

# **Appendix A**

## IPL Null Results Guidelines

# IPL Null Results Guidelines

## Background

To accumulate knowledge, science requires both transparency and replication. Transparency facilitates the ability of scholars to explore the underlying data and understand the process by which inferences were made from that data. Replication enables scholars to probe whether published results are robust to alternative specifications and measurements. It also builds on the findings of prior studies in establishing the conditions under which demonstrated relationships may or may not hold in other contexts. A commitment to publishing studies with null results—studies where the results are not statistically significant at conventional levels—has important implications for both replication and transparency. This type of commitment would provide full visibility for researchers into the entire range of studies that have been carried out, facilitating our collective ability to build on prior work and update our beliefs on the basis of both published and unpublished studies.

Unfortunately, the publication of null results remains uncommon in the social sciences due in part to publication bias, in which journals prize statistically significant findings on a given topic and not those without such relationships. Publication bias disincentivizes scholars from investing time in writing up and sharing null findings, ultimately preventing valuable information from reaching the scientific community.

At the Immigration Policy Lab (IPL), we believe that a commitment to posting null results reflects an important part of the scientific enterprise. The following guidelines are designed to mitigate the issue of publication bias within our scholarly community by establishing a standardized and efficient mechanism to disseminate studies with null results. We will adhere to these guidelines within IPL to the best of our ability.

## Eligibility

To avoid publication bias, all pre-registered work involving an IPL faculty director or funded by IPL must be made available to the scholarly community in some form. If, for any reason, the authors of a pre-registered study with null results decide against publishing the study in a peer-reviewed journal or publicly available working paper, they will post a null results report as described in this document. Note that when the authors decide to publish a study with null results in a peer-reviewed journal or post a working paper, they do not need to produce a null results report.

Studies that are not pre-registered can also be published as null results reports. This process largely applies to non-experimental work, and may involve a set of rejected conjectures in the course of analyzing observational data. While authors are not required to produce null results reports for

these types of studies, we encourage them to do so if they decide against publishing them in peer-reviewed journals or as working papers. Whether the study has been written or only some analysis has been done before deciding against publishing the study in a journal or as a working paper, we encourage authors to write a short report as part of IPL's null results series.

## Format

Reports should be short to ensure that 1) authors are not deterred by the need to invest a lot of time writing them and 2) other scholars can digest the main findings quickly. As a result, we suggest a maximum limit of 5 single-spaced pages, with shorter reports encouraged as well.

The reports should include the following sections: First, they should begin with an introduction that provides an overview of the project and the hypotheses it was designed to test. Next, they should offer a brief overview of the research design, which will be followed up by the main results. The results section should be very brief and only report the results of the main hypotheses that are presented in the description section and follow the pre-analysis plan (if there is one). The authors can also include additional tests, including subgroup analysis, in an optional appendix which can be referenced in the results section. After presenting the results, scholars should engage in an informed discussion about the possible explanations for the null results according to the interpretation guidelines described below. Finally, the reports should conclude with a short section describing implications for the literature and next steps for researchers interested in related topics.

To write the report, authors should use the null results template available on the IPL website. This template should enable authors to complete their reports quickly and comprehensively.

## Interpretation Guidelines

This section is particularly important in the null-results report, but challenging to write. One of the main purposes of the null-results report is to combat publication bias. This section should therefore make clear whether the null results were more likely caused by design and implementation, or by a flaw in the theory the study attempted to test. Before addressing the standing of the theory, this section should comment on all of the following:

First, this section should comment on the statistical power of the study. Was the sample size large enough to detect an effect? The authors can refer to effect sizes in related studies, confidence intervals, and power analysis to discuss the statistical power of the study.

Second, this section should discuss the measurement strategy. Were the outcome and treatment variables measured accurately? Are there alternative measures of these variables that may have caused other results? Are the results insignificant across a variety of measures?

Third, this section should discuss the implementation of the research design. Were there any difficulties during the implementation? Is it plausible that these difficulties may have caused the null results? How can future researchers who implement similar studies avoid such difficulties?

Fourth, this section should discuss any possible spillover or contamination of the control and

treated groups. If possible, this section can also refer to analysis of possible spillover that is shown in the appendix.

Finally, the authors should discuss potential problems with the theory after ruling out alternative explanations for the null results, such as whether the theory may apply differently across contexts.

## **Data Sharing**

In line with transparency standards for published quantitative research, data relevant to results in the report should be shared online. This data sharing will enable replication by other scholars. All data should be anonymized and in line with general standards for online data publication. Publicly posting data is subject to relevant data use agreements and Institutional Review Board approval.

## **Internal Review Process**

Before publication, each report must complete an internal review process. First, the results should be replicated within the team of scholars on the project. Second, the full report should be reviewed by an IPL scholar who was not involved in the original project.

## **Logistics**

Upon obtaining a null result, all co-authors should discuss whether to write a full working paper for submission to a peer-reviewed journal or publish a short null-results report. The project director(s) can make a final decision if there are different preferences among co-authors.

Null results reports should be cited in the following format:

Authors. Year. "Article Name." IPL Null Results Series.

Authorship of null results reports should follow the Author Guidelines for Collaborative Projects in the Immigration Policy Lab.

To post your null results report to the IPL website, please contact [Insert Name]. In your email, please indicate who outside the research team reviewed the study. IPL will provide a cover page for the report. Once you append the cover page, please post the report to a preprint server of your choosing. The link will then be used to complete the posting on the IPL website.

Finally, authorship of relevant projects is dictated according to the Author Guidelines for Collaborative Projects in the Immigration Policy Lab.

# Template for Null Results Report

## Introduction

In one or two paragraphs, explain what motivated the project, the academic literature to which it relates, and the argument it was designed to address.

## Research Design

Briefly review the research design, including but not limited to:

- Data used for the study.
- Identification strategy.
- Independent and dependent variables.
- Primary hypotheses tested in the study.

## Results

In one or two paragraphs (and one or two figures / tables as appropriate), describe the results for the study's primary hypotheses. Any additional analysis can be reported in an appendix as needed.

## Explanation of Null Results

Consider whether the following factors contributed to null findings in the study:

- Statistical Power:
  - Was the sample size large enough to detect an effect?
- Measurement Strategy:
  - Were independent and dependent variables measured correctly?
  - Would alternative measures have produced other results?
  - Are results insignificant across a variety of measures?
- Implementation of Experimental Design:
  - Were unexpected difficulties encountered during implementation that may have contributed to null findings?
  - Were there potential problems with how the treatment was designed?
- Spillover or Contamination of Control and Treated Groups

- Did spillover occur?
- Theoretical Issues
  - If the above explanations are unlikely to account for the null findings, are there shortcomings of existing theories that could explain the results?
  - Briefly describe what these shortcomings might be.

## **Implications**

Briefly describe the implications of the study. When considering implications, examine the following:

- Implications of the study for the academic literature.
- Implications of the study for policymakers and practitioners.
- Suggest next steps for research related to this topic.

## **Appendix B**

Null Results Report: Nudges for Citizenship Fee Waivers

# Null Results Report: Nudges for Citizenship Fee Waivers

Michael Hotard<sup>1,\*</sup>, Duncan Lawrence<sup>1</sup>,  
Jens Hainmueller<sup>1,2,3</sup>, David D. Laitin<sup>1,2</sup>

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<sup>1</sup>Immigration Policy Lab, Stanford University. <sup>2</sup>Department of Political Science, Stanford University. <sup>3</sup>Graduate School of Business, Stanford University. \*Corresponding author: Michael Hotard, Immigration Policy Lab, Stanford University, 417 Galvez Mall, Encina Hall West, Suite 100. Email: mhotard@stanford.edu



# 1 Introduction

Becoming a citizen is an important part of integration for many immigrants. However, each year only 10% of eligible lawful permanent residents in the United States submit an application for citizenship, despite the fact that surveys have shown that the majority of them want to become citizens.<sup>1,2</sup> Previous research on the gap between naturalization intention and action has revealed that lack of language abilities, a lengthy application process, and financial costs are some of the barriers that deter immigrants in the United States from applying for naturalization.<sup>3,4,5</sup> As of 2018, it costs most immigrants in the United States \$725 to apply for citizenship, although there are some lower fee options for low-income immigrants and veterans.

Our team has investigated the relationship between application costs and citizenship application rates through a series of experiments that have been embedded in the NaturalizeNY program. NaturalizeNY is a public-private partnership that offers fee vouchers to low-income immigrants in New York who are eligible to naturalize and have incomes between 150% and 300% of the Federal Poverty Guidelines. The program also informed immigrants whose incomes were below the program’s eligibility requirements about the federal fee waiver, an opportunity for low-income immigrants to apply for citizenship and have the entire cost waived if they receive means-tested benefits or have incomes below 150% of the Federal Poverty Guidelines. Using data from the program’s first year, our research team found that winning a voucher that pays the application fee led to a 41 percentage point increase in likelihood that a person applied for citizenship, essentially doubling the rate.<sup>6</sup> However, five variants of information nudges designed to increase the take-up of the fee waiver all produced null results.<sup>6</sup> These original nudges consisted of phone calls, text messages, and letters reminding immigrants about the opportunity to use a fee waiver. In the second year of the program, we altered the nudge for fee waivers to test whether or not receiving any type of information increased the rate of citizenship applications and usage of the fee waiver. We found that informing people about the fee waiver opportunity increased citizenship applications by 8 percentage points, which constituted a 35 percent increase over the baseline application rate.<sup>7</sup> Both of the nudge results contribute to the literature on informational nudges, which shows mixed results depending on the program and intervention.<sup>8,9,10,11,12,13</sup>

In this experiment, we evaluated another set of low-cost nudges to measure their effectiveness at increasing citizenship applications. We tested whether or not providing more detailed information about the fee waiver program and whether immediately allowing someone to sign up for a citizenship workshop had an effect on the likelihood of submitting a naturalization application.

## 2 Research Design

**Sample:** The sample consists of 749 immigrants who registered for the NaturalizeNY program in 2018 and were screened as likely eligible for the federal fee waiver program. To register for NaturalizeNY and enter into the experiment, an immigrant had to live in New York City, be at least 18 years old, be eligible for naturalization in the United States, and have a household income below 150% of the Federal Poverty Guidelines or be receiving means-tested benefits (e.g. TANF, SNAP, Medicaid, etc.).

