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.

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Keywords: Field experiments; collective action; local politics; housing policy

*The human subject protocol of the research was evaluated and approved by an ethics committee at Yale University (IRB Protocol ID #2000030461). The research design and analyses were pre-registered at: https://osf.io/c84j7. Supplementary material for this article is available in the appendix in the online edition. Replication files are available in the JOP Dataverse (https://dataverse.harvard.edu/dataverse/jop) and on GitHub (https://github.com/tincerti). The empirical analysis has been successfully replicated by the JOP replication analyst. Any and all errors are my own.

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Homeowners are more active in local politics in the United States than renters (Einstein, Palmer and Glick 2019; Hall and Yoder 2022; Yoder 2020). This participation gap is reflected in land use policies that prioritize homeowners' economic interests (Einstein, Glick and Palmer 2019; Fischel 2005; Marble and Nall 2021). Yet these policies also decrease access to housing and increase rents (Glaeser and Gyourko 2018; Glaeser, Gyourko and Saks 2005), giving renters an incentive to participate in local politics and support housing growth.

Differences in the nature of homeowner and renter economic incentives partly explain the participation gap. Self-interest typically motivates political behavior only when benefits are "tangible, large, visible, and certain" (Citrin, Reingold and Green 1990). Homeowners tangibly benefit from halting nearby development, preserving property values, while increased housing gradually lowers rents regionally. How can those with uncertain, long-term gains (such as renters) be motivated to engage in 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 received emails encouraging them to comment at their city council meetings and support pro-housing regulatory policies. Three mechanisms of mobilization were tested: attendance instructions, economic self-interest emphasis, and highlighting costs of political inaction. Receipt of any treatment increased public comments by 1 percentage point (pp), with a 1.4pp increase when emphasizing costs of abstention. Local election voters were more responsive (2.3pp) than non-voters (0.9pp). These effects are substantively large as council meeting attendance is typically low. Treatment-induced comments made up 8% of all comments and 46% of pro-housing comments in treated meetings, and over 50% of treated meetings had majority pro-housing comments.

The results suggest that in contrast with voter turnout, low-cost outreach like email can meaningfully boost political participation in remote settings like city council meetings. Outreach to underrepresented groups can therefore make low-turnout 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 an 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 homeownership increases participation in city council, planning, and zoning meetings (Hall and Yoder 2022; Yoder 2020). Examination of mechanisms suggests homeowner participation is consistent with rational economic behavior and protection of property values (Einstein, Palmer and Glick 2019; Hall and Yoder 2022; Hankinson 2018; Marble and Nall 2021; McCabe 2016; Yoder 2020). By contrast, renters do not consistently oppose new housing (Marble and Nall 2021; Monkkonen and Manville 2019), leading to disparities between council meeting comments in favor of more housing and ballots cast in favor of more housing (Einstein, Palmer and Glick 2019). The makeup of local political participation in majority-renter cities therefore often does not reflect general public opinion. However, it remains unclear if making the economics of housing policy salient for renters would increase their participation.

Encouraging remote political participation

COVID-19 opened city council participation to email, phone, or video call, but remote access did not reduce participatory gaps (Einstein, Glick, Puig and Palmer 2023). Prior research offers lessons and conflicting predictions for encouraging remote political participation.

Experimental research largely finds digital outreach ineffective for in-person political mobilization. Yet the impact of digital outreach on remote political participation is underexplored. Exceptions are absentee voting and online voter registration, where email outreach was ineffective (Green and Gerber 2019). However, digital outreach may boost remote expressive political participation—such as petition signatures and small donations—through personalized appeals (Coppock, Guess and Ternovski 2016; Gaynor and Gimpel 2021). An expressive action like public comment may therefore also be responsive to digital outreach.

In-person campaigns offer insights for successful appeals. Field experiments suggest

[1] Similar outreach campaigns could also be used on different populations to increase participatory gaps.

merely providing information that one *can* participate does not increase voter turnout (Green and Gerber 2019). However, providing a clear plan for participation has proven effective (Nickerson and Rogers 2010). Given renters' lower local political involvement (Ansolabehere 2012; McCabe 2016), offering them guidance on how to participate and providing a direct, clickable public comment link may facilitate their engagement. Additional research suggests economic motivations drive political participation. Homeowner participation is hypothesized to be driven by economic self-interest, as blocking development can have a large, immediate impact on nearby property values. However, as benefits to renters are longer term and less tangible, it is unclear if economic motivators will increase renter participation (Citrin, Reingold and Green 1990). I therefore test if priming economic self-interest can increase renter participation, despite lack of a tangible asset such as a home. Other studies suggest psychic motivators are most effective at driving participation (Citrin, Green, Muste and Wong 1997). Aytaç and Stokes (2019) posit that high psychological costs of abstention combined with low participation costs maximize collective action. I test this theory with messaging that highlights lack of renter participation as a contributor to policy capture and personal economic harm, increasing the perceived cost of abstention.

The theory above leads to three hypotheses of motivation to collective action, which are tested with three treatment messages. Treatment 1 (T1) reduces participation costs with detailed instructions, but effects may be minimal. All treatments thus provide a Zoom link for spoken comments and an email link for submitting a sample written comment (while noting individuals may draft their own comment).[2] Treatment 2 (T2) primes economic self interest by providing information that lack of housing supply increases rents and should increase attendance more than instructions only. Treatment 3 (T3) highlights costs of abstention and should increase attendance more than instructions or economic-self interest alone.

Past research presents conflicting theories on encouraging remote political participation. Digital outreach may prove ineffective in real-world settings, regardless of digital access.

[2]See "sample comment" in the appendix for wording of the sample message.

Alternatively, expressive participation like public comment may respond to outreach with the right appeals. I adjudicate this debate and offer empirical and theoretical advancements to the literature on political participation. First, I document the real-world response of a low-participation group to instructional, economic, and psychological motivators to collective action, and show that highlighting costs is more effective than instructional appeals. Second, I challenge conclusions that digital outreach is a poor motivator by looking beyond voting to a domain where expressive real-world political participation can be conducted remotely.

Research design and analytical procedures

The experiment was fielded in LA County in collaboration with a pro-housing NGO as cities updated their 2021-2029 "Housing Elements" (HE)—a required analysis of a city's housing needs and strategies to meet those needs—and targeted city council meetings with the HE on the agenda. Written comments could be submitted by email and were read during or distributed to council members prior to the meeting. Council members should therefore be aware of the sentiments in spoken and written comments. Interventions involving governmental processes should be held to high ethical standards. For discussion of research ethics, please see "Ethics" in the appendix.

The experiment proceeded as follows: (1) renters in the voter file were identified using City Planning records, (2) council meetings discussing the HE were identified for the messaging campaign, (3) renters were randomly assigned to one of the three email treatments encouraging comment or a placebo control, (4) names in treatment groups were matched with names of those who submitted a public comment, (5) pre-registered analysis was performed.

Renters were identified by matching addresses in the LA County voter file with City Planning records of multi-unit apartment buildings using the FastLink linkage algorithm (Enamorado, Fifield and Imai 2019). This yielded 641,184 matched renters, including 266,057 with listed email addresses. City council meetings in LA County were monitored for HE agenda items throughout fall and winter 2021, and renters with email addresses living in cities with HE agenda items during this period received emails prior to their meeting.

