Combatting capture in local politics: Evidence from eight field experiments *

Trevor Incerti†

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Abstract

Understanding how to motivate individuals with long-term collective interest to engage in costly political behavior is an enduring question in political economy. While renters have an economic incentive to participate in local politics and encourage housing growth, their participation lags homeowners who yield immediate financial returns from participation. I conducted 8 email field experiments to investigate how to motivate renters (n=19,951 households) to comment at city council meetings in opposition to regulations that harm them. Opening a message highlighting high costs of abstention caused a 1.4 percentage point increase in public comments. Treatment-induced comments represented 8% of total comments and 46% of pro-housing comments across all treated meetings. These results suggest that increasing the perception that abstention is costly is an effective motivator of collective action, and that outreach can make civic bodies greater reflect the broader public where increases in accessibility alone do not.

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†Department of Political Science, Yale University.

Homeowners are more likely than renters to participate in local government, and participate in ways consistent with protecting their property values (Hall and Yoder Forthcoming; Marble and Nall 2021; McCabe 2016; Yoder 2020). Evidence suggests homeowners are more likely than renters to oppose new housing development (Einstein, Palmer and Glick 2019; Hankinson 2018), increase their turnout in elections when zoning rules are on the ballot (Yoder 2020), and participate more often in city council, planning, and zoning meetings (Yoder 2020). Yet the same policies that benefit homeowners 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*a*; Lens and Monkkonen 2016; Quigley and Rosenthal 2005; Reeves 2018). Renters therefore also have a monetary incentive to participate in local politics and oppose these policies, but their participation lags that of homeowners. How to translate renter economic self interest into participation in local politics remains unclear.

This phenomenon exhibits classic symptoms of the collective action problem—those with only a long-term collective interest in a political decision are less likely to participate in politics than those who realize direct, short-term private gains (Olson 1965; Ostrom 2000). For homeowners, blocking a neighboring development yields direct and immediate individual returns in the form of preserved property value. For renters, large-scale new development will only depress rents and increase access to homeownership throughout a diffuse geographic region in the long term. How then can those who only benefit through diffuse, long-term gains such as renters be motivated to engage in personally costly political behavior?

Literature in political economy, behavioral economics, and political psychology offer suggestions. First, lack of residential stability may make renters less connected to their communities and local political system (Ansolabehere 2012; McCabe 2016). Providing instructions and lowering the costs of attendance may therefore encourage participation (Milkman, Beshears, Choi, Laibson and Madrian 2011; Nickerson and Rogers 2010). Second, priming rational economic self interest may encourage participation. Third, renters may be unaware

that lack of housing supply is driven by local regulations reflecting homeowner preferences. Pointing out that lack of participation is costly and pressing renters to participate to counteract these regulations may therefore also be a highly effective motivator (Aytaç and Stokes 2019).

I test these instructional, economic cost, and costly abstention theories through a series of email outreach field experiments among approximately 20,000 renter households in 8 cities in Los Angeles (LA) County encouraging individuals to participate in their city council meetings by making public comments. Renter households in LA County were identified by geo-matching addresses in the LA County voter file to Department of City Planning records of multi-unit apartment buildings. Due to COVID-19, public comment was limited to online participation via email, telephone, or videoconference. The experiments were designed and deployed in partnership with a local non-profit organization with years of experience advocating for increased housing supply. Three treatment groups (and a placebo control) tested the three theories outlined above of how to encourage renters to translate their economic self interest into costly political behavior.

Overall, receipt of any treatment increased public comments by 1 percentage point (pp), while highlighting the cost of abstention increased comments by 1.4pp. Individuals already engaged in local politics in the form of voting in local elections were more responsive to treatment (2.3pp) than non-voters (0.9pp). Including all treatment groups, treatment-induced comments represented 8% of total comments and 46% of pro-housing comments across all city council meetings. Pro-housing comments made up a majority of comments in over 50% of treated meetings. This contrasts sharply with previous findings that pro-housing comments typically are in the minority in most council meetings in equilibrium (Einstein, Glick, Puig and Palmer 2021; Yoder 2020).

The results support three primary theoretical and substantive conclusions. First, when abstention is perceived as highly costly, highlighting its consequences is an effective motivator of political participation. Second, the large change in the composition of comments caused

by the treatments shows that outreach can change the representation of civic bodies to be more reflective of the broader public where increases in accessibility alone do not. Finally, unlike voting, email is able to effectively increase political participation when participation is also conducted online, particularly amongst those already engaged in politics.

Motivation

Lack of renter participation in local politics

Homeowners are more likely than renters to participate in local politics across numerous dimensions. Renters are less likely to run for office (Einstein, Ornstein and Palmer 2019), vote in local elections, donate to local political candidates, or participate in city council meetings (Yoder 2020).

Moreover, there is evidence that homeowners actively participate in politics to oppose housing development. Homeowner voter turnout roughly doubles in elections when zoning regulations are on the ballot (Hall and Yoder Forthcoming), and those who participate in city council, planning, and zoning board meetings are much more likely to oppose new housing construction than the general public (Einstein, Palmer and Glick 2019; Fischel 2005). In addition, homeowner participation rates are an increasing function of home value (Hall and Yoder Forthcoming; Marble and Nall 2021).

The makeup of local political participation therefore does not typically reflect general public opinion. This is visible in the difference between the percentage of public comments in support of additional housing and the percentage of votes in favor of additional housing on ballot measures. Einstein, Palmer and Glick (2019) show that in Massachusetts, over 50% of voters in the majority of townships supported a ballot measure in favor of affordable housing, while public comments overwhelmingly oppose new developments.

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*a*), and supply restrictions are estimated to be a net welfare loss for society (Glaeser and Gyourko 2018). This lack

of supply is not primarily due to natural geographic scarcity or construction costs, but government regulation (Brueckner 2009; Glaeser, Gyourko and Saks 2005*a*,0; Gyourko and Molloy 2015; Molloy et al. 2020; Ortalo-Magné and Prat 2014). In fact, Glaeser, Gyourko and Saks (2005*b*) estimate that the effective "regulatory tax"[1] on home prices in Los Angeles and San Francisco were roughly 1/3 and 1/2 respectively in 1999, and as of 2014 home prices were more than double that of production costs (Glaeser and Gyourko 2018).

Impact of lack of housing supply

While economic growth and housing growth used to occur in concert, these processes have decoupled (Glaeser and Gyourko 2018). While new home construction in California was an average of 0.011 houses per capita per year in the late 1960s, this rate declined to 0.002 in the late 2010s (United States Census Bureau 2020). Instead of leading to large increases in home construction and encouraging low-skill migration, economic booms in coastal cities now primarily increase housing prices (Ganong and Shoag 2017).

This decrease in housing supply increases rents, reduces real income for renters, and keeps homeownership out of reach for an increasing number of Americans. Real housing prices in the top quintile of the price distribution doubled in Los Angeles and New York, and tripled in San Francisco since 1970 (Glaeser, Gyourko and Saks 2005*a*; Hankinson 2018). A quarter of renters in the United States currently spend over half their incomes on housing, and this number is expected to grow (Charette et al. 2015). High housing costs also constrain worker mobility, reducing real US economic growth by an estimated 36% from 1964 to 2009 (Hsieh and Moretti 2019) and real GDP by at least 2% (Glaeser and Gyourko 2018).

By contrast, higher housing prices benefit current homeowners, increasing their net worth and exacerbating income inequality. This housing wealth is concentrated amongst individuals in coastal regions who purchased homes in the decades before regulatory and political constraints on housing development were imposed (Glaeser and Gyourko 2018). For exam-

[1]Note that this is not a literal tax levied by the government, but rather the increased cost of housing caused by local regulations that restrict supply.

ple, between 1983 and 2013 housing net worth increased by an average of 57% for those 65 and older in the 90th percentile of the wealth distribution (Glaeser and Gyourko 2018).[2]

Lack of housing supply also increases energy use and greenhouse gas (GHG) emissions. Some studies estimate that increasing urban infill—for example, replacing surface parking lots with apartment buildings—would have a larger effect on GHG emissions reductions than mass adoption of electric vehicles (Wheeler, Jones and Kammen 2018). Others are less bullish, but still highlight the importance of increasing urban infill on reducing GHG emissions (Cervero and Murakami 2010).

Theory and hypotheses

Lack of collective action by renters

Evidence of why renters in particular tend not to participate in politics is scarce. However, some possible theories can be evaluated on the basis of the extant empirical evidence.

First, lack of residential stability may make renters less connected to their communities and local political system (Ansolabehere 2012; McCabe 2016). Simple increases in accessibility offered by moving city council meetings online during the COVID-19 pandemic did not meaningfully alter renter participation in 2020 (Einstein et al. 2021),[3] suggesting that high costs of attendance are likely not the primary barrier.[4] Lower community connectedness and a resulting lack of information on how to participate may therefore meaningfully contribute to lack of renter participation. Homeowners may learn of development related council meeting agenda items through community groups and local online platforms that renters are not a part of. Providing information about the content of council meeting agenda items and instructions of how to participate is therefore a necessary minimum to spur collective action.

[2]For those in the 95th and 99th percentiles these numbers are 65% and 112%, respectively. See Figure A.1 for a visualization of changes in all age groups and income percentiles.

[3]Homeowners still made up 78% of commenters, and anti-housing comments comprised the majority in 35 out of 36 towns examined in Einstein et al. (2021)'s study in the Boston area.

