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Surfacing Electoral Processes: Operational Transparency Increases Trust in Local Elections

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Abstract

Trust in elections is critical to the stability of democratic institutions, and recent declines in election confidence highlight a need for effective interventions. Distrust may stem from election processes being hidden from voters. We test whether operational transparency, showing voters how elections are run and secured, can increase trust. We start with two pilot studies, one lab and one field experiment, and culminate in a pre-registered field experiment with 109,277 voters in Shasta County, California. Operational transparency postcards increased trust in local elections (0.28 points on a five-point scale, adjusted for political orientation; 95% CI 0.05 to 0.50), mediated by perceived transparency and the perception that one's vote has been counted, not by understanding. Contrary to our pre-registration, two postcards also increased turnout (up to 0.8 percentage points; 95% CI 0.1 to 1.6). These experiments present a positive picture for operational transparency.

Trust is essential for the functioning of democracy[1–3]. It influences power, politics, and social exchanges and serves as the glue that holds society together, enabling cooperation and the functioning of democratic systems[4]. In the context of elections, trust is particularly crucial. It pertains to the confidence that voters have in the electoral process and its outcomes. It involves believing that votes are counted accurately, election officials act fairly, and the overall electoral process is transparent and secure. Without this trust, citizens may question the legitimacy of election results, leading to decreased political participation and heightened political polarisation[2]. As such, fostering trust in the electoral system is essential to maintaining the integrity and stability of democratic institutions.

Understanding how to increase trust in elections amongst voters is a central challenge to the continued functioning of our democratic systems. The growing spread of misinformation and political polarisation has led people to distrust government agencies, elected politicians, and even their fellow voters[5–7]. Confidence in US election administration fell by 19 percentage points between 2018 and 2020 (from 81% very or somewhat confident to 62%) and had only partly recovered, to 70%, by 2022[8]. Gallup data show that satisfaction with the way democracy is working in the USA fell to a record low of 28% at the end of 2023 and was 34% a year later, still below the 35% measured shortly after Jan. 6, 2021, where distrust in elections was a significant factor contributing to rioters' storming of the Capitol[9]. Trust in government, generally, is near historic lows since first measured in 1958, at 17% in 2025[10]. Ahead of the 2026 midterms, fewer than half of registered voters were confident that votes would be counted accurately, with confidence among Democrats falling and among Republicans rising since 2022, a reminder that trust in elections can move with who holds power rather than with how elections are run[11]. With the highly polarised nature of US elections, where candidates openly disparage and threaten the electoral process, there has never been a more important time to understand and improve trust in elections.

While the antecedents of trust and distrust in elections are complex[12], current levels of distrust may be partially the result of election operations and processes being hidden from voters (following from Suzanne Mettler's argument about underappreciation of governments at large in The Submerged State)[13]. Election processes, varying by county and state, are often hidden from voters. One consequence is that voters may not understand how elections work. Compounding this, people typically know less about complex policies than they think they do, a phenomenon known as the illusion of explanatory depth (IOED). IOED is linked to political polarisation,

where the phenomenon inflates one's confidence in the causal understanding of political phenomena like election outcomes[14]. Vitriol and Marsh link IOED to the endorsement of conspiracy beliefs, particularly for supporters of the losing candidate and less experienced voters[15]. This phenomenon was clearly visible following the 2020 US presidential election, wherein a variety of election actors endorsed a wide variety of conspiracy theories alleging mishandling of the election.

If distrust results from not understanding how elections work, the most natural fix is to explain, and many efforts to build trust in elections do exactly this. Carey et al. show, in survey experiments in the US and Brazil, that prebunking and corrections from credible sources raise confidence in elections, with the effect appearing to come from new factual information[16]. Evidence from the field, however, is weaker. In a pre-registered field experiment during the 2024 presidential election, Kousser et al. found that a campaign explaining election integrity protections to conservative voters in Texas had mostly null effects on trust and turnout, with effects much smaller than those found in survey experiments[17]. Similarly, close replications have not reproduced the finding that asking people to explain a policy moderates their political attitudes[18]. Understanding may therefore not be the only, or the most effective, route to trust in practice.

A second way in which hidden processes may undermine trust is that voters do not see the work that goes into running elections. Building on Mettler, Buell et al. propose operational transparency: uncovering hidden processes, people and effort to surface the work of government[19]. Operational transparency could build trust either by increasing understanding (in doing so, combating IOED), or by making hidden processes visible to voters.

While increasing trust in elections with operational transparency of election processes is a novel application, studies of operational transparency in relation to government service platforms have revealed it to be an effective intervention for increasing government trust levels[19,20]. Buell et al. found residents who interacted with a website that visualised service requests and government efforts to resolve them became 14% more trusting and 12% more supportive of the government[19]. Government responsiveness increased residents' feelings of personal efficacy and long-term engagement, suggesting operational transparency, revealing work performed by the government for the benefit of residents, increased trust[19].

We apply this idea of transparency as a trust-builder to elections in the US, where there seems to be a significant appetite for clear information around less visible voting methods like voting by mail. Our central thesis is that by increasing the operational transparency of election

processes and security, we will increase trust in elections. Our theory encompasses the operational transparency intervention itself (e.g., a postcard sent directly to voters), perceived transparency (e.g., a person's evaluation of the intervention), and trust in government and elections (e.g., that individual's trust measure). We hypothesise that if we increase operational transparency of mail voting through communication interventions, people will trust the vote by mail process more, and ultimately be more likely to participate in it. We measure, but do not predict, a directional effect on voting itself.

Operational transparency is, however, a multidimensional concept. While the core idea is to make the processes and operations that are otherwise invisible visible, this can take a multitude of forms, for instance transparency of process, effort, workers and decision-making. There is not, yet, research exploring this nuance. As such, we operationalise the theory into four components of elections where transparency can be highlighted: the vote-by-mail process, ballot security, the security of the voter file, and the effort of election officials and workers. From these interventions, we predict perceived transparency will best mediate the effect of the interventions on trust and compare it to other possible mediators: belief that participants' own vote was accurately counted, perceived understanding of components of elections, feeling like they know their election official, perceived election official capability, and motivation. Comparing these mediators also allows us to test which of the two routes operational transparency follows. If it works through understanding, we would expect perceived understanding to mediate the effect on trust. If it works by making hidden processes visible, we would expect perceived transparency to mediate it instead.