Identified renters were randomly assigned to an email treatment encouraging public comment at their city council meeting, or a placebo control. Individuals were block randomly assigned by city and cluster randomly assigned by address. Assignment probabilities were 10% in placebo and 30% each for T1, T2, or T3.[3] All treatments included identical subject lines and preview texts to ensure equal compliance rates across treatment arms.

The primary outcome is a binary indicator of whether an individual submitted a spoken *or* written comment. Names in treatment groups were matched with comments using administrative records and video recordings. I also examine the nature of comments by creating separate indicators for spoken, written, pre-written, custom, pro-housing, and anti-housing comments. Further, I investigate if the treatments changed overall comment makeup by comparing the number of comments that were treatment-induced with those that were not.[4]

The primary estimand is the Complier Average Causal Effect (CACE) of email opening on public comment submission. I use a placebo-controlled design—rather than assignment to treatment as an instrument—to mitigate statistical uncertainty. I estimate the CACE including pre-treatment covariates using the estimator derived by Lin (2013).[5] Standard errors are clustered at the address level. Results are analyzed as one experiment with city fixed effects as well as aggregated using meta-analysis. As the outcomes are "rare event" right-skewed binomial distributions (see Figure A13), I calculate randomization inference p-values (RI p) free from distributional assumptions and re-estimate all models using penalized maximum likelihood (Table A14 and Table A15) (Cook, Hays and Franzese 2020). I also examine heterogeneous treatment effects by: building density, income, and local election turnout by regressing comments on treatments and the treatment-covariate interaction, and use randomization inference as a robustness check. More detailed description of the procedures in this section are in "Analytical procedure details" in the appendix.

[3]Balance tables by treatment status and treatment group can be found in the online appendix.

[4]I define "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.

[5]Covariates are: *city, number of units in the building, gender, age, building age, primary language spoken, vote history*, and *party affiliation*.

Results

Across all council meetings, the CACE was 1.02pp [RI p = 0.044; 95% CI 0.66, 1.38]. The effect of assignment to treatment (the ITT) on submitting a comment was 0.19pp [RI p = 0.075, 95% CI 0.06, 0.31]. Both estimates are depicted in Figure A1. Compliance rates were 17% in placebo, 17% in T1, 16% in T2, and 18% in T3 (see Figure A8). CACEs for individual council meetings can be found in Figure A2, which also contains meta-analytic estimates of the aggregate CACE. The point estimate using fixed effects meta-analysis is 0.91 [95% CI 0.56, 1.25] (see Figure A12).

Figure 1 shows that T3 had the largest effect on turnout (CACE = 1.44pp; RI p = 0.011; 95% CI [0.73, 2.15]), T2 was second most effective (CACE = 1.01pp; RI p = 0.071; 95% CI [0.39, 1.63]), and T1 was least effective (CACE = 0.54pp; RI p = 0.386; 95% CI [0.06, 1.03]).[6] 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 other at the 10% level based on one-tailed tests (see Table A9 and Table A14).[7] 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.

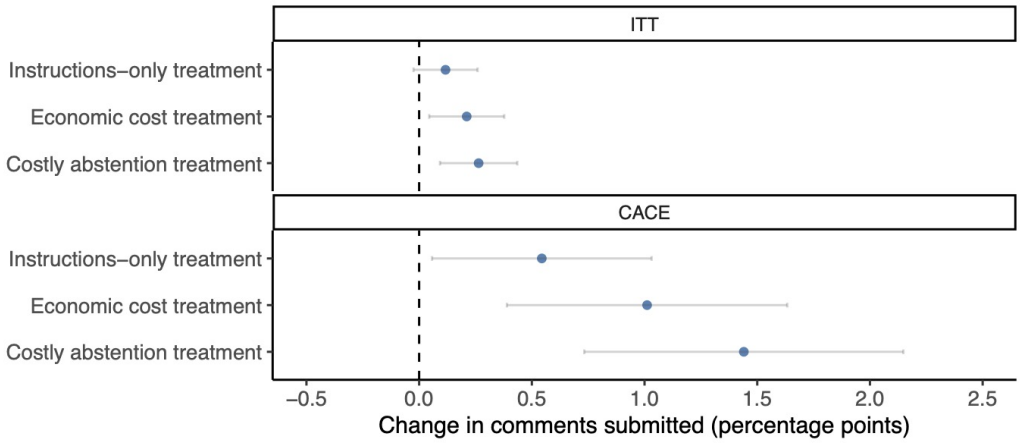

Figure 1: Effects by treatment group, all cities

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

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

To further asses confidence T3 was most effective, I fit a Bayesian linear multilevel model using prior distributions from my pre-registration power analysis, and compute Bayes factors for hypotheses that the differences between treatments are greater than zero (see Figures A14 and A15). This analysis suggests that T3 is 5 times as likely to be larger than the T2 than not, and 97 times as likely to be larger than the T1 treatment than not.

These results align with the pre-registered theoretical predictions. Participation instructions led to a minor increase in participation. Priming economic concerns appears more effective than lowering participation costs. The strongest evidence is that highlighting costs of abstention is more effective than lowering attendance costs. The combined efficacy of the economic cost and costly abstention treatments suggests that economic or psychological motivators are more effective than instructions or a clickable link alone.

Turnout in local elections is also associated with an increase in the likelihood of making a public comment. Voters were more likely to open the emails (see Table A6) and 1.4pp more likely to comment than non-voters (see Figure A3, RI $p = 0.06$).[8] Participation in local politics through voting appears to encourage other forms of engagement. Further research is needed to understand this link. Perhaps voters in low-turnout municipal elections may particularly not want to miss a chance to be heard, or may be more responsive to treatment due to pre-existing interest in housing policy.

Comment contents, substantive impact, and changes in representation

I also examine the types of comments individuals submitted (spoken or written, custom or pre-written, pro or anti-housing) in Figure A4. The majority of individuals (93%) submitted written comments, and the effect for spoken comments is only significant at the 10% level. However, even written submissions were not costless. While most used the sample message, 29% included personal, custom comments. Custom comments touched on personal experiences with high housing costs, such as homelessness, concerns of being priced out of subsidized senior housing, and young renters lamenting their inability to buy a home like

[8]The uncertainty of the estimates are a result of low turnout (9.4% in the sample population).

their parents. Anti-housing comments constituted only 4% of total comments.

Comments by treated individuals represented 8% of total written public comments across all meetings, and 46% of all pro-housing comments (see Table A1). This shifted the balance of pro- vs. anti-housing comments and made council meeting comments more representative of the broader public when remote access alone did not. These large effects on turnout contrast sharply with GOTV.[9] In voter turnout settings, the large number of voters makes campaign-induced changes in turnout relatively small. However, even a few new participants in council meetings can significantly influence comment composition due to low participation rates. Council members in observed meetings also referenced the makeup of comments when discussing and voting on issues, indicating their awareness of comment dynamics.