[4]I recognize that homeowners are on average older, more likely to be retired, and wealthier (Yoder 2020), and therefore may have a lower opportunity cost of attendance, even in an online setting. While I do not test this theory directly here, it is likely a constant presence across all of my treatment arms.

Second, lack of renter participation on housing issues does not appear to be caused by general opposition to development, unlike homeowners. While homeowners consistently oppose new housing across all geographies, renters do not (Hankinson 2018; Marble and Nall 2021; Monkkonen and Manville 2019). Further, while even renters in high-cost cities sometimes oppose market-rate housing at the neighborhood level, they do not at the city level (Hankinson 2018). Messages highlighting the public benefits of city-wide housing growth and highlighting "affordable" (i.e., government subsidized) or "missing middle" housing developments have been shown to increase support for increased housing (Doberstein, Hickey and Li 2016). By contrast, messaging that highlights developers or developer profits is met with backlash (Monkkonen and Manville 2019). Messages encouraging collective action should therefore focus on the city-wide public benefits of increased housing, and highlight affordable housing.

Encouraging political participation: general evidence

Research in political economy, behavioral economics and experimental psychology offer lessons for encouraging participation inside and outside of the ballot box. I first provide a brief overview of the evidence these literatures provide on which mode of delivery is most effective in order to maximize participation, how instructions should be given, and which behavioral motivators should be used. I then show how my treatments are consistent with best practices from these literatures.

In terms of instructions and message structure, past studies suggest that merely overcoming information costs should not have a large impact on political behavior (Green and Gerber 2019; Riker and Ordeshook 1973). By contrast, giving individuals a detailed plan of how and when to participate has been shown to be an effective method of increasing both voter turnout (Nickerson and Rogers 2010) and vaccination rates (Milkman et al. 2011). These studies suggest that clear, concrete instructions of how and when to participate lower the cost of participation.

In terms of mode of delivery, get-out-the-vote (GOTV) experiments find email has proven largely ineffective at increasing voter turnout (Green and Gerber 2019; Malhotra, Michelson, Valenzuela et al. 2012; Nickerson et al. 2007).[5] However, the efficacy of email at increasing political participation that can itself be conducted electronically—lowering the cost of participation substantially—is less clear. Due to COVID-19, public comment in 2021 was limited to online participation via email, telephone, or videoconference. Green and Gerber (2019) note that "it is one thing to present recipients with options that they can choose from immediately from the comfort of their chair, such as visiting a website that tells them where to vote. More difficult is to motivate the recipient to get out of that chair on some future date in order to cast a ballot." We possess scarce evidence of the effectiveness of email on participation that does not require the recipient to get out of the chair.

In terms of message content and behavioral motivations, economic self-interest alone is typically not the most effective motivator of collective action mobilization (Citrin, Green, Muste and Wong 1997; De Rooij, Green and Gerber 2009; Ostrom 2000; Sears and Funk 1991). By contrast, psychological motivators such as highlighting social norms and comparing treated individuals' behavior to neighbors and peers has been shown to induce costly prosocial behavior such as voting and energy saving (Allcott 2011; Gerber, Green and Larimer 2008). Relatedly, Aytaç and Stokes (2019) posit that there are often high psychological costs to abstention from political participation, and that messages that elicit emotional responses (e.g., shame, anger, anxiety, etc.) draw people to collective action. These costs relate to neighbor and peer comparisons as abstention in the housing context is costly due to a lack of abstention by homeowner neighbors. Formally, Aytaç and Stokes (2019) posit a model with rewards of participation $P = A - C + D_E$ where P is rewards from participation, A is the cost of abstention, C is the cost of participation, and D_E is social pressure. A treatment that increases the costs of abstention A, decreases the cost of participation C, and provides social pressure D_E should therefore maximize the rewards from participation.

[5]Even in a low salience election, emails sent from the county registrar of San Mateo, California only increased turnout by 0.56 percentage points (Malhotra et al. 2012).

In sum, theory and lessons from prior research can inform the design of a campaign encouraging individuals to participate in local politics. Messaging strategies that: (1) lower costs of participation with concrete reminders of dates and simple but detailed instructions of how to participate, (2) increase social pressure by comparing non-participants to their neighbors, and (3) emphasize the high (economic and psychological) costs of abstention should be particularly effective. While emails are likely less effective than direct conversations, emails may be more effective in an online public comment context compared to GOTV due to the lower costs of electronic participation.

Tying theory to treatments

The observations in Lack of collective action by renters and Encouraging political participation: general evidence lead to three distinct but related theories of motivation to collective action, which I test with three distinct treatment arms. The relative efficacies of each treatment arm hypothesized below were pre-registered.

First, a treatment (T1) that lowers costs of participation with simple but detailed instructions of how to participate should increase attendance, but the effects should be small in magnitude (Green and Gerber 2019; Riker and Ordeshook 1973). To lower costs of participation, all treatment messages include a link that opens an email message with a pre-filled sample public comment[6] that is pre-signed with the respondent's name, while also noting that individuals may draft their own comment. All treatment messages also include the phone number or Zoom link needed to submit a spoken comment.[7]

Second, a treatment (T2) providing information that lack of housing supply increases rents should increase attendance more than attendance instructions only by also priming economic self interest.

Third, a treatment (T3) that not only lowers costs of participation, but also pressures renters to participate and points out that a lack of participation is costly (i.e., costly absten-

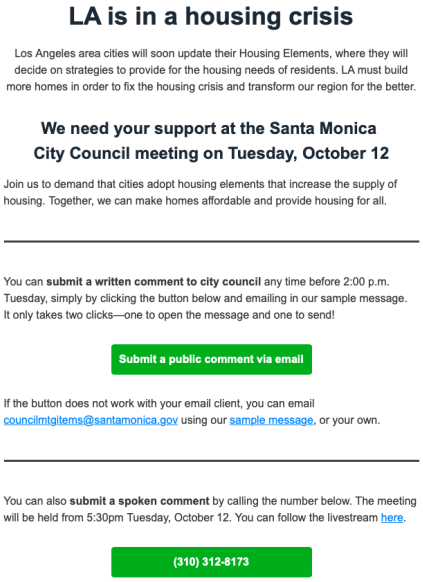
[6]See Sample comment for the wording of the sample message.

[7]Individuals were not encouraged to attend council meetings in person (even if possible) due to the COVID-19 pandemic.

tion theory) should increase attendance more than lower costs of participation or economic self-interest alone. This provides the first experimental test of Aytaç and Stokes (2019) costly abstention theory in a real-world setting, and extends their theory into an understudied vein of civic participation—city council meetings.

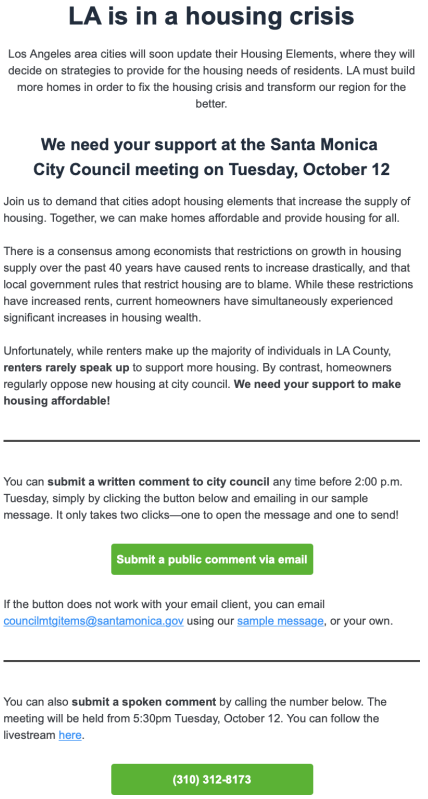
This paper therefore provides three major empirical advancements. First, I provide the first real-world test of how to motivate renters to participate in local politics by comparing three theoretically-motivated treatment arms that allow me to examine the efficacy of three distinct potential motivators of collective action. Second, I examine if this motivation can meaningfully change the makeup of participation in city councils. Third, I test whether email is an effective motivator of online political participation.

(a) Placebo treatment message



(b) Instructions only treatment message

(c) Economic treatment message



(d) Incentive treatment message

Figure 1: Treatment groups

Research design

Context

The COVID-19 pandemic moved city council meetings online. Some hypothesized that the more accessible nature of online meetings would diversify participation (Hernández 2021; Los Angeles Times 2021; The Boston Globe 2021). However, the participation gap between renters and homeowners did not decrease (Einstein et al. 2021).

The experiment was fielded in Los Angeles (LA) County in partnership with a pro-housing NGO, and to the best of our knowledge is the first field experiment to examine participation in city council meetings. LA County cities were in the process of updating their 2021-2029 "Housing Elements." Housing Elements are updated once per decade as required by California state law, and are an analysis of a city's housing needs for all income levels and strategies to be used to meet those needs. State law requires that each city accommodate its fair share of affordable housing, requires local governments to adopt land-use plans and regulations that provide opportunities "for, and do not unduly constrain," housing development by the private sector, and specifically requires the LA region to provide housing for roughly 800,000 additional residents. The experiment therefore targets city council meetings in which the updated Housing Element is on the agenda, and treatments encourage individuals to advocate for historically high housing growth.

