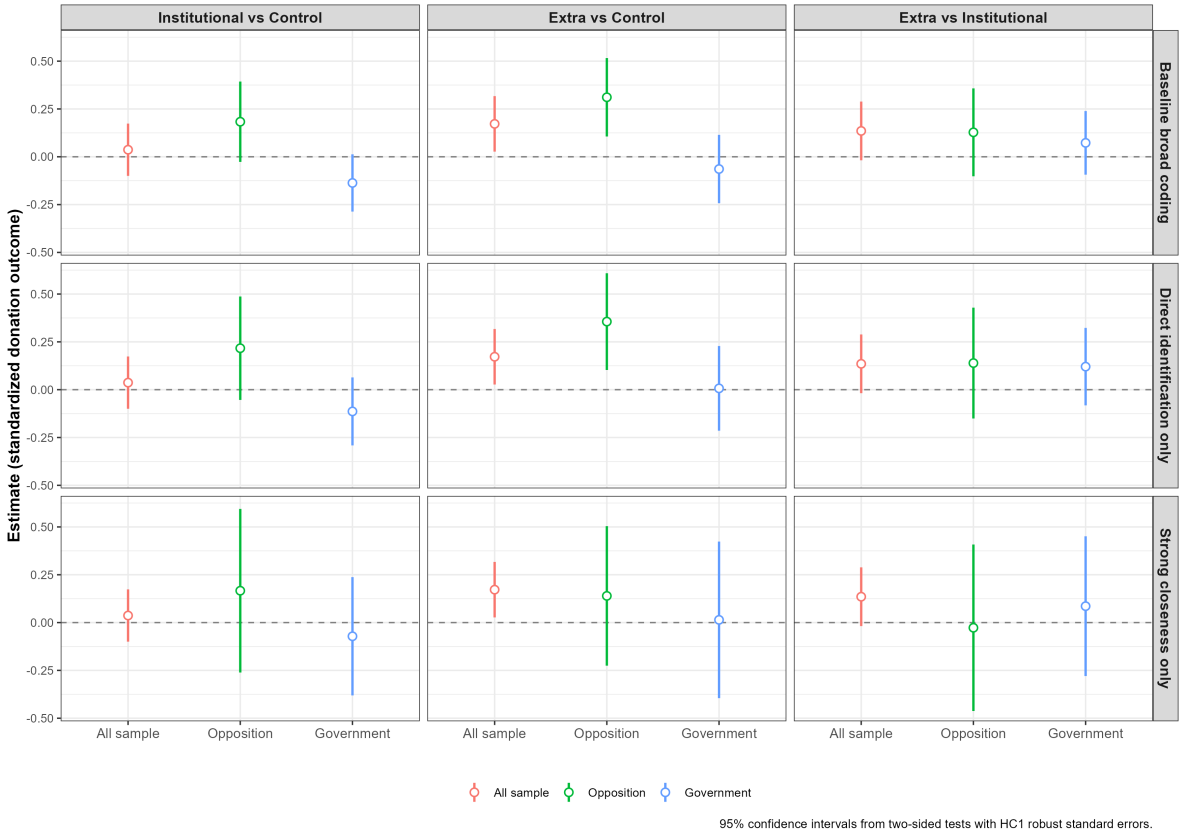

Figure 3: Robustness to alternative partisan coding.

F8. Robustness to Duration Trimming and Duration Outlier Rules

The final analytic sample used in the article excludes responses in the top and bottom 1 percent of the survey-duration distribution. This rule was adopted to reduce the influence of unusually fast or unusually slow completion times while preserving a transparent and symmetric trimming procedure. Because duration-based exclusions can sometimes affect treatment estimates, I assess whether the principal donation result depends heavily on this particular rule.

Table 18 compares the main pooled interaction specification for the election-monitoring donation outcome across alternative duration-based sample definitions. In addition to the article's main trimming rule, I consider specifications that exclude only the bottom 1 percent of durations, only the top 1 percent, and observations flagged as duration outliers under an interquartile-range rule, as well as a specification with no duration trimming. This comparison allows a direct assessment of whether the article's main treatment pattern is being driven by a particular choice about survey-duration exclusions.

In the revised manuscript, the symmetric top-and-bottom 1 percent trimming rule remains the main specification because it is simple, transparent, and does not single out either unusually short or unusually

long responses for special treatment. The estimates reported here provide a robustness check on that choice rather than a competing preferred design.

Table 18: Robustness of the principal donation model to alternative duration trimming and outlier rules.

rule	n	Extra-institutional	Institutional	Institutional x Government	Extra-institutional x Government
No trimming	1122	0.311*** (0.102)	0.205* (0.107)	-0.361*** (0.132)	-0.398*** (0.136)
Drop bottom 1% only	1111	0.310*** (0.103)	0.204* (0.108)	-0.344*** (0.132)	-0.377*** (0.136)
Drop top 1% only	1110	0.312*** (0.104)	0.185* (0.107)	-0.337** (0.132)	-0.397*** (0.139)
Drop top and bottom 1% (main rule)	1099	0.311*** (0.105)	0.183* (0.107)	-0.320** (0.132)	-0.375*** (0.139)
IQR outlier rule	1069	0.320*** (0.105)	0.193* (0.108)	-0.358*** (0.134)	-0.392*** (0.142)

Notes: Entries are OLS coefficients with HC1 robust standard errors in parentheses from the pooled interaction specification for the election-monitoring donation outcome. Within each sample definition, the outcome is standardized using that sample's distribution. Significance stars reflect conventional two-sided p-values: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. No multiple-testing adjustment is applied.

Appendix G. Exploratory Mechanisms

This appendix reports exploratory evidence on possible pathways linking treatment exposure to the article's main allocation result. These analyses are not intended to establish a definitive mechanism. Instead, they are presented to clarify what kinds of post-treatment responses are and are not consistent with the observed increase in support for election-monitoring resources among opposition supporters.