Conclusion

Understanding how to increase collective action when gains are long-term and uncertain is an enduring question in political economy. While homeowners with direct financial stakes actively engage in local politics, there is little evidence of how to motivate groups with uncertain payoffs such as renters to engage in costly political behavior. I contribute to our understanding using 8 email-outreach field experiments encouraging renters to comment at city council meetings. Three treatments tested the effectiveness of messages that: (1) reduced participation costs, (2) primed economic self-interest, or (3) highlighted the costs of abstention. Receipt of any treatment increased public comments by 1pp, while highlighting abstention costs increased comments by 1.4pp. Local election voters were more responsive to treatment. Treatment-induced comments comprised 8% of all and 46% of pro-housing comments across all meetings. The treatments therefore overcame many traditional barriers to renter collective action, making civic bodies more representative of the broader public.

The results yield the following key insights. First, unlike voting, email can increase local political participation when remote participation is possible, particularly among those

[9]Cost-effectiveness is also notably different, with comments costing $4.80 each, compared to over $450 per vote in GOTV Facebook campaigns and $37 per vote in the most effective text messaging campaigns.

already engaged in local politics. Second, low-cost outreach can significantly increase political participation in low-turnout settings like city council meetings. Third, outreach can make representation 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 motivate collective action.

Acknowledgements

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Biographical statement

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Online Appendix:
Countering capture in local politics: Evidence from eight field experiments

Additional figures

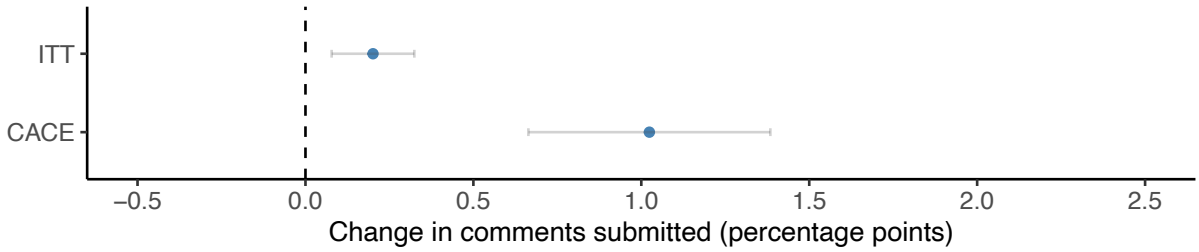


Figure A1: Intent-to-treat effect and complier average causal effect, all cities

Note: Tabular results can be found in Table A7 and Table A8

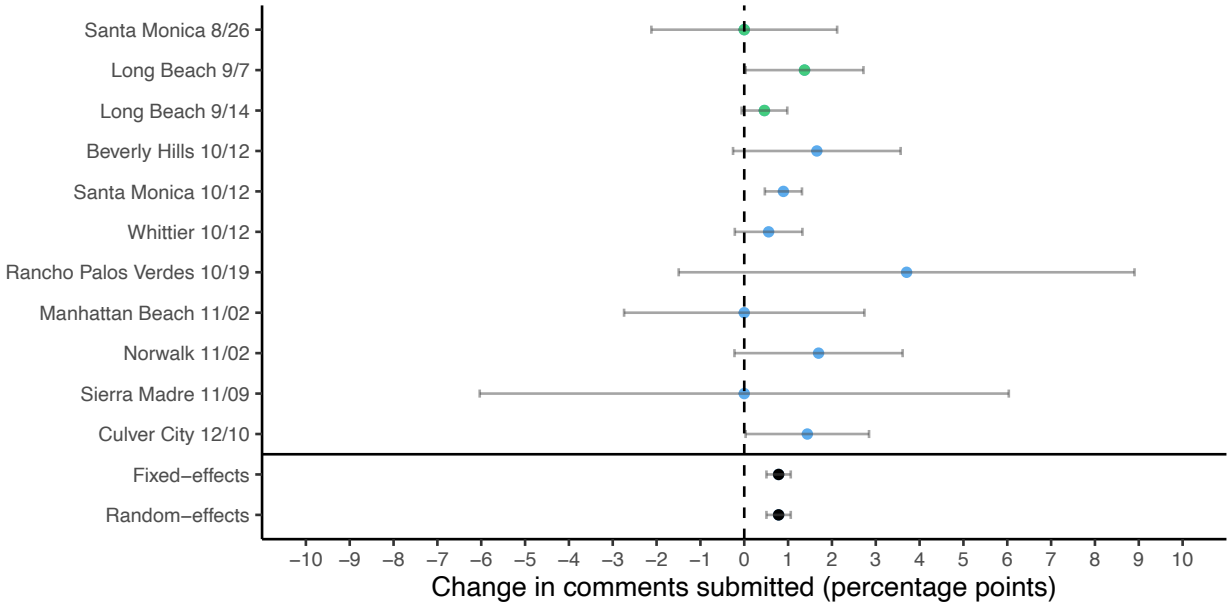


Figure A2: Meta-analysis of complier average causal effects, by council meeting

Note: Pilot studies in green. Tabular results can be found in Table A10 and Table A11.

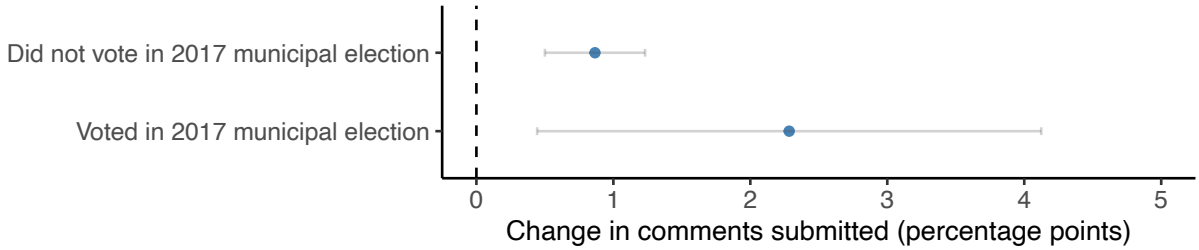


Figure A3: Complier average causal effects by turnout

Note: Tabular results can be found in Table A12.