While there is a vocal anti-development contingent in Los Angeles, the general voting public appears to support additional housing as a number of local anti-development ballot measures have recently failed. Measure S, which would have drastically curbed high-density development in the city, failed with only 30% support. Meanwhile, Measure JJJ—which grants zoning changes to developments that include affordable housing—passed. Measure H, which instituted a sales tax increase to fund (among other things) affordable housing, also passed. Further, 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 leads to a majority of new housing developments replacing parking lots or commercial buildings, not existing housing stock. Roughly 14% of land, or over 200 square miles, is currently dedicated to parking (Chester, Fraser, Matute, Flower and Pendyala 2015). In addition, the city of Los Angeles requires affordable housing in exchange for increases in density above current zoning limits. For example, the transit-oriented-communities program (part of Measure JJJ passed by voters in 2016) requires at least 25% of units to be set aside at the low income level in exchange for the largest density bonuses near major transit stops.

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, 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.

Second, 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.

Third, 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.

Fourth, 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 and involve no deception.

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 were monitored for agenda items discussing their Housing Element, and these council meetings were selected for the messaging campaign, (3) renters were randomly assigned to one of three email treatments asking them either to turn out to support increases in housing supply or a placebo control, (4) names in all treatment groups were matched with names of individuals who submitted a public comment, (5) analysis was performed using pre-registered outcomes and estimators. More detailed explanations of the processes follows below.

Identifying renters

I identified renters in Los Angeles County by geo-matching addresses in the voter file with Los Angeles County Department of City Planning records of multi-unit apartment buildings. This process was conducted using the FastLink probabilistic linkage algorithm developed in Enamorado, Fifield and Imai (2019). Only records with a 99.2% or greater posterior probability of a correct match were kept,[8] resulting in 641,184 matched renters, 266,057 of whom listed their email addresses in the voter file.

Identifying council meetings

Partner organizations monitored city council meetings in LA County for agenda items discussing the Housing Element throughout fall and winter 2021. Renters identified in the

[8]Manual checking of a random sample of 100 records indicated that 98% of matches with posterior probability above 99.2% were correct, while 96% of matches below posterior probability 99.2% were false positives.

voter file as living in these cities were then targeted to receive emails encouraging them to submit a public comment on the Housing Element agenda item at their city council meeting. Ultimately, 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 council meetings in the cities of (in chronological order) Beverly Hills, Santa Monica, Whittier, Rancho Palos Verdes, Manhattan Beach, Norwalk, Sierra Madre, and Culver City.

Treatment assignment

Likely 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[9] and cluster randomly assigned by address.[10] Treatment assignment probabilities were as follows: 10% probability of assignment to a placebo message with no information on how to attend a meeting, 30% probability of assignment to T1, 30% probability of assignment to T2, and 30% probability of assignment to T3. 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 A.4.

Outcomes

The primary, pre-registered outcome of interest is a binary indicator of whether an individual submitted a spoken *or* written comment. As participation in a public hearing is a matter of public record, I match the names of those in the treatment group(s) with spoken or written comments. I also investigate *how* individuals commented through the creation of separate binary indicators for: spoken comments, written comments, comments that used our pre-written messages, custom comments, pro-housing comments, and anti-housing comments.

[9]While random assignment took place simultaneously for all cities, treatments were launched at different points in time for each city. For this reason it is also reasonable to think each city as a separate experiment, rather than as blocks in a single simultaneous experiment.

[10]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.

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

Analytical procedures

Analytical procedures were pre-registered with the Center for Open Science Open Science Framework (OSF) prior to data collection or analysis. The primary (and pre-registered) estimand of interest is the complier average causal effect (CACE),[11] of opening an email on submission of a public comment. In other words, I estimate the average treatment effect for only the subset of individuals who opened the emails (i.e., compliers). I employ a placebo-controlled design in order to mitigate statistical uncertainty (Broockman, Kalla and Sekhon 2017; Nickerson 2008). 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.[12] I can then compare email openers in treatment directly to email openers in placebo.

I therefore monitor if an email was opened as a measure of compliance, and estimate the CACE using the estimator outlined by Lin (2013). I include the following pre-registered pre-treatment covariates in the regression specification: *city, number of units in the building, gender, age, building age, primary language spoken, vote history, and party affiliation.*[13] Missing covariates are mean imputed. As units were cluster randomly assigned by address, standard errors are clustered at the address level. Results are also reported in Robustness without covariate adjustment. I therefore estimate the OLS specifications below:

[11] Also commonly known as local average treatment effect (LATE).

[12] Tests for differential compliance by treatment group and differential covariate predictiveness of compliance can be found in Figure A.5 and Table A. While some covariates are predictive of compliance, they tend to be similarly predictive of compliance across treatment groups.

[13] I show that these variables are balanced between the placebo and treatment groups in Balance.

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

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

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.

As the outcome data take the form of "rare event" right-skewed binomial distributions (see Figure A.10), I also calculate randomization inference based p-values (RI p)[14] free from distributional assumptions as an extra robustness test. In addition, I re-estimate all models using penalized maximum likelihood.[15]

Results are analyzed as above (i.e., as one large experiment with city fixed effects), as well as aggregated using precison-weighted[16] fixed effects and random effects meta-analysis. This pre-registered, prospective multi-site study can be shown to be a valid application of meta-analysis.[17] Fixed effects meta-analysis—which assumes estimates vary across studies only due to having just a sample of observations from the total population—is often not appropriate in the social sciences. However, as identical pre-registered experiments were

[14]Specifically, I simulate a large number of "fake" random assignments for all units using the same procedure as the real random assignment, and estimate 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.

[15]I do not use traditional logistic regression due to the skewed nature of my outcome variable (i.e., becuase the comments in my sample represent are "rare events"). See King and Zeng (2001) and Cook, Hays and Franzese (2020) for discussions of maximum likelihood estimation in the case of rare events.

[16]With weights equal to the inverse of the variance.

[17]Borrowing the framework, language, and notation of Slough and Tyson (2021), the constituent studies $\mathcal{E}_i$ of the meta-study contain a measurement strategy m_i (a binary comment indicator), contrasts (ω_i', ω_i'') where ω_i' is the control condition and ω_i'' is the treatment condition, and setting/city (θ_i) specific treatment effect $\tau_{mi}(\omega_i', \omega_i'' \mid \theta_i)$, where all studies in the meta-analysis are constituent comparable $(\tau_{m1}(\omega_1', \omega_1'' \mid \theta_1) = \tau_{mi}(\omega_i', \omega_i'' \mid \theta_i)\ \forall\ i \in \{1, ..., n\})$ and that all studies are measurement harmonized $(m_1 = m_i\ \forall\ i \in \{1, ..., n\})$ and contrast harmonized $(\omega_1' = \omega_i'\ \forall\ i \in \{1, ..., n\}$ and $\omega_1'' = \omega_i''\ \forall\ i \in \{1, ..., n\})$.

administered to different populations and measured the same outcome, it may be appropriate in this context. Nevertheless, I also include estimates using random effects meta-analysis, as well as excluding results from three pilot studies for robustness purposes.[18] 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).[19]

I also examine pre-registered heterogeneous treatment effects by the density of the building in which an individual lives, median area income, and turnout in the most recent local election.[20] Individuals who live in high-density buildings may be pre-disposed to a more dense urban environment. Income may correlate with a desire for, in particular, affordable housing development. Voters in local elections are pre-engaged in local politics, and may therefore perceive abstention as more costly than others and/or vote in part due to pre-existing opinions about development. I analyze heterogeneous treatment effects in two ways. First, I take the traditional (and pre-registered) experimental approach of regressing the outcome variables on treatments and the interaction between the treatment and the covariate, sometimes referred to as a conditional average treatment effect (CATE).[21] I also use randomization inference as a robustness check.[22]

[18] Meta-analysis excluding the pilot studies is performed for robustness purposes.

[19] 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 $\sqrt{\hat{p}(1-\hat{p})/n}$."

[20] The most recent county-wide local elections at the time of data acquisition were the March 7, 2017 consolidated municipal and special elections.

[21] Note that this is analogous to conducting separate analyses by subgroup.

[22] Specifically, 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

Overall

Across all council meetings,[23] 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 graphically in Figure 2. 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 A.5 for a formal test of differential compliance by treatment group).

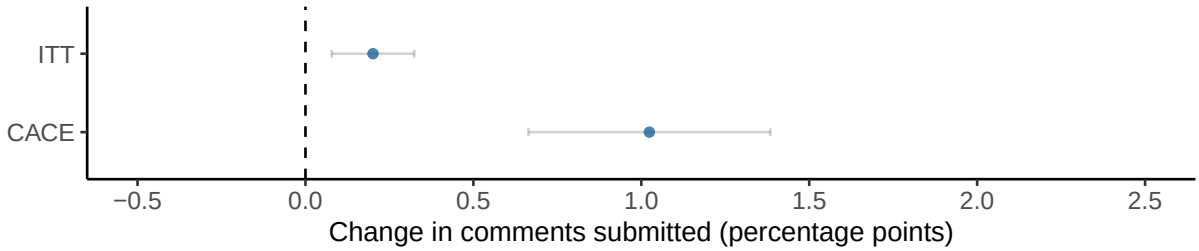


Figure 2: Intent-to-treat effect and complier average causal effect, all cities

CACEs for individual council meetings can be found in Figure 3. In addition to depicting the CACE in individual cities, Figure 3 also contains estimates of the CACE across council meetings using fixed and random effects meta-analysis. Figure 3 contains individual and meta-analytic estimates from 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 A.9).

[23]Not including the aforementioned pilot studies.

