Countering capture in local politics: Evidence from eight field experiments[*]

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Abstract

In the first field experiments to encourage participation in local civic bodies, I examine if outreach can reduce inequalities in who participates in city council meetings. Renter participation in local politics lags that of homeowners, who often participate to oppose housing growth. 19,951 renter households received randomly assigned emails encouraging them to comment at their city council meetings and support housing growth. Opening a message highlighting potential costs of abstention from local politics increased public comments by 1.4 percentage points versus placebo. These effects are substantively large: treatment-induced comments represented 8% of total comments and 46% of pro-housing comments across all targeted meetings. The results suggest that even low-cost outreach strategies can meaningfully increase participation in lesser-known settings like city councils and make these bodies more reflective of the general public. Further, increasing the perception that abstention is costly appears to be an effective motivator of collective action.

Homeowner participation in local politics in the United States outpaces that of renters (Einstein, Palmer and Glick 2019; Yoder 2020; **?**). Even moving city council meetings online in 2020 did not increase renter attendance (Einstein, Glick, Puig and Palmer 2021). Research into local housing policy suggests that this participation gap is reflected in land use and zoning policies that represent the economic interests of homeowners (Einstein, Glick and Palmer 2019; Fischel 2005; Marble and Nall 2021). Yet these policies often harm renters through decreased access to housing and higher rents (Charette, Herbert, Jakabovics, Marya and McCue 2015; Ganong and Shoag 2017; Glaeser and Gyourko 2018,0; Glaeser, Gyourko and Saks 2005; Lens and Monkkonen 2016; Quigley and Rosenthal 2005). Renters therefore also have an incentive to participate in local politics and support housing growth, but their participation lags homeowners.

Differences in economic incentives partially explain this participatory gap. Economic self-interest typically only motivates political behavior when benefits are "tangible, large, visible, and certain" (Citrin, Reingold and Green 1990). Homeowners can receive tangible benefits from halting neighboring development through preserved property value. For renters, more housing will only reduce rents throughout a diffuse geographic region in the long term. How then can those who only benefit through long-term and uncertain gains (like renters) be motivated to engage in personally costly political behavior?

In the first field experiments to motivate participation in local civic bodies, 19,951 renter households in 8 cities in Los Angeles (LA) County were randomly assigned to receive emails encouraging them to comment at their city council meetings and support pro-housing regulatory policies. Three mechanisms of mobilization were tested by randomizing messaging to: (1) provide attendance instructions only, (2) prime rational economic self interest, or (3) highlight the costs of abstention from local politics.

Receipt of any treatment increased public comments by 1 percentage point (pp) versus placebo, while highlighting costs of abstention increased comments by 1.4pp. Voters in local elections were more responsive to treatment (2.3pp) than non-voters (0.9pp). These effects

are substantively large as council meeting attendance is typically low. Treatment-induced comments comprised 8% of total comments and 46% of pro-housing comments across all meetings. A majority of comments were pro-housing in over 50% of treated meetings, in stark contrast with previous findings that pro-housing comments are typically in the minority in council meetings in equilibrium (Einstein et al. 2021; Yoder 2020).

These results suggest that in direct contrast with voter turnout, low-cost outreach strategies like email can meaningfully increase political participation in remote settings such as commenting at city council meetings. As council meetings have low baseline rates of attendance, the increases in participation caused by this outreach are substantively large. Outreach targeted at underrepresented groups can therefore make civic bodies more reflective of the broader public, unlike allowing remote access alone. In terms of messaging, increasing perceived costs of abstention appears to be a particularly effective motivator of participation.

Homeownership and political participation

This paper examines if direct outreach can make participation in local civic bodies more reflective of the broader public.[1] Research using administrative data finds that becoming a homeowner increases individuals' propensity to vote in local elections or participate in city council, planning, and zoning meetings (Yoder 2020; ?). Examination of the mechanisms driving homeowner participation suggests this behavior is consistent with rational economic behavior in the form of protection of property values (Marble and Nall 2021; McCabe 2016; Yoder 2020; ?). Homeowners are more likely to support policies that restrict new housing development, raising the value of existing homes (Einstein, Palmer and Glick 2019; Hankinson 2018).[2]

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[1]Similar outreach campaigns could also be used on different populations to increase participatory gaps. However, due to pre-existing asymmetries in information and incentives, it is unclear if such campaigns would be as effective as those contacting underrepresented groups. Further research is necessary to establish how high-participation groups are mobilized.

[2]From an economic policy perspective, the ability of residents to block new housing construction is regularly cited as a key cause of decreases in housing supply (Glaeser, Gyourko and Saks 2005). These supply restrictions are estimated to reduce geographic mobility, reduce real income for renters, and lower aggregate US economic growth (Glaeser and Gyourko 2018; Hsieh and Moretti 2019).

The makeup of local political participation in majority-renter cities therefore does not typically reflect general public opinion. Unlike homeowners, renters do not consistently oppose new housing (Hankinson 2018; Marble and Nall 2021; Monkkonen and Manville 2019). This leads to discrepancies between the percentage of council meeting comments in favor of additional housing and the percentage of ballots cast in favor of additional housing (Einstein, Palmer and Glick 2019). Even moving council meetings online due to COVID-19 did not reduce the participation gap between renters and homeowners (Einstein et al. 2021). However, while increased access did not increase renter participation, it remains unclear if making the economics of housing policy salient for renters would increase their participation, just as homeownership causes participation to increase through economic channels.[3]

Encouraging remote political participation

City council participation was limited to email, phone, or video calls due to COVID-19 at the time of the experiment, and these remote options remain in place to this day. Remote access did not, however, reduce pre-existing participatory gaps (Einstein et al. 2021), and prior research offers both lessons and conflicting predictions for encouraging remote political participation.

Experimental research primarily finds digital outreach ineffective for in-person political mobilization (Green and Gerber 2019). However, tests of the efficacy of digital outreach at increasing political participation that can itself be conducted remotely remain limited. Exceptions are absentee voting and online voter registration, where email outreach was also ineffective (Nickerson et al. 2007). However, digital outreach has shown promise at increasing more expressive forms of remote political participation when the right appeals are made. Social media outreach can increase petition signatures, but only after direct personal appeals (Coppock, Guess and Ternovski 2016). Email solicitations also appear to

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[3]Participation is also highly unequal in project-by-project approval institutions such as city planning and zoning meetings. While project approval often occurs within city councils in the LA area, this is not always the case. Whether outreach campaigns are also effective at increasing turnout in more bureaucratic settings such as planning and zoning meetings is an important area for future research.

increase small donations, with donors responsive to the content of messaging (Gaynor and Gimpel 2021).[4] Unlike in-person participation, a remote and expressive political action like public comment may therefore also be responsive to digital outreach, with gains from appeals identified in in-person campaigns carrying over into remote participation.

In-person campaigns offer lessons on which appeals may be successful in the remote context. Field experiments suggest merely providing information that one *can* participate does not have a large impact on voter turnout (Green and Gerber 2019). However, providing a plan of how and when to participate has proven effective (Milkman, Beshears, Choi, Laibson and Madrian 2011; Nickerson and Rogers 2010). As renters are on average less connected to their local political system (Ansolabehere 2012; McCabe 2016), providing renter households with information on how to participate and making access easy by providing a direct, clickable link to public comment may encourage participation.

A secondary line of research suggests that economic motivations drive participation. As noted above, homeowner participation in politics is hypothesized to be driven by economic self-interest, as blocking a development can have a large and immediate impact on neighboring property values. However, as the benefits to renters are longer term and less tangible, it is unclear if economic motivators will increase renter participation (Citrin, Reingold and Green 1990; Sears and Funk 1991). I therefore test if priming economic self-interest can also increase renter participation, despite a lack of a tangible asset such as a home.

Other studies, however, suggest psychic motivators are more effective at driving participation than instructional information or economic self-interest alone (Citrin, Green, Muste and Wong 1997; De Rooij, Green and Gerber 2009; Ostrom 2000; Sears and Funk 1991). Aytaç and Stokes (2019) posit that high psychological costs of abstention combined with low costs of participation maximize collective action. I therefore provide the first field-experimental test of Aytac and Stokes' theory by testing messaging that highlights lack of renter political participation as a contributor to policy capture and personal economic harm, thereby

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[4]Literature in campaign finance argues that small donors have expressive motivations (Ansolabehere, De Figueiredo and Snyder Jr 2003; Huddy, Mason and Aarøe 2015; Shieh and Pan 2010).

increasing the perceived cost of abstention.

Past research provides competing theories for encouraging political participation in remote contexts. Digital outreach may simply be ineffective at driving participation in real-world settings, regardless of whether they can also be accessed digitally. Alternatively, expressive participation such as public comment may be responsive to outreach, particularly when the right appeals are made. I adjudicate this debate and offer key empirical and theoretical advancements to the literature on political participation. First, I document the costly, real-world response of a historically low-participation group to distinct instructional, economic, and psychological motivators to collective action, and show that instructional appeals are less effective at driving turnout than those that highlight costs. Second, I challenge previous conclusions that digital outreach is a poor motivator of collective action by extending this literature beyond the voting booth and into a domain where expressive real-world political participation can be conducted remotely.

Hypotheses and treatment messages

The observations above leads to three (pre-registered) hypotheses of motivation to collective action, which are tested with three distinct treatment messages. A treatment (T1) that lowers costs of participation with detailed participation instructions should increase attendance, but effects should be small in magnitude. All treatment messages therefore include a Zoom link for spoken comments, or link to submit a pre-filled sample public comment via email (while noting that individuals may draft their own comment) for written comments.[5] A treatment (T2) providing information that lack of housing supply increases rents should increase attendance more than attendance instructions only by priming economic self interest. A treatment (T3) that also highlights costs of abstention should increase attendance more than treatments that lower costs of participation or prime economic self-interest alone.

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[5]See Sample comment for the wording of the sample message.

