

REGISTERING RETURNING CITIZENS TO VOTE*

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Abstract

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.

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1 Introduction

Millions of people in the United States with felony convictions are currently eligible to vote (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 13 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). 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 institutions at the center of efforts to mobilize the electorate neglect returning citizens in order 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 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, we know less 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).¹ Because accurate samples of this population are difficult to construct and traditionally overlooked by

¹By comparison, 73% of the full adult citizen population in the US was registered to vote as of the November 2020 election (Current Population Survey, 2021).

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 send random sub-samples of this population information on eligibility rules and how to register to vote. We then 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 the 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 fielded study 4, the “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%, $p < 0.10$).²

We tested several variations of our main mailer, randomizing whether people were sent versions with slightly different messages or without a paper registration form. The differences across these approaches are quite limited, indicating that simply contacting this population and providing basic information about eligibility requirements was the key to our intervention’s success.

Given racial disparities in the criminal justice system, the effect of carceral contact on racial disparities in civic engagement is a primary concern. We find that our intervention was more effective for white recipients than Black recipients. This result is not due to

²As a point of reference, [Green and Gerber \(2019\)](#) survey the literature on voter registration efforts and report that studies targeted to unregistered minorities, who in one study were newly eligible to vote having turned 18 and in another study had recently moved across county lines, improved registration by about 1.5 percentage points and turnout by about .5 percentage points. Thus, we are slightly less effective at encouraging new registration, but our target population is significantly disadvantaged and difficult to reach, relatively speaking.

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. Overall, 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+).³ Additionally, and striking given broader public conversations about the likely political consequences of felon disenfranchisement, we find that new registrants were fairly evenly split between across political parties, with a substantial share registering as unaffiliated with any party.

Finally, we constructed a comparison group of individuals not listed in the North Carolina conviction database 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 – we wished to know whether there was added value to specifically targeting individuals with felony convictions, or whether responses to voter registration mail interventions from this population would mirror those of similarly situated individuals without any kind of conviction. These comparison 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 overlook people with records even as they reach their neighbors.

This project makes two contributions, one methodological and the other substantive. Methodologically, our research contributes a process for reaching difficult-to-contact populations. We are able to contact a much broader sample of returning citizens than the most relevant previous registration study focused on this population (Gerber et al., 2015). 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.

³We 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.

Findings from this project suggest that rather than thinking of people with felony convictions as low propensity voters, they are more akin to newly eligible voters, insofar as providing them with the means to get registered and instructions on how to do so via a light-touch mailer intervention, is effective at engaging them. 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 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.

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). It is less clear whether these low rates of participation could be changed by interventions that encourage people to get involved or provide information about how to do so.

Some work suggests that people with felony convictions face barriers to participation that will be hard to overcome. Many did not participate before their conviction, and continue to have low rates of registration and voting afterward (Burch, 2013; Lerman and Weaver, 2014; White, 2019). Burch (2011) estimates that in North Carolina – 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.⁴ Carceral contact has personal and social implications,

⁴By 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). Our data matching procedure unfortunately precludes us from computing a parallel estimate in 2020. While the conviction records in North Carolina provide full dates of birth, the voter registration file does not. In looking for individuals with convictions in the voter registration files we were extremely conservative, omitting anyone who we thought might be either ineligible or a valid match to the registration file. As a consequence, we are not able to compute reliable baseline estimates of registration among individuals with convictions, even as we are reasonably confident that individuals in our sample are not themselves registered.

exacerbating resource barriers to participation that existed prior to criminal legal entanglement (Pettit and Western, 2004).⁵ Further, people with carceral contact report low rates of 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 already-registered voters, and, therefore, neglect eligible people at society's margins, including returning citizens, inhibiting their full incorporation into the polity (Owens, 2014). 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.

However, there are some indications 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 biases.⁶

Further, there are reasons to expect that simply removing practical barriers to voter registration could increase participation rates among people with felony convictions. Many eligible voters with felony convictions do not know they are eligible to vote, and they often are not notified of their restored rights; when states do a better job of telling people about their eligibility, it appreciably increases participation (Meredith and Morse, 2015). Researchers recognize, moreover, that requirements that one register in order to vote place an additional

⁵Barriers 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).

⁶An 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.

burden on voters, and that those who have overcome this precondition are already highly likely to turn out.⁷ 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 get-out-the-vote (GOTV) efforts to convince already-registered voters to turn up at the polls (Mann and Bryant, 2020). Whereas these experiments sample from lists of already-registered people, evaluating how to encourage voter registration is considerably more difficult, because there is no universal database of eligible but unregistered people (Mann and Bryant, 2020). Because researchers studying voter registration efforts cannot rely on readily available lists (as in GOTV studies) they 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). Such approaches are challenging to apply to people with criminal legal contact, who may be less likely to have stable addresses, to be listed on utility bills or issued credit cards, or 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 when they are not already registered.

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 focused on a relatively narrow subgroup of recently-released people, making them particularly easy to find: the researchers partnered with the state government, which provided release-address information for those included in the study. This study, while groundbreaking, uses a strategy that cannot be applied to the millions of people who completed their sentences years or decades ago and thus do not have a current address on file with correctional agencies. It also leaves open the question of whether non-governmental organizations can effectively do this outreach, or whether only letters from government agencies will work. Finally, the restricted sample precludes analysis of the intervention's effectiveness among population subgroups.

Considering the broad impact of the criminal legal system and the variety of effective interventions identified in the voter mobilization literature, we know relatively little about the baseline capacity for mobilization among justice-involved individuals and how it compares to other marginalized people without convictions (Uggen, Manza and Thompson, 2006).

⁷One 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).

On one hand, criminal legal entanglement and its consequences are associated with many barriers to voting. 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 voting-eligible people with criminal records can be mobilized to register to vote if given information about eligibility and the registration process. 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.

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.⁸ 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).⁹ Individuals can register by mail, online, or in person.