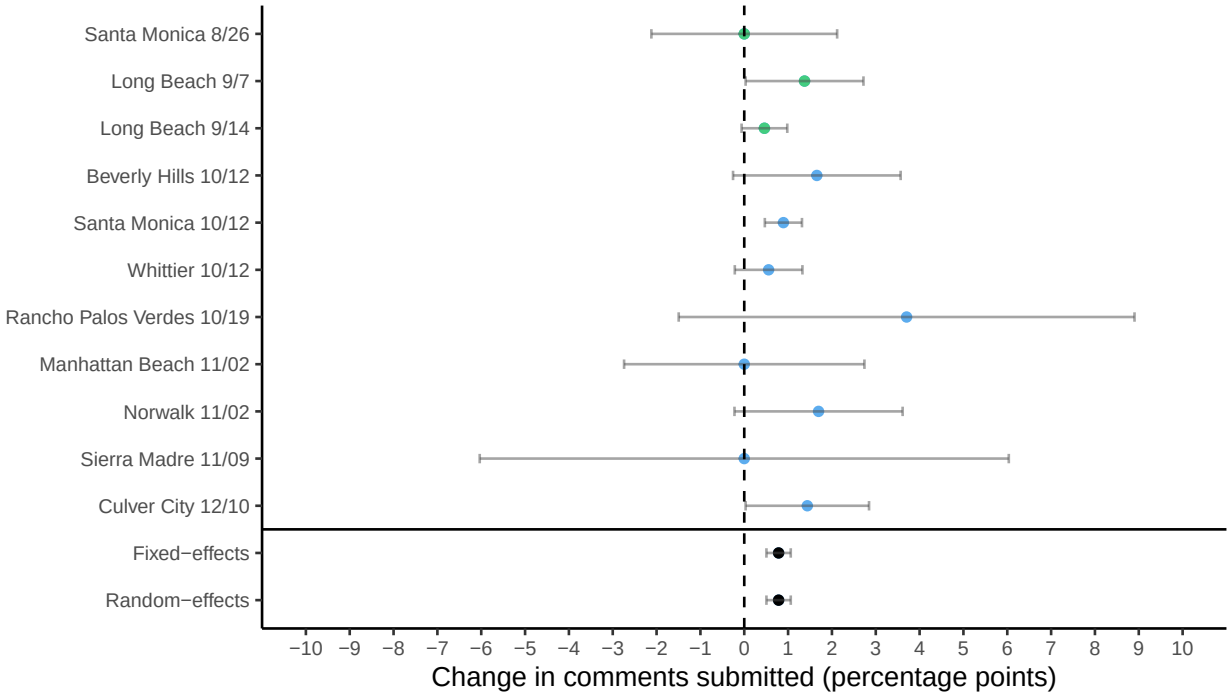


Figure 3: Meta-analysis of complier average causal effects, by council meeting

Note: Pilot studies in green.

By treatment group

Figure 4 depicts CACEs and ITTs by individual treatment group. In line with pre-registered hypotheses, Figure 4 shows that highlighting the costs of abstention had the largest effect on turnout (CACE = 1.44pp; RI p = 0.011; 95% CI [0.73, 2.15]), priming economic self interest was the second most effective (CACE = 1.01pp; RI p = 0.071; 95% CI [0.39, 1.63]), and the instructions-only treatment was the least effective (CACE = 0.54pp; RI p = 0.386; 95% CI [0.06, 1.03]).[24] Using the pre-registered Analytical procedures, the instructions-only treatment was significantly different from zero at the 5% level, while the economic cost and costly abstention treatments were each significantly different from zero at the 1% level. Only the estimates for the costly abstention and instructions-only treatments are significantly different from each other at the 5% level.[25]

[24]ITT randomization inference p-values are: 0.380 for T1, 0.089 for T2, and 0.039 for T3.
[25]Based on a two-tailed linear hypothesis test.

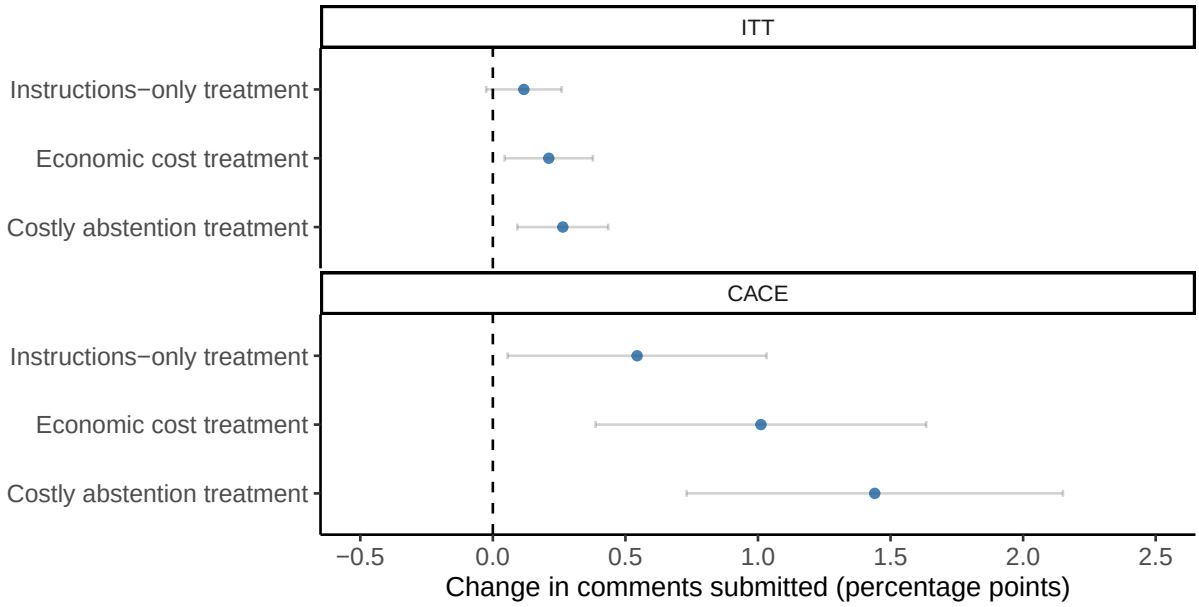


Figure 4: Effects by treatment group, all cities

To further asses confidence the costly abstention treatment was the most effective and aid interpretation, I fit a Bayesian linear multilevel model using prior distributions from the power analysis in my pre-registration. Coefficient estimates and posterior distributions can be found in Figure A.11. Figure A.12 provides a visualization of the posterior distributions of each coefficient and the posterior distributions of the differences between each coefficient, finding strong evidence that the null hypothesis they are equivalent can be rejected. Next, I compute Bayes factors[26] 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.[27] This provides strong evidence that

[26]The posterior odds of one hypothesis when the prior probabilities of the two hypotheses are equal. Or more colloquially, the ratio of the likelihood of one particular hypothesis to the likelihood of another hypothesis. A Bayes factor of 5 implies the alternative hypothesis is 5 times as likely as the null hypothesis given the data.

[27]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.

the costly abstention treatment was more effective than the instructions only treatment, and moderate evidence that it was more effective than the economic cost treatment.

These results confirm the (pre-registered) theoretical predictions. Lowering costs of participation with simple but detailed instructions of how to participate may have increased attendance, but only marginally. Priming economic concerns appears to be more effective than lowering participation costs alone. Finally, the strongest evidence supports Aytaç and Stokes (2019) theory that highlighting the perceived costs of abstention is more effective than lowering costs or economic self-interest alone.

Heterogeneous treatment effects

I find suggestive evidence that turnout in local elections is associated with an increased likelihood of being persuaded to make a public comment, and that the magnitude of this association is sizable.[28] OLS including a treatment-by-covariate interaction suggests that voters in local elections who opened the messages were 1.4pp more likely to comment than those who did not vote (see Figure 5). However, this association is only significant at the 10% level ($p = 0.086$).[29] A randomization inference based hypothesis test returns a p-value of 0.06. Voters were also more likely to open the emails across all treatment groups (see Table A), suggesting greater engagement in local politics in general.

There is therefore suggestive evidence that participation in local politics in the form of voting begets willingness to participate in other forms, such as attending council meetings, submitting public comments, and engaging with outreach campaigns. This information is potentially relevant to practitioners on a limited budget, as they may see higher returns to participation by targeting likely voters.

[28] I do not uncover evidence that the other pre-registered covariates of interest—building density or median area income—are strongly associated with commenting.

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

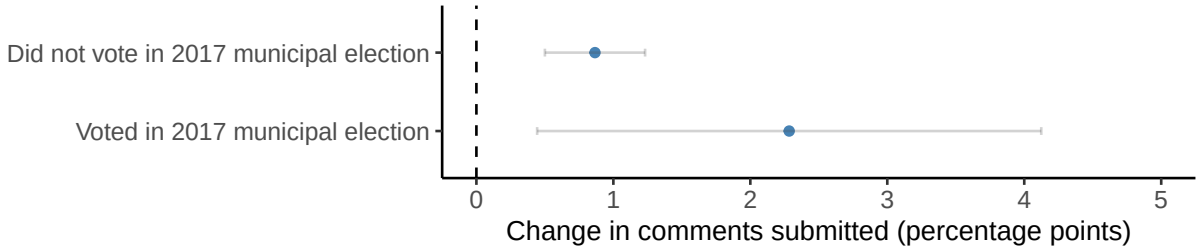


Figure 5: Complier average causal effects by turnout

How individuals commented

The vast majority of individuals (93%) submitted written public comments. In fact, the null hypothesis of no effect for spoken comments cannot be rejected. 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 (see Figure 6).