G1. Emotional Responses

One possible pathway is that extra-institutional manipulation generates stronger negative emotional reactions than institutional intervention or the procedural control condition. To assess this possibility, the survey measured several post-treatment emotions, including anger, disgust, frustration, worry, hope, happiness, excitement, and tranquility.

Figure 4 summarizes the treatment pattern across these emotional responses. The results are suggestive rather than decisive. Both treatment conditions appear to shift some post-treatment emotions relative to the procedural control condition, and in several cases the extra-institutional condition appears to generate somewhat stronger negative affect. At the same time, the estimates are not uniformly precise, and the emotional pattern does not map cleanly onto the article's main opposition-specific allocation result. For that reason, these measures are best interpreted as descriptive evidence consistent with, but not sufficient to establish, an emotional pathway.

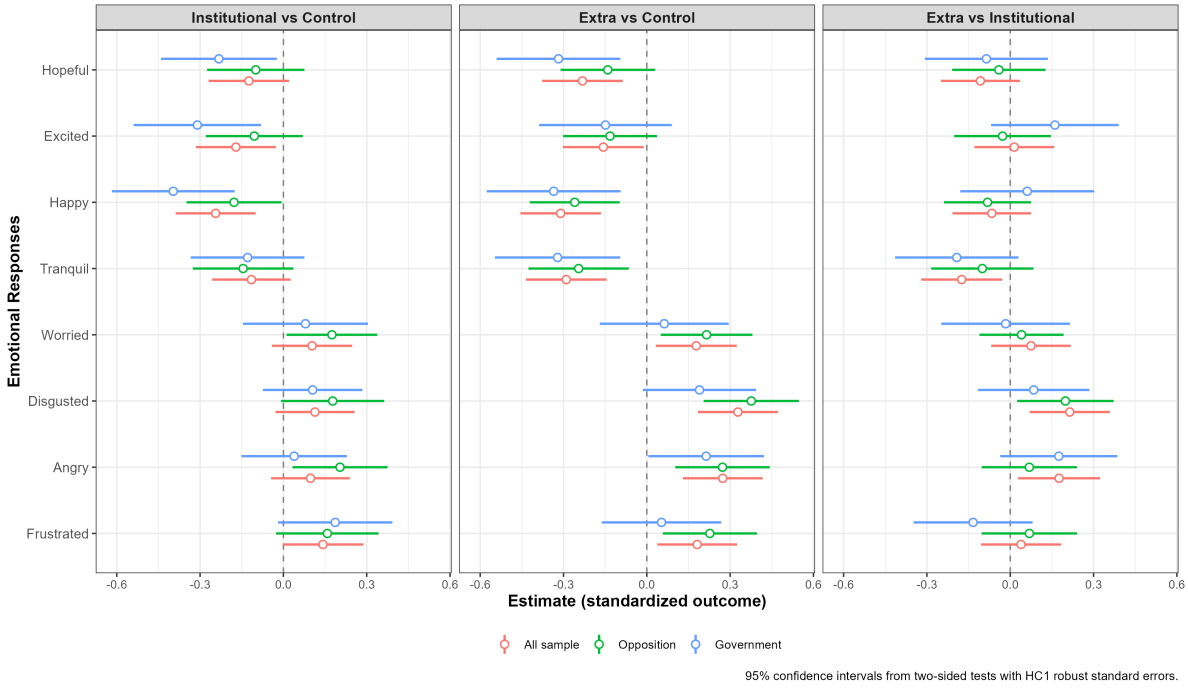

Figure 4: Exploratory treatment patterns for emotional responses.

G2. Policy Approval and Democratic Consistency

A second possible pathway concerns post-treatment evaluations of the proposed reform itself. The survey therefore measured both policy approval and perceived democratic consistency. These items provide a useful window into whether respondents treated the institutional and extra-institutional interventions as equally legitimate or instead interpreted the extra-institutional condition as more normatively troubling.

Figure 5 summarizes the treatment pattern for these post-treatment policy evaluations. The results are informative but limited. Extra-institutional intervention often appears less favorably evaluated than the procedural control condition, but these differences do not line up cleanly with the article's main opposition-specific increase in support for election-monitoring resources. As a result, the legitimacy-based interpretation remains plausible but not decisive.

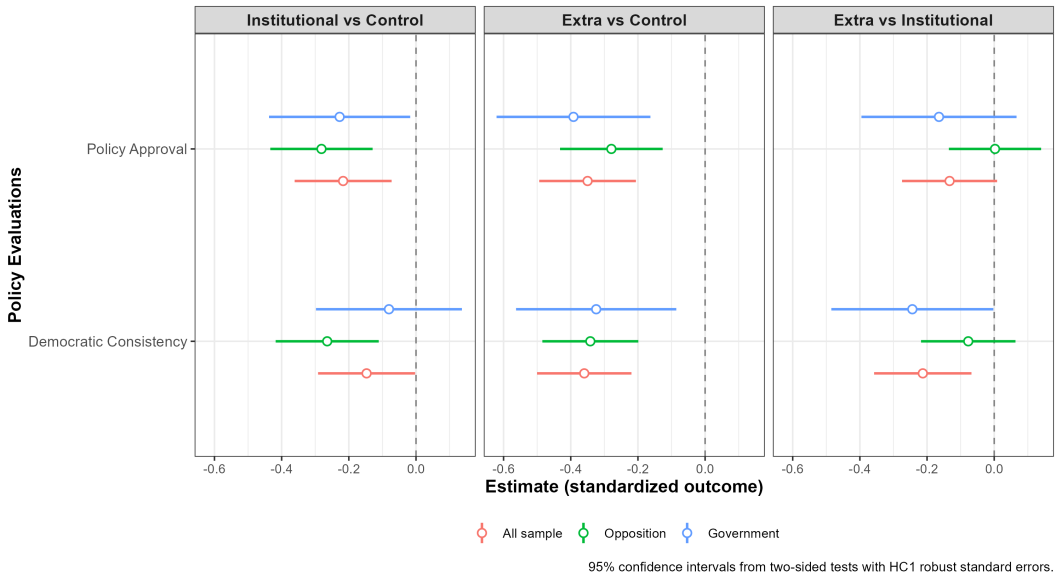

Figure 5: Exploratory treatment patterns for policy approval and democratic consistency.