In summary, we draw on Mettler's argument about the submerged state[13] and build on the operational transparency results from Buell et al.[19]. We apply the theory to an election context, which has yet to be done, where we test its effectiveness on trust in elections and on voting. Further, we explore the mechanisms that mediate the relationship between operational transparency and trust in elections. We draw on both laboratory and field research, with the latter increasing the methodological robustness of research in this context through a large field experiment in which voting behaviour is measured from administrative records (whereas Buell et al.[19] focus only on engagement). We report three studies: a lab study testing which content increases trust, a field pilot in Shasta County, California, and a pre-registered field experiment with all registered voters in the same county, for which we show both voter file (election outcomes) and survey-based (psychological outcomes) results.

Results

Study design and samples

We tested how operational transparency interventions work in a laboratory study and two field studies in Shasta County, California, looking at the impact on trust and voting outcomes. We focused the transparency interventions on the vote-by-mail process, the security of mail ballots, the security of the voter file and the effort of election workers (Table 1). We were not able to test all ideas in each study given sample size limitations, so each study built on the one before it.

Table 1: **Summary of intervention focus, coverage and medium across the three studies.**

Focus area	Description of the concept	Lab study	Shasta County pilot	Shasta County field experiment
Vote-by-mail process	Highlights the steps voters take to complete the mail-in voting process and how the ballot is processed and counted.	Videos (US-wide and state-specific) and infographic	Email	Postcard
Vote-by-mail security	Highlights the security measures in place for mail ballots, such as signature checks, two-person handling, certified equipment and audits.	–	Email	Postcard
Ballot tracking	Highlights how voters can track their mail ballot through the process.	Video	–	–
Voter file security	Highlights how the list of registered voters is kept accurate and protected.	–	–	Postcard
Effort	Highlights election officials, volunteers and election workers, and the work they do for elections.	–	–	Postcard
Comparison group		Unrelated video or infographic	Basic reminder email	No contact
Sample		906 online participants	40,171 voters; 406 survey respondents	109,277 voters; 633 survey respondents

In the lab study, 942 Amazon Mechanical Turk workers were recruited around the 2022 primary elections, of whom 907 completed the study and 906 could be analysed. Participants were randomly assigned to watch a video or view an infographic about the vote-by-mail process,

a video about tracking a mail ballot, or, as a control, an unrelated video or infographic. They then rated their trust in local, state and federal government when it concerns elections.

In the pilot field study, 40,171 registered voters in Shasta County were randomly assigned before the June 7th, 2022 statewide primary to receive a basic reminder email, or the reminder with additional content on either the vote-by-mail process or vote-by-mail security. Of the voters invited to a follow-up survey, 472 responded and 406 could be analysed.

In the main field study, which was pre-registered, all 111,566 voters registered in Shasta County were randomized before the November 2022 midterm elections into a no-contact control group and four postcard conditions: vote-by-mail process, vote-by-mail security, voter file security and local election workers; 109,277 remained in the voter file after the election and are analysed. A follow-up survey was completed by 711 voters, of whom 633 could be analysed.

We assess the impact of the field interventions on voting behaviour using the voter file data administratively tracked by the county, and on self-reported trust in elections using the follow-up surveys. Analyses are conducted separately because the survey and voter file data cannot be linked at an individual level.

Lab study: which content increases trust

In the lab study, the general vote-by-mail process video increased trust in elections at every level of government (Fig. 1 and Extended Data Table 1). Compared with the control, trust increased by 5.2 points on a 0–100 scale overall (95% CI 1.0 to 9.5, $p = 0.015$), by 5.3 points for local government ($p = 0.028$), 5.3 points for state government ($p = 0.037$) and 5.1 points for federal government ($p = 0.046$). The ballot tracking video, the state-specific process video and the infographic also increased trust, by between 2 and 5 points, but these effects were not significant, apart from the infographic on trust in state government ($p = 0.035$). When we combine the four conditions, they do not differ significantly from the control ($p = 0.12$), and so we see the general vote-by-mail process as the most promising content, rather than as clearly outperforming the others. The effects of the conditions by political orientation are shown in Supplementary Table 2.

In exploratory analyses, the process video and the infographic both increased participants' understanding of the vote-by-mail process (0.33 and 0.39 points on a five-point scale, $p < 0.001$), and the process video increased confidence in election officials ($p = 0.028$; Supplementary Table 3). However, understanding was only weakly related to trust ($r = 0.16$), and accounting for it

reduced the effect of the process video on trust only slightly, from 5.2 to 4.6 points. Interestingly, the infographic, which increased understanding the most, increased trust less than the process video. The conditions did not change confidence in science or religion, trust in other people, or intention to vote, which we believe suggests the effect is specific to trust in elections rather than a general increase in positive responses.

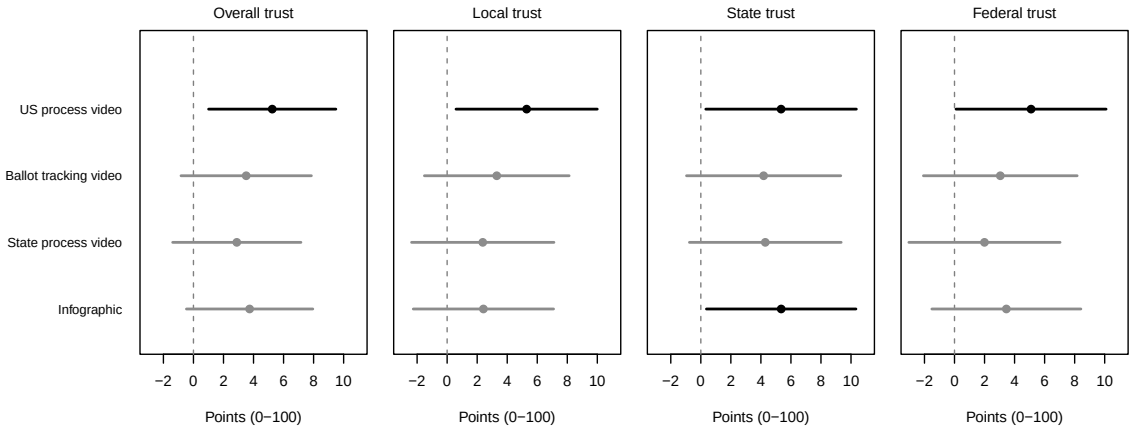


Figure 1: **Lab study: effect of each condition on trust in elections at each level of government.** Points show the difference from the control group (an unrelated video or infographic) on a 0–100 scale, from regression models adjusting for political orientation and age group; lines show 95% confidence intervals. Black points are significant at $p < 0.05$; grey points are not. $n = 906$.

Trust in elections in the field

In the field pilot, the vote-by-mail security email increased trust in local government by 0.48 points on a seven-point scale compared with the basic reminder email (95% CI 0.05 to 0.91, $p = 0.029$, $n = 406$; Fig. 2 and Supplementary Table 6). It trended towards significantly increasing trust in election officials and the federal government ($p = 0.06$ and 0.09, respectively), with a smaller, non-significant increase in trust in state government ($p = 0.11$). The vote-by-mail process email did not change trust.