Research design

The experiment was fielded in LA County in collaboration with a pro-housing NGO. Cities were in the process of updating their 2021-2029 "Housing Elements," which are a required analysis of a city's housing needs and strategies to meet those needs. The experiment therefore targets council meetings in which the Housing Element is on the agenda. COVID-19 moved city council meetings online, where comments can be made in spoken or written format. Written comments can be submitted by email and are either read aloud during the meeting or distributed to council members prior to the meeting. Council members should therefore be aware of the sentiments expressed in public comments, spoken or written.

While there is a vocal anti-development contingent in Los Angeles, the general voting public appears to support additional housing as anti-development ballot measures have recently failed.[6] Only 28% of respondents in a survey of LA County residents oppose a hypothetical local development (Monkkonen and Manville 2019). The geographic and regulatory landscape in Los Angeles also leads to a majority of new housing developments replacing parking lots or commercial buildings, not existing housing stock.[7] Nevertheless, interventions involving participation in governmental processes should be held to high ethical standards. For a discussion of research ethics, please see Ethics in the appendix.

Experiment overview

The experiment proceeded in the following steps: (1) renters in the voter file were identified using LA City Planning records, (2) city council meetings discussing their Housing Element were targeted for the messaging campaign, (3) renters were randomly assigned to one of three email treatments encouraging them to submit a comment or a placebo control, (4) names in all treatment groups were matched with names of individuals who submitted a

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[6]Measure S, which would have curbed high-density development in the city, failed with 30% support. Measure JJJ—which grants zoning changes to developments that include affordable housing—and Measure H—which instituted a sales tax increase to fund affordable housing—passed.

[7]Roughly 14% of land, or over 200 square miles, is currently dedicated to parking (**?**). Affordable housing is also required for density above zoning limits.

public comment, (5) analysis was performed using pre-registered outcomes and estimators.

Identifying renters and council meetings

Renters were identified by geo-matching addresses in the LA County voter file with Department of City Planning records of multi-unit apartment buildings using the FastLink probabilistic linkage algorithm (Enamorado, Fifield and Imai 2019). This resulted in 641,184 matched renters, 266,057 of whom listed their email addresses in the voter file. Partner organizations then monitored city council meetings in LA County for agenda items discussing the Housing Element throughout fall and winter 2021. Identified renters with email addresses living in all cities with Housing Element agenda items during this period then received emails prior to their meeting.[8]

Treatment assignment

Identified renters in the voter file were randomly assigned to an email treatment encouraging them to submit a public comment at their city council meeting, or a placebo control. Individuals were block randomly assigned by city and cluster randomly assigned by address.[9] Treatment assignment probabilities were: 10% probability of assignment to a placebo message with no information on how to attend a meeting, and 30% probability of assignment to each of T1, T2, or T3.[10] All treatments included identical subject lines and preview texts to ensure equal compliance rates across treatment arms.

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[8]Recruitment starting and stopping dates were pre-registered. One council meeting in Santa Monica and two council meetings in Long Beach were selected for pilot studies, followed by pre-registration and treatment of individuals targeting meetings in the cities of (in chronological order) Beverly Hills, Santa Monica, Whittier, Rancho Palos Verdes, Manhattan Beach, Norwalk, Sierra Madre, and Culver City.

[9]While random assignment took place simultaneously for all cities, treatments were launched at different points in time for each city. If a unit number was available, clustering took place at the unit level. If a unit number was not available, clustering took place at the building level.

[10]Balance tables by treatment or placebo status, as well as for each treatment group can be found in Balance, and a map of all cities that received treatment can be found in Figure A3.

Outcomes

The primary, pre-registered outcome of interest is a binary indicator of whether an individual submitted a spoken *or* written comment. I match the names of those in the treatment groups with spoken or written comments using administrative records and video recordings of council meetings. I also examine *how* individuals commented by creating separate indicators for: spoken comments, written comments, comments that used our pre-written messages, custom comments, pro-housing comments, and anti-housing comments. In addition, I investigate whether the treatments changed the overall makeup of council meeting comments by comparing the number of pro-housing comments that were likely treatment induced with those that were not.[11]

Analytical procedures

The primary pre-registered estimand of interest is the complier average causal effect (CACE) of opening an email on submission of a public comment. In other words, the average treatment effect for individuals who opened the emails only (i.e., compliers). I employ a placebo-controlled design—rather than use assignment to treatment as an instrument—to mitigate statistical uncertainty (Broockman, Kalla and Sekhon 2017; Nickerson 2008). I estimate the CACE including pre-registered pre-treatment covariates using the estimator derived by Lin (2013).[12] Standard errors are clustered at the address level.

Results are analyzed as above (i.e., as one large experiment with city fixed effects), as well as aggregated using precision-weighted fixed effects and random effects meta-analysis. As the outcome data are "rare event" right-skewed binomial distributions (see Figure A9), I also calculate randomization inference based p-values (RI p) free from distributional assumptions

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[11]I define "likely treatment induced" comments as those submitted by individuals in the three treatment groups. This seems reasonable, as no comments were made by compliers in the placebo group.

[12]The included covariate are: *city, number of units in the building, gender, age, building age, primary language spoken, vote history, and party affiliation.* The Lin (2013) estimator performs OLS adjustment using treatment-by-covariate interactions and ensures that adjustment does not hurt asymptotic precision. Results without covariate adjustment are reported in Robustness.

and re-estimate all models using penalized maximum likelihood as robustness tests (see Table A13 and Table A14) (Cook, Hays and Franzese 2020; King and Zeng 2001).

I also examine pre-registered heterogeneous treatment effects by: building density, median area income, and turnout in the most recent local election. I regress comments on treatments and the treatment-covariate interaction, and use randomization inference as a robustness check. Readers interested in more detailed description of the procedures in this section can explore Analytical procedure details in the appendix.

Results

Across all council meetings,[13] the effect of opening any treatment email on submitting a public comment (i.e., the CACE) was 1.02 [RI p = 0.044; 95% CI 0.66, 1.38] percentage points (pp). The effect of being assigned to treatment (i.e., the ITT) on submitting a public comment was 0.19pp [RI p = 0.075, 95% CI 0.06, 0.31]. Both estimates are depicted in Figure 1. Estimates in tabular form and without covariate adjustment are reported in the appendix. Compliance rates by treatment group were 17% in placebo, 17% in T1, 16% in T2, and 18% in T3 (see Figure A4).
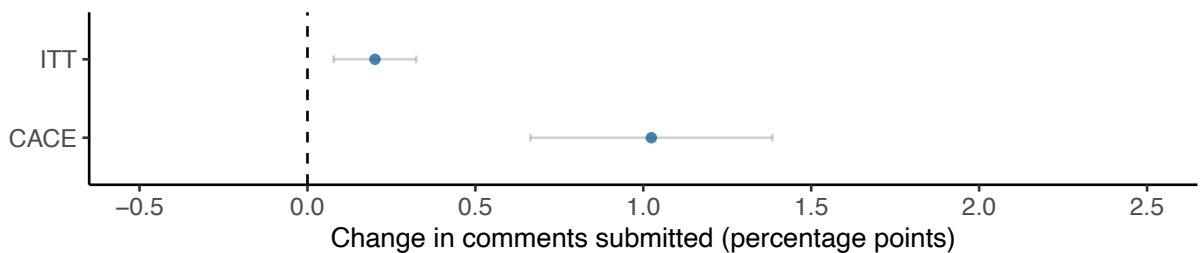

Figure 1: Intent-to-treat effect and complier average causal effect, all cities
Note: Tabular results can be found in Table A6 and Table A7

CACEs for individual council meetings can be found in Figure 2, which also contains meta-analytic estimates of the aggregate CACE.[14] Figure 2 also contains estimates from

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[13]Not including pilot studies.

[14]Precise null results in the Santa Monica pilot, Manhattan Beach, and Sierra Madre likely stem from small sample size. The Santa Monica pilot contained 91 opened emails (i.e., compliers), Manhattan Beach contained 70, and Sierra Madre contained 31. As 1 comment was submitted per 109 treated compliers across

three pilot studies, increasing the sample size to over 27,000 households. The point estimate using fixed effects meta-analysis including the pilot studies is 0.78 [95% CI 0.51, 1.06], and excluding the pilot studies is 0.91 [95% CI 0.56, 1.25] (see Figure A8).
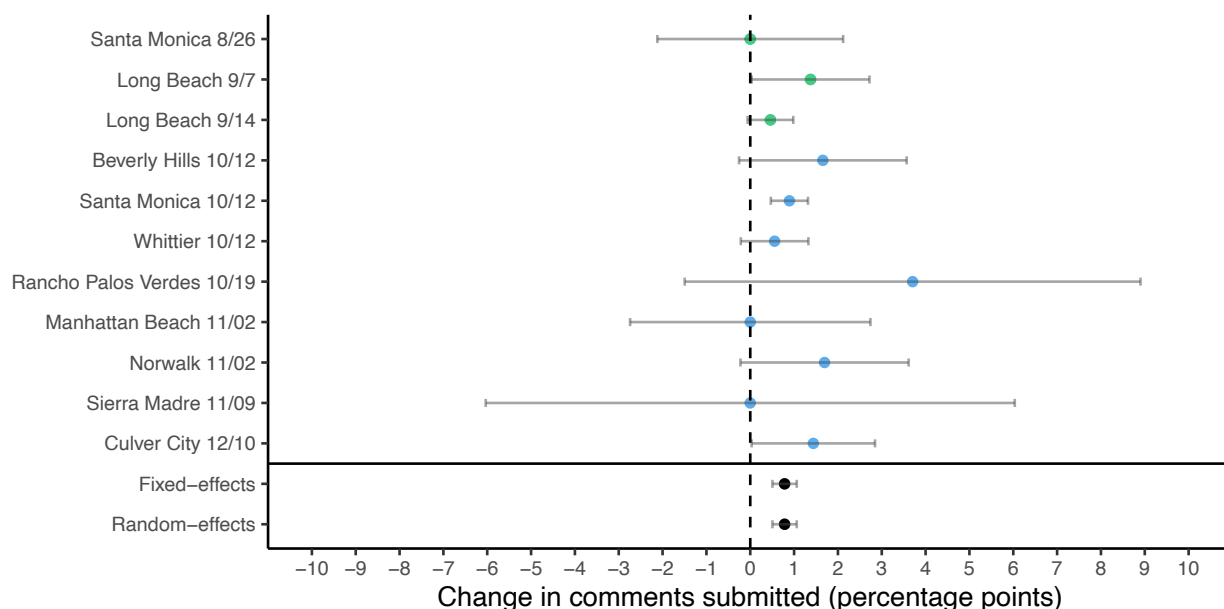

Figure 2: Meta-analysis of complier average causal effects, by council meeting

Note: Pilot studies in green. Tabular results can be found in Table A9 and Table A10.