G3. Compact Exploratory Regression Summaries

To complement the figures, Tables 19 and 20 report compact pooled interaction regression summaries for the policy-evaluation and emotional-response outcomes. These tables are included for transparency and interpretation rather than as co-equal tests of the article's main claim. Because all of these outcomes are measured after treatment exposure, they should be interpreted as exploratory post-treatment responses rather than identified mediators.

Table 19: Pooled interaction OLS models for policy approval and democratic consistency.

term_label	Policy Approval	Democratic Consistency
Extra-institutional	-0.279*** (0.078)	-0.342*** (0.073)
Institutional	-0.281*** (0.078)	-0.264*** (0.078)
Government supporter	0.975*** (0.096)	0.848*** (0.096)
Institutional x Government	0.054 (0.132)	0.184 (0.136)
Extra-institutional x Government	-0.113 (0.140)	0.017 (0.142)
N	1,099	1,099

Table 20: Pooled interaction OLS models for emotional responses.

term_label	Hopeful	Excited	Happy	Tranquil	Worried	Disgusted	Angry	Frustrated
Extra-institutional	-0.140 (0.087)	-0.132 (0.086)	-0.260*** (0.083)	-0.245*** (0.092)	0.215** (0.084)	0.376*** (0.088)	0.273*** (0.087)	0.227*** (0.087)
Institutional	-0.100 (0.089)	-0.105 (0.089)	-0.178** (0.087)	-0.145 (0.092)	0.175** (0.083)	0.177* (0.095)	0.204** (0.087)	0.158* (0.094)
Government supporter	0.838*** (0.099)	0.616*** (0.105)	0.765*** (0.102)	0.549*** (0.099)	-0.697*** (0.103)	-0.759*** (0.094)	-0.789*** (0.091)	-0.687*** (0.099)
Institutional x Government	-0.133 (0.139)	-0.205 (0.147)	-0.219 (0.143)	0.016 (0.139)	-0.095 (0.142)	-0.072 (0.132)	-0.165 (0.130)	0.028 (0.141)
Extra-institutional x Government	-0.178 (0.143)	-0.017 (0.149)	-0.076 (0.148)	-0.076 (0.147)	-0.152 (0.145)	-0.186 (0.136)	-0.059 (0.137)	-0.174 (0.140)
N	1,099	1,099	1,099	1,099	1,099	1,099	1,099	1,099

Notes: Entries are OLS coefficients with HC1 robust standard errors in parentheses. Each outcome is standardized using the full-sample distribution of that outcome. Significance stars reflect conventional two-sided p-values: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. No multiple-testing adjustment is applied. These outcomes are measured after treatment exposure and are interpreted as exploratory post-treatment responses rather than identified mediators.

Appendix H. Preregistration, Deviations, and Ethics

This appendix documents the study's preregistration, the main deviations from the original analysis plan, and the ethical and transparency information relevant to the project. The goal is to clarify how the revised article relates to the original design while distinguishing design-stage commitments from the narrower empirical focus of the present manuscript.

H1. Preregistration Overview

The survey experiment was preregistered prior to fielding. The preregistration described the treatment conditions, the main outcome families, the anticipated subgroup comparisons, and the inferential strategy planned at the design stage. The present article follows the core experimental design and treatment structure set out in that preregistration, while adopting a more focused presentation of outcomes and a more conventional inferential framework in the revised manuscript. Preregistration link omitted for blind review.

The study was fielded after institutional ethical approval and administered through an online survey platform with informed consent obtained before treatment exposure. IRB approval details omitted for blind review.

H2. Deviations from the Preregistration

The first deviation concerns the wording surrounding the treatment vignette. Before fielding, the attention text shown immediately before and after the treatment article was revised to clarify that respondents were reading a hypothetical news item. This change was made to reduce the risk that respondents would interpret the vignette as a real contemporaneous news report rather than as part of a survey experiment.

A second deviation concerns the wording of the extra-institutional treatment. Prior to launch, the reference to the *State of Emergency* (OHAL) was removed and replaced with more general language referring to "extraordinary powers." This change was made because, in the Turkish context, explicit reference to OHAL could activate associations with security and emergency governance that were not central to the article's theoretical contrast. The revised wording preserves the intended procedural distinction while reducing the risk of confounding the extra-institutional treatment with specific security-related narratives. The original and revised wording is available upon request or in archived materials.

A third deviation concerns the article's emphasis in the revised manuscript. The original design contemplated a broader set of attitudinal and behavioral outcomes. In the revised article, the empirical presentation is more tightly centered on the donation to the independent election-monitoring organization as the principal outcome. Secondary self-reported participation outcomes, emotions, and post-treatment attitudinal measures

remain available and are reported in the appendix, but they no longer play the same central role in the article's main evidentiary contribution.

H3. Changes in Inference and Outcome Prioritization

The preregistration anticipated a broader outcome set and an inferential strategy that relied more heavily on one-sided tests and multiple-testing adjustments across a larger family of outcomes. In the revised article, I instead adopt conventional two-sided inference and prioritize a smaller, more clearly organized set of principal analyses centered on the election-monitoring donation outcome and the extra-institutional versus procedural-control contrast.

This change reflects the article's revised contribution rather than a change to the underlying experimental design. The main objective of the revised manuscript is to present the clearest and most substantively meaningful result in a compact format. For transparency, the broader set of outcomes and alternative specifications remains documented in the appendix.

