An Experiment on the Supply of Higher Education Services

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1. Overview

This experiment studies the supply side of public investment in higher education. We randomize information provided to elected officials about their constituents' demand for higher education services. We then measure both the elected officials engagement with this information and their implemented policies.

The information about constituents' demands comes from previous experiments where participants wrote letters to the elected officials of their choosing: the governor, and state Senate and state House majority/minority leaders.[1] Our information provision includes summary statistics from the experiment on demand for public investment in higher education, three highlighted positive letters from their constituents who requested we send the letter to that elected official, and the remaining letters requested to be sent. In our email, we will ask the elected officials to consider a resolution to not decrease public investment for higher education, and ask if they would like to meet with us to discuss more.

We randomize each elected official in our sample into one of three conditions: (1) con-

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[1]The previous experiments and pre-analysis plans were titled: "Do Perceptions of Public Good Quality Affect Support? Evidence from Higher Education Appropriations" and "Government Spending perceptions and Support: Evidence from Higher Education Appropriations."

trol; (2) contact only; and (3) treated. The treated elected officials (in group 3) receive the information provision described above, including the constituent letters, in April 2023. The contact only group receives an email from the researchers that does not include any information about constituent demand, also in April 2023. The control group does not receive an email in April 2023. We randomize half of the elected official into group (3) and then 25 percent each into (1) and (2). Since our main interest is in how elected officials respond to constituent demand, we will pool (1) and (2) for some of the analyses. In early 2024, elected officials in (1) and (2) will receive the treated email. We therefore randomize the timing of the contact.[2]

2. Sample

Our starting sample consists of 248 elected officials in the US, based on the set of elected officials to whom constituents wrote letters in our earlier experiment. This is five elected officials per state, except Nebraska where there is a unicameral legislature and therefore only three. We drop both North Dakota and South Dakota because we had an insufficient number of letters written by residents of these states.[3]

2.1 Recruitment

We will directly email each elected official, when possible. Most officials, especially state representatives, have an official government email address listed on the state Senate or House's website. If an elected official does not have a publicly listed email address, we will contact them through the "contact" page on their website.[4]

Our design randomizes the timing of contact, and the content of the initial contact. We

[2]As part of the experimental design from the previous experiments, we must send out all of the constituent letters.

[3]More specifically, our treatment includes three highlighted positive letters about support for public investment in higher education. In these two states, no elected official had three such letters.

[4]Many contact pages allow us to attached a document, which will allow us to effectively treat those elected officials. If we are unable to directly email or attach a document through the contact form, we will request through the contact form an email to forward the materials.

plan to initially contact treated elected officials in April 2023, and then contact the elected officials in our two control conditions in January 2024.

3. Randomization

We randomize state-parties into each of our three conditions so that each elected official in a given state of the same political party receives the same treatment status. Our randomization procedure has two parts. First, we implement matched pairs within state so that each state has one party (Republicans or Democrats) in group (3) and one party in our combined control group of (1) or (2). We then randomize half of the combined control to (1) and half to (2).

We test for balance along several dimensions. We first compare our randomized conditions on state-party level variables. We then compare Democratic elected officials on state level variables.[5] Additionally, our matched pairs design ensure that we are balanced across treatment and pooled control on state level variables (for example the level of state spending).

Balance tests conducted:

- State-party level variables: governor is in treated party; number of letters sent to state-party; average sentiment from text analysis of letters sent to state-party; number of elected officials with fewer than three positive letters (that are used as our highlighted letters in the information provision)

- State level variables: governor is Republican; number (and percent) of Republican seats in state Senate; number (and percent) of Republican seats in state House; state appropriations per student (level and rank); graduation rate (level and rank)

We use rerandomization to ensure that there are no differences between treatment and control, using 2000 sets of random number draws. For each set of draws, we calculate the p-values associated with each test for differences between all three groups, as well as between

[5]Our design implies that the p-values for Republicans are identical to Democrats for these tests.

group (3) and pooled (1) and (2), and store the minimum p-value. We use the allocation associated with the largest minimum p-value.

4. Data Collection

We use Mailmetor to send out emails. Emails include tracking pixels to determine which emails are opened, and customized trackable links from `tinyurl.com` to determine which representatives clicked on links to see and download comments and the resolution.

Several elected officials, mainly governors, do not publicly list their email address. In those cases, we instead contact them through their websites. For these elected officials, we will not be able to see whether the emails are opened, but we will be able to see whether they clicked on our trackable links.

5. Outcomes and Analysis

We will conduct three types of analyses. First we will estimate treatment effects on elected-official level and state-party level outcomes for groups (3) and (2), separately, relative to (1) as well as group (3) relative to pooled (1) and (2). We will also estimate heterogeneous treatment effects by party for these outcomes. Second, we will estimate treatment effects on state-level outcomes for Democrat compared to Republican treated states. Both of these analyses will be based on data from the April 2023 point of contact. Finally, after all elected officials have received the letters for constituents, we will compare elected-official level outcomes between Democrats and Republicans (which does not use randomization).

1. Elected official level outcomes:

    - Clicks on list of letters from constituents in email
    - Clicks on resolution text in email
    - Responds to email

- Requests meeting

2. State-party level outcomes:

    - Member of state-party introduces resolution (or similar higher education resolution/bill, e.g. tuition freeze)

3. State level outcomes:

    - State appropriations (per student)

Since randomization is conducted at the state-party level, we will cluster our standard errors at that level.