⁸The full pre-registration and analysis plan can be found here: <https://www.socialscienceregistry.org/trials/4574>. 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.

⁹The 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.

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 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.¹⁰ 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.¹¹

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 who either were not convicted of a felony (but passed through the system due to a misdemeanor) or for whom the classification of the final conviction was unclear (63.8%), duplicate observations (0.2%), people over age 70 (2.4%)¹², individuals without a last name (0.02%), and non-citizens (0.8%).

After narrowing down the dataset to voting-eligible people with felony convictions, we worked with a commercial data vendor (‘data vendor’) to find contact information for as many of them as possible.¹³ We drew iterative samples to be matched by the vendor,

¹⁰Other 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.

¹¹<https://www.ncsbe.gov/results-data/voter-registration-data>

¹²We dropped people over age 70 to avoid unintentionally sending mailers to a large number of people who were deceased.

¹³As part of our agreement, the data vendor asked not to be identified in this study.

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.¹⁴ After excluding records sampled for the pilots, 136,368 eligible records remained, from which we were able to match 35,249 with valid addresses, our final analysis sample for the main study. Appendix Section A.1 discusses how this final sample compares to the full universe of eligible voters and how its composition shifted throughout the stages of the process. 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.

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—the main study.

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. We focused primarily on developing a means to reach individuals via mail, following directly from and building on Gerber et al.'s (2015) ground-breaking study, which leveraged the same method. However, in the pilot studies we also tested two methods of reaching justice-involved people: mailers and phone (via text-message in pilot 2 and with

¹⁴In Pilots 2 and 3 we also needed valid phone numbers. This reduced the match rate slightly for those studies.

follow up phone calls in pilot 3).

The literature offers two competing possibilities for mail intervention effectiveness in this population. On one hand, scholars find that mailers have particularly weak effects among low-propensity voters, and also that subtle manipulations in messaging do not have significant impact (Green and Gerber, 2019). On the other hand, researchers have demonstrated that for members of racial minority groups who need to become new registrants either because of their age or a relevant address change, providing information about how to register and the means to do so via mail can increase registration rates. Gerber et al. (2015) validate the latter possibility among people with felony convictions. However, they targeted a narrow and easy to find sub-group of this population based on characteristics—recent release dates and excluding those convicted of the most serious crimes—that might make them more likely to register than the average person with a felony conviction. We target a wider swath of justice-impacted people whose convictions represent a wide variety of charges and who may have lived at multiple addresses since their release. It is, therefore, less likely that our mailers will be effective – and we take any evidence that a mail intervention improves registration among people with felony convictions as encouraging evidence of their latent capacity for political action.

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.¹⁵ 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

¹⁵Readers 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.

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.¹⁶ The primary purpose of this pilot experiment was to establish whether we could identify people with felony convictions who were eligible to vote, find contact information for them, and reach them via direct mail. We fielded the mailer well in advance of North Carolina’s spring primary, with mailers arriving in mailboxes the week of January 11, 2020. The primary was held on March 3. The registration deadline is 25 days prior to election day. Individuals targeted by our mailers had about a month to register in order to be eligible to vote in the primary.

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 our mailers are sent, producing a gap in registration rates between the two groups. 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 (around the deadline by which individuals must have been registered in order to be eligible to vote). 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.¹⁷ Individuals in the treatment group were similarly more likely to vote in the primary (displayed in 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.

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

¹⁶All mailers are shown in Appendix B.

¹⁷As 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.

a couple weeks after our mailers are sent for both treatment groups. It is important to note that, again, this second pilot was fielded primarily to assess the feasibility of obtaining phone numbers for and contacting justice impacted people via telephone. Although the mailer indicated the importance of the general election which would be held seven months later, the mailer itself was not timed to coincide with any particular election, nor were individuals facing a registration deadline in the short-term. In keeping with pilot 1, we present the impact of our intervention 30 days following its implementation.

Regression results for Pilot 2 are shown in Column 2 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 days¹⁸ Column 3 of Panel A in Table 3 shows the effect of any 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.¹⁹

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 when measured at the 30-day mark. 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

¹⁸2.1 percent of the control group registered to vote by 30 days after the mailers were sent.

¹⁹5.5 percent of the control group voted in the November 2020 election. 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. Moreover, the intervention was itself fielded several months in advance of the November election, where researchers typically do not expect mobilization efforts to have meaningful effects when launched so far in advance of a given election (Coppock and Green, 2016).

staff and volunteers.²⁰ The primary objective of pilot 3 was to establish the framework for working together with a partner organization; the intervention was not fielded in conjunction with any specific election, although the upcoming 2020 general election was highlighted in the body of the mailer. As above, we present estimates measured at 30 days following the fielding of the intervention.

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 persists over time.

Column 3 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.²¹ 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.²²

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. 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 the 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.²³ This study allowed us to observe

²⁰All mailers are shown in Appendix B.

²¹1.1 percent of the control group registered to vote by 30 days after the mailers were sent.

²²6.0 percent of the control group voted in the November 2020 election.

²³As 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

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. Although we measured the impact of each of our pilot interventions on turnout in the November election, we did not anticipate or observe significant effects simply because our interventions preceded the general election by several months (Coppock and Green, 2016). However, we did observe a significant boost in turnout after fielding our first pilot, which was timed in accordance with the spring primary. Similarly, the timing of the scaled-up main study offers us the opportunity to assess the impacts of our intervention on turnout in addition to registration.

Because they did not appear to provide any meaningful benefit and we have questions about the quality of the phone numbers we obtained, 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 slightly increased the intervention’s efficacy. Across all pilots, our intervention appeared to effectively boost registration, 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 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);²⁴
4. the basic mailer, with no registration form or pre-addressed, postage-paid envelope (no registration form); and

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.

²⁴Note 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.