In the main field study, most notable from the survey data is the effect of vote by mail process and vote by mail security, which both significantly increase trust (or trend towards significance) across the election official, election workers, local elections and US elections outcomes, after adjusting for political orientation and age (Table 2). The clearest effects are on trust in local elections, which increased by 0.36 points on a five-point scale for the vote-by-mail process postcard (95% CI 0.01 to 0.72) and 0.41 points for the vote-by-mail security postcard (95% CI 0.09 to 0.73; Fig. 2). The election workers and voter file security postcards had smaller effects that were

not significant. These positive effects appear to be concentrated among voters who identify on the right of the political spectrum, although the interactions are mostly not significant. This is contrary to our pre-registered expectation that the postcards would have less impact on voters on the right, and that the security conditions could backfire. When we combine all the treatments, receiving any postcard leads to a significant increase in trust in local elections (0.28 points, 95% CI 0.05 to 0.50; Supplementary Table 10). Respondents' political orientation differed between conditions, and without this adjustment the effects are smaller. Only the effect of vote by mail security on trust in local elections remains significant (Supplementary Table 12).

Across the three studies, we see the same content increasing trust, namely information about the vote-by-mail process and the security of mail ballots, with the clearest effects on trust in local government and local elections.

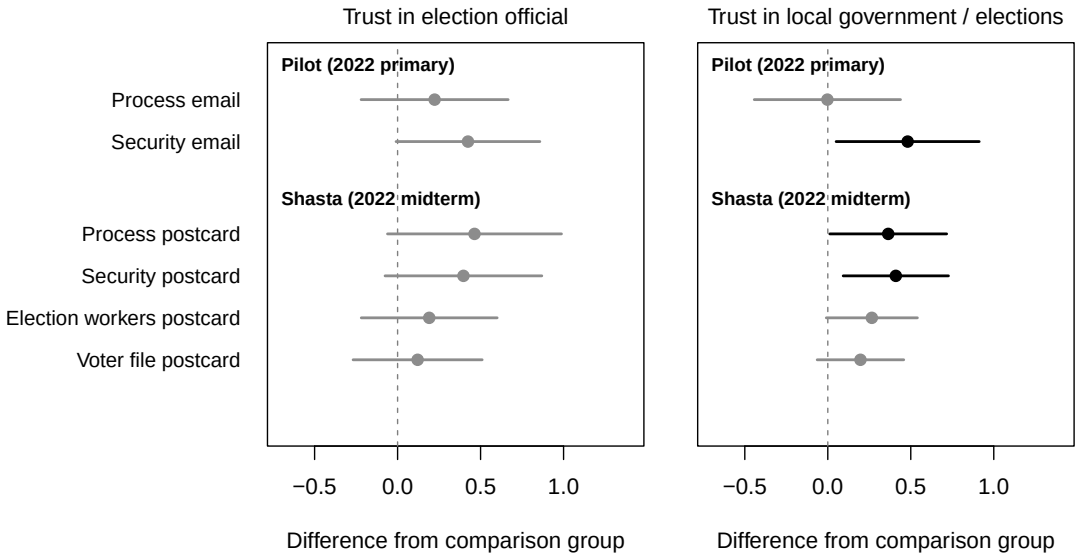


Figure 2: **Field studies: effect of each condition on trust.** Pilot: difference from the basic reminder email (trust measured 1–7). Main field study: difference from the no-contact control (trust in election official 1–7; trust in local elections 1–5). Models adjust for political orientation and age; lines show 95% confidence intervals. Black points are significant at $p < 0.05$; grey points are not. Pilot $n = 405$–406; main study $n = 630$–633.

Perceived transparency and mediation

Because the postcards most clearly increased trust in local elections, we test the six pre-registered mediators on this outcome (Fig. 3 and Extended Data Table 2). In the main field study, perceived transparency significantly mediated the treatment's impact on trust in local elections (indirect effect = 0.19, 95% CI 0.04 to 0.34, $p = 0.012$). Such a mediation implies the postcards work, at least in part, because they increase how transparent voters perceive elections to be. The only

Table 2: **Shasta County survey data regression analysis.**

	Without interactions				With condition × lean interactions			
	Election official	Election workers	Local elections	US elections	Election official	Election workers	Local elections	US elections
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Election Workers	0.191	0.210	0.265*	0.045	0.292	0.192	0.214	0.090
	(0.208)	(0.187)	(0.140)	(0.143)	(0.338)	(0.302)	(0.226)	(0.233)
Vote-by-Mail Process	0.463*	0.447*	0.364**	0.306*	0.769*	0.780**	0.533**	0.298
	(0.267)	(0.241)	(0.179)	(0.184)	(0.402)	(0.364)	(0.269)	(0.276)
Vote-by-Mail Security	0.397*	0.446**	0.410**	0.277*	0.723*	0.672**	0.412	0.241
	(0.241)	(0.216)	(0.161)	(0.166)	(0.380)	(0.340)	(0.255)	(0.261)
Voter File Security	0.121	0.101	0.197	−0.026	0.211	0.229	0.193	−0.071
	(0.198)	(0.177)	(0.133)	(0.136)	(0.315)	(0.282)	(0.211)	(0.216)
Lean Left Politically	2.912***	2.456***	1.906***	2.336***	3.275***	2.693***	2.057***	2.387***
	(0.158)	(0.142)	(0.106)	(0.109)	(0.337)	(0.301)	(0.225)	(0.231)
Middle Politically	2.097***	1.684***	1.276***	1.635***	1.961***	1.708***	0.944***	1.448***
	(0.185)	(0.166)	(0.124)	(0.127)	(0.439)	(0.393)	(0.294)	(0.301)
Age	0.191***	0.113***	0.139***	0.159***	0.188***	0.112***	0.136***	0.158***
	(0.047)	(0.042)	(0.032)	(0.032)	(0.048)	(0.043)	(0.032)	(0.033)
Left × Election Workers					−0.404	−0.082	−0.125	−0.168
					(0.468)	(0.420)	(0.314)	(0.323)
Left × Vote-by-Mail Process					−0.554	−0.459	−0.302	−0.057
					(0.607)	(0.547)	(0.407)	(0.418)
Left × Vote-by-Mail Security					−0.953*	−0.633	−0.291	−0.249
					(0.532)	(0.477)	(0.357)	(0.366)
Left × Voter File Security					−0.230	−0.249	−0.158	0.102
					(0.445)	(0.398)	(0.298)	(0.305)
Middle × Election Workers					0.336	0.240	0.529	0.147
					(0.574)	(0.515)	(0.384)	(0.394)
Middle × Vote-by-Mail Process					−0.371	−0.723	−0.188	0.185
					(0.715)	(0.644)	(0.480)	(0.493)
Middle × Vote-by-Mail Security					0.409	0.213	0.677	0.777*
					(0.684)	(0.614)	(0.459)	(0.471)
Middle × Voter File Security					0.127	−0.092	0.387	0.109
					(0.543)	(0.488)	(0.364)	(0.374)
Constant	2.163***	2.982***	1.873***	1.252***	2.044***	2.881***	1.879***	1.269***
	(0.292)	(0.260)	(0.194)	(0.199)	(0.345)	(0.305)	(0.228)	(0.234)
Observations	630	630	633	631	630	630	633	631
R^2	0.374	0.340	0.362	0.447	0.381	0.346	0.370	0.453
Adjusted R^2	0.367	0.333	0.355	0.440	0.366	0.330	0.355	0.439
Residual Std. Error	1.744	1.566	1.173	1.202	1.746	1.569	1.173	1.203
degrees of freedom	622	622	625	623	614	614	617	615
F Statistic	53.04***	45.77***	50.73***	71.80***	25.16***	21.64***	24.14***	33.90***
degrees of freedom	7; 622	7; 622	7; 625	7; 623	15; 614	15; 614	15; 617	15; 615

