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Do Elections Encourage Public Actors To Be More Responsive?

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Abstract

In the U.S. many public services are provided by individuals who are selected in local elections. We ask whether elections encourage public actors to be responsive to citizens. We design a novel field experiment where we send an information request to a random sample of prosecutor offices. Whether someone replies to the request is our measurement of responsiveness. We show that offices whose head is up for re-election are more likely to respond. We also show that offices in states that appoint their local prosecutors are substantially less likely to respond than a matched set of offices with elected leadership.

Keywords: election; email; field experiment; prosecutor; responsiveness; survey

JEL Codes: K4; H1; C93

1 Introduction

In the United States local officials provide important public services. The prosecution of crime is a noteworthy example. A substantial amount of public resources are devoted to the criminal justice system. With the goal of providing ‘justice’, the decisions made affect the deterrence of crime, the size of the prison population, and the community’s public finances. Local prosecutors in the United States make the decision on how to handle criminal cases.

The United States is unique in the world with its use of popular elections to select and retain public actors who provide public services, such as prosecutors. Prosecutors have the discretion on whether to file or dismiss criminal charges, which to file, and how many to file. They make resource allocation decisions choosing investigative effort and the number of staff hours to devote to cases. Further, prosecutors choose whether to plea bargain cases or proceed to trial. These are just some of the discretionary decisions they make. The primary check used to hold prosecutors accountable for how they exercise their enormous discretion is the election mechanism (Wright, 2014). Most prosecutors in the U.S. are elected in popular elections held at

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the county (or district) level. The insufficiency of the election mechanism has led critics to argue that there is an accountability deficit (Miller and Wright, 2010).

Criticisms to the election mechanism come in numerous forms. To be effective it requires voters to be actively acquiring information on prosecutor office decisions and be able to expertly evaluate the appropriateness of the choices. Voters are not only poorly informed, but they face a free riding problem as becoming informed is a public good (Tullock, 1971). Voters typically lack the human capital necessary to properly evaluate choices made as well. Further, incumbent prosecutors are infrequently contested in their re-elections, so even if the incentive, information, and knowledge deficiencies were solved, voters would often be unable to express their preferences with their votes (Wright, 2009).

Nevertheless, recent empirical evidence strongly suggests that prosecutors are responding *as if* voters are actively evaluating their choices. Studies take advantage of voter myopia where incumbents are held accountable for decisions made closer to the election season. Temporal variation in outcomes has been shown to be correlated with electoral cycles. For example, Dyke (2007) shows that case dismissals coincide with prosecutor elections. Bandyopadhyay and McCannon (2014) show that the rate at which cases are taken to trial increases in the year a prosecutor runs for re-election, and that the rate is escalated when that re-election is contested. This observation is confirmed by Nadel, Scraggs, and Bales (2017) who document electoral cycles in incarceration flows. McCannon (2013) compares appealed criminal convictions where the initial conviction occurred in the months before a prosecutor's re-election to those not near an election and shows that the former are more likely to be either reversed or modified years later by an appellate court. McCannon and Wilson (2019) focus on a high profile wrongful accusation involving Duke University's lacrosse team. They provide evidence that the willingness to prosecute accused sex offenders reduces specifically in the election season across the state after the scandal.¹ All of these results suggest that elections adjust the prosecutors' incentives.

It is reasonable to draw the normative implication that the election mechanism is suboptimal. Here, we ask whether there are benefits to the election mechanism that support its use over an alternative that uses appointments (such as what is used at the Federal level or in other Western democracies). Our intuition is that an important benefit is that the mechanism encourages prosecutors to be responsive to the district's residents whom they serve. Our objective is to test this with a field experiment. Do elections encourage prosecutors to be more responsive?

To answer this we design a novel field experiment. We randomly sample over 400 prosecutor offices in the country and ask them, through an email solicitation, to complete an online survey. The survey draws questions from the last *Census of State Prosecutors* conducted in 2007 by the U.S. Department of Justice inquiring about staffing levels and funds available. Our primary outcome variable, though, is whether anyone from the office bothers to respond to the information request. We take this as our measure of responsiveness.

¹In a cross-sectional analysis, Rasumussen, Raghav, and Ramseyer (2009) provide evidence that elected prosecutors have higher conviction rates. Their correlational analysis suggests that there may be a discrepancy between elected and appointed prosecutors even outside of the election season.

We investigate the effect of the election mechanism in two ways. First, we randomly sample prosecutor offices in the U.S. in the 45 states that use popular elections. We identify which offices are led by a prosecutor who is up for re-election in 2020. As with the observational-data research, if re-elections incentivize responsiveness, then we hypothesize that offices with the head up for re-election are more likely to respond to the information request.

Second, we collect contact information for the offices in the states that appoint local prosecutors² and use Propensity Score Matching to build a sample of counties that most closely match the counties in these states. Responsiveness of this (nonrandom but observationally-similar) control sample can be compared to the responsiveness of the offices led by appointed leaders.

For both measures we provide evidence that the election mechanism encourages prosecutors to be more responsive. First, those prosecutors up for re-election in 2020 are 52.2% more likely to respond than those who are not up for re-election this year (6.4 percentage points). Second, matched offices from states that use the election mechanism are 12.4 percentage points more likely to respond than offices in states that use appointments. Taken together, our results provide strong evidence that the election mechanism encourages responsiveness.

Our research design is novel, but shares features of other studies. Notably, our work is similar to audit studies that submit fictitious job applications and evaluate callbacks requesting an interview. Past work has looked for racial disparities (Bertrand and Mullainathan, 2004), consequences of prolonged unemployment duration (Kroft, Lang, and Notowidigdo, 2013; Farber, Silverman, and Wachter, 2017), the effect of eliminating questions on criminal records on employment applications (Agan and Starr, 2018), and age discrimination (Neumark, Burn, and Button, 2019; Carlsson and Eriksson, 2019). We too are interested in whether the subjects in our field experiment respond, but we do not study labor hiring decisions or engage in deception. Our work is in the same spirit as Berliner, Bagozzi, and Palmer-Rubin (2018) who consider information requests made to the Mexican government analyzing the text of the requests, and Lowande (2019) who analyzes information requests from Congressional committees and staff to executive agencies. While these two consider historical requests not made by the researchers, field experiments more similar to ours have asked whether government representatives respond to freedom of information requests (Worthy, John, and Vannoni, 2017; Grimmelikhuijsen *et al.*, 2019), social welfare help in authoritarian regimes (Chen *et al.*, 2016), and how politicization of the bureaucracy matters (Wood and Lewis, 2017). A field experiment recording Swiss legislators who are sent information requests while running for office report high response rates for requests made by both constituents who reside within the district and from those outside the district (Giger, Lanz, de Vries, 2020). Similarly, Driscoll *et al.* (2018) consider Brazilian legislature candidates' responses to information requests before and after their election. We extend this work to providers of local public services and compare responsiveness to non-elected officials. Thus, our design may be most closely related to that of Chong *et al.* (2014) who sent letters to fictitious businesses at nonexistent addresses in

²We exclude Alaska from our analysis.

159 countries and return addresses in the U.S. Their outcome of interest is whether the letter was returned. Like us, they use this as a measure of government effectiveness, but we differ by using it as an evaluation of the election mechanism. Emails sent to legislators asking for help registering to vote are less likely to be responded to if the sender has a putatively black name Butler and Brockman (2011). Finally, Charasz and Vogler (2020) evaluate responses to emails sent to Polish municipal governments posing as a nonprofit organization promoting foreign investment. Like us, they are interested in the efficacy of local government service provision, but they evaluate the role that EU funding has in building state capacity.

Our work also connects to an expansive literature on the decision to use elected officials or bureaucrats in numerous alternative public service contexts. This literature builds off of the idea that elections can discipline self-interested behavior out of politicians (Barro, 1973). Ferejohn (1986), for example, makes an early contribution by asking to what degree can the election mechanism encourage an officeholder’s efforts. Maskin and Tirole (2004) provide a valuable theoretical framework to identify when each accountability mechanism is best. In their framework, the election mechanism opens up the possibility of the public actor pandering to a poorly informed majority. This is traded off with a self-interested (i.e., “non-congruent”) bureaucrat. Alesina and Tabellini (2007; 2008) also provide a theoretical investigation into this institutional design question by thinking about the policymaking process being a combination of effort and skill of the public actor. Unelected bureaucrats are motivated to please their peers (i.e., career concerns), while elected officials are motivated to please voters. Thus, the optimal institution depends on the relative importance of the public actor’s skill versus the degree of redistribution involved. Besley and Coate (2003), considering regulation, point out that elections provide voters with the ability to express themselves on that sole issue whereas appointed regulators bundle the actions with other issues when holding policymakers accountable. Icarzower, Lewis, and Shum (2013) use a structural model to distinguish appointed and elected high court justices in their ability to acquire information and make good rulings. We contribute to this broad literature by also exploring how the election mechanism can incentivize effort and providing field-experiment data confirming its efficacy.³

In Section 2 we describe the methods employed including descriptions of the sample’s construction, the data collection protocols, and the survey designed. The test distinguishing response rates of offices whose head is up for re-election and those not is provided in Section 3. Section 4 investigates the second hypothesis that offices led by appointed prosecutors are less likely to respond. Section 5 presents the information provided by those who responded and compares it to historic data from a previous survey in 2007. Section 6 concludes.

³Bushway, Redlich, and Norris (2014) provide a field experiment also using real prosecutors as research subjects. They evaluate plea bargaining behavior.

2 Method

2.1 Construction of the Sample

To conduct the field experiment, we first obtained a list of every county in the United States. We excluded those counties from the five states that do not elect their prosecutors: Alaska, Connecticut, Delaware, New Jersey, Rhode Island. We then used a random number generator to create a 400 county sample. We identified the prosecutor office that serves each county. Quite a few states allow for consolidation of counties into a single prosecutor office. For example, in North Carolina there are 100 counties, but only 45 prosecutor offices. In these cases, the office that serves the selected county is included in the sample. Infrequently, two counties were selected that are part of the same prosecutorial district. In these cases, the first county selected is used and the second county is removed. A new county from the list is randomly selected to replace it in our sample. Thus, we have 400 counties with unique local prosecutor offices serving them from states that use popular elections to select and retain the office’s leader.