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).²⁵

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.²⁶

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 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. The coefficients are derived from an interaction between receiving any mailer and an indicator for calendar week, where the comparison week is the week before the treatment. The coefficients therefore reflect the difference between the pooled treatment and control in a given week, relative to the week prior to fielding the experiment. We see an immediate jump in voter registrations during the first two weeks after the mailer landed, after which the effect returns to zero. This is to be expected. Mailers reached individuals' mailboxes between one and two weeks prior to the registration deadline. We would not expect the treatment to have persistent effects relative to the control group after the registration deadline. Figure A6 presents the effects separately for each treatment arm, which all show a similar pattern.

²⁵The 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].”

²⁶We present specifications with and without baseline covariates included; controlling for them makes little difference.

Table 5 includes regression results. First, in Panel A, we consider the combined effect of any treatment, relative to the control group. Column 1 shows that sending any mailer increased registration by November 2020 by 0.8 percentage points (12%, $p < 0.05$).²⁷ 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%, $p < 0.10$).²⁸ 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.

How should we understand the size of the effect of the mail intervention on both registration and turnout? Recall that the intervention fielded by Gerber et al. (2015) improved registration among targeted individuals by 1.8 percentage points, and turnout by .9 percentage points. Scholars elsewhere have targeted members of racial minority groups who were newly eligible to vote or who needed to re-register due to a move using informational mailers similar to those we employ, and successfully boosted registration by about 1.5 percentage points and turnout by .5 percentage points (Green and Gerber, 2019). Here, we boosted registration by .8 percentage points and turnout by .5 percentage points (when mailers are pooled). We are slightly less successful at registering new voters than previous studies. However, there are important differences between our target population and those included in

²⁷6.6 percent of the control group registered to vote by November 2020.

²⁸4.6 percent of the control group voted in the November 2020 election.

previous studies. Recall that [Gerber et al. \(2015\)](#) targeted a very narrow (and privileged, relative to the overall custodial population) and easy-to-find group of recently released non-violent offenders who has served minimal time. Likewise, the broad group of individuals with felony convictions targeted in this study here are likely more transient and contend with multiple forms of disadvantage not obviously faced by those aging into the electorate or who recently moved. For these reasons, we are encouraged by the positive and significant findings we observe here, even if the size of the effect is comparatively modest.

How should we understand the lack of significant differences across treatment arms in Study 4? On one hand, we might expect that providing extra information about post-conviction eligibility and/or extra civil rights framing would increase effect sizes, since some studies have found that emphasizing the importance of making one's voice heard is especially effective ([Green and Gerber, 2019](#)). On the other hand, [Green and Gerber \(2019\)](#) conclude from a meta-analysis of large scale GOTV experiments that the effects of mailers are particularly weak among low propensity voters, and that subtle variations in wording are unlikely to yield significant differences. At the same time, they also conclude that for newly eligible and unregistered voters, providing instructions on how to get registered and the requisite materials to do so can have a substantive impact. We did not have strong priors about what to expect in terms of people with felony convictions who are nevertheless eligible to vote. The evidence from Study 4 suggests that people with felony convictions are similar to newly eligible rather than low-propensity voters. Thus, we interpret our results as suggesting that, at least on average, simply contacting people in this target group and providing basic information about eligibility requirements, is enough to increase voter registration and turnout.

6 Auxiliary Analyses

6.1 Heterogeneous effects across subgroups

Criminal justice contact disproportionately affects Black Americans, likely leading to a larger negative effect on civic engagement within this group. Since our intervention seeks to counter this negative effect, we are interested in whether the impact of our intervention varies with race. To do this, we focus 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. These differences are not simply due to a lack of statistical power or higher baseline rates of registration 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. 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.²⁹ 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. 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. But, it is important to note that: our basic mailer performed equally well across these groups; the gap is driven by the other mailer types, which themselves are not statistically distinguishable from one another. Thus, we prescribe caution when interpreting these co-coefficients. The striking racial differences observed here raise the need for further research on how best to reach justice impacted individuals who are non-white.

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

²⁹6.3 percent of Black control group members and 7.1 percent of white control group members registered to vote by November 2020.

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).

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. That is, our no-criminal-record (no-CR) comparison group allows us to further assess whether people with felony convictions are better understood as low-propensity voters, or low-information

voters needing to be newly registered.

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 registration for the no-CR group. The coefficient is near-zero and statistically insignificant.³⁰ 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%; $p < 0.05$).³¹ 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.³² 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.³³ The comparable treatment effect from Study 4, for individuals with a felony conviction, is 0.7 percentage points (15%; $p < 0.10$).³⁴ 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

³⁰4.8 percent of the relevant control group registered to vote by November 2020.

³¹6.9 percent of the relevant control group registered to vote by November 2020.

³²7.2 percent of the relevant control group registered to vote by November 2020.

³³3.7 percent of the relevant control group voted in the November 2020 election.

³⁴4.8 percent of the relevant control group voted in the November 2020 election.

population that is not reached by standard outreach methods (even though they live in the same neighborhoods), (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), and (3) people with felony convictions are unique relative to similarly situated individuals without convictions insofar as they do not respond to mail interventions in ways the literature would lead us to expect low-propensity voters to respond.

6.4 Party of registration

Our mailers and the partners we worked with were non-partisan. 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.³⁵ We thus consider the party that individuals registered with, among those who registered to vote.

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

³⁵To 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).

they are hard to reach, may lack valid mailing addresses, and are largely understood to be unlikely to participate in the electorate (Owens and Walker, 2018).

At the same time, a nascent line of research suggests that, under the right circumstances, people with felony convictions can be politically mobilized. Though they remain low-propensity voters, this research largely finds that justice-involved individuals can become mobilized into other, non-voting activities. Whether the political energy observed among justice-impacted people can transform into political power via the vote remains an outstanding question. 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 we 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 sourced valid mailing addresses from commercial data. 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 that provides information about how to register and the means to do so increases 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. Perhaps 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.

