

DISCUSSION PAPER SERIES

IZA DP No. 15121

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## ABSTRACT

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### Registering Returning Citizens to Vote\*

Millions of people in the US are eligible to vote despite past criminal convictions, but their voter participation rates are extraordinarily low. In this study, we report the results of a series of randomized controlled trials (RCTs) of mail-based interventions aimed at encouraging people with criminal records to register to vote in North Carolina. We use a novel approach to identify and contact this population, using a combination of administrative data and data from a commercial vendor. In our main experiment, conducted in the fall of 2020, we find that, on average, our mailers increased voter registration by 0.8 percentage points (12%), and voter turnout in the general election by 0.5 percentage points (11%). By contrast, our treatment has no effect on a comparison group of people without criminal records who live in the same neighborhoods. We find suggestive evidence that treatment effects vary across demographic groups and with the content of mailers. For instance, effects were smaller for Black recipients, and smaller when extra “civil rights framing” was added to the mailer text. Overall, we demonstrate that it is possible to identify, contact, and mobilize a marginalized group that is not effectively targeted by existing outreach efforts. Our results speak to how organizations can increase voter registration and turnout among people with criminal records, without necessarily changing laws to broaden eligibility.

**JEL Classification:** K42, K16

**Keywords:** criminal justice reform, civic engagement, voting, crime

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# 1 Introduction

Millions of people in the United States with felony convictions are eligible to vote despite their criminal record (Manza and Uggen, 2008). Only eleven states permanently disenfranchise certain people with felony convictions, most states do not permanently bar anyone from voting, and since 2016 eleven states and the District of Columbia expanded access to the franchise for people involved in the criminal legal system (*Felon voting rights*, 2021; Uggen et al., 2020). For example, Kentucky and Alabama expanded access to the vote to people convicted for non-violent offenses, and New Jersey enfranchised people still on probation or parole. Researchers estimate that between the 2016 and 2020 general elections, over a million people gained the right to vote (Uggen et al., 2020).

But formally regaining eligibility does not guarantee that one will participate in the electorate, and existing research suggests that justice-involved people rarely vote. This is true for a variety of reasons: people may not know they have the right to vote (Meredith and Morse, 2015); cumulative disadvantage that results from criminal legal entanglement inhibits access to resources important to registering and voting (White, 2019; Pettit and Western, 2004); negative experiences with the enforcement arm of the state may lead people to believe that their civic voice doesn’t matter (Lerman and Weaver, 2014); and mobilizing institutions at the center of efforts to mobilize and turn out the electorate neglect returning citizens because they are incentivized to target high propensity voters, rather than to invest in expanding the electorate (Owens and Walker, 2018). Even so, a nascent line of research suggests that under the right conditions, and when asked, returning citizens may choose to participate, both electorally (Gerber et al., 2015) and in other kinds of related political activities (Walker, 2020). We therefore ask: how can individuals with felony convictions who are not registered, even though they are eligible, be converted into voters?

This question is deceptively hard to study. Researchers know very little about how to identify and locate people with felony convictions (Gerber et al., 2015, 2017). Custodial citizens are hard to reach, relatively transient, and, with few exceptions, never the target of traditional “get-out-the-vote” (GOTV) outreach efforts (Owens and Walker, 2018). Most GOTV efforts target lists of registered voters, and no universal list of unregistered voters exists (Mann and Bryant, 2020). Even as the literature is replete with knowledge about how to mobilize the already-registered, very little is known about how to expand registration. This focus on already-registered people will miss many people with past convictions, as researchers looking across multiple states have estimated that only about 20 percent of people with prior criminal legal involvement are registered to vote (Gerber et al., 2015; Burch, 2011).<sup>1</sup> Because

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<sup>1</sup>By comparison, 73% of the full adult citizen population in the US was registered to vote as of the

accurate samples of this population are difficult to construct and traditionally overlooked by people interested in voter mobilization, little is known about differences between registered and unregistered returning citizens, how to reach them, and whether traditional methods of voter mobilization—mailers, phone calls, or in-person efforts—are effective. Therefore, whether we can construct and reach a sample of returning citizens that is representative of the population is a central question and contribution of the paper.

To address these questions we run a series of field experiments, leveraging several different kinds of data. We bring together administrative data from the Department of Public Safety (DPS) and voter files to identify people with past felony convictions in North Carolina who are eligible to vote but not registered, then work with a commercial data vendor to find their contact information. We then send random sub-samples of this population information on eligibility rules and how to register to vote. We track their registration and voting behavior in the administrative data to measure the effects of our intervention. We find that our relatively low-touch, mail-based intervention effectively increased registration rates and turnout among eligible voters with felony convictions.

To develop a final design, we ran three pilot studies in North Carolina during the first half of 2020. Across these pilots, our intervention increased voter registration by 0.9 percentage points over the subsequent 30 days (82% of the control group mean,  $p < 0.01$ ).

We implemented a larger “main” study in the fall of 2020, also in North Carolina. This main study allowed us to test the efficacy of providing voter eligibility and registration information to a larger sample of people with criminal records, and to test the contribution of particular components of our intervention. On average, sending our mailers increased voter registration by 0.8 percentage points (12%,  $p < 0.05$ ), and voter turnout by 0.5 percentage points (11%,  $p < 0.10$ ).<sup>2</sup>

In this main study we randomized the type of mailer we sent to individuals in our sample, to test the mechanisms driving our basic mailer’s effect. Our basic mailer includes an opening paragraph highlighting the eligibility of people with criminal records to vote; a clear list of eligibility requirements (including that only those *currently* incarcerated or on probation/parole are barred from voting); and a stamped, addressed registration form. We test three alternatives to this basic mailer: (1) removing the opening paragraph highlighting the criminal record information, (2) removing the enclosed registration form, and (3) adding an extra, motivational paragraph at the end about how voting can help determine criminal justice policy and civil rights.

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November 2020 election ([Current Population Survey, 2021](#)).

<sup>2</sup>[Green and Gerber \(2019\)](#) survey the literature on GOTV efforts with previously-targeted populations and report that studies using mailers increase turnout by 0.5 percentage points, on average.

Contrary to our expectations (based on conversations with community groups), we find that *not* highlighting the criminal record info in the opening paragraph did not reduce our mailer’s efficacy; the point estimate is actually larger. Similarly, removing the registration form did not reduce the effect on voter registration. This may be because recipients could register online in the state, but it was still somewhat surprising; on the margin, including a stamped, addressed registration form should have made registration easier. Nor did adding extra civil rights framing at the end of our mailer appear to add an additional boost to registration. This was a surprise to our team; based on earlier research and conversations with local organizations, we expected this paragraph to have a mobilizing effect.

Our results suggest that simply contacting this population and providing basic information about eligibility requirements was the key to our intervention’s success. The other components of our mailers had no additional effect.

Given racial disparities in the criminal justice system, the effect of carceral contact on racial disparities in civic engagement is a primary concern. We thus consider whether our effects vary with race. We find that our intervention was more effective for white recipients than Black recipients. This difference is not due to differences in address quality across groups. It appears that marginal Black registrants in this target population may be more difficult to mobilize, or require different interventions than marginal white registrants.

We use a machine learning approach developed by [Wager and Athey \(2018\)](#) and applied in [Davis and Heller \(2017\)](#) to test for heterogeneity across other baseline characteristics. Individuals that had the largest (most positive) treatment effects were, on average, more likely to be male and more likely to have a history of incarceration. Those with the smallest (most negative) treatment effects were more likely to be Black and more likely to be older (age 55+).

Finally, we constructed a comparison group of individuals without felony convictions who lived in the same neighborhoods as those in our data, and fielded a parallel experiment. The goal of this parallel experiment was to provide context for our main results – are they about what we’d expect for a socioeconomically-disadvantaged target population? Sending this group mailers had precise null effects on voter registration. We interpret this to mean that the effectiveness of our intervention is unique to people with felony convictions. This may be because the information in the mailers is uniquely relevant to people with criminal records, or because existing outreach efforts somehow miss people with records even as they reach their neighbors.

How representative was our analysis sample of the broader target population (people who had been convicted of a felony in the past but are now eligible to vote)? People were only included in our analysis sample if we could match them with a valid mailing address.

We considered how this affected the composition of our sample, in terms of observable characteristics. Our analysis sample looks similar to this broader group in terms of race and gender. However, those who were released from DPS custody more recently were more likely to match to mailing addresses, and so average time-since-release is lower for our main study sample (9.1 years) than the broader population (17.3 years). Our analysis sample is also slightly younger in age (43.9 years for our main study sample vs. 51.3 years in the full sample), because we focus our experiment on people age 70 and younger.<sup>3</sup>

This study was deliberately non-partisan. We sought to understand how to broaden the electorate, not how to register people with any one political party. However, we wondered if our intervention might have unintentionally been more effective for people with particular political leanings. We compare party of registration in our treatment and control groups and find little evidence that this was the case. Strikingly, given broader public conversations about the likely political consequences of felon disenfranchisement, we find that new registrants were fairly evenly split between Democrats and Republicans, with a substantial share registering as unaffiliated with any party.

This project makes two contributions, one methodological and the other substantive. Methodologically, our research contributes a process for reaching difficult-to-contact populations. The various benchmarks we provide suggest that barriers to targeting unregistered citizens for mobilization can be overcome. Since people with criminal records are often on the margins of a variety of systems and institutions (employment, education, social services)—in part because of their records—it is unclear how to identify this group for outreach. Even the data we use from commercial vendors was of previously-unknown quality for this group. We show that it is possible to find and contact a meaningful share of these individuals, and that the resulting samples are a reasonable proxy for the underlying population. This is a chief contribution of this project.

Substantively, this project contributes to a small but growing body of work suggesting that returning citizens are not lost to the polity. Instead, they are a latent political force. Many formerly-incarcerated people have spoken about the personal significance of reclaiming citizenship rights (Owens, 2014). Researchers have further pointed out that carceral contact itself means that custodial citizens are policy stakeholders across a number of issue areas (Owens and Walker, 2018). Restoring the right to vote to people with felony convictions is a first (necessary) step toward their political integration. The potential benefits from bringing marginalized citizens into electoral politics for public policy, democratic legitimacy, and community health are potentially both deep and broad. This study considers the causal

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<sup>3</sup>We dropped people over age 70 because we wanted to avoid unintentionally sending mailers to a large number of people who were deceased.

effects of strategies aimed at increasing the civic participation of people with criminal records, and contributes to our understanding of how to reach and mobilize members of this group. The following section provides the theoretical grounding for this project. Next, we review our data and methods, and present the results of our pilot studies, main study, and comparison study. We then turn to our auxiliary analyses, after which, we conclude.

## 2 Background

Public debates about felon disenfranchisement laws have focused on the millions of Americans who are ineligible to vote because of criminal convictions, but the majority of these individuals regain their right to vote upon completion of all or part of their sentence. Despite fairly widespread rights restoration, scholars have documented very low rates of registration and voting, even among those who are eligible to vote (Lerman and Weaver, 2014; Gerber et al., 2015; White, 2019; White and Nguyen, forthcoming).

Existing research suggests that people with felony convictions face barriers to participation that are similar to those faced by other marginalized people. Carceral contact has personal and social implications, exacerbating resource barriers to participation faced prior to criminal legal entanglement (Pettit and Western, 2004).<sup>4</sup> Barriers related to cumulative disadvantage are compounded by the corrosive effect of carceral contact on attitudes requisite to voting. Such attitudes include trust in government, the belief that elected officials care about one’s voice, and the belief that change is possible (Lerman and Weaver, 2014; Burch, 2013; Uggen and Manza, 2002). Finally, because individuals with felony convictions are unlikely voters, they are often overlooked by the kinds of organizations central to mobilization during elections, campaigns, political parties, and related interest groups. These groups are incentivized to spend their limited resources turning out individuals already on the voter rolls. Because of this, eligible voters at society’s margins, like returning citizens, are neglected for outreach efforts. This further inhibits their full incorporation into the polity (Owens, 2014). For all these reasons, researchers find that all kinds of criminal legal contact are associated with a declining likelihood of registering and turning out (Burch, 2011, 2013; Lerman and Weaver, 2014; White, 2019). Burch (2011) estimates that in North Carolina –

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<sup>4</sup>Barriers to employment in sectors that might otherwise provide stable working conditions for formerly incarcerated people provide an example: researchers estimate that 25 percent of jobs in the United States, such as nursing, education and construction, require that one hold a license, but licensure very often requires individuals to pass a background check (Rodriguez and Avery, 2016). The American Bar Association documents 12,000 instances across all 50 states where a misdemeanor or felony conviction disqualifies one for employment in a given occupation (Rodriguez and Avery, 2016). Similar statutes prohibit access to public housing and welfare benefits for individuals convicted of certain drug-related crimes (Remster, 2019; McCarty et al., 2012).

the state we focus on in this study – 36% of residents with felony convictions were registered to vote in 2008, and 24% turned out in that election.<sup>5,6</sup> One conclusion might be that barriers to participation associated with contact with the criminal legal system might be too difficult to overcome, even with targeted mobilization efforts.