**Study Design:** Randomized Experiment. The experiment included participants who completed

an online registration (consisting of demographic and eligibility questions) for the NaturalizeNY program between May 25, 2018, and July 2, 2018, and who were screened as likely being eligible for the federal fee waiver program. When participants completed their registration, they were randomly assigned by the registration system (using a random assignment feature in Qualtrics) into one of three groups, which would determine the final screens and messages that they would see.

The first group, referred to as the control group, received a simple message on the final screen of the registration that stated they were not eligible for the NaturalizeNY voucher but they could likely apply for free using a fee waiver. It provided a website and phone number that a person could visit to receive more information.

The second group, referred to as the enhanced message, were told they were not eligible for the NaturalizeNY voucher but could likely apply for free using a fee waiver. However, compared to the control group, the message they received contained more information about the fee waiver as well as formatting that would draw attention to the information. This group also received an immediate follow-up email about the fee waiver that contained the same information as the screen at the end of registration.

The third group, referred to as the invitation message, received a similar message as the second group. However, the participants in this group also received an additional question asking if they would like to register for an upcoming workshop where they could receive assistance with their citizenship and fee waiver applications. If a participant answered affirmatively to the question about attending the workshop, he or she was given the opportunity to schedule a time at a specific citizenship workshop in New York City through an online reservation system.

The randomization was set to assign 20% of participants into the control group and 40% into both treatment arms. The actual randomization assigned 143 participants into the control group (19.1%), 306 participants into the enhanced treatment group (40.9%), and 300 participants into the invitation group (40.1%).

Data for the results came from a follow-up survey that was conducted approximately seven months after the participants registered for the program. The survey asked participants if they had submitted a citizenship application and if they had attended the workshop that some participants had been invited to. We received survey responses from 561 out of 749 participants (74.9%). The full treatments and survey are shown in Appendix I.

**Hypotheses:** We tested the following primary hypotheses:

- Enhanced information nudges would increase the likelihood of a person submitting a citizenship application.
- Allowing a person to schedule an appointment to receive assistance with their citizenship and fee waiver applications would increase the likelihood of a person submitting their citizenship application.

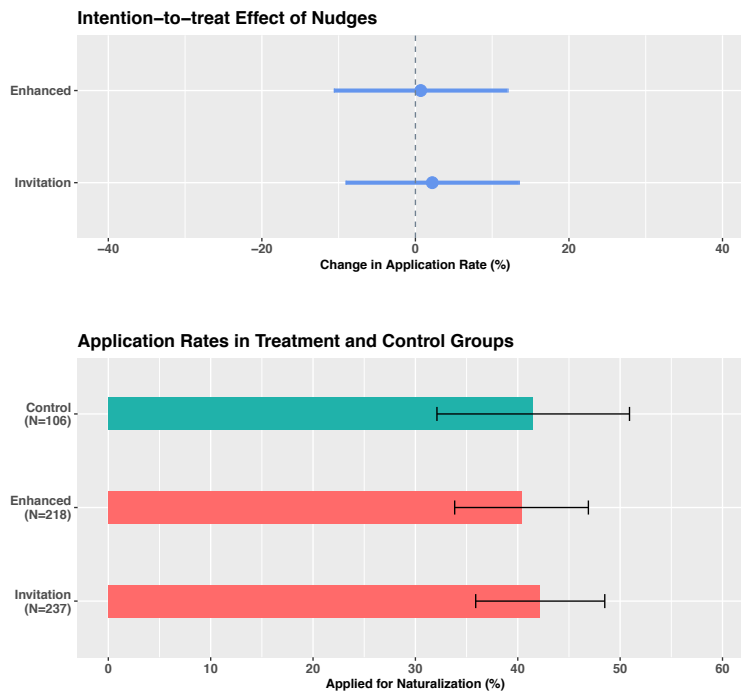
**Outcomes:** Respondents were surveyed from January 29, 2019 to March 23, 2019, and asked whether they had applied for citizenship. The survey also collected additional information on whether they had paid to submit their citizenship application (if they had submitted one) and whether they attended the citizenship workshop that was specifically mentioned in the invitation

treatment arm.

### 3 Results

Results for the information nudges are shown in Figure B1. Neither of the nudges produced a significant increase in citizenship application rates compared to the baseline information treatment. The top chart of Figure B1 shows the estimated effects of the enhanced message and the invitation message on the application rate of participants from a covariate-adjusted linear regression model (enhanced effect: 0.69pp, CI: -10.6pp to 12.0pp; invitation effect: 2.19pp, CI: -9.1pp to 13.5pp). The bottom part of Figure B1 shows the application rates for each group. The fraction of respondents who reported that they had applied for citizenship was 41.5% for the control group (95% CI: 32.1% to 50.91%), 42.2% for group that received the enhanced message (95% CI: 33.8% to 46.9%), and 42.4% for the group that received the invitation (95% CI: 35.9% to 48.5%). See Table B4 in Appendix III for complete regression results.

Figure B1: Information Nudge Effects



Notes: Effects of the Information Nudges on Citizenship Application Rates. The top chart shows the change in percentage points in application rates for the enhanced and invitation treatments relative to the rate of the control group, using a covariate-adjusted model. The effect sizes are not significantly different than zero. The bottom chart shows the average application rate the control group and each treatment arm.

## 4 Interpretation

Below, we discuss several possible explanations for the null result findings, including methodological and theoretical issues.

### Methodological Explanations

**Power:** Given our sample size, the confidence interval around each effect is approximately 10 to 12 percentage points. In previous studies, we found interventions that increased the baseline application rates by 8.6 percentage points (through information) and 41 percentage points (through financial assistance). We cannot rule out the possibility that changing the text of the message or encouraging an immediate appointment led to a small effect, such as a 3 or even 8 percentage point increase in citizenship applications. Given that changing a message is relatively costless, even a small increase would be helpful for immigrant-service providers that want to do anything they can to increase naturalization rates. We do not have the power to detect small effects though, and we cannot make a recommendation on how to improve outreach based on our experiment.

**Measurement:** Because the outcome is self-reported through a survey, there could be measurement error from people who misreport whether they have submitted their application. Social desirability bias may cause some respondents to erroneously report submitting an application because they believe that is the correct answer during a follow-up interview about a naturalization program. However, we do not believe that this bias would be correlated with the treatment assignment. Also, in previous experiments, we found significant differences between naturalization encouragements using similar designs and surveys, which provides evidence that differences could be measured if they were large enough.

**Attrition:** We had a 25% attrition rate, costing us some much needed power. We also examined the response rates for treatment groups (see Table B3) to determine if there was differential attrition. There is a difference of 8 percentage points between the enhanced message group and the invitation message group. Because neither group would have known that they were receiving one treatment versus the other and neither treatment had monetary value associated with it, we do not believe that either treatment should have had a systematic effect on the response rate. Also, because of the wide confidence levels around the effect estimates, this differential rate of attrition is unlikely to be the reason why there was no significant difference detected.

**Treatment Design:** It is very likely that limiting aspects of treatment design contributed to the results of our study. We set out to test whether eliminating the administrative challenges of the citizenship application would increase application rates in a similar way that lowering the financial barriers does. However, our invitation nudge was limited in scope. All of the participants had to choose from time slots at only one location and only one date based on when our partner in New York was holding a large citizenship workshop. Perhaps, a better design would have enabled each registrant to pick a date, time, and location that would have been most convenient for them, but such an “ideal” system is unlikely to exist in reality. It is possible that a scheduling intervention in which a person chooses dates and locations would have been more successful than our treatment.

Additionally, our enhanced treatment message only tested one possible variant of a message that we hoped would be easier to comprehend. There are an infinite number of ways to communicate the message about a person’s likely eligibility for the fee waiver, and it is impossible to rule out

that an alternate design choice could have made a larger difference. We now have evidence that even a short message with basic information performs relatively well compared to a message with concrete steps and more detailed instructions on how to use the fee waiver.

**Spillover:** Our study could be affected by spillover if study participants informed one another about the fee waiver after receiving different treatments, but it is unlikely that this is a major contributor to the null effect. Registration for the study was done online and the sample was geographically diverse, with participants in many different neighborhoods in New York City. If there were spillover, then we should not expect to find a significant difference in the share of participants who attended the in-person event. However, we found a significant difference in this first stage effect (see Table B6), meaning that at the very least, recipients of the invitation message were more likely to attend the event that they were invited to.

**Sample Selection:** Our sample consisted of immigrants who were actively seeking a way to relieve the financial burden of citizenship and may have been especially attuned to any information provided to them. The fact that they completed an online registration may also show they have the motivation and wherewithal to schedule an appointment with an immigrant-service provider themselves, and do not need the assistance of automatic scheduling. We do not know what effects our interventions would have had on a different group of low-income immigrants who may, on average, be less attentive to information about financial assistance and less motivated to apply for citizenship.

## Theoretical Explanations

**Additional Application Barriers:** Learning about the fee waiver program can help a person overcome the financial cost of applying for citizenship, but there are additional barriers besides cost that prevent a person from applying for citizenship. For example, the application for citizenship (N-400) is a complicated form that is estimated to take between five and twelve hours to complete, and low-income immigrants may find it challenging to set aside the time required to complete this form. More intensive interventions may be needed to address additional barriers.

**General Theory of Nudge Failures:** Building a more general theory of nudge success is challenging because the effectiveness of any individual nudge is greatly affected by the localized context and sample. Sunstein<sup>14</sup> offers a helpful framework for thinking about failed nudges that can provide guidance for classifying failures. Nudge designers should first consider whether a failed nudge actually reveals the desired preferences of the recipients, which for our experiment would mean, contradicting their expressed preferences when they registered for the program, that they actually did not want to apply for citizenship. This seems unlikely in our case. In the case that the social welfare benefit is not in question, then Sunstein offers additional reasons why a nudge may fail. Perhaps, he conjectures, the nudge caused confusion among recipients, or that any effects were too short term to have a lasting impact, or that the nudge designers failed to understand the choice architecture that participants actually face. Distinguishing among these reasons would require additional follow-up to understand the motivations, decision-making process, and actions of those who do not respond in the predicted direction to the nudge. Nudges are popular interventions because data on their success is often easy to obtain and analyze, but understanding a failed nudge often requires more robust follow-up and understanding.