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 (although it is worth noting that the basic mailer was equally effective among white and Black recipients). 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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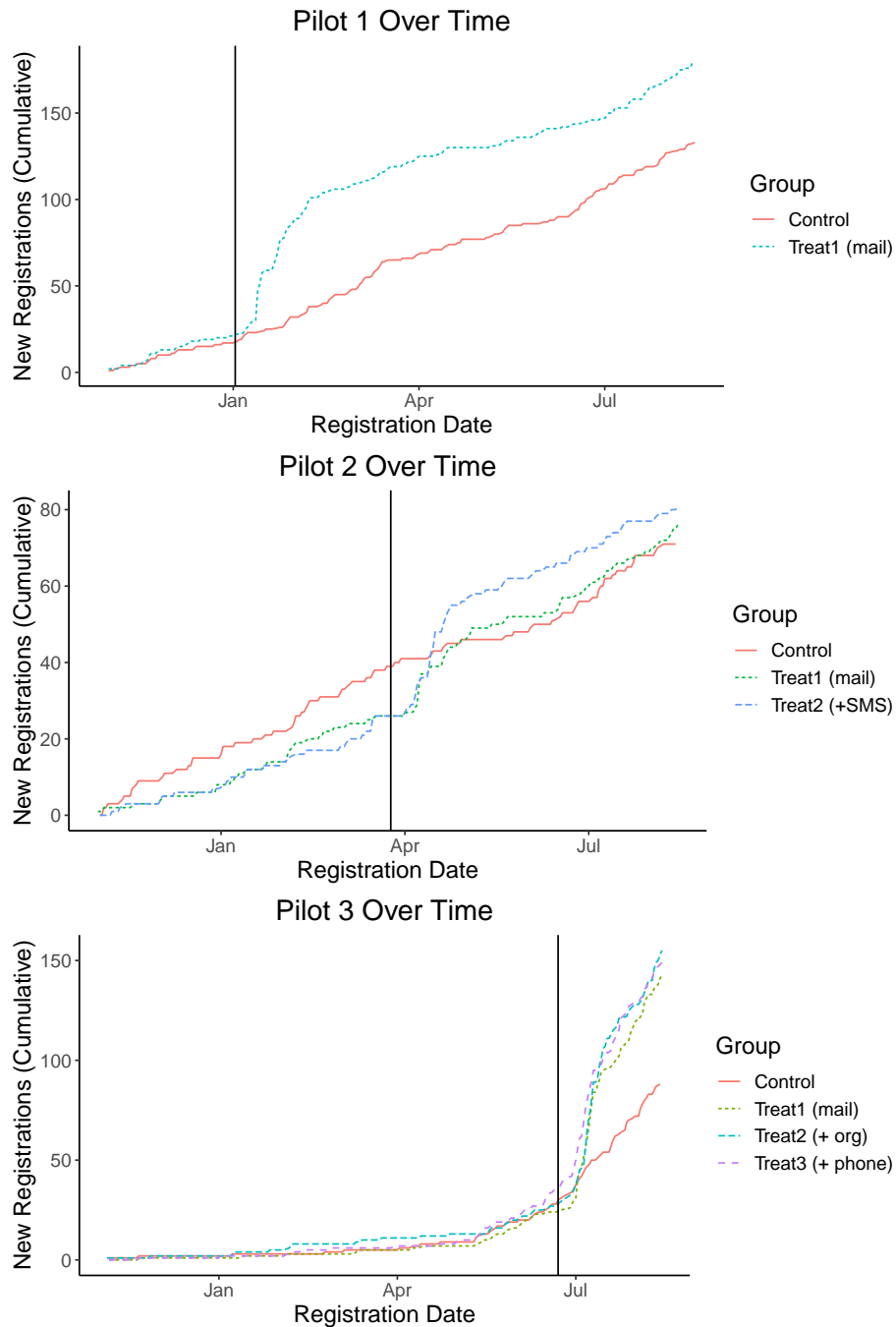
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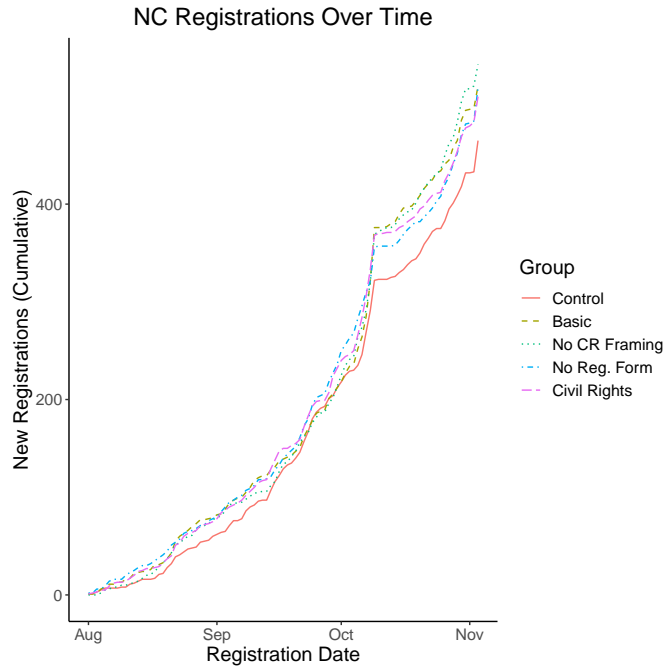
8 Figures and Tables

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.

