Bottlenecks for Evidence Adoption*

Stefano DellaVigna
UC Berkeley and NBER

Woojin Kim
UC Berkeley

Elizabeth Linos
UC Berkeley

April 2022

Abstract

Governments increasingly use RCTs to test innovations before scale up. Yet, we know little about whether and how they incorporate the results of the experiments. We follow up with 67 US city departments which collectively ran 73 RCTs in collaboration with a national Nudge Unit to improve city communications using nudges. The city departments adopt the nudge treatment in follow-on communication in 27% of the 73 RCTs. As potential determinants of adoption we consider (i) the strength of the evidence, (ii) features of the organization, such as "state capacity" of the city and whether the staff member working on the RCT is still involved, and (iii) features of the treatment, such as whether it was implemented as part of pre-existing communication. We find (i) a limited impact of strength of the evidence and (ii) some impact of city features, especially the retention of the original staff member. By far, the largest predictor of adoption is (iii) whether the communication was pre-existing, as opposed to a new communication. We consider two main interpretations of this finding: organizational inertia, in that changes to pre-existing communications are more naturally folded into the year-to-year city communication, and costs, since new communications may require additional funding. We find the same pattern for electronic communications, with zero marginal costs, supporting the organizational inertia explanation. The pattern of results differs from the predictions of both experts and practitioners, who over-estimate the extent of evidence-based adoption. Our results underline the importance of considering the barriers to evidence adoption, beginning at the stage of experimental design and continuing after the RCT completion.

*Preliminary and incomplete, do not cite without permission. We are very grateful to the Behavioral Insights Team North America for supporting this project and for countless suggestions and feedback as well as to Joaquin Carbonell for invaluable advice. We thank Fred Finan, Supreet Kaur, Gautam Rao, Richard Thaler, Eva Vivalt, and participants in seminars at the ASSA 2022, the Munich CESIfo Behavioral Conference, the MiddExLab seminar, Stanford University, and the University of California, Berkeley for helpful comments.

1 Introduction

In a drive to incorporate evidence into their policy-making, governments at all levels have increasingly rolled out RCTs to test policy innovations before scale up (e.g., Baron, 2018; Foundations for Evidence-based Policymaking Act, 2018; DIME, 2019).

This experimentation has the potential to improve public policy, if the most successful innovations are adopted into ongoing policies. But is this necessarily the case? How often are the innovations tested in RCTs actually adopted? To what extent do factors other than the strength of the evidence moderate this adoption, such as state capacity, turnover of personnel, or organizational inertia?

We know of little systematic evidence. Kremer et al. (2019) documents that out of a sample of 41 USAid-funded RCTs, the innovations from the RCTs were adopted at scale in only a dozen cases. Hjort et al. (2021) show that Brazilian mayors that received information on a successful tax collection nudge RCT are 10 percentage points more likely to adopt the tax communication. Vivalt and Coville (2022), Nakajima (2021), and Toma and Bell (2021) examine the interest in adoption by policy-makers of policies in mostly hypothetical scenarios. These path-breaking studies, as valuable as they are, do not indicate how policy organizations utilize evidence from trials they themselves are involved in conducting. Most related, Wang and Yang (2021) examine the policy experimentation by cities in China, and document patterns of adoption of evidence.

Related work from the private sector and non-profit organizations documents mixed evidence on whether results from A/B testing are adopted, even though the use of A/B testing continues to grow rapidly (Athey and Luca, 2019; List, 2022).

In this paper, we bring new evidence to bear from the context of Nudge Units, specifically from BIT-North America (BIT-NA). During the period under study, BIT-NA primarily supported North American cities to develop or revise light-touch government communications (e.g., a letter or an email) aimed at improving policy outcomes of interest to the city, such as the timely payment of bills and the recruitment of a diverse police force. Specifically, the behavioral scientists at BIT-NA and the staff members in the relevant department at the partner city co-designed different versions of a given communication and then tested what works using an RCT.

BIT-NA shared all the RCTs conducted between 2015 and 2019. As documented in DellaVigna and Linos (2022), the average nudge intervention in these 73 trials increases

the outcome of interest by 1.9 percentage points, a 13 percent increase relative to the baseline average of 15 percentage points, with substantial heterogeneity in the effect size. This data set though does not indicate whether the nudge innovation is adopted in subsequent communication by the city. This is not surprising, as data sets tracking adoption of the RCT innovations, as in Kremer et al. (2019), are sparse.

Thus, over the course of a year, starting in March 2021, we contacted each city department involved, and asked about the adoption of the featured communication, as well as additional information, e.g., staff retention. Ultimately, we are able to assess the adoption for *all* 73 RCTs and can thus estimate the rate of evidence adoption, as well as its determinants. We compare these results to predictions by researchers and by Nudge Unit staff members, along the lines of DellaVigna, Pope, and Vivalt (2019).

Before we turn to the results, we emphasize some features of our setting that make it a good fit to evaluate the adoption of the treatment innovations. For one, we observe the entirety of RCTs run by this unit and their adoption, not just the successful cases. Also, the sample of RCTs is large enough to grant statistical power, and yet the RCTs are comparable enough to enable inference. Furthermore, there is sufficient variation in the effectiveness of the interventions, the characteristics of the policy partner (the city), and the design of the trials, to provide evidence on a range of predictors of adoption.

We first document the overall level of adoption. Out of 73 trials, the nudge innovation is adopted in post-trial communications by the city 27% of the time. This level is comparable to the average prediction of the forecasters (32%).

We then consider three main determinants of evidence adoption: (i) the strength of the evidence, as measured both by statistical significance and by effect size, the latter being a normative benchmark, provided that the effect sizes after adoption are related to the original estimates; (ii) features of the organization (city), such as the "state capacity" of the city and whether the original staff member working on the RCT is still involved; and (iii) the experimental design, namely the type of nudge treatment used, and whether the communication was pre-existing or new.

We find surprisingly limited support for the role of evidence in adoption. We find no difference in adoption among results with negative point estimates (25% adoption), results with positive but not statistically significant estimates (25%), and estimates that are positive and statistically significant (30%). The likelihood of adoption increases with effect size (measured in percentage points), from 17% for effect sizes in the bottom

third to 36% for effect sizes in the top third, though this difference is not statistically significant at conventional levels. Along both of these dimensions, the impact of the evidence is less than what forecasters expect. We do find that the effect size is taken into account, *conditional* on adoption: in cases with multiple nudge treatment arms, the most effective arm was adopted in 5 cases out of 6.

Next, we find modest evidence for the predictive power of features related to the organizational capacity of a city. As a first proxy for overall government capacity, we use city population, finding a modest impact on adoption (32% for larger cities above the median versus 22% for smaller ones). As a second proxy, we compare cities that have been certified by What Work Cities as "data-driven" versus those that have not, finding again small differences (30% versus 24%). We do find a larger impact of whether the original city contact for the RCT is still employed by the city (33% versus 17%), though the difference is not statistically significant at conventional levels.

We thus turn to the last set of factors, the experimental design. The adoption rate is somewhat higher for interventions involving simplification (33%), as opposed to personal information and social cues (19% and 24% respectively), even conditional on the effect size of the intervention, a pattern anticipated by the forecasters.

By and far, though, the strongest predictor of adoption is another aspect of the experimental design—whether the city was already sending out the communication tested in the trial, in which case the trial involved changing the pre-existing communication to incorporate insights from behavioral science. In the 21 trials for which the communication was pre-existing, the adoption rate is 67% (14 out of 21). Conversely, in the 52 trials for which no similar communication had been sent prior to the collaboration with BIT, the adoption rate is only 12% (6 out of 52). This 55 percentage point difference, which is highly statistically significant, is far beyond the expectation of academics and BIT members, who expect a difference of only 11 pp.

This impact of the pre-existing communication is not only large but also robust. The impact is 53 pp. (s.e.=0.13) when only experimental design factors are taken into account, and 57 pp. (s.e.=0.15) when including all controls, including city fixed effects.

How do we interpret this finding? We discuss three potential mechanisms: (i) *cost allocation*, (ii) *state capacity*, and (iii) *organizational inertia*. First, pre-existing communications are already included in the city budget sheet, but new communications are not assured of funding in the years to come (*cost allocation*). When we compare online

communications, which have near zero marginal cost, to paper communications, which require financing of the mailer, though, we find nearly the same adoption gap between pre-existing and new communications. Second, cities with pre-existing communications may have better infrastructure for outreach, which is why they were already sending these communications, whereas other cities were not (*state capacity*). However, we find the same adoption gap when we control for city fixed effects, as well as for the policy area of the communication (e.g., parking ticket notifications are sent by all cities).

Thus, we argue that the primary interpretation is *organizational inertia*: in cases with pre-existing communication, there is a routine process to send the communication, and altering the wording to adopt an effective innovation is relatively straightforward, leading to high adoption. In cases with a communication set up specifically for the experiment, instead, there is no automatic pathway to send it again, leading to low adoption. Indeed, the low adoption of nudge treatments for experiments with new communication is entirely due to the cities sending no communication at all following the RCT. Instead, in cases of non-adoption of the nudge treatments of a pre-existing communication, the cities continue to send the status quo version in 5 out of 7 cases.

This inertia effect has a large impact. If all the effective nudges had been adopted, the RCTs would have increased the targeted outcome by 2.70 pp. (assuming the RCT effect sizes are stable over time). In contrast, the actual improvement is estimated to be 0.89 pp., thus realizing just one third of the potential gains. This gap is almost entirely due to the RCTs with new communication, which achieve only one tenth of the potential gains. For trials with pre-existing communications, the cities realize seventy percent of the gains. In the conclusion, we discuss a few implications, such as focusing the experimental design on interventions that are likely to be adopted (if successful), and allocating resources and attention to the adoption of successful policies.

An important question is how the adoption in our setting is likely to compare to the adoption in other settings, such as, for example, RCTs run in lower-income countries by researchers affiliated with organizations such as JPal, IPA, or CEGA. The BIT-NA setting is arguably one in which adoption is at least as likely as in most comparable settings. The primary goal of the RCTs in this case was to improve policy outcomes, as opposed to, say, testing models of behavior; thus, the incentives should be aligned for adoption of successful interventions. Also, the target adopter—the city department—was directly involved in the design, thus reducing the political or contextual barriers to post-

trial adoption. Finally, the cities in our sample are self-selected in pursuing evidence-based policy, being early partners in the BIT-NA network (Allcott, 2015). This suggests that the bottlenecks that we identified are likely of relevance to other contexts as well.

The paper relates to the literature on nudges (e.g., Thaler and Sunstein, 2008; Benartzi et al., 2017; Milkman et al., 2021), as well as to the literature on research transparency (Simonsohn, Nelson, and Simmons, 2014; Brodeur et al., 2016; Camerer et al., 2016; Christensen and Miguel, 2018; Andrews and Kasy, 2019). Nudge Units, with a mandate to collect evidence via RCTs to improve public policy, have emerged as an example of best-practice transparency, from the initial stage of (typically) posting pre-analysis plans to tracking down the implementation of evidence.

The paper also relates to the literature on scaling RCT evidence (Banerjee and Duflo, 2009; Allcott, 2015; Muralidharan and Niehaus, 2017; Meager, 2019; Vivalt, 2020). The Nudge Unit interventions were already partially "at scale", since they applied nudge treatments in the literature to a policy setting, with larger sample sizes, as documented in DellaVigna and Linos (2022). We point out a critical bottleneck in the further scaling of the evidence: the translation of the RCT results into continuing government practice.

2 Setting and Data

2.1 Trials by Nudge Unit BIT-NA

Nudge Units. We analyze all city-level trials conducted between 2015 and 2019 by a large "Nudge Unit" operating in the US: the Behavioral Insights Team's North America office (BIT-NA). This team, like other "Nudge Units," aims to use behavioral science to improve the delivery of government services through rigorous RCTs, and to build the capacity of government agencies to use RCTs independently. In 2015, the UK-based Behavioural Insights Team (BIT) opened its North American office (BIT-NA), to support a new initiative called "What Works Cities." This initiative aimed to provide technical assistance to mid-sized cities across the US in using data and evidence in policy-making. Mainly through the What Works Cities initiative, BIT-NA has collaborated with over 50 U.S. cities to implement behavioral experiments within local government agencies. In interviews, the leadership noted that their primary goal of these experiments is to measure "what works" in moving key policy outcomes. The vast majority of their

projects during the period under study are similar in scope and methodology. They are almost exclusively RCTs, with randomization at the individual level; they often involve a low-cost nudge using a mode of communication that does not require in-person interaction (such as a letter or email); and they aim to either increase or reduce a behavioral variable, such as increasing voting, or reducing late utility bill payments. Furthermore, BIT-NA embraces practices of good trial design and research transparency. All trial protocols, including power calculations, and results are documented in internal registries irrespective of the results. All data analyses go through multiple rounds of code review.

Figure A.1a-b shows an intervention from BIT-NA aimed to increase the payment of delinquent fines from traffic violations. The control group received the status-quo letter (Figure A.1a), while the treatment group received a simplified letter (Figure A.1b). The outcome is measured as the share of recipients making a payment within three months.

Process of Experimentation. In order to better understand adoption of RCT evidence, it is useful to consider the prior process of conducting an RCT in this context. The left panel of Figure 1a outlines the process. Trials are developed out of an initial submission by a specific city that is interested in collaborating with BIT-NA, as part of a broader technical assistance package. In most cases, a series of scoping calls between a city staff member and a BIT-NA behavioral scientist help define the exact behavioral outcome of interest; the potential sample size; and the possibility for a scalable light-touch intervention. The latter criterion is noteworthy: unlike purely academic research whose main purpose is to contribute to generalizable knowledge, most trials are explicitly designed with scalability in mind.

Once BIT-NA confirms that a well-powered trial is possible, department staff and other city stakeholders (e.g., legal team and communications team) collaborate with behavioral scientists at BIT-NA to co-design the specific intervention and evaluation plan. This stage also is important in the context of potential adoption—many of the hurdles for scaling up evidence such as legal or political barriers have already been overcome at the RCT design stage. Moreover, the regular interaction with city staff in the design stage of the RCT creates a natural agent to sustain the implementation of the experimental results. Before running the trial, the intervention and evaluation design as well as the related hypotheses are recorded.

Following the RCT, the BIT-NA staff analyze the results and produce a non-technical

report typically a couple pages long that is shared with the city alongside a presentation to the relevant stakeholders. The results of all trials for a given city are also shared with city leadership. This process of creating policy briefs and presentations for policy audience should ensure that the relevant players can understand and act on the evidence. Indeed, in the BIT-NA case, several of the staff contacts in the cities reported remembering the results, and in 14 cases out of 15 cases, they recalled them correctly. After this stage, while there are at times additional interactions between the city and BIT-NA team, any adoption of the nudge treatment is not recorded systematically (which is what we achieve with our follow-up investigation).

Sample of Trials. To identify the relevant BIT-NA trials, we adopt a very similar sample selection as in the DellaVigna and Linos (2022) paper which analyzed the average treatment effects of the RCTs run by BIT-NA, as well as by the Office of Evaluation Sciences (OES). As Figure 1b shows, from the universe of 93 trials conducted, we limit our sample to projects with a randomized controlled trial in the field, removing just 2 trials. We then remove 8 trials without a clear "control" group, such as horse races between two behaviorally-informed interventions, 3 trials with monetary incentives, and limit the scope further to trials with a primary outcome that is binary, removing just 2 trials. Compared to the sample in DellaVigna and Linos (2022), we exclude 8 trials run with partners other than US cities (charities and cities in Canada and Africa), in order to focus on a more comparable set of trials in terms of adoption. Finally, while contacting cities, we identified and added 3 additional trials run by the same cities in collaboration with BIT in later years. This yields the final sample of 73 trials.