## 5 Implications and Next Steps

**Information Works, More Information May Not Help:** Over the course of three studies, we found that telling people about the fee waiver increased their likelihood of submitting an application, but reminding them about the fee waiver, or providing more detailed information about the fee waiver did not increase application rates more than the simple message telling people about the fee waiver. This suggests that once motivated people become aware of the fee waiver program, they are able to find the information they need to access its benefits. One interpretation of this pattern is that this population of immigrants has a general lack of knowledge about the program rather than confusion about how to access it.

**More Research Needed on In-Person Workshops:** We found that the invitation treatment increased the likelihood of a person attending the in-person workshop by 17 percentage points, but we did not detect a difference in the actual application rates between the invitation and control groups. This suggests that the people who attended the workshop may have been able to complete the application without attending that specific workshop. One explanation is that the immigrants who attend workshops are already a self-selected group of interested and able applicants, and the invitation is not effective as assisting those with less motivation or capacity. Because we only asked about one specific workshop in our follow-up survey, these results would also be consistent with a scenario in which members of the control group attended a citizenship workshop at the same rate as the invitation group, but not the specific workshop that was asked about on the survey. However, this scenario seems unlikely.

**Additional Barriers Exist:** The results suggest that the financial barrier caused by the application fee is not the only barrier preventing low-income immigrants from applying for citizenship. Additional research could be done to understand what other barriers exist. Immigrant service providers should consider rigorous evaluations of their outreach strategies to determine what are the most effective ways to encourage naturalization among eligible immigrants.

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## 6 Appendices for Nudges for Citizenship Fee Waivers

### 6.1 Appendix I: Full Treatments

Figure B2: Control closing message.



Good news! Based on your responses it appears that you are likely eligible for a federal fee waiver to cover the cost of the naturalization application and therefore you do not need to participate in the fee voucher lottery. It appears you are likely eligible for the federal fee waiver because you receive means-tested benefits and/or your household income is below 150% of the Federal Poverty Guidelines.

Please contact your local [Office for New Americans Opportunity Center](#) to find out how you can apply for the federal fee waiver and learn about the naturalization process.



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Figure B3: Enhanced closing message.



Thank you for completing the NaturalizeNY registration. We have some exciting information to share with you.

Based on the information you provided about your income and government benefits you receive, you may be eligible for a U.S. Citizenship and Immigration Services (USCIS) fee waiver. Everyone who is eligible for the waiver can apply for free. Therefore, you do not need to participate in the NaturalizeNY voucher lottery.

In order to apply for a fee waiver, you will need to complete an additional form when you submit your citizenship application.

Here are next steps you can take to apply for a fee waiver and citizenship.

- 1) **Review** the required [forms to apply for the fee waiver](#).
  
- 2) **Call** a [service provider](#) close to you to schedule an appointment to receive free assistance with your application. Be sure to ask them about the Federal Fee Waiver.
  
- 3) **Submit** your application with the fee waiver form and, if you are eligible, avoid paying the expensive fee.

Find out more about this opportunity and find a local service provider to receive free application assistance by contacting the New Americans Hotline at 1-800-566-7636 or visiting the [Office for New Americans website](#).



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Figure B4: Enhanced message sent via email.



Thank you for completing the NaturalizeNY registration. We have some exciting information to share with you.

Based on the information you provided about your income and government benefits you receive, you may be eligible for a U.S. Citizenship and Immigration Services (USCIS) fee waiver. Everyone who is eligible for the waiver can apply for free. Therefore, you do not need to participate in the NaturalizeNY voucher lottery.

In order to apply for a fee waiver, you will need to complete an additional form when you submit your citizenship application.

Here are next steps you can take to apply for a fee waiver and citizenship.

- 1) **Review** the [required forms to apply for the fee waiver](#).
- 2) **Call** a [service provider](#) close to you to schedule an appointment to receive free assistance with your application. Be sure to ask them about the **Federal Fee Waiver**.
- 3) **Submit your application** with the fee waiver form and, if you are eligible, avoid paying the expensive fee.

Find out more about this opportunity and find a local service provider to receive free application assistance by contacting the New Americans Hotline at 1-800-566-7636 or visiting the [Office for New Americans website](#).

Figure B5: Invitation closing message.



English

Thank you for completing the NaturalizeNY registration. We have some exciting information to share with you.

Based on the information you provided about your income and government benefits you receive, you may be eligible for a U.S. Citizenship and Immigration Services (USCIS) fee waiver. Everyone who is eligible for the waiver can apply for free. Therefore, you do not need to participate in the NaturalizeNY voucher lottery.

In order to apply for a fee waiver, you will need to complete an additional form when you submit your citizenship application.

Would you like to schedule an appointment to apply for citizenship and a fee waiver?

There is an upcoming citizenship workshop on **Saturday, June 30** at John Jay College in Manhattan where you can receive help filling out your application.

Would you like to make an appointment for that event now?

Yes

No

NEXT



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Figure B6: Invitation closing message if a participant did not want to schedule an appointment.



In order to apply for a fee waiver, you will need to complete an additional form when you submit your citizenship application.

Here are next steps you can take to apply for a fee waiver and citizenship.

- 1) **Review** the required [forms to apply for the fee waiver](#).
- 2) **Call** a [service provider](#) close to you to schedule an appointment to receive free assistance with your application. Be sure to ask them about the **Federal Fee Waiver**.
- 3) **Submit your application** with the fee waiver form and, if you are eligible, avoid paying the expensive fee.

Find out more about this opportunity and find a local service provider to receive free application assistance by contacting the New Americans Hotline at 1-800-566-7636 or visiting the [Office for New Americans website](#).



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## 6.2 Appendix II: Outcome Questions

1. Have you submitted your U.S. citizenship application?

- Yes
- No

2. When you submitted your application, did you have to pay a fee?

- Yes, I paid a fee of \$725.
- Yes, I paid a fee of \$405.
- No, I did not pay a fee.

3. Did you attend CUNY Citizenship Now's citizenship event at John Jay College on June 30, 2018?

- Yes
- No
- I don't know

### 6.3 Appendix III: Regression Outcomes

Table B1: Balance Checks for Nudge Study (All Participants)

	Treatment Groups:	
	Enhanced	Invitation
Household income p. capita (binned to quartiles)	0.108 (0.095)	0.029 (0.093)
Years on green card (binned to quartiles)	0.135 (0.099)	0.117 (0.100)
Age (binned to quartiles)	-0.018 (0.115)	-0.018 (0.117)
Female	0.221 (0.209)	0.298 (0.212)
Dominican Republic	0.077 (0.258)	-0.051 (0.263)
Ecuador	-0.445 (0.404)	-0.617 (0.421)
Jamaica	-0.247 (0.400)	-0.039 (0.393)
Married	0.013 (0.311)	-0.181 (0.309)
Single	0.090 (0.315)	-0.147 (0.312)
High School/GED degree	-0.069 (0.310)	-0.212 (0.308)
Some College	0.069 (0.312)	-0.227 (0.313)
BA degree or higher	0.301 (0.324)	0.186 (0.323)
Spanish (registered)	0.502 (0.257)	0.336 (0.265)
Constant	-0.154 (0.576)	0.421 (0.590)
Observations	748	
Pseudo $R^2$	0.013	
Chi-Square	19.54	
P-value	0.813	

*Note:* Multinomial logit regression coefficients shown with robust standard errors in parentheses. The reference category is participants in the control group that received no additional nudge. Chi-Square and P-value in the bottom rows are from an omnibus Chi-Square test against the null that all slope coefficients are jointly equal to zero. This model is based on the sample of all participants. One participant did not report their gender, which is why analyses that include Female have only 748 observations.

Table B2: Balance Checks for Nudge Study (Responders to follow-up survey)

	Treatment Groups:	
	Enhanced	Invitation
Household income p. capita (binned to quartiles)	0.077 (0.112)	0.012 (0.107)
Years on green card (binned to quartiles)	0.097 (0.122)	0.164 (0.122)
Age (binned to quartiles)	-0.070 (0.135)	0.013 (0.135)
Female	0.210 (0.244)	0.333 (0.242)
Dominican Republic	0.152 (0.309)	0.111 (0.304)
Ecuador	-0.328 (0.462)	-0.477 (0.474)
Jamaica	0.053 (0.479)	0.127 (0.462)
Married	-0.217 (0.354)	-0.101 (0.354)
Single	-0.045 (0.361)	0.089 (0.361)
High School/GED degree	-0.052 (0.356)	-0.121 (0.348)
Some College	0.503 (0.372)	0.015 (0.369)
BA degree or higher	0.490 (0.383)	0.370 (0.378)
Spanish (registered)	0.705 (0.301)	0.305 (0.302)
Constant	-0.054 (0.674)	-0.010 (0.690)
Observations	561	
Pseudo $R^2$	0.020	
Chi-Square	23.81	
P-value	0.587	

*Note:* Multinomial logit regression coefficients shown with robust standard errors in parentheses. The reference category are participants in the control group that received no additional nudge. Chi-Square and P-value in the bottom rows are from an omnibus Chi-Square test against the null that all slope coefficients are jointly equal to zero. This model is based on the samples of all participants who responded to the follow-up survey.



Table B3: Survey Response Checks for Fee Waiver Group

	Responded to Survey	
	(1)	(2)
Enhanced Message	-0.029 (0.045)	-0.036 (0.045)
Invitation Message	0.049 (0.044)	0.047 (0.044)
Household income p. capita (binned to quartiles)		-0.008 (0.014)
Years on green card (binned to quartiles)		-0.044 (0.015)
Age (binned to quartiles)		0.005 (0.018)
Female		-0.004 (0.033)
Dominican Republic		0.014 (0.039)
Ecuador		0.063 (0.063)
Jamaica		0.119 (0.061)
Married		-0.067 (0.045)
Single		-0.008 (0.046)
High School/GED degree		-0.071 (0.045)
Some College		-0.033 (0.047)
BA degree or higher		-0.023 (0.048)
Spanish (registered)		0.166 (0.039)
Constant	0.741 (0.037)	0.849 (0.093)
Covariates	No	Yes
Observations	749	748
Adjusted R <sup>2</sup>	0.004	0.036
F-value	2.508	2.920
P-value	0.082	0.055

*Note:* Regression coefficients shown with robust standard errors in parentheses. Model 1 regresses whether a participant responded to the survey on the nudge group assignment indicators (the control group that received no additional nudge is the reference category). Model 2 adds covariates. The F-test is against the null that the regression coefficients on the nudge group assignment indicators are jointly equal to null.