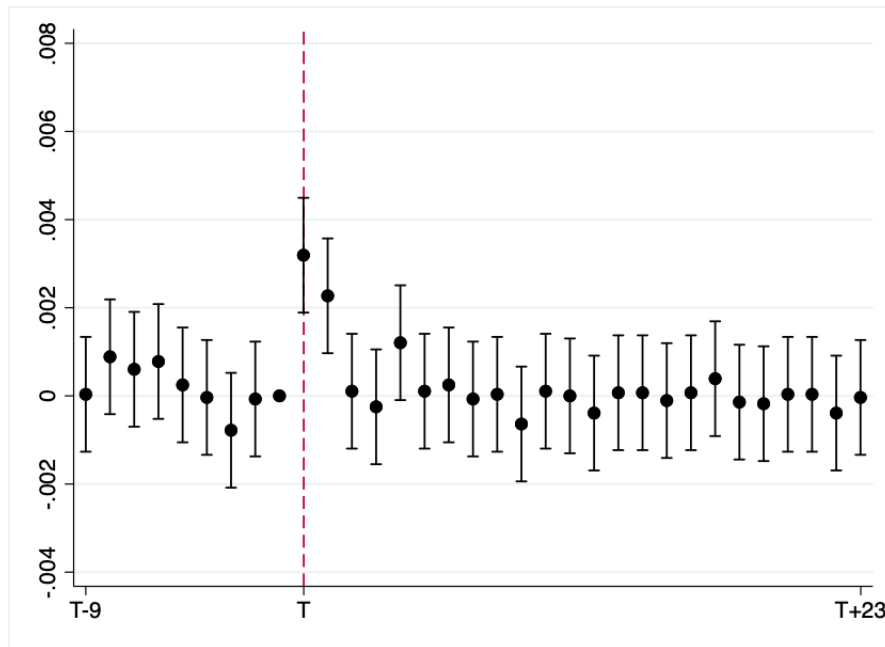


Figure 3: Treatment effects by week. This figure shows treatment effects of sending a mailer (all treatment arms combined), relative to the control group. The x-axis shows the week since mailers were scheduled to arrive in mailboxes. The y-axis shows effect on registering to vote.

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: Effects on Voter Registration

	<i>Dependent variable:</i>			
	Pilot 1	Registration		All
	(1)	Pilot 2	Pilot 3	(4)
	(1)	(2)	(3)	(4)
Panel A: All Arms Combined				
Treatment (Any mailer)	0.013*** (0.003)	0.002 (0.004)	0.009*** (0.002)	0.009*** (0.002)
Panel B: Separate Treatment Arms				
Basic Mailer	0.013*** (0.003)	-0.0005 (0.004)	0.007*** (0.003)	0.008*** (0.002)
Basic Mailer + Text		0.005 (0.004)		0.009** (0.004)
Branded Mailer			0.010*** (0.003)	0.011*** (0.002)
Branded Mailer + Call			0.010*** (0.003)	0.010*** (0.002)
Study Fixed Effects				X
Control Group Mean	0.007	0.021	0.011	0.01
Observations	8,621	6,584	21,763	36,968
<i>Note:</i>			*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)
Panel A: All Arms Combined					
Treatment (Any mailer)	0.002* (0.001)	-0.0002 (0.005)	0.007 (0.006)	0.004 (0.004)	0.003 (0.003)
Panel B: Separate Treatment Arms					
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)
Panel A: All Arms Combined				
Any Treatment	0.008** (0.003)	0.008** (0.003)	0.005* (0.003)	0.005* (0.003)
Panel B: Separate Treatment Arms				
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)
Panel A: All Arms Combined			
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)
Panel B: Separate Treatment Arms			
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>							
	Comparison	Voter Registration			Comparison	Voter Turnout		
		Study 4	Study 4 Urban	All		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

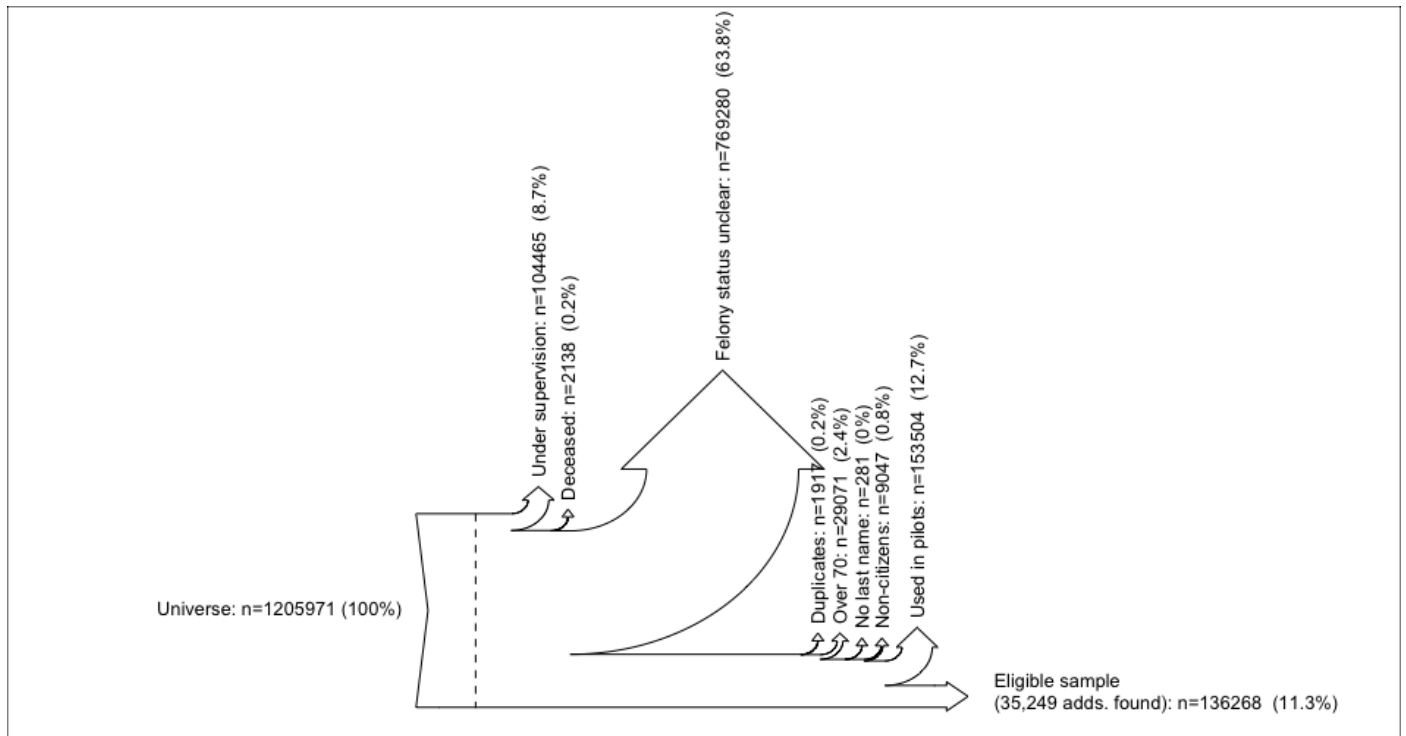
A Additional figures and tables

A.1 Sample Construction

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.