The true conclusion is likely more nuanced. Many eligible voters with felony convictions do not know they are eligible to vote, and they often are not notified of their restored rights (Meredith and Morse, 2015). Researchers recognize, moreover, that requirements that one register in order to vote place an additional burden on voters, and that those who have overcome this precondition are already highly likely to turn out. For this reason, one of the only electoral reforms researchers have identified that effectively enhances turnout among low propensity voters after the passage of the Voting Rights Act is same day registration (Grumbach and Hill, 2022). Yet, despite the recognition that getting registered to vote is a major step often out of reach for marginalized people, most research around voter mobilization focuses on GOTV efforts to convince already-registered voters to turn up at the polls (Mann and Bryant, 2020). Evaluating how to encourage voter registration is considerably more difficult than studying the impact of efforts to encourage turnout among those already registered, because there is no universal database of eligible but unregistered people (Mann and Bryant, 2020). Traditional GOTV experiments sample from lists of already-registered people.

And there is reason to believe that removing practical barriers to registration might play a role in increasing registration among individuals with felony convictions, further emphasizing the importance of studying registration in addition to voting. Emerging research suggests that people impacted by the criminal legal system can be mobilized under certain circumstances (Walker, 2020; Laniyonu, 2019; Ang and Tebes, 2021; Gerber et al., 2015). Individuals who view their experiences as unjust, who are involved with other kinds of organizations that are both political and provide services to returning citizens, and who are situated in electoral contexts where criminal justice issues are relevant, are all more likely to participate than those for whom those things are not true (Walker, 2020; Owens and Walker, 2018; Laniyonu, 2019). Almost all such studies, however, are observational in nature and threatened by selection, response (in the case of surveys), and omitted variable bias.<sup>7</sup>

Researchers wishing to assess the effect of voter registration efforts cannot rely on readily

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<sup>5</sup>Our data matching procedure unfortunately precludes us from computing a parallel estimate in 2020.

<sup>6</sup>By comparison, 70% of the full adult citizen population in North Carolina was registered to vote as of the November 2020 election (Current Population Survey, 2021).

<sup>7</sup>An exception is Ang and Tebes (2021), who find that when students are exposed to apparently-unjust police violence in their neighborhood, they are more likely to vote. They use the as-if random location of such violence within small geographic areas to argue that this reflects a causal effect of perceived injustice on subsequent civic engagement.

available lists (as in GOTV studies), and therefore often target specific, known groups, like college students, or engage in a more general door-knocking approach in given neighborhoods (Bennion and Nickerson, 2016; Mann and Bryant, 2020; Nickerson, 2015). As a consequence of the difficulty of constructing lists of unregistered people, we know much more about how to convince marginal voters to participate than we do about how to expand the electorate through engaging new voters. Researchers face added challenges with respect to returning citizens, because people with criminal convictions are often deeply marginalized prior to conviction, which is exacerbated by criminal legal entanglement. They may be less likely to have stable addresses, less likely to be listed on utility bills or issued credit cards, and thus less likely to appear in consumer or other commercial datasets. Returning citizens are therefore a hard-to-reach population, and little is known about how to effectively encourage their civic engagement.

Only one study of which we are aware examines the responsiveness of formerly incarcerated people to messages encouraging them to register and vote. Gerber et al. (2015) find that a basic mailer targeted to recently-released people improved turnout by 1.8 percentage points relative to the control group, suggesting that people can be re-incorporated into political life if they can be found and encouraged. This study, however, focused on an easy-to-find subgroup: those recently released from prison (average time-since-release was 1.8 years; max was 3 years), where a state government provided their release-address information to facilitate the research.<sup>8</sup> It is hard to know how to extend these results to the much broader pool of people with a felony conviction in their past (but without current contact information on file with the government), or whether non-governmental mobilization efforts would work.

Even though millions of people across the country have criminal convictions, we know relatively little about their baseline capacity for mobilization and how that compares to other marginalized people without convictions (Uggen, Manza and Thompson, 2006). On one hand, criminal legal entanglement and its consequences either explicitly exclude returning citizens from voting or lead them to opt out. On the other hand, carceral contact itself creates policy stakeholders, and some research suggests that individuals can be compelled to participate under the right conditions (Owens, 2014). Yet, data limitations and difficulty identifying unregistered voters has hindered the development of knowledge around how to effectively mobilize this group. Serious questions remain around the capacity to develop a representative list of people with felony convictions to target for intervention.

To speak to these issues we investigate whether, when resource barriers to registration

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<sup>8</sup>For various reasons, this study also focused on individuals who served relatively short sentences (less than three years), for mostly non-violent felonies. We do not restrict our sample based on crime type, length of sentence, or time-since-release, and so can speak to outreach efficacy for a much broader group.

reduced, with the provision of information about voting eligibility and the registration process, voting-eligible people with criminal records can be mobilized to register to vote. In order to develop knowledge around how to reach returning citizens, we take great care to evaluate the quality of our constructed sample, who we reach, and the eligible voters we successfully register and turn out. We describe our data, interventions and analytic strategy in detail below. In keeping with traditional voter outreach efforts, our interventions take place entirely through mail, text messages and phone calls. Focusing on the knowledge and resource obstacles to participation people with felony convictions often face, our efforts aim to reduce barriers to registration through the provision of information around eligibility and how to register.<sup>9</sup>

### 3 Data and Methods

This project focuses on North Carolina, using a combination of state administrative data and information from a commercial data vendor to identify unregistered, voting-eligible people with past criminal convictions.<sup>10</sup> People who have been convicted of felonies in North Carolina are temporarily ineligible to vote, but their eligibility is automatically restored after they have completed their sentences (including probation or parole). During the period of this study, North Carolina required that an individual registering to vote: (1) be a United States citizen; (2) be a resident in the county of registration for at least 30 days prior to the date of the election; (3) be at least 18 years old by the date of the general election; and (4) not be serving a sentence for a felony conviction, including probation, parole, or post-release supervision.<sup>11</sup> Individuals can register by mail, online, or in person.

#### 3.1 Constructing the Sample

In this subsection, we offer an overview of our approach to constructing the experimental sample. We pay special attention to composition and quality of the sample, overall, since a

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<sup>9</sup>The full pre-registration and analysis plan can be found here: <https://www.socialscienceregistry.org/trials/4574>

<sup>10</sup>The original design for the main experiment included an expansion into the state of Texas. Ultimately, we omit Texas from the analysis presented here due to implementation challenges that lead us to doubt the accuracy of our results in that state. See Appendix C for further details on the design, implementation and findings in Texas.

<sup>11</sup>The North Carolina guidelines are available here: <https://www.ncsbe.gov/registering/who-can-register>. Individuals may be serving an extended period of probation or parole due to outstanding fines or fees; during the period of our experiment, this made them ineligible to vote. A recent court order changed this, making those whose community supervision was extended due to outstanding debt eligible to vote.

primary contribution of this project is that it offers an answer to the following question: *Is it possible to construct a list of hard to reach people—those with felony convictions who are eligible but not registered to vote—that contains valid contact information and is representative of the underlying population?*

To construct the sample, we draw on administrative data of criminal records.<sup>12</sup> We use publicly available data from North Carolina’s Department of Public Safety (DPS) to identify people convicted of a felony and sentenced to DPS custody (incarceration or supervision) who have completed the terms of their sentence. We then use the publicly-available North Carolina voter file to identify individuals who are already registered to vote and, therefore, not included in the study. We return to the voter file after fielding our trials to observe which subjects registered and voted following the intervention.<sup>13</sup>

Beginning with the dataset of all North Carolinians who had been sentenced to state custody after a felony conviction, we used information from the DPS data to figure out who was currently eligible to register and vote. Appendix Figure A1 displays the steps of that process and the proportion of data lost at each stage relative to the universe of people in the full DPS dataset (N=1,205,971). A large share of people were still under supervision (8.7%) and therefore not eligible to vote, or had been recorded by DPS as deceased (0.2%). We also removed people whose incarceration/supervision status was unclear (63.8%), duplicate observations (0.2%), people over age 70 (2.4%)<sup>14</sup>, individuals without a last name (0.02%), and non-citizens (0.8%).

After narrowing down the dataset to voting-eligible people, we worked with a commercial data vendor (‘data vendor’) to find contact information for as many of them as possible.<sup>15</sup> We drew iterative samples to be matched by the vendor, preparing a new list for each pilot and the main study. We sampled a total of 153,504 records, combined, for pilots 1-3, and successfully matched 36,963 with valid mailing addresses.<sup>16</sup> After excluding records sampled for the pilots, 136,368 eligible records remained, from which we were able to match 35,249

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<sup>12</sup>Other approaches, such as respondent-driven sampling (or snowball sampling) of individuals with criminal records, have at least two shortcomings relative to our approach: (1) They are more labor-intensive, in that they require contacting individuals to elicit information. (2) The sample produced will depend on who researchers contact first, and who responds to their inquiries. Since social networks typically share common attributes (for instance, political engagement), this could introduce important selection bias. While our approach will also produce a selected sample (based on which individuals appear in the commercial data), we expect the result to be more representative. Importantly, the composition of our sample does not depend on the identity of a ‘seed’ respondent or individuals’ willingness to engage.

<sup>13</sup><https://www.ncsbe.gov/results-data/voter-registration-data>

<sup>14</sup>We dropped people over age 70 to avoid unintentionally sending mailers to a large number of people who were deceased.

<sup>15</sup>As part of our agreement, the data vendor asked not to be identified in this study.

<sup>16</sup>In Pilots 2 and 3 we also needed valid phone numbers. This reduced the match rate slightly for those studies.

with valid addresses, our final analysis sample for Study 4.

We consider how data loss at each stage of sample construction impacts the composition of our final sample by using the following demographic information included in the North Carolina DPS records: race, time since the conclusion of one’s supervision, gender, and age. Figures A2 - A5 display how the composition of the sample changes with respect to each characteristic at each point in the process of constructing the sample. Aside from key metrics that change as expected with design choices (e.g. excluding individuals over the age of 70 drops the mean age of the sample from 51 to 47), the composition of the samples used in the pilot and main analyses are not notably different from that of the larger sample of voting-eligible returning citizens in North Carolina.

Race and gender were least affected by the process of data loss. Black individuals made up 42.7% of the full set of returning citizens eligible to vote, 48.7% of the samples pulled for the pilots, and 43.6% of the sample pulled for the main study (Figure A2). Similarly, white individuals made up 49.2, 48.7 and 50.1% of the full, pilot and final samples respectively. In terms of gender composition (Figure A4), the full sample was 22.8% female, as were 20% and 24.9% of the pilot and final study samples. Average time since release from supervision (Figure A3) and average age were more notably impacted, likely due to some of the choices we made in restricting the sample. For the full sample, average time since release was 17.3 years. This declined to 13.4 years in the samples used in the pilots, and 9.1 years in the main study sample. The most pronounced change in time since release occurred when we omitted people whose status was unclear and when we restricted the sample to those who could be successfully matched to a valid mailing address. Average age (Figure A5) declined from 51.3 among the full sample to 47.6 among the pilot samples and 43.9 among the final sample. The greatest drop in average age occurred when we omitted records for people over 70 and when we restricted the sample to those who could be successfully matched to a valid mailing address.

Thus, the composition of the sample randomized for treatment in study 4 was slightly younger and had been released from supervision for fewer years than the overall population of returning citizens who may be eligible to vote. We anticipate, however, that this may bias results away from observing any impact of our treatments, since previous research suggests that the overall likelihood of registering and voting is lower among young people, relative to their older counterparts. Setting aside issues related to age, the iterative process of exclusion leading to data loss described above did not yield a final sample significantly different from the full sample based on available factors. We therefore conclude that we can construct a list of people with felony convictions, who are eligible but not registered to vote, and for whom we can find valid mailing addresses, that is reasonably representative of the

underlying population of eligible but unregistered returning citizens in North Carolina. In the next section, we describe the interventions and findings developed through a series of three pilot experiments, before turning to study 4.

## 4 Pilots: Empirical Strategy and Findings

This section describes a series of three small RCTs, conducted in January, March and June of 2020. The goal of these pilots was to establish a method for identifying, contacting, and mobilizing people with felony convictions who are eligible to vote but not registered. We used what we learned to implement a larger experiment in September 2020, before the general election. Table 1 includes descriptive statistics for the samples used in Studies 1-3, as well as tests for baseline covariate balance across the treatment groups (described below). On average, individuals in Studies 1-3 are 44-46 years old, 72-75% are male, 43-48% are Black, and 54-55% were previously incarcerated, similar to the target population in North Carolina. On average it had been 10-11 years since they were released from prison.<sup>17</sup> For each study, the last column in Table 1 shows the p-value of a joint F-test that the means are different across groups. The baseline characteristics look very similar, and we cannot reject the null hypothesis that they are equal, across groups, for any covariate.

### 4.1 Study 1 (Pilot)

In Study 1 (January 2020) we performed a straightforward test of whether information about voting eligibility and encouragement to register increases registration among those who have previously been in NC DPS custody. We randomly assigned individuals to treatment and control conditions with equal probability. Those assigned to the treatment condition were sent a brief letter detailing the requirements for voting in North Carolina, encouraging recipients to register, and including the state’s registration form and a postage-paid envelope for recipients to use to submit their registration forms. We did not contact people assigned to the control condition.<sup>18</sup>

The top panel of Figure 1 shows the effects of the treatment in Study 1 on voter registration, relative to no intervention. Prior to the intervention, the two groups register at the same rate. However, registrations increase for the treatment group several weeks after

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<sup>17</sup>Readers may wonder why the control means are different across these three studies. This is because the pool of eligible, unregistered voters was changing as people gradually registered to vote over time, and we updated registration status before each new study. Each subsequent study can therefore be thought to target a slightly harder-to-reach group within this already-hard-to-reach population. This may help explain differences in fade-out effects across studies.