Note: *p<0.1; **p<0.05; ***p<0.01

other pre-registered variable that significantly mediated the impact of the treatment conditions on trust was "the belief that my vote is counted accurately in all elections" (indirect effect = 0.18, 95% CI 0.02 to 0.36, $p = 0.03$), a similar, though less precise, effect to perceived transparency. Our pre-registered prediction that perceived transparency would be the strongest mediator is therefore only partly supported. Perceived understanding of elections, feeling like voters know their election official, and perceived election official capability and motivation to conduct an accurate election do not mediate this model. When we include perceived transparency and the

belief that one's vote is counted in the same model, we see that each still mediates part of the effect, and together they account for 86% of the effect of the postcards on trust in local elections (combined indirect effect = 0.24, 95% CI 0.05 to 0.42), with a small, non-significant direct effect remaining (0.04, 95% CI −0.09 to 0.18; Supplementary Table 16). We believe this suggests that these are related but distinct ways in which the postcards work, by increasing how transparent voters perceive elections to be and by reassuring voters that their own vote is counted.

All six mediators were strongly related to trust in local elections. However, the postcards only increased perceived transparency (0.26 points, $p = 0.015$) and the belief that one's vote is counted (0.24 points, $p = 0.034$), and did not change perceived understanding of elections (0.01 points, $p = 0.94$), knowing one's election official, or views of election officials' capability and motivation. The vote-by-mail process postcard increased both perceived transparency (0.41 points, $p = 0.014$) and the belief that one's vote is counted (0.40 points, $p = 0.021$), and the vote-by-mail security postcard increased perceived transparency (0.34 points, $p = 0.023$; Supplementary Table 15). We see a similar pattern in the lab study and the pilot, where the interventions increased trust without understanding explaining the effect. In the pilot, the vote-by-mail security email increased perceived transparency (0.30 points on a five-point scale, $p = 0.038$) but not understanding of the vote-by-mail process ($p = 0.86$, Supplementary Table 7). As the mediators and trust were measured in the same survey, we treat these indirect effects with some caution (see Limitations).

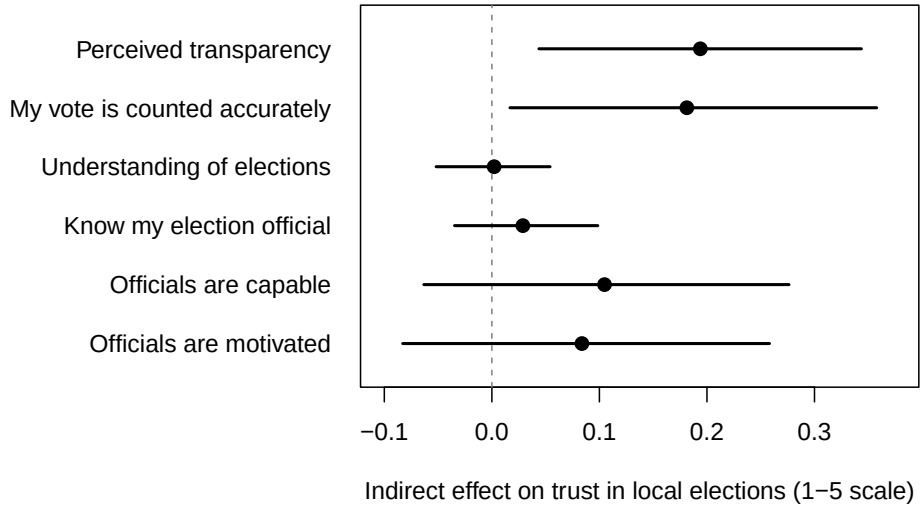


Figure 3: **Indirect effect of the postcards on trust in local elections through each pre-registered mediator.** Points show the indirect effect (the effect of any postcard on the mediator multiplied by the effect of the mediator on trust, holding the postcards constant); lines show 95% confidence intervals from 10,000 nonparametric bootstrap draws. All models adjust for political orientation and age. Trust in local elections is measured on a 1–5 scale. $n = 630$–633.

Voter turnout and voting method

In the main field study, the vote by mail process and vote by mail security conditions both had significant, positive main effects on voting and vote by mail behaviour for the 2022 midterm elections (Table 3). For vote by mail security, the effect on voting is only significant once we control for voting history. In terms of size, vote by mail process increased voting by 0.8 percentage points (95% CI 0.1 to 1.6) and vote by mail security increased voting by mail by 0.9 percentage points (95% CI 0.2 to 1.7; Fig. 4). In the raw data, 62.7% of voters who received the vote by mail process postcard voted, compared with 61.9% of the control group, or about 160 additional voters. The election workers and voter file security postcards did not affect voting or voting by mail. When we combine all the treatments, we see that receiving any treatment leads to a significant increase in voting by mail and trends towards an increase in voting (Supplementary Table 9). These effects on voting are contrary to our pre-registration, in which we predicted no effect on voting, and they trend towards significance after adjusting for multiple comparisons (see Robustness). In the pilot, neither email changed voting or voting by mail in the June 2022 primary (Fig. 4).