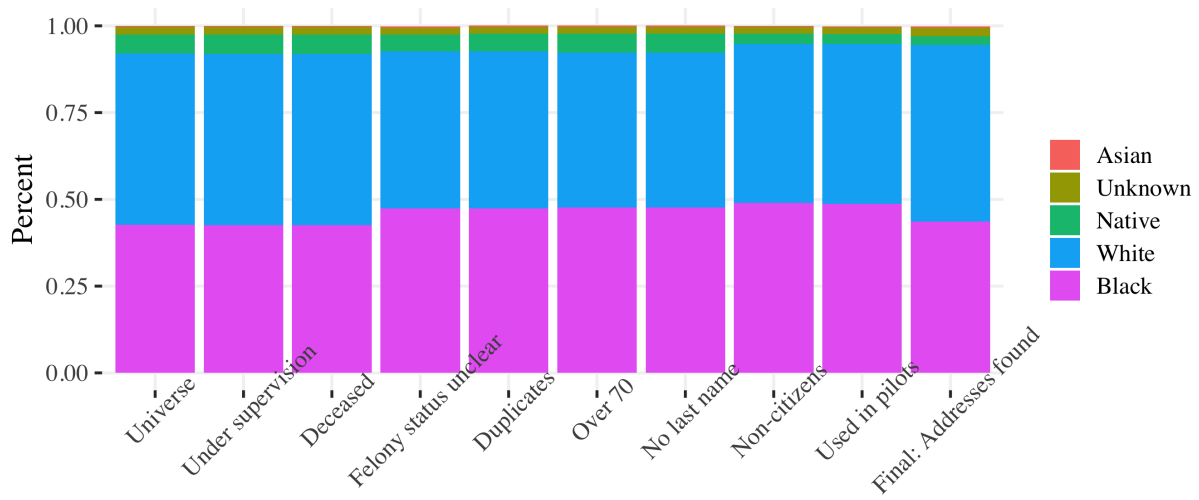
Race and gender were least affected by the process of data loss. Black individuals made up 42.7% of the full set of returning citizens eligible to vote, 48.7% of the samples pulled for the pilots, and 43.6% of the sample pulled for the main study (Figure A2). Similarly, white individuals made up 49.2, 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.

Figure A1: Construction of Sample



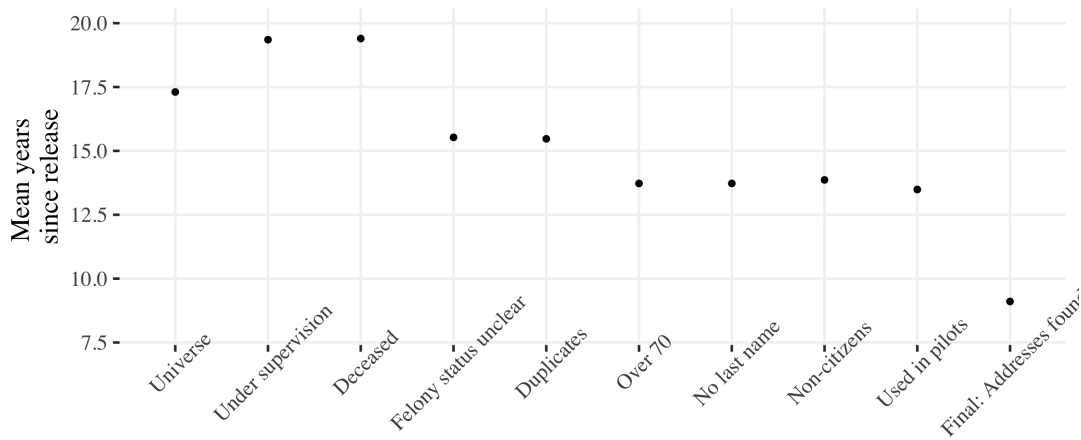
Notes: This figure shows our sample population, relative to the broader set of people with criminal records in North Carolina.

Figure A2: Describing Data Loss: Race



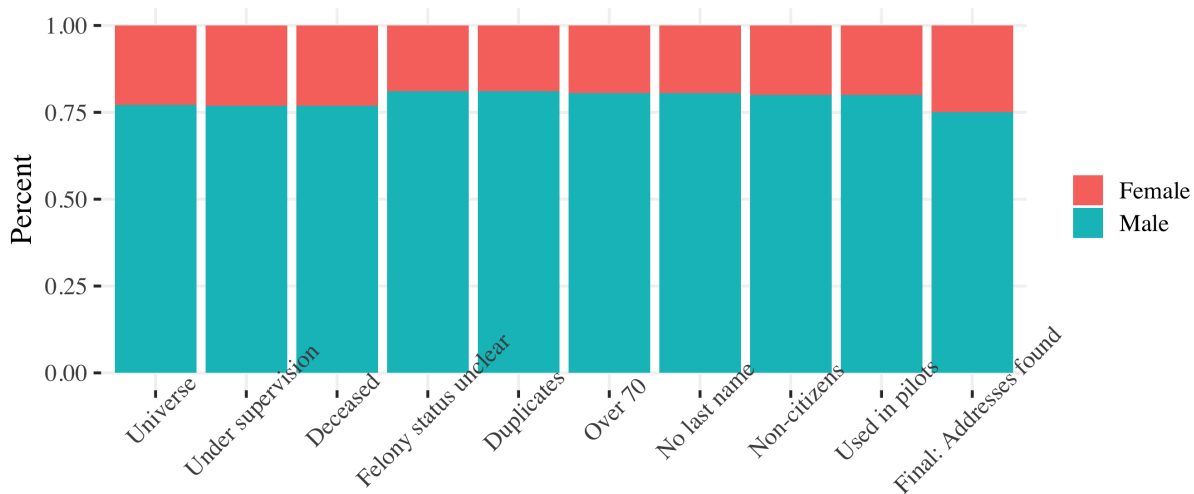
Notes: This figure shows how the racial composition of the sample changed as we omitted records to arrive at our final analysis sample.