By treatment group

In line with pre-registered hypotheses, Figure 3 shows that highlighting the costs of abstention (T3) had the largest effect on turnout (CACE = 1.44pp; RI p = 0.011; 95% CI [0.73, 2.15]), priming economic self interest (T2) was the second most effective (CACE = 1.01pp; RI p = 0.071; 95% CI [0.39, 1.63]), and the instructions-only treatment (T1) was the least effective (CACE = 0.54pp; RI p = 0.386; 95% CI [0.06, 1.03]).[15] This translates to 1 comment per 67 emails opened in T3, 1 per 96 in T2, and 1 per 201 in T1. T3 and T1 are significantly different from each other at the 5% level based on randomization inference and two-tailed linear hypothesis tests, while T2 and T1 are significantly different from each

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all cities, it is not unexpected to receive no comments in these cities.

[15]ITT randomization inference p-values are: 0.380 for T1, 0.089 for T2, and 0.039 for T3.

other at the 10% level based on one-tailed tests (see Table A8 and Table A13).[16] When grouped together, T2 and T3 are significantly different from T1 at the 5% level using both randomization inference and a two-tailed linear hypothesis test.
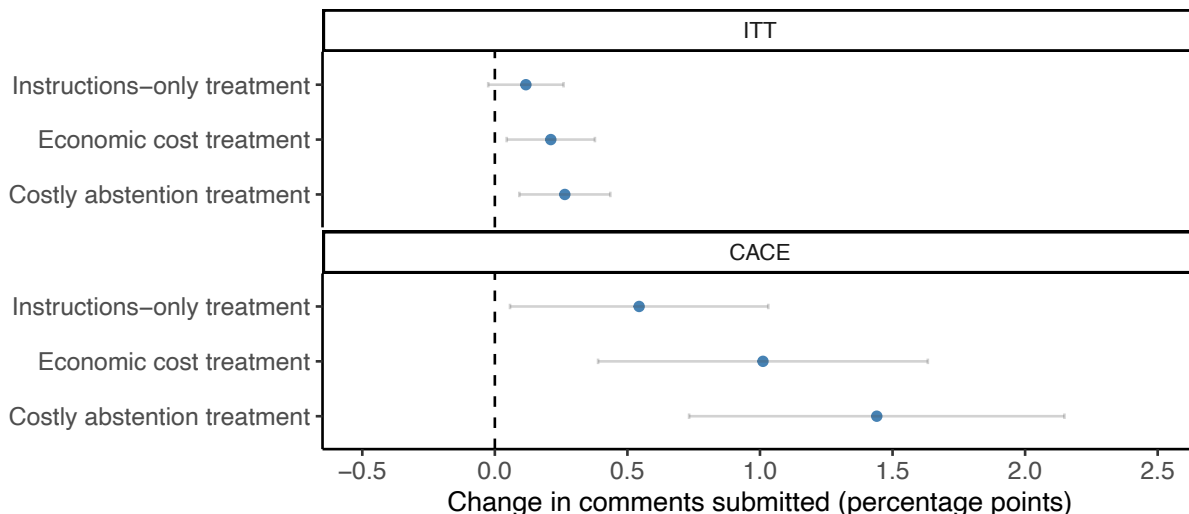

Figure 3: Effects by treatment group, all cities

Note: Tabular results can be found in Table A6 and Table A7

To further asses confidence the costly abstention treatment was most effective, I fit a Bayesian linear multilevel model using prior distributions from my pre-registration power analysis,[17] and compute Bayes factors[18] for hypotheses that the differences between treatments are greater than zero. This analysis suggests that the costly abstention treatment is 5 times as likely to be larger than the economic cost treatment than not, and 97 times as likely to be larger than the instructions only treatment than not.

These results align with the pre-registered theoretical predictions. Providing instructions of how to participate increased participation, but only marginally. Priming economic concerns appears to be more effective than lowering participation costs alone. The strongest evidence supports the theory that highlighting costs of abstention is more effective than lowering attendance costs alone. Additionally, the collective strength of the economic cost and

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[16]A one-tailed test may be justified due to pre-registration of the relative magnitudes of effect sizes.

[17]Coefficient estimates and posterior distributions can be found in Figure A10. Figure A11 depicts the posterior distributions of each coefficient and the differences between each coefficient.

[18]The ratio of the likelihood of one particular hypothesis to the likelihood of another hypothesis.

costly abstention treatments compared to the instructions-only treatment imply that economic or psychological motivators are more effective at driving participation than providing instructions or a clickable link alone.

Heterogeneous treatment effects

I find suggestive evidence that turnout in local elections is associated with a sizable increase in the likelihood of making a public comment. Voters were both more likely to open the emails (see Table A5) and 1.4pp more likely to comment than non-voters (see Figure 4, RI p = 0.06).[19] There is therefore suggestive evidence that participation in local politics in the form of voting begets willingness to participate in other forms. Further research is necessary to uncover the mechanisms behind this finding. It is possible that missing an opportunity to have one's voice heard may feel particularly costly for renters who already make the effort to participate in low-turnout municipal elections. Alternatively, renters who vote in local elections may already have a strong interest in local development, making them more responsive to treatment due to pre-existing interest in housing policy.
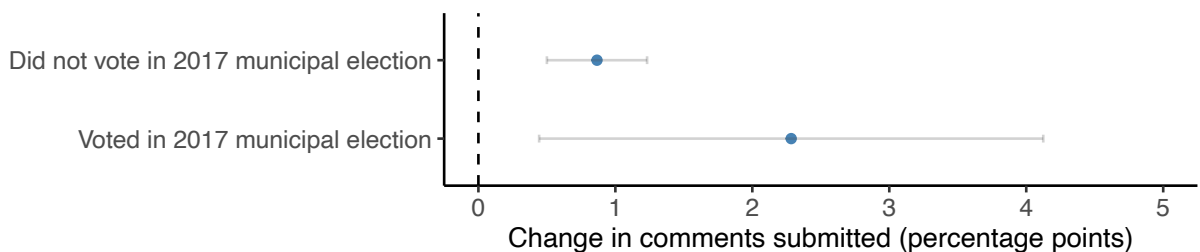
Figure 4: Complier average causal effects by turnout

Note: Tabular results can be found in Table A11.

Comment contents

I examine the content of each comment to determine if individuals submitted: spoken or written comments, custom comments or used the pre-written comment supplied in the emails, and pro or anti-housing comments (see Figure 5). The vast majority of individuals (93%)

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[19]The uncertainty of the estimates are a result of low turnout (9.4% amongst the sample population)

submitted written public comments, and the effect for spoken comments is only significant at the 10% level. However, even written submissions were not purely costless. While the majority of written comments used the sample message included in the email, 29% represented custom, personal comments. Many of these custom comments were deeply personal and reflected individuals' lived experiences with high housing costs.[20] For example, some discussed near experiences with homelessness, senior commenters discussed fear of being priced out of subsidized senior housing, and young renters lamented their inability to purchase a home like their parents. While some anti-housing comments were submitted, they represented only 4% of total comments, and never comprised a majority of experimentally-induced comments in any council meetings.
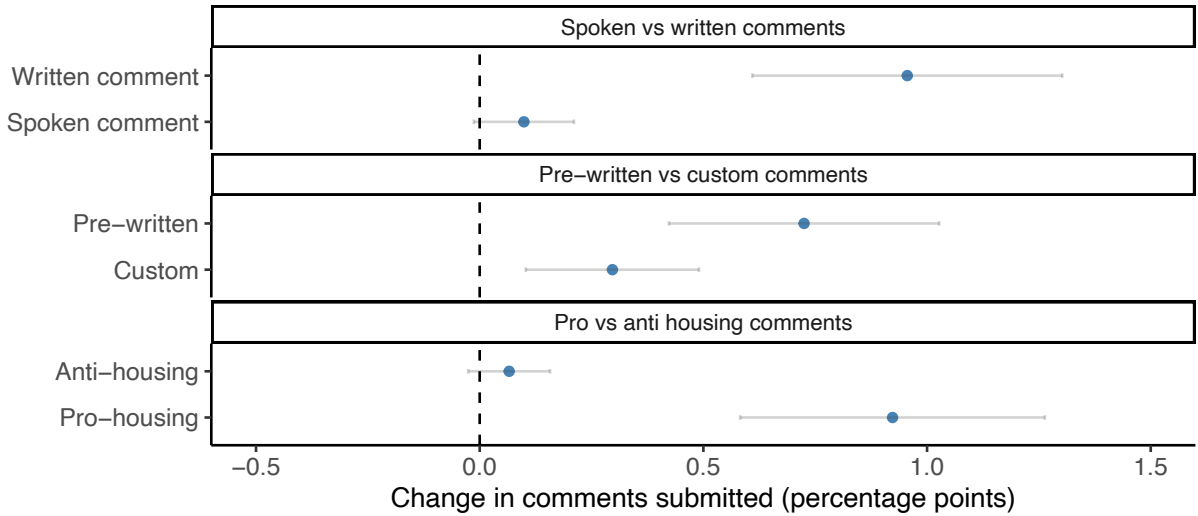
Figure 5: CACE by type of comment

Note: Tabular results can be found in Table A12.