The prioritization of the extra-institutional-versus-control contrast as the principal comparison was not specified in the original preregistration. I adopt this prioritization here based on the design logic outlined in the main text, while reporting the full set of preregistered outcomes and alternative comparisons in the appendix.

H4. Ethics, Consent, and Transparency

All respondents were presented with an informed-consent screen before beginning the survey. The study used partial disclosure at the recruitment and consent stages in order to avoid revealing the article's precise theoretical interest in electoral manipulation before treatment exposure. This design choice was made to preserve the integrity of the experimental manipulation while ensuring that respondents understood that they were participating in a research study.

No personally identifying information is reported in the article or appendix. Replication materials, analysis code, and de-identified data will be posted in a public repository upon publication or acceptance, in accordance with journal requirements and the governing ethical approval.

H5. Pre-Registration Reporting Table

The preregistration framed the study around broad political participation and mobilization. The manuscript adopts a narrower evidentiary claim: extra-institutional electoral intervention increases opposition supporters' relative allocation to election-monitoring resources compared with an EU-framed control, while evidence

for broader participatory change and for the sharper extra-institutional-versus-institutional comparison is weaker.

The identifying preregistration URL has been provided to the editors. Because the public registration identifies the author, an anonymized summary of the preregistration and its correspondence with the manuscript is provided in this appendix.

Table 21: Pre-Registration Reporting Table

Reporting category	**Specified in pre-registration?**	**Reported in manuscript?**	**Deviations**
Sampling	Yes. The preregistration specified a Meta-recruited online survey of Turkish citizens over age 18, recruited through Facebook and Instagram, with a target of 800 participants and attempted recruitment up to approximately 1,000.	Yes. Main text, Research Design; Online Appendix B and D.	The final analytic sample includes 1,099 respondents after exclusions, slightly above the preregistered target and attempted recruitment range. This reflects recruitment dynamics during the Meta advertising period.
Sample exclusions	Yes. The preregistration stated that incomplete submissions would be excluded, that respondents failing attention checks would be excluded, and that duplicate-IP responses with abnormally short completion times would be excluded.	Yes. Main text, Research Design; Online Appendix D and H.	The main analysis excludes incomplete submissions, duplicate entries, and extreme survey-duration observations. The main analysis does not condition inclusion on post-treatment attention-check performance because attention checks are measured after treatment and may themselves be affected by treatment exposure. Attention-pass specifications are reported as robustness checks.
Experimental conditions	Yes. The preregistration specified three randomly assigned groups: control, institutional electoral manipulation, and extra-institutional electoral manipulation.	Yes. Main text, Treatment Design; Online Appendix C.	The implemented design follows the preregistered three-arm structure. One wording change was made to the extra-institutional treatment: the registered version referenced OHAL/state-of-emergency powers, while the manuscript uses a softened extraordinary decree-law framing to reduce direct emergency-governance associations.

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Reporting category	Specified in preregistration?	Reported in manuscript?	Deviations
Observed Measure 1: Election-monitoring donation allocation	Yes. The preregistration included a fixed-sum hypothetical donation task allocating 500 TL across NGOs, including independent election-observer NGOs.	Yes. Main text, Outcome Measurement and Results; Online Appendix E and F.	The manuscript treats the election-monitoring allocation as the principal outcome. The preregistration grouped this measure under political participation more broadly. The manuscript narrows interpretation and describes it as a structured stated allocation, not observed behavior.
Observed Measure 2: Other donation allocations	Yes. The preregistration included allocations to NGOs focused on violence against women, stray animals, and poverty, alongside election monitoring.	Yes. Main text, Results; Online Appendix E and F3.	These outcomes are reported as compositional counterparts to the election-monitoring allocation rather than as independent confirmatory outcomes, because the donation task is fixed-sum.
Observed Measure 3: Self-reported political participation	Yes. The preregistration included self-reported participation outcomes, including contacting politicians, protest voting, vote switching, costly voting, protest participation, and volunteering as an election observer.	Yes. Main text, Secondary Outcomes; Online Appendix E, F2, and F3.	The preregistration treated these outcomes as central political participation outcomes. The manuscript treats them as secondary because they show no clear or uniform participatory response and are more vulnerable to overstatement and expressive responding.
Observed Measure 4: Policy approval and democratic consistency	Yes. The preregistration included post-treatment policy approval and democratic-consistency items as manipulation-check / majoritarian-attitude measures.	Yes. Main text, Exploratory Pathways; Online Appendix E and G.	These measures are reported as exploratory post-treatment outcomes rather than causal mediators. The manuscript does not claim that these measures identify a causal mechanism.

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Reporting category	Specified in preregistration?	Reported in manuscript?	Deviations
Observed Measure 5: Emotional responses	Yes. The preregistration included emotional response measures and exploratory hypotheses about aversive and positive emotions.	Yes. Main text, Exploratory Pathways; Online Appendix E and G.	The manuscript reports emotional responses descriptively as exploratory pathways and does not estimate or interpret causal mediation, because emotions are post-treatment and not independently manipulated.
Observed Measure 6: Pretreatment moderators and covariates	Yes. The preregistration specified pretreatment demographics, political affiliation, prior vote choice, democratic preferences, economic satisfaction, and derived variables including government alignment.	Yes. Main text, Research Design and Empirical Strategy; Online Appendix D, E, and F.	The manuscript uses pretreatment government-support status as the main moderator. Covariate-adjusted models are reported as robustness checks rather than as the primary specification.
Hypothesis 1: Extra-institutional manipulation increases political participation	Yes. H1A preregistered that extra-institutional manipulations would mobilize voters for political participation.	Partly. Main text reframes the claim around election-monitoring resources; broader participation outcomes are reported as secondary.	The submitted manuscript narrows this broad mobilization hypothesis. The evidence does not support a generalized participation effect across the wider outcome battery. The manuscript therefore centers the narrower election-monitoring allocation result and treats broader participation outcomes as secondary.
Hypothesis 2: Extra-institutional manipulation has stronger effects than institutional manipulation	Yes. H1B and H2B preregistered that extra-institutional manipulation would have stronger mobilizing effects than institutional manipulation, especially among opposition supporters.	Yes, but interpreted cautiously. Main text, Empirical Strategy and Results; Online Appendix F4 and I.	The manuscript reports the extra-institutional-versus-institutional comparison but treats it as secondary and imprecise. The strongest evidence concerns the extra-institutional-versus-control contrast, while the sharper procedural contrast does not provide decisive evidence.