<sup>18</sup>All mailers are shown in Appendix B.

our mailers are sent, producing a gap in registration rates between the two groups. The registration gap closes in October, potentially due to extensive voter registration efforts in advance of the election (by November, 10.4% of the control group were registered to vote.)

Regression results are displayed in Table 2. Panel A shows the combined effect of any treatment; Panel B shows effects separately by treatment arm. Since there is only one treatment arm in Study 1 these are the same.

Column 1 of Table 2 shows the effect of the basic mailer on registration as of 30 days after the mailers were sent. At that point, individuals in the treatment group were 1.3 percentage points (186%,  $p < 0.01$ ) more likely to be registered to vote than individuals in the control group.<sup>19</sup> However, Column 2 indicates that by November 2020 there was no difference in registration.<sup>20</sup> A similar pattern holds with respect to turnout (displayed in Columns 1 and 2 of Table 3). Individuals in our treatment group were 0.2 percentage points (100%,  $p < 0.10$ ) more likely to vote in NC’s March primary, though there is no significant difference in turnout in the November 2020 election.<sup>21</sup>

## 4.2 Study 2 (Pilot)

Study 2 (March 2020) included three arms: (1) a control group, (2) a group who received the basic mailer (replicating the first study), and (3) a group who received the basic mailer followed by a text message. The goal of this study was to determine whether the follow-up text message meaningfully increased registration, relative to the mailer alone. This sample was restricted to individuals for whom we found valid mailing addresses as well as phone numbers. The middle graph in Figure 1 shows the effect of this study’s interventions. The control group is registering at slightly higher rates than the treatment groups (though statistically indistinguishable) prior to the interventions, but we, again, see a jump in registrations a couple weeks after our mailers are sent for both treatment groups. This produces a gap in registration rates that persists but is no longer statistically significant by November 2020.

Regression results for Pilot 2 are shown in Columns 3 and 4 of Table 2. Panel A shows the combined effect of both treatments. Being assigned to either treatment group increased registration by 0.2 percentage points (10%, n.s.) in the first 30 days and 0.8 percentage points (10%, n.s.) by November 2020.<sup>22</sup> Column 3 of Panel A in Table 3 shows the effect of any

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<sup>19</sup>As of 30 days after the mailers were sent, 0.7 percent of the control group had registered to vote. All control group means are at the bottom of the relevant tables.

<sup>20</sup>8.7 percent of the control group registered to vote by November 2020.

<sup>21</sup>0.2 percent of the control group voted in the March 2020 primary, and 5.6 percent voted in the November 2020 election.

<sup>22</sup>2.1 percent of the control group registered to vote by 30 days after the mailers were sent; 8.3 percent registered by November 2020.

treatment on turnout in the November election. There is a positive coefficient (0.7 percentage points, 13% of the control group mean), but the effect is not statistically significant.<sup>23,24</sup>

Panel B of Tables 2 and 3 show the effects of each treatment arm separately, for registration and turnout respectively. On average receiving a follow-up text message after the mailer increased the treatment effect on registration slightly at the 30-day mark, but this effect was statistically insignificant and was smaller than the mailer-alone effect by November. The effects of each treatment arm on voter turnout are almost identical. We interpret these results as suggesting that adding a text message did not meaningfully improve our treatment effects.

### 4.3 Study 3 (Pilot)

In Study 3 (June 2020), we tested the impact of partnering with a local organization already involved in voter outreach, since some research suggests that members of marginalized groups are more receptive to organizations rooted in their communities (Sinclair, McConnell and Michelson, 2013; Michelson, 2006). We partnered with a North Carolina organization, You Can Vote (YCV), to refine the text of our mailer and craft the treatments. You Can Vote wished to execute follow up calls. Our third pilot therefore included four treatment arms: (1) a control group, (2) a group who received a basic mailer without YCV branding (again replicating the first study), (3) a group who received the mailer with YCV branding, and (4) a group who received both the YCV-branded mailer and a follow-up call from YCV staff and volunteers.<sup>25</sup>

The bottom graph in Figure 1 shows the effect of the Study 3 interventions. Registration rates are similar for all four groups prior to the interventions. However, registrations increase for all four treatment groups a couple weeks after our mailers are sent. This increase produces a persistent gap in registration rates between the treatment and control groups. This time, all three treatment groups perform similarly, and the gap between the treatment and control groups is still visible.

Column 5 of Panel A in Table 2 shows the effect of any treatment on voter registration after 30 days. At that point, individuals in a treatment group were, on average, 0.9 percentage points (82%,  $p < 0.01$ ) more likely to be registered to vote than those in the control group.<sup>26</sup> Column 6 shows the treatment effect as of November 2020; the coefficients suggest

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<sup>23</sup>5.5 percent of the control group voted in the November 2020 election.

<sup>24</sup>Note that Study 2 was conducted about a month after the federal government declared a national emergency due to COVID-19. People who received mailers may have been distracted by these events, reducing the effect of our intervention.

<sup>25</sup>All mailers are shown in Appendix B.

<sup>26</sup>1.1 percent of the control group registered to vote by 30 days after the mailers were sent.

an average 0.7 percentage point increase (8% of the November control group mean), but this effect is not statistically significant.<sup>27</sup>

Column 4 of Panel A in Table 3 shows the effect of any treatment on turnout in the 2020 general election. The coefficient is positive (0.4 percentage points, 6%), but not statistically significant.<sup>28</sup>

Panel B in Tables 2 and 3 show the breakdown of these effects by treatment arm. Effects on voter registration appear slightly larger at the 30-day mark when using the YCV-branded mailer, but there was no additional benefit from adding a phone call. By November those effect sizes had fallen slightly, and overall our basic mailer and the branded mailers appear to have performed similarly. The effect on turnout appears to be slightly larger for our basic mailer than the two branded-mailer arms, but differences are not statistically significant.

## 5 Main Study: Empirical Strategy and Findings

In September 2020 we fielded Study 4—the scaled-up main study—in North Carolina during the lead up to the November 2020 general election.<sup>29</sup> This study allowed us to observe experimental effects on both registration and voter turnout in a high-turnout general election, using a larger sample than in any of the previous trials. Results from the pilots informed the design of Study 4. Because they did not appear to provide any meaningful benefit, we dropped the expensive and logistically-challenging text message and phone call treatments, focusing instead on mailers. We maintained our partnership with YCV for the mailer branding, as YCV-branded mailers did not cost any more than our basic mailers, and our third pilot provided suggestive evidence that YCV-branding increased the efficacy of the intervention slightly (as of the 30-day results we had when we made our decision).

Across all pilots, our intervention appeared effective, at least in the short term. However, the mailers used in the pilots were a “package” of several components. We designed Study 4 both to provide greater statistical power than the pilots and to investigate the

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<sup>27</sup>8.6 percent of the control group registered to vote by November 2020.

<sup>28</sup>6.0 percent of the control group voted in the November 2020 election.

<sup>29</sup>As noted above, this study included a planned component in Texas. Power calculations used to develop the overall sample size, particularly with respect to intended subgroup analysis, included the Texas sample. We fielded the same treatments we detail below in the state. However, post-treatment we discovered a coding error that affected the entire design in Texas. We inadvertently included a large number of people we did not intend to target: people who were already registered, who did not have felony convictions, or whose voting eligibility was uncertain. Moreover, omitting individuals we did not intend to treat introduced some imbalance across treatment groups on gender that suggests our results may be confounded by unobservable differences across groups. This issue, in addition to implementation problems during the process of mailing out treatment letters (mailers landed in mailboxes later than we intended — and perhaps after registration deadlines), led us to relegate analyses of the Texas data to the Appendix. We detail the issues faced and present findings with respect to registration in Appendix C.

relative effectiveness of the constituent elements of the mailer. These components include specific messaging about eligibility for people with felony convictions, a registration form and pre-paid, pre-addressed return envelope, and additional messaging encouraging people to participate. Study 4, therefore, includes the following five treatment groups:

1. a control group that does not receive any kind of treatment;
2. the basic mailer package (mailer and registration form with pre-addressed, postage-paid envelope), signed by a local non-profit organization (basic mailer);
3. the basic mailer package, without highlighted information about eligibility among people with felony convictions (no criminal record framing);<sup>30</sup>
4. the basic mailer, with no registration form or pre-addressed, postage-paid envelope (no registration form); and
5. the basic mailer package, with additional messaging about how issues related to civil rights are on the ballot and the importance of voting (extra civil rights framing).<sup>31</sup>

All mailers are shown in Appendix B. We randomly assigned individuals in our Study 4 sample across these five groups, with equal probability.

Table 4 shows descriptive statistics for Study 4 (described in Section 5 below), and tests for covariate balance. In Study 4, individuals in our sample are about 44 years old; 75% are male, 43% are Black, 57% had been incarcerated, and on average, it had been about 9.5 years since their release. The last column shows p-values of joint F-tests that covariate means are different across treatment and control groups. We cannot reject the null hypothesis that means are equal across groups, for any covariate.<sup>32</sup>

## 5.1 Study 4: Findings

Figure 2 shows a raw data plot of voter registration rates across treatment arms through time. Across all groups there is a clear upward trend in voter registrations during the months

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<sup>30</sup>Note that these mailers still included a list of eligibility criteria, including information relevant to those with criminal records. But they do not include an opening paragraph highlighting this information.

<sup>31</sup>The closing paragraph of the “extra civil rights framing” mailer reads as follows: “Criminal Justice and Civil Rights are on your ballot. Members of Congress and the state legislature decide what is a crime and how it should be punished. They make rules on how our courts, prisons, and jails are managed and how people should be treated when they are in custody. Judges decide who gets detained and for how long, and who goes to prison and for how long. Elected officials have an impact on how equal protection is enforced and are responsible for ensuring freedom of speech, assembly and religion, and specific rights including voting rights. Find out what’s on your ballot and *why your vote matters* at [url].”

<sup>32</sup>We present specifications with and without baseline covariates included; controlling for them makes little difference.

leading up to the election. In early October, when our mailers land in mailboxes, we see a differential jump in registrations for those in all treatment arms, relative to those in the control group. The gaps between these groups remain until the November 2020 election.

Figure 3 shows a coefficient plot of the treatment effect of being sent a mailer, by week. In this figure, all treatment arms are pooled and compared with the control group. The dashed vertical line shows the week that mailers were scheduled to land in mailboxes. We see an immediate jump in voter registrations during the first two weeks after the mailer landed, after which the effect returns to zero. Figure A6 presents the effects separately for each treatment arm, which all show a similar pattern.

Table 5 shows regression results. First we consider the combined effect of any treatment, relative to the control group; these results are shown in Panel A. Column 1 shows that sending any mailer increased registration by November 2020 by 0.8 percentage points (12%,  $p < 0.05$ ).<sup>33</sup> Column 2 shows that controlling for covariates has no effect on this estimate.

Columns 3 and 4 show the effect of any treatment on voter turnout, without and with covariates respectively. We find that sending any mailer increased voter turnout by 0.5 percentage points (11%,  $p < 0.10$ ).<sup>34</sup> This implies that a substantial fraction of people who were induced to register by our treatment mailers ultimately voted in the next election.

Panel B of Table 5 presents these results separately by treatment arm. We focus on Columns 1 and 3 (estimates without covariate controls), but estimates with controls (Columns 2 and 4) are nearly identical.

Column 1 shows the effect on voter registration. The basic mailer increased voter registration by 0.8 percentage points (12%,  $p < 0.10$ ). The mailer with no criminal record framing increased registration by 1.1 percentage points (17%,  $p < 0.01$ ). The mailer with no registration form increased registration by 0.8 percentage points (12%,  $p < 0.10$ ). And the mailer with extra civil rights framing increased registration by 0.6 percentage points (9%, n.s.). We do not have sufficient statistical power to reject that all four mailers had equal effects.

Column 3 shows the mailers' effects on voter turnout in the November 2020 general election. The basic mailer increased voter turnout by 0.8 percentage points (17%,  $p < 0.05$ ). The mailer with no criminal record framing increased voter turnout by 0.7 percentage points (15%,  $p < 0.10$ ). The mailer without a registration form increased turnout by 0.3 percentage points (7%, n.s.). The mailer with extra civil rights framing increased voter turnout by 0.4 percentage points (9%, n.s.). Again, we cannot reject the null hypothesis that these mailers all had equal effects.

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<sup>33</sup>6.6 percent of the control group registered to vote by November 2020.

<sup>34</sup>4.6 percent of the control group voted in the November 2020 election.

The lack of significant differences across these treatment arms was somewhat surprising. Based on conversations with our local partners as well as the prior literature, we expected that the criminal record framing and extra civil rights framing would both increase effect sizes. The point estimates suggest no or even detrimental effects. Similarly, we expected that including a registration form would increase effect sizes, by reducing the time/logistical costs of registration. It did not, though that is perhaps because individuals could register online in North Carolina. This treatment arm might have done better in states where an online option was not available.

We interpret these results as suggesting that, at least on average, simply contacting people in this target group and providing basic information about the eligibility requirements, was enough to increase voter registration and turnout.