Substantive impact of comments and changes in representation

I also investigate the substantive impact of the campaigns on each council meeting. Table 1 shows that the treatments meaningfully changed the quantity and composition of comments. Comments by treated individuals represented 8% of total written public comments across

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[20]I do not provide quotes of custom experimentally-induced comments as I did not ask for consent to re-print individuals' public comments.

all meetings, and 46% of all pro-housing comments. Treatment-induced comments swung the balance of pro-versus-anti housing comments toward a more equal footing, altering the imbalances of comment makeup highlighted by Yoder (2020) that were not changed merely by moving to an online setting (Einstein et al. 2021). The treatments therefore caused the makeup of council meeting comments to be more reflective of the broader public where remote access alone did not.

Meeting	Total comments (incl. treatment induced)	Pro-housing comments (not incl. treatment induced)	Pro-housing comments (incl. treatment-induced)	Anti-housing comments (incl. treatment-induced)
Beverly Hills 10/12	19	4	8	5
Santa Monica 10/12	67	15	38	11
Whittier 10/12	4	0	4	0
Rancho Palos Verdes 10/19	121	2	3	54
Manhattan Beach 11/02	225	0	0	0
Norwalk 11/02	7	0	3	0
Sierra Madre 11/09	20	0	0	8
Culver City 12/10	71	25	31	23
Total	534	46	85	101

Table 1: Examination of public comments in treated council meetings

These large effects of contact on overall turnout contrast sharply with, e.g., GOTV.[21] In voter turnout settings, the large number of individuals who regularly vote makes the change in overall turnout due to campaigns relatively small. By contrast, even a few new participants in city council meetings can meaningfully change the composition of comments due to low equilibrium participation rates. Council members in observed meetings also alluded directly to the overall makeup of public comments when discussing and voting on issue items, implying that they are aware of the tenor of comments.

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[21]The cost-effectiveness of the campaign also contrasts sharply with GOTV. Comments were generated at a cost of $4.80 per comment, compared with over $450 per vote in GOTV Facebook campaigns and $37 per vote in the most effective text messaging campaigns (Green and Gerber 2019).

Conclusion

Understanding how to motivate individuals to engage in personally costly collective action when gains from mobilization are long-term and uncertain is an enduring question in political economy. Homeowners with direct financial payoffs participate in local politics at disproportionately high rates. However, there is little evidence to suggest how to motivate those such as renters—who face long-term and uncertain payoffs—to participate.

I contribute to our understanding of how to motivate underrepresented groups to engage in costly political behavior using 8 email-outreach field experiments encouraging renters to participate in local politics in the form of commenting at city council meetings. In addition, I document how these campaigns changed the balance of participation in civic bodies. Three treatment arms tested the effectiveness of messages that: (1) lowered the costs of participation only, (2) primed economic self-interest, or (3) highlighted the costs of abstention. Receipt of any treatment increased public comments by 1pp, while highlighting the cost of abstention increased comments by 1.4pp. Individuals already engaged in local politics were more responsive to treatment. Treatment-induced comments represented 8% of total comments and 46% of pro-housing comments across all city council meetings. The treatments therefore overcame many of the traditional barriers to renter collective action, and changed the representation of civic bodies to be more reflective of the broader public.

The results support the following theoretical and substantive conclusions. First, unlike voting, email can effectively increase political participation when participation can also be conducted remotely, particularly amongst those already engaged in politics. Second, low-cost outreach strategies can meaningfully increase political participation in low-turnout and lesser-known settings such as city council meetings. Third, outreach can change the representation of civic bodies to be more reflective of the broader public where increases in accessibility alone—such as online access—do not. Fourth, informational outreach alone is not particularly effective, but increasing perceived costs of abstention appears to be an

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effective motivator of collective action.

Acknowledgements

Biographical statement

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Supporting Information

Housing net worth

Figure A1: Change in housing net worth by age and income percentile

Source: Glaeser and Gyourko (2018)

Voter file descriptive statistics

	Confirmed renter (N=6,411,84)		Not confirmed renter (N=5,045,990)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
Email	0.41	0.49	0.34	0.48	-0.07	<0.001
Phone	0.52	0.50	0.52	0.50	-0.005	<0.001
Age	43.39	17.70	47.84	18.90	4.46	<0.001
Years registered	3.98	6.53	6.29	9.82	2.31	<0.001
Female	0.54	0.50	0.53	0.50	-0.009	<0.001
Speak English	0.93	0.25	0.94	0.24	0.003	<0.001
CA native	0.48	0.50	0.54	0.50	0.07	<0.001
Democrat	0.57	0.49	0.52	0.50	-0.05	<0.001
Republican	0.11	0.31	0.18	0.38	0.07	<0.001
Independent	0.25	0.43	0.24	0.43	-0.01	<0.001
Voted in 2020 general election	0.69	0.46	0.74	0.44	0.05	<0.001
Voted in 2017 municipal election	0.10	0.30	0.14	0.35	0.04	<0.001
Voted in 2016 general election	0.43	0.49	0.53	0.50	0.10	<0.001

Table A1: Balance table: confirmed renters vs. non-confirmed renters

	Email listed (N=266,057)		Email not listed (N=3,751,27)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
Phone	0.80	0.40	0.32	0.47	-0.48	<0.001
Age	38.43	14.75	46.91	18.75	8.48	<0.001
Years registered	1.87	2.99	5.47	7.83	3.59	<0.001
Female	0.53	0.50	0.54	0.50	0.01	<0.001
Speak English	0.96	0.20	0.92	0.28	-0.04	<0.001
CA native	0.52	0.50	0.44	0.50	-0.08	<0.001
Year building constructed	1967.48	21.55	1966.61	20.93	-0.87	<0.001
Units in building	43.41	66.82	40.60	61.00	-2.81	<0.001
Democrat	0.59	0.49	0.56	0.50	-0.04	<0.001
Republican	0.10	0.30	0.11	0.32	0.01	<0.001
Independent	0.24	0.43	0.26	0.44	0.02	<0.001
Voted in 2020 general election	0.77	0.42	0.63	0.48	-0.13	<0.001
Voted in 2017 municipal election	0.09	0.29	0.11	0.31	0.02	<0.001
Voted in 2016 general election	0.40	0.49	0.45	0.50	0.05	<0.001

Table A2: Balance table: renters with emails listed in voter file vs. those without

Treatment messages

(a) **Placebo treatment message**

(b) **Instructions only treatment message**

(c) **Economic treatment message**

(d) **Costly abstention treatment message**

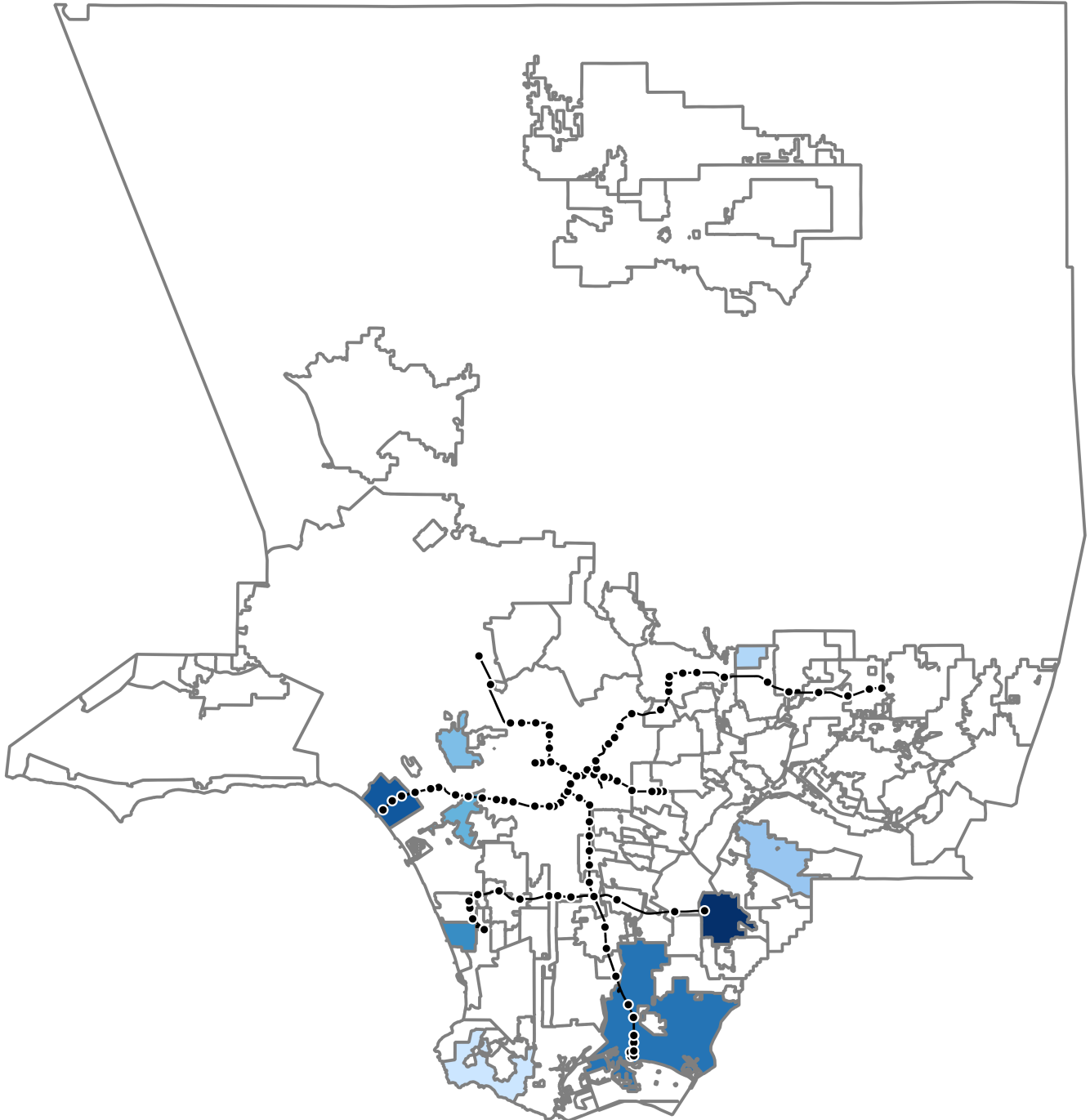**Figure A2: Example treatments and wording (Santa Monica experiment)**

Figure A3: Map of cities in Los Angeles county by experiment status

Note: Cities in which an experiment was launched in blue. Cities shaded by population density. Los Angeles Metro rail lines and rail stations in black.