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Reporting category	Specified in pre-registration?	Reported in manuscript?	Deviations
Hypothesis 3: Extra-institutional manipulation increases participation among opposition supporters	Yes. H2A pre-registered that extra-institutional electoral irregularities would increase political participation among opposition supporters.	Yes, narrowed. Main text, Results and Secondary Outcomes; Online Appendix F.	The manuscript finds stronger evidence for increased election-monitoring allocation among opposition supporters than for broader self-reported participation. The claim is therefore narrowed from political participation generally to support for election-monitoring resources.
Hypothesis 4: Extra-institutional manipulation decreases participation among ruling coalition supporters	Yes. H2C and H2D preregistered that extra-institutional irregularities would decrease political participation among ruling coalition supporters, and more than institutional irregularities.	Yes, mostly as null or secondary evidence. Main text, Results and Exploratory Pathways; Online Appendix F and G.	The manuscript does not find a comparable increase in election-monitoring allocation among government supporters and does not center the preregistered demobilization claim. Government-supporter patterns are discussed as part of partisan asymmetry and exploratory interpretation.
Hypothesis 5: Extra-institutional manipulation increases aversive emotions	Yes. Exploratory H3A and H3B preregistered that extra-institutional manipulation would increase aversive emotions and more than institutional manipulation.	Yes. Main text, Exploratory Pathways; Online Appendix G.	Reported as exploratory only. The manuscript does not treat emotional responses as confirmatory tests or causal mediators.
Hypothesis 6: Institutional manipulation increases positive emotions among ruling party supporters	Yes. Exploratory H4C and H4D pre-registered positive-emotion expectations among ruling coalition supporters.	Partly. Online Appendix G reports emotional outcomes.	The manuscript does not foreground this hypothesis because the revised article centers the election-monitoring allocation result and treats emotional outcomes as exploratory. Positive-emotion patterns are reported for completeness but not emphasized in the main argument.

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Reporting category	Specified in pre-registration?	Reported in manuscript?	Deviations
Empirical test of H1	Yes. The preregistration specified multivariate regressions of political participation outcomes on institutional and extra-institutional treatment indicators and covariates, with tests of whether the extra-institutional coefficient is positive and larger than the institutional coefficient.	Yes, with modifications. Main text, Empirical Strategy and Results; Online Appendix F.	The manuscript uses pooled interaction OLS models with treatment indicators, government-support status, and treatment-by-government interactions as the principal specification. It reports covariate-adjusted models as robustness checks. The manuscript uses two-sided inference rather than preregistered one-sided tests.
Empirical test of H2	Yes. The preregistration specified subgroup regressions among opposition supporters and ruling coalition supporters.	Yes, with modifications. Main text, Results; Online Appendix F.	The manuscript estimates partisan heterogeneity primarily through pooled interaction models rather than separate subgroup regressions. This allows direct estimation of interaction terms within a common model. Subgroup patterns are also shown graphically and in robustness analyses.
Multiple testing / inference criteria	Yes. The preregistration specified $p < 0.05$ and Holm-Bonferroni correction.	Yes, deviation reported. Main text and Online Appendix H.	The manuscript uses conventional two-sided inference and does not apply Holm-Bonferroni correction to the broad secondary/exploratory outcome battery. This reflects the revised structure: one principal allocation outcome, secondary self-reported outcomes, and exploratory pathways rather than one large confirmatory family.
Standardization / transformations	Yes. The preregistration stated that ordinal political participation, emotion, and policy-opinion questions would be standardized.	Yes. Main text, figures, and Online Appendix F/G.	Donation and other outcomes are standardized for regression tables and coefficient plots where appropriate. Binary and ordered-logit alternatives are reported in the appendix.
Mediation analyses	Yes. The preregistration anticipated exploratory mediation analyses for aversive emotions.	No causal mediation analysis is presented. Exploratory emotional and policy-evaluation outcomes are reported.	The manuscript does not present mediation estimates because post-treatment emotional measures are not independently manipulated and would require stronger assumptions than the article is designed to support. This is discussed in the Exploratory Pathways section and Online Appendix H.
Were there studies included in pre-registration that are not reported in the manuscript?	No. The preregistration described one survey experiment.	Yes. The survey experiment is reported.	N/A.

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Reporting category	Specified in pre-registration?	Reported in manuscript?	Deviations
Additional comments	N/A.	N/A.	The manuscript is narrower than the preregistered plan. The preregistration framed the project around broad political participation and mobilization; the submitted manuscript centers the most stable result, election-monitoring allocation, while reporting broader participation outcomes, robustness checks, and exploratory pathways transparently in the appendix.

Appendix I. Power and Detectable Effects

This appendix reports both the original design-stage power analysis and a revised detectable-effects assessment tailored to the manuscript's current empirical focus. The original power analysis is retained for transparency because it reflects the broader planning logic used prior to fielding. The revised assessment is included because the present manuscript emphasizes a narrower set of outcomes and contrasts, especially the election-monitoring donation outcome and the contrast between extra-institutional intervention and the procedural control condition.

I1. Original Design-Stage Power Analysis

Prior to fielding, I conducted simulation-based power analyses to assess the sample sizes required to detect treatment effects under a multi-arm design with heterogeneous partisan responses. These simulations reflected the broader inferential goals anticipated at the design stage, including multiple treatment contrasts and subgroup-specific effects. For transparency, I retain those original calculations here.