Impact of Nudges. DellaVigna and Linos (2022) estimate the average impact of nudges in terms of percentage point on the policy outcome, relative to the control group. We reproduce the regression in Column 1 of Table A.1, and in Column 2, we present the average for the city sample used in this paper. For BIT-NA trials, we estimate an impact of 1.9 percentage points (s.e.=0.6), a 13 percent increase relative to a control group level of the outcome of 15.1 pp. In Figure 2 we present the trial-by-trial evidence for the BIT-NA sample, plotting the effect size for the most effective nudge arm compared against the take-up of the targeted outcome in the control group. The figure also denotes the adoption and the pre-existence of the trials, two key aspects we revisit later.

Features of Trials. In Table 1 we briefly describe the characteristics of the 73 trials in our sample. In Column 1, we first summarize information on the entire sample,

starting with the effect size: 45% of the trials have at least one arm with a positive and statistically significant effect size, and 47% of trials have at least one arm with an effect size larger than 1 percentage point. In the next rows, we split trials based on organizational features of the city. We consider whether the city has been certified by What Works Cities, which uses a set of pre-determined criteria to validate that a city is a "data-driven, well-managed local government", and whether the city contact for the trial is still employed by the same city department.

We then categorize the trials by the experimental design. This includes whether the communication was pre-existing or not before the trial and which behavioral mechanisms were used in the nudge communication. There are typically multiple mechanisms applied within a single nudge treatment. The most frequent mechanisms include simplifying the communication, for example, by using clear instructions or plain language (53% of trials); drawing on personal motivation such as personalizing the communication or using loss aversion to motivate action (58% of trials); and exploiting social cues or building social norms into the communication (56% of trials).

Next, we consider the policy area. A typical "revenue & debt" trial involves nudging people to pay fines after being delinquent on a utility payment, while an example of a "benefits & programs" trial encourages households to apply for a homeowners tax deduction. The "workforce and education" category includes prompting police applicants to show up for their in-person examination. One "health" intervention urges people to take up a free annual physical exam. A "registration" nudge asks business owners to register their business online as opposed to in-person, and a "community engagement" intervention motivates community members to attend a local town hall meeting. The most common categories are revenue & debt, registration, and workforce & education.

Finally, we present information on the medium of communication. The communication is delivered via a physical medium in the majority of cases, either in a physical letter (38%) or postcard (22%), as opposed to online or digital forms of delivery.

Columns 2 to 7 characterize subsamples of the trials along three main determinants of interest that we considerlater in the results: a split by the median of the effect sizes (Columns 2 and 3), by whether the original city collaborator has departed or has been retained (Columns 4 and 5), and by whether trials that used a new versus a pre-existing communication (Columns 6 and 7). Reassuringly, we have more than 20 trials out of 73 total within each subsample, which allows us to identify the impacts of each dimension.

There are some differences in the charactertistics of trials along these dimensions. For example, pre-existing communications appear to have higher effect sizes and tend to be physical letters, while fewer city staff members have been retained for trials that use physical letters. These correlations highlight the importance of collecting data across potential determinants and investigating adoption in a multivariate setting.

2.2 Adoption of Nudge Treatments

While we can code the effect size for each intervention from the remarkably comprehensive record that BIT keeps about every trial they conduct with city partners, the records do not keep track of whether the interventions used in a trial were adopted into ongoing government practice after the trial. That is, it was unknown whether or not the city communications following the RCTs incorporated the wording and format used in the nudge treatment arms.

As summarized in the right panel of Figure 1a, we thus emailed each city department involved in the RCTs and followed up with additional emails and occasionally phone calls. Collecting the full data set took one year and an average of four interactions with each city department. In our conversations with the city staff, we first described the context of the past collaboration with BIT, provided the templates of the communications sent out in the trial, and asked whether the city was still sending the communication. If so, we asked them to send us the current version. If they were not sending the communication, we confirmed whether they had sent the communication anytime after the trial, even if they were no longer doing so (e.g., due to Covid). In addition, we asked whether the communication had been used before the trial or was sent for the first time in the trial itself (i.e., whether it was pre-existing or new). We also checked whether the city staff members who worked on the trial were still employed by the city. From our correspondences, we took note when they referenced the results of the trial (which we did not reveal) and also recorded any barriers they mentioned in adopting the nudges. Figure A.2 provides some further information on the number of contacts and time taken to obtain the information for each trial.

Ultimately, we were able to contact and obtain responses about the adoption for all 73 RCTs. We define adoption as the case in which "*one nudge treatment arm has been used in communications from the city department after the RCT*". In the large majority

of cases, whether a nudge treatment arm was adopted was straightforward to code. In the case of the example in Figure A.1, the communication used most recently (Figure A.1c) is clearly based on the nudge treatment letter (Figure A.1b), and we thus code this case as an instance of adoption. In other cases, the recent communication resembles the communication in the RCT control group, or there is simply no communication sent out in the years following the RCT; we code cases like these as instances of no adoption.

In a small number of cases, the coding of adoption is not obvious. In such cases, documented in Online Appendix Section A, we use the following criteria. First, in case there are multiple components to the nudge intervention, we count an RCT result as adopted if at least 50% of the nudge components pre-specified in the BIT trial protocol are present in the post-trial communication. For example, suppose a trial tested a revised utility bill by simplifying the payment request, adding a peer comparison, and personalizing the message. If the current utility bill incorporates the simplification and the peer comparison but not the personalization, we still count it as adoption, but if it only includes personalization, we do not. Second, in case there is no communication as of the most recent year (2021 or 2022), but the city department documents that there was a communication from the city similar to the nudge treatment sometime after the RCT, we count it as adoption.

2.3 Forecasts of Results

Forecast Survey. Along the lines of DellaVigna, Pope, and Vivalt (2019), we collect predictions of research results to compare with the actual results, to provide evidence on the direction of updating in the research community. We posted on the Social Science Prediction Platform a 10-minute Qualtrics survey (reported in the Online Appendix Section B) which asked for predictions of the main results. This survey was posted before any of the results were posted publicly.

Specifically, after presenting the setting and the question, we asked for (i) a prediction of the average rate of adoption for the 73 nudge RCTs; (ii) an open-ended question on possible reasons for non-adoption: "*When cities do not adopt the nudges from the trials, what do you think are the main reasons?*"; (iii) the prediction of how adoption would vary as a function of 7 determinants, 2 about strength of evidence (1 on effect size, 1 in statistical significance); 3 about city characteristics (1 about staff retention, 1 about

state capacity, 1 about certification as an evidence-based city); 2 about experimentation conditions (1 about nudge content and 1 about pre-existing communication); (iv) we asked for a qualitative assessment of how the likely adoption of evidence in this context would differ from the adoption of evidence in firms, and in RCTs run in low-income countries (e.g., by J-Pal or CEGA).

We obtain 118 responses, as detailed in Table A.2, with 19 response from individuals affiliated with Nudge Units, 67 researchers (university faculty, post-docs, and graduate students), and 14 government workers, among others.

3 Results

3.1 Average Adoption

The first result is the average rate of adoption. In Figure 3 we display three relevant benchmarks. As the first columns show, 78% of the trials have at least one nudge arm leading to a positive effect size, that is, an improvement in the outcome variable, and 45% of the trials have a nudge arm with a positive and statistically significant increase. These are plausible benchmarks for normative rates of adoption. The third column shows the average prediction among forecasters, at 32%; thus, the forecasters are pessimistic regarding adoption, compared to the first two benchmarks (which they were shown in the survey). Forecasters working in nudge units are slightly more optimistic, with a forecast of 37%, compared to 32% for researchers (Table A.2).

As the final column shows, the average rate of adoption is 27%, that is, adoption in 20 out of 73 trials. The result is not statistically significantly different from the average forecast, though it is significantly lower than the initial two benchmarks based on the share of positive, or significantly positive, results.

3.2 Determinants of Adoption and Survey Predictions

Before we consider the determinants of adoption separately, we present some evidence from the open-ended responses that the forecasters contributed when we asked them about possible bottlenecks for evidence adoption. As the word cloud in Figure 4 shows, the forecasters stress the potential importance of effect size ("small", "lack" and "effect",

stressing that small effect sizes may not be implemented), organizational inertia ("inertia" and "status quo"), cost of implementation ("cost" and "budget"), the importance of the staff ("staff", "people", and "turnover"). Thus, the survey respondents highlight some of the key channels we focus on, even though the open-ended question predates the presentation of the variables we analyze.

3.3 Adoption: Evidence-Based Determinants

The first set of determinants are the ones which are arguably normative determinants of adoption. To the extent that the long-term expected impact of a communication is monotonically related to the results in the RCTs, the rate of adoption should be related to the effect size (in percentage points) in the RCT, as well as to the statistical significance of the nudge arms.

In Figure 5a we present the rate of adoption as a function of the effect size, splitting the RCTs into thirds by the percentage point effect of the most effective nudge arm in each trial. In the first three grey bars, we plot the average prediction among forecasters of the adoption rate. On average, the forecasters expect an adoption rate of just 13% in the lowest third, and of 49% in the top third. In reality, the adoption is increasing in the effect size, but the impact is not as large as forecasted, and is not statistically significant at conventional significance levels: the actual adoption is 17% in the bottom third for effect size, 28% in the middle third, and 38% in the top third. Considering the evidence in finer bins, as the bin scatter in Figure 5c shows with 10 bins, the responsiveness to effect size is quite tentative.

It is possible though that cities are responding even more to statistical significance than to effect size. The two measures differ because not all statistical arms are equally powered (though they are generally well powered, compared to a typical academic paper on nudges, as documented in DellaVigna and Linos, 2022). In Figure 5b, we show that on average forecasters indeed expect a strong response by statistical significance. In reality, as the blue bars on the right show, the rate of adoption is the same for results that are negative or zero (25%), or positive but not statistically significant (25%), and only slightly higher for results that are positive and statistically significant (30%). Thus, statistical significance does not seem to play a role in adoption.

We consider one final component to evidence-based adoption: for RCTs with multiple

nudge treatment arms and one of them is adopted, is the most effective innovation adopted? Figure 5d answers largely in the affirmative: out of 6 such trials, in 5 cases the treatment with the highest effect size is the one adopted. Thus, when there has been a decision to adopt, effect size does play a key role. In the next sections we thus explore what factors limit the extent of evidence-based adoption.

3.4 Adoption: Organizational Features

An important set of determinants to adoption discussed in the literature are organizational features that may drive or hinder adoption of evidence (see de Vries, Bekkers, and Tummers, 2015, for a systematic review). For example, some organizations may have more "organizational slack" or state capacity to enact reforms and act on the evidence accumulated (Besley and Persson, 2009) Previous evidence suggests that the main determinants of "organizational slack" are size, wealth, and personnel. In particular, larger or wealthier organizations are more likely to innovate (Naranjo-Gil, 2009; Fernandez and Wise, 2010). In our context, an agency may be more likely to act on evidence if they are larger or wealthier. An institution may also be more likely to act on the evidence if the personnel responsible for the experiments is still working in the relevant unit.

To be sure, many studies also point to political constraints, external pressures, or networks outside of a given organization that may drive or limit the adoption of successful innovations. In our setting, such factors are not likely to be as important in the short-term since the type of innovations that are tested using an RCT have already been vetted for political, legal, and communications feasibility in order for the field experiment to be approved.

We measure "state capacity" in multiple ways. First, we partition cities into halves by population. As Figure 6a shows, there is some difference by city size, though relatively modest, with 22% adoption in the smaller cities, and 32% adoption in the larger cities. As a second proxy for "state capacity", we consider the certification from What Works Cities described in Section 2.1. As Figure 6b shows, there is an even more modest difference along this line, 24% versus 30%.

A different dimension of the organization, as mentioned above, is the personnel. We separate trials depending on whether at least one of the original city staff members who helped to design and implement the experiment is still working in the city at the time of

contact, which is typically a few years after the initial experiment.[1] If the staff member is still employed, it is more likely that the city has an an internal "champion" with the expertise and the institutional memory to continue the nudge innovation. As Figure 6c shows, there is a positive impact of this staff retention level, with adoption rates of 19% in cases when the original staff left, versus 33% when they were retained, but this 14 pp. difference falls short of statistical significance (p=0.12).

3.5 Adoption: Experimental Design

The final set of conditions considers the experimental design. We examine first whether policy-makers have a preference for particular behavioral mechanisms, even conditional on the effect sizes that the treatments yield. We distinguish between simplification as a mechanism, which seems uncontroversial, versus social comparisons or personal motivation which can, at least in some contexts, be seen as more aggressive interventions. Figure 7a shows that forecasters on average expect trials with simplification to be more often adopted than trials using other behavioral mechanisms. Indeed, the results follow this pattern, with 33% of trials adopted for simplification versus 19% for personal motivation and 24% for social cues (though the difference between simplification and the other conditions is not statistically significant at conditional levels).

Next, we turn to a second aspect of the experimental design, whether the communication in the trial was pre-existing. To clarify, suppose that in a trial, BIT and the city send reminder letters for timely utility bill payment. We label such letters *new communication* if the city had not been sending such letters before the trial. We label them as *pre-existing communication* if the city had been sending the letters before the trial, and the trial incorporated new nudge features in the treatment arms, compared to the status-quo communication. As Figure 7b shows, in the 21 trials in which there was a pre-existing communication and the city tested variations using nudges, the adoption is 67% (14 out of 21). Conversely, in the 52 trials in which the communication was new, the adoption rate is only 12% (6 out of 52).[2]

[1]Most trials have only one (42% of trials) or two (34%) affiliated city staff members listed on the trial protocol. We checked whether at least one of these listed staff members is still working in the same city *department*. In two trials, the staff member was still working for the city, but had moved to a different department. We do not count these two trials as cases of staff retention, but including them does not change the results.

[2]The *new-communication* category actually includes two groups of trials, a first group in which the

This 55 pp. difference, which is highly statistically significant ($p<0.01$), is five times larger than the expectation of forecasters who predict only an 11 pp. difference on average. Government workers who may have more experience with such matters are more accurate than nudge unit staff or researchers, but their average predicted difference of 22 pp. is still less than half the actual impact (Table A.2).

To appreciate how predictive of adoption this one single variable is, we revisit Figure 2, which reports all the nudge treatment effects and also labels whether the nudges were adopted (green versus pink) and whether the communication was pre-existing (diamond) versus new (circle). Figure 2 shows that the large majority of cases of adoption are for pre-existing communication. Conversely, almost all new communication does not lead to adoption, including two of the most positive treatment effects of over 20 percentage points.

3.6 Adoption: Multivariate Evidence

So far, we have considered each determinant of adoption on its own, but there could be a correlation between the different factors. What if, for example, part of the impact of pre-existing communication captures different effect sizes, or different features of the cities implementing the trials?

In Table 2 we present the estimates from a linear probability model predicting adoption considering first only evidence-based determinants (Column 1), only organizational features (Column 2), then only experimental design features (Column 3), and finally all three conditions together (Column 4). Column 1 shows that there is essentially no predictive power for adoption from whether the results are statistically significant and from effect size in percentage points. Turning to the organizational features, Column 2 indicates some impact from city staff retention (0.13 pp., s.e.=0.09), with smaller impacts from the other city features. Focusing on the features of the experimental design, Column 3 indicates a modestly higher impact of simplification compared to personal motivation and social cues (both of which are compared to other mechanisms). Most importantly, Column 3 shows a very large and statistically significant impact (t=4) of

nudge treatment arm is compared to a control arm which also receives a (new) communication, and cases in which the nudge arm is compared to a group that receives no communication. As Figure A.3a, the adoption rate is very low in both groups. We thus do not distinguish further between these two cases.

the pre-existing of communication, 0.53 pp. (s.e.=0.13). One indication of the predictive power of the pre-existing factor is the rise in R-squared to 0.34 in Column 3, from 0.01 considering just the effect sizes and statistical significance in Column 1 or 0.04 including only city-based factors in Column 2.