## 6 Auxiliary Analyses

### 6.1 Heterogeneous effects across subgroups

We are interested in the extent to which our treatment effects vary with baseline characteristics of the people in our sample – particularly race. Because criminal justice contact disproportionately affects Black Americans, the negative effect of that contact on civic engagement is likely larger for that group. Since our intervention seeks to counter this negative effect, we want to know whether the impact of our intervention varies with race.

We focus our attention on people who are coded as Black or white – the vast majority of our sample. Figure A7 shows raw registration data over time, separately for each group. Based just on these raw data we can see suggestive evidence of racial differences: the basic mailer (T2) appears to work better for Black recipients, while the no criminal record framing mailer (T3) appears to work better for white recipients.

Table 6 presents regression results, showing how the treatment effects in Study 4 vary with race. Columns 1 and 2 present the effects for Black and white individuals, separately. Column 3 combines these groups and interacts “Black” with the treatment indicator to formally test for differential effects by race. Panel A shows the combined effects of any treatment; Panel B shows effects separately by treatment arm.

The results suggest substantial differences in the effectiveness of the mailers across racial groups. We see consistently large and positive treatment effect estimates for white mailer recipients across the four different types of mailers. The point estimates among Black mailer recipients are smaller, not always positive, and never statistically distinguishable from zero.<sup>35</sup>

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<sup>35</sup>These differences are not simply due to a lack of statistical power or higher baseline rates of registration

In Panel A we see that being in any treatment group increased voter registration by 0.3 percentage points (5%, n.s.) for Black individuals vs. 1.3 percentage points (18%,  $p < 0.05$ ) for White recipients.<sup>36</sup> This difference is not statistically significant, but it is certainly striking.

Turning to Panel B: Our basic mailer increased voter registration for Black individuals by 0.7 percentage points (11%) vs. 0.8 percentage points (11%) for white individuals. The mailer with no criminal record framing increased registration by 0.2 percentage points (3%) for Black individuals vs. 1.8 percentage points (25%) for white individuals. The mailer with no registration form increased registration by 0.3 percentage points (5%) for Black individuals vs. 1.3 percentage points (18%) for White individuals. And the mailer with extra civil rights framing *reduced* registration by 0.1 percentage points (2%) for Black individuals vs. a 1.2 percentage point (17%) increase for White individuals. As shown in column 3, only the difference for the “no criminal record framing” mailer is significant, but we view all of these results as a red flag on an important issue: well-meaning interventions may exacerbate racial disparities in civic engagement rather than reduce them. It may be that marginal Black registrants are more difficult to mobilize than marginal white registrants, or that these groups will respond differently to different interventions. (Indeed, our basic mailer performed equally well across these groups; the gap is driven by the other mailer types.)

In addition to these differential effects by race, we test for heterogeneous effects using a machine-learning approach developed by Wager and Athey (2018) and applied in Davis and Heller (2017). The goal of this approach is to identify subgroups with larger treatment effects in a principled way that minimizes concerns about data mining. This allows us to consider subgroups that more standard binary comparisons might miss (for instance, Black men in their 40s with a history of incarceration). The approach uses separate training and testing samples: we randomly selected a portion of the sample to be excluded from the training data, and use it to test the predictions made based on the training sample. In this way, we use machine learning to generate hypotheses about which subgroups are most affected by our mailers, then test those hypotheses in the holdout sample. This helps us avoid concerns about overfitting and multiple hypothesis testing. Because we draw the sample from administrative data maintained by DPS, we have relatively complete information on a number of relevant background characteristics. We use the following characteristics to examine heterogeneity: gender, race/ethnicity, past incarceration, past supervision, age (binned into quintiles), and time since release (binned into quintiles; missing for people never incarcerated).

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among Black mailer recipients: Column 1 has a similar number of observations as Column 2, and the control-group registration rates differ by less than one percentage point.

<sup>36</sup>6.3 percent of Black control group members and 7.1 percent of white control group members registered to vote by November 2020.

Table A1 shows the results of this analysis. This table divides individuals into four bins of predicted treatment effects (from most negative to most positive), then shows the mean characteristics of people in each bin. For instance, the first bin has a predicted treatment effect of -0.03 – that is, the mailers reduced voter registration by 3.0 percentage points (45% of the control group mean, 6.6 percent). The second predicted treatment effect bin had an average treatment effect of -0.1 percentage points (1.5%), the third bin had an average treatment effect of 2.0 percentage points (30%), and the fourth bin had an average treatment effect of 5.0 percentage points (75%). Those in the highest treatment effect bin – where we see the biggest positive effects on voter registration – are, on average, more likely to be male and more likely to have a history of incarceration. We do not observe such clear patterns when it comes to age and time since release, although individuals in the highest treatment effect bin are less likely to be over the age of 55. We also don’t see clear patterns by race/ethnicity, except that those in the lowest treatment effect bin (with a negative treatment effect, on average) are more likely to be Black.

## 6.2 Treatment effects vs. mailing address quality

Our estimated treatment effects are a function of (1) the likelihood that the intended recipient received our mailer, and (2) the effect of the mailer (if received) on the recipient’s behavior. We wondered whether the differential effects discussed above might be due to differences in our ability to deliver the mailers, rather than differences in how people responded to them. If the address data from our data vendor varied in quality across race, for instance, we might see different effects across racial groups simply because some people never received the mailers we tried to send them.

To explore this possibility, we ran a small followup study. We mailed postcards to the sample from Study 4 and used postal-service tracking tools to observe whether the postcards were successfully delivered. This allowed us to assess the quality of the addresses we obtained from our data vendor. (Note that we sent these postcards in the summer of 2021. People may have moved during the year between our main study and this follow-up postcard study, so the results likely underestimate the accuracy of addresses at the time of the main study.)

Table A2 shows how demographic characteristics correlated with whether the postcard “bounced” (that is, whether it was not successfully delivered). We interpret a bounce as an indicator of having an incorrect address. Overall, 86% of the postcards were successfully delivered — a high success rate for a population that is relatively transient and difficult to reach. Postcards were less likely to bounce if they were sent to men. The probability of bouncing increased with the intended recipient’s age and decreased with their time since

release. Race does not predict whether a postcard bounced. It thus appears that the racial disparities in our estimates, described above, are not driven by racial differences in address quality.

### 6.3 Comparison to people without criminal records

To provide context for our treatment effects, and to address whether our approach is particularly effective at mobilizing those with felony convictions, we construct a comparison sample of people without felony convictions who live in “high-incarceration neighborhoods” in North Carolina. Conducting a parallel experiment with non-registered individuals who do *not* have criminal records but are otherwise similar (socioeconomically) to those in our analysis sample helps us interpret the magnitude of our estimates. Are our results about what we would expect for an economically- and socially-vulnerable population, or does the criminal record itself predict the efficacy of our intervention?

There are at least two reasons that the criminal record itself might matter. If people with criminal records are not targeted by existing outreach efforts, or if misinformation about how a past conviction affects eligibility is suppressing registration, then our effects might be *larger* than what we see for a similar population without records. Alternatively, given the low baseline rates of registration among returning citizens, both before and after conviction, and the demobilizing effect of carceral contact demonstrated in the extant literature, returning citizens may be *less* responsive to registration and mobilization efforts such as ours. Our no-criminal-record (no-CR) comparison group allows us to investigate these hypotheses.

To construct this comparison sample, we identified zip codes with high concentrations of people with felony convictions, and we drew a sample of people (from the data vendor address database) who were neither in the DPS dataset nor registered to vote. The top six zip codes yielded enough residents for the comparison group, which was spread across six cities of various sizes: Charlotte, Raleigh, Greenville, Greensboro, Gastonia, and Winston-Salem. These 35,708 individuals were randomized into either a control condition or one treatment condition. Those in the treatment group were sent our basic mailer and registration package, omitting information specific to people with felony convictions (treatment arm 2 in the main study). Table A3 shows descriptive statistics for this comparison group, based on the limited information provided by the data vendor. On average they are 44.7 years old. When we impute race based on name, we find that about 10% are Black and 8% are Hispanic. Joint F-tests cannot reject the null hypothesis that our treatment and control groups are balanced on these characteristics.

Table 7 shows the results. Column 1 shows the effect of our treatment on voter reg-

istration for the no-CR group. The coefficient is near-zero and statistically insignificant.<sup>37</sup> Column 2 shows the treatment effect from the comparable treatment arm in Study 4; our mailers increased voter registration among people with felony convictions by 1.1 percentage points (16%;  $p < 0.05$ ).<sup>38</sup> Since our comparison sample is drawn from urban areas, we consider whether this is simply an urban-rural difference. Column 3 shows the Study 4 treatment effects in urban areas only; it is very similar to the overall Study 4 effect, though statistically insignificant due to limited power.<sup>39</sup> Column 4 formally tests whether the difference between the estimates in columns 1 and 2 is statistically significant; the treatment effect for people with criminal records is indeed significantly larger than the effect for the no-CR group.

Columns 5-8 of table 7 show the effects on turnout in the November 2020 general election. Column 5 indicates that our treatment had no effect on turnout among the no-CR group.<sup>40</sup> The comparable treatment effect from Study 4, for individuals with a felony conviction, is 0.7 percentage points (15%;  $p < 0.10$ ).<sup>41</sup> As shown in Column 8, the difference in the effects across these groups is marginally significant.

In sum, our intervention only affects those with a felony record, not similarly-situated individuals without felony records. We interpret this as evidence that (1) we are reaching a population that is not reached by standard outreach methods (even though they live in the same neighborhoods), and/or (2) our mailers are more effective for people with felony records than they are for similar people without felony records (perhaps because of baseline differences in knowledge about whether they are eligible to vote).

## 6.4 Party of registration

Our experiment was intentionally non-partisan. We did not set out to register people with particular political leanings. We also worked with community organizations that were non-partisan in their outreach efforts. At the same time, readers may be curious about the potential downstream political consequences of such outreach efforts. While much punditry anticipates that returning citizens are likely Democratic voters, little substantive research supports this proposition.<sup>42</sup> We thus consider the party that individuals registered with, among those who registered to vote.

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<sup>37</sup>4.8 percent of the relevant control group registered to vote by November 2020.

<sup>38</sup>6.9 percent of the relevant control group registered to vote by November 2020.

<sup>39</sup>7.2 percent of the relevant control group registered to vote by November 2020.

<sup>40</sup>3.7 percent of the relevant control group voted in the November 2020 election.

<sup>41</sup>4.8 percent of the relevant control group voted in the November 2020 election.

<sup>42</sup>To the extent that researchers have found potential partisan consequences of returning citizens participating in elections, these consequences are indirect, where partisanship is shaped by other factors like race and income (Manza and Uggen, 2008; Morse, 2021; Burch, 2011).

Table [A4](#) shows the number of people registering with each party. Column 1 shows the number of new registrants by party, for those assigned to a treatment group from our Study 4 sample. Column 2 shows the equivalent numbers for those assigned to the control group in Study 4. Columns 3 and 4 show the numbers for people from our no-CR comparison group. Overall the distribution of party registrations seems similar for the treatment and control groups in each sample. That is, it appears that our intervention was not disproportionately effective for people inclined to vote for one party over another. These numbers also tell us about the political leanings of people with felony convictions. In North Carolina, 36% of new registrants registered as Democrats, 35% registered as Republicans, 0.7% registered as Libertarians, and 28% registered as unaffiliated.

## 7 Discussion and Conclusion

Public discourse about increasing the civic engagement of people with criminal records typically focuses on expanding voting rights in places where these individuals are currently not eligible to vote. However, millions of people with felony convictions are already eligible to vote ([Manza and Uggen, 2008](#)). In fact, individuals with felony convictions regain their rights at some point in the vast majority of states. Participation rates for this group are low and traditional mobilization campaigns tend not to prioritize returning citizens, because they are hard to reach and may lack valid mailing addresses ([Owens and Walker, 2018](#)). Our findings suggest that, if mobilized, over 100,00 of the returning citizens that were re-enfranchised between 2016 and 2020 could become active voters.

At the same time, a nascent line of research suggests that, under the right circumstances, people with felony convictions can be politically mobilized. Targeting people with felony convictions who are already registered to vote is a potentially effective way to expand the electorate. In this project, we asked: can we identify returning citizens who are nevertheless eligible to vote, find them, contact them, and convert them into active, registered voters? With a combination of administrative and private data we developed a method for identifying difficult-to-reach potential voters, and show through a series of randomized control trials that it is possible to increase registration and voting among this population.

One contribution of this project is the method by which we constructed the sample. We were able to use publicly-available administrative data and voter files to identify members of the population of interest, their voting eligibility status, and whether they were already registered. A data vendor enabled us to find valid mailing addresses. We can imagine this process being useful for research on mobilizing other difficult-to-reach or under-mobilized populations, including those with other types of contact with the criminal legal system or

transient populations.

The other contribution of this study is the experimental results, which show that a light-touch, mail-based intervention increases voter registration and turnout for people with past felony convictions. We find suggestive evidence that our treatment effects vary across demographic groups and with the content of our mailers. It may be that learning about eligibility requirements and the registration process is particularly costly for people with past criminal justice contact; reducing these costs (with a simple mailer, or in other ways) can thus be effective. Our findings may be of interest to nonprofits and campaigns in addition to researchers, as they point to potential cost savings for these organizations—a simple mailer providing useful information appears to be as or more effective than a more lengthy mailer or phone calls and text messages.