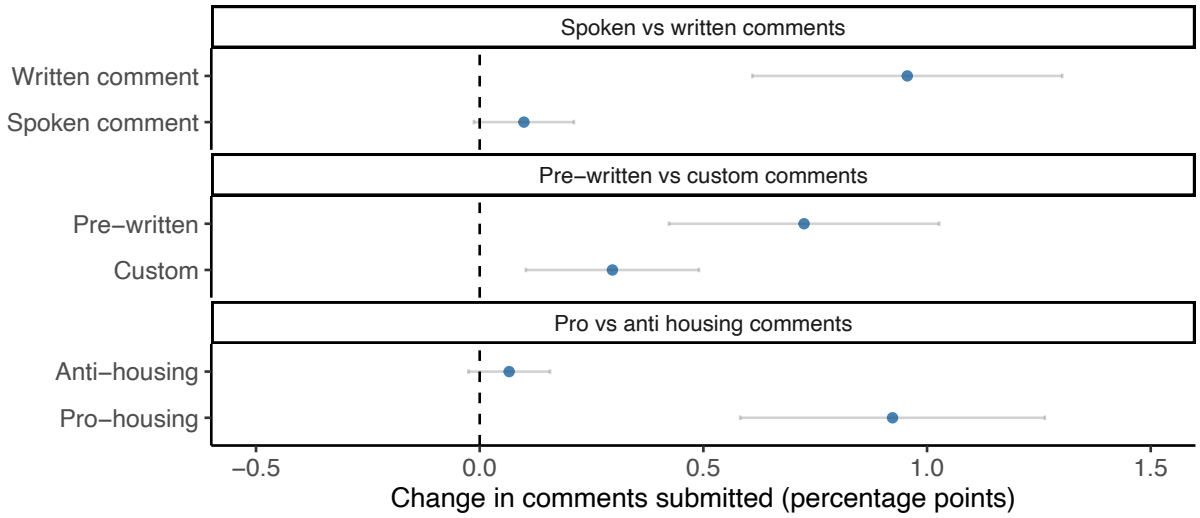


Figure A4: CACE by type of comment

Note: Tabular results can be found in Table A13.

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	6	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 A1: Examination of public comments in treated council meetings

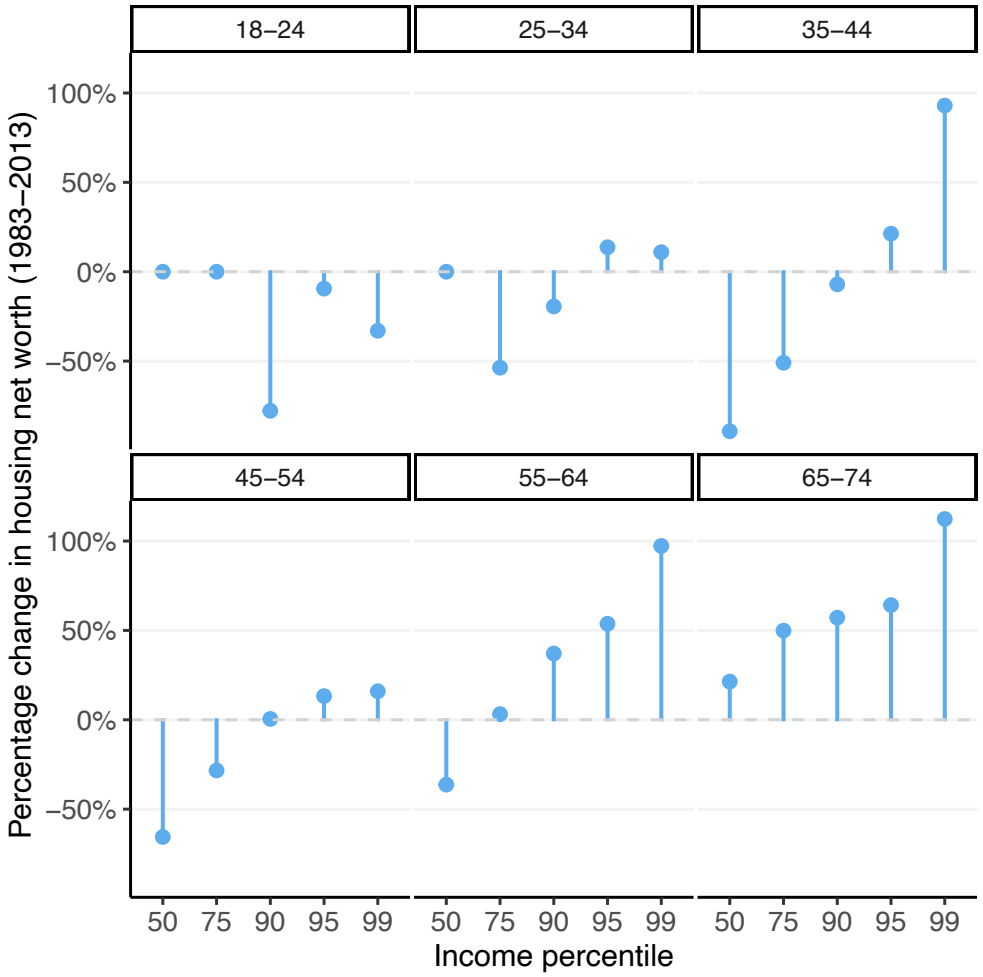


Figure A5: Change in housing net worth by age and income percentile

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 A2: 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 A3: Balance table: renters with emails listed in voter file vs. those without

Treatment messages

LA is in a housing crisis

Los Angeles area cities will soon update their Housing Elements, where they will decide on strategies to provide for the housing needs of local residents. LA County is required to build 800,000 more homes by 2029, which has the potential to fix the housing crisis and transform our region for the better.

(a) Placebo treatment message

LA is in a housing crisis

Los Angeles area cities will soon update their Housing Elements, where they will decide on strategies to provide for the housing needs of residents. LA must build more homes in order to fix the housing crisis and transform our region for the better.

We need your support at the Santa Monica City Council meeting on Tuesday, October 12

Join us to demand that cities adopt housing elements that increase the supply of housing. Together, we can make homes affordable and provide housing for all.

You can **submit a written comment to city council** any time before 2:00 p.m. Tuesday, simply by clicking the button below and emailing in our sample message. It only takes two clicks—one to open the message and one to send!

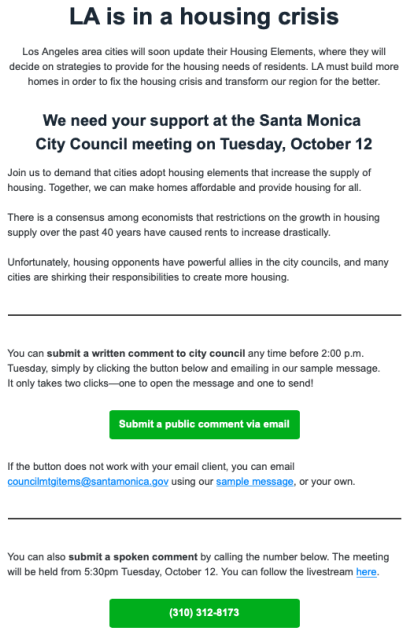
Submit a public comment via email

If the button does not work with your email client, you can email councilmtgitems@santamonica.gov using our sample message, or your own.

You can also **submit a spoken comment** by calling the number below. The meeting will be held from 5:30pm Tuesday, October 12. You can follow the livestream here.

(310) 312-8173

(b) Instructions only treatment message



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We need your support at the Santa Monica City Council meeting on Tuesday, October 12

Join us to demand that cities adopt housing elements that increase the supply of housing. Together, we can make homes affordable and provide housing for all.

There is a consensus among economists that restrictions on the growth in housing supply over the past 40 years have caused rents to increase drastically.

Unfortunately, housing opponents have powerful allies in the city councils, and many cities are shirking their responsibilities to create more housing.

You can **submit a written comment to city council** any time before 2:00 p.m. Tuesday, simply by clicking the button below and emailing in our sample message. It only takes two clicks—one to open the message and one to send!