With this baseline sample, we then collect three basic pieces of information from each prosecutor office. First, we record the name of the officeholder. Second, we collect the email address posted for external contact. Third, we record the last year the office’s head ran for election and the year of the next election.

To gather this information, we first visit the web site associated with each office. Typically, each prosecutor office has a web site as either a stand-alone page or connected to the county’s web page. These sites typically provide contact information for the office. We pull the name and email address (when provided). Occasionally, the office site will also give information on the head prosecutor’s election cycle (often in a bio). To fill in missing election year information, we first conduct an internet keyword search using the office holder’s name, position name, and the search word ‘election’. Given extensive local newspaper coverage of county-level elections, this process almost always provides information on the last election. Terms of office are four years for the vast majority of states. Exceptions include Tennessee, who has the prosecutors serve eight year terms, Louisiana and Kentucky, where they have prosecutors serve six year terms, and New Hampshire, where prosecutors serve only two year terms. In the small handful of observations where there is still missing election information, the local Board of Elections web site is used to identify the last election for these observations. Also, in a few instances, the current head prosecutor is an interim appointment as the elected official has resigned. For example, the head prosecutor could have won a recent election to a judgeship, or be appointed by the Governor to such a position. In situations like this, the common mechanism is for there to be an interim appointment to lead the prosecutor office finishing out the current term. The existence of the appointment, the year it was made, and the year of the next election are all recorded.

Unfortunately for our study, not all prosecutor offices provide email contact information. When it is provided, typically there is a generic account setup, such as “prosecutor@...” or “info@...”. Alternatively, the name of the head prosecutor is used as the email address’s local-part. Infrequently, the head prosecutor does not provide his/her email address (and there is not a generic address for the office), but instead an

assistant prosecutor’s or office administrator’s email address is provided.⁴ We take as the contact information the highest ranking individual in the office. This process, though, does not result in complete coverage. After exhausting county and office web sites, we search statewide data bases of contact information for prosecutors. First, we visit state bar association websites. Many provide contact information for attorneys which include email addresses. Next, we visit the state prosecutor associations. After exhausting these sources, we are able to find contact information for 363 of the 400 counties selected (90.8%). These 363 counties make up our *Core* subsample.

To deal with the missing email information, we follow two distinct methods. First, every office with missing email information provides a contact telephone number. We called each missing observation requesting an email address. We explained that we are researchers conducting a survey of prosecutor offices and need an email address to send the survey to. The telephone calls were made in early January 2020. The 37 offices in which we made a telephone call become our *Called* subsample.

The benefit of this extra step is that it provides us full coverage in our data set. The shortcoming is that we may have primed the office to the value of the survey, which can be expected to increase the response rate. All telephone calls are made no less than one month before the survey is distributed to mitigate this effect. In the upcoming analysis, we will consider both the data set with telephoned observations, and the data set excluding them.

Our second approach is to select replacement counties. We follow the same procedures as was used to construct our initial sample. Contact information from these 43 additional counties are collected.⁵ We refer to this as our *Replacements* subsample. Combining the core, called, and replacement subsamples creates our *Full Sample*. In our upcoming analysis section, we will consider the results both with and without these added observations.

The benefit of this extra data collection is that we maintain the full 400 observation sample without adding the contamination of contacting the office before executing the field experiment. The shortcoming is that the exclusion of those not providing contact information is likely non-random. Less-populated, smaller offices are typically the ones that do not have full web sites developed. Importantly, those who do not provide contact information are likely those who are less responsive to the public. By using both methods, though, we will be able to assess our results’ sensitivity to the missing observations. In addition, adding both the telephoned offices and the replacement offices to the core group creates a larger sample size. The full sample, then, has 443 observations.

The appendix provides information on the number of offices in the sample for each state, basic information about the prosecutor offices, and a breakdown on the variation in email address collected. For the full sample, for example, 71% of the addresses use the prosecutors name, or an abbreviation/shortening of it, as the local-part of the email address. Also, 87% use an official office account in the domain of the email address. The

⁴In a small proportion of cases, an email address is not provided, but an online contact form is available. In these cases, we submit our information request on the contact form.

⁵We chose to include 43 counties in this extension because, along with our 37 called we had three where no contact information of any kind could be obtained and three had only Facebook sites.

rest use private addresses such as, for example, gmail (5.5%).

2.2 Data Collection

With this information in hand, we conduct our field experiment. We create an email informing the respondent that we are university-affiliated researchers conducting a survey-based study of prosecutor offices. We use the standard cover letter created by our university’s Institutional Review Board as our email solicitation. In it, the respondent is informed that a sample of questions from the Bureau of Justice Statistics’ 1997 *Census of State Prosecutors* are selected (discussed in the next subsection). This provides legitimacy to our survey and gives the added benefit of allowing us to assess changes in office resources over the past decade. The letter informs them that we are interested in studying the “staffing, functioning, and funding” of state prosecutor offices and that any employee of the office is eligible to complete the survey. The respondent is asked to click on a hyperlink provided in the email and fill out a survey that would take 3 to 5 minutes. Therefore, it is clear to the respondent that they are being asked questions that would inform academic work on the functioning of prosecutor offices. A reproduction of the emailed cover letter is provided in the appendix.

The subject line of the email states simply “information request”. We felt that this is the most appropriate because we do not want our email to be confused with the reporting of a crime or pertaining to a legal matter.

The university email address of the lead researcher is used in the correspondence. We choose this as we believe that the “.edu” ending would maintain the non-deceptive nature of our intervention. An important design dimension to our study is that we avoid deception. That is, we choose not to misrepresent ourselves as constituents in the prosecutor’s district. Instead, we choose the honest representation as researchers investigating prosecutor offices. The use of a university-affiliated email address, an email signature that includes contact information and the web site of the lead researcher, and a formal university cover letter document with IRB contact information gives the field experiment authenticity and reliability to the subjects. The limitation is that it is possible that prosecutors would respond to constituent’s information requests, but not academic researchers. If this is the case, then it would result in a level decrease in the response rate. Alternatively, one could imagine that office employees would be more likely to respond to an academic survey than a general email from someone in the community due to perceived importance (e.g., academic research could lead to press releases and, consequently, newspaper coverage, that reach more potential voters). There is no reason to believe, though, that there is a distinction between officials close to their re-election and officials farther from their election in the difference in their willingness to respond to a constituent versus an academic. Thus, this design decision should not bias our results.

We divide our sample randomly into four cohorts. Each cohort receives the information request on the first Monday of the month. Two weeks later, a reminder email is sent to those who did not respond to the first request. The initial cover letter is forwarded at the bottom of the email, and the email starts with a simple reminder message:

Two weeks ago, I reached out to you requesting your help in a data collection, research project on the functioning of prosecutor offices in the United States. Would you (or someone in your office) please click on the link below and complete the survey. It should take no more than 5 minutes. I would really appreciate it. Thank you.

The active link is provided. Our intention was to send the information requests across the four months of the 2020 primary season. Our pre-registered plan was to send the information request on the first Monday of the months of February, March, April, and May.⁶ The novel Coronavirus interrupted this plan. We did send out the March 2 information request. We felt, though, that it was inappropriate to conduct the experiment during the pandemic. It was our assessment that, since by late summer 2020 all state offices were open and functioning, re-starting our data collection in August and September was acceptable. Thus, our information request (with follow-up two weeks later) is made to approximately one-fourth of the sample each in the months of February, March, August, and September of 2020.

2.3 Survey

Our objective is to design a simple survey that would not be onerous to complete and take only a short amount of time. We focus on asking for factual information that does not require excessive time and labor to obtain. We decided to build a survey based on the 1997 *Census of State Prosecutors* effort. In the Census, the Department of Justice Statistics was able to build a comprehensive data set of every prosecutor office in the U.S. for that year. Basic information such as staffing levels, employee salary, budget, caseload, and case handling was asked. Our goal is not to create an opinion survey, but rather to collect the same information as obtained previously by the Department of Justice. The information in the survey will also allow us to assess the composition of the offices in our sample, how they have changed since 2007, and even identify characteristics of those who fail to respond to the survey. Data from the 2007 survey has been used in past academic research (Rasmusen, Raghav, and Ramseyer, 2009; Gorman and Ruggiero, 2009; Detotto and McCannon, 2017; 2020).

First, after asking respondents whether they agree to participate (creating a shortened consent form commonly used in laboratory experiments), we ask which state the office is located in and which counties it serves. The survey ends with a question asking the respondent to provide information on his/her role in the office (e.g., prosecutor, support staff, etc.).

We ask a sample of questions from the DOJ survey. They relate to the tenure of the head prosecutor, the office's staffing numbers, existence of funds in the budget for specific expenses (e.g., staff training), what type of forensic lab the office uses, and the existence of different types of threats against employees in the office. A reproduction of the full survey is provided in the appendix.

We use Qualtrics to create the survey. Along with the responses, it records the amount of time it took for the respondent to complete the survey and where the survey when completed. By asking for the state and counties the office covers, we can identify which offices completed the survey and which did not.

⁶The protocol was registered under the "AEARCTR-0005395". See <https://www.socialsciregistry.org/trials/5395>.

3 Are Prosecutors More Responsive in Their Re-election Year?

We establish our first main result by comparing the response rate of prosecutors up for re-election in 2020 to those up for re-election in later years.

3.1 Identification Concerns

Our identification strategy hinges on two key premises. First, while the selection of the offices in our sample was done randomly, we need to ensure that there is not selection into which offices have heads up for re-election in 2020. To investigate this, we conduct a randomness check. Second, our identification relies on responses to our academic study to correlate with responses to residents within the prosecutor’s jurisdiction. We devise a test of concept to address this concern as well.

First, we verify the treatment’s randomness. A random number generator was used to select the counties in the sample. The variable of interest is whether the head of the prosecutor office is up for re-election in 2020. If there is a systematic pattern to which offices hold elections in 2020, then omitted variable bias could taint the validity of our results.

To address this, we collect numerous county characteristic statistics for each county in our sample from the American Community Survey. This is the same data that we use in the matching method for our second hypothesis. Hence, details on the data are provided in Section 4.1.