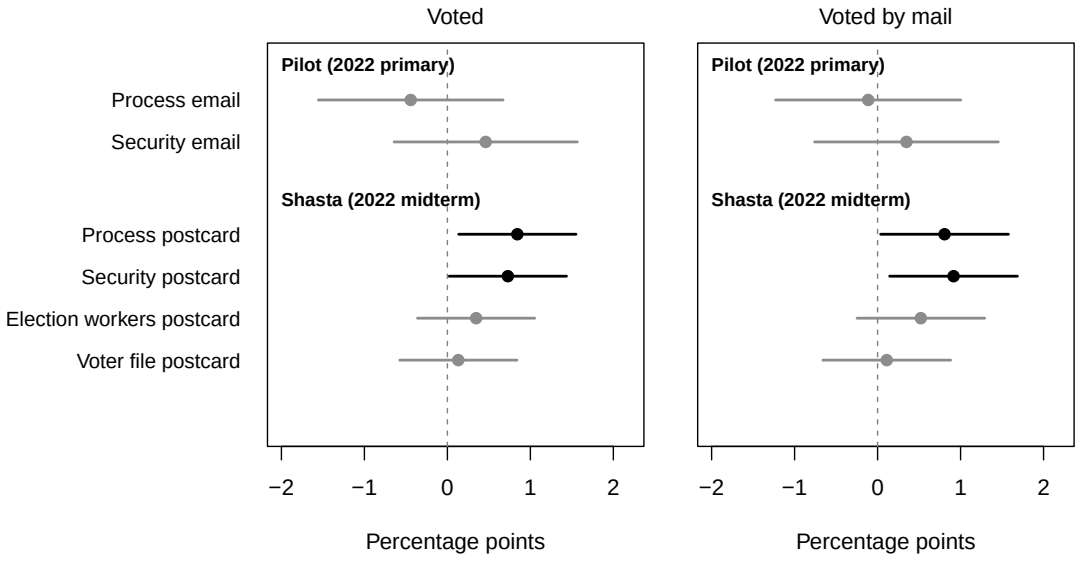


Figure 4: **Effect of each condition on voting and voting by mail.** Points show the difference from the comparison group in percentage points (pilot: basic reminder email; main field study: no contact), from linear probability models adjusting for political party, age and voting history; lines show 95% confidence intervals. Black points are significant at $p < 0.05$; grey points are not. Pilot $n = 35{,}152$; main study $n = 101{,}058$. The pre-registered logistic models are in Table 3.

Table 3: **Shasta County voter file regression analysis.**

	Voting			Voting-by-Mail		
	(1)	(2)	(3)	(4)	(5)	(6)
Election Workers	0.017	0.022	0.009	0.018	0.029	0.020
	(0.021)	(0.025)	(0.035)	(0.020)	(0.023)	(0.031)
Vote-by-Mail Process	0.051**	0.062**	0.062*	0.044**	0.048**	0.047
	(0.021)	(0.025)	(0.035)	(0.020)	(0.023)	(0.031)
Vote-by-Mail Security	0.034	0.052**	0.044	0.041**	0.054**	0.040
	(0.021)	(0.025)	(0.035)	(0.020)	(0.023)	(0.031)
Voter File Security	0.016	0.007	−0.014	0.012	0.006	−0.010
	(0.021)	(0.025)	(0.035)	(0.020)	(0.023)	(0.031)
Left Political Party	−0.365***	−0.419***	−0.418***	−0.033**	0.020	0.008
	(0.017)	(0.020)	(0.038)	(0.016)	(0.019)	(0.035)
No Party Preference	−0.860***	−0.485***	−0.520***	−0.590***	−0.201***	−0.222***
	(0.018)	(0.021)	(0.040)	(0.018)	(0.020)	(0.037)
Age	0.044***	0.021***	0.021***	0.039***	0.018***	0.018***
	(0.0004)	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Frequency of Voting in Eligible Elections		4.074***	4.074***		3.271***	3.271***
		(0.029)	(0.029)		(0.026)	(0.026)
Left × Election Workers			0.006			0.024
			(0.061)			(0.056)
Left × Vote-by-Mail Process			0.048			0.058
			(0.062)			(0.056)
Left × Vote-by-Mail Security			−0.037			−0.002
			(0.061)			(0.056)
Left × Voter File Security			−0.019			−0.012
			(0.061)			(0.056)
No Party Preference × Election Workers			0.053			0.019
			(0.063)			(0.060)
No Party Preference × Vote-by-Mail Process			−0.051			−0.063
			(0.064)			(0.060)
No Party Preference × Vote-by-Mail Security			0.080			0.075
			(0.064)			(0.060)
No Party Preference × Voter File Security			0.114*			0.090
			(0.064)			(0.060)
Constant	−1.452***	−2.358***	−2.351***	−1.726***	−2.490***	−2.483***
	(0.025)	(0.030)	(0.033)	(0.024)	(0.029)	(0.031)
Observations	102,695	101,058	101,058	102,695	101,058	101,058
Akaike Inf. Crit.	117,850.8	89,370.9	89,375.0	126,510.5	104,553.3	104,558.0

Note: *p<0.1; **p<0.05; ***p<0.01

Differences by party registration

When looking at a partisan split, we only see trending effects, where the vote by mail process condition increases voting for those on the right and the voter file security condition increases voting for those with no party preference ($p < 0.1$; Table 3). Neither remains after adjusting for multiple comparisons. Contrary to our pre-registered expectation, the effect of the postcards on trust in local elections did not vary with age ($F = 1.53$, $p = 0.19$).

Robustness

In our initial pre-registration, we had not included controlling for voting history or whether there was an email on file. The effects persist when we control for voting history and whether voters have an email on file (Supplementary Table 13). In the survey, respondents' political orientation differed between conditions. Without adjusting for political orientation and age, the effects on trust are smaller, and only the effect of the vote by mail security condition on trust in local elections remains significant (Supplementary Table 12).

Logistic regression was pre-registered for the voter file data; we also use linear probability models for ease of interpretation, and results do not substantially differ between approaches (39 of 40 condition effects reach the same level of significance; Supplementary Table 14). Given that the study includes several conditions, we performed a robustness check using the Benjamini–Hochberg procedure to control for the false discovery rate with behavioural outcomes (Supplementary Table 17). This does cause some of the findings to reduce in significance. The effects of the vote by mail process and vote by mail security conditions on voting and voting by mail ($p < 0.05$) are downgraded to trending towards significance (adjusted $p = 0.074$ to 0.075), and the trending interaction effects are no longer significant. Nevertheless, we follow the approach taken by similar studies[21] and report these while relying most on the unadjusted analysis in the main text.