Figure A3: Describing Data Loss: Time Since Release



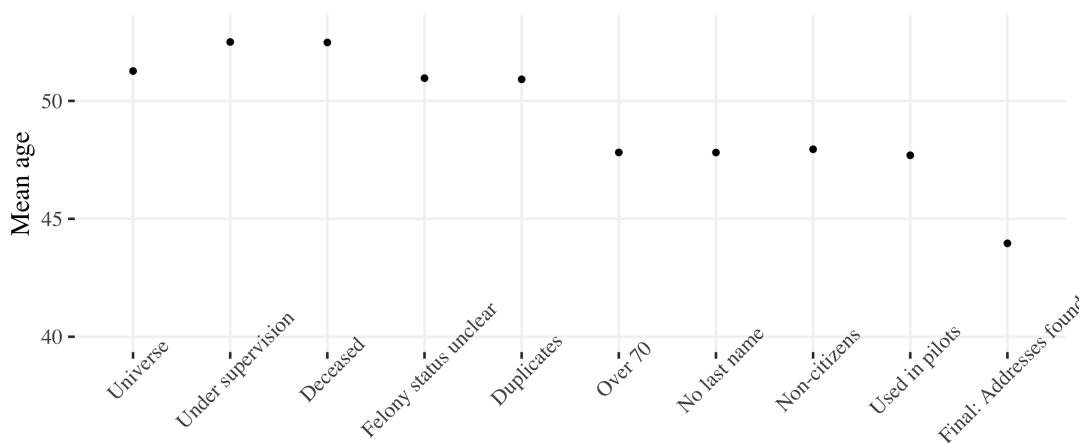
Notes: This figure shows how the average time since release of the sample changed as we omitted records to arrive at our final analysis sample.

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.