We use 29 measurements as independent variables in a regression with an indicator variable equal to one if the office’s head is up for re-election in 2020 as the dependent variable. The results are provided in the appendix. For the vast majority of the variables, the coefficient is statistically insignificant. The only exceptions are those related to changes in the population’s composition (change in population, death rate, domestic migration) and gender. Importantly, features such as the educational attainment, income, racial distribution, political partisanship, labor market outcomes, and crime rates are all unrelated to the treatment variable. As a sensitivity check, we will add the statistically significant descriptors as controls to our main result to assess whether it is sensitive to the inclusion of these controls.

Second, we engage in a test of concept. We recruited a small pool of “research helpers” to send emails on our behalf to their local prosecutor offices.⁷ Each was instructed to not use an email address ending in “.edu”. The email’s subject line is “information request”. The text of the email is:

I am from [insert county name] County. I would like to know [insert question].
Thanks.
[insert helper’s name]

To select the question to ask, we visited FAQ pages on prosecutor web sites and selected four questions commonly found.

⁷Specifically, we use Ph.D. students in WVU’s Economics Department. While the student body is primarily made up of students from the U.S., there is fairly wide geographic dispersion. Overall, our research helpers contacted 28 distinct prosecutor offices.

- ... where is your office located
- ... what is your office’s jurisdiction
- ... where I can get a copy of a police report
- ... where I can get information on someone’s case

The first two are office-related questions. They are simple, factual questions that residents might want to know. The second two are specific information requests. They were chosen to avoid the perception that a crime is being reported and be common questions residents of a community may need to know. As stated, these questions are reported by actual prosecutor offices as questions commonly asked by the public. The questions are randomly allocated to our research helpers. The emails are sent out in October 2020 and our helpers informed us if they received a response.

We also follow our standard protocol and sent our survey to these same offices. Thus, this test of concept is a within-subject design. This is valuable as we cannot ensure a representative coverage of offices in this small sample. Our procedure selected one-half of the sample of counties to be contacted first by our research helpers, and the other half were initially contacted by us (Week 1). As with our primary study, two weeks later we followed up our contacted half with a reminder email (Week 3). Also in Week 3, offices first contacted by our research helpers are solicited by us with the survey. Again, we followed up two weeks later (Week 5). For those we initially contacted, our research helpers sent their information requests one week after our reminder email (Week 4).

Thus, we vary the order of the information requests and vary the content of those requests, without having two requests come in at the same time. Further, we use the true counties our research helpers are “from” to again avoid deception.

Using a within-subject design, this test of concept allows us to compare responses to our survey to replies to residents of a prosecutor’s jurisdiction. If the correlation between the two is high, then we gain confidence in our main study’s design, outcomes, and implications.

The response rate to our information request (21.4%) is in line with our main study, which will be reported on in Section 3.2, but is quite a bit lower than the response rate to our research helpers (53.6%). What is important, though, is whether responses correlate.

The pairwise correlation coefficient between responses is strong; $r = 0.49$ ($p < 0.01$). Further, partitioning the sample into those who replied to the residents and those who do not, the difference in response rates to our survey is statistically significant ($t = 2.84$, $p < 0.01$). In fact, every office who responds to our survey also responds to the email from the resident. Also, every office who neglects to respond to the resident also ignores our survey. Thus, even with a modest sample, our test of concept strongly suggests that measuring responsiveness through completing our survey is a valid proxy for responsiveness to the public.

3.2 Outcomes

Turning to the data collected, Table 1 provides basic summary information on the observations in our full sample.

Table 1: Outcomes

Up for re-election in 2020	37.5%
Respond to the information request	14.7%
Prosecutor’s office covers a single county	64.3%
Current prosecutor is an interim appointment	4.1%
Term ends in 2020 and is not seeking re-election	1.4%
Republican incumbent prosecutor	58.9%
Democrat incumbent prosecutor	28.2%
Non-partisan incumbent prosecutor	12.9%

Proportion of observations with a “yes” answer (i.e., indicator variable equal to one) provided. An office is coded as responding if at least 50% of the survey is completed. A prosecutor is coded as being in the 2020 election if the current incumbent is running for re-election in 2020. This excludes those whose terms expire in 2020 and choose not to run for re-election. The full sample, including the *Called* subsample and *Replacements* subsample, is considered; $N = 443$.

More than one-third of our randomly-selected sample of prosecutor offices in the U.S. have the head prosecutor up for re-election in 2020. Considering the distribution of years in which prosecutors’ terms end, there are approximately 30% more re-elections in 2022, but odd numbered years also see a small number of terms ending.

We require that a respondent must complete at least 50% of the survey to be coded as a response.⁸ Approximately 15% of our sample did so. Hence, response to our information request was moderate. Our objective, though, is to assess whether there is a pattern to the response rate.

A potentially important institutional feature of the prosecutor’s office is whether it is a district-wide office serving multiple counties, or an office serving a single county. Detotto and McCannon (2020) document important returns to scale gains from the consolidation. Our sample is rather divided with approximately two-thirds coming from offices serving a single county. A simple t -test indicates that the response rate does not differ by the office’s jurisdiction structure.

Finally, we see only a small number of the offices in our sample are being led by a prosecutor who is an interim appointment, or who has announced s/he will not seek re-election.⁹ Note that it is possible that those up for re-election in 2020 can include some of the interim appointments who are completing the term of the last-elected prosecutor who has left the post due to retirement, appointment or election into a new post (e.g., judge), or left due to scandal.¹⁰ In our upcoming analysis, we will remove those not seeking re-election from the pool of prosecutor’s whose term ends in 2020, and continue to include interim appointments who are seeking election in 2020.

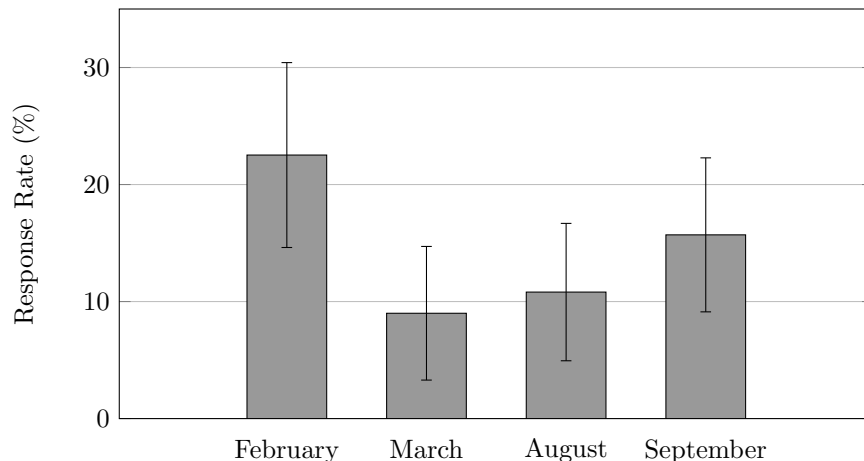
As discussed, we rolled out our survey across four months. Our original intention was to begin the data

⁸We also record those who reply to us separately without attempting the survey (through telephone calls or emails) and those who click on the survey link but close it without answering any questions. Including these, 18.5% of the full sample responds.

⁹Our information collection occurred during the winter of 2019-20. We re-identified the races in late October and early November of 2020 and found a few instances where retirements and other announcements that the incumbent will not seek re-election over the course of the year. Re-estimating our main result (presented in the upcoming Table 2) are unaffected by the updating of these two controls.

¹⁰Conducting a news search on the prosecutors in our sample who are not seeking election, we found each of these circumstances occurring.

Figure 1: Responsiveness Across the Months



The 95% confidence intervals depicted. An office is coded as responding if at least 50% of the survey is completed. A prosecutor is coded as being in the 2020 election if the current incumbent is running for re-election in 2020. This excludes those whose terms expire in 2020 and choose not to run for re-election. The full sample, including the *Called* subsample and the *Replacements* subsample, is considered, $N = 443$. This is broken down into $N = 111, 100, 111, 121$ for the four months.

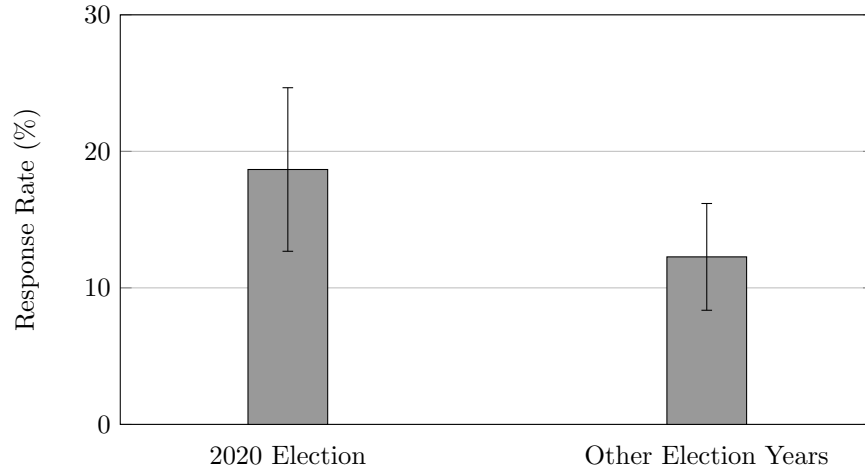
collection in February 2020 and continue it through the spring, as that is the active primary season for local offices. Unfortunately, for many reasons, the novel Coronavirus disrupted our data collection. As discussed previously, we paused the field experiment during the height of the pandemic, and restarted it once we felt public services had returned to a closer-to-normal level. Figure 1 depicts the response rate for each month of our study period.

Our design was to send out the initial emailed information requests the first Monday of the month, and then send out one reminder email two weeks later. Thus, we started the March cohort on March 2. Our reminder email on March 16 did not receive much attention. Thus, the response rate for this month is substantially lower. By the late summer (August and September) the response rates start to climb. It is reasonable to presume that this observed pattern arises because the office’s decision to respond is traded off with other demands on the staff’s time and attention. A silver lining to the pandemic and our lower response rate in March is that it provides further, suggestive evidence that these offices are engaging in a cost-benefit analysis. The benefit to being responsive to the public is being traded off with the costs of doing so. During the height of the pandemic, the costs were high and, thus, the response rate falls off. Our investigation uses a similar argument. We hypothesize that re-election pressures increase the benefits to replying, leading to more responsiveness than when the election pressures are absent.