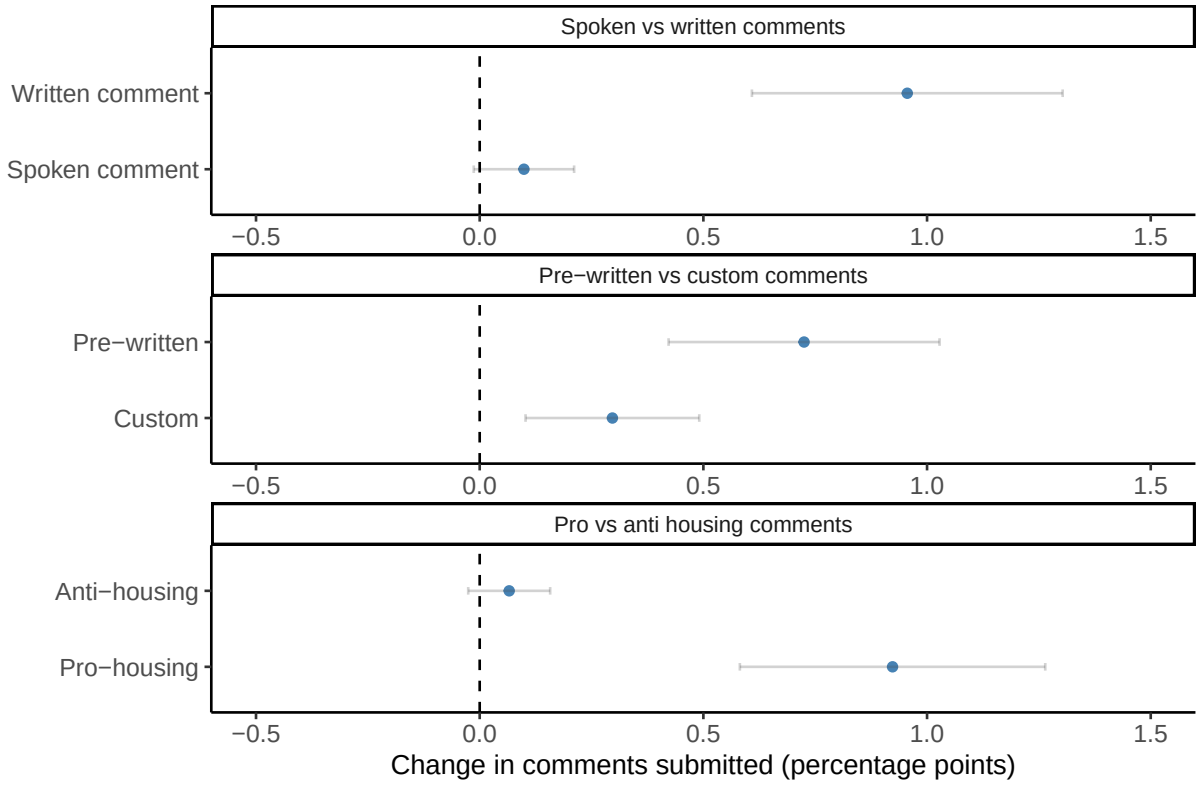


Figure 6: CACE by type of comment

Comment contents

I also examine the content of the comments in order to determine if the treatments provoked backlash in the form of anti-housing comments, if individuals submitted spoken or written comments, and if individuals submitted custom comments or used the pre-written comment supplied in the emails.

First, I investigate whether the treatments provoked backlash in the form of *anti*-housing comments. While some anti-housing comments were submitted (see Figure 6), they represented only 4% of total comments, and never comprised a majority of experimentally-induced comments in any council meetings.

As noted in How individuals commented, 29% of written commenters did not use our sample message, but instead drafted their own custom comments. I do not provide quotes of custom experimentally-induced comments as I did not ask for consent to re-print individuals' public comments. However, many of these custom comments were deeply personal and reflected individuals' lived experiences with high housing costs. 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.

Substantive impact of comments and changes in representation

In addition to the effect of contact on comment submission at the individual level, 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 in individual meetings. Overall, likely treatment-induced comments—i.e., comments made by individuals in one of the three treatment groups—represented 8% of total written public comments across all meetings. More significantly, likely treatment-induced comments represented 46% of pro-housing comments, and therefore swung the balance of pro-versus-anti housing comments toward a more equal footing.

The imbalances of comment makeup highlighted by Yoder (2020) that were not corrected merely be moving to an online setting (Einstein et al. 2021) appear to have been significantly altered by the treatments. This large change in the composition of comments caused by the treatments shows that outreach can change the representation of civic bodies to be more reflective of the broader public where simple increases in accessibility may not.

Non-experimental campaigns conducted by other groups, while not directly measurable, also appear to have had large impacts on comments in some of the observed meetings. For example, the abnormally large number of Manhattan Beach City Council meeting public comments on November 2 were related to an agenda item seeking agreement on language drafted by the local History Advisory Board for a plaque acknowledging the city's racially-motivated use of eminent domain to force the sale of beachfront property owned by Black families in 1927. This agenda item became the subject of "vitriolic public criticism backed by a viral, anonymous newsletter attacking [the History Advisory Board's] work" (McDermott 2021).

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	9	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 marginal effects of contact on overall turnout contrast sharply with, e.g., GOTV. In voter turnout settings, the large number of individuals who regularly turn out to vote makes the change in overall turnout due to campaigns relatively small. By contrast,

even a few new participants in city council meetings can drastically change the composition of comments due to generally low equilibrium participation rates.

Conclusion

Understanding how to motivate individuals to engage in personally costly collective action when their gains from mobilization are diffuse and long-term is an enduring and fundamental question in political economy. Well-established research indicates how and why homeowners with direct financial payoffs participate in local politics at disproportionately high rates. However, there is little evidence to suggest how to motivate renters—who face diffuse and long-term payoffs—to overcome the collective action problem.

I contribute to our understanding of how to motivate these groups to engage in costly political behavior using 8 email-outreach field experiments encouraging renters (N = 19,951 households) to participate in local politics in the form of commenting at city council meetings. In addition, I document how these motivational 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.

These results support three main theoretical and substantive conclusions. First, the high efficacy of a treatment arm applying social pressure and highlighting the high economic costs of abstention from local politics supports Aytaç and Stokes (2019)'s theory that abstention can be perceived as highly costly by individuals. While it is difficult to pin down if the

specific mechanism for this larger effect hinged on highlighting economic costs of abstention versus queuing emotions such as anger towards an out-group, the results nevertheless show that highlighting financial harm by an out-group is effective at raising the perceived costs of abstention.

Second, the large change in the composition of comments caused by the treatments shows that strategic outreach can make representation more reflective of the broader public where simple increases in accessibility do not. Pro-housing comments made up a majority of comments in over 50% of treated meetings, contrasting sharply with previous research finding majority pro-housing comments in less than 5% of online council meetings in equilibrium (Einstein et al. 2021). As these civic bodies make regular decisions that directly impact the day-to-day lives of residents and the status quo of participation is highly unrepresentative in equilibrium, outreach may have the ability to change local officials' perceptions of resident preferences toward a more representative picture.

Finally, the results show that unlike e.g., voting, email is able to increase political participation when participation is also conducted online. It therefore appears possible to meaningfully increase political participation in under-appreciated and low-turnout settings such as city council meetings using relatively low cost strategies.

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A Appendix

Housing supply and housing prices

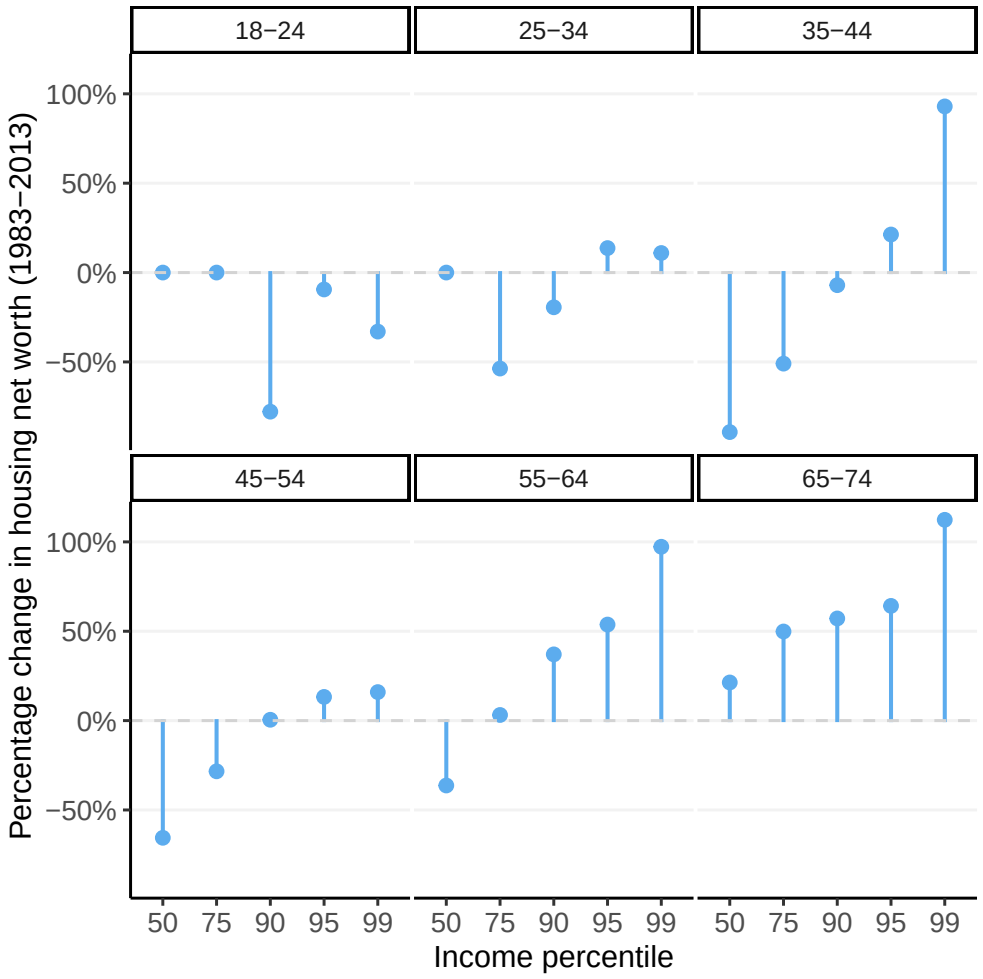

Figure A.1: Change in housing net worth by age and income percentile

Source: Glaeser and Gyourko (2018)