In Column 4 we consider all the factors together. Interestingly, the standard errors for the various point estimates do not generally increase and in fact decrease in some cases (e.g., on the evidence-based factors). The key determinant remains the pre-existence of communication, which is nearly unaltered at 0.53 pp. (s.e.=0.13). None of the other determinants is statistically significant in the regression.

In the next Column 5, we add fixed effects for each city in the sample, further controlling for any city-level features and identifying adoptions only comparing within city across different trials.[3] This extra set of controls does not meaningfully alter the results, and leaves the coefficient on the pre-existence of communication at 0.59 (s.e.=0.14).

Finally, in Column 6 we estimate a specification with the most comprehensive set of controls, including fixed effects for the different policy areas, an indicator for online (as opposed to in-print) communication, the level of take-up in the control group of the targeted policy outcome, which could be a proxy for how malleable the outcome is (e.g., a control-group take-up of 1% indicates a rare behavior that may be hard to affect), and the number of years since the trial was conducted, to control for any differences in earlier versus later trials (e.g., from institutional learning in BIT) or the decay of adoption over time. These additional controls are in particular motivated by a comparison in Table 1 between the trials with new communication versus pre-existing communication, which shows that the two groups of trials differ to some extent in certain policy areas (such as revenue collection), as well as in the incidence of simplification as a mechanism. Adding all these controls raises the R-squared up to 0.76 while scarcely affecting most of the coefficients, leaving the impact of pre-existing communication at 0.57 (s.e.=0.15). The additional controls do shift somewhat the impact of the treatment effect size, which now is statistically significant (0.24, s.e.=0.11).

Robustness. In Table A.3 we estimate the same specifications using a logit model, leading to parallel results, with the magnitudes expressed in log points. In the speci-

[3]In the sample, 11 cities have only one trial each. The remaining 19 cities in the sample each have at least two trials, which provides the within-city variation. The coefficient on pre-existing communication is identified by 10 cities that each have at least one trial with pre-existing communication and one without, covering 36 trials altogether.

fication with all determinants (Column 3) the impact of pre-existing communication is estimated to have an impact on adoption of 294 log points (s.e.=70), that is an increase of over 1,000 percent over the baseline.

We also consider the impact of alternative definitions of adoption in Table A.4. In Column 2 we drop four observations in which the evidence, while suggesting adoption, is not as straightforward as in the other cases; this has no impact on the results. In Column 3 we adopt a strict definition of adoption and consider only cases in which we were able to obtain documents on the actual wording of the communication, and do not rely on cases in which the city stated their adoption (and confirmed it with follow up questions). In this specification, as well as in Column 4 which adopts both restrictions, the mean level of adoption is sizably lower, but the impact of pre-existing communication is still a strong determinant.

4 Interpretation and Implications

4.1 Interpretations

As we documented, the most important determinant of adoption of the nudge innovations is whether the communication is pre-existing. All other determinants play more limited roles for the adoption of the nudge treatments, though it could be argued that two other determinants may have suggestive impacts depending on the specification: whether the initial staff involved in the experiment is still involved with the city department; and the strength of the evidence.

Taken together, these findings suggest a natural interpretation for the results: *organizational inertia*. In cases with pre-existing communication, there is presumably a process to send out the communication each year, and altering the wording to the most effective one is relatively straightforward, thus leading to high adoption. In the cases in which instead the communication was set up specifically for the experiment, there is no automatic pathway to do so again in the following years, leading to low adoption. This would explain explain why there is little weight placed on the RCT findings, given the inertial decision-making, and is consistent with some impact of other organizational inertia factors, such as the staff retention.

Interestingly, the forecasters in their open-ended reasons for adoption do stress the

importance of inertia (Figure 3): a third of the forecasters mention factors related to inertia or status quo. At the same time, even these forecasters do not appear to anticipate the channel through which inertia operates: the forecasters who mention intertia on average anticipate the same impact of pre-existing experimentation as those who do not mention inertia. In addition, only 12% of all forecasters predict pre-existence as the determinant with the highest impact on adoption. Most forecasters seem to propose inertia as a force dampening the adoption of innovations generally, rather than manifesting through a sharp distinction between pre-existing and new communications.

In addition to organizational inertia, though, other interpretation of the results are possible. One natural possibility is *cost allocation*. While for preexisting communication there is a pre-existing budget line to cover the cost of the communication, for new communications set up with BIT there may not be the funding for the following years to continue the communication. To address this, in Figure 8a we consider the impact of pre-existing communication separately for online communications, which have near zero marginal cost, and for paper communications, which require financing the mailer. We find a nearly identical effect size in the two categories. This suggests that the cost of the communication is not the primary reason for the key findings in the paper.

Another interpretation is that cities with pre-existing communications may have better *state capacity*, which is why they were already sending the pre-existing communications. This same state capacity enables them to implement more of the nudge innovations. Of course, we do have two proxies of state capacity, the population size as well as certification by a third-party (What Works Cities), but these variables may only be rough proxies for the decision-making ability of a city. To further control for this, the specification with city fixed-effects in Column 5 of Table 1 controls for all city-level variation in state capacity (or other factors). Yet, as we discussed above, there is no impact of adding such controls. This argues against the state capacity interpretation, at least assuming that state capacity operates at the city level.

Returning to the *organizational inertia* interpretation, we note an important distinction regarding the low adoption rate for the nudges in the *new-communication* trials. This low adoption could be due to the fact that simply no communication is sent out in the later years, or the fact that there is follow on communication, but it follows the wording and format of the control group, or a different wording altogether. If the lack of adoption is due to organizational inertia, we would expect the former case to be true, not

the latter. Figure 8b compares the benchmark measure of adoption (first two bars) with a measure of whether any communication is sent in the next years last two bars). The figure shows that strikingly for the RCTs with new communication, the two measures are the same, since there is no case in which a communication is sent out with a control wording. This lends further support to the *organizational inertia* hypothesis.

Finally, if the trials with pre-existing communication are ones in which the cities are better able to make the non-inertial decisions, we would expect not just that the level of adoption would be higher, but also that adoption would be more sensitive to the strength of the evidence for the nudge treatments. In Figures 9a-b we consider the trails with new communication versus those with pre-existing communication and within each of these two groups we consider the adoption as a function of whether the findings were statistically significant or not (Figure 9a) and by whether the effect size with higher or lower than median (Figure 9b). The first split by statistical significance provides evidence supportive of this hypothesis: for new communication there is no response to the statistical significance, or even in a perverse direction, while for preexisting communications the adoption rises from 45% for non-statistically significant results to 90% for statistically significant results. At the same time, in Figure 9b the evidence is much more muted when we consider the split by effect size along the median.[4] In this regard, we conclude that the evidence is not conclusive.

4.2 Implications and Counterfactuals

We can build on the results above to compute simple counterfactuals for the impact of adoption on the effectiveness of policy-making in the years following the RCT. That is, how much did the evidence collected from the RCT improve the targeted policy outcome, given the evidence on adoption? And how much could it have improved it under other counterfactuals?

Specifically, we assume that the treatment effects of the RCTs would replicate in subsequent years if the same treatments were used and adopted, and when no nudge treatment is adopted, we assume an improvement of 0 pp. That is, for each trial i, we take the highest effect size $\hat{\beta}_i$ across treatment arms. The average actual "improvement" is calculated as $\frac{1}{73}\sum_{i=1}^{73}\hat{\beta}_i\mathbf{1}\{i \text{ is adopted}\}$, where $\mathbf{1}\{i \text{ is adopted}\}$ is an indicator

[4]Instead of halves, Figure A.3b partitions trials by effect size into thirds, but the findings are similar.

for whether trial i has been adopted. The answer is shown in the first bar of Figure 10: across all 73 trials, the evidence from the RCTs is predicted to have improved policy outcomes by 0.89 pp. based on actual adoptions, a statistically significant improvement.

In contrast, the second bar presents a counterfactual of how much the RCTs would have improved outcomes, had all the treatments with positive effect size been adopted: the improvement would have been 2.70 pp. This comparison highlights the importance of bottlenecks to policy adoption: the achieved gains from the RCTs of 0.89 pp. are only one third of the achievable gains of 2.70 pp.

We can also compare these two metrics to that implied by the forecasts based on the predicted adoption probability as a function of effect size. As described, the forecasters expect adoption to be fairly elastic to effect size. For trials with effect sizes in the lowest third, they predict the average adoption rate to be 13%. Hence in our calculation, we weight those trials by 0.13. On the other end, the prediction for the highest third is 48%, and similarly, we weight trials falling in that bin by 0.48. Taking the weighted average, we calculate an implied predicted improvement of 1.22 pp. Thus, the forecasters are slightly optimistic about the impact of RCTs on policy outcomes.

To highlight the role of organizational inertia, we compute counterfactuals separately for trials with new communication, versus with pre-existing communication. For the 52 trials with new communication, in comparison to the 2.48 pp. achievable with optimal adoption, the actual adoption creates an improvement of only 0.32 pp., about one tenth of the possible surplus. Conversely, for the 21 trials with pre-existing communication, the estimated policy improvements from adoption is 2.31 pp., quite close to the optimal counterfactual of 3.24 pp. Thus, for the cases in which organizational inertia is more conducive to adoption, the evidence collected in the RCTs largely translated into actual significant policy improvements.

5 Discussion and Conclusion

Organizations from the World Bank to US federal agencies run experiments to gather evidence on how to best achieve outcomes of public policy interest. In our context, US cities who already supported data-driven policy-making experimented on how to achieve policy goals such as the timely payment of municipal taxes or the recruitment of a diverse police force, by varying the communications on these goals to citizens. The interventions

they chose were inexpensive to implement and they received technical assistance in the design and interpretation of the results. But does the gathering of evidence guarantee the improvement of the outcomes, or are there bottlenecks to the adoption of evidence, even under such favorable conditions?

At least in our context, there are indeed substantial bottlenecks: the innovations from the RCTs yield only about one third of their potential benefits. This is because the rate of adoption is fairly low, 27%, and is only modestly sensitive to the effectiveness of the intervention. As a consequence, several high-return nudge innovations are not adopted into policy in years subsequent to the experiment. Thus, even organizations that value and produce rigorous evidence are not immune to challenges in evidence adoption.

To an extent this is bad news for evidence-based policy-making. But there is good news too: the barriers to adoption, in our context, do not appear to be due to intractable problems such as political divisions or funding challenges for the roll-out, but more simply to organizational inertia. When the RCTs take place in the context of ongoing communication to citizens—such as altering the content of a yearly mailer about registering business taxes—the adoption rate is high at 67% and, to an extent, more sensitive to evidence. For such ongoing communications there is a routine process, and organizations incorporate the successful changes. It is instead for the one-time communications which were not pre-existing that the adoption rate is very low, at 12%. Following the experiment, inertia tilts communication back to the previous status quo.

A first implication of these findings is that designing interventions with an eye to such bottlenecks should achieve a higher conversion rate. In our context, government agencies could decide to focus their experimentation on cases with pre-existing communication, given the lower inertial barriers, rather than the interventions where they expect the most precise or largest effect sizes. They could also set in place, and discuss with the city partner, routines and processes to guarantee adoption of any successful intervention. Nudge units already frame experimentation as an opportunity to test "what works" for the purposes of scaling. Given that adoption does not seem to arise naturally, heavier investments could be made to support the process of adoption.

A second implication is that we should collect more systematic evidence on such bottlenecks. The evidence on adoption at scale of the results of RCTs is typically limited to success cases, with few systematic records of adoption (e.g., Kremer et al., 2019). A natural consequence of not having such knowledge is that experts and practitioners alike

understand that barriers exist but are less able to predict what the specific barriers are. Figure A.4a plots the average expert predictions along each of the 7 dimensions that they forecast, against the actual result in adoption along that dimension. The predictors are directionally correct in many dimensions, but they are unable to discern the most important predictor, to the point that the predictions are negatively correlated to the actual determinants. Interestingly, this pattern is near identical for both researchers and practitioners, unlike in DellaVigna and Linos (2022), where practitioners did significantly better at predicting the average nudge effect size in the nudge units.

An important caveat is that the findings are, to an extent, specific to our context. To have at least some sense on perceived bottlenecks in other contexts, we asked respondents of the forecasting survey to rank the likelihood of adoption, as well as the responsiveness of adoption to evidence, compared to firms doing A/B experiments, and to development RCTs in low-income countries. The respondents thought on average that evidence-based adoption would be higher in firms, but that our context and the development RCTs would be similar in terms of adoption (Figure A.4b).

Regarding the A-B experimentation in firms, we know of no comprehensive data set on adoption like ours, but certainly there are known instances of non-adoption of successful innovations (Cho and Rust, 2010). In general, profit motives for firms make it less likely that researchers will be able to access comprehensive records of adoption for a whole set of experiments within a firm, compared to the transparency with which BIT-NA shared the record of all their experiments. Lacking such evidence, we conjecture that bottlenecks are likely to be an issue even in firms that have online platforms for experimentation, given that the adoption post A-B testing takes an active decision. Only platforms that automatically adopt the most successful experimentation arm, used in some companies, remove the inertial barrier to adoption.

Finally, we recognize that in other settings, the political barriers to adoption may be higher, or the costs of rolling out an innovation at scale often will be larger than the cost of sending an envelope or an email. In general, we would expect that those issues would tend to make adoption of innovations at scale even trickier. While those sources of bottlenecks may be harder to address, our findings suggest that at least one should aim to put in place systems to get around, as much as possible, the organizational inertia. Good architecture design should apply to experimentation as well.

References

Allcott, Hunt. 2015. "Site Selection Bias in Program Evaluation." *The Quarterly Journal of Economics* 130 (3): 1117-1165.

Andrews, Isaiah and Maximilian Kasy. 2019. "Identification of and Correction for Publication Bias." *American Economic Review* 109 (8): 2766-94.

Athey, Susan and Michael Luca. 2019. "Economists (and Economics) in Tech Companies." *Journal of Economic Perspectives* 33 (1): 209-230.

Banerjee, Abhijit V. and Esther Duflo. 2009. "The Experimental Approach to Development Economics." *Annual Review of Economics* 1: 151-178.

Baron, J. 2018. "A Brief History of Evidence-based Policy." *The Annals of the American Academy of Political and Social Science*, 678 (1): 40-50.

Benartzi, Shlomo, John Beshears, Katherine L. Milkman, Cass R. Sunstein, Richard H. Thaler, Maya Shankar, Will Tucker-Ray, William J. Congdon, and Steven Galing. 2017. "Should Governments Invest More in Nudging?" *Psychological Science* 28 (8): 1041-1055.

Besley, Tim and Torsten Persson. 2009. "The Origins of State Capacity: Property Rights, Taxation, and Politics." *American Economic Review* 99 (4): 1218-1244.

Brodeur, Abel, Mathias Lé, Marc Sangnier, and Yanos Zylberberg. 2016. "Star Wars: The Empirics Strike Back" *AEJ: Applied Economics* 8 (1): 1-32.

Camerer, Colin F., et al. 2016. "Evaluating Replicability of Laboratory Experiments in Economics." *Science* 351 (6280): 1433-1436.

Cho, Sungjin and John Rust. 2010. "The Flat Rental Puzzle." *The Review of Economic Studies*, 77(2), 560–594.

Christensen, Garrett and Edward Miguel. 2018. "Transparency, Reproducibility, and the Credibility of Economics Research." *Journal of Economic Literature* 56 (3): 920-980.

DellaVigna, Stefano and Elizabeth Linos. "RCTs to scale: Comprehensive evidence from two nudge units" *Econometrica* 90, 81-116.