3.3 Re-election and Responsiveness

Turning to our main question, Figure 2 partitions the full sample into those up for re-election in 2020 and those not. It depicts the response rate for each subsample, along with the 95% confidence intervals.

Figure 2: Re-election Pressure and Responsiveness



A two-tailed t -test generates $t = 1.85$ & $p = 0.066$. An office is coded as responding if at least 50% of the survey is completed. A prosecutor is coded as being in the 2020 election if the current incumbent is running for re-election in 2020. This excludes those whose terms expire in 2020 and choose not to run for re-election. The full sample, including the *Called* subsample and the *Replacements* subsamples, is considered, $N = 443$.

The response rate is 6.4 percentage points higher for the subsample of prosecutors who are up for re-election in 2020. This is a 52.2% increase in the response rate. Thus, we have an economically significant effect. Also, the t -statistic is 1.85. Thus, we have a statistically significant effect as well.

This finding is strengthened if the distinction we find for those up for re-election in 2020 cannot be found for those up for re-election in other years. Figure 3 provides the response rate for every other year.

Response rates are consistently smaller than the 18.7% level that was observed in Figure 2. Redoing the t -test for election differences, as was done for the 2020 election, the p -values all exceed 0.38. Thus, the response rate of those up for re-election in 2020 are distinct from any other cohort of prosecutor offices in our sample.

The finding presented in Figure 2 pools all months and all subsamples included in the field experiment. Also, as discussed, the response rate may be different for the subsample who was telephoned. In addition, month of the year seems to affect the response rate. It is appropriate, then, to check that the result continues to hold if these factors are included as control variables in the estimation of a linear probability model. In addition, we include important controls about the prosecutor, such as whether his/her office covers a single county, whether the prosecutor is an interim appointment or has chosen not to seek re-election in 2020, and his/her partisan affiliation. Table 2 presents the results with an indicator variable equal to one if the office completed the survey as the dependent variable.

The table provides the estimated coefficient and standard error for each independent variable. The standard errors are clustered at the state level. In addition, state fixed effects are included to control for any institutional, political, or cultural factors that may vary by state.¹¹

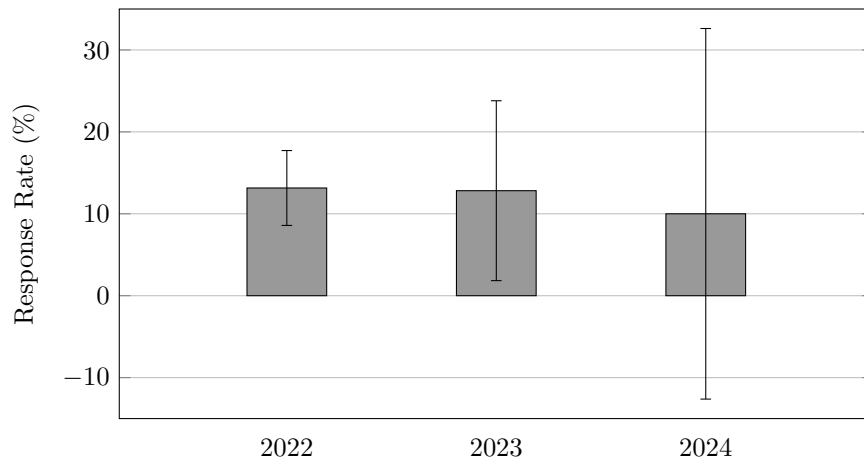
¹¹The statistical significance remains if we exclude state fixed effects or do not cluster our standard errors. The estimated

Table 2: Regression Results

	Full Sample	Full Sample	Without Called Subsample	Without Replacements Subsample	Without Either
	(1)	(2)	(3)	(4)	(5)
2020 Re-election	0.117 ** (0.045)	0.119 ** (0.052)	0.118 ** (0.053)	0.126 ** (0.059)	0.124 ** (0.060)
Called Sample		0.004 (0.058)		-0.006 (0.062)	
Replacements Sample		0.021 (0.063)	0.012 (0.065)		
Single County District		-0.001 (0.046)	0.026 (0.049)	-0.001 (0.054)	0.027 (0.059)
Interim Appointment		-0.042 (0.097)	-0.010 (0.130)	-0.008 (0.107)	0.044 (0.145)
Will Not Seek Re-election		-0.010 (0.072)	-0.009 (0.077)	-0.025 (0.074)	-0.024 (0.078)
Republican		0.017 (0.042)	0.018 (0.045)	0.039 (0.044)	0.045 (0.046)
Non-Partisan		-0.005 (0.064)	0.011 (0.069)	-0.035 (0.060)	-0.011 (0.066)
Month Fixed Effects?	Yes	Yes	Yes	Yes	Yes
State Fixed Effects?	Yes	Yes	Yes	Yes	Yes
R^2	0.126	0.127	0.130	0.144	0.148
AIC	285.9	299.3	272.0	253.1	226.5
N	440	440	390	397	347

Results from a linear probability model presented where the dependent variable is equal to one if the office responded to the survey (completing at least 50%). Standard errors clustered by state are presented in parentheses (45 clusters); *** 1%, ** 5%, and * 1% level of significance. Ultimately, for three offices even the telephone calls were unable to provide contact information, so these three observations are omitted.

Figure 3: Does the Effect Exist for Other Election Years?



The 95% confidence intervals depicted. Each column represents the response rate for prosecutors up for re-election in each year. The 2024 election year also includes those up for re-election in 2026. The 2021 election year is not included since there are only nine observations and none responded to the information request. An office is coded as responding if at least 50% of the survey is completed. The full sample, including the *Called* subsample and the *Replacements* subsample, is considered. $N = 217, 50, 10$ for each year, respectively. For those up for re-election in 2020, the mean response rate is 18.7%, which is greater than each other year presented. The two-tailed p -value from a t -test of the difference between each election year and offices without a re-election that year has $p = 0.38, 0.73, 0.67$ for the four, respectively.

While the first column re-establishes the observation in Figure 2, column (2) adds the controls. Importantly, the size of the coefficient is relatively unchanged and the statistical significance is maintained. Thus, the re-election effect is not sensitive to the timing of the request¹², jurisdictional composition¹³, partisan affiliation, or method of contact/subsample. Here, the marginal effect of being up for re-election is to increase the response rate by approximately 12 percentage points. This is substantially higher than the differences in the means observed in Figure 2 and indicates that controlling for state characteristics, within-state variation in electoral timing is quite important.

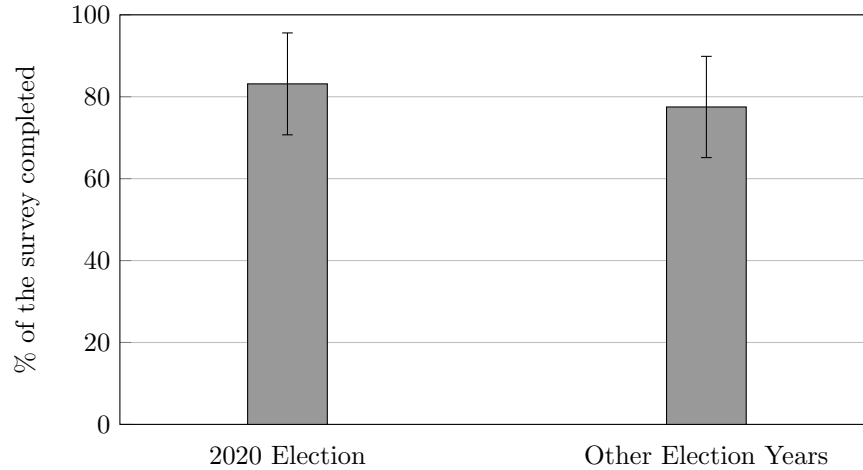
To further establish that the data collection method is not influencing the result, the last three columns sequentially drop subsamples. Column (3) drops those offices called on the telephone, due to the concern that we primed their response or their is a selection effect of who chooses not to post contact information online. Column (4) instead drops the subsample of replacements, which were not in our original randomly-selected sample of 400, but were chosen to replace the *Called* subsample. The final column drops both. The magnitude of the treatment effect is relatively unaffected when the subsamples are removed.

marginal effects of being up for re-election return to the level arising in the raw data.

¹²In fact, if observations contacted in the month of March are removed, the statistical significance slightly improves ($p = 0.022$) and the treatment effect grows ($\beta = 0.166$).

¹³The appendix compares those observations coming from prosecutor offices that cover only one county to those that cover a multi-county district. The confidence intervals overlap and the two-tailed, difference in means t -test has a p -value greater than 0.5.

Figure 4: Re-election Pressure and Completion of the Survey



A two-tailed t -test of the hypothesis that the response rate is higher for those up for re-election in 2020 generates $t = 0.70$ & $p = 0.49$. Here we include responses of even those who do not complete at least 50% of the survey. A prosecutor is coded as being in the 2020 election if the current incumbent is running for re-election in 2020. This excludes those whose terms expire in 2020 and choose not to run for re-election. The full sample, including the *Called* and *Replacements* subsample, is considered, $N = 82$.

3.4 Degree of Responsiveness

Our data collection method allows us to investigate other margins of activity. In this section, we consider data from those offices who responded for an analysis of the intensive margin. First, Qualtrics calculates the proportion of the survey that is completed, as some respondents stop the survey before the end. Figure 4 compares the mean completion rate for those up for re-election in 2020 and those in the sample who are not up for re-election. We also include those who responded to our request, but did not complete at least 50% of it.

Here, the mean completion proportion is 7.3% greater, but this difference is statistically insignificant ($p = 0.49$).