There are many outstanding questions. In particular, we hope that future research further investigates which types of interventions work for different groups, as efficacy is likely to vary, particularly as it pertains to race. Our treatments were most effective among returning citizens who are white and male. By what means can we more effectively contact and mobilize voters of color, who make up a disproportionate share of people impacted by the criminal justice system? Future research may also probe the partisan dynamics of these types of interventions. A potential concern when it comes to restoring rights and mobilizing returning citizens is whether such an expansion of the electorate might change electoral outcomes. Our results suggest that the partisan consequences of criminal justice involvement may not be as straightforward as is often assumed. The form that the voice of returning citizens might take, and the kinds of issues around which individuals might coalesce, is an outstanding question. Nevertheless, our research indicates that returning citizens are a latent political force that can be activated.

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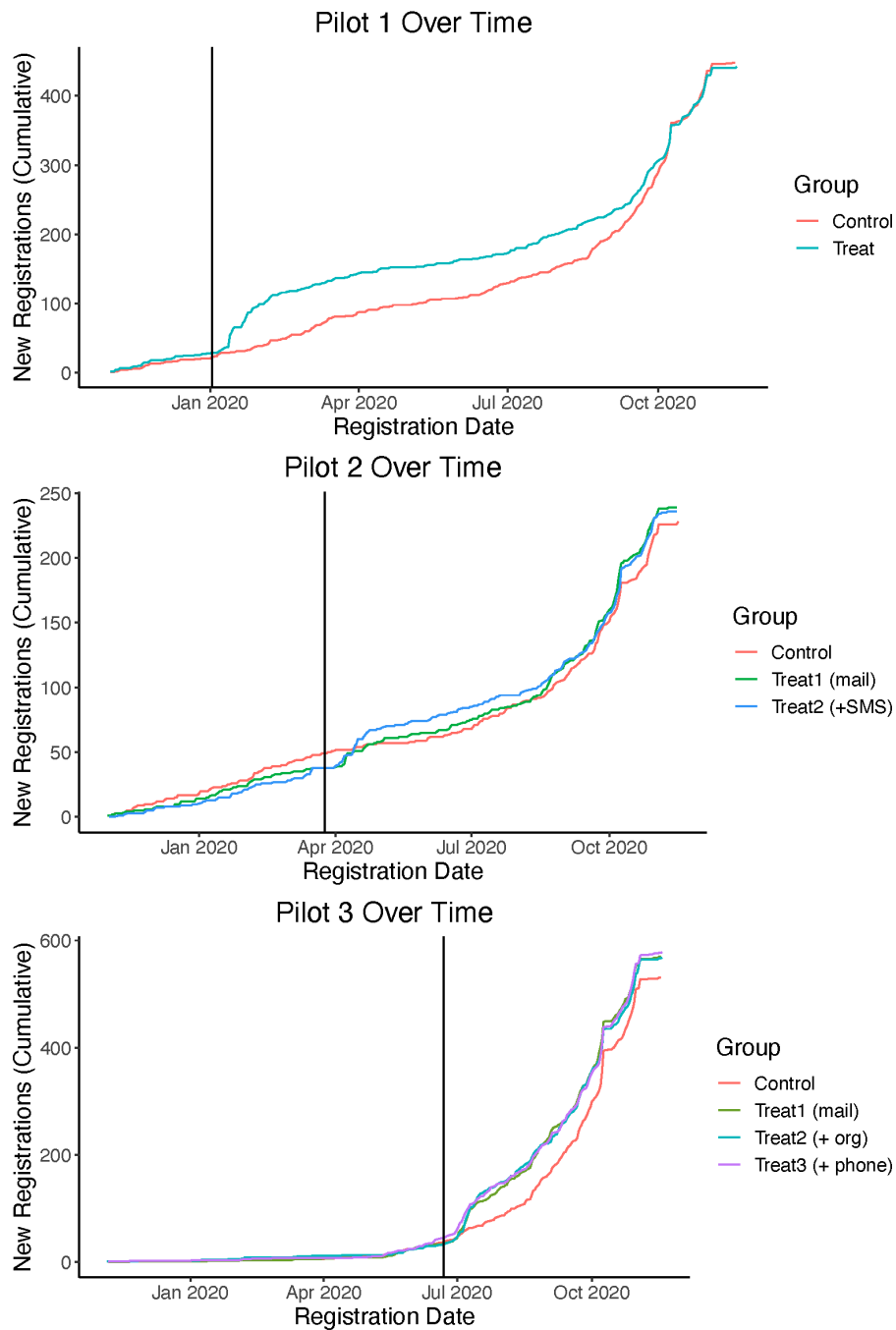
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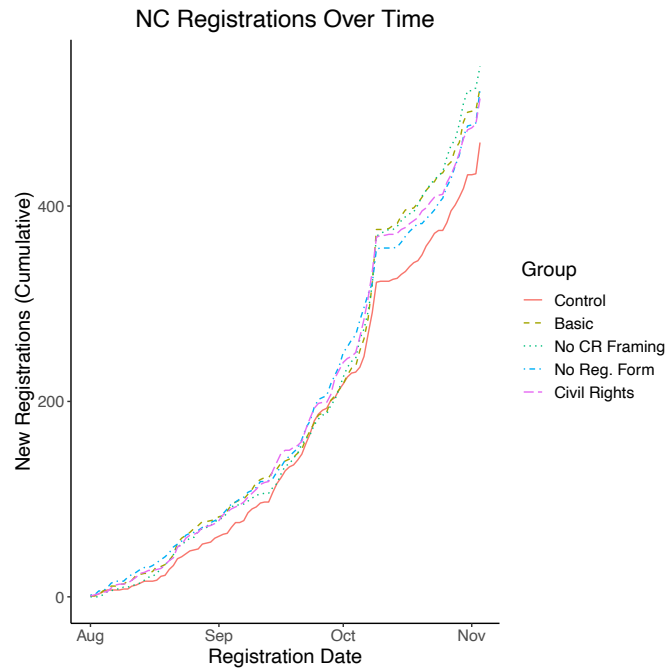
## 8 Figures and Tables

Figure 1: Registrations over time: Studies 1-3



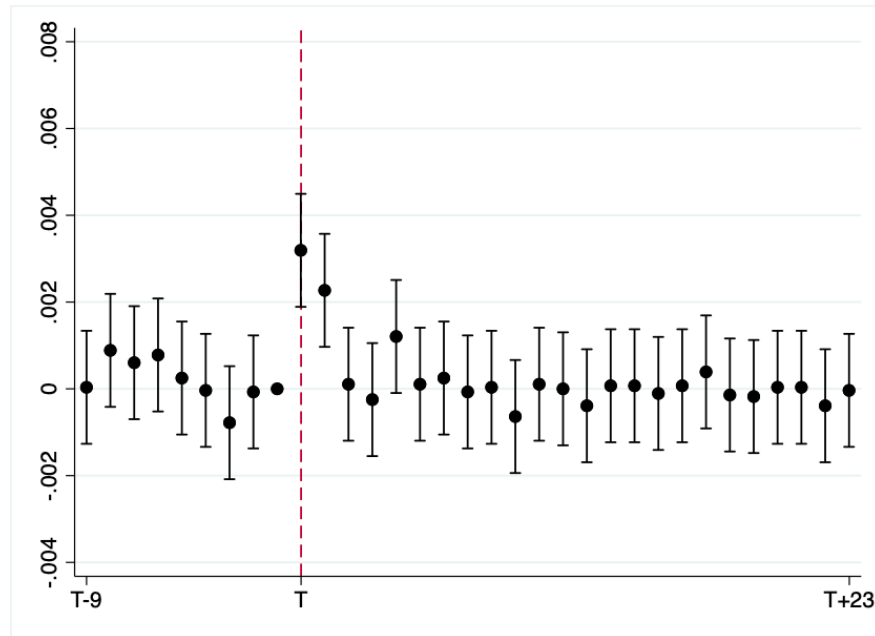
Notes: This figure plots cumulative new registrations in each treatment and control group over time.

Figure 2: Registrations over time: Study 4



Notes: This figure plots cumulative new registrations in each treatment and control group over time.

Figure 3: Treatment effects by week



(a) North Carolina

Notes: This figure shows treatment effects of sending a mailer (all treatment arms combined), relative to the control group. The x-axis shows the week since mailers were scheduled to arrive in mailboxes. The y-axis shows effect on registering to vote.

Table 1: Studies 1-3: Covariate balance across treatment arms

	Study 1			Study 2				Study 3				
	Control mean	Difference T1	F-test p-val	Control mean	Difference T1	Difference T2	F-test p-val	Control mean	Difference T1	Difference T2	Difference T3	F-test p-val
Male	0.75	0.01 (0.01)	0.42	0.72	-0.01 (0.01)	-0.01 (0.01)	0.78	0.74	0.00 (0.01)	-0.01 (0.01)	0.00 (0.01)	0.63
Age	45.81	-0.24 (0.25)	0.34	44.13	0.28 (0.33)	0.20 (0.33)	0.68	43.85	0.23 (0.22)	0.13 (0.22)	0.13 (0.22)	0.76
Black	0.47	0.00 (0.01)	0.84	0.48	-0.01 (0.02)	-0.00 (0.02)	0.65	0.43	0.00 (0.01)	0.01 (0.01)	0.01 (0.01)	0.56
Past Incarc.	0.54	0.00 (0.01)	0.96	0.55	-0.00 (0.02)	0.02 (0.02)	0.32	0.54	0.00 (0.01)	0.00 (0.01)	0.01 (0.01)	0.70
Days since release	4197.44	-20.84 (100.97)	0.84	3901.99	-6.31 (134.20)	-126.79 (132.92)	0.56	3590.60	63.33 (86.63)	5.96 (86.55)	25.90 (86.25)	0.88
Observations	4310	4311		2194	2195	2195		5441	5441	5440	5441	

**Notes:** This table shows descriptive statistics and tests of covariate balance for the three pilot studies (Studies 1-3). For each study, the first column shows the control group mean for each covariate at baseline, the next columns show differences between that control group mean and the mean for each treatment group, and the last column shows the p-value from a joint F-test testing that the group means are different.

Table 2: Studies 1-3: Short- and Longer-Term Effects on Voter Registration

	<i>Dependent variable:</i>							
	30 days	November	30 days	November	30 days	November	30 days	November
	Pilot 1		Pilot 2		Pilot 3		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A: All Arms Combined</b>								
Treatment (Any mailer)	0.013*** (0.003)	−0.003 (0.006)	0.002 (0.004)	0.008 (0.007)	0.009*** (0.002)	0.007 (0.005)	0.009*** (0.002)	0.004 (0.003)
<b>Panel B: Separate Treatment Arms</b>								
Basic Mailer	0.013*** (0.003)	−0.003 (0.006)	−0.0005 (0.004)	0.010 (0.009)	0.007*** (0.003)	0.008 (0.006)	0.008*** (0.002)	0.004 (0.004)
Basic Mailer + Text			0.005 (0.004)	0.006 (0.009)			0.009** (0.004)	0.004 (0.008)
Branded Mailer					0.010*** (0.003)	0.006 (0.006)	0.011*** (0.002)	0.005 (0.005)
Branded Mailer + Call					0.010*** (0.003)	0.007 (0.006)	0.010*** (0.002)	0.005 (0.005)
Study Fixed Effects							X	X
Control Group Mean	0.007	0.087	0.021	0.083	0.011	0.086	0.01	0.083
Observations	8,621	8,621	6,584	6,584	21,763	21,763	36,968	36,968

*Notes:* This table shows the short and long term effect of each treatment (relative to the control), as well as pooled treatment arms relative to control, on voter registration. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 3: Studies 1-3: Effects on Voter Turnout

	<i>Dependent variable:</i>				
	Voted in March 2020	Voted in November 2020			
	Study 1	Study 1	Study 2	Study 3	All Studies
	(1)	(2)	(3)	(4)	(5)
<b>Panel A: All Arms Combined</b>					
Treatment (Any mailer)	0.002* (0.001)	-0.0002 (0.005)	0.007 (0.006)	0.004 (0.004)	0.003 (0.003)
<b>Panel B: Separate Treatment Arms</b>					
Basic Mailer	0.002* (0.001)	-0.0002 (0.005)	0.007 (0.007)	0.007 (0.005)	0.004 (0.003)
Basic Mailer + Text			0.008 (0.007)		0.007 (0.006)
Branded Mailer				0.001 (0.005)	-0.0003 (0.004)
Branded Mailer + Call				0.004 (0.005)	0.003 (0.004)
Study fixed effects					X
Control Group Mean	0.002	0.056	0.055	0.06	0.055
Observations	8,621	8,621	6,584	21,763	36,968

*Notes:* This table shows the effect of each treatment (relative to the control), as well as pooled treatment arms relative to control, on voter turnout. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 4: Study 4: Covariate balance across treatment arms

	Control Mean	Difference from Control				Joint F-test p-val
		Basic Mailer	No CR Framing	No Reg. Form	Civil Rights Framing	
Male	0.75	0.00 (0.01)	-0.00 (0.01)	0.00 (0.01)	-0.00 (0.01)	0.89
Age	44.32	0.17 (0.12)	0.08 (0.12)	0.08 (0.12)	0.17 (0.12)	0.62
Black	0.43	0.01 (0.01)	0.00 (0.01)	0.00 (0.01)	0.02 (0.01)	0.18
Past Incarc.	0.57	0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	0.56
Days since release	3474.65	3.27 (75.51)	58.59 (75.84)	-49.04 (75.83)	16.84 (75.72)	0.73
Observations	7049	7049	7049	7049	7049	

**Notes:** This table shows descriptive statistics and tests of covariate balance for the main study (study 4). The first column shows the control group mean for each covariate at baseline, the next columns show differences between that control group mean and the mean for each treatment group, and the last column shows the p-value from a joint F-test testing that the group means are different.