Balance

	Placebo (N=2007)		Treatment (N=17944)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
gender	0.52	0.50	0.53	0.50	0.02	0.11
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
yearbuilt	1964.93	18.63	1964.83	18.03	-0.14	0.75
units	34.25	64.90	34.39	66.40	0.08	0.96
dem	0.57	0.49	0.58	0.49	0.01	0.41
rep	0.13	0.33	0.11	0.32	-0.01	0.21
npp	0.24	0.43	0.24	0.43	0.00	0.73
vote_2020_general	0.79	0.40	0.81	0.40	0.01	0.28
vote_2017_municipal	0.10	0.30	0.09	0.29	-0.01	0.28
vote_2016_general	0.45	0.50	0.44	0.50	0.00	0.75

Figure A.2: 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	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
gender	0.52	0.50	0.52	0.50	0.54	0.50	0.54	0.50
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
yearbuilt	1964.93	18.63	1964.83	17.88	1964.83	18.33	1964.84	17.88
units	34.25	64.90	34.31	66.10	34.01	66.54	34.86	66.56
dem	0.57	0.49	0.58	0.49	0.60	0.49	0.58	0.49
rep	0.13	0.33	0.11	0.32	0.11	0.31	0.12	0.33
npp	0.24	0.43	0.25	0.43	0.24	0.43	0.24	0.43
vote_2020_general	0.79	0.40	0.80	0.40	0.81	0.40	0.81	0.39
vote_2017_municipal	0.10	0.30	0.09	0.29	0.10	0.30	0.09	0.29
vote_2016_general	0.45	0.50	0.45	0.50	0.45	0.50	0.43	0.50