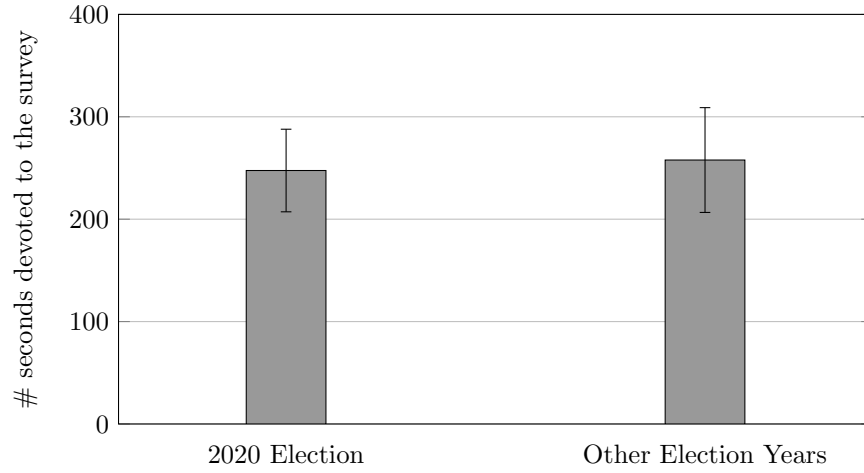
Another outcome variable we track is the amount of time, measured in seconds, that the respondent took to complete the survey. In Figure 5 we drop outliers who likely left the survey open on their internet browser. Eight observations had response times more than seven times the median. Also, we drop the ten observations from those who replied in some form, but completed 0% of the questions.

No meaningful distinction arises between the two cohorts. The difference is approximately 10 seconds, which is, of course, not statistically significant.

Finally, we know which day they responded and, thus, consider in Figure 6 the number of days between the initial information request and the response.

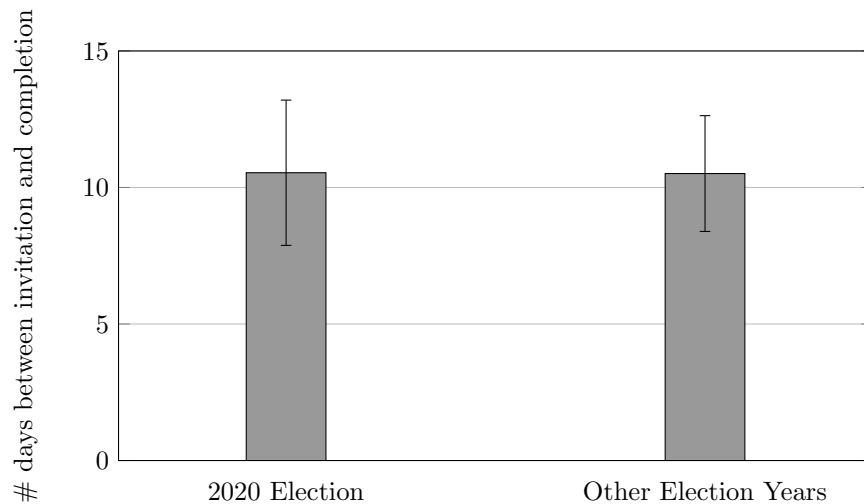
Again, no noticeable difference arises. Taken together, we do not find a difference in the “degree” of responsiveness by prosecutors currently running for re-election. Once the decision to reply and complete the survey was made, equivalent efforts were put into it.

Figure 5: Re-election Pressure and Time Spent on the Survey



A two-tailed t -test of the hypothesis that the response rate is higher for those up for re-election in 2020 generates $t = 0.31$ & $p = 0.76$. Here we include responses of even those who do not complete at least 50% of the survey. A prosecutor is coded as being in the 2020 election if the current incumbent is running for re-election in 2020. This excludes those whose terms expire in 2020 and choose not to run for re-election. The full sample, including the *Called* and *Replacements* subsamples, is considered. We drop the eight observations where the time spent is excessively high (more than seven times the median) and the ten observations where zero seconds were spent, $N = 63$.

Figure 6: Re-election Pressure and Completion Date



A two-tailed t -test of the hypothesis that the response rate is higher for those up for re-election in 2020 generates $t = 0.02$ & $p = 0.99$. Here we include responses of even those who do not complete at least 50% of the survey. A prosecutor is coded as being in the 2020 election if the current incumbent is running for re-election in 2020. This excludes those whose terms expire in 2020 and choose not to run for re-election. The full sample, including the *Called* and *Replacements* subsamples, is considered, $N = 82$.

3.5 Heterogeneous Effects

One can think of our results, up to this point, as establishing that there is an (average) “treatment” effect of an election on prosecutor office behavior. It is appropriate, then, to ask whether there are important heterogeneous treatment effects. Not all offices are likely equally sensitive to re-election concerns. In this section, we explore two possibilities.

First, we ask whether the partisan ideological divide matters. Our hypothesis is that prosecutors who are representing a divided population are more likely to be concerned about challenges in their re-elections than prosecutors who serve in a county that is dominated by one political party. Presumably, if a prosecutor serves in a county where the residents disproportionately support one political party, then general election contests do not pose much of a threat to the incumbent’s job security. As a consequence, the incentive created by the election mechanism should be primarily working for those who reasonably expect to be challenged in elections where the population is divided in their political affiliation.

To measure the partisan divide, we use voting data from the 2016 Presidential election. This is the last Presidential election before our data collection took place. The value of considering the Presidential election is that all voters had the same two candidates to choose from so that differences in measures of partisanship across the country are not driven by differences in candidate quality and policy platforms adopted. We define a county as being *Close* if Donald Trump’s share of the votes, relative to Hillary Clinton’s share of the votes, is between 40% and 60%. We do not include third-party votes. A county does not have a partisan divide if the vote share is outside this range. Such a county is *Not Close*. Of the 166 offices in the full sample whose leader is up for re-election in 2020, 29 (17.5%) ran in divided-partisanship districts.

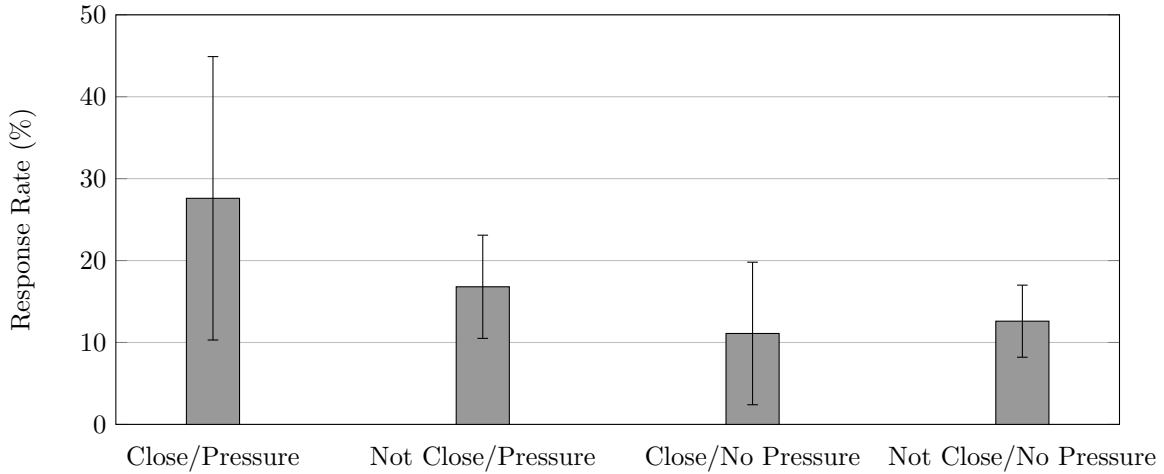
Figure 7 partitions our sample into four subsamples. We consider the response rate for counties with a divided electorate and the head is up for re-election (*Close/Pressure*), counties not divided with a head up for re-election (*Not Close/Pressure*), and counties without a head up for re-election being either divided or not (*Close/No Pressure* and *Not Close/No Pressure*). Figure 7 depicts the response rates for each.

What stands out is that the response rate for prosecutors in districts that have a partisan divide and are up for re-election in 2020 is substantially higher than the other groups ($t = 2.04$, $p = 0.042$). While prosecutors up for re-election in non-divided districts have a slightly higher response rate, it cannot be (statistically) distinguished from the others.

Considered differently, if we only consider offices whose head is running for re-election, a one-tailed t -test of the hypothesis that those from close districts have a higher response rate has $t = 1.35$ and $p = 0.089$. Similarly, if we consider only offices in divided partisanship districts, the hypothesis that offices whose head is up for re-election has a higher response rate generates $t = 1.93$ with $p = 0.029$. These findings provide further support for our hypothesis that it is the incentives created by having to gain resident support to keep one’s job that is motivating responsiveness.

As a second heterogeneous treatment check, we differentiate those prosecutors whose term is up in 2020 and are being challenged in their race, are uncontested in their re-election bid, or who have decided to not

Figure 7: Partisan Divide



An office is coded as responding if at least 50% of the survey is completed. A prosecutor is coded as being in the 2020 election if the current incumbent is running for re-election in 2020. This excludes, then, those whose terms end in 2020 and choose not to run for re-election. An office has a close partisan divide if the proportion of votes for Donald Trump, relative to Hillary Clinton, in the 2016 Presidential election is between 40% and 60%. The full sample, including the *Called* and *Replacements* subsamples, is considered, $N = 443$.

seek re-election. Our hypothesis is that the incentives of the election mechanism should be stronger if the candidate is being contested.

In late October and early November 2020 we collected election information for each head prosecutor in our data set whose current term expires in 2020. For each we identify whether they are contested or uncontested in the election.¹⁴ We viewed sample ballots posted by state/county Board of Elections and use local media sources to get full coverage of our sample.