Sample comment

Subject:

Public comment for [DATE] council meeting agenda item [ITEM NUMBER]

Body:

Dear City Council,

I'm writing to express my concern about our affordable housing shortage and its impact on the future of our city. Exclusionary zoning and land use practices have led to an undersupply of affordable medium- and high-density housing near jobs and transit, and have perpetuated segregated living patterns and the exclusion of historically disadvantaged communities.

[CITY] has an opportunity to address the need for more housing in a way that furthers equity, environmental sustainability, and economic recovery in its housing element update. We should update the housing element in a way that encourages historically high housing growth, while furthering fair housing opportunities and undoing patterns of discrimination in housing. We can't miss this opportunity to fix our city's housing crisis.

I urge you to legalize more housing, make housing easier to build, fund affordable housing and end homelessness, and strengthen tenants' rights.

Sincerely,

FIRSTNAME LASTNAME

Ethics

Any intervention motivating individuals to change their behavior should be held to high ethical standards, particularly when the intervention involves participation in and effects on governmental processes. Beyond IRB approval, I argue this project falls within ethical bounds for the reasons outlined below.

First, these messaging campaigns are commonly conducted by political campaigns and nonprofit organizations, and individuals in the voter file therefore would have received messages with or without researcher randomization and measurement.

Second, the interventions are designed to minimize a pre-existing imbalance in representation by increasing representation amongst a historically underrepresented group. Treatments are designed to encourage renters to participate (albeit not coercively) and make local governance more reflective of the general population.

Third, the interventions do not directly effect electoral outcomes (as highlighted by **?** and **?**). I recognize that local officials may change their votes based on perceived changes in support levels that the experiment might cause. However, ultimate decisions and votes still rest with local elected officials.

Fourth, the interventions focus on increasing the supply of housing generally across the LA region, not on particular developments or neighborhoods. Treatment and sample messages also specifically encourage individuals to advocate for *affordable* (i.e., government subsidized) housing developments. We should therefore expect the targeted groups to benefit from the research through decreased rents and increased access to affordable housing.

Fifth, in social-welfare enhancing interventions such as "green nudges," **?** and **?** argue that it should be possible "for everyone who is watchful to unmask the manipulation." The interventions meet this criteria, as the messages come from an advocacy group that is transparent in their motivations.

While informed consent was not received from individuals prior to treatment, the research is: (1) minimal risk compared to similar outreach emails that individuals who listed their

email addresses in the voter file would otherwise receive without researcher measurement, (2) permission to obtain the voter file and conduct the research was obtained from the Los Angeles County Registrar in addition to a university IRB, (3) individuals would have received similar messages from advocacy organizations with or without researcher measurement, (4) treatment messages noted that they were part of a "collaboration between Abundant Housing Los Angeles and academic researchers at [redacted for peer review]" and were transparent in motivation, and (5) participant behavior may have changed if subjects were aware they were part of an academic study. The only potential deception was therefore anonymized data collection for the purpose of measurement.

Analytical procedure details

By randomly assigning individuals to a placebo control with no mention of council meetings, but featuring the same subject line and preview text as the treatment emails, I am able to observe the outcomes of a random sample of compliers (email openers) in the placebo group. Email opens are monitored using software that detects whether an individual opens a message. Tests for differential compliance by treatment group and differential covariate predictiveness of compliance can be found in Figure A4 and Table A5.

For the primary estimand (i.e., the CACE), I estimate the OLS specifications below:

$$Y_i = \alpha + \beta_1 Z_i + \beta_2 X_i^c + \gamma X_i^c Z_i + \delta_{city} + \epsilon_i \qquad \text{(With Lin (2013) covariate adjustment)}$$

$$Y_i = \alpha + \beta_1 Z_i + \delta_{city} + \epsilon_i \qquad \text{(Without covariate adjustment)}$$

where Y_i is the individual-level comment outcome, Z_i is an indicator for the treatment group, X_i^c is a vector of pre-treatment covariates for unit i that have been centered to have mean zero, and δ_{city} are city (block) fixed effects.

The following pre-registered pre-treatment covariates are included in the regression specification: *city, number of units in the building, gender, age, building age, primary language spoken, vote history, and party affiliation.* I show that these variables are balanced between the placebo and treatment groups in Balance. Missing covariates are mean imputed.

Randomization inference p-values for the ITT are calculated by simulating a large number of "fake" random assignments for all units using the same procedure as the real random assignment, and estimating a treatment effect for each fake random assignment. I then calculate a p value as the proportion of times fake treatment assignments resulted in an effect size larger than the actual treatment effect. For the CACE, I make the additional assumption that observed compliance would exist regardless of treatment status and hold compliers constant across simulations. I conduct 10,000 simulations for the CACE and 1000 simulations for the ITT. All simulations were performed without covariate adjustment

due to high computational demands. For CATEs, I generate the full schedule of potential outcomes under the null hypothesis that the true treatment effect is constant and equal to the estimated CACE. Then, I simulate random assignment 10,000 times and calculate the proportion of instances the simulated estimate of the interaction effect is at least as large (in absolute value) as the actual estimate.

In the precision-weighted fixed effects meta-analysis, weights are equal to the inverse of the variance. For council meetings where no comments are reported in treatment or placebo, I estimate standard errors according to the procedure described in **?**. See p. 17, footnote 1: "Consider a survey of size n with y Yes responses and $n - y$ No responses. The estimated proportion of the population who would answer Yes to this survey is $\hat{p} = y/n$, and the standard error of this estimate is $\sqrt{\hat{p}(1-\hat{p})/n}$. This estimate and standard error are usually reasonable unless $y = 0$ or $n - y = 0$, in which case the resulting standard error estimate of zero is misleading. A reasonable quick correction when y or $n - y$ is near zero is to use the estimate $\hat{p} = (y+1)/(n+2)$ with standard error $\sqrt{\hat{p}(1-\hat{p})/n}$."

Note that while replication code is available for the creation of the identified renter sample (i.e., merging the voter file with Los Angeles Department of City Planning records of multi-unit housing developments), the full voter file cannot be provided for both legal and ethical reasons. However, all data used in the analyses described in this section are available in anonymized form.

Balance

	Placebo (N=2007)		Treatment (N=17944)			
	Mean	SD	Mean	SD	Diff. in Means	p value
Female	0.52	0.50	0.53	0.50	0.02	0.11
Speak English	0.98	0.12	0.98	0.14	0.00	0.27
Age	41.60	15.76	41.25	15.62	-0.37	0.31
Year building constructed	1964.93	18.63	1964.83	18.03	-0.14	0.75
Units in building	34.25	64.90	34.39	66.40	0.08	0.96
Democrat	0.57	0.49	0.58	0.49	0.01	0.41
Republican	0.13	0.33	0.11	0.32	-0.01	0.21
Independent	0.24	0.43	0.24	0.43	0.00	0.73
Voted in 2020 general election	0.79	0.40	0.81	0.40	0.01	0.28
Voted in 2017 municipal election	0.10	0.30	0.09	0.29	-0.01	0.28
Voted in 2016 general election	0.45	0.50	0.44	0.50	0.00	0.75

Table A3: Covariate balance and difference in means test: treatment vs. placebo

	Placebo (N=2007)		Treatment 1 (N=5984)		Treatment 2 (N=6002)		Treatment 3 (N=5958)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Female	0.52	0.50	0.52	0.50	0.54	0.50	0.54	0.50
Speak English	0.98	0.12	0.98	0.14	0.98	0.13	0.98	0.14
Age	41.60	15.76	41.16	15.61	41.35	15.63	41.23	15.62
Year building constructed	1964.93	18.63	1964.83	17.88	1964.83	18.33	1964.84	17.88
Units in building	34.25	64.90	34.31	66.10	34.01	66.54	34.86	66.56
Democrat	0.57	0.49	0.58	0.49	0.60	0.49	0.58	0.49
Republican	0.13	0.33	0.11	0.32	0.11	0.31	0.12	0.33
Independent	0.24	0.43	0.25	0.43	0.24	0.43	0.24	0.43
Voted in 2020 general election	0.79	0.40	0.80	0.40	0.81	0.40	0.81	0.39
Voted in 2017 municipal election	0.10	0.30	0.09	0.29	0.10	0.30	0.09	0.29
Voted in 2016 general election	0.45	0.50	0.45	0.50	0.45	0.50	0.43	0.50