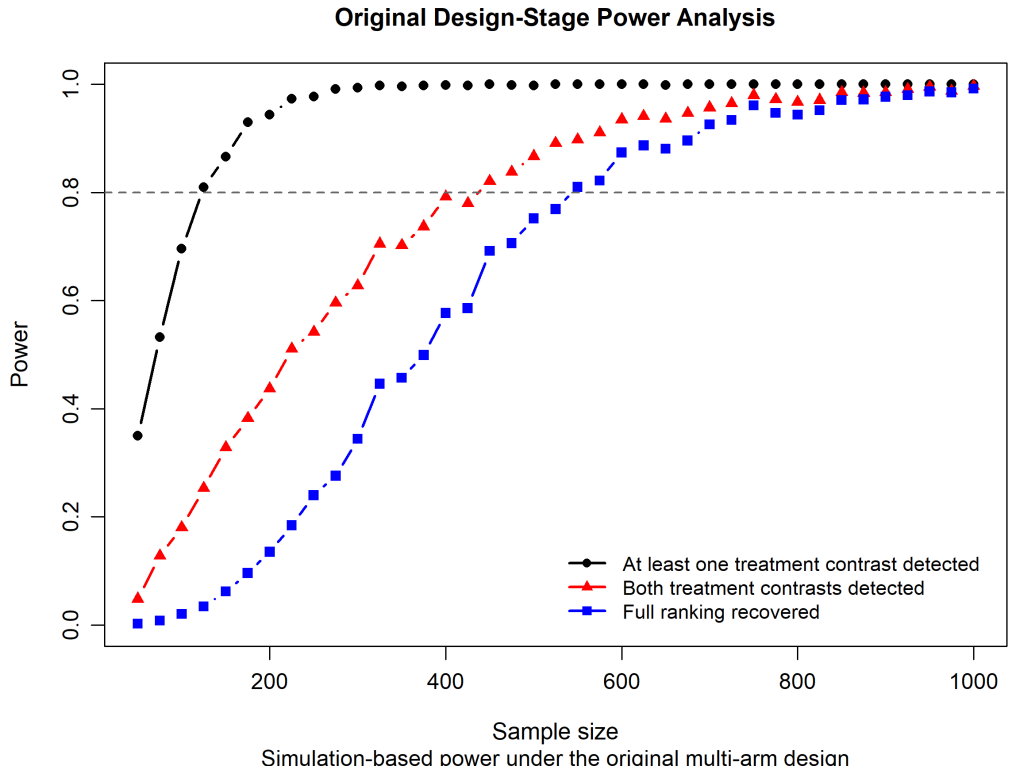

Figure 6: Original Design-Stage Power Analysis.

Figure 6 shows that the original design was generally expected to be informative for moderate treatment effects in the full sample, but substantially less informative for sharper subgroup contrasts and for more

demanding joint comparisons across treatment arms. These calculations document the original planning logic, but they do not determine the evidentiary standard for the revised manuscript, which focuses on a smaller and more clearly prioritized set of contrasts.

I2. Current-Design Power Curves Based on Observed Standardized Effects

The revised manuscript places primary emphasis on the election-monitoring donation outcome and on a narrower set of substantively central contrasts. For that reason, I report a second set of calculations based on the final analytic sample and the observed distribution of the current data.

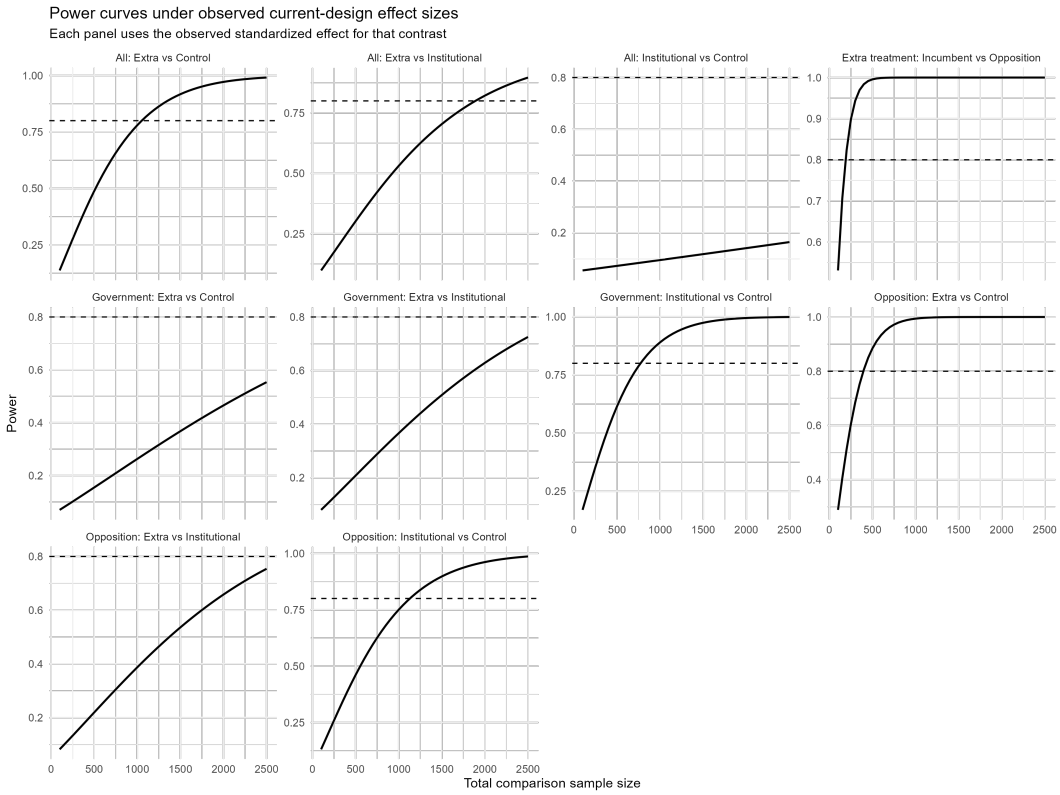

Figure 7: Power Analysis for Current Result.