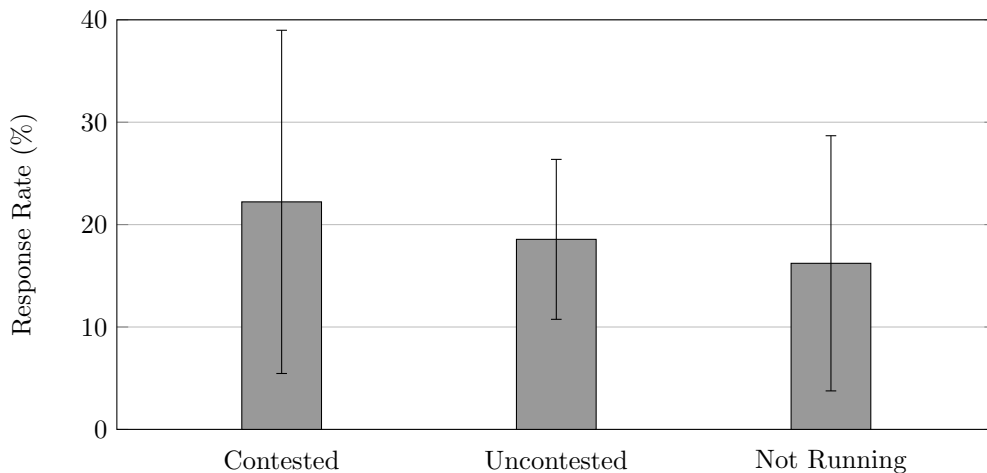
In our main analysis discussed previously, we controlled for prosecutors whose head’s term ends in 2020 but announced s/he is not seeking re-election. This data collection effort took place in the winter of 2019-20. In our sample ballot data dive in October 2020, we identified a small number of situations where prosecutors made late announcements that they were not seeking re-election. Therefore, our third “not running” category here includes all prosecutors who declined to seek another term in office.

Table 8 compares the response rates for these three cohorts. There are 27 prosecutors who were contested, 97 uncontested, and 37 who chose not to seek re-election.

There is a monotonic relationship between election pressure and response rate. Those prosecutors not running for re-election respond 16.2% of the time, which is not much different from the full sample mean of 14.7%. Prosecutors up for re-election but uncontested in their bid respond 18.6% of the time. Those who are contested in their re-election bid have a response rate of 22.2%. Relative to the full sample mean, this is a 51.1% increase in the response rate. Given that the number of observations within each cohort is modest, the confidence intervals are wide and the differences are not statistically significant. Further,

¹⁴We include a prosecutor in the contested category if s/he was challenged in the primary election as well.

Figure 8: Contested vs. Uncontested Races



An office is coded as responding if at least 50% of the survey is completed. The full sample, including the *Called* and *Replacements* subsamples, is considered, $N = 161$.

the distinction does not account for the endogeneity of who is challenged. Nevertheless, it does provide additional, somewhat-suggestive evidence that the election mechanism, and concerns over keeping one’s job, is influencing how the office interacts with the public.

As a final point, after the November 2020 elections we track who won these contested elections. Within our sample, 72.7% win. Interestingly, 87.5% of those who responded to our information request win, while only 68.0% of those who did not respond to us are victorious. While the sample is small, making it difficult to make statistical claims, the correlation coefficient between the two is almost 0.20.¹⁵

4 Are Elected Prosecutors More Responsive Than Those Appointed?

Our second measurement of the election mechanism’s effect is to compare those offices in states that do not elect their head prosecutors to “similar” offices in the country that do have popular elections. We hypothesize that offices whose leadership must ever run for re-election will be more responsive than offices whose heads are appointed by state officials. Past research has shown judges’ behavior differs by retention mechanism. For example, Gordon and Huber (2007) considers judges in Kansas where in some parts of the state they are elected while in others they are appointed. They show that elected judges sentence more severely. Therefore, it is reasonable to presume that for those prosecutors who are appointed, they too will be affected by the difference in their selection process.¹⁶ As argued by Besley and Coate (2008), when numerous public policy

¹⁵Here $\rho = 0.188$ with $p = 0.296$. A one-tailed t -test of the hypothesis that those who respond have greater victory rates has $t = 1.06$ with $p = 0.148$.

¹⁶Gordon and Huber (2002) provide a theoretical model of prosecutor behavior under the presumption that electoral oversight can affect prosecutors’ incentives to exert effort. One can think of our work as quantifying effort through responses to information

issues are “bundled” into one office, it is unlikely that voters can hold this higher officeholder accountable for all outcomes. Thus, if elected state Attorney Generals oversee local criminal justice as well, the decisions of the prosecutor serving a community are unlikely to be fully salient in the Attorney General’s election.

To do this, we collect the same information as before for those offices in the states of Connecticut, Delaware, New Jersey, and Rhode Island.¹⁷ We also created a sample of “matched” counties to use as our comparison group. In the next subsection we describe the method used to create this control group.

4.1 Matching Procedure

We use Propensity Score Matching to select the set of counties that most closely match those under an appointment system. To do so, we first collect basic socio-economic data on every county in the U.S. from the U.S. Census Bureau’s American Community Survey. Ultimately, we use 29 variables to identify the measurable characteristics of each county.

Regarding demographics, we collect the population, change in population, number of deaths, international and domestic migration, and the gender distribution of each county. For race, we track the proportion of the county’s population that is Asian, Black, Native American, and White (along with separate variables for those recorded as being of an “other” race and “multiple” races). We also record basic economic data. Specifically, we include the labor force participation rate, unemployment rate, poverty rate, child poverty rate, and median income. Regarding educational attainment, we use a set of indicator variables for level of completion. We also record the number of violent crimes, property crimes, and murders in each county. Finally, we include the distribution of votes for Donald Trump, Hillary Clinton, and third-party candidates in the 2016 election to measure partisanship. Table 3 provides the summary statistics on the county-level variables used.

We create an indicator variable denoting whether the county is in one of the appointment states and use it as the dependent variable in the first-stage regression. Each of our county-level, socio-economic variables are used as independent variables in the linear probability model regression. We then use these estimated coefficients to create fitted values for each county in the U.S. This can be interpreted as the likelihood that the county is treated by being in a state that does not elect their local prosecutors.

Obviously, the treated counties tend to have the highest fitted value. Most of the appointed counties are in the top 2.5% of the counties if ranked by the size of their fitted values (top 75). Thus, we use the counties from the election states that are also in the top 2.5% as our control sample. In addition, for those appointment counties not in this top group, we identify the county (which uses the election mechanism) that has the closest fitted value to it, and add it to the control group. Following this procedure, we end up with a matched sample of 68 counties for our 34 treated counties.

The final column in Table 3 provides a check on the quality of the matching procedure. Here, we consider

requests. The implications of judicial independence has been explored by Hanssen (2000; 2004).

¹⁷We chose to exclude Alaska because criminal prosecution is all handled by the Attorney General’s office.

Table 3: County Summary Statistics

Variable	Obs.	μ	σ	min	max	t -stat
Demographics						
Population	3192	204771	1257264	88	3.96x10 ⁷	1.98 *
Change in Population	3186	0.011	0.078	-0.264	1.14	0.43
Deaths	3192	10.33	2.700	0	21.49	0.24
International Migration	3192	1.139	2.253	-3.99	38.69	1.06
Domestic Migration	3192	-0.065	11.37	-64.43	66.12	0.18
Female	3094	0.499	0.023	0.268	0.569	1.69 *
Race						
Black	3142	0.090	0.145	0	0.869	1.83 *
Asian	3142	0.013	0.028	0	0.429	1.12
Native American	3141	0.020	0.077	0	0.928	2.19 **
Race: Other	3142	0.020	0.038	0	0.588	0.72
Race: Multiple	3142	0.0233	0.019	0	0.286	1.75 *
Economy						
Labor Force Part. Rate	3191	0.470	0.074	0	1.273	1.16
Unemployment Rate	3190	0.041	0.015	0.013	0.199	0.06
Median Income	3190	51221	13488	22679	136191	1.43
Poverty Rate	3191	0.147	0.059	0.029	0.568	0.54
Child Poverty Rate	3191	0.047	0.023	0.007	0.223	0.20
Education						
Less than a H.S. Degree	3192	0.094	0.044	0.008	.0452	1.68 *
High School Degree	3192	0.236	0.058	0.047	0.458	4.61 ***
Some College	3192	0.209	0.039	0.068	0.460	2.51 **
Crime						
Violent Crime	3142	2.557	3.520	0	116.7	1.38
Property Crime	3142	17.919	37.35	0	1637.3	1.28
Murder Rate	3142	3.709	10.44	0	269.0	1.10
Elections						
Republican Vote Share	3111	0.636	0.157	0.041	0.953	3.15 **
Third Party Vote Share	3111	0.047	0.028	0.003	0.353	1.24

The indicator variables for White, College Degree, and Democratic Vote Share are omitted since the race, education, and partisanship controls each sum to one. For the final column *** denotes the 1% level of significance, while ** is for the 5% level of significance, and * for the 10% level.

the sample of treated and matched counties and estimate a simple linear probability model with an indicator variable for being a treated county (i.e., being a county in a state that appoints its local prosecutors) as the dependent variable. All county characteristic variables are included as explanatory variables. The final column presents the t -stat for the test of whether that variable’s coefficient is different from zero. The asterisks represent the statistical significance of each.

Our matching procedure does fairly well. The mortality, migration, labor, poverty, income, and crime variables are all statistically indistinguishable. There is some distinction in the race variables. Our treated sample is more white, with an average proportion of 76.9% in the appointment counties compared to 71.4% in the matched counties.¹⁸ Also, there seems to be differences in educational attainment. The treated counties have a lower proportion of their residents with a college degree (24.9% on average) than the matched counties (30.2%). Finally, the treated counties supported President Trump more than the matched counties (44.2% vs. 38.4%). Overall, the samples match well. Also, the set of matches is reasonable. It is made up of counties in California (# = 10), Virginia (10), and New York (8), with a handful of counties in Maryland, Massachusetts, Illinois, and Pennsylvania. Together, counties from these states make up 74% of the *Matches* subsample, which presumably closely resemble counties in Connecticut, Delaware, New Jersey, and Rhode Island.

With these two additional subsamples created, we followed the same protocols as previously described. These additional counties were included randomly in the four monthly information request emails sent out in February, March, August, and September.

4.2 Appointments and Responsiveness

The response rate between prosecutor offices in the appointment states and the matched counties can be contrasted. Figure 9 depicts the difference.