Discussion

In this paper, we propose and evaluate a method of trust building for elections in the form of operational transparency. Building on initial studies of operational transparency[19], we aimed to determine if increasing the perceived transparency of election processes would increase voters' trust in elections. Across a lab study, a field pilot and a pre-registered field experiment, we find that interventions to increase operational transparency can increase trust, an effect that is mediated by perceived transparency and the perception that one's own vote is counted. Contrary to our pre-registered prediction, we see that the postcards that increase trust in Shasta County also increase voter turnout, an effect that may reflect increased trust in vote by mail. The other postcards sent in Shasta County did not change voting behaviour, suggesting that the effect on voting outcomes is not simply the result of a reminder. Ultimately, we find operational transparency interventions communicating election processes to be a promising method for af-

fecting trust. This timely finding builds upon existing literature by expanding the operational transparency theory into an election context and providing promising, but realistic, estimations of its practical value.

Interestingly, we see the same content working across all three studies. In the lab, the video explaining the general vote-by-mail process was the only condition to increase trust at every level of government. In the field, the emails and postcards describing the vote-by-mail process and the security of mail ballots increased trust, whereas the postcards about election workers and the voter file did not. We believe this suggests that operational transparency is most effective when it surfaces what happens to a voter's own ballot, rather than the people or systems behind the election. We also see that the effects are clearest for trust in local government and local elections. The postcards were sent and signed by the county's Registrar of Voters and described the county's own process, which we suspect goes some way in explaining why trust in the local election office moved the most, while the effect on trust in US elections only trended towards significance.

The mediation results also give us some insight into how these interventions work. One possibility we raised is that distrust may partly stem from a lack of understanding of how elections operate. However, understanding does not appear to be the route through which operational transparency builds trust. In the main field study, the postcards did not change perceived understanding of elections, and understanding did not mediate their effect on trust. Similarly, in the lab, the videos and the infographic increased understanding of the vote-by-mail process, but this explained little of their effect on trust. Instead, the postcards worked through perceived transparency and the belief that one's own vote is counted. This suggests that operational transparency builds trust by making a process that is usually hidden feel visible, and by reassuring voters that their own ballot is safe, rather than by teaching voters how elections work. Carey et al. found that new factual information appeared to drive increases in confidence in elections in survey experiments[16], whereas in our studies trust moved with perceived transparency rather than understanding, indicating that seeing and knowing may be different routes to trust.

Our field results also add to recent evidence on public information campaigns. Kousser et al. found that a campaign explaining election integrity protections to conservative voters in Texas during the 2024 presidential election had mostly null effects on trust and turnout, except for a possible small increase in trust in vote by mail[17]. Our field effects were also relatively modest, but the process and security postcards did increase local trust and voting. Several differences may

help explain this. Our postcards showed voters the process rather than explaining safeguards, they came from the county's own Registrar of Voters, and they reached all registered voters rather than only those predicted to distrust elections. Notably, the one effect Kousser et al. found was on trust in vote by mail, the very process our postcards made visible. Together with our mediation results, this suggests that operational transparency may be most effective when it lets voters see the specific process they worry about, such as what happens to their own ballot, rather than telling them in general terms that elections are secure.

From a practical standpoint, it is intriguing that simply confirming that one's own vote was counted in the election emerges as an important mediating factor. This finding relates to research showing that voting methods impact voter confidence: participants assigned to an in-person voting condition who were able to confirm that their vote was counted reported the highest voter confidence[22]. Similarly, those who do not cast a ballot on election day, through voting absentee or early voting, have lower voter confidence, which may be due to a fear that their ballot was lost[23]. Alongside our findings, this suggests that low trust in vote by mail may partly reflect being unsure whether the vote successfully went through.

While the interventions in our study appeared to reassure voters, a priori, that their vote would make it, it would be possible to further push this mechanism in future studies, perhaps with 'vote received receipts', tracking messages that voters receive when their ballot reaches different parts of the process (sent, received, counted) or easy access to a portal to follow one's vote. This insight could also go beyond vote by mail. For instance, in-person voting machines could provide audible feedback when ballots are entered; municipalities could develop public livestreams of their ballot counting rooms; or election offices could offer in-person tours showing how votes are counted. These latter ideas are naturally more involved or 'higher touch' interventions, which could, relative to communications, have larger impacts on trust, especially for those with the lowest trust in how elections work.

We also suspect that the medium of the intervention matters. In the pilot, the vote-by-mail security email increased trust in local government but did not change voting, while in the main field study, the postcards increased both trust and voting. The pilot emails were added to a reminder that all voters received, and emails are easily overlooked, whereas the postcards were a standalone piece of mail from the election office. We cannot separate the effect of the medium from the differences between a primary and a midterm election, but this comparison suggests that how operational transparency is delivered may be as important as what it shows.

We centre our contribution on the implicitly psychological construct of trust, which we measured through surveys with admittedly low response rates (likely due to lack of incentive for the survey). Our response rates may bring fair concerns about the validity of our survey-based assertions, which we discuss with the other limitations of this work in Methods. Another way our studies could be strengthened is by linking the voter file and survey data to enable mediation analysis between the interventions, survey-based measures, and behaviour-based measures. This would give a clearer sense of whether it is indeed trust that is acting as the mechanism to improve voter outcomes. This was not possible here, as the survey and voter file data cannot be linked at an individual level.

Further research into operational transparency and trust in elections should test operational transparency in other US states, and globally. Our field studies were conducted in a single county, and so it would be especially compelling to see the impact of these interventions in counties and states with different levels of trust and different voting methods, and in 'purple' states with fine margins, where a slight boost in turnout could matter most.

Overall, we demonstrate that simple communication-based interventions can be effective at increasing trust in elections while also increasing voting, particularly when they make the vote-by-mail process and its security visible to voters. Our findings contribute to the literature but, perhaps more importantly, to election officials' capacity for protecting and restoring trust in American elections in the face of unprecedented threats.

Methods

Ethics

This study, titled "Operational Transparency to Increase Trust in Elections", was approved by the Departmental Ethics Review Committee of the Psychological and Behavioural Science Department at the London School of Economics (reference 292216, dated 03/11/2023). The approval was for secondary data analysis, where data was shared after the studies were complete. All procedures performed in this research were in accordance with the ethical standards of the Departmental Ethics Review Committee. The laboratory study was approved under the Duke University IRB (2022-0087). Survey respondents gave informed consent before taking part. Voter file data are administrative records held by the county election office and were shared with the research team in anonymised form.

Pre-registration

The main field study in Shasta County was pre-registered on the Open Science Framework (`https://osf.io/m2vk6`). The primary hypothesis, that operational transparency increases trust in elections, was not condition-specific. We pre-registered that the postcards would increase voting by mail but would not affect overall voting.