Table A4: Covariate balance across all treatment groups
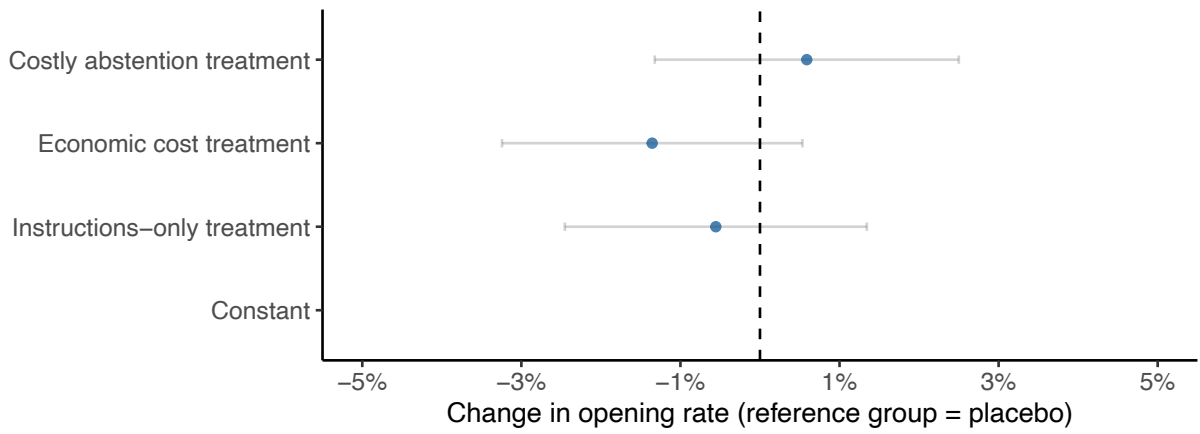
Tests for differential compliance

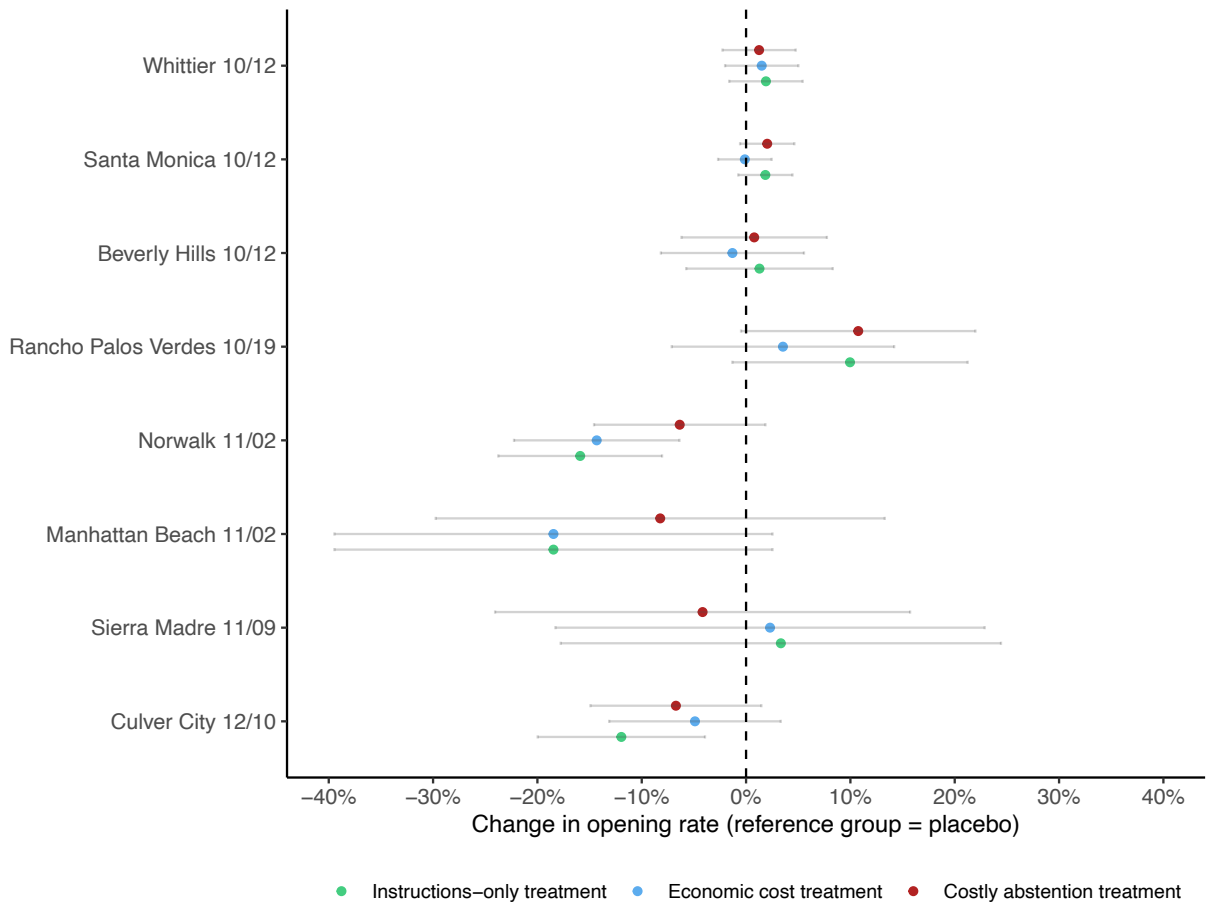**Figure A4: Average treatment effect on email opening, all cities**

Figure A5: Average treatment effect on email opening, by city

	Placebo	Treatment 1	Treatment 2	Treatment 3
(Intercept)	−0.321	−0.535	−0.565	0.216
	(0.980)	(0.569)	(0.560)	(0.563)
Female	−0.028	0.004	−0.012	−0.004
	(0.017)	(0.010)	(0.010)	(0.010)
Speak English	0.009	0.045	−0.020	−0.042
	(0.069)	(0.031)	(0.037)	(0.040)
Age	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)
Year building constructed	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Units in building	0.000	0.000*	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Democrat	0.033	0.012	0.033+	0.030
	(0.033)	(0.020)	(0.019)	(0.021)
Republican	0.021	−0.008	0.003	−0.009
	(0.039)	(0.023)	(0.023)	(0.024)
Independent	0.054	0.000	0.017	0.011
	(0.036)	(0.021)	(0.021)	(0.022)
Voted in 2020 general election	0.028	0.031**	0.062***	0.030*
	(0.021)	(0.012)	(0.011)	(0.013)
Voted in 2017 municipal election	0.041	0.057**	0.040*	0.035+
	(0.033)	(0.020)	(0.018)	(0.019)
Voted in 2016 general election	−0.006	0.012	0.002	−0.019+
	(0.019)	(0.011)	(0.010)	(0.011)
Number of observations	2007	5984	6002	5958

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A5: Covariate predictiveness of compliance by treatment group

Tabular results

	All treatment groups vs. placebo		Individual treatments vs. placebo	
Constant	0.0005	0.0005	0.0005	0.0005
	(0.0005)	(0.0013)	(0.0005)	(0.0013)
	[−0.0005, 0.0015]	[−0.0022, 0.0031]	[−0.0005, 0.0015]	[−0.0022, 0.0031]
Treated	0.0020**	0.0020**		
	(0.0006)	(0.0006)		
	[0.0008, 0.0032]	[0.0007, 0.0032]		
Instructions-only treatment			0.0012	0.0011
			(0.0007)	(0.0007)
			[−0.0003, 0.0026]	[−0.0003, 0.0026]
Economic cost treatment			0.0021*	0.0021*
			(0.0008)	(0.0009)
			[0.0004, 0.0038]	[0.0004, 0.0038]
Costly abstention treatment			0.0026**	0.0027**
			(0.0009)	(0.0009)
			[0.0009, 0.0044]	[0.0009, 0.0044]
Covariate adjustment:	Yes	No	Yes	No
Num.Obs.	19 951	19 951	19 951	19 951

Notes: Standard errors clustered at the address level in parentheses. 95 percent confidence intervals in brackets.

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A6: Intent-to-treat effects

	All treatment groups vs. placebo		Individual treatments vs. placebo	
Constant	0.0000	0.0061	0.0000	0.0063
	(0.0000)	(0.0086)		(0.0086)
	[0.0000, 0.0000]	[−0.0107, 0.0230]		[−0.0106, 0.0231]
Treated	0.0102***	0.0104***		
	(0.0018)	(0.0019)		
	[0.0066, 0.0138]	[0.0066, 0.0141]		
Instructions-only treatment			0.0054*	0.0052*
			(0.0025)	(0.0023)
			[0.0006, 0.0103]	[0.0006, 0.0098]
Economic cost treatment			0.0101**	0.0106**
			(0.0032)	(0.0033)
			[0.0039, 0.0163]	[0.0041, 0.0171]
Costly abstention treatment			0.0144***	0.0148***
			(0.0036)	(0.0037)
			[0.0073, 0.0215]	[0.0075, 0.0222]
Covariate adjustment:	Yes	No	Yes	No
Num.Obs.	3381	3381	3381	3381

Notes: Standard errors clustered at the address level in parentheses. 95 percent confidence intervals in brackets.

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A7: Complier average causal effects

	p value	
	Two-tailed	One-tailed
Economic cost > Instructions only	0.165	0.082
Costly abstention > Economic cost	0.391	0.196
Costly abstention > Instructions only	0.025	0.013
Costly abstention and economic cost > Instructions only	0.026	0.013

Table A8: Linear hypothesis tests

Meeting	CACE	95% CI	N
Pilot studies			
Santa Monica 8/26	0	[-2.119 , 2.119]	91
Long Beach 9/7	1.375	[0.031 , 2.719]	346
Long Beach 9/14	0.460	[-0.061 , 0.981]	727
Primary studies			
Beverly Hills 10/12	1.656	[-0.256 , 3.568]	194
Santa Monica 10/12	0.893	[0.47 , 1.317]	2,102
Whittier 10/12	0.556	[-0.216 , 1.327]	396
Rancho Palos Verdes 10/19	3.704	[-1.495 , 8.902]	57
Manhattan Beach 11/02	0	[-2.742 , 2.742]	70
Norwalk 11/02	1.695	[-0.223 , 3.613]	213
Sierra Madre 11/09	0	[-6.034 , 6.034]	31
Culver City 12/10	1.439	[0.031 , 2.847]	318

Note: Standard errors in parenthesis. Figures rounded to nearest thousandth decimal place. N is equal to the number of compliers in each city.