DellaVigna, Stefano, Devin Pope, and Eva Vivalt. 2019. "Predict science to improve science." *Science* 366 (6464): 428-429.

de Vries, Hanna, Victor Bekkers, and Lars Tummers. 2015. "Innovation in the Public Sector: A Systematic Review and Future Research Agenda." *Public Administration.*

Development Impact Evaluation (DIME). 2019. "Science for Impact: Better Evidence for Better Decisions." *World Bank Group.* https://documents1.worldbank.org/curated/en/942491550779087507/pdf/134802-AR-PUBLIC-DIME-AnnRpt19-WEB.pdf

Fernandez, Sergio and Lois Wise. 2010. "An Exploration of Why Public Organizations 'Ingest' Innovations." *Public Administration*, 88 (4): 979-998.

Foundations for Evidence-Based Policymaking Act, H.R. 4174, 115th Cong. 2018. https://www.congress.gov/bill/115th-congress/house-bill/4174

Halpern D. 2015. *Inside the Nudge Unit: How Small Changes Can Make a Big Difference.* London, UK: WH Allen.

Hjort, Jonas, D Moreira, Gautam Rao, and JF Santini "How research affects policy: Experimental evidence from 2,150 brazilian municipalities" *American Economic Review* 111 (5), 1442-80.

Michael Kremer, Sasha Gallant, Olga Rostapshova, and Milan Thomas. 2019. "Is Development Innovation a Good Investment? Which Innovations Scale? Evidence on social investing from USAID's Development Innovation Ventures", Working paper.

List, John. 2022. *The Voltage Effect: How to Make Good Ideas Great and Great Ideas Scale.* New York, NY: Random House.

Meager, Rachael. 2019. "Understanding the Average Impact of Microcredit Expansions: A Bayesian Hierarchical Analysis of Seven Randomized Experiments." *American Economic Journal: Applied Economics* 11 (1): 57-91.

Mehmood, Sultan, Shaheen Naseer, and Daniel Chen. 2021. "Training Policymakers in Econometrics." Working paper.

Milkman, Katherine L., Dena Gromet, Hung Ho, et al. (2021). "Megastudies Improve the Impact of Applied Behavioural Science." *Nature,* 600, 478-483.

Muralidharan, Karthik and Paul Niehaus. 2017. "Experimentation at Scale." *Journal of Economic Perspectives* 31 (4): 103-24.

Nakajima, Nozomi. 2021. "Evidence-Based Decisions and Education Policymakers." Working paper.

Naranjo-Gil, D. 2009. "The Influence of Environmental and Organizational Factors on Innovation Adoptions: Consequences for Performance in Public Sector Organizations." *Technovation*, 29 (12): 810-818.

Simonsohn, Uri, Leif D. Nelson, and Joseph P. Simmons. 2014. "P-curve: A key to the file-drawer." *Journal of Experimental Psychology: General* 143 (2), 534–547.

Thaler, Richard and Cass Sunstein. 2008. *Nudge*. New Haven, CT: Yale University Press.

Toma, Mattie and Elizabeth Bell. 2021. "Understanding and Improving Policymakers' Sensitivity to Program Impact." Working paper.

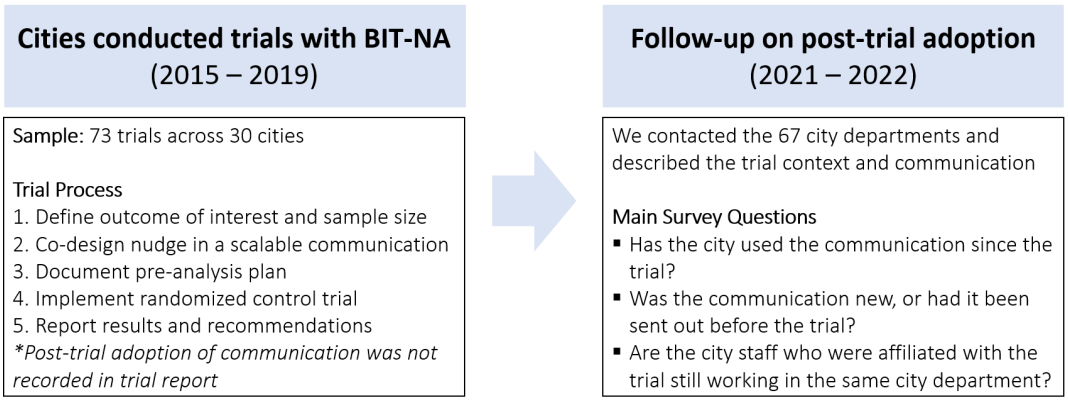
Vivalt, Eva. 2020. "How Much Can We Generalize from Impact Evaluations?" *Journal of the European Economic Association* 18 (6), 3045–3089.

Vivalt, Eva and Aidan Coville. 2022. "How Do Policymakers Update Their Beliefs?" Working paper.

Wang, Shaoda and David Yang. 2021. "Policy Experimentation in China: The Political Economy of Policy Learning." *NBER Working Paper No. 29402.*

Figure 1: Study design and sample restrictions and study design

(a) Study design



(b) Sample restrictions

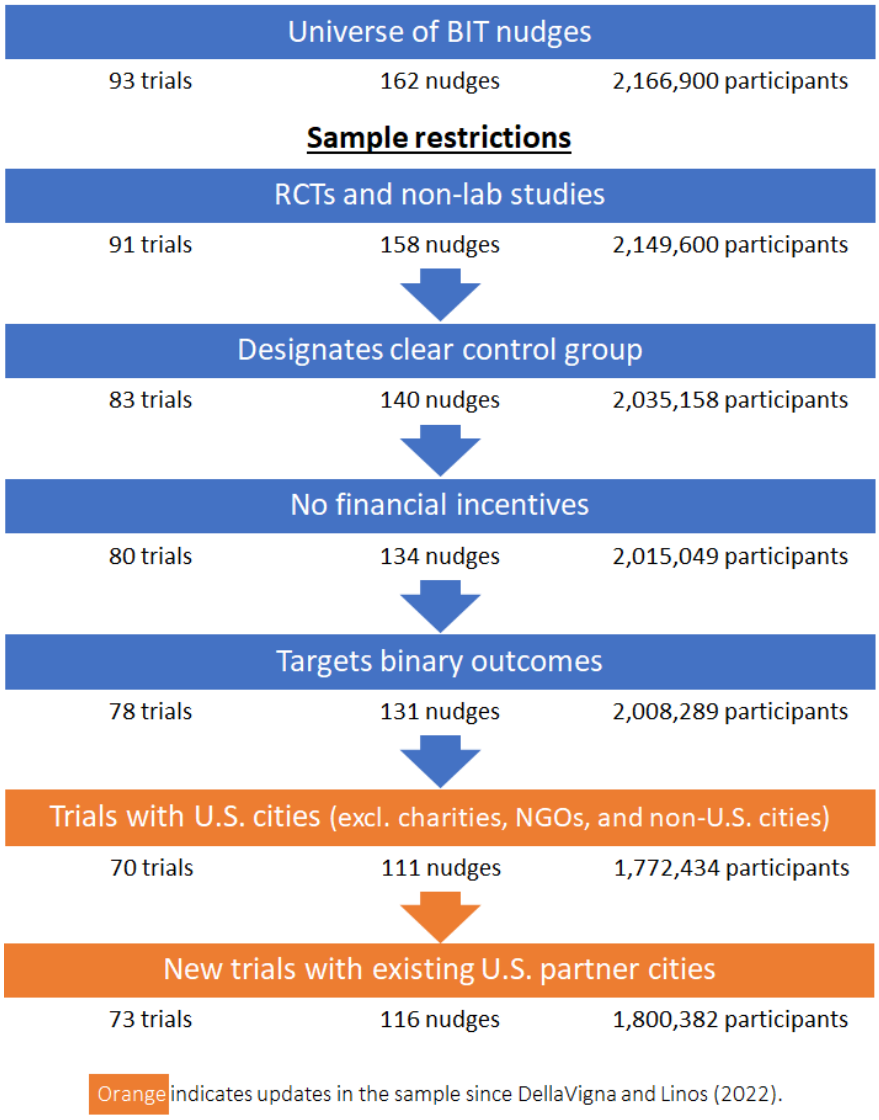
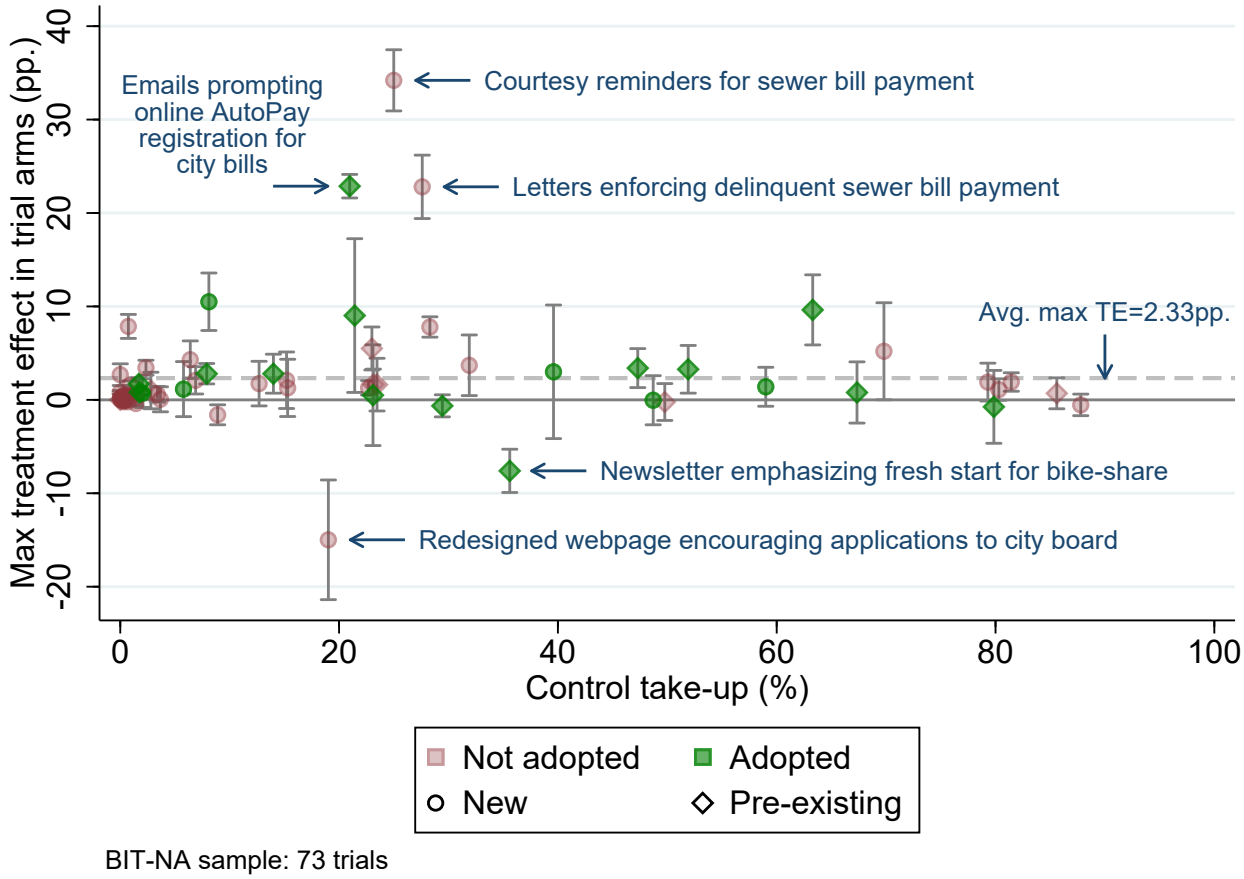


Figure 2: Trial-by-trial adoption and effect sizes



BIT-NA sample: 73 trials

Figure 3: Adoption of nudges: Observed compared to benchmarks

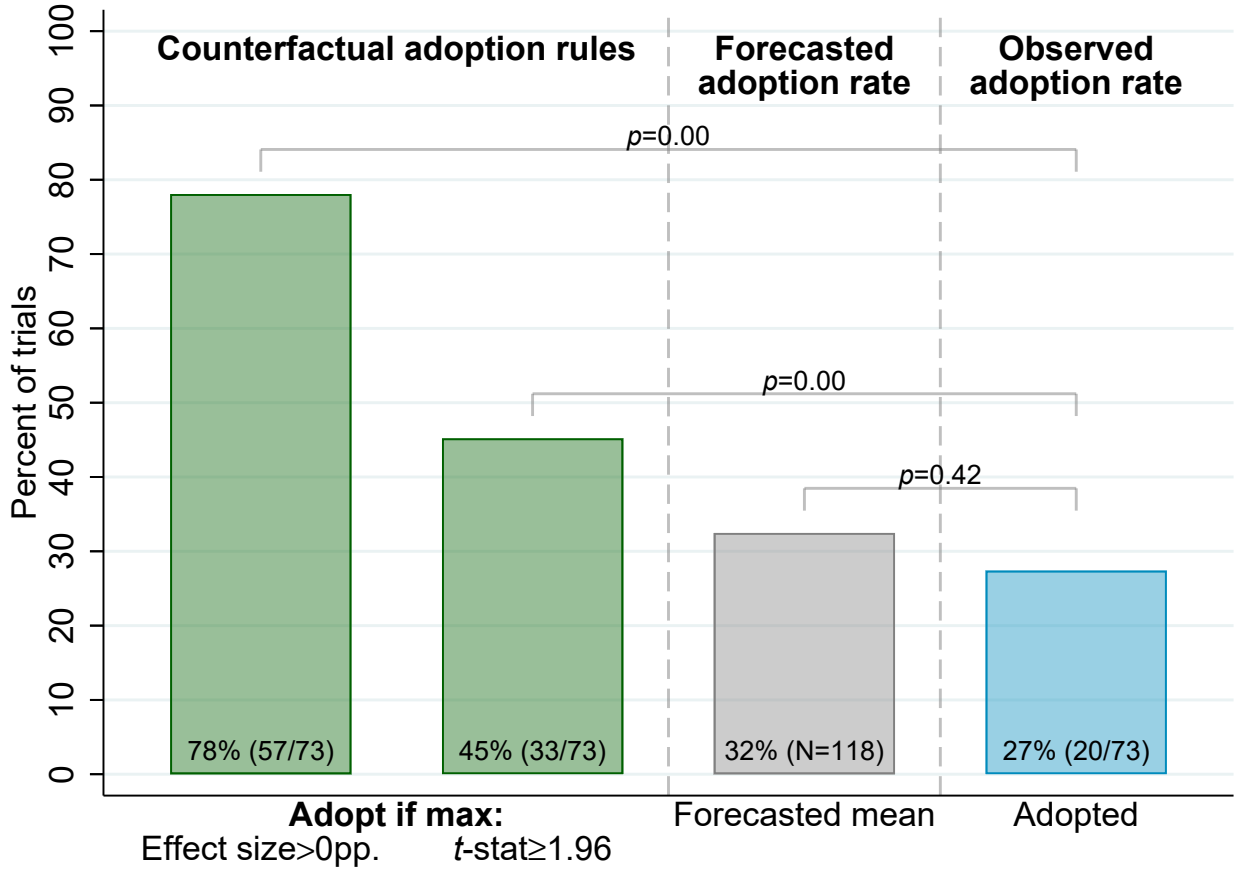
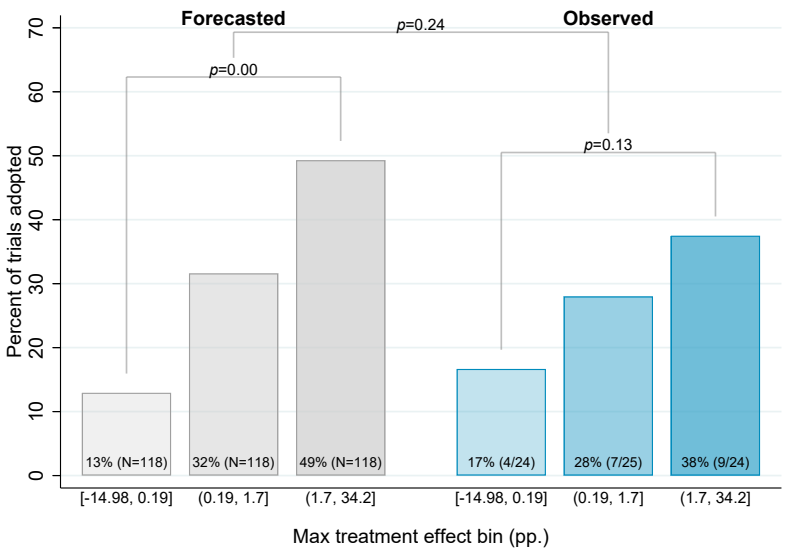