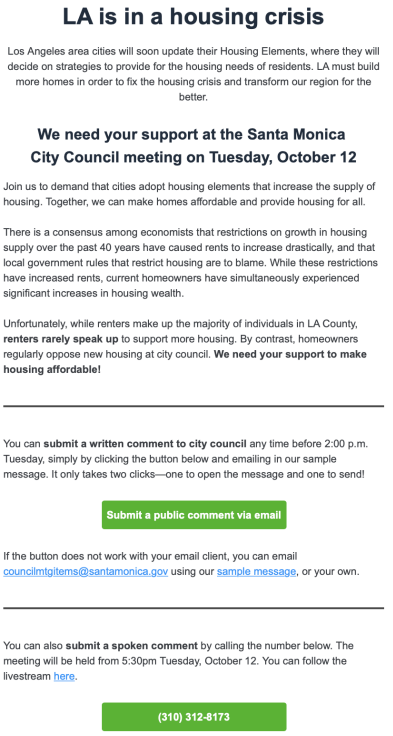
Submit a public comment via email

If the button does not work with your email client, you can email councilmtgitems@santamonica.gov using our sample message, or your own.

You can also **submit a spoken comment** by calling the number below. The meeting will be held from 5:30pm Tuesday, October 12. You can follow the livestream here.

(310) 312-8173

(c) Economic treatment message



LA is in a housing crisis

Los Angeles area cities will soon update their Housing Elements, where they will decide on strategies to provide for the housing needs of residents. LA must build more homes in order to fix the housing crisis and transform our region for the better.

We need your support at the Santa Monica City Council meeting on Tuesday, October 12

Join us to demand that cities adopt housing elements that increase the supply of housing. Together, we can make homes affordable and provide housing for all.

There is a consensus among economists that restrictions on growth in housing supply over the past 40 years have caused rents to increase drastically, and that local government rules that restrict housing are to blame. While these restrictions have increased rents, current homeowners have simultaneously experienced significant increases in housing wealth.

Unfortunately, while renters make up the majority of individuals in LA County, **renters rarely speak up** to support more housing. By contrast, homeowners regularly oppose new housing at city council. **We need your support to make housing affordable!**

You can **submit a written comment to city council** any time before 2:00 p.m. Tuesday, simply by clicking the button below and emailing in our sample message. It only takes two clicks—one to open the message and one to send!

Submit a public comment via email

If the button does not work with your email client, you can email councilmtgitems@santamonica.gov using our sample message, or your own.

You can also **submit a spoken comment** by calling the number below. The meeting will be held from 5:30pm Tuesday, October 12. You can follow the livestream here.

(310) 312-8173

(d) Costly abstention treatment message

Figure A6: Example treatments and wording (Santa Monica experiment)

Treatment details

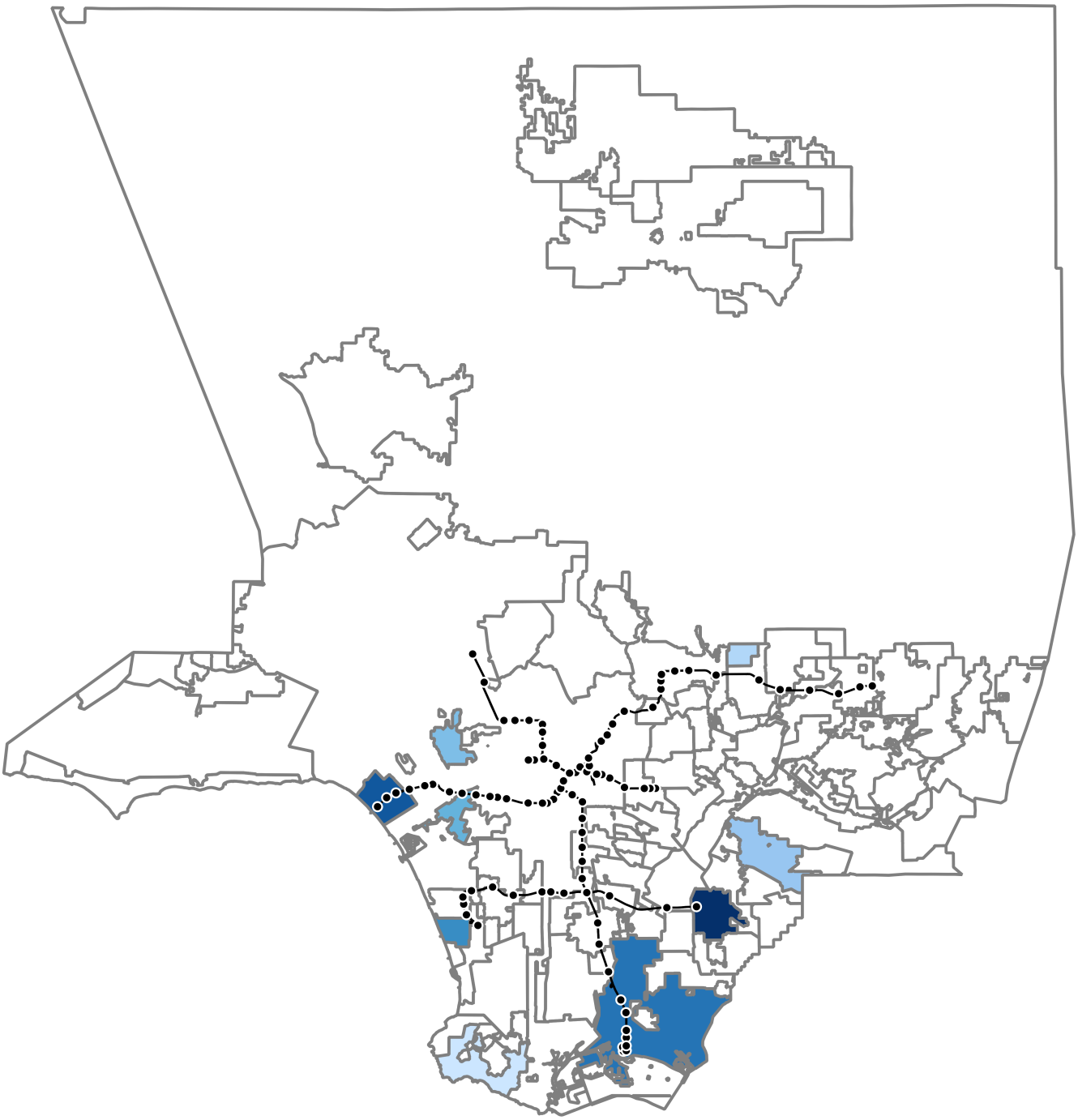


Figure A7: Map of cities in Los Angeles county by experiment status

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

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.[1] 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.[2] Nevertheless, interventions involving participation in governmental processes should be held to high ethical standards.

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 Slough (2019) and McDermott and Hatemi (2020)). 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

[1]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.

[2]Roughly 14% of land, or over 200 square miles, is currently dedicated to parking (Chester, Fraser, Matute, Flower and Pendyala 2015). Affordable housing is also required for density above zoning limits.