Table B4: Intention-to-treat Effect Estimates for Nudge Study

	Applied for Naturalization	
	(1)	(2)
Enhanced Message	-0.011 (0.058)	0.007 (0.058)
Invitation Message	0.007 (0.058)	0.022 (0.058)
Household income p. capita (binned to quartiles)		0.006 (0.019)
Years on green card (binned to quartiles)		-0.028 (0.020)
Age (binned to quartiles)		-0.023 (0.024)
Female		-0.002 (0.044)
Dominican Republic		-0.021 (0.053)
Ecuador		-0.033 (0.086)
Jamaica		-0.170 (0.078)
Married		0.069 (0.060)
Single		0.032 (0.061)
High School/GED degree		-0.079 (0.060)
Some College		-0.042 (0.063)
BA degree or higher		-0.004 (0.064)
Spanish (registered)		-0.128 (0.052)
Constant	0.415 (0.048)	0.571 (0.124)
Covariates	No	Yes
Observations	561	561

*Note:* Regression coefficients shown with robust standard errors in parentheses. Models 1 and 2 regress the outcome of whether someone submitted their citizenship application on indicators for whether participants were assigned to the various nudge groups (the control group that received no additional nudge is the reference category).

Table B5: Intention-to-treat Effect Estimates for Fee Paid

	Paid Fee for Naturalization if Applied	
	(1)	(2)
Enhanced Message	-0.113 (0.085)	-0.117 (0.085)
Invitation Message	-0.064 (0.082)	-0.088 (0.080)
Household income p. capita (binned to quartiles)		-0.022 (0.029)
Years on green card (binned to quartiles)		-0.013 (0.035)
Age (binned to quartiles)		0.019 (0.037)
Female		0.058 (0.067)
Dominican Republic		0.196 (0.086)
Ecuador		0.214 (0.118)
Jamaica		0.126 (0.142)
Married		0.062 (0.094)
Single		0.009 (0.100)
High School/GED degree		-0.089 (0.102)
Some College		-0.113 (0.108)
BA degree or higher		-0.056 (0.110)
Spanish (registered)		0.059 (0.090)
Constant	0.744 (0.067)	0.707 (0.199)
Covariates	No	Yes
Observations	224	224

*Note:* Regression coefficients shown with robust standard errors in parentheses. Models 1 and 2 regress the outcome of whether someone paid to submit their citizenship application on indicators for whether participants were assigned to the various nudge groups (the control group that received no additional nudge is the reference category).

Table B6: Citizenship Workshop Attendance

	Attended Specific Citizenship Workshop	
	(1)	(2)
Enhanced Message	-0.000 (0.032)	0.007 (0.033)
Invitation Message	0.179 (0.040)	0.183 (0.040)
Household income p. capita (binned to quartiles)		0.003 (0.014)
Years on green card (binned to quartiles)		-0.030 (0.015)
Age (binned to quartiles)		0.033 (0.018)
Female		-0.036 (0.033)
Dominican Republic		0.032 (0.040)
Ecuador		0.016 (0.057)
Jamaica		-0.020 (0.057)
Married		-0.088 (0.049)
Single		-0.072 (0.050)
High School/GED degree		-0.012 (0.044)
Some College		-0.030 (0.044)
BA degree or higher		0.029 (0.053)
Spanish (registered)		-0.059 (0.040)
Constant	0.073 (0.027)	0.150 (0.097)
Covariates	No	Yes
Observations	524	524

*Note:* Regression coefficients shown with robust standard errors in parentheses. Models 1 and 2 regress the outcome of whether someone attended a specific citizenship workshop on indicators for whether participants were assigned to the various nudge groups (the control group that received no additional nudge is the reference category).

## **Appendix C**

Null Results Report: Improving Attitudes toward Refugees in Jordan

# Null Results Report: Improving Attitudes toward Refugees in Jordan

Ala Alrababah<sup>1,2</sup>, Andrea Dillon<sup>2</sup>, Scott Williamson<sup>1,2</sup>,  
Jens Hainmueller<sup>1,2,3</sup>, Dominik Hangartner<sup>2,4,5</sup>, and Jeremy Weinstein<sup>1,2</sup>

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<sup>1</sup>Department of Political Science, Stanford University. <sup>2</sup>Immigration Policy Lab, Stanford University and ETH Zurich.

<sup>3</sup>Graduate School of Business, Stanford University. <sup>4</sup>Center for International and Comparative Studies, ETH Zurich. <sup>5</sup>Department of Government, London School of Economics and Political Science.

# 1 Introduction

Millions of refugees and other migrants have left their home countries in recent years, and they have often faced hostility and backlash in their new homes. This problem underscores the need for additional research on attitudes toward migrants, particularly in countries that are close to conflict zones and host the most refugees. We implemented a survey in Jordan to test strategies for changing attitudes toward Syrian refugees in the country, which currently hosts over 600,000 registered refugees from Syria’s civil war.

Our focus on attitudinal change builds on recent innovative work examining the efficacy of strategies to generate greater openness toward migrant populations (e.g. Facchini et al. 2016; Adida et al. 2018). We designed an experiment to study the impact of small interventions for promoting generosity on attitudes toward refugees, with the interventions and associated mechanisms grounded in the literature on what motivates charitable giving (Bekkers and Wiepking 2011). Specifically, we attempted to improve attitudes toward refugees by priming Jordanians to think about the struggles and needs of Syrian refugees, to feel a “warm glow” associated with their country’s generosity toward Syrian refugees, and to reflect on religious values and their implications for how to treat Syrian refugees.\*

## 2 Research Design

**Sample:** The survey was implemented by the Center for Strategic Studies at the University of Jordan with a nationally-representative sample of 1,200 Jordanian citizens, in addition to a sample of 300 Jordanians in areas with high refugee density. See the online for details on the sampling process.

**Study Design:** Randomized Experiment. Respondents were randomly assigned with equal probability to a control group or one of three treatments associated with the generosity-inducing mechanisms of: awareness of needs; joy of giving; and religious values. Each treatment included a short paragraph of text followed by a video of approximately two minutes. For **awareness of needs**, respondents read information and watched a video about the suffering experienced by Syrian refugees. For **joy of giving**, respondents were given a thank you note from a Syrian refugee child and watched a video about the gratitude Syrian refugees felt for Jordanians. For **religious values**, respondents read an interpretation of the Quran advocating for generosity toward refugees and listened to a recitation of verses on this subject. The full treatments are shown in the online appendix.

**Hypotheses:** We tested the following primary hypotheses:

- **Awareness of Needs:** Respondents who receive the needs treatment will exhibit more positive and generous attitudes toward Syrian refugees.
- **Joy of Giving:** Respondents who receive the gratitude treatment will exhibit more positive and generous attitudes toward Syrian refugees.

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\* This study was registered under EGAP ID: 20180110AB.

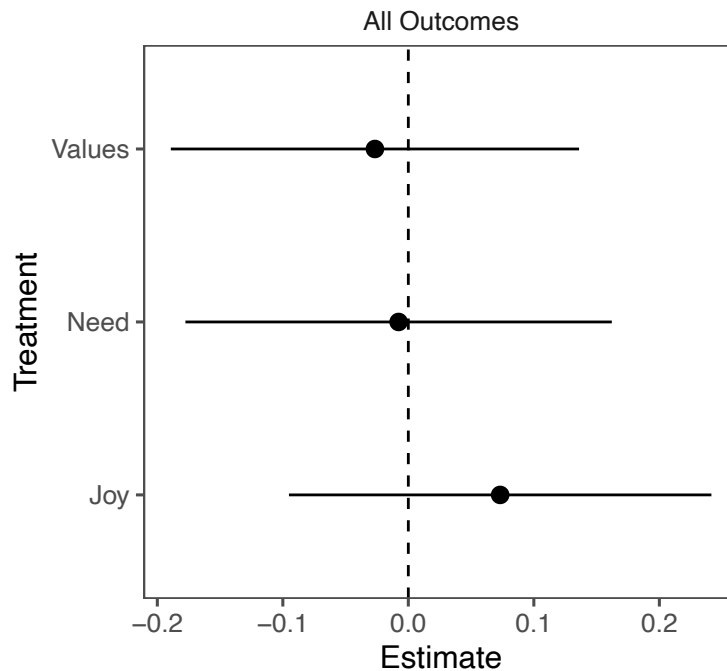
- **Religious Values:** Respondents who receive the values treatment will exhibit more positive and generous attitudes toward Syrian refugees.

We also pre-registered additional hypotheses about moderating variables that we do not focus on here, but for which details and results can be found in the online appendix.

**Outcomes:** Following these treatments, respondents were asked a series of questions to measure their: (a) attitudes toward Syrian refugees, (b) views of how Syrian refugees have impacted various aspects of Jordanian society, (c) support for refugee-related policies, and (d) behavioral generosity toward refugees. We use principal components analysis to create an aggregate outcome measure consisting of all of these questions, and then additional measures for the four categories of attitudes, impact, policy, and behavior. See the online appendix for details on the outcome measures. The principal components were normalized to have mean = 0 and standard deviation = 1.

### 3 Results

Figure C1: Coefficient plot showing the effect, point estimate and associated 95% confidence intervals, of each treatment on the first principal component of all outcomes.