Table A9: CACEs for each city council meeting

Value	Estimate	95% CI	N
Weighted fixed effects, w/ pilot studies	0.008 (0.001)	[0.005 , 0.011]	4545
Random effects, w/ pilot studies	0.008 (0.001)	[0.005 , 0.011]	4545
Weighted fixed effects, w/o pilot studies	0.009 (0.002)	[0.006 , 0.012]	3381
Random effects, w/o pilot studies	0.009 (0.002)	[0.006 , 0.012]	3381

Note: Standard errors in parenthesis. N is equal to the number of compliers.

Table A10: Meta-analysis estimates

	CATE
Constant	0.006
	(0.009)
Treated	0.009***
	(0.002)
Voted in 2017 municipal election	0.000
	(0.001)
Treated x Voted	0.014+
	(0.008)
City fixed effects:	Yes
Num.Obs.	3381

Notes: CATE standard errors clustered at the address level.

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A11: Conditional complier average causal effect

Comment type	Spoken	Written	Pro-housing	Anti-housing	Custom	Pre-written
Constant	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated	0.001+	0.010***	0.009***	0.001	0.003**	0.007***
	(0.001)	(0.002)	(0.002)	(0.000)	(0.001)	(0.002)
Num.Obs.	3381	3381	3381	3381	3381	3381

Notes: Standard errors clustered at the address level.

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

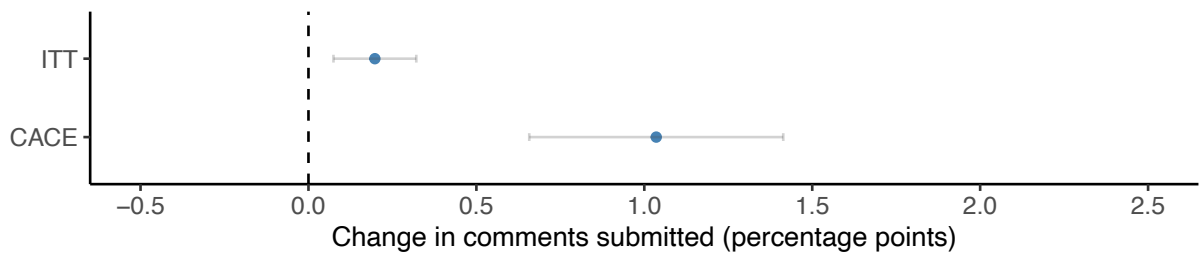**Table A12: Complier average causal effects by outcome**

Robustness

Figure A6: Intent-to-treat effect and complier average causal effect, all cities (without covariate adjustment)
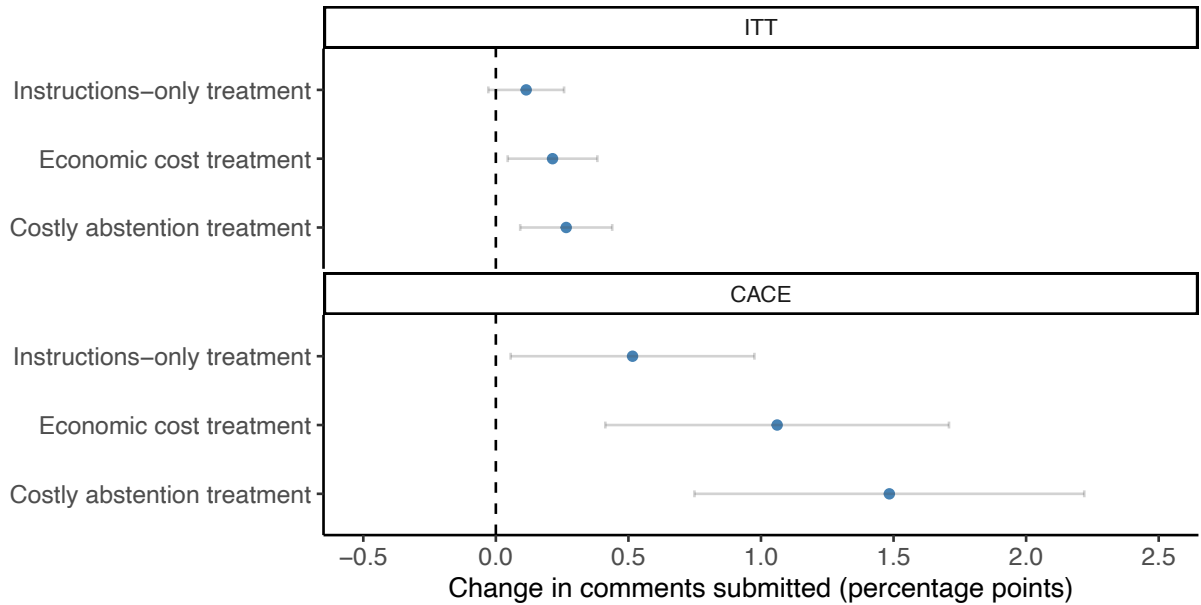

Figure A7: Effects by treatment group, all cities (without covariate adjustment)

Estimand		p value
CACE:	All treated vs. placebo	0.044
CACE:	Instruction-only vs. placebo	0.386
CACE:	Economic cost vs. placebo	0.071
CACE:	Costly abstention vs. placebo	0.011
CACE:	Economic cost vs. instruction-only	0.198
CACE:	Costly abstention vs. instruction-only	0.021
CACE:	Costly abstention vs. economic cost	0.326
CACE:	Costly abstention & economic cost vs. instructions-only	0.034
ITT:	All treated vs. placebo	0.075
ITT:	Instruction-only vs. placebo	0.380
ITT:	Economic cost vs.placebo	0.089
ITT:	Costly abstention vs.placebo	0.039
ITT:	Economic cost vs. instruction-only	0.266
ITT:	Costly abstention vs. instruction-only	0.082
ITT:	Costly abstention vs. economic cost	0.565
ITT:	Costly abstenion & economic cost vs. instructions-only	0.086

Table A13: Randomization inference p values

Note: Randomization inference conducted using 10,000 simulations for CACEs and 1000 simulations for ITTs. Covariates not included due to computational demand.

	All treatment groups vs. placebo		Individual treatments vs. placebo	
	ITT	CACE	ITT	CACE
Constant	−7.1987***	−6.5439***	−7.1987***	−6.5439***
	(0.8170)	(1.4173)	(0.8170)	(1.4173)
	[−9.3648, −5.9318]	[−11.3781, −4.6301]	[−9.3648, −5.9318]	[−11.3781, −4.6301]
Treated	1.2239+	1.9864*		
	(0.8304)	(1.4285)		
	[−0.0850, 3.4045]	[0.0265, 6.8285]		
Instructions-only treatment			0.8548	1.3414
			(0.8735)	(1.4804)
			[−0.5931, 3.0816]	[−0.8391, 6.2197]
Economic cost treatment			1.3048+	2.0372+
			(0.8534)	(1.4509)
			[−0.0776, 3.5102]	[−0.0157, 6.8950]
Costly abstention treatment			1.4797*	2.3874*
			(0.8479)	(1.4388)
			[0.1150, 3.6792]	[0.3850, 7.2367]
Num.Obs.	19 951	3381	19 951	3381

Notes: Standard errors clustered at the address level in parentheses. 95 percent confidence intervals in brackets.

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

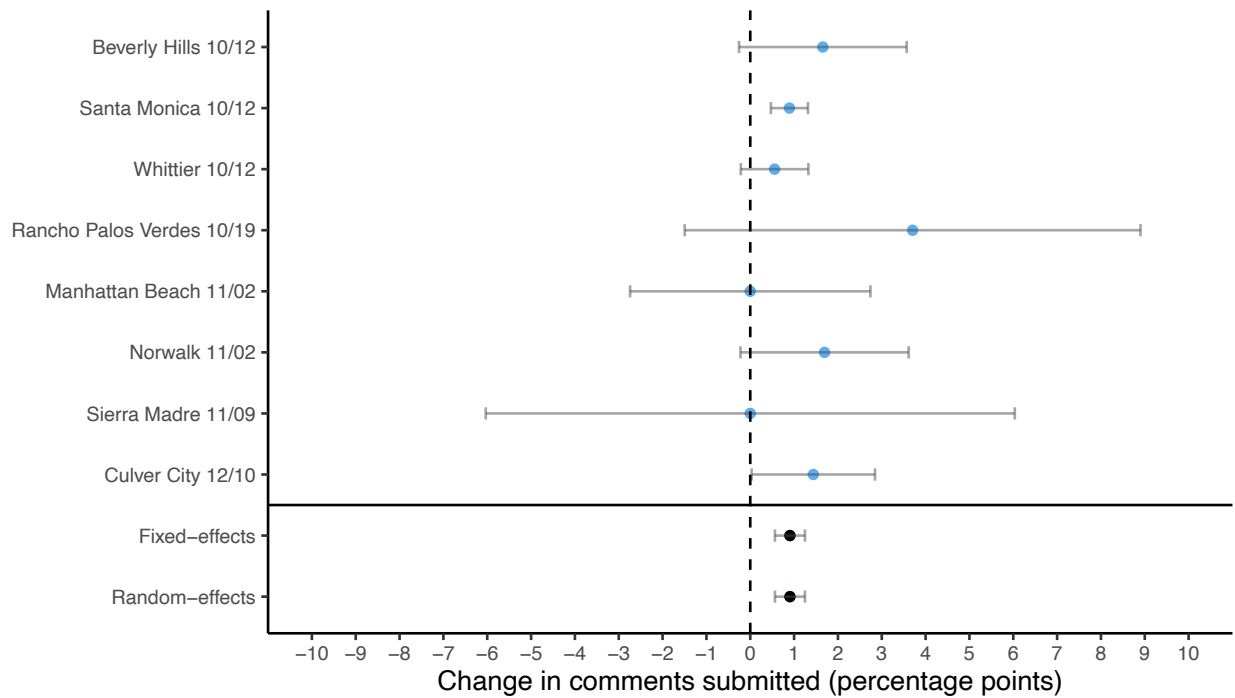**Table A14: ITT and CACE estimates from penalized maximum likelihood**