Table 5: Study 4: Effects on Voter Registration and Turnout

	<i>Dependent variable:</i>			
	Voter Registration		Voted November 2020	
	(1)	(2)	(3)	(4)
<b>Panel A: All Arms Combined</b>				
Any Treatment	0.008** (0.003)	0.008** (0.003)	0.005* (0.003)	0.005* (0.003)
<b>Panel B: Separate Treatment Arms</b>				
Basic mailer	0.008* (0.004)	0.008* (0.004)	0.008** (0.004)	0.007** (0.004)
No criminal record framing	0.011** (0.004)	0.011** (0.004)	0.007* (0.004)	0.007* (0.004)
No registration form	0.008* (0.004)	0.008* (0.004)	0.003 (0.004)	0.003 (0.004)
Extra civil rights framing	0.006 (0.004)	0.006 (0.004)	0.004 (0.004)	0.004 (0.004)
Control Mean	0.066	0.066	0.046	0.046
Covariates		X		X
Observations	35,245	35,245	35,245	35,245

*Notes:* This table shows the effect of each treatment (relative to the control), as well as pooled treatment arms relative to control, on voter registration by November 2020 and subsequent turnout. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 6: Study 4: Racial Heterogeneity

	<i>Dependent variable:</i>		
	Voter Registration		
	Black	White	Both
	(1)	(2)	(3)
<b>Panel A: All Arms Combined</b>			
Treatment (Any mailer)	0.003 (0.005)	0.013** (0.005)	0.013*** (0.005)
Black			-0.008 (0.006)
Treatment * Black			-0.010 (0.007)
<b>Panel B: Separate Treatment Arms</b>			
Basic mailer	0.007 (0.006)	0.008 (0.006)	0.008 (0.006)
No criminal record framing	0.002 (0.006)	0.018*** (0.006)	0.018*** (0.006)
No registration form	0.003 (0.006)	0.013** (0.006)	0.013** (0.006)
Extra civil rights framing	-0.001 (0.006)	0.012* (0.007)	0.012* (0.006)
Black			-0.008 (0.006)
Basic mailer * Black			-0.001 (0.009)
No criminal record framing * Black			-0.016* (0.009)
No registration form * Black			-0.010 (0.009)
Extra civil rights framing * Black			-0.013 (0.009)
Control Mean	0.063	0.071	0.067
Observations	15,280	17,694	32,974

*Notes:* This table shows the effect of the treatment (sending a mailer) on voter registration by race group. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

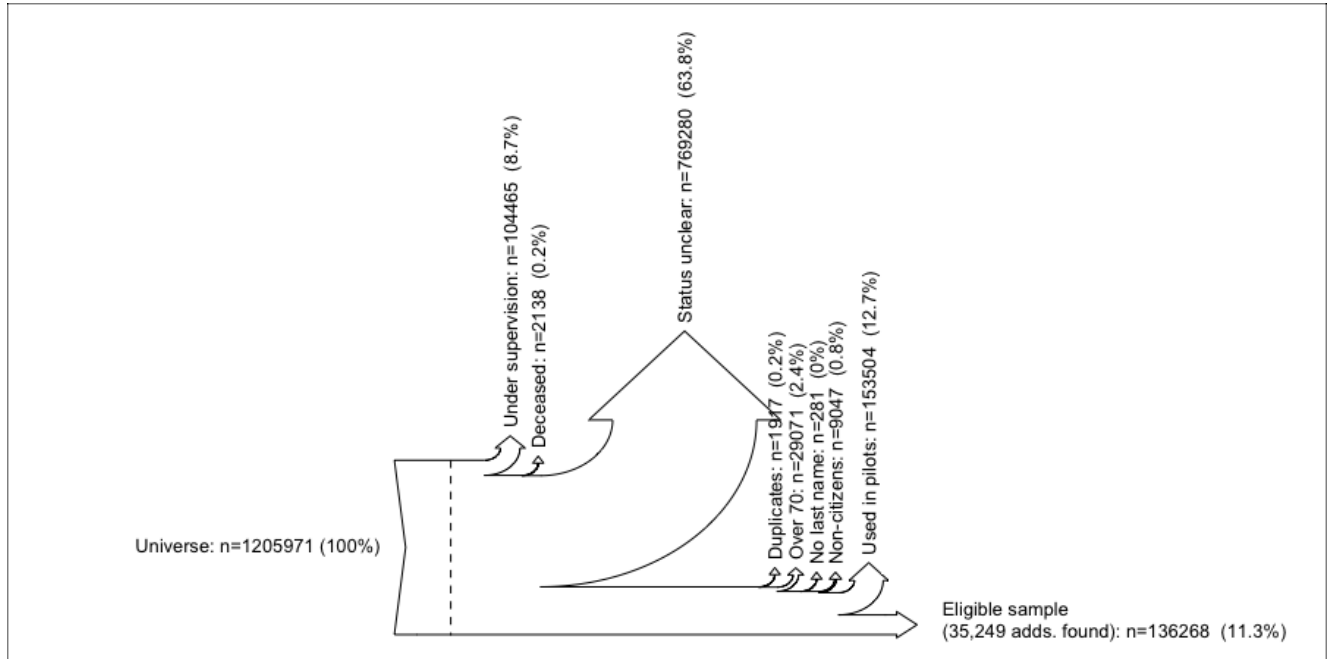
Table 7: Treatment Effects for Comparison Group Versus Main Study Group

	<i>Dependent variable:</i>							
	Voter Registration				Voter Turnout			
	Comparison	Study 4	Study 4 Urban	All	Comparison	Study 4	Study 4 Urban	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment mailer	-0.0003 (0.002)	0.011** (0.004)	0.008 (0.006)	-0.0003 (0.002)	-0.0003 (0.002)	0.007* (0.004)	0.005 (0.005)	-0.0003 (0.002)
Criminal record group				0.021*** (0.003)				0.011*** (0.003)
Treatment * Record				0.011** (0.005)				0.008* (0.004)
Control Mean	0.048	0.069	0.072	0.054	0.037	0.048	0.049	0.04
Observations	35,708	14,098	8,030	49,806	35,708	14,098	8,030	49,806

*Notes:* This table shows the effect of the treatment (sending a mailer) on voter registration and turnout by November 2020, for the comparison sample (people living in high-incarceration neighborhoods in NC, without criminal records) versus the criminal record group (the “no criminal record framing” treatment from Study 4). \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

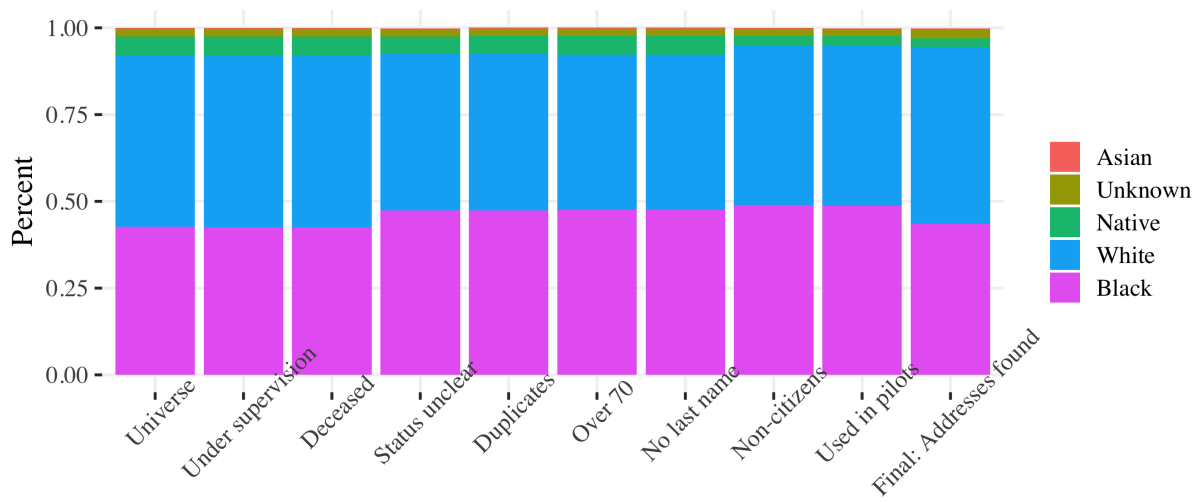
## A Additional figures and tables

Figure A1: Construction of Sample



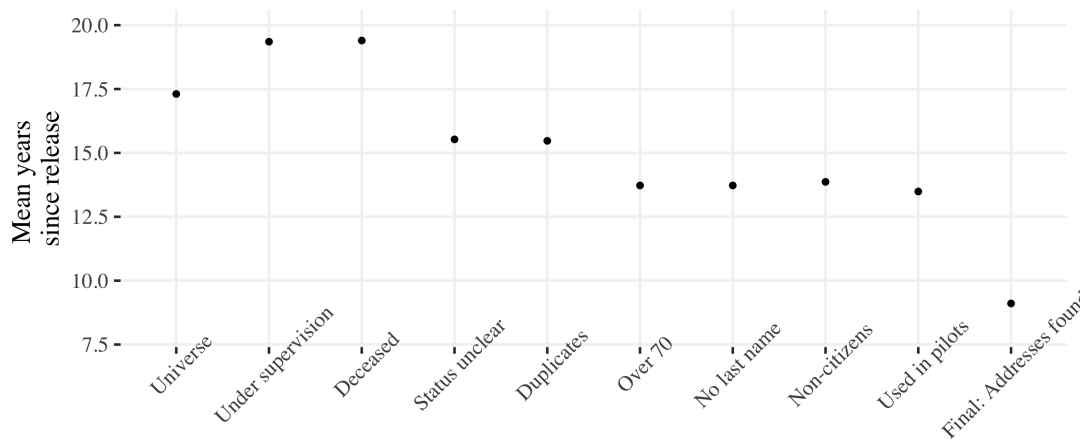
Notes: This figure shows our sample population, relative to the broader set of people with criminal records in North Carolina.

Figure A2: Describing Data Loss: Race



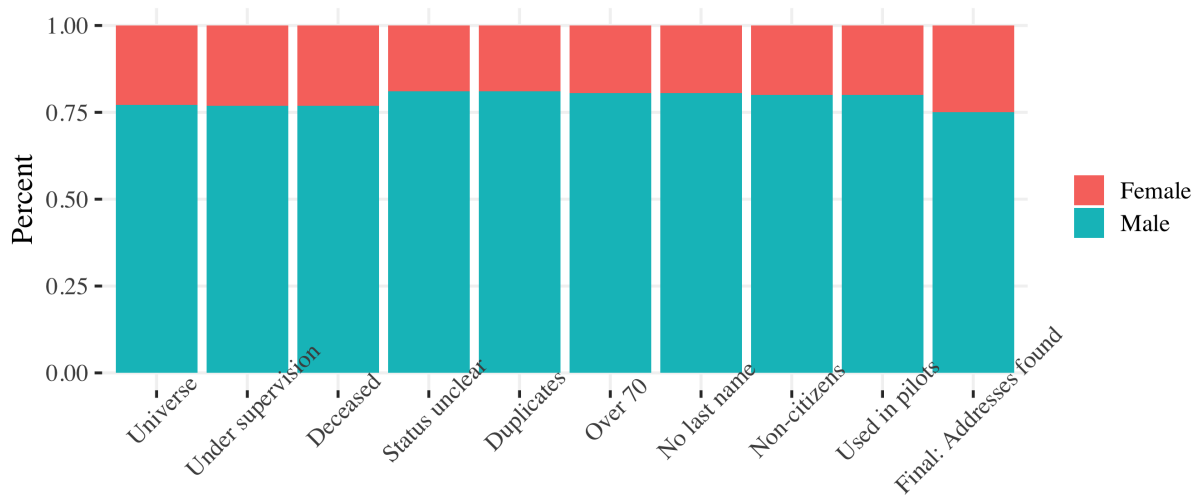
Notes: This figure shows how the racial composition of the sample changed as we omitted records to arrive at our final analysis sample.

Figure A3: Describing Data Loss: Time Since Release



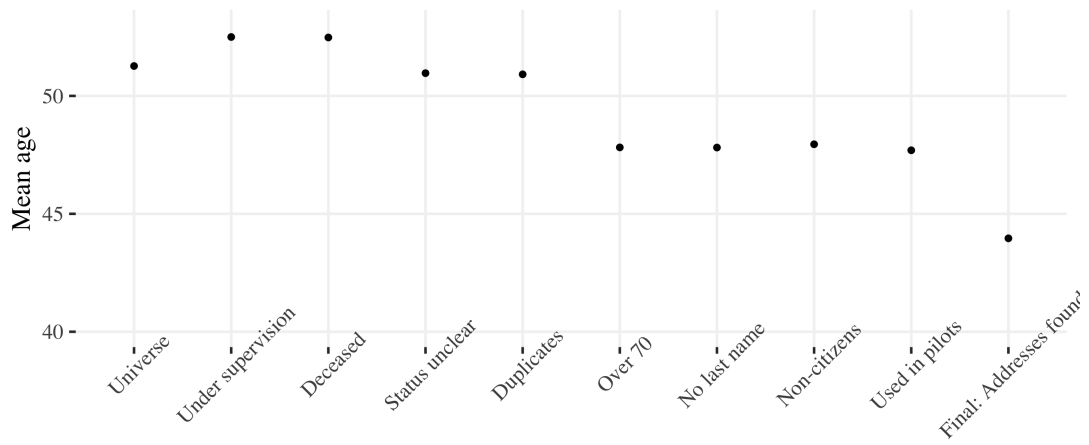
Notes: This figure shows how the average time since release of the sample changed as we omitted records to arrive at our final analysis sample.