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," Bovens (2009) and Schubert (2017) 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 Yale University" 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

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 in an address, clustering took place at the unit level. If a unit number was not available, clustering took place at the building level.

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 A8 and Table A6.

For the primary estimand (i.e., the CACE), I use the Lin (2013) estimator, which performs OLS adjustment using treatment-by-covariate interactions and ensures that adjustment does not hurt asymptotic precision. Specifically, 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.

Results are also analyzed using precision-weighted fixed effects and random effects meta-analysis. 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 Gelman and Hill (2006). 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 A4: 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 A5: Covariate balance across all treatment groups

Tests for differential compliance

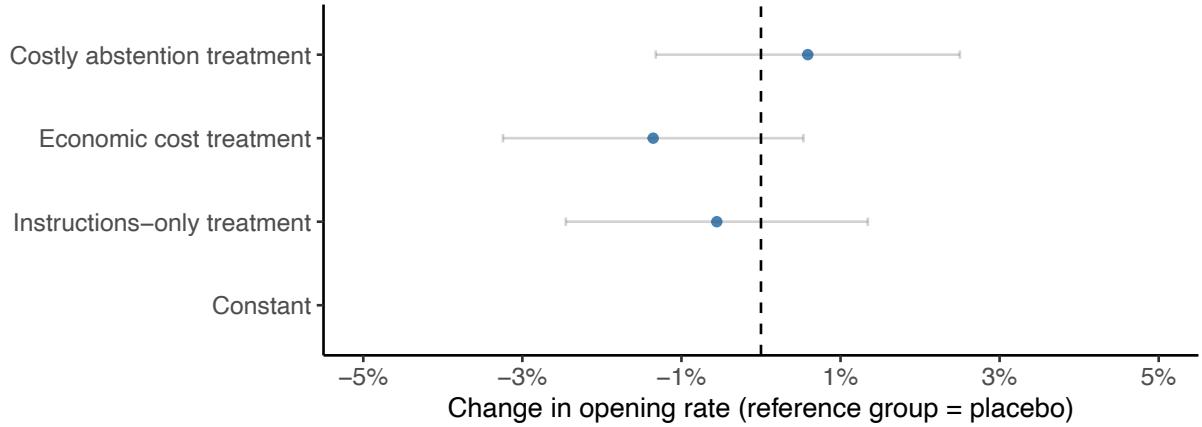


Figure A8: Average treatment effect on email opening, all cities

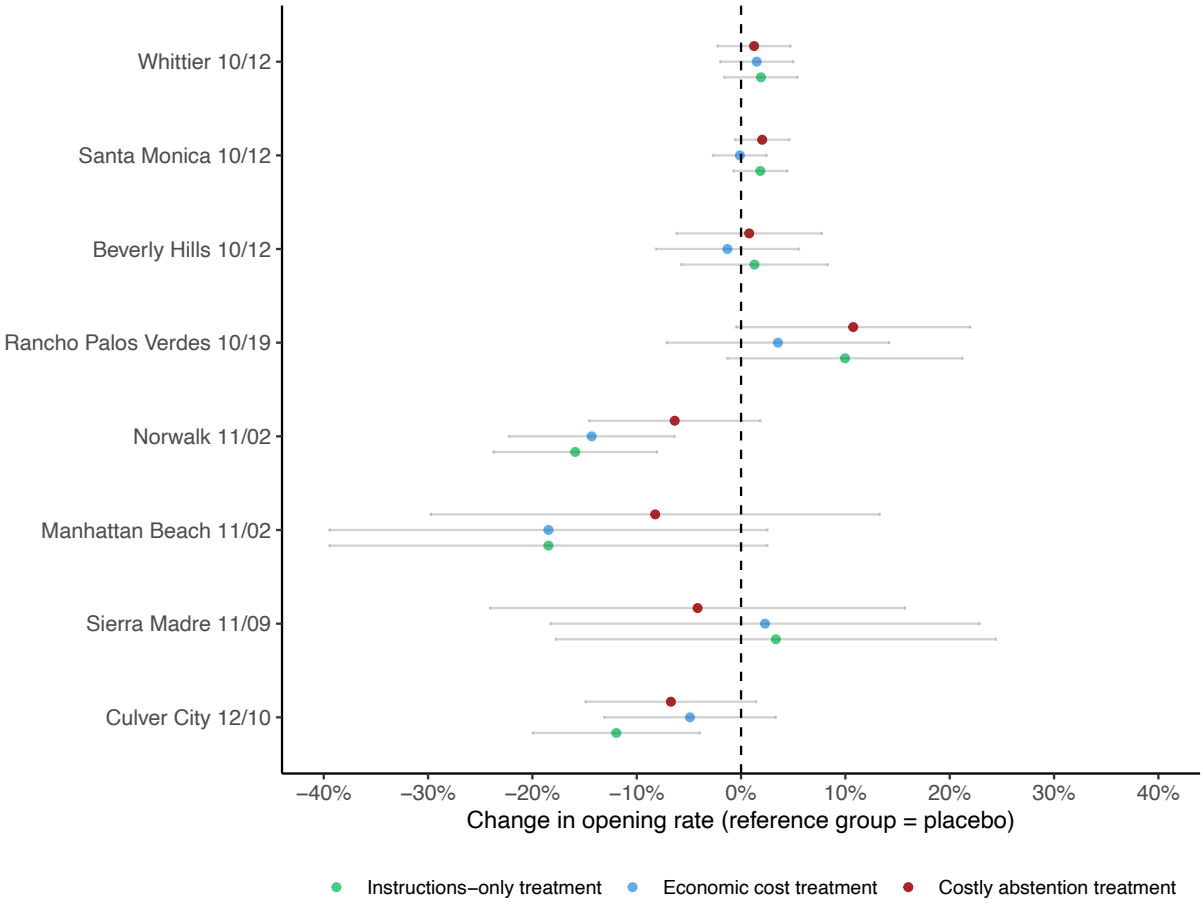


Figure A9: Average treatment effect on email opening, by city

	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 A6: 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 A7: 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 A8: 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 A9: 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 A10: CACEs for each city council meeting

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

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

Table A11: 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 A12: 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 A13: Complier average causal effects by outcome

Robustness

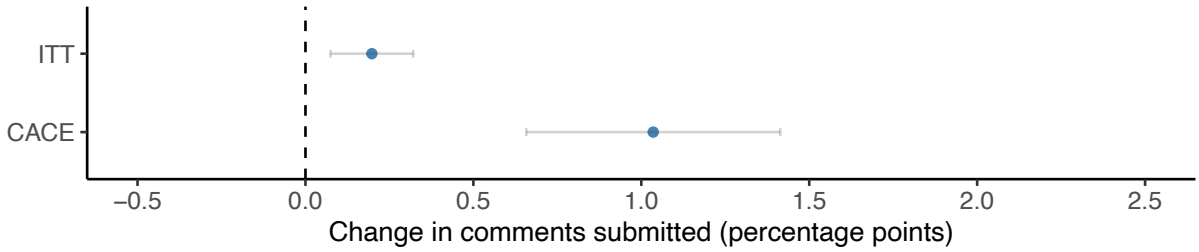


Figure A10: Intent-to-treat effect and complier average causal effect, all cities (without covariate adjustment)