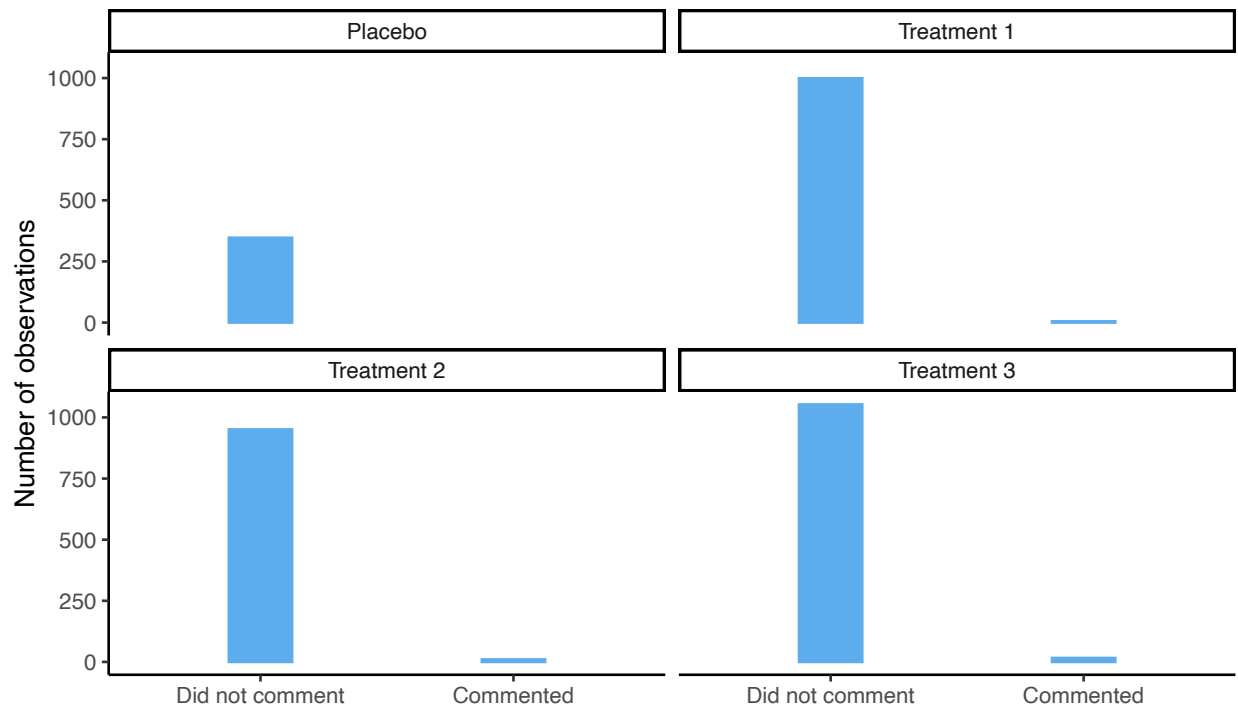Figure A8: Meta-analysis of complier average causal effects by city, excluding pilot studies

Figure A9: Distribution of outcomes by treatment group (compliers only)

The Bayes factors in the results section are computed for hypotheses that the differences between treatments are greater than zero (e.g., costly abstention treatment - instructions only treatment > 0) and its alternative using the Savage-Dickey density ratio method. The Bayes factors are 97 and 5 for the costly abstention treatment vs. the instructions only treatment and costly abstention treatment vs. economic cost treatment, respectively. The posterior probability exceeds 95% for a one-sided hypothesis test in both comparisons, and exceeds 95% for a two-sided test in the first comparison. Given that the directionality and relative magnitudes of the treatment effects were pre-registered and negative treatment effects are theoretically implausible, a one-sided hypothesis test seems reasonable.
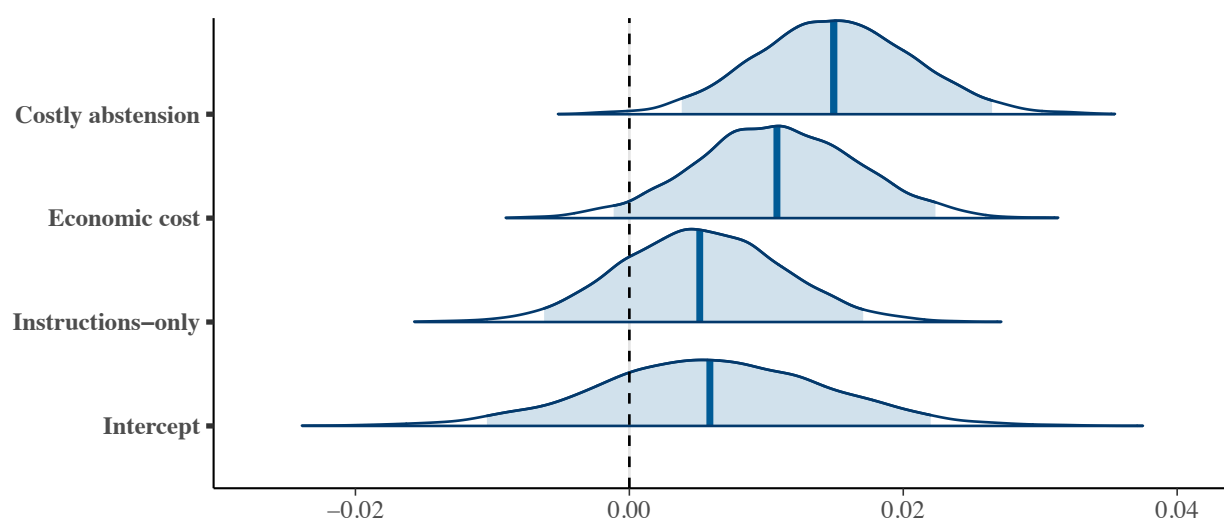

Figure A10: Bayesian multilevel model: coefficient estimates and posterior distributions (includes city fixed effects)
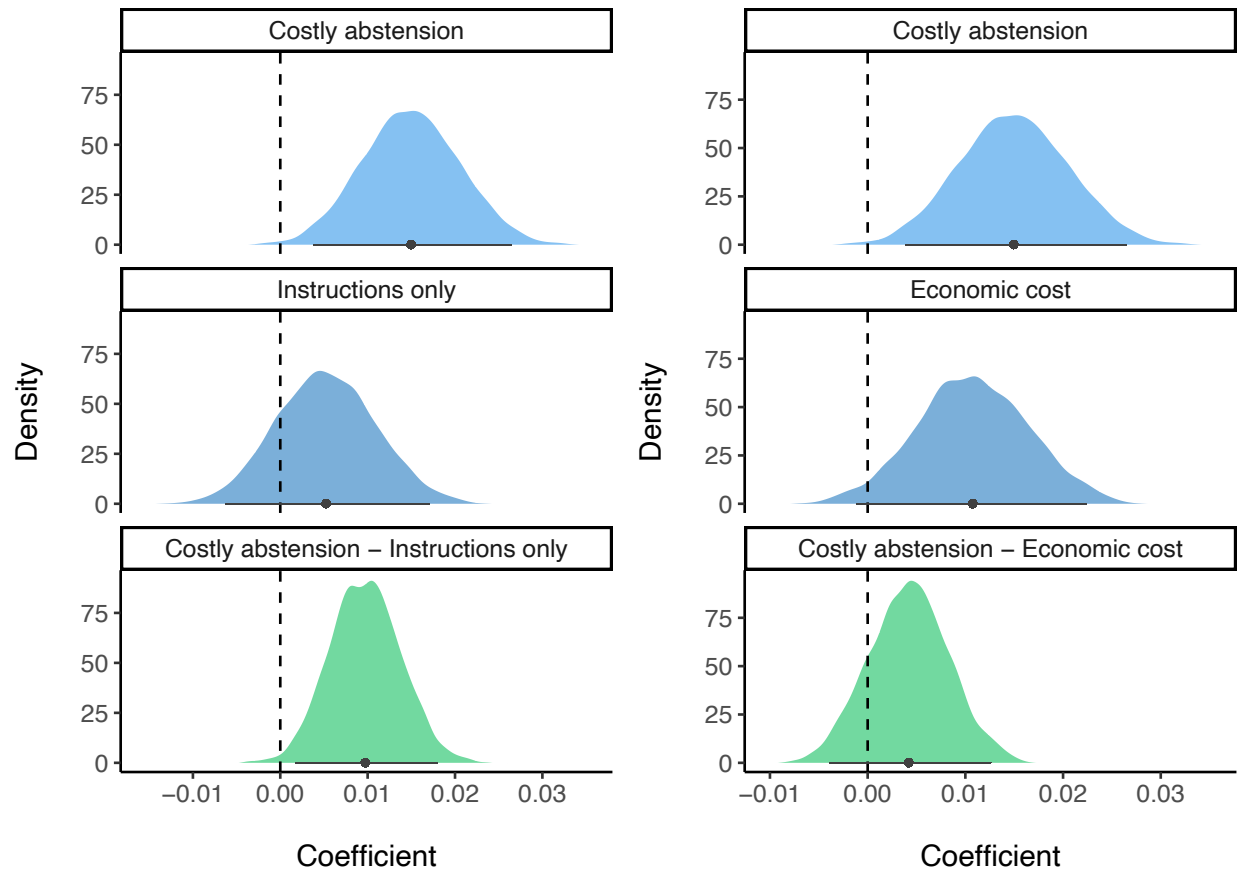
Figure A11: Posterior distributions of costly abstention treatment, instructions only treatment, and difference