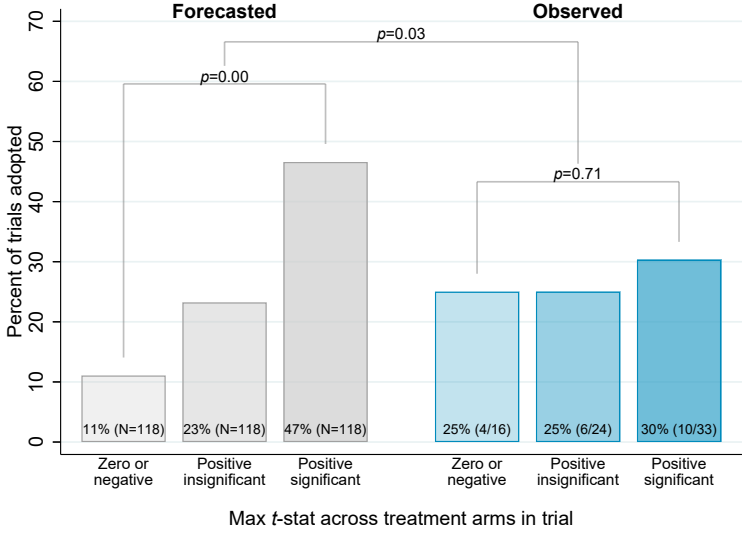
Figure 4: Word cloud from open-ended forecasts of adoption determinants

Figure 5: Adoption of nudges by effectiveness

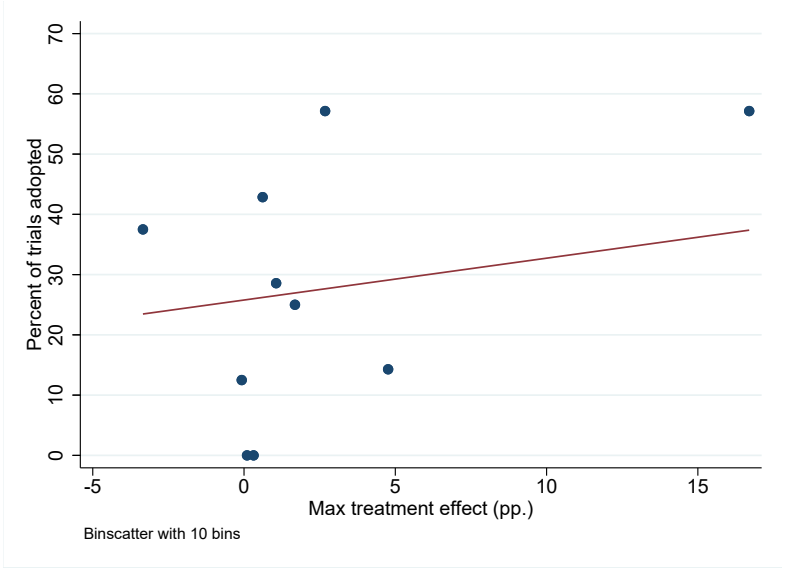
(a) By effect size



(b) By statistical significance



(c) By effect size (bin scatter)



(d) By treatment arm among adoptions

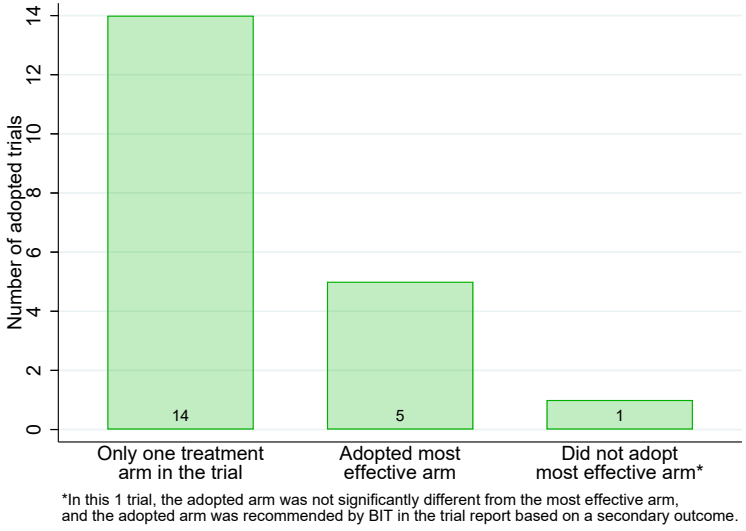
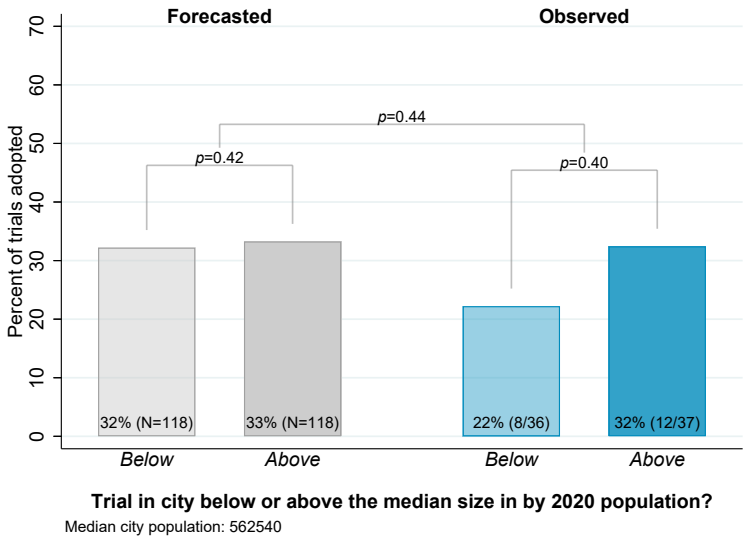
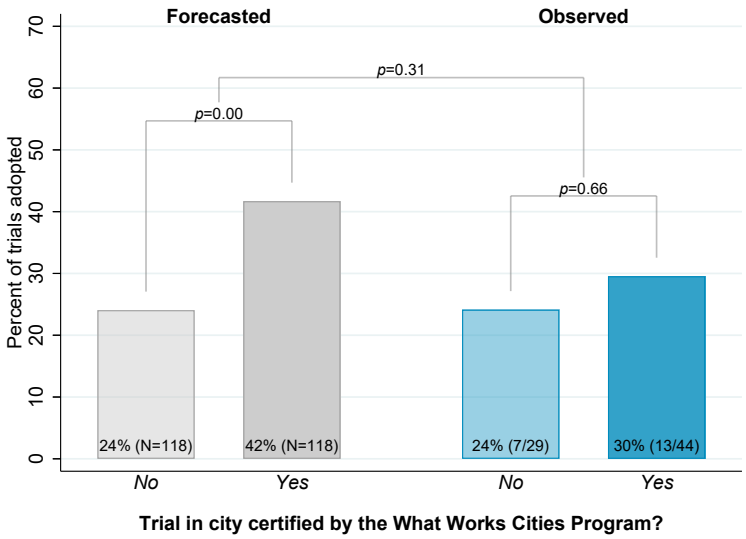


Figure 6: Adoption based on city context

(a) By "state capacity" (proxied by city size)



(b) By certification of evidence-based cities



(c) By staff retention

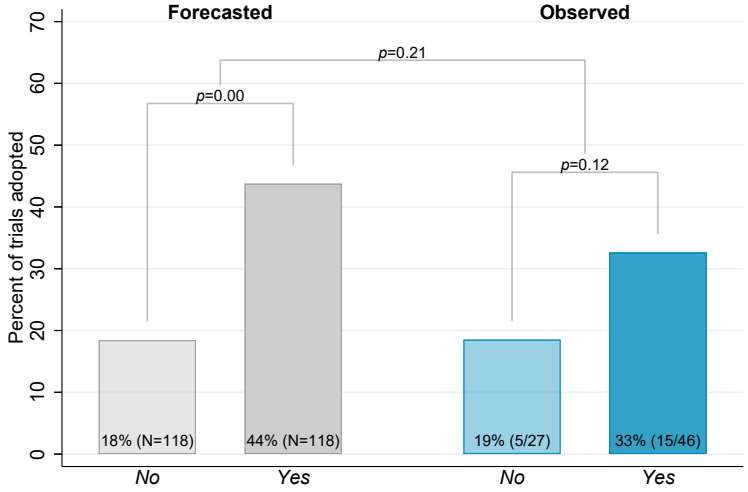
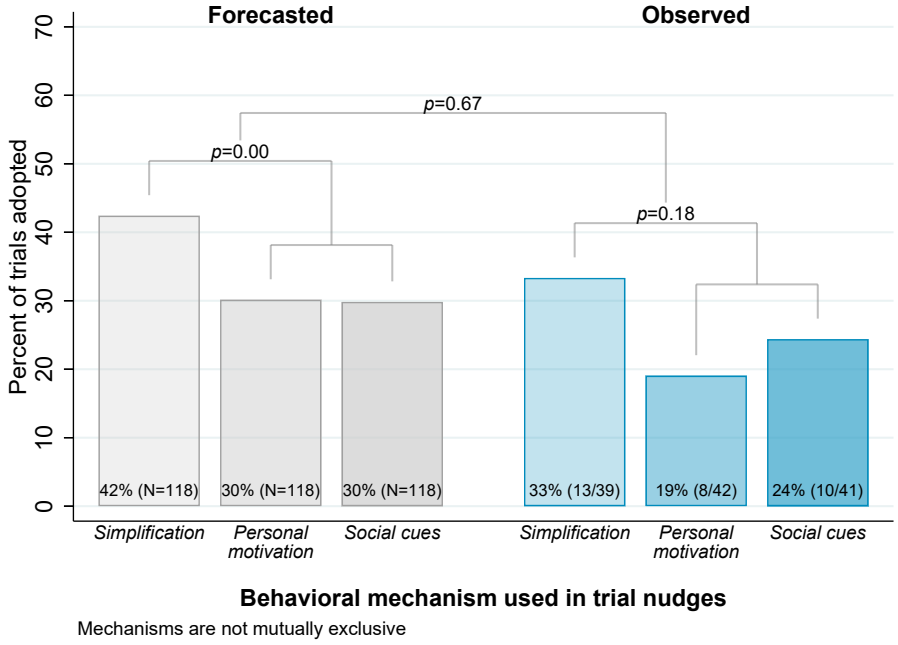


Figure 7: Adoption based on trial context

(a) By behavioral mechanism



(b) By pre-existence

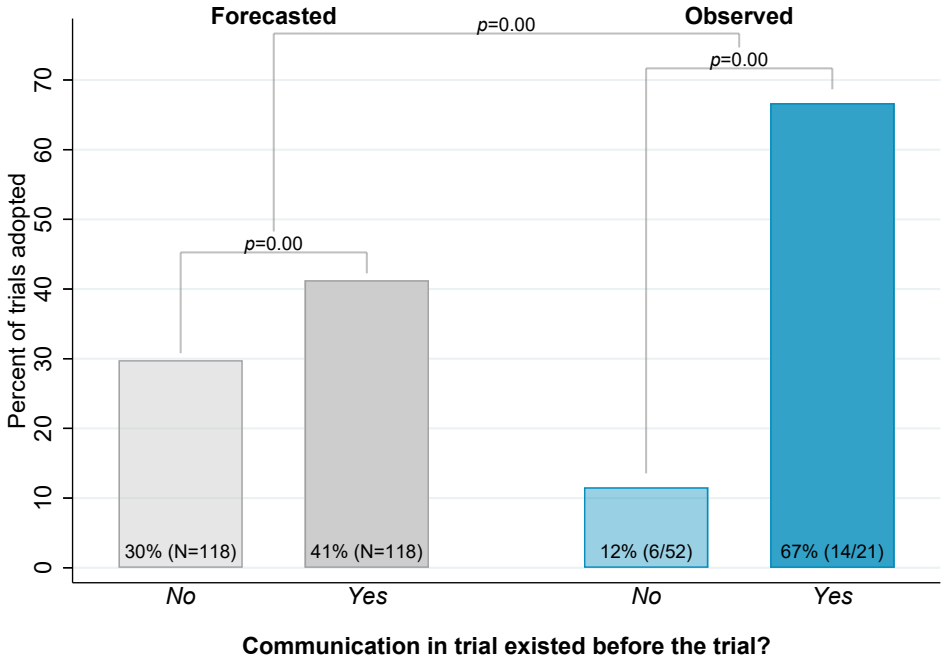
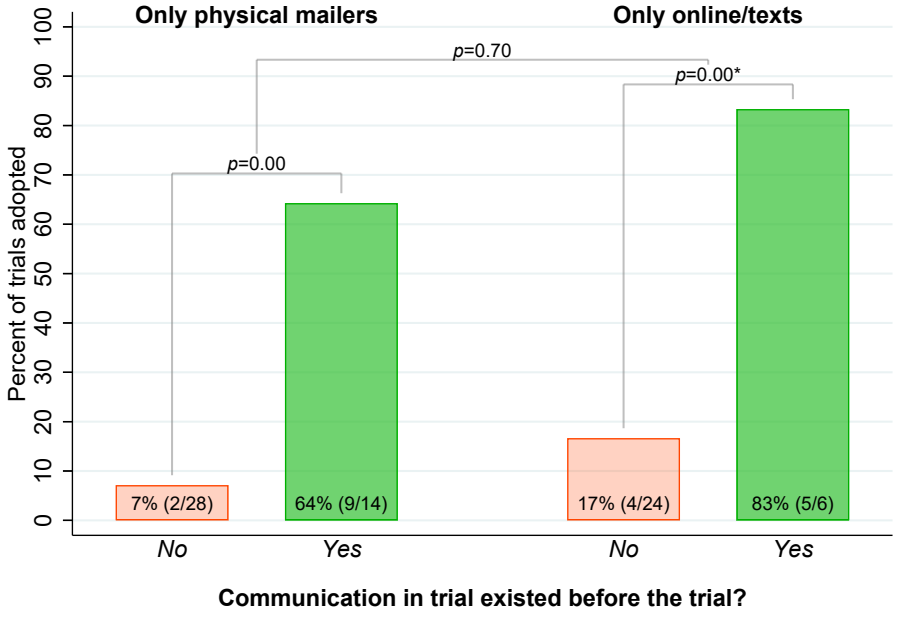


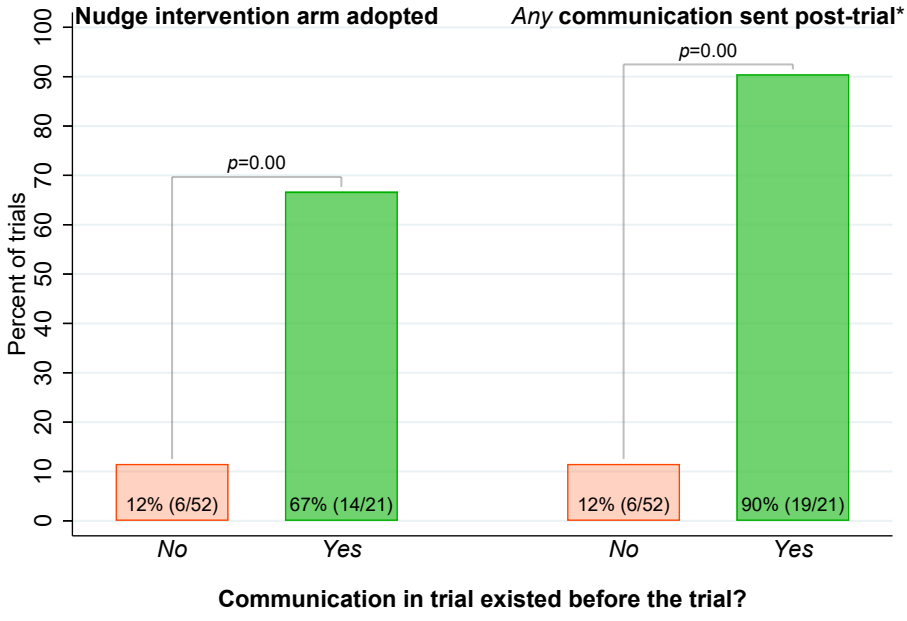
Figure 8: Mechanisms behind the effect of pre-existence