Figure 7 plots power as a function of sample size for the current set of contrasts, using the standardized effects implied by the observed data. Table 22 reports, for each contrast, the comparison-specific sample sizes, group means and standard deviations, the raw and standardized observed effects, achieved power at the current sample size, and the minimum detectable effect required for 80 percent power.

Table 22: Current-design contrasts, observed effects, achieved power, and minimum detectable effects.

label	n_a	n_b	mean_a	sd_a	mean_b	sd_b	raw_effect	std_effect	achieved_power	mde_std_80	mde_raw_80
All: Extra vs Control	353	377	62.569	80.334	49.911	65.489	12.659	0.173	0.647	0.208	15.176
Government: Extra vs Control	133	153	41.759	58.997	46.466	53.582	-4.707	-0.084	0.109	0.333	18.717
Opposition: Extra vs Control	220	224	75.150	88.617	52.263	72.530	22.887	0.283	0.845	0.266	21.559
All: Institutional vs Control	369	377	52.626	74.166	49.911	65.489	2.715	0.039	0.083	0.205	14.362
Government: Institutional vs Control	165	153	36.406	45.918	46.466	53.582	-10.060	-0.202	0.435	0.315	15.691
Opposition: Institutional vs Control	204	224	65.745	88.777	52.263	72.530	13.482	0.167	0.406	0.272	21.926
All: Extra vs Institutional	353	369	62.569	80.334	52.626	74.166	9.943	0.129	0.408	0.209	16.133
Government: Extra vs Institutional	133	165	41.759	58.997	36.406	45.918	5.353	0.103	0.142	0.328	17.083
Opposition: Extra vs Institutional	220	204	75.150	88.617	65.745	88.777	9.405	0.106	0.193	0.273	24.208
Extra treatment: Incumbent vs Opposition	133	220	41.759	58.997	75.150	88.617	-33.391	-0.424	0.970	0.309	24.313

Notes: For within-subgroup contrasts, arm A is the first condition named in the contrast label. For the cross-group comparison, arm A is government supporters under the extra-institutional treatment. Standardized effects and minimum detectable effects are computed using the pooled standard deviation within each comparison sample. Achieved power and minimum detectable effects are calculated using a two-sided alpha of 0.05 and target power of 0.80.

The revised calculations clarify an important feature of the current design. The final analytic sample is informative for the article's main focal contrast, especially the comparison between extra-institutional intervention and the procedural control condition among opposition supporters. For that contrast, the observed standardized effect is moderate in size and the corresponding achieved power is comparatively high. By contrast, many secondary contrasts, particularly those involving government supporters or the distinction between extra-institutional and institutional intervention within subgroups, are substantially less well powered at the observed effect sizes.

The implication is not that the design is uninformative overall. Rather, it is that the current study is well positioned to detect moderate effects in the article's most central comparison, but less well positioned to rule out small or modest effects in secondary contrasts. For that reason, null results outside the principal focal comparison should be interpreted as comparatively imprecise rather than as strong evidence of no effect.

Appendix J. Full Survey Instrument

Appendix J1. Pre-treatment: Demographics

- "Pre_Age" What is your age? Choose the appropriate range.

1. "18-24"
2. "25-34"
3. "35-44"
4. "45-54"
5. "55-64"
6. "65 and older"

- "Pre_Sex" What is your gender?

1. "Male"
2. "Female"
3. "Other"

- "Pre_Educ" What is the highest level of education you have completed?

1. I haven't finished primary school
2. Primary school graduate
3. High school graduate
4. College graduate
5. Higher education graduate

- "Pre_SES" What is your socioeconomic class based on your income level?

1. Lower class
2. Lower middle class
3. Upper middle class
4. Upper class

- "Pre_Ethnicity" We are all citizens of the Republic of Turkey, but we may be of different ethnic origins; How do you know or feel your identity?

1. Turkish
2. Kurdish

3. Zaza
4. Arab
5. Other

- "Pre_RelSect" What religion and sect do you feel you belong to?

1. Sunni Muslim
2. Alevi Muslim
3. Other

Appendix J2. Political Choices

- "Pre_Politization" How interested are you in politics?

1. I'm not interested at all
2. I'm not very interested
3. I'm somewhat interested
4. I'm very interested

- "Pre_Affiliation" Which one do you find yourself closer to??

1. Ruling coalition
2. Opposition coalition
3. Other

- "Pre_AffiliationOther" [DISPLAYED IF Pre_Affiliation == 3] If you had to choose one of the two coalitions, which would you find yourself close to?

1. Ruling coalition
2. Opposition coalition

- "Pre_AffiliationGover" [DISPLAYED IF Pre_Affiliation == 1] How close do you find yourself to the ruling coalition?

1. I find it a little close
2. I find it somewhat close
3. I find it pretty close
4. I find it very close

- "Pre_AffiliationOppos" [DISPLAYED IF Pre_Affiliation == 2] How close do you find yourself to the opposition coalition?

1. I find it a little close
2. I find it somewhat close
3. I find it pretty close
4. I find it very close

- "Pre_Voting" Did you vote in the 2018 General Elections?

1. Yes
2. No

- "Pre_VotingParty" [DISPLAYED IF Pre_Voting == 1] Who did you vote for in the 2018 General Elections?

1. AKP
2. CHP
3. HDP
4. MHP
5. IYI Parti
6. Saadet Partisi
7. Other

Appendix J3. Pretreatment: Democratic Preferences

- "DemSupport1" Do you agree with the following statement: "A democratic system may have flaws, but it is better than other political systems."