Setting and participants

Two studies were conducted around the 2022 primary elections to develop and pilot materials for the main experiment. First, we conducted a lab study in the spring of 2022 with 942 Amazon Mechanical Turk workers, of whom 907 completed the study. Following the lab study, we piloted operational transparency materials with the election office in Shasta County, California, with the 40,171 registered voters with an email on file, before the June 7th statewide primary. As the first direct field collaboration with election officials, the pilot was also meant to inform the feasibility and design of studies during the midterm elections. The main field study took place during the November 2022 midterm elections and included all 111,566 voters registered in Shasta County before the study launched in October 2022, of whom 109,277 remained in the voter file after the election and are analysed. The difference reflects the usual movement of voters on and off the voter file around an election, as people register or are removed after moving away. The number of postcards that could be sent was limited to 80,000, so the no-contact control group is larger than the other conditions.

Randomisation

In the main field study, individual voters were randomized into a no-contact control group and four postcard conditions. To make the conditions as similar as possible, 30 randomizations were run and the one with the smallest average difference between conditions (0.55%) was chosen, checking the share of voters with an email on file, registered to each of the main parties, and who had voted, and voted by mail, in the 2018, 2020 and 2022 primary elections. Voters in the vote by mail security condition were on average 0.34 years younger than the control group ($p < 0.1$), and no other characteristic differed (Supplementary Table 8).

Interventions

We focused the transparency interventions on the vote-by-mail process, the security of mail ballots, the security of the voter file and the effort of election workers (Table 1).

In the lab study, two conditions served as control groups providing information unrelated to elections, either a video about making a doughnut or an infographic about making a book. The other four conditions displayed information about the mail voting process, either as a video or infographic: a video about the general US vote-by-mail process, a video about tracking a mail ballot, a video with state-specific procedures for voting by mail, and an infographic about the vote-by-mail process. For the state-specific condition, we required participants to be in the matching state (Ohio, Missouri, California or Washington). The pilot field study tested two versions of a transparency email, vote by mail process and vote by mail security, against a basic election reminder email. In the main field study, the election office directly mailed postcards to voters, except those in the control group, ahead of the November 8th election. All postcards were sent via a mail distribution company on behalf of the Shasta County election office, signed by the county's Registrar of Voters, and arrived in homes the week of October 24th, 2022. The four postcard conditions were vote by mail process, vote by mail security, voter file security and local election workers. All intervention materials are shown in the Supplementary Information.

Outcome measures

In the lab study, trust was measured as trust in local, state and federal government when it concerns elections, on 0–100 scales, using the three dimensions established in the organisational trust literature: integrity, competence and benevolence[24]. For each level of government, participants were randomly shown the items for one of the three dimensions, and their score is the mean of those items; overall trust is the mean of the three levels. As exploratory outcomes, we also measured understanding of the vote-by-mail process (1–5), confidence in election officials, science and religion (1–4), trust in other people (0–100) and intention to vote in 2024 (1–5).

In the field studies, voting behaviour was taken from the voter file data administratively tracked by the county: whether each voter voted in the election, and whether they voted by mail. Trust was measured in follow-up surveys. In all cases, requests to complete a follow-up survey were sent via email. For both Shasta populations, the survey was sent shortly after the election. In the pilot, the survey measured trust in the election official and in local, state and federal government (1–7 scales), as well as perceived transparency (1–5) and understanding of the

vote-by-mail process (1–5). In the main field study, the survey measured trust in election officials and election workers (1–7 scales), trust in local elections and US elections (1–5 scales), and trust in local, state and federal government. The pre-registered mediators were perceived transparency, the belief that one's own vote is counted accurately in all elections, perceived understanding of components of elections, feeling like one knows one's election official, and perceived election official capability and motivation (all 1–5 scales). Voter file and survey data cannot be linked at an individual level.

Statistical analysis

In the lab study, we use OLS regression with condition, political orientation and age group (18–34, 35–54 and 55 and over). The two control conditions are combined. Political orientation is the mean of two 0–100 self-placement scales (social and economic), grouped as right (40 or below), middle (above 40 to 60) or left (above 60). One participant without a political orientation is excluded, leaving 906 participants. Interaction models add condition by political orientation terms, and we test the four conditions jointly against the control with an F-test.

We assess the impact of the field interventions on voting behavior, participation and method using logistic regression, as pre-registered for the voter file data. Models include condition, party registration and age, and are shown with and without frequency of voting in eligible elections (the share of past elections in which the voter was eligible and voted). Party registration is grouped as right (Republican, Libertarian), left (Democratic, Peace and Freedom, Green) or no party preference; voters registered with other parties are excluded from these models, as are recorded ages over 100, and models with voting history exclude voters with no eligible past elections. The analysed samples are 102,695 voters in the main field study (101,058 with voting history) and 37,755 in the pilot (35,152 with voting history). Interaction models add conditions by party terms. We then use the survey data to determine the impact of interventions on self-reported trust in elections, using OLS regression with condition, self-reported political orientation (right, middle or left, from a 1–7 self-placement scale) and age. Analyses are conducted separately because the data cannot be linked at an individual level. Figure 4 shows voting and voting by mail effects from linear probability models with the same covariates as the logistic models with voting history (main study n = 101,058; pilot n = 35,152), so that effects are in percentage points, and Fig. 2 shows trust effects from the survey models without interactions (main study n = 630–633; pilot n = 405–406).

Mediation was tested in the main field study survey, comparing any postcard with the no-contact control, with trust in local elections as the outcome, and with political orientation and age in both the mediator and outcome models. Each of the six pre-registered mediators was tested in a separate model ($n = 630$–633). With linear models and no treatment-by-mediator interaction, the indirect effect is the product of the effect of the postcards on the mediator and the effect of the mediator on the outcome. 95% confidence intervals are from 10,000 non-parametric bootstrap resamples of respondents (percentile intervals; random seed 2022, set separately for each mediator). As a robustness check, we used the Benjamini–Hochberg procedure to control the false discovery rate with behavioural outcomes. This method rank-orders p-values within a family of tests and compares them to an adjusted critical value to determine significance. Voting and voting by mail models are adjusted together, and main and interaction models are adjusted separately (Supplementary Table 17). All tests are two-sided; we describe effects with $0.05 \leq p < 0.1$ as trending towards significance. Analyses were conducted in R (version 4.5.1).

Deviations from pre-registration

In our initial pre-registration, we had not included controlling for voting history or whether there was an email on file. Through our analysis we see voting history as the most valid measure of engagement: it strongly and consistently predicts voting and voting by mail. These variables were not pre-registered because we were not aware they were available when placing the registration. Throughout the analysis, we show the models both with and without voting history, and we see the same results when we also control for whether there was an email on file (Supplementary Table 13).