(a) Marginal cost of communication



*Calculated using Fisher's exact test

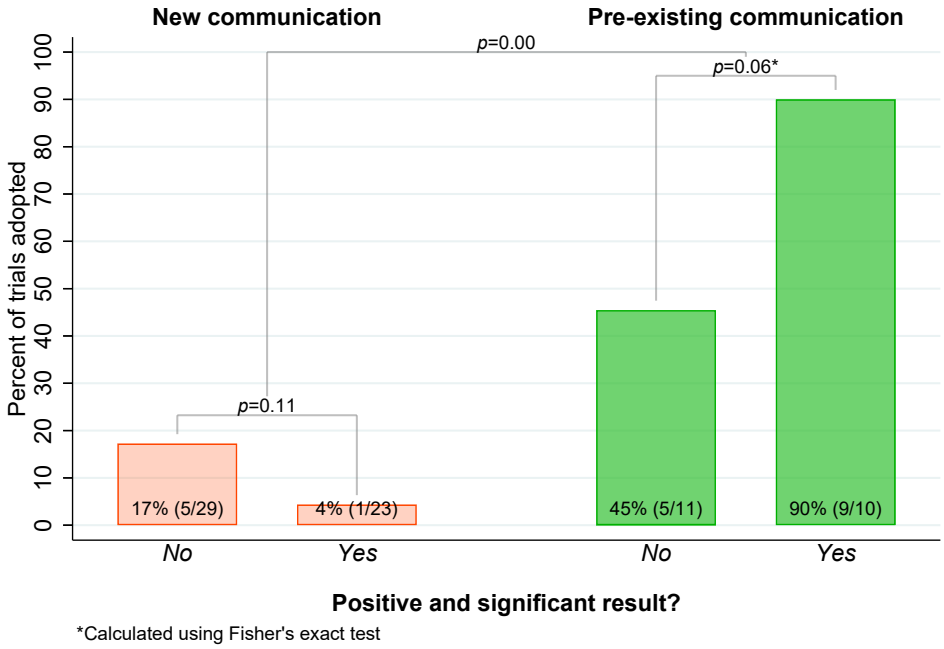
(b) Any communication sent post-trial



*This comprises adoption of the communication in either the nudge arm or the control arm.

Figure 9: Pre-existence and evidence based adoption

(a) Pre-existence and statistical significance



(b) Pre-existence and effect size (median bins)

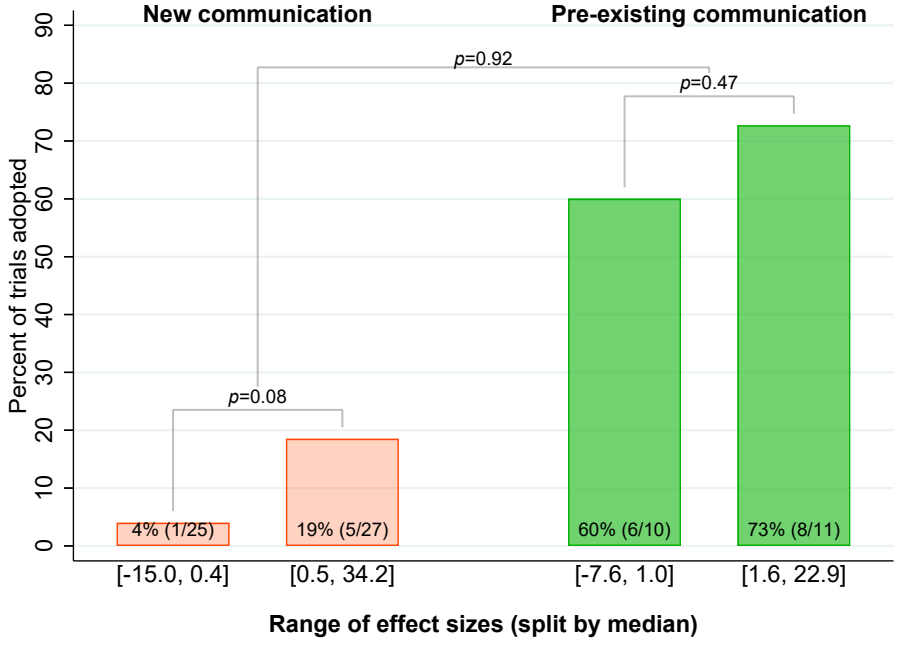


Figure 10: Counterfactual adoption rules

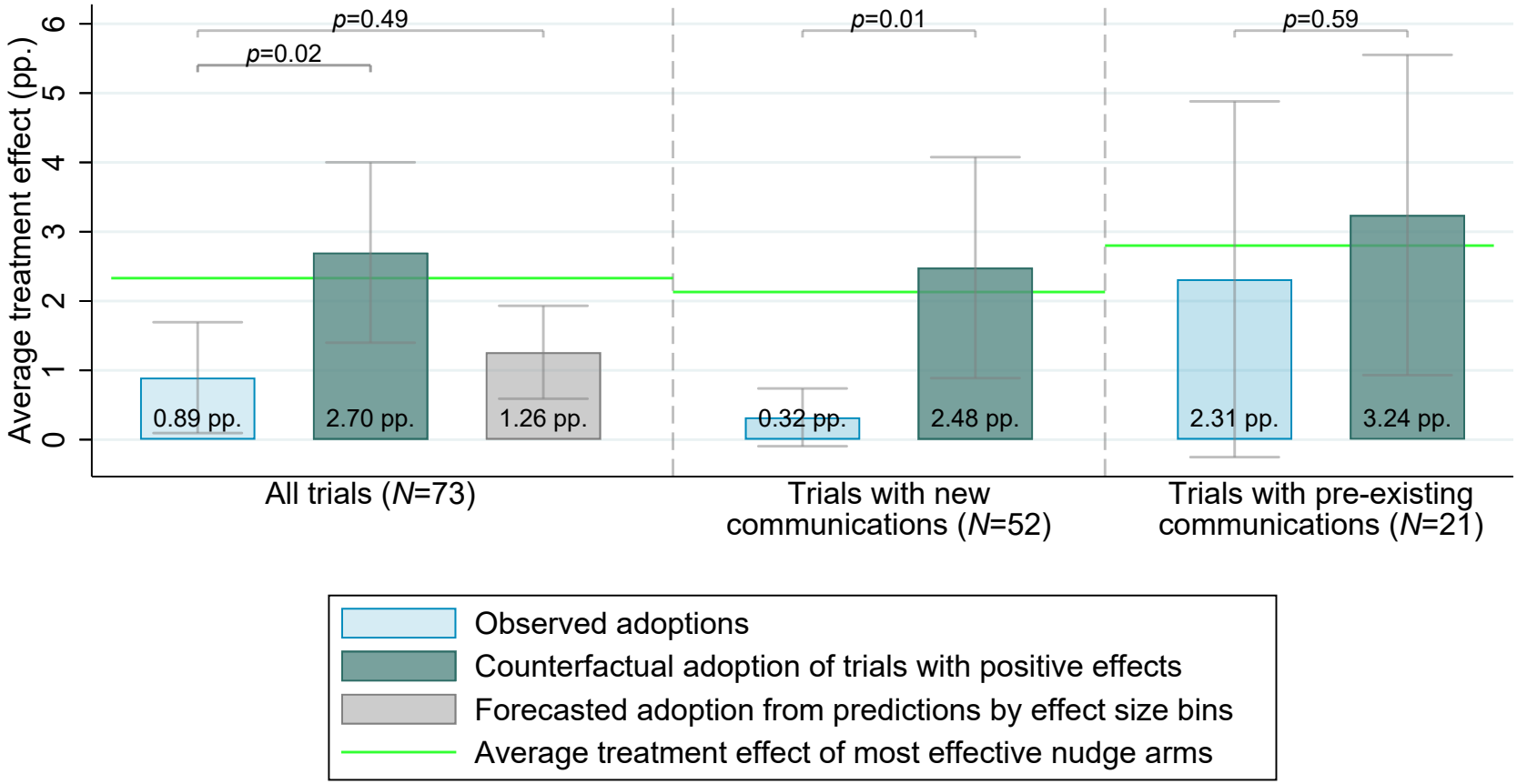


Table 1: Sample characteristics

	Overall	Effect size≥median		City staff retained		Comm. pre-existed	
Frequency in category (%)	(1)	(2) No	(3) Yes	(4) No	(5) Yes	(6) No	(7) Yes
Nudge effectiveness							
Max $t \geq 1.96$	45.21	21.62	69.44*	44.44	45.65	44.23	47.62
Max treatment effect ≥ 1 pp.	46.58	0.00	94.44*	40.74	50.00	42.31	57.14
Organizational features							
City certified by What Works Cities	60.27	64.86	55.56	62.96	58.70	63.46	52.38
City staff member from trial retained	63.01	59.46	66.67	0.00	100.00*	59.62	71.43
Experimental design							
Communication pre-existed before trial	28.77	21.62	36.11	22.22	32.61	0.00	100.00*
Nudge communication uses Simplification	53.42	48.65	58.33	59.26	50.00	44.23	76.19*
Nudge communication uses Personal Motivation	57.53	56.76	58.33	70.37	50.00	61.54	47.62
Nudge communication uses Social Cues	56.16	59.46	52.78	51.85	58.70	55.77	57.14
Policy area							
Revenue collection & debt repayment	24.66	16.22	33.33	29.63	21.74	17.31	42.86
Registration & regulation compliance	20.55	13.51	27.78	14.81	23.91	19.23	23.81
Workforce & education	20.55	29.73	11.11	25.93	17.39	23.08	14.29
Take-up of benefits and programs	13.70	16.22	11.11	11.11	15.22	15.38	9.52
Community engagement	13.70	18.92	8.33	11.11	15.22	17.31	4.76
Health	5.48	5.41	5.56	7.41	4.35	5.77	4.76
Environment	1.37	0.00	2.78	0.00	2.17	1.92	0.00
Medium							
Physical letter	38.36	29.73	47.22	51.85	30.43	25.00	71.43*
Email	30.14	27.03	33.33	22.22	34.78	32.69	23.81
Postcard	21.92	27.03	16.67	22.22	21.74	30.77	0.00*
Text message	10.96	10.81	11.11	3.70	15.22	11.54	9.52
Website	4.11	5.41	2.78	0.00	6.52	3.85	4.76
Number of trials	73	37	36	27	46	52	21

This table shows the frequencies of trials for each category listed in the leftmost column. Column 1 shows the frequences for all trials. Columns 2 and 3 partition the sample along the median of the maximum effect size in each trial. Columns 4 and 5 consider separately trials for which all the city collaborators from the trial have departed versus trial that have at least one original staff member still working in the same city department. Columns 6 and 7 distinguish between trials that tested nudges in a new communication and those that added nudges to a pre-existing communication that the city had been sending before the trial.

*Asterisk indicates that the p-value of the difference<0.05. When there are fewer than 5 trials in one of the 2×2 cells, p-values are calculated using the two-sided Fisher's exact test instead.

Table 2: Determinants of nudge adoption

Dep. Var.: Nudge adopted (0/1, OLS)	(1)	(2)	(3)	(4)	(5)	(6)
Max $t \geq 1.96$	0.02			-0.02	-0.16	-0.23
	(0.13)			(0.08)	(0.10)	(0.10)
Max treatment effect (10pp.)	0.06			0.10	0.14	0.24
	(0.12)			(0.08)	(0.09)	(0.11)
City staff retained		0.13		0.07	0.00	0.00
		(0.09)		(0.08)	(0.11)	(0.12)
Above-median city population		0.04		0.07		
		(0.13)		(0.10)		
What Works Cities certified		0.05		0.13		
		(0.12)		(0.11)		
Communication pre-existed			0.53	0.53	0.59	0.57
			(0.13)	(0.13)	(0.14)	(0.15)
Mechanism						
Simplification & information			0.01	0.04	0.06	0.11
			(0.10)	(0.10)	(0.13)	(0.10)
Personal motivation			-0.13	-0.12	-0.00	0.03
			(0.11)	(0.12)	(0.14)	(0.12)
Social cues			-0.06	-0.07	0.06	0.07
			(0.08)	(0.08)	(0.06)	(0.08)
Control take-up (10%)						0.02
						(0.03)
Uses online mediums						0.22
						(0.11)
Years since trial						-0.06
						(0.07)
Constant	0.25	0.13	0.22	0.03	0.07	0.22
	(0.07)	(0.13)	(0.10)	(0.17)	(0.11)	(0.37)
Average adoption	0.27	0.27	0.27	0.27	0.27	0.27
City fixed effects					✓	✓
Policy area fixed effects						✓
Number of trials	73	73	73	73	73	73
Number of cities	30	30	30	30	30	30
R^2	0.01	0.03	0.34	0.38	0.69	0.76

Standard errors clustered by city are shown in parentheses. "Policy area fixed effects" includes a dummy for each city and each of the policy areas (Community engagement; Environment; Health; Registration & regulation compliance; Revenue collection & debt repayment; Take-up of benefits and programs; and Workforce & education).

Online Appendix

A Marginal Cases of Non-Adoption

As mentioned in the text, we defined a city as adopting a trial if the city has used one of the communications in the nudge treatment arms again following the RCT. This includes cases when the city had used the communication after the trial but was not currently doing so, for example, because it was not an election year. When cities have made further changes to the communication since the trial, we counted adoption as incorporating at least 50% of the nudge features as pre-specified in the internal trial protocol or report.

Most cases of (non-)adoption were clear according to this rule, but there were 4 cases of non-adoption for which the post-trial communication seemed to include some nudge features, but did not meet our criteria upon close inspection. We describe each of these marginal non-adoption cases below.

Furthermore, in 11 out of the 20 cases of adoption, the city contacts verbally provided a description of the communication they had used after the trial that matched the treatment arm, but they could not send us a template of the exact communication for us to independently verify due to bureaucratic or technical issues (e.g., they were no longer using the same email system from which the newsletter had been sent before).

As a robustness check in Table A.4, we drop the marginal non-adoption and verbal-only adoption cases and replicate the main specification (Column 4) from Table 1. The key finding, namely on the importance of pre-existence, remains large and significant.

List of marginal non-adoption cases

1. This city sent new postcards encouraging local business owners to renew their license online. The control arm used a slogan on convenience, whereas the treatment arm used one with normative language. Both postcards were equally effective. The city no longer sends the postcards, but uses the same exact slogan from the control arm in letters sent to businesses about their licenses.

2. This city police department sent recruitment postcards to local neighborhoods. The version of the postcards that had a message emphasizing the benefits and salary had the strongest effect. Now on its website, the police department has adopted this message to advertise applications.

3. This city police department used online ads to recruit applicants from Historically Black Colleges and Universities (HBCUs). The control ads, based on a prior pilot, highlighted the relatable background of current police officers, and the treatment ads also offered a "personal concierge" service to guide applicants through the process. The control ads were significantly more effective. The police department still

uses online ads to target applicants from HBCUs, but does not use the treatment messaging.