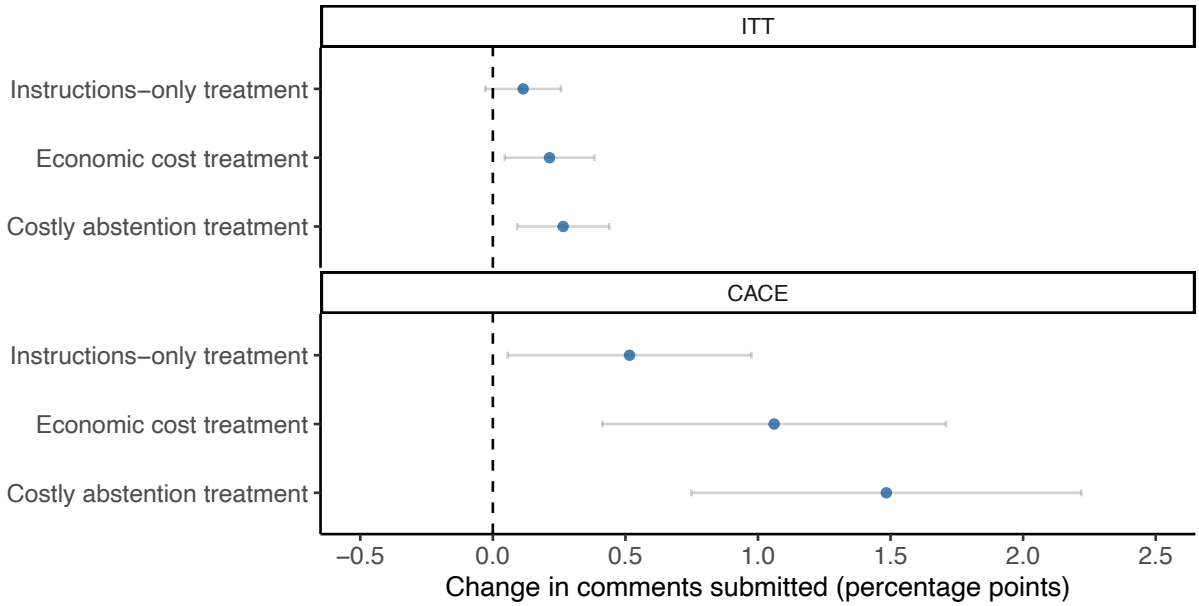


Figure A11: Effects by treatment group, all cities (without covariate adjustment)

	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 A14: 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 A15: ITT and CACE estimates from penalized maximum likelihood

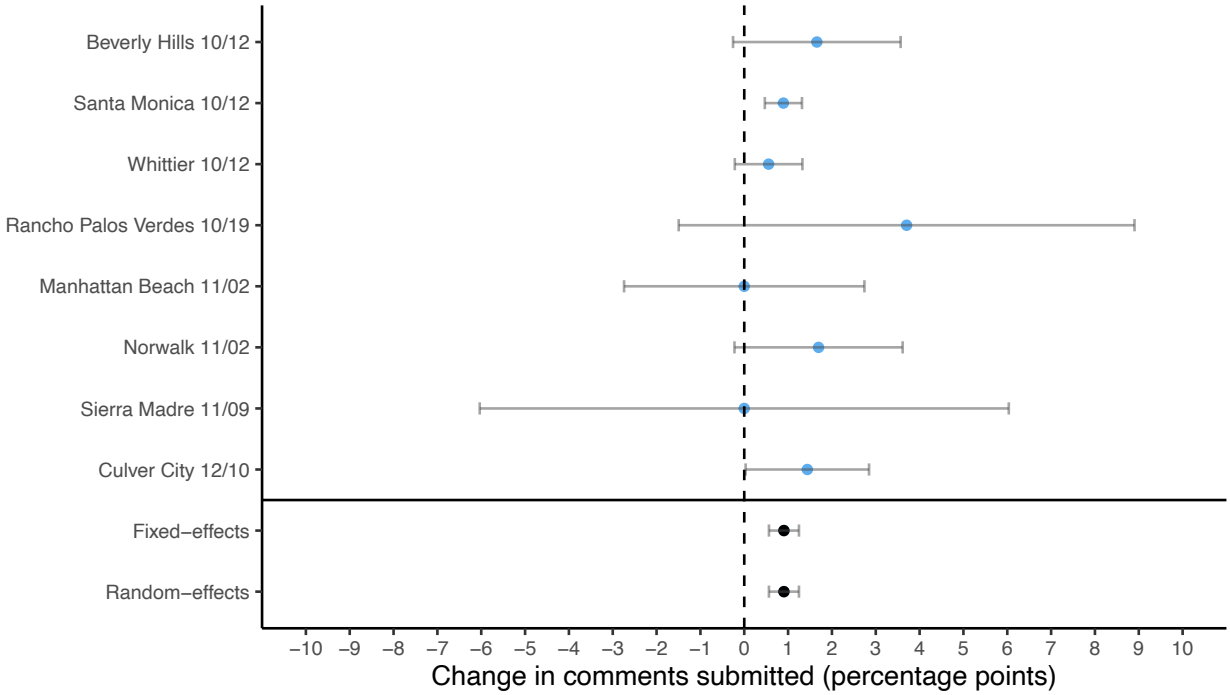


Figure A12: Meta-analysis of complier average causal effects by city, excluding pilot studies