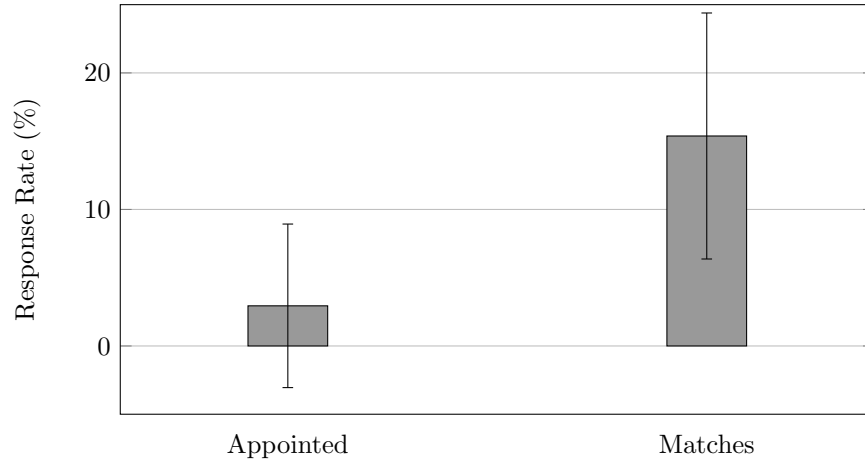
Few offices in the counties with appointed head prosecutors responded to the information request. The response rate of those in the matched sample (15.4%) is quite close to the response rate of the full sample analyzed in the previous section (14.7%). The difference in the response rate of the appointment counties, then, is both economically and statistically significant.

Table 4 combines all prosecutor offices contacted and compares the subsamples. It includes month fixed effects to account for any temporal variation that affects response rates (such as the novel coronavirus pandemic) and state fixed effects. Standard errors are clustered at the state level.

Ultimately, the offices in the states that do not elect their head prosecutors are the only group that shows a statistically significant difference in the response rate. The estimated coefficient coincides with the raw data – their response rate is 12.8 percentage points less, which given the expanded sample’s mean value for the dependent variable, represents an 76.1% lower response rate.

¹⁸The difference in the proportion that are Black is negligible (10.5% vs. 10.1%), but the proportion Asian is quite different (5.4% vs. 9.4%). A partial explanation of this is that quite a few of the matched counties are in California. While similar to the greater NYC metropolitan area in economy, crime, and political partisanship, they differ in the size of their Asian population.

Figure 9: Appointed Prosecutors and Their Matches



A two-tailed t -test of the hypothesis that the response rate is higher for those up for re-election in 2020 generates $t = 1.89$ with a p -value of $p = 0.062$. Here we include responses of even those who do not complete at least 50% of the survey. $N = 34$ in the appointed sample and $N = 65$ in the matched sample.

Table 4: Responsiveness of Appointed Prosecutors

	(1)	(2)	(3)
Appointed Subsample	-0.080 *	-0.120 **	-0.128 ***
	(0.039)	(0.022)	(0.025)
Matched Subsample			-0.058 (0.048)
Called Subsample			-0.022 (0.053)
Replacements Subsample			0.026 (0.053)
Month Fixed Effects?	Yes	Yes	Yes
R^2	0.028	0.021	0.025
AIC	30.7	372.9	376.8
N	99	535	535

Results from a linear probability model presented where the dependent variable is equal to one if the office responded to the survey (completing at least 50%). Standard errors clustered by state are presented in parentheses (25 clusters for (1) and 47 clusters for (2) and (3)); *** 1%, ** 5%, and * 10% level of significance. The first column includes only the *Appointed* and *Matched* subsamples. The second column includes all offices contacted; adding the *Core*, *Called*, and *Replacements* subsamples.

5 Who Responds and What Are Their Responses?

The analysis up to this point has focused on whether someone in the prosecutor’s office responded to the information request – not on what information was provided. While descriptive statistics on staffing and funding of prosecutor offices is potentially interesting, a comparison between the responses in 2007 and 2020 provide more insight. Specifically, it allows us to further evaluate the selection of who it is who responds to our information request. Since we have data from all prosecutor offices in 2007, we can compare the responses provided by those who completed our survey to those who chose not to respond to our information request. If staffing and funding levels look similar between the two cohorts in 2007, then it seems unlikely that funding and staffing levels are driving the difference in response rates in 2020.

In this section, we consider the offices in the full sample analyzed in Section 3. We exclude the appointed and matched, similar counties. The Department of Justice survey conducted in 2007 did not present distinct staffing and funding information for each office in the states with appointments. These offices are treated as branches of the states’ Attorney General’s office and only the aggregate statewide values are given.

Of those who chose to disclose in our survey, 77.5% of the responses were provided by the head prosecutor. Another 12.7% were provided by an assistant prosecutor in the office. The remaining 9.9% were done by supporting staff. The mean length of service for the current office head is 10.6 years.¹⁹ There is wide variation in staffing and funding levels within our sample of those who reported in 2020. What is important is whether the distribution of office characteristics in 2007 of those who responded in 2020 match the distribution of characteristics in 2007 of those who did not respond in 2020.

First, in our survey we ask the office representative to provide the number of assistant prosecutors, investigators, and other supporting staff. They were instructed to count any part-time employees as being equal to one-half of a worker. Figure 10 presents the comparisons.

The light gray columns are the mean number of employees in each category in 2007 for those offices who were randomly selected to be part of the study (the full sample of 443 offices), but chose not to respond. The dark gray columns are the mean number of employees in each job type in 2007 for those offices selected randomly to be in our investigation, and did respond to our information request in 2020.

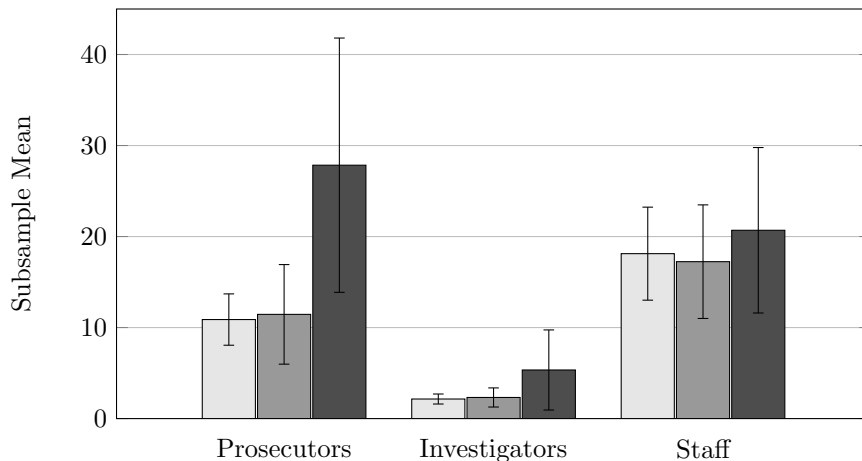
As one can see, there is very little difference in staffing levels between the two groups. Any difference is statistically indistinguishable from zero. Thus, there is not evidence that differences in staffing can explain differences in the response rates.

The black columns provide the mean staffing levels in 2020 for those who responded to our information request in 2020. Interestingly, office size, measured by the number of employees, has grown. Most noteworthy is the increase in the number of prosecutors in these offices.

Another series of questions asked by the Department of Justice in 2007 was what violence employees within the prosecutors’ offices are exposed to. Prosecutors and supporting staff can receive threatening

¹⁹There is a lot of dispersion in this value. The standard deviation is 9.0 with a minimum of 2 months and a maximum of 35 years of service.

Figure 10: Selection Using 2007 DOJ Data: Staffing



The light gray columns depict the sample mean, in the 2007 data, for those prosecutor offices that did not respond to the information request in 2020 ($N = 364, 414, 414$). The dark gray columns depict the sample mean, in the 2007 data, for those prosecutor offices that responded to the information request in 2020 ($N = 78, 76, 76$). The black columns depict the sample mean, in the 2020 data, for those prosecutor offices that responded to the information request in 2020 ($N = 71, 71, 73$). The variation in sample sizes is due to missing information in either the 2007 data set or incomplete responses in the 2020 data collection effort. The 95% confidence intervals are depicted. Two-tailed t -tests do not produce a statistically significant difference in the means between those who responded and those who did not respond in 2020 within the 2007 data ($t = 0.17, 0.26, 0.16$ for the three, respectively, with p -values of $p = 0.86, 0.79, 0.88$, respectively). Here, we include all responses, even if less 50% of the survey is completed.

telephone calls or written letters (including email). They can receive verbal threats, or even become the victims of battery/assault. Here, we assess whether there is any systematic difference between the offices that replied to us and those who did not by looking at violence experienced in 2007. Figure 11 illustrates.

Once again, comparing those who responded to us and those who did not, there is only negligible differences in threats experienced in 2007. The last column in each set shows that threats are increasing slightly over time. The only difference that is statistically significant is that there is an increase in written threats. A potential explanation for this is the expanded access to email since 2007.²⁰

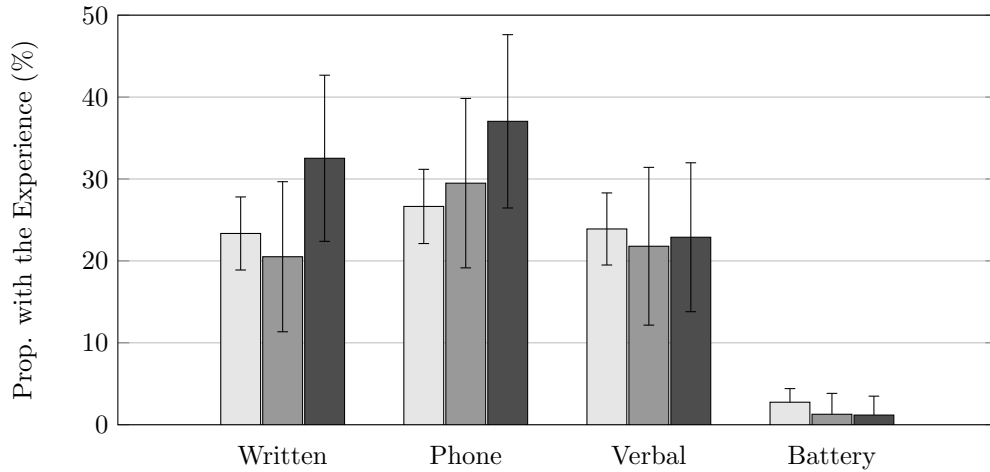
A third bit of information that the Department of Justice was interested in collecting was the opportunities for prosecutor offices to use forensic laboratories for DNA testing. Specifically, offices were surveyed regarding whether they use the FBI forensic lab, a state forensic lab, a local forensic lab, or a private one for DNA testing. Figure 12 illustrates usage.