Figure A.3: Covariate balance across all treatment groups

Treatment details

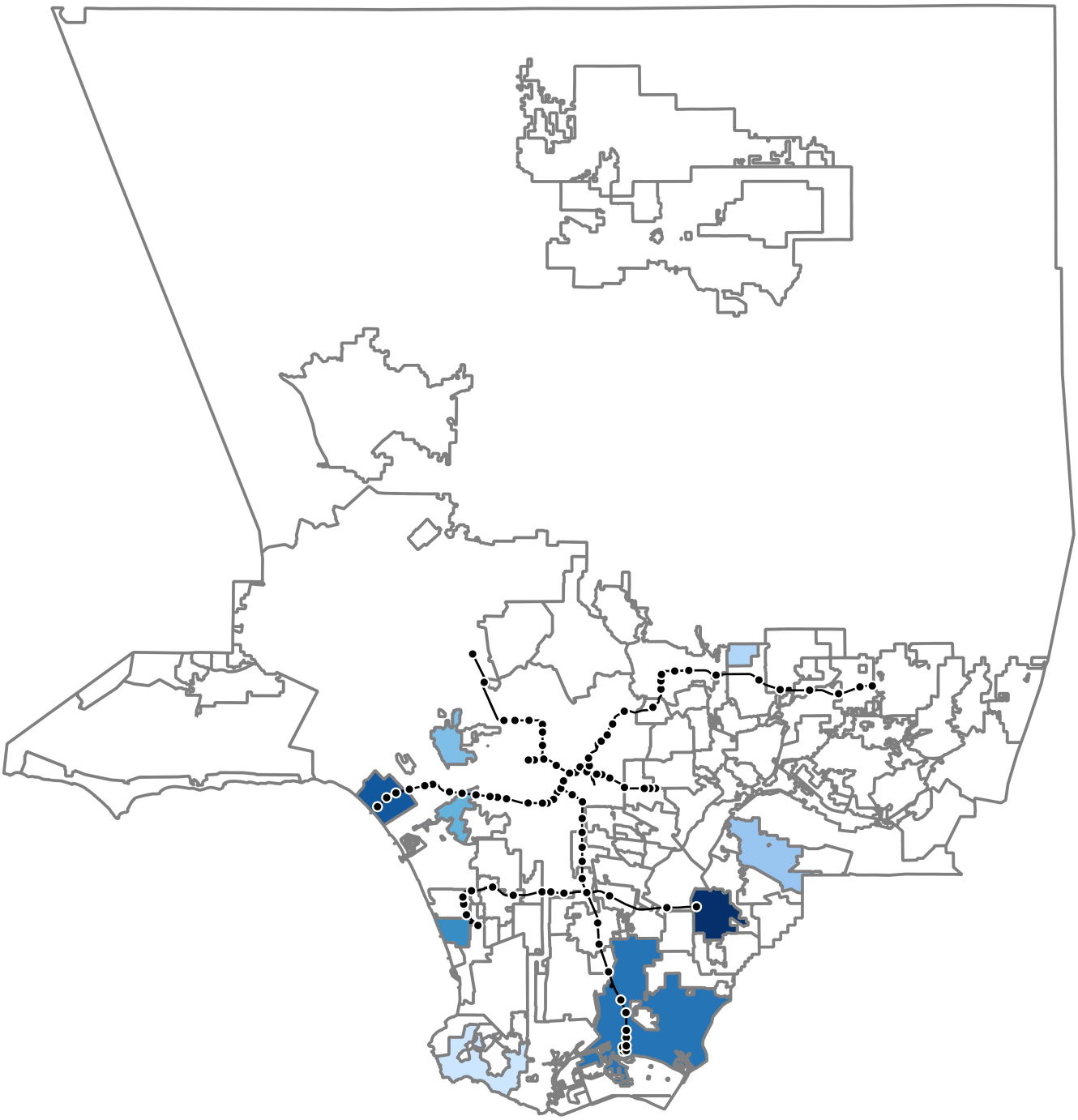

Figure A.4: Map of cities in Los Angeles county by experiment status

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

Tests for differential compliance

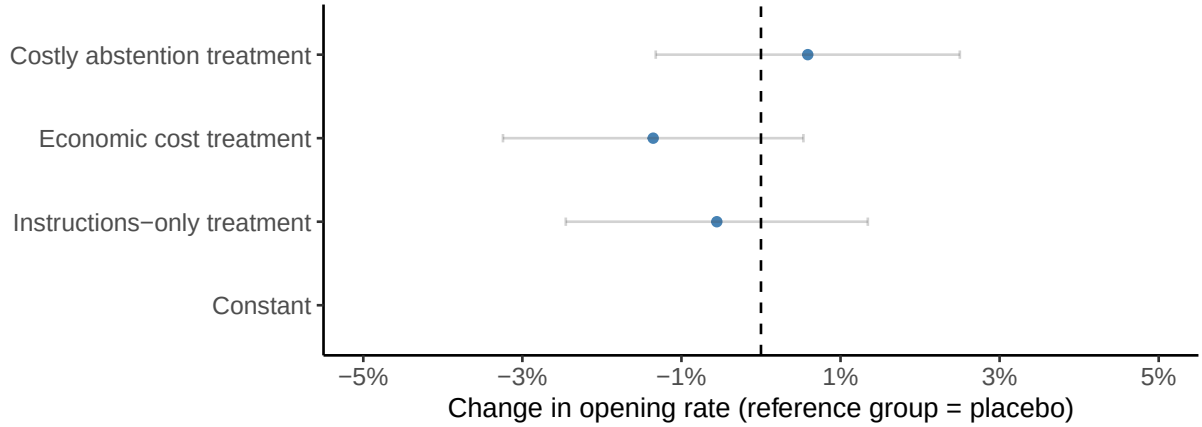

Figure A.5: Average treatment effect on email opening, all cities

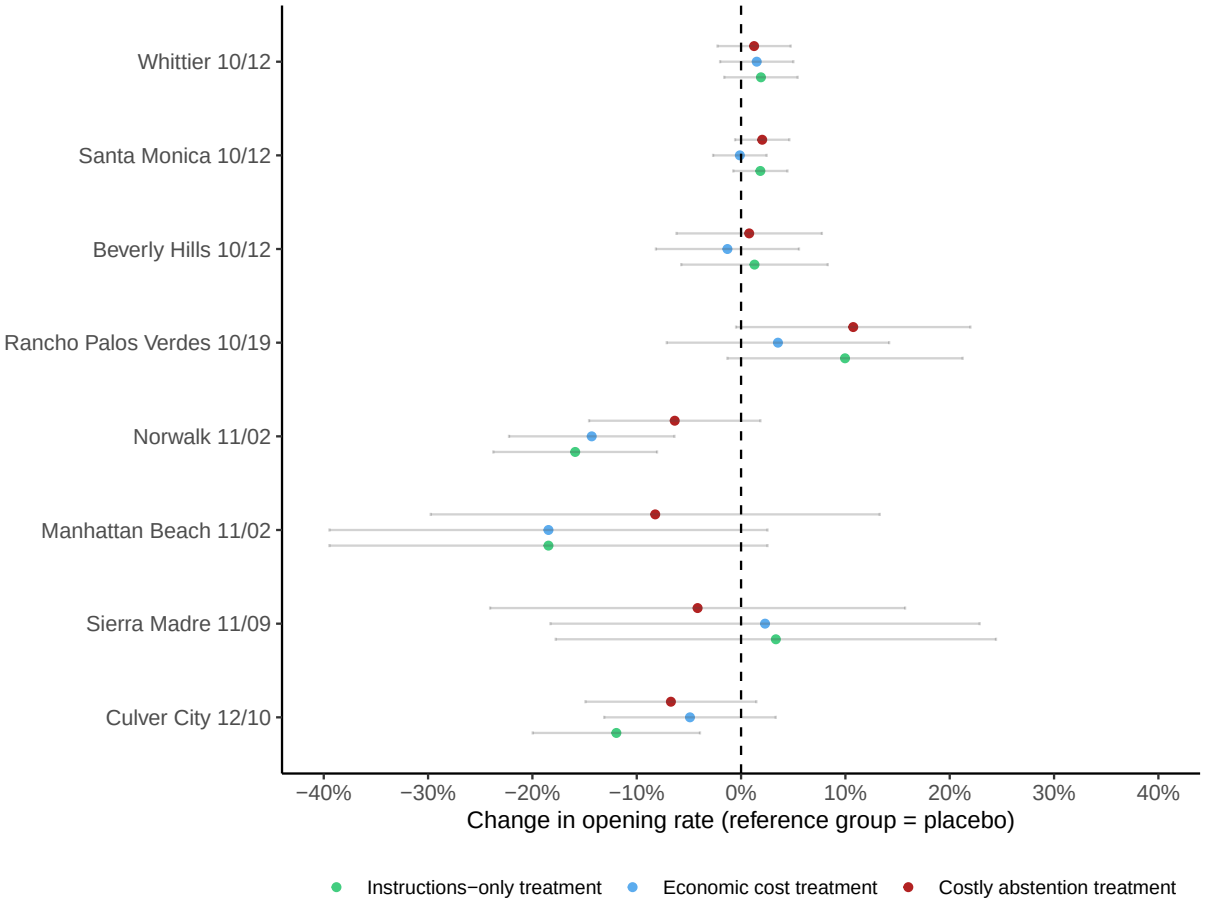

Figure A.6: Average treatment effect on email opening, by city

	Placebo	Treatment 1	Treatment 2	Treatment 3
(Intercept)	−0.321	−0.535	−0.565	0.216
	(0.980)	(0.569)	(0.560)	(0.563)
gender	−0.028	0.004	−0.012	−0.004
	(0.017)	(0.010)	(0.010)	(0.010)
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)
yearbuilt	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
units	0.000	0.000*	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
dem	0.033	0.012	0.033+	0.030
	(0.033)	(0.020)	(0.019)	(0.021)
rep	0.021	−0.008	0.003	−0.009
	(0.039)	(0.023)	(0.023)	(0.024)
npp	0.054	0.000	0.017	0.011
	(0.036)	(0.021)	(0.021)	(0.022)
vote_2020_general	0.028	0.031**	0.062***	0.030*
	(0.021)	(0.012)	(0.011)	(0.013)
vote_2017_municipal	0.041	0.057**	0.040*	0.035+
	(0.033)	(0.020)	(0.018)	(0.019)
vote_2016_general	−0.006	0.012	0.002	−0.019+
	(0.019)	(0.011)	(0.010)	(0.011)
Num.Obs.	2007	5984	6002	5958

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

Table A.1: 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 A.2: 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 A.3: Complier average causal effects

	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 A.4: Conditional complier average causal effect

Robustness

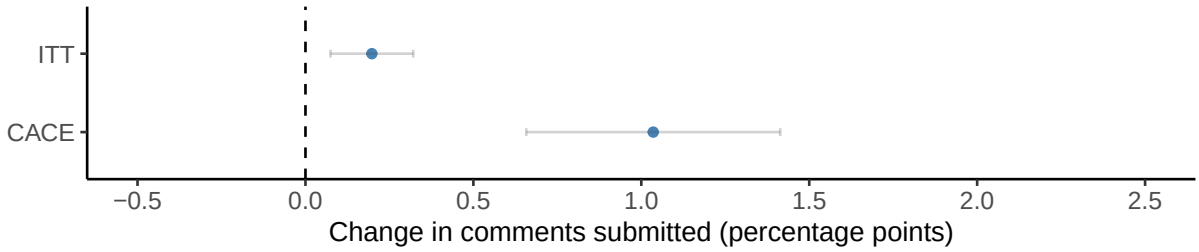

Figure A.7: Intent-to-treat effect and complier average causal effect, all cities (without covariate adjustment)

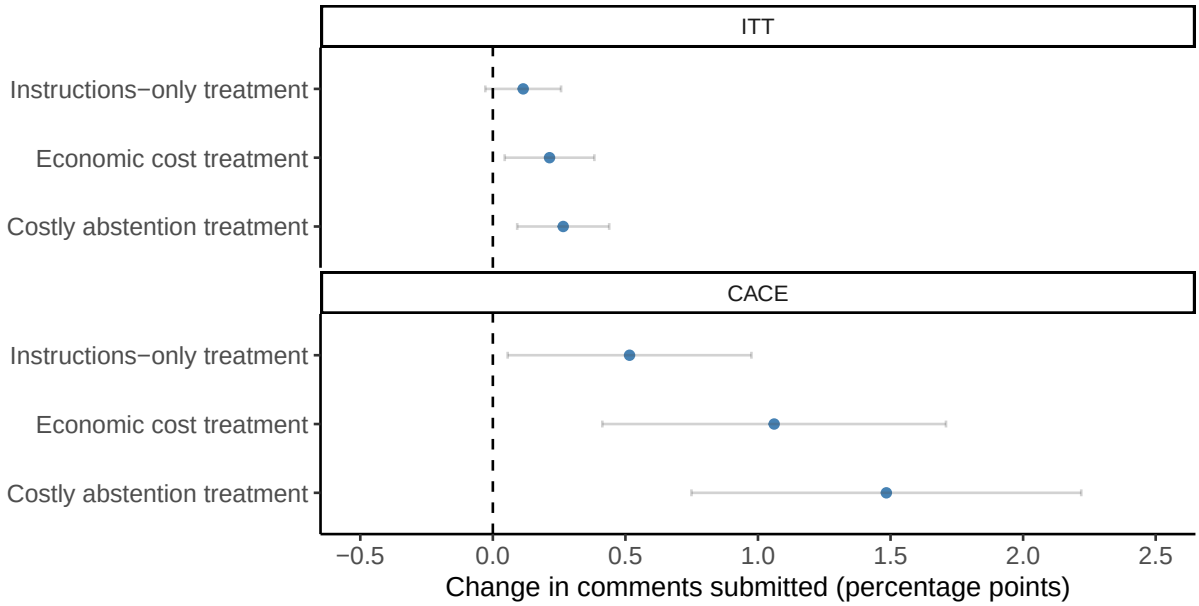

Figure A.8: Effects by treatment group, all cities (without covariate adjustment)

	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 A.5: ITT and CACE estimates from penalized maximum likelihood

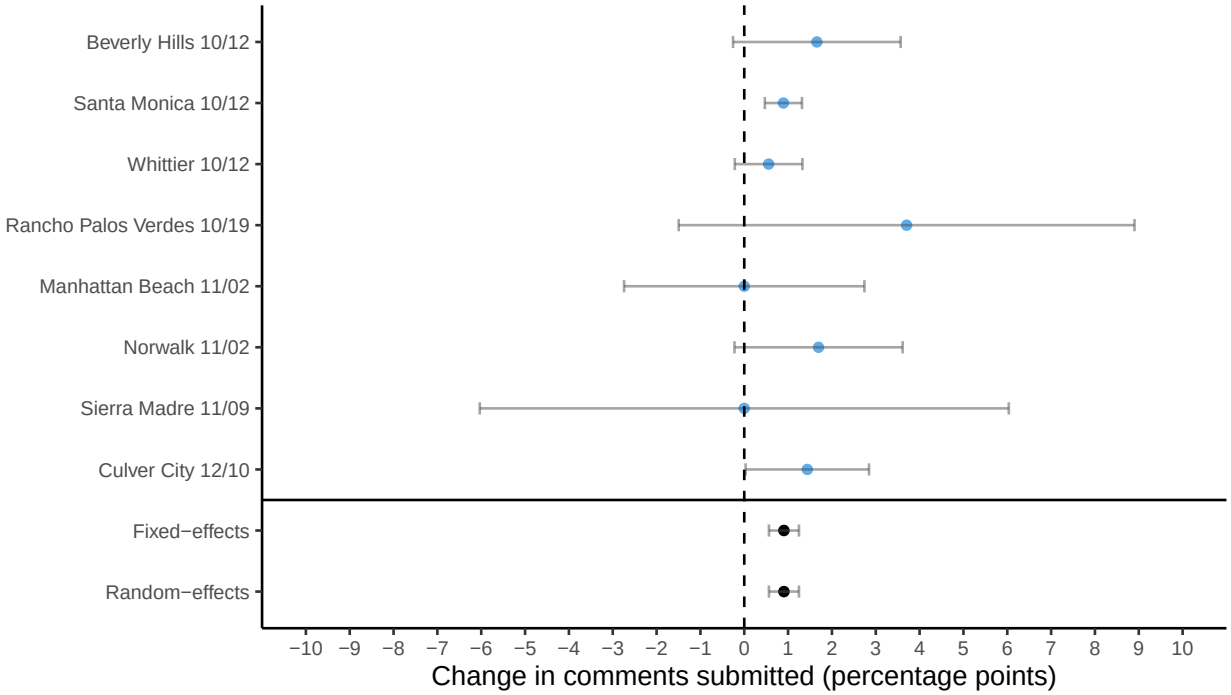

Figure A.9: Meta-analysis of complier average causal effects by city, excluding pilot studies