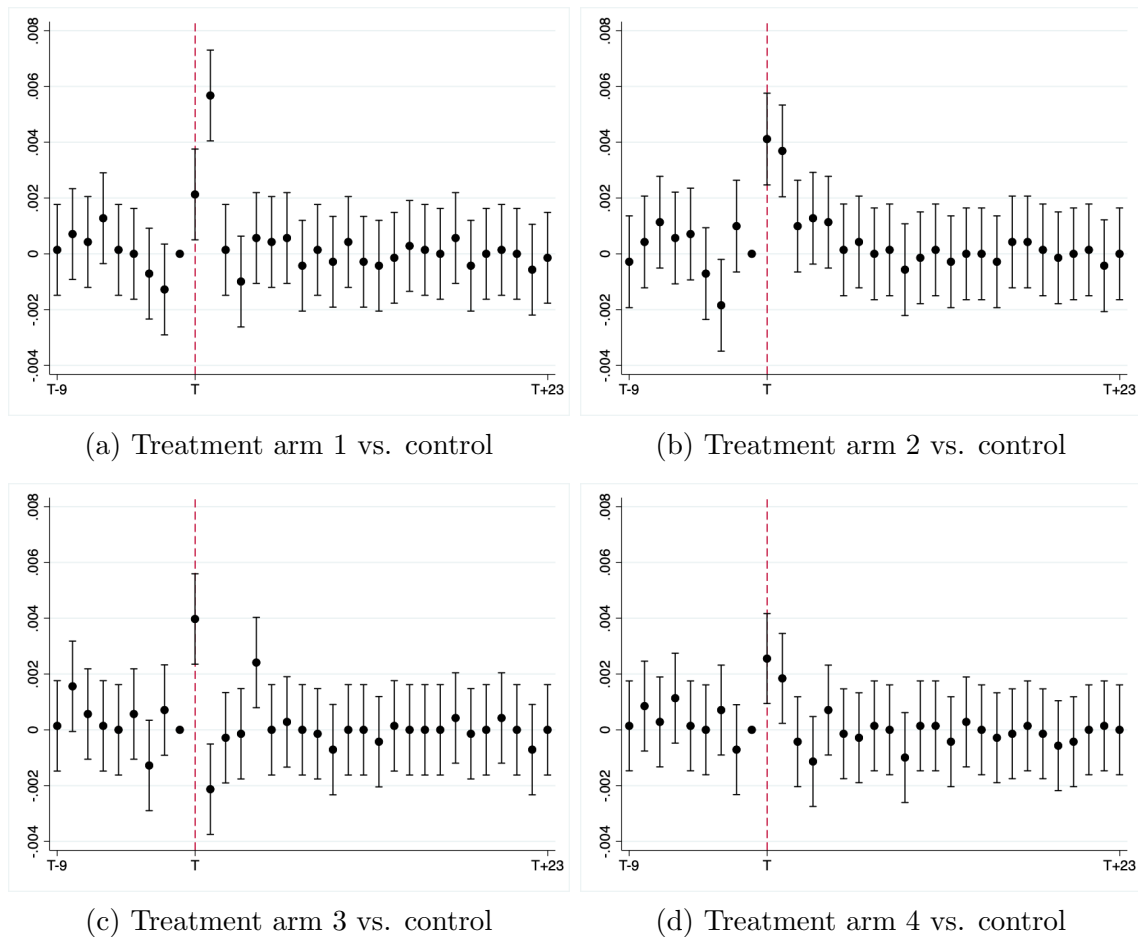
Figure A5: Describing Data Loss: Age



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

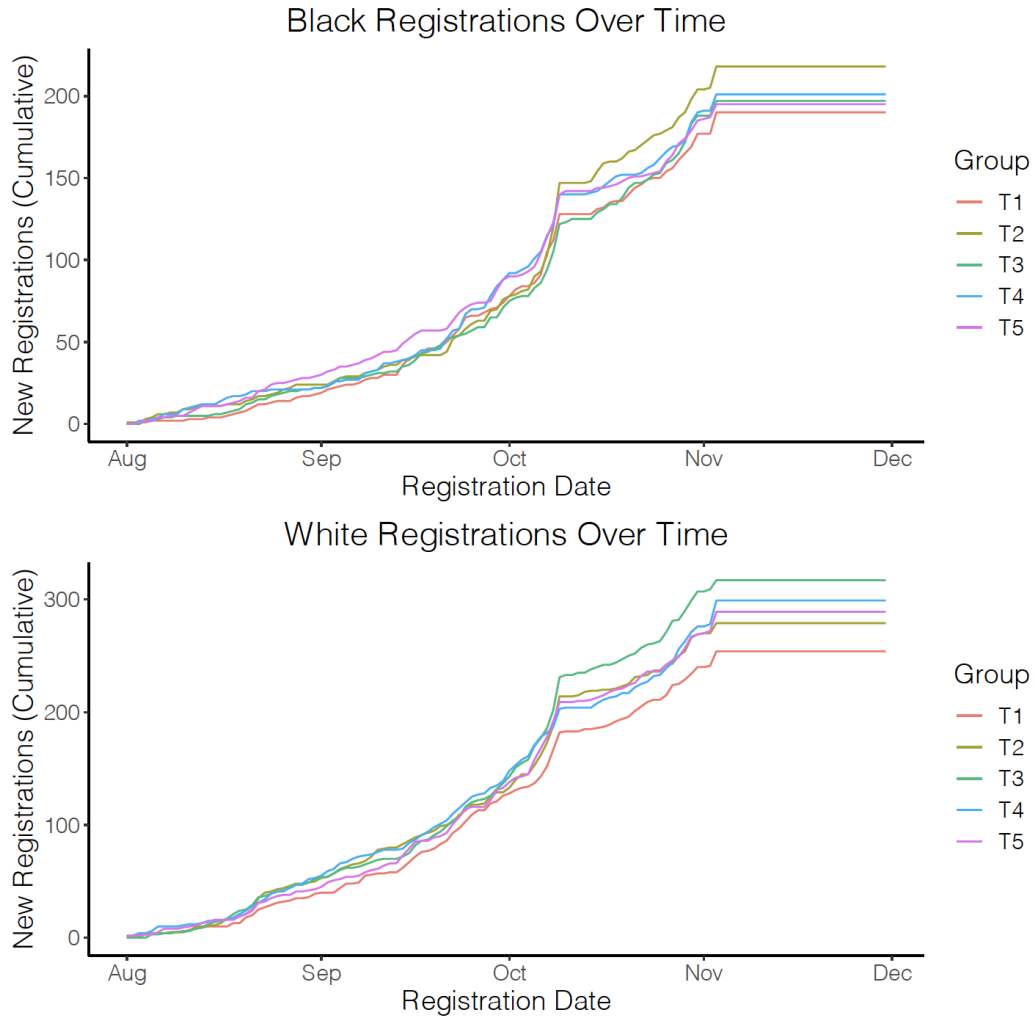
A.2 Additional Analysis of Study 4

Figure A6: Treatment effects by week



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
Age (Years)						
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
Time Since Release (Years)						
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
- You must be 18 years old by Election Day AND
- 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

STEP 1: GET REGISTERED AT YOUR CURRENT ADDRESS. A registration form is included in this letter.

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.
2. If you don't get mail where you live, enter a valid mailing address in Section 5.
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.

You can also register online, if you have a North Carolina DMV ID card or license. Visit 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 to request to vote by mail, look up your polling location, and more!

The 2020 Election is right around the corner. This year we will vote for local, statewide, and federal offices including US President and US Senate, NC Governor, statewide and district court judges, and many more elected offices. These office-holders make decisions that directly impact you and those you care about. By voting for people who care about the issues you do, you help shape your future. **Your vote actually does matter.**

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



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
- You must be 18 years old by Election Day AND
- 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

STEP 1: GET REGISTERED AT YOUR CURRENT ADDRESS. A registration form is included in this letter.

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.**
2. If you don't get mail where you live, enter a valid mailing address in Section 5.
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 youcanvote.org/BOE.

You can also register online, if you have a North Carolina DMV ID card or license. Visit 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 youcanvote.org/voting to request to vote by mail, look up your polling location, and more!

The 2020 Election is right around the corner. This year we will vote for local, statewide, and federal offices including US President and US Senate, NC Governor, statewide and district court judges, and many more elected offices. These office-holders make decisions that directly impact you and those you care about. By voting for people who care about the issues you do, you help shape your future. **Your vote actually does matter.**

Thank you,

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

Kate Fellman
Executive Director

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Figure B5: Study 4: Basic Mailer (NC)



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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.

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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 to access the online voter registration portal!

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

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



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.

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3. Vote by Mail. Visit youcanvote.org/voting to request to vote by mail, look up your polling location, and more!

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.

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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.³⁶ 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

³⁶The 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 C1 shows the effect of any treatment and each treatment arm on voter registration, for the TX sample. Table C1 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 (1)	Drop only pre-reg (2)	Drop in-custody (3)	Voter Registration Drop missing-end-date (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<0.1; **p<0.05; ***p<0.01

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