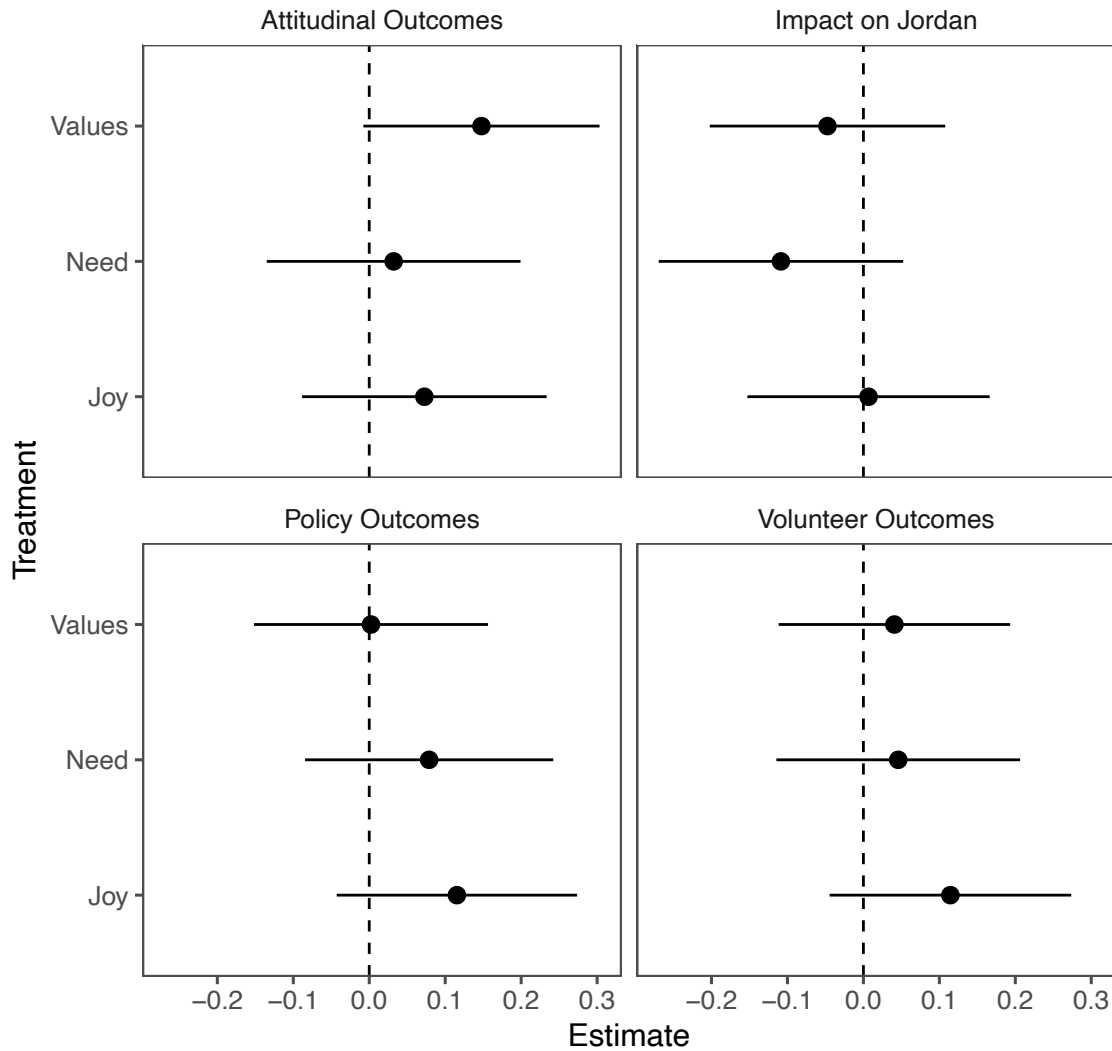


Results for the aggregate measure across all outcomes are shown in Figure 1, and results for the aggregate attitudinal, impact, policy, and behavioral outcomes are shown in Figure 2. None of the treatments produced significant effects as a whole or for any of the four subcategories. **Joy of giving** generated a slightly positive but not significant effect (see Figure 1), while **awareness of**



**need** and **religious values** resulted in fairly precisely estimated null effects. These patterns were similar across the four outcome categories (see Figure 2).

Figure C2: Coefficient plots showing treatment effects, point estimate and 95% confidence intervals, on (1) the first principal component of attitudinal outcomes, (2) the first principal component of respondents' view of the effect of Syrian refugees on Jordan, (3) the first principal component of policy outcome questions, and (4) the first principal component of the two behavioral outcome questions.



## 4 Interpretation

We consider several possible explanations for these findings, covering both methodological and theoretical issues. While we cannot say with certainty why the study returned insignificant results, our evidence suggests that some of the explanations are more credible than others.

## Methodological Explanations

- **Power:** First, it is possible that our face-to-face survey was underpowered. We performed power calculations that suggested an adequate sample size, and the precisely estimated null effects for **awareness of need** and **religious values** with the aggregate outcome are not indicative of issues related to power (See the online appendix for the power analysis). However, **joy of giving** may have been significant with a substantively larger sample (assuming that the point estimate would remain the same).
- **Measurement:** We do not believe that our results can be explained by imprecise measurement. We included outcome measures that are typical for studies of attitudes toward migrant populations. The use of PCA should also minimize issues associated with noisy measurement.
- **Treatment Design:** There is some possibility that our experiment returned null results due to issues with the implementation of our research design. Survey enumerators reported that respondents often appeared bored and distracted during the videos, which ran for approximately two minutes. While we were able to verify that enumerators were not skipping the videos, it may be that the length of the treatment, or the context in which it was delivered, actually inhibited the message.
- **Spillover:** It is not plausible that spillover from the treatment to the control group could explain our results. Enumerators traveled to neighborhoods, towns, and villages across Jordan to administer the face-to-face survey. While the treatment was assigned at the respondent/-household level, it is very unlikely spillovers from one household to another are responsible for the null effects.

## Theoretical Explanations

- **Differential Effects on Outcomes:** Some of our outcomes appear to have been more affected by the treatments than others. While these results remain insignificant, it is noticeable that effects were generally positive for the attitudinal, policy, and behavioral outcomes, while they were negative or null for the impact outcomes across the three treatments. In hindsight, it is plausible that increasing generosity may not change individuals' assessments of how refugee waves have impacted their country, even if this generosity makes them more favorable toward refugees at the same time.
- **Difficulty of Changing Attitudes:** The treatments were designed to reflect theories about the factors that motivate generosity toward others. We believe it is important that our null findings should not be overinterpreted to call these theories into question, or to suggest that these factors cannot help to improve attitudes toward refugees. Rather, we interpret the results as evidence of the general difficulty of changing attitudes toward vulnerable outgroups, since individuals typically resist updating such attitudes (Tesler 2015). Given the small and insignificant but positive effects for **joy of giving** (and maybe for the other treatments on the outcome categories other than impact) it may be appropriate to consider the contexts in which similar interventions would produce relatively larger or smaller effects, rather than ruling out these approaches entirely.
- **Learning vs. Priming:** There are reasons Jordan may be a particularly difficult environment in which to improve attitudes toward refugees by priming generosity. For one, the

magnitude of the crisis suggests that most Jordanians are already thinking about its many dimensions with some frequency. To speculate, it may be more useful in such a context to design interventions that generate new information or new frames through which to understand the crisis, rather than priming respondents with information of which they may already be aware. Of all of our treatments, the video of Syrian refugees thanking Jordanians came closest to providing information that might be new to respondents, and likewise this treatment was only one that might be somewhat successful in shifting respondents' attitudes toward refugees.

- **Ceiling Effects:** As our findings from the control group show, Jordanians hold quite positive attitudes toward Syrian refugees, especially when considering the intensity of the refugee crisis and the extent to which Jordanian society has responded to the needs of refugees. Given these positive attitudes at baseline, it is possible that our interventions did not increase generosity in this context due to ceiling effects.

## 5 Implications and Next Steps

**Consistently Contradictory Findings:** The prejudice reduction literature, along with the subset of research focused on improving attitudes toward refugees and immigrants, is replete with contradictory effects from one-off interventions that evaluate different strategies for changing attitudes. As mentioned above, one general lesson this literature imparts is that prejudice reduction is difficult to accomplish. Our study supports this conclusion. Yet, plenty of research also suggests that attitudes can change, even if shifts are small. Some of our results are relatively consistent with this dynamic as well, even if effects are not statistically significant.

**Questionable Efficacy of Emotion-Based Interventions:** Another conclusion emphasized by the literature is that attempts to change attitudes by providing information about a minority or vulnerable group are less successful than strategies that seek to manipulate emotions (e.g. Adida et al. 2018). Our study, which sought to prime feelings of generosity, indicates that interventions rooted in emotions can also struggle to move attitudes in a meaningful way.

**Importance of Context:** Social science research testing the effectiveness of different prejudice reduction strategies has important implications for our theoretical understanding of outgroup attitudes and for the design of policies and interventions to counter it. However, the contradictory findings described above suggest that this literature should strive further to understand the contexts in which these strategies are likely to be effective. Yet, existing research has little to say on the conditions under which different strategies are more or less likely to work, and why.

**Need for Larger-Scale Studies:** Finally, if we accept that shifting attitudes is difficult but possible in small doses, we believe it is important to consider the fact that most social science experiments are done at a small scale. If our treatments had been implemented via repeated commercials on popular Jordanian television channels, and if they generated effect sizes similar to those observed in our face-to-face survey, would these still-tiny effects hold meaningful implications for attitudes toward Syrian refugees in the country? Perhaps so, and maybe even in a way that would justify expenses by the government and NGOs for such a campaign. In that sense, identifying substantively small effects on attitude change can still have important consequences for policies aimed at shifting public attitudes. A better understanding of these dynamics, however, would

require significantly scaling up the kinds of interventions social scientists conduct.

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## 6 Appendices for Improving Attitudes toward Refugees in Jordan

### 6.1 Appendix I: Sampling Process

We used two-stage clustering to sample respondents. In the first stage, we randomly sampled 150 blocks across all of Jordan's 12 governorates. In the second stage, we conducted a random walk procedure to select 8 houses within each block. In addition, as thirty blocks were among Jordan's top quantile of refugee density areas, we recruited an additional sample of 300 people in each of them. In each surveyed household, respondents were identified using the next-birthday method.

### 6.2 Appendix II: Full Treatments

- **Control:** Some people think that the Jordanian government should focus on meeting the needs of its own citizens before hosting Syrian refugees. Others think that Jordan should host and assist Syrian refugees.
- **Awareness of Needs:** Some people think that the Jordanian government should focus on meeting the needs of its own citizens before hosting Syrian refugees. Others think that Jordan should host and assist Syrian refugees because they are in desperate need: over half of Syria's population has been displaced from their homes, and of those remaining in Syria, nearly 70% face daily hunger. Tens of thousands of Syrians have been killed by barrel bombs alone since the start of the conflict. We would now like you to watch the following two-minute video clip that describes the struggles one Syrian family has faced.

– Link: <https://www.youtube.com/embed/8iq-QWTUOSA?>

- **Psychological Benefits - Joy of Giving:** Some people think that the Jordanian government should focus on meeting the needs of its own citizens before hosting Syrian refugees. Others think that Jordan should host and assist Syrian refugees. Many Syrians feel grateful to Jordanians for hosting and assisting them. As one example, here is a note from a Syrian child thanking Jordanians for their hospitality:

Thank you for hosting me in your beautiful country and without you I would not be living safely. Jordan is a beautiful flower that smells nicely with its perfume and beauty. I hope that Jordan remains safe and stable. And I thank the army soldiers that stay up late to ensure our safety.