1. I strongly disagree
2. I somewhat disagree
3. I somewhat agree
4. I strongly agree

- "DemSupport2" Do you agree with the following statement: "The most suitable political system for Turkey is democracy."

1. I strongly disagree

2. I somewhat disagree
3. I somewhat agree
4. I strongly agree

- "DemSupport3" Do you agree with the following statement: "Democracy leads to political chaos."

1. I strongly disagree
2. I somewhat disagree
3. I somewhat agree
4. I strongly agree

- "EconomicPerformance" What is your level of satisfaction with the current economic situation of the country?

1. I am extremely dissatisfied
2. I somewhat dissatisfied
3. I somewhat satisfied
4. I am extremely satisfied

Appendix J4. Posttreatment: Attention

- "Attention_Proposal" What did the government propose in the article you just read?

1. Proposal for a law change changing constituencies
2. Proposal for a Decree-Law changing constituencies
3. Proposal for a law change election time
4. Proposal for a Decree-Law election time

- "Attention_Seat" According to the experts in the article you just read, how many more seats will the government be able to win with the new regional system?

1. Five seats
2. Ten seats
3. Fifteen seats
4. Twenty seats

Appendix J5. Posttreatment: Emotion Mediators

- In a randomized order:
- "Emot_Hopeful" Below are a series of words that describe different emotions. Thinking about the text you just read, please indicate how much you feel the following emotions: [HOPEFUL]

1. Not at all
2. Little
3. Somewhat
4. A lot

- "Emot_ Disgusted" Below are a series of words that describe different emotions. Thinking about the text you just read, please indicate how much you feel the following emotions: [DISGUSTED]

1. Not at all
2. Little
3. Somewhat
4. A lot

- "Emot_Angry" Below are a series of words that describe different emotions. Thinking about the text you just read, please indicate how much you feel the following emotions: [ANGRY]

5. Not at all
6. Little
7. Somewhat
8. A lot

- "Emot_Frust" Below are a series of words that describe different emotions. Thinking about the text you just read, please indicate how much you feel the following emotions: [FRUSTRATED]

5. Not at all
6. Little
7. Somewhat
8. A lot

- "Emot_Excited" Below are a series of words that describe different emotions. Thinking about the text you just read, please indicate how much you feel the following emotions: [EXCITED]

9. Not at all
10. Little
11. Somewhat
12. A lot

- "Emot_Worried" Below are a series of words that describe different emotions. Thinking about the text you just read, please indicate how much you feel the following emotions: [WORRIED]

9. Not at all
10. Little
11. Somewhat
12. A lot

Appendix J6. Posttreatment: Manipulation Checks – Majoritarian Attitudes

- "Policy_Approval" Do you support the constituency change proposal in the article you just read?

1. I absolutely do not support
2. I somewhat do not support
3. I somewhat support
4. I absolutely support

- "Policy_ Consistency" Do you find the constituency change proposal in the article you just read compatible and consistent with democracy?

1. I absolutely do not find it compatible and consistent with democracy
2. I somewhat do not find it compatible and consistent with democracy
3. I somewhat find it compatible and consistent with democracy
4. I absolutely find it compatible and consistent with democracy

Appendix J7. Posttreatment: Political Participation - Self-reported

- In a randomized order:

- "PolPart_Pol" There are many ways to improve things in Turkey. Considering the text you read, which of the following would you do? [I contact a politician or local government official]

- "PolPart_ProVot" There are many ways to improve things in Turkey. Considering the text you read, which of the following would you do? [I cast a protest vote in elections]

- "PolPart_VotSwit" There are many ways to improve things in Turkey. Considering the text you read, which of the following would you do? [I vote for another party in elections]
- "PolPart_CostlyVot" There are many ways to improve things in Turkey. Considering the text you read, which of the following would you do? [I buy a bus ticket to be able to vote in the province where I am registered in the elections.]
- "PolPart_Prot" There are many ways to improve things in Turkey. Considering the text you read, which of the following would you do? [I participate in protests or demonstrations]
- "PolPart_Mont" There are many ways to improve things in Turkey. Considering the text you read, which of the following would you do? [I volunteer at the polls as an election observer]
- "Post_Voting" If you were to vote in the next general election tomorrow, who would you vote for?

1. AKP
2. CHP
3. HDP
4. MHP
5. IYI Parti
6. Democrat Party
7. Saadet Partisi
8. Gelecek Partisi
9. DEVA

Appendix J8. Posttreatment: Political Participation - Donation

- In a randomized order:
- "Don_Woman" Suppose you have 500 TL to donate. Please share your 500 TL among the following four non-governmental organizations as you wish. Donations should amount to 500 TL in total. [Non-Governmental Organizations Against Violence Against Women] Continuous Variable in [0,500] such that Don_Woman + Don_Animal + Don_Monit + Don_Povert = 500
- "Don_Animal" Suppose you have 500 TL to donate. Please share your 500 TL among the following four non-governmental organizations as you wish. Donations should amount to 500 TL in total. [Non-Governmental Organizations for the Protection of Stray Animals] Continuous Variable in [0,500] such that Don_Woman + Don_Animal + Don_Monit + Don_Povert = 500

- "Don_Monit" Suppose you have 500 TL to donate. Please share your 500 TL among the following four non-governmental organizations as you wish. Donations should amount to 500 TL in total. [Independent Election Observer Non-Governmental Organizations] Continuous Variable in [0,500] such that Don_Woman + Don_Animal + Don_Monit + Don_Povert = 500

- "Don_Povert" Suppose you have 500 TL to donate. Please share your 500 TL among the following four non-governmental organizations as you wish. Donations should amount to 500 TL in total. [Non-Governmental Organizations Against Poverty]
  Continuous Variable in [0,500] such that Don_Woman + Don_Animal + Don_Monit + Don_Povert = 500

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