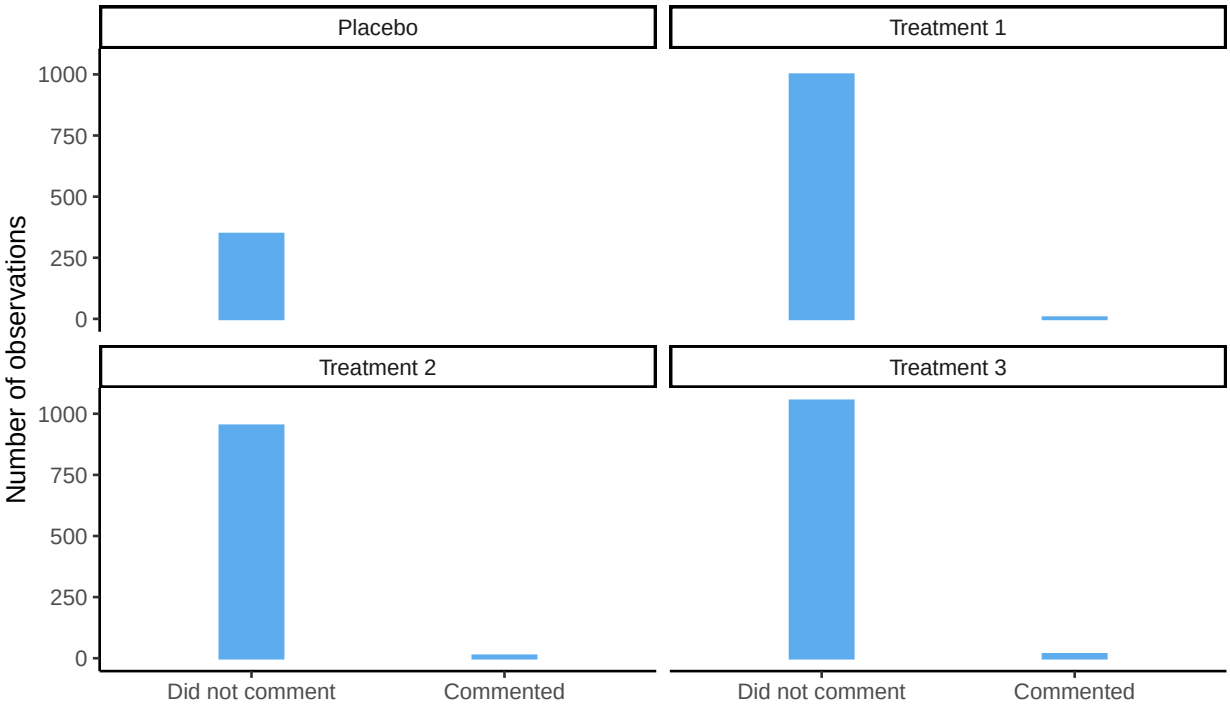

Figure A.10: Distibution of outcomes by treatment group (compliers only)

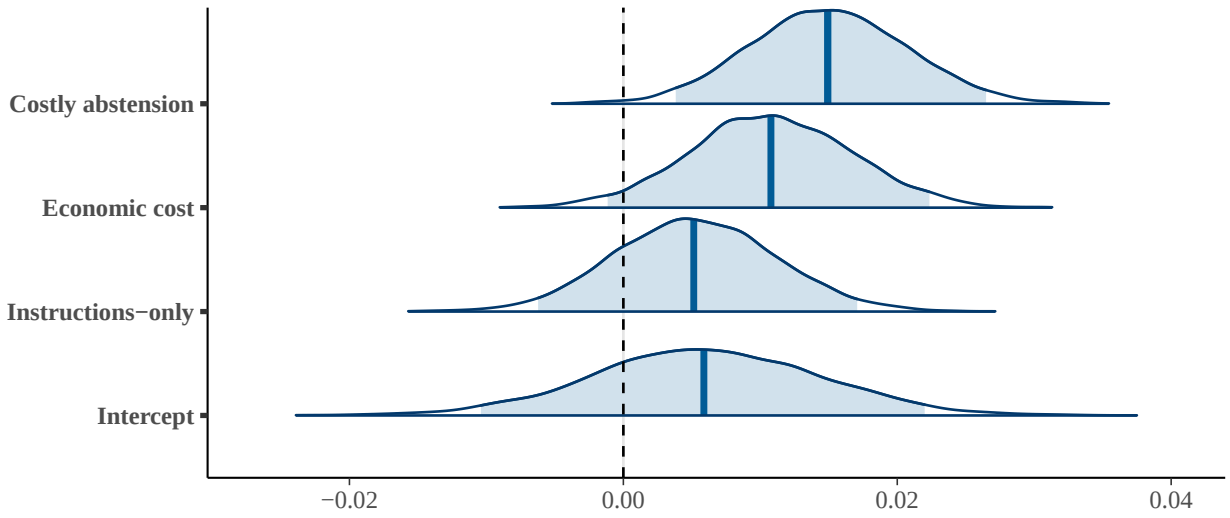

Figure A.11: Bayesian multilevel model: coefficient estimates and posterior distributions (includes city fixed effects)

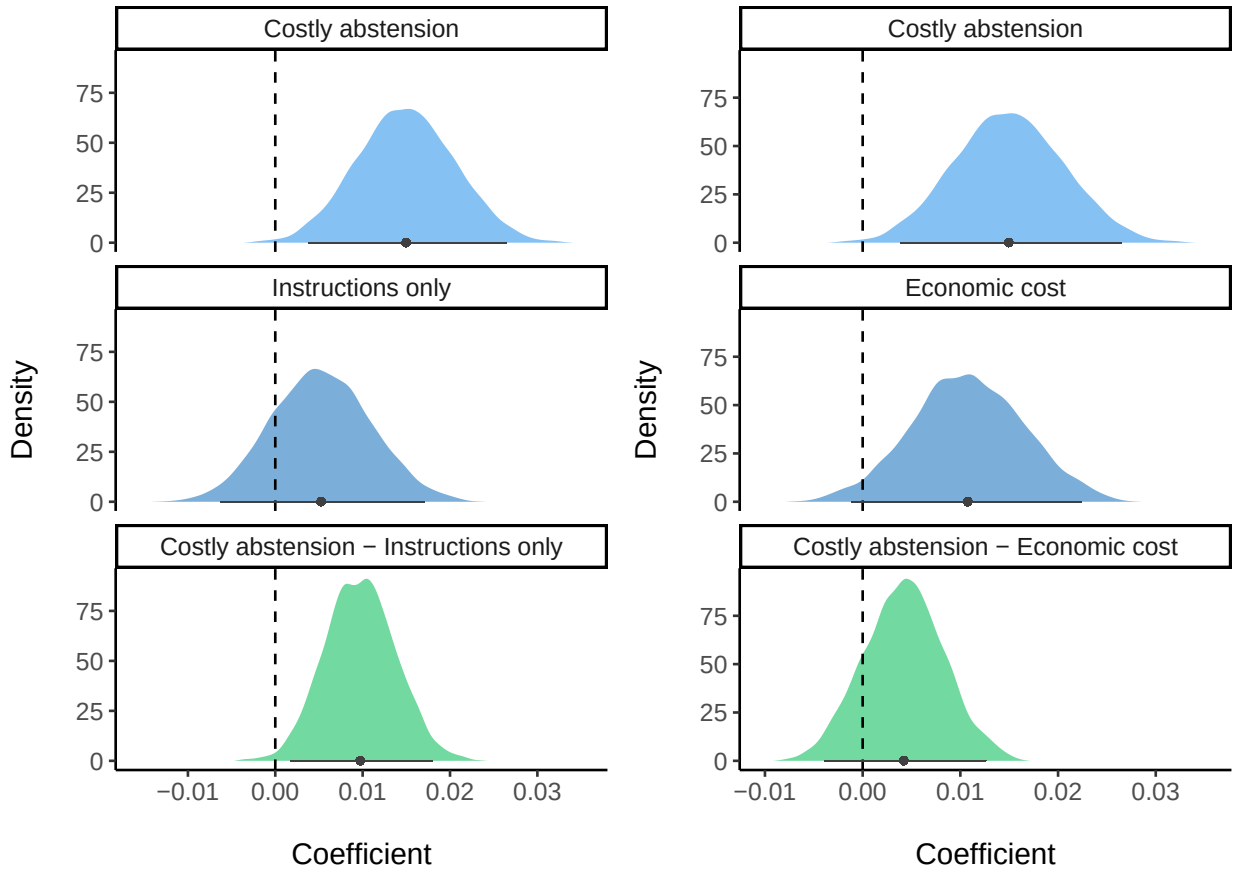

Figure A.12: Posterior distributions of costly abstention treatment, instructions only treatment, and difference