Figure A4: Describing Data Loss: Gender



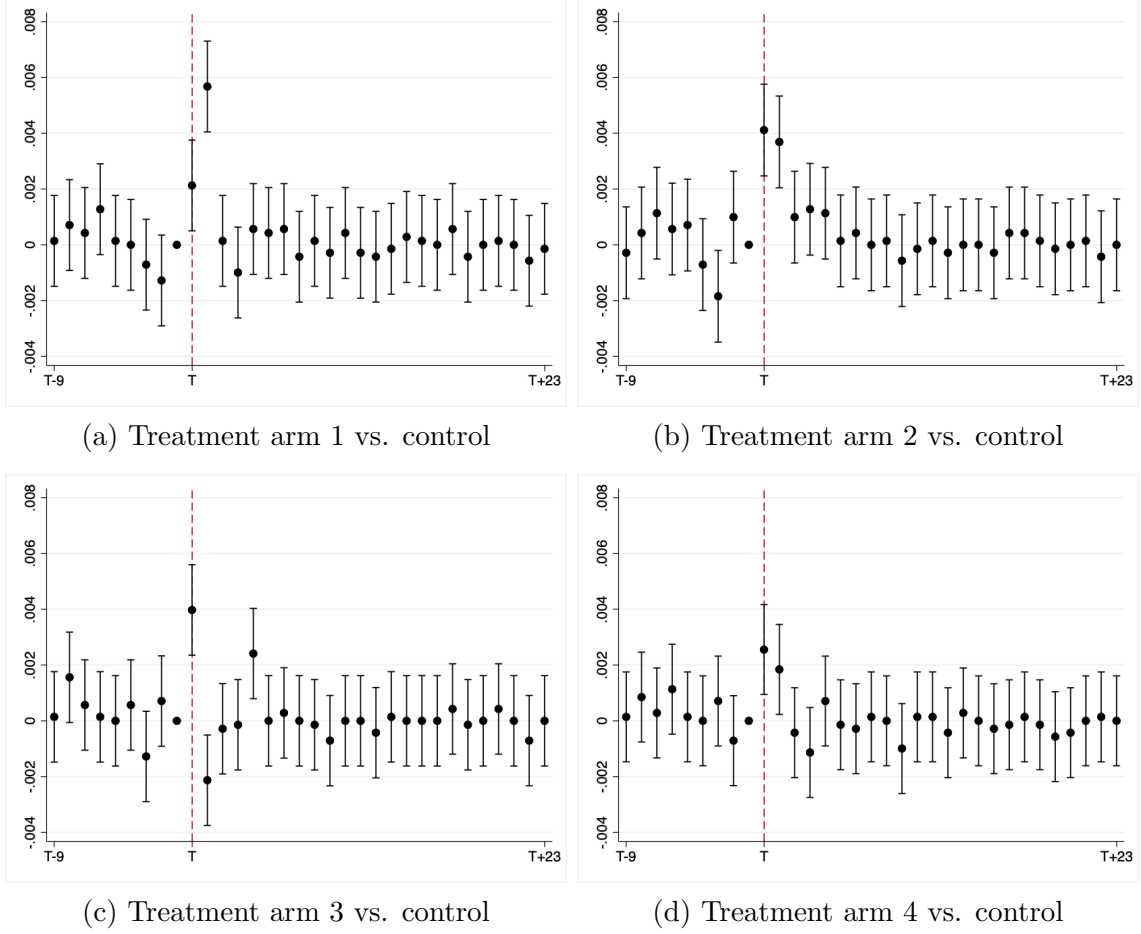
Notes: This figure shows how the gender composition of the sample changed as we omitted records to arrive at our final analysis sample.

Figure A5: Describing Data Loss: Age



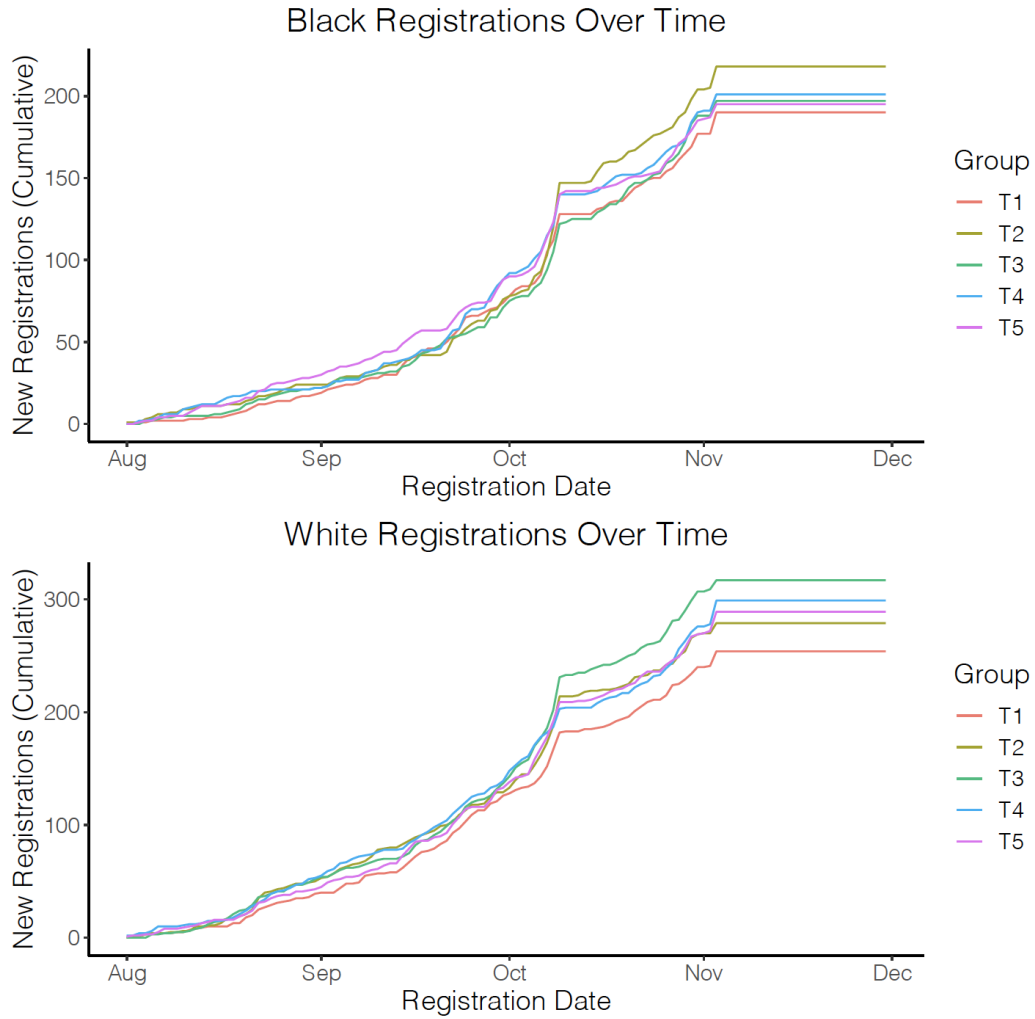
Notes: This figure shows how the average age of the sample changed as we omitted records to arrive at our final analysis sample.

Figure A6: Treatment effects by week in North Carolina



Notes: This figure shows treatment effects for each treatment arm, relative to the control group. The x-axis shows the week since mailers were scheduled to arrive in mailboxes. The y-axis shows effect on registering to vote. Treatment arm 1 is our basic mailer, with registration form included. Treatment arm 2 is the basic mailer with no criminal record framing. Treatment arm 3 is the basic mailer without the registration form. Treatment arm 4 is the basic mailer with *extra* civil rights framing.

Figure A7: Registrations over time: Study 4, by race



Notes: This figure plots cumulative new registrations in each treatment and control group over time, separately by race. T1 is the control group. T2 is the basic mailer. T3 is the mailer with no criminal record framing. T4 is the mailer with no registration form included. T5 is the mailer with extra civil rights framing.

Table A1: Heterogeneity Results

Tau Quantile	Mean Treatment Effect	Male	Black	Hispanic	Previous Incarcera- tion	Previous Supervision
1	-0.03	0.65	0.61	0.01	0.54	0.97
2	-0.00	0.73	0.27	0.02	0.28	1.00
3	0.02	0.72	0.37	0.06	0.56	0.97
4	0.05	0.90	0.49	0.03	0.90	0.94
<b>Age (Years)</b>						
Tau Quantile	Mean Treatment Effect	Q1: [19,33]	Q2: (33,40]	Q3: (40,47]	Q4: (47,55]	Q5: (55,70]
1	-0.03	0.10	0.19	0.32	0.07	0.31
2	-0.00	0.22	0.24	0.04	0.32	0.18
3	0.02	0.29	0.17	0.27	0.13	0.15
4	0.05	0.26	0.21	0.18	0.23	0.12
<b>Time Since Release (Years)</b>						
Tau Quantile	Mean Treatment Effect	Q1: [0.3,1.7]	Q2: (1.7,4.2]	Q3: (4.2,9.0]	Q4: (9.0,17.1]	Q5: (17.1,46.8]
1	-0.03	0.17	0.13	0.25	0.22	0.23
2	-0.00	0.26	0.20	0.23	0.23	0.07
3	0.02	0.19	0.08	0.35	0.25	0.12
4	0.05	0.21	0.32	0.06	0.14	0.27

Notes. This table shows the results of a machine learning heterogeneity analysis of treatment effects in Study 4. This exercise divides individuals into bins according to their predicted treatment effect (based on regressions run in a separate training sample). For example: The top row of each panel shows the average characteristics of people in the bottom predicted-treatment (tau) quantile; these individuals have an average predicted effect on voter registration of -0.03. The bottom row of each panel shows the average characteristics of people in the top predicted-treatment quantile; these individuals have an average predicted effect on voter registration of 0.05. In this analysis, all treatment arms are combined and compared to the control group. Age and time since release are binned into quintiles for the analysis.

Table A2: Postcard Followup: Predicting Bounced Mailers

	<i>Dependent variable:</i>
	Bounced
Male	−0.013* (0.007)
Black	0.005 (0.005)
Age	0.001*** (0.0003)
Time since Release	−0.002*** (0.0003)
Constant	0.125*** (0.013)
Observations	18,664

*Notes:* This table shows the relationship between individual characteristics and whether a mailer “bounced” (was returned to sender) – a proxy for a wrong address. Specifically, it shows the results of a regression with “mailer bounced” on the left-hand side, and individual characteristics on the right-hand side. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table A3: Comparison group characteristics

	Control	Treatment	Joint F-test p-val
Age (years)	44.65	0.09 (0.135)	0.504
Black (race imputed)	0.102	-0.002 (0.003)	0.437
Hispanic (race imputed)	0.081	-0.004 (0.003)	0.129

**Notes:** This table shows baseline descriptive statistics for the comparison sample (people living in high-incarceration neighborhoods in NC, without criminal records) The first column shows the control group mean for each covariate at baseline, the next column shows differences between that control group mean and the mean for the treatment group, and the last column shows the p-value from a joint F-test testing that the group means are different. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table A4: Party of Registration for Registrants in Main Study and Comparison Group

	Criminal Record Sample		Comparison Group	
	Any Treatment	Control	Treatment	Control
Democratic	752	192	371	392
Republican	767	167	203	199
Libertarian	16	3	7	8
Not Affiliated	630	122	266	253
Total new registrants	2165	484	847	852

**Notes:** This table shows the number of people in each sample who registered to vote, by their party of registration.

## B Mailers

Figure B1: Study 1: Basic Mailer



Dear Future Voter,

You are receiving this letter because we think you may be eligible to vote, but records indicate that you may not be registered at this address. **We would like to encourage you to register and use your voice in upcoming elections!** If you think you may be registered already, or if you would like to verify your voter registration status, you can check it here: <https://vt.ncsbe.gov/RegLkup/>.

**What elections are coming up?** There will be a statewide primary election on **March 3, 2020** with runoff elections in April and May, as necessary. These primary elections determine the final candidates who will run for office in the general election. The general election will be on **November 3, 2020**, during which you can vote for state and federal offices, including the president.

**Do you meet the following criteria?**

- ☒ I am a U.S. citizen
- ☒ I am a resident of the county where I live, and I have lived here for at least 30 days
- ☒ I am at least 18 years old, or will be on election day
- ☒ If I have a previous felony conviction, I have completed all the terms of my sentence
- ☒ I am not currently on probation or parole
- ☒ I am not registered in another county or state
- ☒ If I am registered in another county or state, I am willing to rescind that registration

**If you do, you are an eligible voter.**

**We've made it easy for you!** We have included a voter registration form for you to fill out, sign, and mail to your county board of elections using the included postage-paid, pre-addressed envelope. Your registration form must be postmarked and mailed **25 days** prior to an election in order for you to be able to vote in the election. If you miss the deadline, you may still register in-person at one-stop early voting sites during the early voting period. You can learn more about this process here: <https://www.ncsbe.gov/Voting-Options/One-Stop-Early-Voting>.