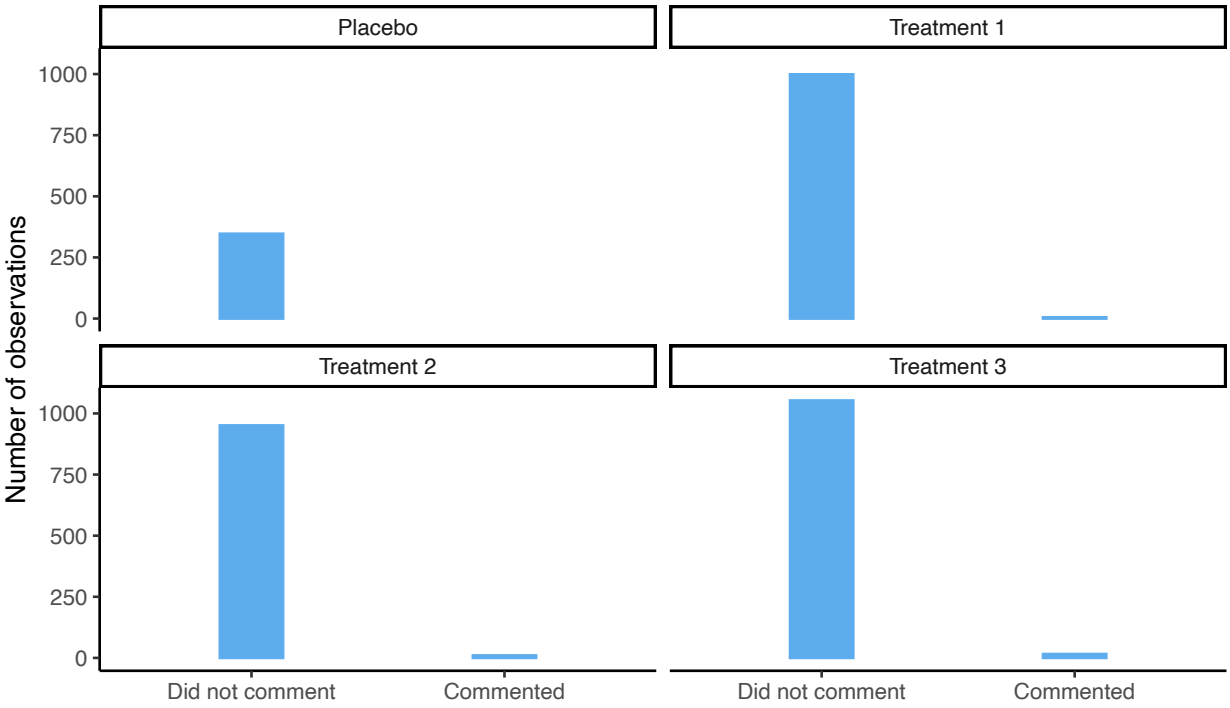


Figure A13: Distribution of outcomes by treatment group (compliers only)

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.

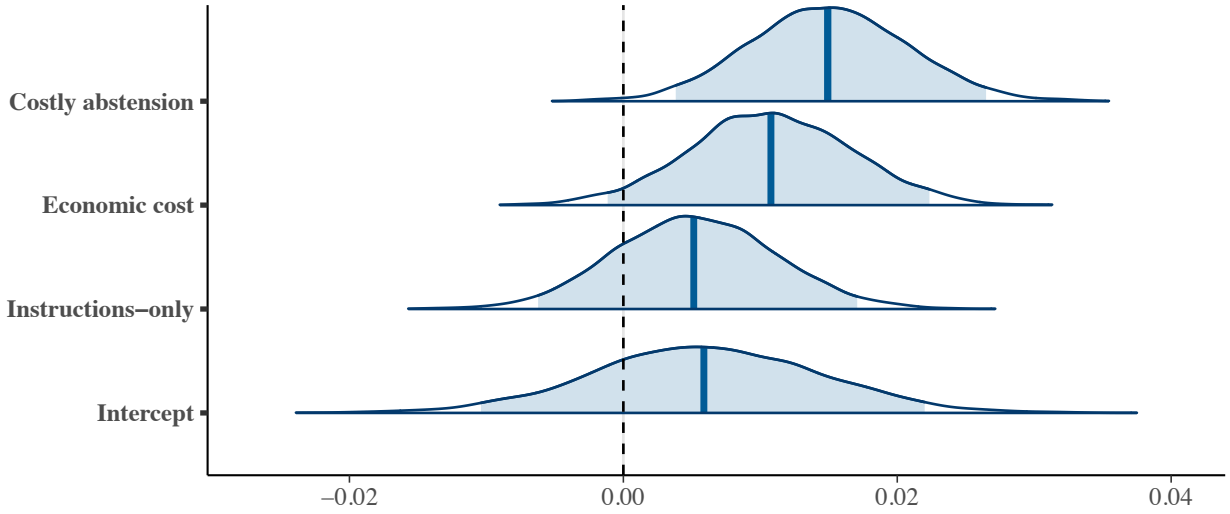


Figure A14: Bayesian multilevel model: coefficient estimates and posterior distributions (includes city fixed effects)

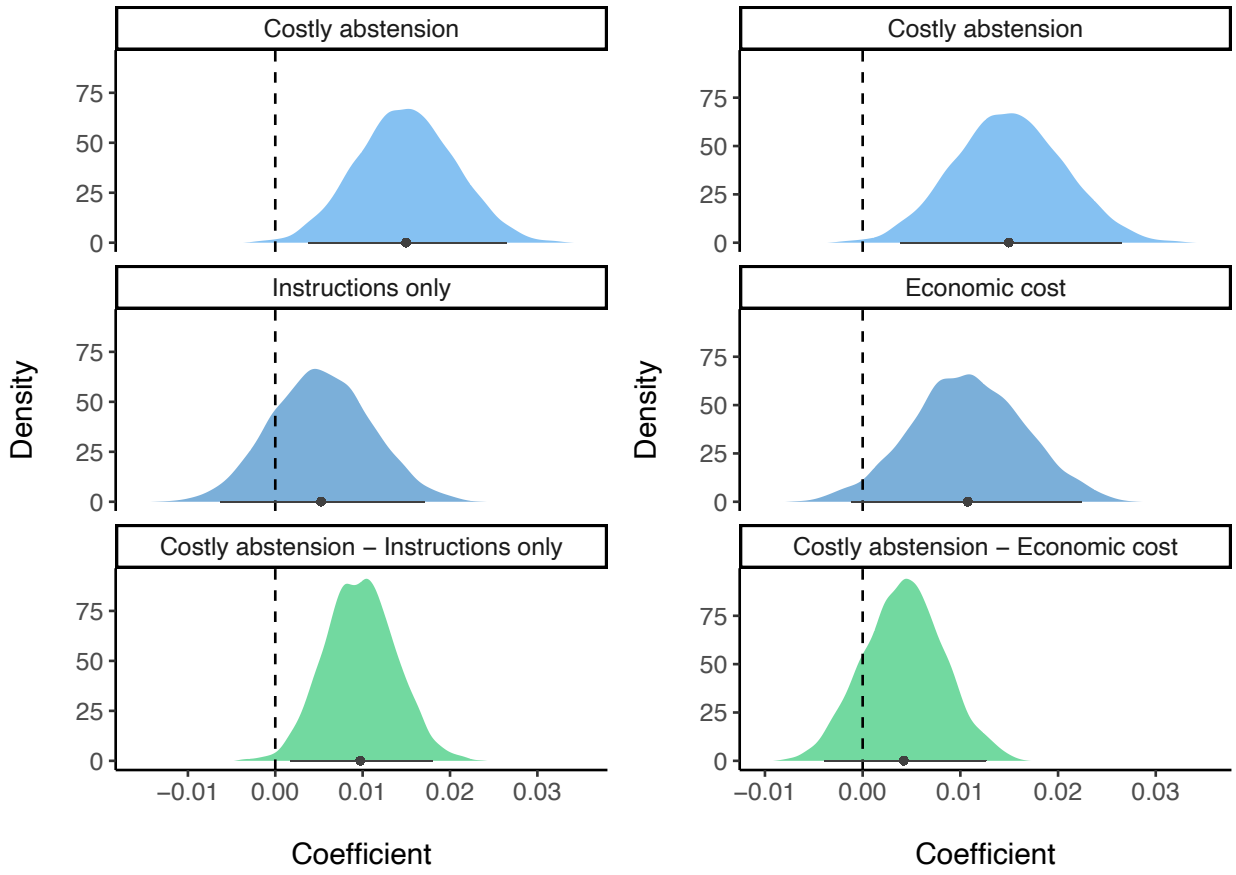


Figure A15: Posterior distributions of costly abstention treatment, instructions only treatment, and difference

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