4. In both the control and treatment arms of this trial, the city added a new checkbox to the water utility bill for easy enrollment into a local charity program. The utility bill in the treatment arm also included colorful ASCII art and a message requesting recipients to sign up for the program. There was not a significant difference between the control and treatment arms. The city continues to send the utility bill with only the checkbox from the control arm.

B Forecasting Survey

This section details the 10-minute forecasting administered through the Social Sciences Prediction Platform[5]. In total, 118 forecasters submitted their predictions on the platform over 25 days. The survey first summarized the setting and main result of DellaVigna and Linos (2022), and then introduced the focus for the current paper on the adoption rate of the nudge interventions *after* the RCT collaborations with the cities and on the determinants of adoption. The survey described the sample of trials and highlighted that each trial was co-designed by BIT and the partnering city and that the results were shared with the city in a report after the trial. Next, the survey showed two randomly selected examples of communications used in trials with a brief description of the policy area and targeted outcome.

The forecasters then made their first prediction on the baseline adoption rate. Specifically, we asked, "*What percent of the 73 trials do you think have been adopted by the cities?*" The forecasters provided their answer in percentages (from 0 to 100). We defined adoption as: "*We count a city as "adopting" a trial if one of the nudge treatment arms has been used in city communications after the trial with BIT.*" We gave an example of an adopted trial, showing the nudge communication used in the trial next to the comparable current communication in use by the city. For reference, we provided two statistics: 78% of the trials had at least one nudge intervention arm that led to an improvement relative to the control group, and 45% of the trials found a nudge that led to a significant improvement with $p < 0.05$. On the same page, we asked forecasters to write a short list or a couple sentences in an open-ended text box on which determinants of adoption they expect to matter most.

We then introduced the determinants of adoption that we consider: statistical significance, effect size, retention of the original city staff collaborator, state capacity (proxied by city population), What Works Cities certification, pre-existence of the communication in the trial, and behavioral mechanism used in the nudge intervention. (At this point, forecasters could not return to the previous page to change their baseline prediction nor

[5] `https://socialscienceprediction.org/`

their open-ended responses.) Next, the survey asked for the predicted adoption rate for each of these determinants separately page-by-page. The survey randomized the order of the determinants between two different orderings.

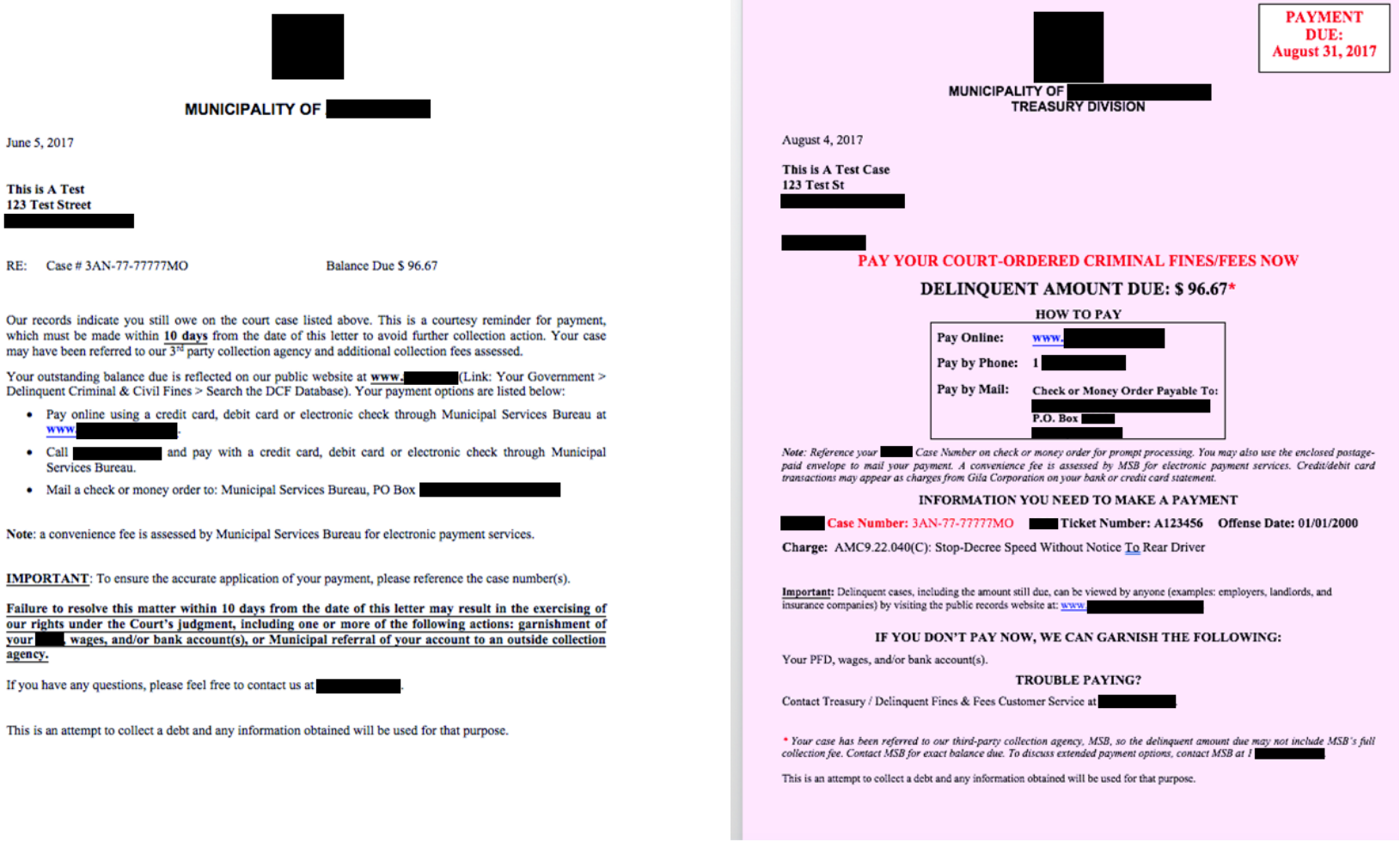
For each determinant, the sample of trials was separated into relevant bins with the number of trials in each bin shown, and forecasters predicted the adopted rate within each bin. For example, for statistical significance, we asked what percent the forecasters think have been adopted for trials that found: (i) a statistically significant improvement (i.e., $t \geq 1.96$, covers 45% of all trials, $n = 33$), (ii) a statistically insignificant improvement (i.e., $0 < t < 1.96$, covers 33% of all trials, $n = 24$), and (iii) a zero or negative effect (covers 22% of all trials, $n = 16$).

On every page, we reminded forecasters of their predicted baseline adoption rate from the very first question. For comparison, we displayed the weighted average adoption rate implied by their forecasts for the determinant on the page as a soft "nudge" to help them give forecasts that were consistent with their initial predicted baseline rate. For example, if they predicted that the adoption rates were 50% for statistically significant trials, 30% for statistically *in*significant trials, and 10% for zero or negative trials, then the weighted average we calculated for them would be $(50\% \times 0.45) + (30\% \times 0.33) + (10\% \times 0.22) = 34.6\%$. Lastly, we asked forecasters to compare our sample of RCTs in U.S. municipal cities with similar representative samples of trials conducted by large multinational firms and by governments of low-income countries. We asked forecasters to rank these three samples by the overall adoption rate and by the responsiveness to evidence in adoption.

Figure A.1: Example of adoption of BIT-NA trial

(a) Status-quo control arm communication

(b) Nudge treatment arm communication



MUNICIPALITY OF [redacted]

June 5, 2017

This is A Test
123 Test Street
[redacted]

RE: Case # 3AN-77-77777MO Balance Due $ 96.67

Our records indicate you still owe on the court case listed above. This is a courtesy reminder for payment, which must be made within **10 days** from the date of this letter to avoid further collection action. Your case may have been referred to our 3rd party collection agency and additional collection fees assessed.

Your outstanding balance due is reflected on our public website at **www.**[redacted](Link: Your Government > Delinquent Criminal & Civil Fines > Search the DCF Database). Your payment options are listed below:

- Pay online using a credit card, debit card or electronic check through Municipal Services Bureau at www.[redacted].
- Call [redacted] and pay with a credit card, debit card or electronic check through Municipal Services Bureau.
- Mail a check or money order to: Municipal Services Bureau, PO Box [redacted]

Note: a convenience fee is assessed by Municipal Services Bureau for electronic payment services.

IMPORTANT: To ensure the accurate application of your payment, please reference the case number(s).

Failure to resolve this matter within 10 days from the date of this letter may result in the exercising of our rights under the Court's judgment, including one or more of the following actions: garnishment of your [redacted] wages, and/or bank account(s), or Municipal referral of your account to an outside collection agency.

If you have any questions, please feel free to contact us at [redacted].

This is an attempt to collect a debt and any information obtained will be used for that purpose.

[redacted]

PAYMENT DUE: August 31, 2017

MUNICIPALITY OF [redacted]
TREASURY DIVISION

August 4, 2017

This is A Test Case
123 Test St
[redacted]

[redacted]

PAY YOUR COURT-ORDERED CRIMINAL FINES/FEES NOW

DELINQUENT AMOUNT DUE: $ 96.67*

HOW TO PAY

Pay Online: www.[redacted]
Pay by Phone: 1[redacted]
Pay by Mail: **Check or Money Order Payable To:** [redacted] **P.O. Box** [redacted]

Note: Reference your [redacted] Case Number on check or money order for prompt processing. You may also use the enclosed postage-paid envelope to mail your payment. A convenience fee is assessed by MSB for electronic payment services. Credit/debit card transactions may appear as charges from Gila Corporation on your bank or credit card statement.

INFORMATION YOU NEED TO MAKE A PAYMENT

[redacted] **Case Number:** 3AN-77-77777MO [redacted] **Ticket Number: A123456** **Offense Date: 01/01/2000**

Charge: AMC9.22.040(C): Stop-Decree Speed Without Notice To Rear Driver

Important: Delinquent cases, including the amount still due, can be viewed by anyone (examples: employers, landlords, and insurance companies) by visiting the public records website at: www.[redacted]

IF YOU DON'T PAY NOW, WE CAN GARNISH THE FOLLOWING:

Your PFD, wages, and/or bank account(s).

TROUBLE PAYING?

Contact Treasury / Delinquent Fines & Fees Customer Service at [redacted]

* *Your case has been referred to our third-party collection agency, MSB, so the delinquent amount due may not include MSB's full collection fee. Contact MSB for exact balance due. To discuss extended payment options, contact MSB at 1*[redacted]

This is an attempt to collect a debt and any information obtained will be used for that purpose.

Figure A.1: Example of adoption of BIT-NA trial

(c) Current communication

PAYMENT DUE: COLUMN A

[redacted]

MUNICIPALITY OF [redacted]
TREASURY DIVISION
FINAL DEMAND

COLUMN B

COLUMN C COLUMN D COLUMN E COLUMN F
COLUMN G
COLUMN H, COLUMN I COLUMN J

PAY YOUR COURT-ORDERED TRAFFIC FINES/FEES NOW

DELINQUENT AMOUNT DUE: $ COLUMN K*

HOW TO PAY

Online:	www.[redacted].com – Local Payments, Jurisdiction [redacted], Type: DCF Payments
Phone:	1 [redacted] (Press 3, then Jurisdiction [redacted])
Mail:	Check or Money Order Payable To: Municipality of [redacted] P.O. Box [redacted] [redacted]
In Person:	City Hall, [redacted] [redacted]

***Note**: Reference your [redacted] Case Number on check or money order for prompt processing. A convenience fee of 2.55% is assessed by ACI for electronic payment services. Credit/debit card transactions may appear as charges from ACI Payments Inc. on your bank or credit card statement.*

INFORMATION YOU NEED TO MAKE A PAYMENT

[redacted] **Case Number:** COLUMN L [redacted] **Ticket Number: M** **Offense Date: N**

Charge: O

Important: Delinquent cases, including the amount still due, can be viewed by anyone (examples: employers, landlords, and insurance companies) by visiting the public records website at: http://www.[redacted].

IF YOU DON'T PAY NOW, WE CAN GARNISH THE FOLLOWING:

Your [redacted], your wages, and/or your bank account(s).

IF YOU HAVE ANY QUESTIONS

Contact Treasury / Delinquent Fines & Fees Customer Service at [redacted].

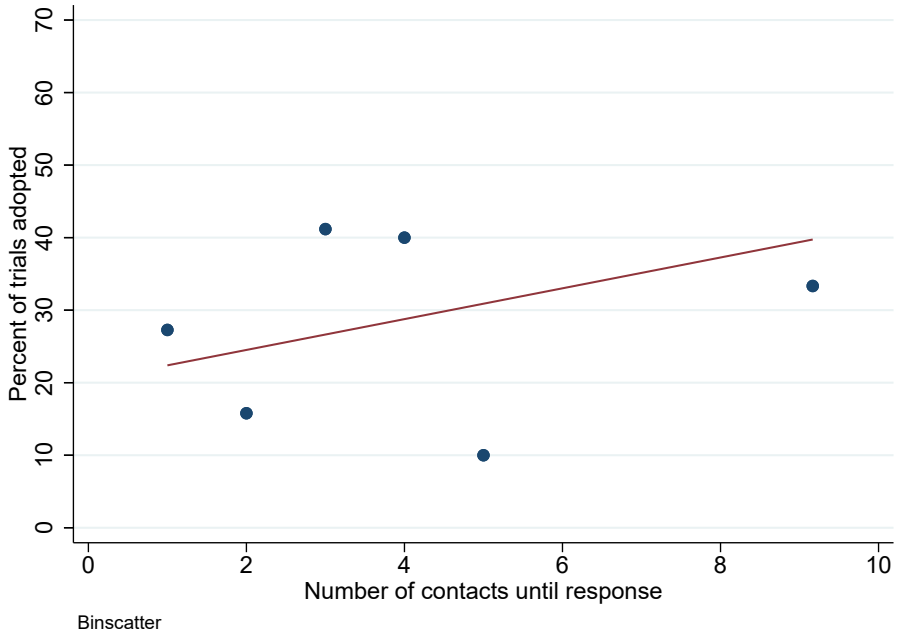
* *If you do not fully pay the delinquent amount due by the payment due date above, the [redacted] may refer your case to our third-party collection agency, Professional Credit, who will add a collection fee of 35.14% to your balance. Your new amount due would then become $P.*

To discuss extended payment options after your case has been referred, contact Professional Credit at 1 [redacted].

This is an attempt to collect a debt and any information obtained will be used for that purpose.