Once again, there is little difference between the two cohorts in the 2007 data. Interestingly, use of each facility drops in 2020. Since respondents in both surveys were asked to select all that applied, this reduction is likely due to specialization. That is, while 15 years ago prosecutor offices are utilizing numerous forensic laboratories, it seems that offices now use only one or two.

Finally, prosecutor offices provide public services beyond just prosecuting the accused. We selected six of

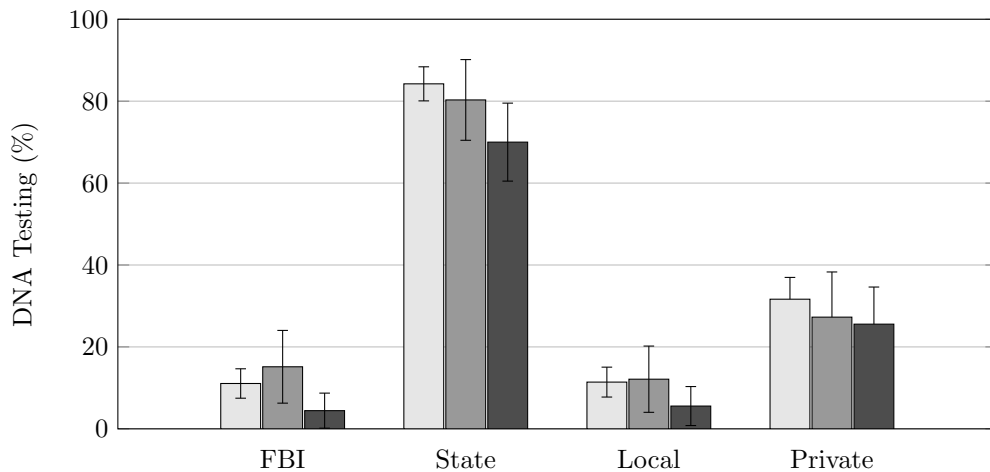
²⁰One curiosity that we cannot explore with our data is whether offices who see the greatest escalation in email-based threats are the ones that less likely to provide email addresses in contact information on the office website and, similarly, are less motivated to respond to emails.

Figure 11: Selection Using 2007 DOJ Data: Threats



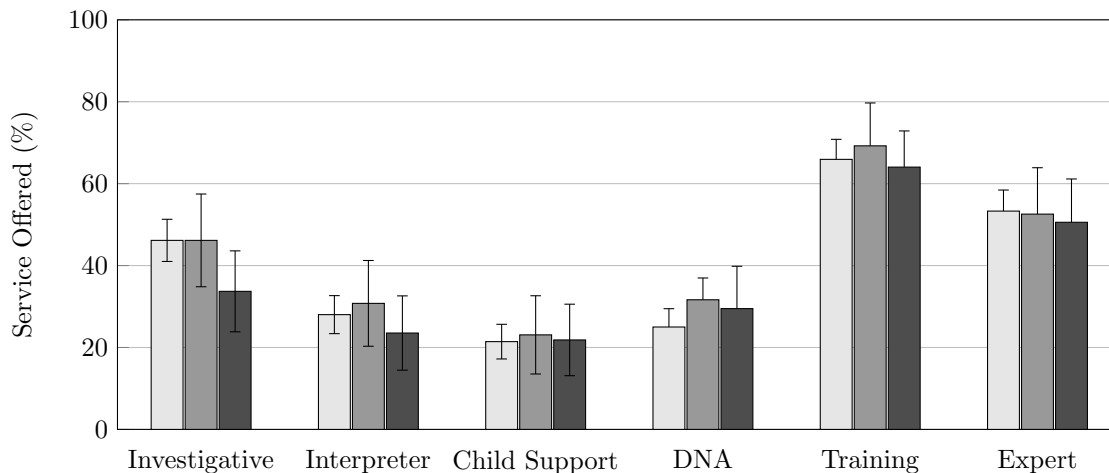
The light gray columns depict the sample mean, in the 2007 data, for those prosecutor offices that did not respond to the information request in 2020 ($N = 364$). The dark gray columns depict the sample mean, in the 2007 data, for those prosecutor offices that responded to the information request in 2020 ($N = 78$). The black columns depict the sample mean, in the 2020 data, for those prosecutor offices that responded to the information request in 2020 ($N = 83, 81, 83, 85$). The variation in sample sizes is due to missing information in either the 2007 data set or incomplete responses in the 2020 data collection effort. The 95% confidence intervals are depicted. Two-tailed t -tests do not produce a statistically significant difference in the means between those who responded and those who did not respond in 2020 within the 2007 data ($t = 0.54, 0.51, 1.19, 0.40$ for the three, respectively, with p -values of $p = 0.59, 0.61, 0.24, 0.69$, respectively). Here, we include all responses, even if less 50% of the survey is completed.

Figure 12: Selection Using 2007 DOJ Data: DNA Testing



The light gray columns depict the sample mean, in the 2007 data, for those prosecutor offices that did not respond to the information request in 2020 ($N = 298, 298, 298, 297$). The dark gray columns depict the sample mean, in the 2007 data, for those prosecutor offices that responded to the information request in 2020 ($N = 68, 66, 66, 66$). The black columns depict the sample mean, in the 2020 data, for those prosecutor offices that responded to the information request in 2020 ($N = 90$). The variation in sample sizes is due to missing information in the 2007 data set. The 95% confidence intervals are depicted. Two-tailed t -tests do not produce a statistically significant difference in the means between those who responded and those who did not respond in 2020 within the 2007 data ($t = 0.93, 0.78, 0.16, 0.69$ for the three, respectively, with p -values of $p = 0.35, 0.44, 0.87, 0.49$, respectively). Here, we include all responses, even if less 50% of the survey is completed.

Figure 13: Selection Using 2007 DOJ Data: Services



The light gray columns depict the sample mean, in the 2007 data, for those prosecutor offices that did not respond to the information request in 2020 ($N = 364$). The dark gray columns depict the sample mean, in the 2007 data, for those prosecutor offices that responded to the information request in 2020 ($N = 78$). The black columns depict the sample mean, in the 2020 data, for those prosecutor offices that responded to the information request in 2020 ($N = 89, 85, 87, 86, 89, 87$). The variation in sample sizes is due to incomplete responses in the 2020 data collection effort. The 95% confidence intervals are depicted. Two-tailed t -tests do not produce a statistically significant difference in the means between those who responded and those who did not respond in 2020 within the 2007 data ($t = 0.93, 0.78, 0.32, 0.69, 0.12$ for the three, respectively, with p -values of $p = 0.35, 0.44, 0.75, 0.49, 0.91$, respectively). Here, we include all responses, even if less 50% of the survey is completed.

the services asked about by the DOJ in 2007: (i) funds for investigative services, (ii) funds for interpreters, (iii) funds for child support collection help, (iv) funds for DNA testing, (v) funds for staff training, and (vi) funds for expert services. Figure 13 illustrates.

Across all service categories, again there is not a statistically significant difference between those who responded to us in 2020 and those who did not in the funds available in 2007. It is interesting to note that funding for all services reduces in 2020.

Taken together, the figures illustrate that there does not seem to be any important measurable differences between those who responded to the information request and those who did not. Combining this observation with the results of the randomness check showing that for almost all county-level characteristics we utilize there is again no difference between the two groups, the two seem identical. Therefore, it seems unlikely that differences in office composition is driving the response rate observed. As a result, we can be quite confident that the discrepancy documented between those offices where the head prosecutor is up for re-election and those not is the causal factor driving responsiveness.

6 Conclusion

Numerous publicly provided services are supplied by local public actors. As with any job, principal-agent concerns exist. The United States is unique in the world in its use of popular elections to select and retain

these officeholders. It is prudent to ask whether the election mechanism is effective. Here, we focus on the public service of the prosecution of crime. Criticisms of the mechanism come from two sides. First, it is argued that voters do not have the ability to effectively hold prosecutors accountable for their actions, as they lack the information, incentive, and human capital to do so. Second, empirical evidence strongly suggests that prosecutors act as if their decisions at work matter as distortions in criminal justice outcomes coincide with the election cycle.

We argue that a positive feature of the election mechanism is that these public actors should be more responsive to the community. We measure responsiveness using an information request. Responding to the email completing the survey, we argue, signifies a prosecutor office that has the community as a priority.

We find that offices whose head is up for re-election are more likely to respond to the information request than offices whose leader is not up for re-election. Also, we find that offices whose head is appointed are less likely to respond than comparable offices in states who elect their prosecutors. Taken together, these findings suggest that a benefit of the election mechanism is that elected prosecutors are more responsive.

We provide these results in an attempt to inform the normative debate about the appropriateness of the election mechanism. Our findings by no means imply that elections are ideal. Rather, the heightened responsiveness we observe should be compared to the costs of distorted criminal justice outcomes observed in past work. It is also not clear to what degree prosecution decisions should be influenced by popular opinion in the community²¹, or the degree of importance that should be placed on responsiveness relative to other functions of the office. Our hope is that our observations inform this debate.

The primary concern with our findings is that responding to academics engaged in a research project does not correlate with responding to potential voters. As argued, what is important to our results is that the difference in an office's propensity to respond does not correlate with the election's timing. In our test of concept we provide suggestive evidence that responses to us correlate with responses to the public. This builds confidence in our method's reliability. We strove to avoid deception in our study, but a project that has information collected from residents who live in the community that the prosecutors serves would fully alleviate this concern. Further, we measure responsiveness by whether someone opened an email and clicked on a link. One can imagine numerous other ways to measure responsiveness. As examples, one could hope that the election mechanism encourages prosecutors to evaluate the level of crime in the community, the burden on local public finances from their decisions, and the welfare of crime's victims. We expect that our measure sufficiently correlates with alternative conceptions, but this is left for future research.

²¹[?] specifically consider how "social sanctions" from the community may affect prosecutors in their plea bargaining decisions. A common normative argument is that judicial independence is preferred hansenAER, but in the context of prosecutors it is less clear to what degree responsiveness is important.

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