We would now like you to watch the following two-minute video clip in which Syrian refugees express gratitude for Jordan's generosity in hosting them.

– Link: <https://www.youtube.com/embed/9cjkFN6dIWg?>

- **Values:** Some people think that the Jordanian government should focus on meeting the needs of its own citizens before hosting Syrian refugees. Some Islamic scholars, however, compare the plight of Syrian refugees today to the plight of Muhajireen during the time of the prophet, and think that Jordan should host and assist Syrian refugees because we have an Islamic duty toward our brothers and sisters from Syria. Prophet Mohammed himself was an immigrant who urged his companions to migrate to Habasha and then Medina to avoid

the oppression in Mecca. They fled without their money and property, in a situation similar to that of Syrian refugees today. As you may know, in Surat Al-Hashr and Surat Al-Anfal, God and his Prophet spoke highly of Ansar in Medina who received the Prophet and the Muhajireen (migrants) and welcomed them into their homes despite their difficult economic circumstances. Please listen to the following two-minute recording of the relevant verse from each Sura.

– Link: <https://www.youtube.com/embed/b1Hfy0067iQ>

### 6.3 Appendix III: Full List of Hypotheses

- H1** Needs: Respondents who receive the needs treatment will exhibit more positive and generous attitudes toward Syrian refugees.
- H2** Gratitude: Respondents who receive the gratitude treatment will exhibit more positive and generous attitudes toward Syrian refugees.
- H3** Values: Respondents who receive the values treatment will exhibit more positive and generous attitudes toward Syrian refugees.
- H4** Competition: Respondents who may be in competition with Syrians for jobs and state resources (below median income or unemployed) will exhibit less positive and generous attitudes as a function of the treatments than other Jordanians.
- H5** Religious Identity: Religious respondents who attend services more frequently (above the median) will exhibit more positive and generous attitudes as a function of religious treatment than other Jordanians.
- H6** Shared Experience: Respondents whose families came to Jordan as refugees (e.g. West Bank Jordanians defined by origin of family) will exhibit more positive attitudes as a function of the needs treatment than other Jordanians.
- H7** Education/Sophistication: Respondents with less education (below the median) and less knowledge of politics (below the median) will exhibit more positive and generous attitudes as a function of the treatments as a result of greater susceptibility to attitude shifts.
- H8** Social Contact - Positive: We will test whether respondents who report higher contact (above the median) with refugees or live in areas with higher refugee density (top 20 percent of sampled districts) exhibit more positive attitudes toward refugees as a function of the treatments due to their increased contact with refugees.
- H9** Social Contact - Negative: We will test whether respondents who report higher contact (above median) with refugees or live in areas with higher refugee density (top 20 percent of sampled districts) exhibit less positive attitudes toward refugees as a function of the treatments due to their increased contact with refugees.

## 6.4 Appendix IV: Outcome Questions

### 6.4.1 Attitudinal Outcomes

A.1 Jordan has obtained financial aid as a result of the Syrian refugee crisis. In your opinion, to what extent has this aid benefitted or harmed Jordanians?

- Greatly benefitted
- Benefitted
- Somewhat benefitted
- Neither benefitted nor harmed
- Somewhat harmed
- Harmed
- Greatly harmed
- Don't know (don't read)
- Refuse to answer (don't read)

A.2 To what extent do you support or oppose Jordan hosting and assisting Syrian refugees?

- Strongly support
- Support
- Somewhat support
- Neither support nor oppose
- Somewhat oppose
- Oppose
- Strongly oppose
- Don't know (don't read)
- Refuse to answer (don't read)

A.2 How do you feel about Syrian refugees in Jordan? A score between 50 to 100 means that you feel favorably toward them, while a score between 0 to 50 means that you feel unfavorably. Choose 50 if you do not feel particularly favorably or unfavorably. There is no right or wrong answer, as people have different opinions on this topic. Please be as honest as possible.

### 6.4.2 Impact outcomes

I.1 In your opinion, to what extent has the impact of Syrian refugees on Jordan been positive or negative?

- Very positive
- Positive
- Somewhat positive

- Neither positive nor negative
- Somewhat negative
- Negative
- Very negative
- Don't know (don't read)
- Refuse to answer (don't read)

I.2 We would now like to understand the impact, if any, you feel Syrian refugees have had on specific aspects of life in Jordan. Please indicate whether each of the following categories has been made better or worse by Syrian refugees coming to Jordan.

- (a) The economy
- (b) Cultural life
- (c) The housing market
- (d) Quality of public services (healthcare, transportation, water, etc.)
- (e) Quality of education
- (f) The level of crime
- (g) The threat of terrorism
- (h) Jordan's image abroad
- (i) The agricultural sector

For these questions, the respondents could answer with one of the following:

- Much better
- Better
- Somewhat better
- Neither better nor worse
- Somewhat worse
- Worse
- Much worse
- Don't know (don't read)
- Refuse to answer (don't read)

### 6.4.3 Policy Outcomes

We would now like to get a sense for how much you support or oppose implementing the following policy measures in Jordan. For each measure, please indicate your degree of support by selecting the appropriate circle.

P.1 Closing the border to all Syrian refugees

P.2 Quarantining all Syrian refugees in camps



P.3 Sending all Syrian refugees back to Syria

P.4 Providing all Syrian refugees with work permits

For these questions, the respondents could answer with one of the following:

- Strongly support
- Support
- Somewhat support
- Neither support nor oppose
- Somewhat oppose
- Oppose
- Strongly oppose
- Don't know (don't read)
- Refuse to answer (don't read)

#### **6.4.4 Behavioral Outcomes**

B.1 The Red Crescent facilitates volunteer opportunities for Jordanians who want to help Syrian refugees. Would you be interested in signing up to volunteer with the Red Crescent?

- Yes, I would be interested in signing up to volunteer with the Red Crescent to help Syrian refugees
- No, I would not be interested in signing up to volunteer with the Red Crescent to help Syrian refugees
- Don't know (don't read)
- Refuse to answer (don't read)

B.2 The UNHCR facilitates opportunities to donate some of your Zakat money to help Syrian refugees. Would you be interested in signing up to donate some of your Zakat money to the UNHCR?

- Yes, I would be interested in signing up to donate some of my Zakat money to help Syrian refugees through the UNHCR
- No, I would not be interested in signing up to donate some of my Zakat money to help Syrian refugees through the UNHCR
- Don't know (don't read)
- Refuse to answer (don't read)

### 6.4.5 Cronbach's Alpha

The outcomes had the following Cronbach Alpha values:

1. All outcomes: 0.87
2. Attitudinal outcomes: 0.63
3. Impact outcomes: 0.84
4. Policy outcomes: 0.76
5. Volunteer outcomes: 0.49

## 6.5 Appendix V: Heterogeneous Treatment Effects Effects

### 6.5.1 Competition

Figure C3: Coef plots showing the effect of the treatments by income group on the first principal component of all outcomes.

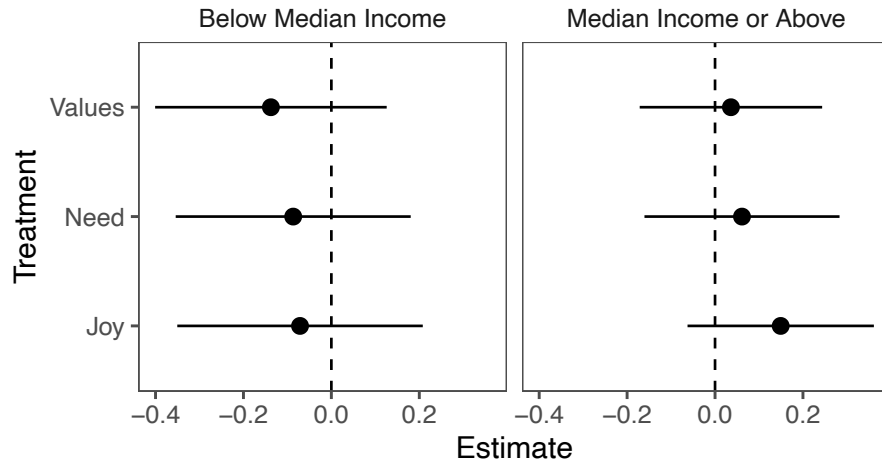
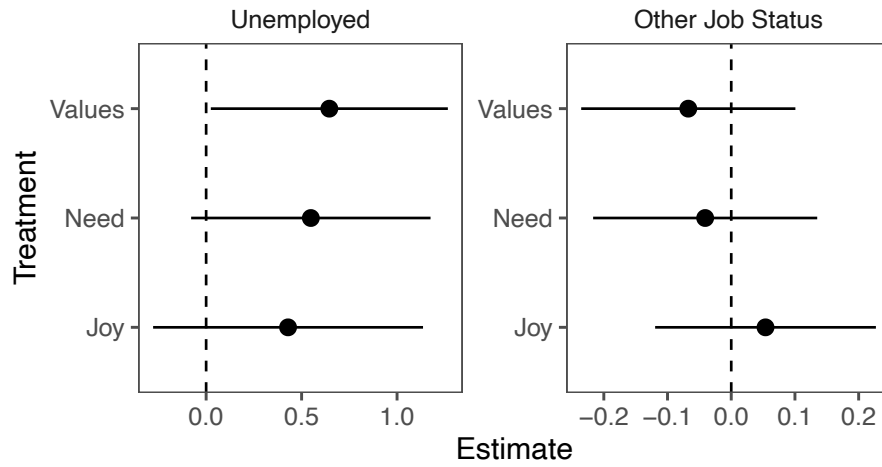
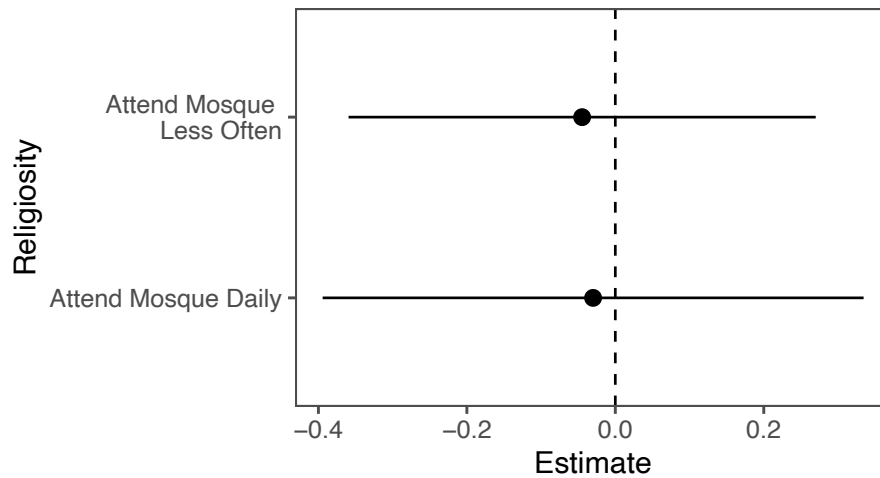


Figure C4: Coef plots showing the effect of the treatments by employment status on the first principal component of all outcomes.