Figure A.2: Adoption by response times (bin scatters)

(a) Number of times contacted until final response



(b) Number of days from first request to final response

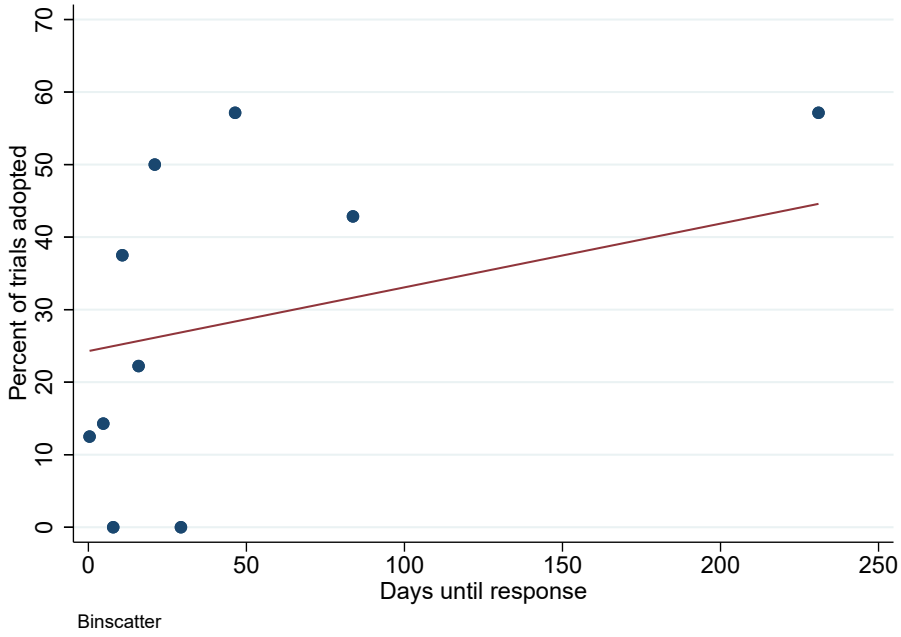
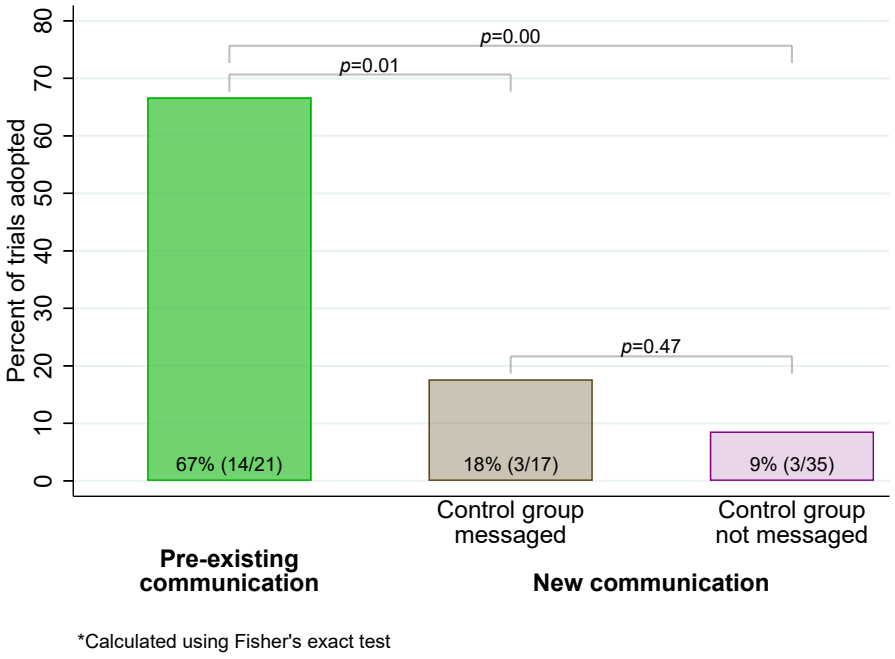


Figure A.3: Adoption of nudges by pre-existence: Additional results

(a) By control group communication



(b) By effect size (third bins)

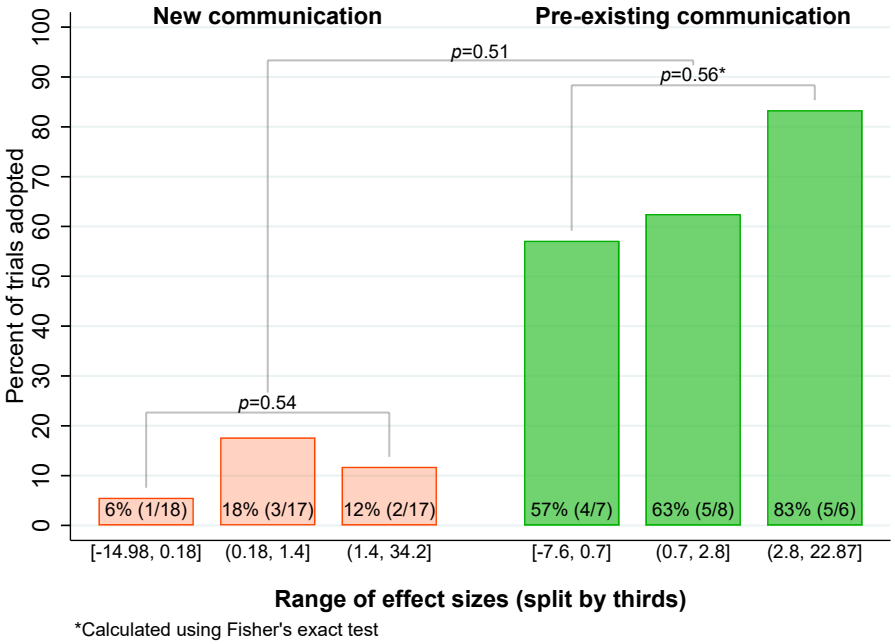
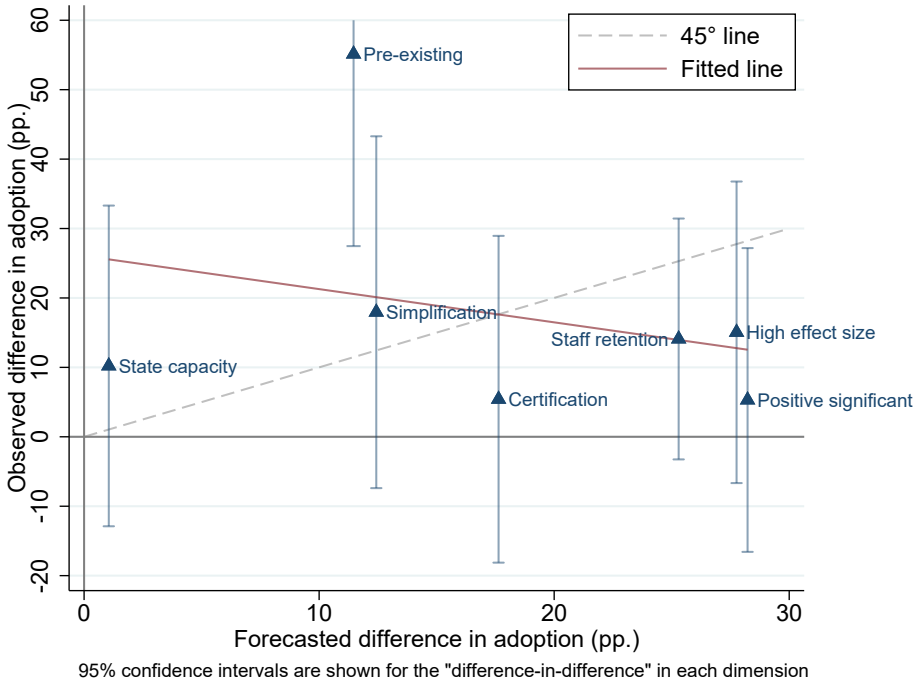


Figure A.4: Comparisons of forecasts and observed adoption

(a) By dimension



(b) Ranking of adoption compared to firms and governments of low-income countries

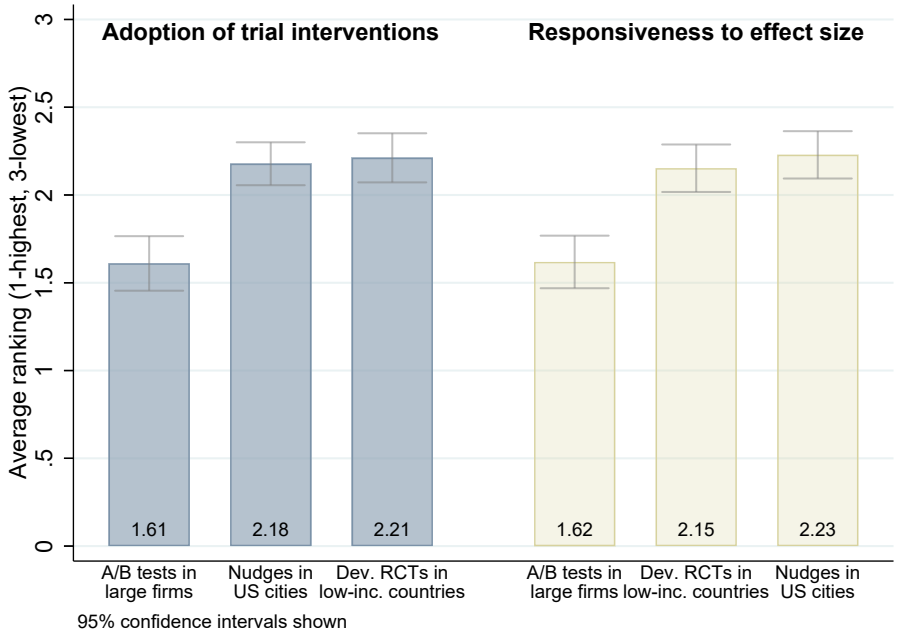


Table A.1: Average nudge treatment effects

	Nudge Units*	Updated BIT-NA
	(1)	(2)
Average treatment effect (pp.)	1.390	1.906
	(0.304)	(0.587)
Nudges	241	116
Trials	126	73
Observations	23,556,095	1,800,382
Average control group take-up (%)	17.33	15.07
Distribution of treatment effects		
25th percentile	0.06	0.01
50th percentile	0.50	0.40
75th percentile	1.40	1.72

This table shows the average treatment effect of nudges. Standard errors clustered by trial are shown in parentheses. pp. refers to percentage point.
*Column 1 replicates Column 2 of Table III in DellaVigna and Linos (2022).

Table A.2: Forecasts summary

	Observed	Forecasts (Mean % [SD])			
Category	(%)	(1) Overall ($N = 118$)	(2) Nudge unit staff ($N = 19$)	(3) Reseachers ($N = 67$)	(4) Government workers ($N = 14$)
Baseline adoption rate	27.40	32.47 [19.06]	37.16 [20.64]	31.91 [19.16]	32.00 [20.40]
By sign and significance:					
Positive & significant	30.30	46.58 [23.66]	48.00 [23.91]	46.49 [23.13]	44.29 [28.01]
Positive & insignificant	25.00	23.22 [20.27]	34.84 [23.51]	21.31 [18.59]	23.29 [22.61]
Zero or negative	30.77	11.06 [16.21]	17.47 [19.25]	10.43 [16.35]	12.93 [17.40]
By effect size:					
High third	37.50	49.31 [24.81]	54.26 [24.80]	48.85 [24.15]	45.43 [30.89]
Middle third	28.00	31.61 [19.60]	40.32 [23.51]	29.57 [16.95]	32.29 [25.08]
Low third	16.67	12.94 [15.42]	18.16 [18.39]	12.60 [15.80]	11.57 [11.88]
By staff retention:					
With original staff	32.61	43.75 [22.64]	48.26 [26.59]	42.13 [20.74]	44.07 [28.02]
Without original staff	18.52	18.45 [16.31]	24.32 [15.14]	17.51 [16.33]	19.71 [18.59]
By state capacity (proxied by 2020 city population size):					
Above median	31.82	33.26 [19.62]	40.16 [22.93]	32.07 [17.76]	31.79 [24.12]
Below median	20.69	32.21 [18.67]	35.84 [19.25]	32.15 [18.89]	29.50 [20.26]
By What Works Cities certification:					
Certified	29.55	41.69 [21.23]	45.26 [22.86]	40.06 [20.37]	42.93 [24.94]
Not certified	24.14	24.06 [17.90]	32.42 [20.61]	22.58 [17.21]	22.14 [17.16]
By pre-existing or new communication:					
New	11.54	29.79 [20.42]	36.32 [22.87]	29.48 [19.48]	25.50 [19.00]
Pre-existing	66.67	41.25 [25.43]	45.63 [26.68]	37.78 [23.11]	47.71 [28.97]
By behavioral mechanism:					
Simplification	33.33	42.42 [22.06]	51.16 [24.34]	40.21 [20.56]	41.00 [25.45]
Personal motivaton	19.05	30.14 [19.30]	35.84 [20.55]	28.37 [18.35]	26.79 [19.42]
Social cues	24.39	29.83 [20.97]	34.74 [23.05]	28.81 [19.58]	30.36 [22.01]

Table A.3: Determinants of nudge adoption (logit)

Dep. Var.: Nudge adopted (0/1, logit)	(1)	(2)	(3)	(4)	(5)	(6)
Max $t \geq 1.96$	0.11			-0.19	-2.19	-0.57
	(0.63)			(0.59)	(1.27)	(0.77)
Max treatment effect (10pp.)	0.29			0.81	3.25	0.34
	(0.54)			(0.55)	(1.99)	(0.56)
City staff retained		0.72		0.56	0.62	0.42
		(0.48)		(0.61)	(2.26)	(0.77)
Above-median city population		0.23		0.36		
		(0.71)		(0.84)		
What Works Cities certified		0.28		1.07		
		(0.64)		(0.83)		
Communication pre-existed			2.76	2.94	5.99	3.62
			(0.68)	(0.70)	(1.99)	(1.02)
Mechanism						
Simplification & information			0.02	0.23	-0.00	0.10
			(0.80)	(0.76)	(2.52)	(0.95)
Personal motivation			-0.92	-0.93	1.52	-1.31
			(0.81)	(0.88)	(1.53)	(0.96)
Social cues			-0.46	-0.62	-0.08	-0.74
			(0.56)	(0.56)	(1.13)	(0.64)
Control take-up (10%)						0.08
						(0.17)
Uses online mediums						1.60
						(0.83)
Years since trial						0.83
						(0.52)
Constant	-1.10	-1.78	-1.34	-2.81	-2.16	-8.43
	(0.39)	(0.79)	(0.61)	(1.31)	(3.60)	(2.98)
Average adoption	0.27	0.27	0.27	0.27	0.27	0.27
City fixed effects					✓	
Policy area fixed effects						✓
Number of trials	73	73	73	73	73	73
Number of cities	30	30	30	30	30	30
Pseudo R^2	0.01	0.03	0.29	0.33	0.73	0.40

Standard errors clustered by city are shown in parentheses. "Policy area fixed effects" includes a dummy for each city and each of the policy areas (Community engagement; Environment; Health; Registration & regulation compliance; Revenue collection & debt repayment; Take-up of benefits and programs; and Workforce & education).

Table A.4: Determinants of nudge adoption (robustness)

Dep. Var.: Nudge adopted (0/1, OLS)	(1) Baseline	(2)	(3)	(4)
Max $t \geq 1.96$	-0.02	-0.06	0.07	0.04
	(0.08)	(0.09)	(0.09)	(0.09)
Max treatment effect (10pp.)	0.10	0.11	0.03	0.03
	(0.08)	(0.08)	(0.06)	(0.06)
City staff retained	0.07	0.04	-0.01	-0.04
	(0.08)	(0.09)	(0.07)	(0.08)
Above-median city population	0.07	0.05	0.13	0.11
	(0.10)	(0.12)	(0.08)	(0.08)
What Works Cities certified	0.13	0.17	0.05	0.10
	(0.11)	(0.12)	(0.12)	(0.12)
Communication pre-existed	0.53	0.56	0.41	0.45
	(0.13)	(0.13)	(0.17)	(0.17)
Mechanism				
Simplification & information	0.04	0.05	0.11	0.12
	(0.10)	(0.09)	(0.06)	(0.06)
Personal motivation	-0.12	-0.15	-0.12	-0.15
	(0.12)	(0.11)	(0.07)	(0.07)
Social cues	-0.07	-0.09	-0.08	-0.11
	(0.08)	(0.09)	(0.09)	(0.09)
Constant	0.03	0.08	-0.03	0.01
	(0.17)	(0.19)	(0.13)	(0.12)
Average adoption	0.27	0.29	0.15	0.16
Dropping marginal non-adopts		✓		✓
Dropping verbal-only adopts			✓	✓
Number of trials	73	69	62	58
Number of cities	30	29	29	27
R^2	0.38	0.41	0.39	0.44

Standard errors clustered by city are shown in parentheses. "Policy area fixed effects" includes a dummy for each city and each of the policy areas (Community engagement; Environment; Health; Registration & regulation compliance; Revenue collection & debt repayment; Take-up of benefits and programs; and Workforce & education). "Baseline" replicates Column 4 of Table 1. See Online Appendix A for details.