If you have further questions about your voting eligibility, how to register and vote, or upcoming elections, you can find more information here: <https://www.ncsbe.gov>, or you can call the Election Protection hotline at 866-687-8683 or visit their website at <https://866ourvote.org/>.

**Your voice starts with your vote.** The right to vote is an important American tradition. The whole point of democracy is that citizens are active participants in government, and democracy functions best when everyone takes part in the voting process. By taking the time to do their civic duty, voters ensure that elected leaders know what they think and how they feel. We encourage you to take the time to fulfill your civic duty by registering and voting!

*The NC Voter Registration Project is not affiliated with the North Carolina Board of Elections or 866-OUR-VOTE. You can reach us with questions or concerns at 919-438-0273.*

Figure B2: Study 2: Basic Mailer



Dear Future Voter,

You are receiving this letter because we think you may be eligible to vote, but records indicate that you may not be registered at this address. **We would like to encourage you to register and use your voice in upcoming elections!** If you think you may be registered already, or if you would like to verify your voter registration status, you can check it here: <https://vt.ncsbe.gov/RegLkup/>.

**What elections are coming up?** There was a statewide primary election on **March 3, 2020**, and there may be runoff elections in April and May, as necessary. The general election will be on **November 3, 2020**, during which you can vote for state and federal offices, including the president.

**Do you meet the following criteria?**

- ☒ I am a U.S. citizen
- ☒ I am a resident of the county where I live, and I have lived here for at least 30 days
- ☒ I am at least 18 years old, or will be on election day
- ☒ If I have a previous felony conviction, I have completed all the terms of my sentence
- ☒ I am not currently on probation or parole
- ☒ I am not registered in another county or state
- ☒ If I am registered in another county or state, I am willing to rescind that registration

**If you do, you are an eligible voter.**

**We've made it easy for you!** We have included a voter registration form for you to fill out, sign, and mail to your county board of elections using the included postage-paid, pre-addressed envelope. Your registration form must be postmarked and mailed **25 days** prior to an election in order for you to be able to vote in the election. If you miss the deadline, you may still register in-person at one-stop early voting sites during the early voting period. You can learn more about this process here: <https://www.ncsbe.gov/Voting-Options/One-Stop-Early-Voting>.

If you have further questions about your voting eligibility, how to register and vote, or upcoming elections, you can find more information here: <https://www.ncsbe.gov>, or you can call the Election Protection hotline at 866-687-8683 or visit their website at <https://866ourvote.org/>.

**Your voice starts with your vote.** The right to vote is an important American tradition. The whole point of democracy is that citizens are active participants in government, and democracy functions best when everyone takes part in the voting process. By taking the time to do their civic duty, voters ensure that elected leaders know what they think and how they feel. We encourage you to take the time to fulfill your civic duty by registering and voting!

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Figure B3: Study 3: Basic Mailer

Dear Future Voter,

**Do you or a loved one have a criminal record? You may still have the right to vote. Know your rights!**

There are many misconceptions about the right to vote for North Carolina citizens. The 2020 Election will be historic and understanding your rights means you have the choice to make *your voice heard* in 2020 and beyond. Citizens are eligible to vote as soon as they have completed the terms of their felony conviction. This means if you are *off papers*, **your right to vote has been automatically restored.**

**WHO IS ELIGIBLE TO REGISTER AND VOTE IN NORTH CAROLINA?**

- ☐ You must be a US Citizen AND
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- ☐ You must be a resident of NC for at least 30 days by Election Day
- ☐ You must *not* be *currently serving* jail time for a felony conviction OR *currently* be on probation or parole for a felony.

**USE THE GUIDE BELOW TO ENSURE YOUR VOICE COUNTS**

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1. Complete ALL required sections (in pink) and provide EITHER your driver's license number OR the last four digits of your social security number if you are able to.
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3. Sign and date the form, and include a phone number so the Board of Elections can contact you if they have questions.
4. Mail or deliver the registration form to your local county Board of Elections. Find your local Board of Elections Office at [vt.ncsbe.gov/BOEInfo](http://vt.ncsbe.gov/BOEInfo).

You can also register online, if you have a North Carolina DMV ID card or license. Visit [youcanvote.org/voting](http://youcanvote.org/voting) to access the online voter registration portal!

**STEP 2: CHOOSE THE BEST VOTING OPTION FOR YOU & VOTE (Once you are registered.)**

1. Vote EARLY at any early voting site in your county—October 15-31 OR
2. Vote on Election Day—Nov 3rd at your assigned polling location, OR
3. Vote by Mail. Visit [ncsbe.gov/Voting-Options/Absentee-Voting](http://ncsbe.gov/Voting-Options/Absentee-Voting) to request to vote by mail, look up your polling location, and more!

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Thank you!

NC Voter Registration Project

*The NC Voter Registration Project is not affiliated with the North Carolina Board of Elections.  
You can reach us with questions or concerns at (919) 213-9936.*

Figure B4: Study 3: Partner-Branded Mailer



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Thank you,

Kate Fellman  
Executive Director

*You Can Vote is a North Carolina nonprofit, nonpartisan 501(c)(3) organization that works to educate, register, and empower NC citizens to cast their vote.*

Figure B5: Study 4: Basic Mailer (NC)



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A handwritten signature in blue ink that reads 'Kate Fellman'.

Kate Fellman  
Executive Director

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Figure B6: Study 4: Mailer with no criminal record framing (NC)



You are receiving this letter because we would like to encourage you to register and use your voice in upcoming elections! If you think you may be registered already, or if you would like to verify your voter registration status, you can check it here: [youcanvote.org/register](https://youcanvote.org/register).

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**USE THE GUIDE BELOW TO ENSURE YOUR VOICE COUNTS**

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Figure B7: Study 4: Mailer with no registration form (NC)



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**USE THE GUIDE BELOW TO ENSURE YOUR VOICE COUNTS**

**STEP 1: GET REGISTERED AT YOUR CURRENT ADDRESS.**

1. Register entirely online if you have a North Carolina DMV ID card or license. If you don't have an NC ID, start the form online and you'll be mailed a form to sign, date and return to the Board of Elections. Visit [youcanvote.org/register](https://youcanvote.org/register) to access the online voter registration portal!

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Executive Director

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Figure B8: Study 4: Mailer with extra civil rights framing (NC)



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Criminal Justice and Civil Rights are on your ballot. Members of Congress and the state legislature decide what is a crime and how it should be punished. They make rules on how our courts, prisons, and jails are managed and how people should be treated when they are in custody. Judges decide who gets detained and for how long, and who goes to prison and for how long. Elected officials have an impact on how equal protection is enforced and are responsible for ensuring freedom of speech, assembly and religion, and specific rights including voting rights. Find out what's on your ballot and *why your vote matters* at [youcanvote.org/wob](http://youcanvote.org/wob).

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## C Texas

Study 4 described in the main paper originally included a component in Texas, with the same treatment conditions as in North Carolina. However, we confronted a series of challenges implementing the project which lead us to seriously doubt the validity of the outcome. Below, we detail the experiment and the related challenges, and present the findings, such as they are.

### C.1 Voter eligibility in Texas

To register to vote in Texas during the period of our experiment, you needed to: (1) be a United States citizen; (2) be a resident of the Texas county in which you were registering; (3) be at least 18 years old on Election Day; (4) if convicted of a felony, have completed the sentence, including any term of incarceration, parole, supervision, or probation, or have been pardoned or otherwise released from the resulting disability to vote; and (5) not have been determined by a court exercising probate jurisdiction to be (i) totally mentally incapacitated; or (ii) partially mentally incapacitated without the right to vote.<sup>43</sup> Texas only allows voter registration by mail or in person, not online.

### C.2 Constructing the Texas Sample

In Texas, we partnered with an organization which we call ABC for anonymity, who provided branding for the letters that were sent out (mailers are very similar to those sent in North Carolina). We obtained the Texas Conviction Database from the Texas Department of Public Safety in order to identify people with past convictions who should now be eligible to vote. However, the Texas voter file is not publicly available, so our procedure for identifying those in our sample who are unregistered was slightly different than in North Carolina, and we relied on the data firm L2 to help identify unregistered people and track whether they registered and voted post-treatment.

The Texas Conviction Database includes 5,166,923 unique individuals. After identifying individuals eligible to vote because they were no longer serving a sentence, we removed those who were deceased. We also removed those who we thought were still incarcerated, on probation, or on parole. We identified 1,746,705 individuals potentially eligible to vote. We randomly selected one million individuals from the resulting sample.

We sent this list to L2 to identify the subset of these individuals who were (1) not already

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<sup>43</sup>The Texas guidelines are available here: <https://www.sos.state.tx.us/elections/pamphlets/largepamp.shtml>.

listed in the Texas voter files and (2) under the age of 70 (in keeping with our procedure in North Carolina). L2 then matched our list with their files and determined which individuals were not yet registered to vote. From the list of individuals returned to us by L2, we randomly selected 250,000 eligible, unregistered individuals, and sent that list to our data vendor to be matched to valid mailing addresses. This yielded a final analysis sample of 89,750 individuals in TX.

This general procedure was similar to what we did in North Carolina, but we ran into a few additional issues in Texas. The sample we randomized across treatment and control arms was the 89,750 individuals who we identified as eligible to vote and for whom we found valid mailing addresses. However, post-treatment we discovered that, due to a coding error, this sample inadvertently included some individuals not eligible to vote due to the fact that they were under supervision at the time of treatment ( $n = 2,284$ , 2.5% of TX sample). We further discovered that some individuals were in fact already registered to vote ( $n = 9,572$ , 11% of the TX sample). There were also a number of records that lacked adequate information to determine the incarceration/supervision status of the individual ( $n = 25,514$ , 28% of the TX sample). While we had intended to target individuals we were certain had a felony conviction (comparable to the sample in North Carolina), only about a third of this sample's most recent conviction was a felony.

Treatment assignment was unrelated to each of these characteristics, but including people already registered, still in custody, without a felony conviction or whose status is unclear will likely attenuate the results. Thus, below, we will show the outcome of the experiment for voter registration successively dropping groups of individuals identified as not fitting within the study's parameters.

### **C.3 Mailing Treatments in Texas**

Finally, we faced issues with the mail vendor as we fielded the experiment, such that the mailing of letters from the vendor was delayed and then they faced further delays due to USPS issues affecting the entire country in fall 2020. In Texas, individuals must return a registration form post-marked by 30 days prior to the election (November 3, 2020). The registration deadline for the 2020 general election was thus October 3. A sample piece of mail addressed to one of the PIs landed in their Texas mailbox on October 1, much later than originally planned. A voter receiving a mailer on October 1 (and many likely received them even later) would have had less than 48 hours to open the mailer, fill out the registration form, and get it into the mail. For this reason, in addition to the sample issues discussed above, we are extremely uncertain about the treatment implementation. It seems highly

likely that the mailers arrived too late to meaningfully affect registration or turnout in the November election. This makes the results below unhelpful for determining whether our intervention affects those outcomes.

## C.4 Results in Texas

Table ?? shows the effect of any treatment and each treatment arm on voter registration, for the TX sample. Table ?? also shows these results when we iteratively drop records for people we did not intend to treat. Column 1 indicates that the basic mailer improved registration by 0.1 percentage point (5% relative to the mean), and that the other treatments are negatively associated with registration overall.

Column 2 shows the impact of our treatments after dropping those individuals already registered to vote. Column 3 shows the effect after we drop those still in custody and ineligible to vote. Column 4 drops those who do not have a felony conviction. Column 5 drops those whose status is unclear. Across all iterations, the exclusion of a registration form is negatively associated with voter registration relative to the control. As we drop individuals we did not intend to treat, the other arms have a consistently positive effect on voter registration. However, the size of the effect is so small that the results are effectively zero. An evaluation of racial heterogeneity does not reveal any additional insight beyond what we gained from North Carolina.

In sum, the results in Texas are null across a variety of metrics and model specifications. However, this finding is biased toward zero for all the reasons detailed above. As such we believe it is inappropriate to extrapolate from these findings. Instead, further research is needed to understand whether Texas is a uniquely difficult context in which to mobilize people with felony convictions, and the ways in which the effectiveness of such efforts might vary across subgroups.

Table C1: Texas, Dropping potentially-ineligible voters

	<i>Dependent variable:</i>			
	No Drops	Drop only pre-reg	Drop in-custody	Voter Registration Drop missing-end-date
	(1)	(2)	(3)	(4)
Basic mailer	0.001 (0.004)	0.002 (0.002)	0.002 (0.002)	0.002 (0.003)
No criminal record framing	−0.002 (0.004)	0.001 (0.002)	0.001 (0.002)	0.001 (0.003)
No registration form	−0.005 (0.004)	−0.002 (0.002)	−0.002 (0.002)	−0.003 (0.003)
Extra civil rights framing	−0.001 (0.004)	0.003 (0.002)	0.003 (0.002)	0.004 (0.003)
Control Mean	0.146	0.042	0.042	0.039
Observations	89,750	80,178	80,137	54,623

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

This table shows the results of our TX experiment, based on increasingly-restrictive sample definitions, as described in the text.