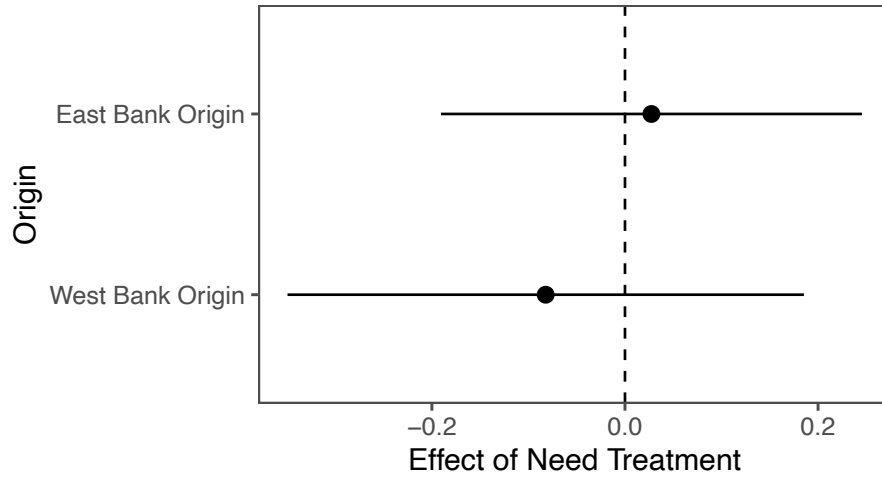
### 6.5.2 Religiosity

Figure C5: Coef plots showing the effect of the values treatment by religiosity status on the first principal component of all outcomes.



### 6.5.3 Shared experience

Figure C6: Coef plots showing the effect of the need treatment by the origin of respondents on the first principal component of all outcomes.



### 6.5.4 Education and Political Knowledge

Figure C7: Coef plots showing the effect of the treatments by education level on the first principal component of all outcomes.

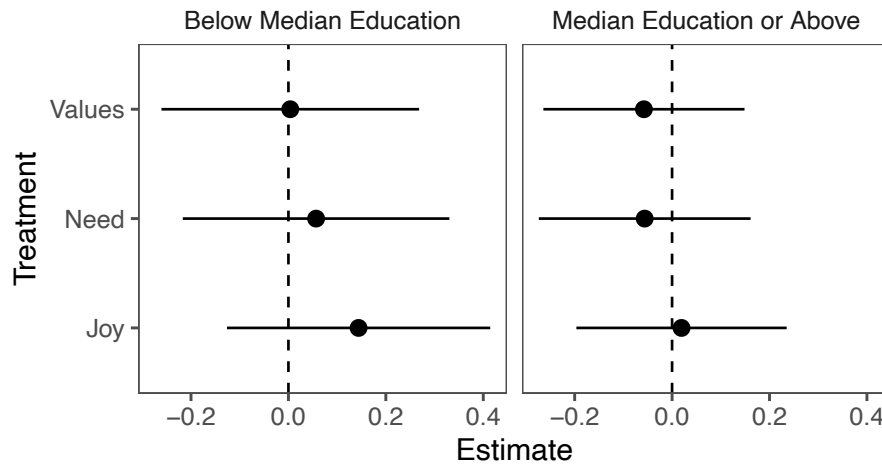
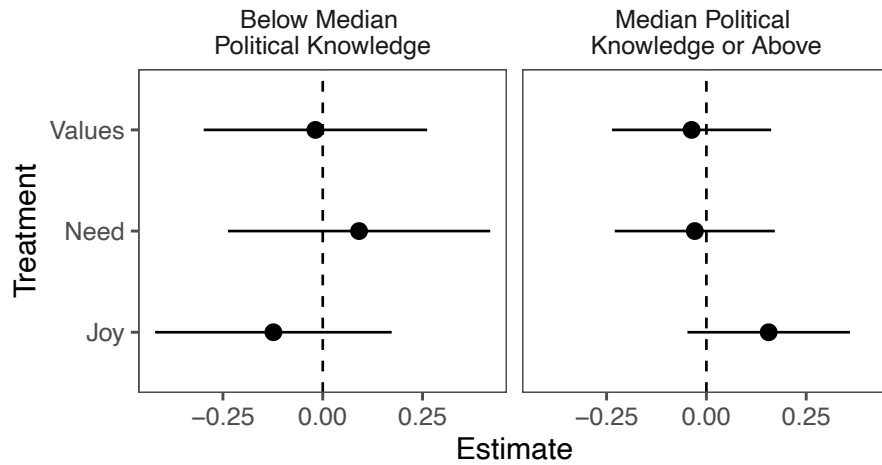


Figure C8: Coef plots showing the effect of the treatments by political knowledge level on the first principal component of all outcomes.



### 6.5.5 Social Contact with Syrians

Figure C9: Coef plots showing the effect of the treatments by self-reported contact with Syrian refugees on the first principal component of all outcomes.

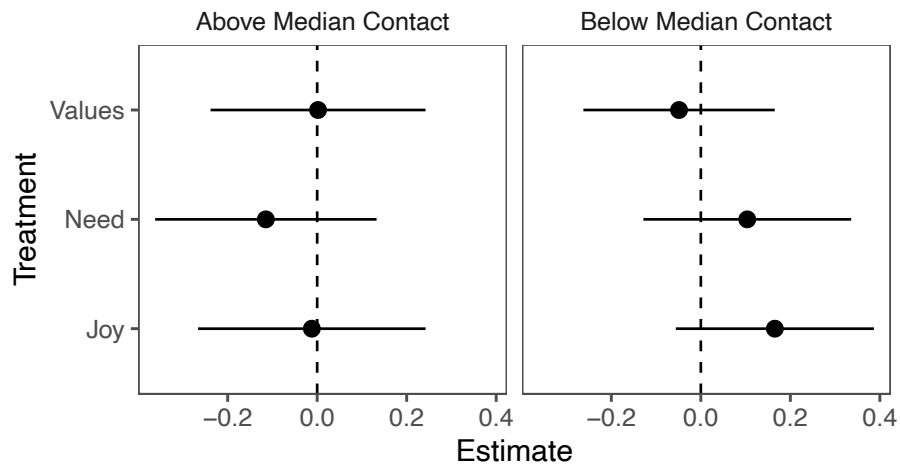
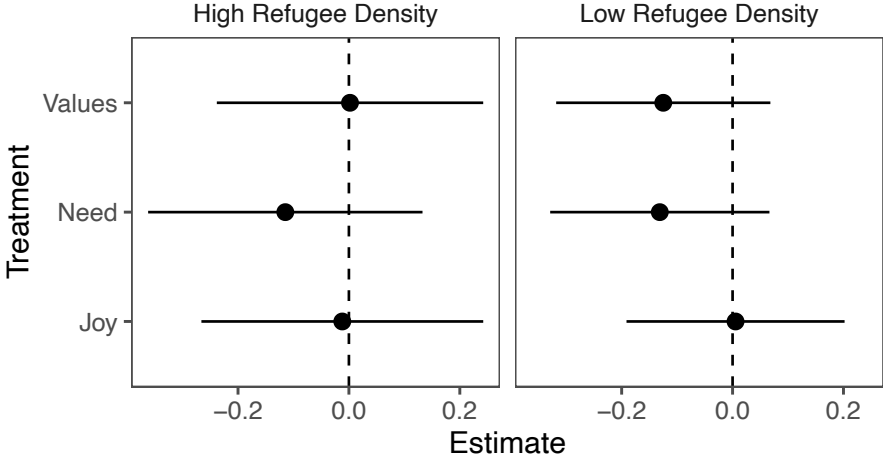
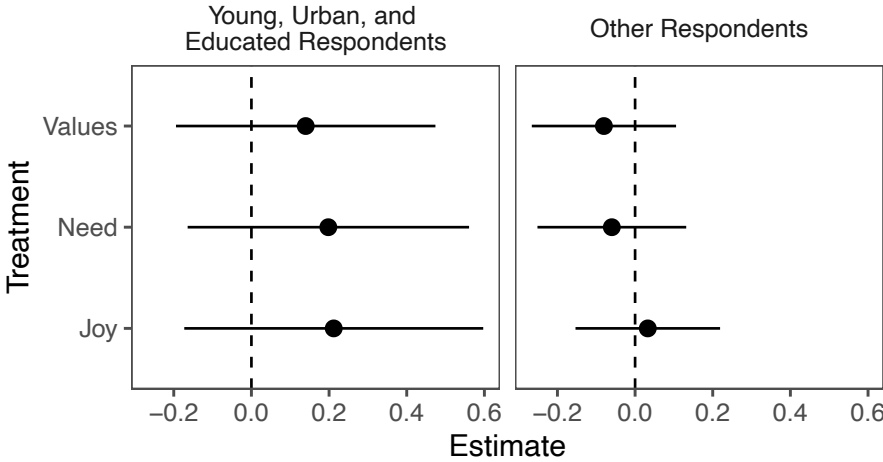


Figure C10: Coef plots showing the effect of the treatments by refugee density on the first principal component of all outcomes. The high density represents the top 20 percent of districts in terms of number of Syrian refugees



**6.5.6 Young, Urban, and Educated Respondents**

Figure C11: Coef plots showing the effect of the treatments by young, urban, and educated respondents on the first principal component of all outcomes.