Our pre-registered behavioural models also included gender, which is not available in the voter file, and previous voting method (new voter, previously voted by mail, or previously did not vote by mail). When we use previous voting method in place of voting history, the effects of the vote by mail process and vote by mail security conditions on voting by mail remain significant ($p = 0.034$ and $p = 0.046$). We had also planned to look at whether voters who had not previously voted by mail switched to voting by mail, but we did not find an effect.

For the survey, our pre-registered models also controlled for income, education and knowing or being an election worker. We leave these out of the main analysis because including them reduces the sample by a fifth (from 633 to 505 respondents). With them included, we see similar or larger effects on trust (for example, for trust in local elections, 0.42 for vote by mail process,

$p = 0.035$, and 0.46 for vote by mail security, $p = 0.012$). We had also planned to exclude fast respondents, but survey durations were too skewed for the rule to be applied as written, and we did not exclude respondents for straight-lining.

Logistic regression was pre-registered for the voter file data; we also use linear probability models for ease of interpretation (Fig. 4), and these give the same pattern of results, with 39 of 40 condition effects reaching the same level of significance (Supplementary Table 14). We had pre-registered no effect on overall voting. Finally, we extended the pre-registered mediation analysis in three ways: we focused on trust in local elections, as this was the outcome the postcards most clearly affected; we report the effect of each postcard on each mediator; and we tested perceived transparency and the belief that one's vote is counted together in one model.

Limitations and sensitivity analyses

Our trust measures come from follow-up surveys with admittedly low response rates, and those who responded were not a random sample of each condition. In the main field study, 711 of the approximately 44,242 voters with an email on file completed the survey, and the number of respondents varied by condition, from 69 in the vote by mail process condition to 216 in the voter file security condition. Respondents' political orientation also differed between conditions: 46.9% of respondents in the vote by mail process condition placed themselves on the right, compared with 40.0% in the control group. As political orientation strongly predicts trust, we adjust for political orientation and age in the survey models. Without this adjustment, only the effect of the vote by mail security condition on trust in local elections remains significant (Supplementary Table 12). The voter file outcomes do not have this problem, as they include every voter in the study.

Similarly, the mediators and trust in local elections were measured in the same survey and are closely related ($r = 0.72$ to 0.84 for the two significant mediators). As such, we see the indirect effects as consistent with, but not proof of, a causal pathway.

Our studies also test several conditions, outcomes and subgroups. After adjusting for multiple comparisons, the effects on voting and voting by mail in the main field study trend towards significance (Supplementary Table 17). In the lab study, the four conditions do not jointly differ from the control, and the exploratory outcomes were not part of the original analysis, so we treat these results as exploratory.

We also measure the effect of being sent a message, rather than of reading it. The lab study

used an online sample and measured trust immediately after the intervention, and in the pilot the transparency emails were compared with a basic reminder email rather than no contact.

Beyond the deviations from our pre-registration described above, voters registered with minor parties (for example, American Independent) and those with recorded ages over 100 are excluded from the party-based analyses. Finally, as the voter file and survey data cannot be linked, we cannot test whether changes in trust explain changes in turnout, and the field studies took place in a single county in California.

Use of AI tools

Claude (Anthropic) was used to assist with code checking and text editing; the authors take full responsibility for the content.

Data availability

The data contain information about individual voters and survey participants and are therefore not publicly available. The Shasta County voter file data can be shared with researchers on request but cannot be made public. Anonymised survey data and analysis code are available from the corresponding author on reasonable request.

Code availability

The R code used to produce all analyses, tables and figures is available from the corresponding author on request.

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Author contributions

J.S. and M.D. conceived and designed the studies. J.S. and M.D. collaborated with the election office and collected the data. M.D. conducted the original analyses. D.A. re-ran and extended all analyses and produced the tables and figures. J.S., M.D. and D.A. wrote and revised the manuscript.

Competing interests

The authors declare no competing interests.

Extended Data

Extended Data Table 1: **Lab study effect on trust in elections at each level of government.**

	Overall (1)	Local (2)	State (3)	Federal (4)
US Vote-by-Mail Process Video	5.245**	5.293**	5.344**	5.098**
	(2.159)	(2.397)	(2.554)	(2.546)
Ballot Tracking Video	3.511	3.304	4.182	3.048
	(2.212)	(2.455)	(2.617)	(2.608)
State Vote-by-Mail Process Video	2.886	2.371	4.293*	1.995
	(2.176)	(2.415)	(2.574)	(2.565)
Vote-by-Mail Infographic	3.740*	2.416	5.352**	3.453
	(2.144)	(2.380)	(2.537)	(2.528)
Lean Left Politically	13.161***	7.503***	13.341***	18.638***
	(1.883)	(2.091)	(2.228)	(2.221)
Middle Politically	7.374***	3.837*	6.660***	11.626***
	(2.042)	(2.266)	(2.416)	(2.407)
Age Group	4.941***	5.808***	4.493***	4.521***
	(1.035)	(1.149)	(1.225)	(1.221)
Constant	34.510***	42.343***	37.778***	23.410***
	(2.709)	(3.008)	(3.206)	(3.195)
Observations	906	906	906	906
R^2	0.078	0.046	0.059	0.087
Adjusted R^2	0.071	0.038	0.052	0.080
Residual Std. Error	21.766	24.162	25.752	25.665
degrees of freedom	898	898	898	898
F Statistic	10.90***	6.14***	8.06***	12.19***
degrees of freedom	7; 898	7; 898	7; 898	7; 898

Note: *p<0.1; **p<0.05; ***p<0.01

Extended Data Table 2: **Mediation of the postcard effect on trust in local elections, by mediator.**

Mediator	Postcards → mediator (a)	Mediator → trust (b)	Indirect effect	95% CI	n
Perceived transparency	0.26**	0.74***	0.19**	0.04 to 0.34	632
My vote is counted accurately	0.24**	0.76***	0.18**	0.02 to 0.36	633
Understanding of elections	0.01	0.26***	0.00	−0.05 to 0.05	630
Know my election official	0.11	0.27***	0.03	−0.03 to 0.10	632
Officials are capable	0.14	0.75***	0.10	−0.06 to 0.28	633
Officials are motivated	0.11	0.75***	0.08	−0.08 to 0.26	632

Note: Indirect effects with 10,000 nonparametric bootstrap draws. All models adjust for political orientation and age. *p<0.1; **p<0.05; ***p<0.01

Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- OperationalTransparencysupplementaryinformation.pdf