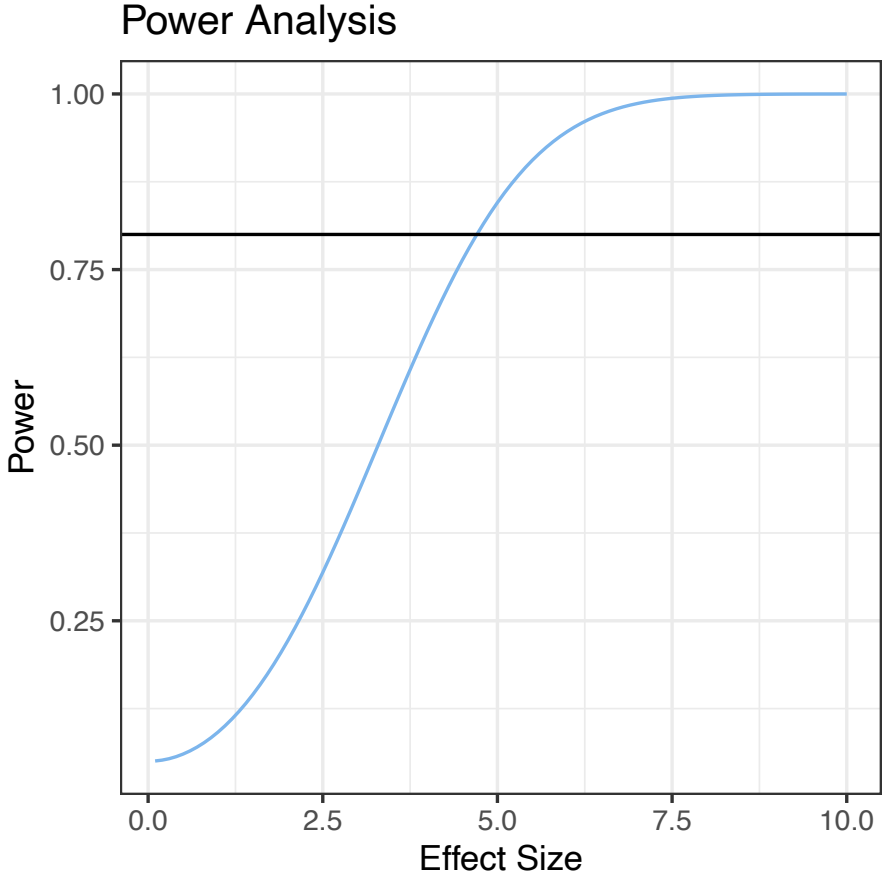


**6.6 Appendix VI: Power Analysis**

In an online pilot (using a convenience sample), we detected treatment effects across multiple outcomes. To conduct the power analysis, we used the feeling thermometer, for which the control group scored an average of 61 out of 100 and for which we detected a treatment effect slightly larger

than 5. Using the information from the pilot, we conducted the following power analysis.

Figure C12: Power analysis for the feeling thermometer outcome. In the pilot study, we detected a treatment effect larger than 5 points on the thermometer outcome. As this figure shows, this study was well powered to detect this effect size.



## **Appendix D**

Studies Used in Figure 1



# Studies Used in Figure 1

## 1 List of studies in AEA RCT Registry

This section presents the list of studies in AEA RCT registry that are related to nudges (included nudges in the description) and that have a start date prior to the end of 2018.

<b>Title</b>	<b>Year</b>
Using messaging-through-a-network to influence volunteering in the UK	2014
Evaluating Lottery Linked Savings Accounts Among the Poor in Mexico	2014
Zusha! A randomized behavior change road safety intervention	2014
Consumption and debt response to increased savings	2015
Nudging and Intrapreneurship	2020
Tablet-Based Financial Education	2015
The Welfare Effects of Behavioral Interventions	2015
The Hidden Cost of Nudging: Experimental Evidence on Reminders and Unsubscribing	2015
WeCanManage	2015
Nudges in Exercise Commitment Contracts	2015
Using Behavioral Insights for Parental Engagement	2015
Behavioral insights and tax compliance: Experimental evidence from Belgium	2015
Madagascar Human Development Cash Transfer Impact Evaluation	2016
Gender Discrimination in Economic Decision-making	2016
How Information Affects Parents' Choice of Schools: A Factorial Experiment	2018
Consequences of Media Censorship	2016
Precision Agriculture using ICT	2016
Universal Basic Income in Kenya	2020
Motivation and monitoring in modern higher education	2017
Reading the Fine Print: Credit Demand and Information Disclosure in Brazil	2017
Commitment at the gym: Can self-funded commitment contracts curb procrastination	2017
Enhancing Organ Donor Registration Rates	2018
Procrastination and Property Tax Compliance in Philadelphia	2017
Using Behavioral Economics to increase charitable giving through phone calls	2017
The Welfare Effects of Food Choice Nudges: Theory and Field Experimental Evidence	2017
Does providing information on government spending alter taxpayer beliefs and compliance?	2018
When nudge comes to shove - soft vs hard incentives for tax compliance	2019
Low-Cost Behavioral Nudges Increase Medicaid Take-Up Among Eligible Residents Of Oregon	2017
EXIT Estiu 2015: Evaluating the effect of texting parents to improve educational results	2017
Behavioral Nudges, Information and Incentives for HIV Testing	2017
Skills and Savings to Succeed	2017
Water Scarcity and Specificity of Saving Tips	2017
Encouraging Protest Participation	2018
Money vs. Pearls of Wisdom: personalising incentives for behavioral change	2018
An impact assessment of EAMDA's banana initiative	2018
Nudging taxpayers to pay taxes on time: Evidence from a field experiment	2018
The making of moral repugnance	2019
Professor will this be on the Exam?	2018
The Welfare Effects of Persuasion and Taxation	2020
Spillovers from Behavioral Interventions	2018
Self-Timed Credit Contracts (STICC)	2018
Nudging new entrants to the tax system to comply	2018
Examining Mobile Banking User Adoption from A Behavioural Perspective	2019

<b>Title</b>	<b>Year</b>
Nudges from outside to help the nudges from inside?	2018
Increasing the Take-Up of Cal Grant Awards Through Improved Notification Letters	2018
Selective Attention in Consumer Finance	2018
Nudge Intervention for College Scholarship	2018
Incidence of an Emotional Tax: The Case of Calorie Menu Labeling	2018
Impact of nutrition rating system on food selection in client-choice food pantries	2018
Low Take-up of Social Benefits and Financial Inclusion	2018
‘Street Cred: an empirical study of anchoring in credit card repayment settings’	2018
Using a SMART Design to Develop a Personalized Literacy Intervention	2019
Pocket Aces: Commitment and Incentives Against Smartphones for Students	2019
Bangladesh Chars Tobacco Assessment Project (CTAP) 2018	2019
Are Parenting Interventions Transferable Across Settings?	2019
Using a Behaviorally-Informed Calendar to Increase the Uptake of Vaccination	2019
The impact of a “carry-around” soft commitment device on savings and temptation spending	2019
Evaluating the Impact of Social Norm Enforcement	2019
Playful Learning	2019
Cash for Work, Behavioral Nudges, and Productive Activities	2019
Testing Behaviorally Designed Interventions to Increase Access and Use of Mobile Money	2019
Nudging organizations: evidence from three large-scale field experiments	2019
Saving water at Cape Town schools by using smart metering and behavioural change	2019
Norm nudges, participation and effort	2019
The Impact of a Personal Development Plan Program on Learning outcomes	2019
The Effect of Behavioral Interventions on Enrollment and Adverse Selection	2020
Overcoming Constraints to Female Labor Force Entry	2020
Encouraging low-carbon Food Consumption using Collaborative Game-Mechanisms	2020
Incentives to Persevere	2020
Conned by a Cashback?	2020
Nudging Timely Wage Reporting	2020
Vaccines at Work	2020
Having skin in the energy game	2020
Nudging Take-up of the Earned Income Tax Credit in California	2020

## 2 List of working papers and published studies

This section presents any working papers or publications based on the registered studies presented in Section 1.

<b>Title</b>	<b>Year</b>
The Long-Term Effects of Temporary Incentives to Save	2018
Results of a large-scale randomized behavior change intervention on road safety in Kenya	2015
Increasing Quantity Without Compromising Quality	2019
Freeing Financial Education via Tablets	2019
The Welfare Effects of Nudges: A Case Study of Energy Use Social Comparisons	2019
The hidden costs of nudging: Experimental evidence from reminders in fundraising	2018
Nudges in Exercise Commitment Contracts: A Randomized Trial	2015
Using behavioral insights to increase parental engagement the parents and children together intervention	2019
How to improve tax compliance? Evidence from population-wide experiments in Belgium	2020
Stereotypes of Prejudice: Behavioral Evidence of Gender Discrimination from Rural India	2017
The choice architecture of school choice websites	2020
The Impact of Media Censorship: 1984 or Brave New World?	2019
Effects of a Universal Basic Income during the pandemic	2020
Reading the Fine Print: Information Disclosure in the Brazilian Credit Card Market	2016
An Experimental Evaluation of Notification Strategies to Increase Property Tax Compliance	2016
Low-cost behavioral nudges increase Medicaid take-up among eligible residents of Oregon	2017
Imperfect Procedural Knowledge: Evidence from a Field Experiment to Encourage Water Conservation	2020
The Unexpected Downside of Paying or Sending Messages to People to Make Them Walk	2020
The Welfare Effects of Persuasion and Taxation: Theory and Evidence from the Field	2020
Spillovers from Behavioral Interventions: Experimental Evidence from Water and Energy Use	2017
Using SMART to develop an adaptive K-2 literacy intervention	2019
Increasing the Take up of Cal Grants	2018
Side Effects of Nudging: Evidence from a Randomized Intervention in the Credit Card Market	2020
Incidence of an Emotional Tax: The Case of Calorie Menu Labeling	2018
Bangladesh Chars Tobacco Assessment Project (CTAP) 2018: a data note	2018
Boosting Peer Influence by Gamification?	2020
Commitment or concealment? Impacts and use of a portable saving device	2020
Nudging Organizations: Evidence from three large-scale field experiments	n.d.
Saving Water at Cape Town Schools by Using Smart Metering and Behavioural Change	2019
Incentives to Persevere	2019
Conned by a cashback? Disclosure, nudges and consumer rationality in mortgage choice	2018
Nudging Timely Wage Reporting	2020
Vaccines at Work	2020
Having Skin in the Energy Game: The Impact of Social Norms on Energy Regime Changes	2018

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