Registering Returning Citizens to Vote: A Field Experiment in North Carolina

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Abstract

Millions of people in the U.S. are eligible to vote despite past felony convictions, but their participation rates are extremely low. This article reports the results of a randomized controlled trial that encouraged people with convictions to register to vote in North Carolina in 2020. We use a novel approach to identify and contact this population, combining administrative and commercial data, and are able to identify a much larger sample than earlier studies, allowing us to examine previously-unobserved heterogeneity in this group's responsiveness to outreach. We find that, on average, our treatments increased voter registration by 0.8 percentage points (12%), and voter turnout by 0.5 percentage points (11%). We do not find evidence that effects vary with mailer content. The treatment has no effect on a comparison group of people without convictions who live in the same neighborhoods. We demonstrate how deeply-marginalized people can re-enter political life, with democratic and policy implications.
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Introduction

Many people in the United States are currently eligible to vote after past felony convictions. Only eleven states permanently disenfranchise certain people with felony convictions, most states do not permanently bar anyone from voting, and since 2016 seventeen states and the District of Columbia have expanded access to the franchise for people involved in the criminal legal system (*Felon voting rights*, 2021; Uggen et al., 2022). Researchers estimate that between the 2016 and 2020 general elections, over a million people gained the right to vote (Uggen et al., 2020). These legal changes, combined with the extent of the criminal legal system in the US, mean that fully six percent of the voting-age population has a felony conviction but is eligible to vote: they represent a group larger than the population of Pennsylvania. Yet, formally regaining eligibility does not mean that one will turn out to vote, and researchers estimate that less than one third of people with a conviction are registered voters – compared to 73% of eligible Americans who were registered as of the November 2020 election (Current Population Survey, 2021; Burch, 2011; Gerber et al., 2015). Given the recent wave of franchise expansions, we ask: can traditional voter mobilization tools be used to identify, contact, and mobilize people with felony convictions who are eligible to vote?

This question is deceptively hard to study. Since there is no universal list of unregistered people, most of what we know about voter mobilization comes from studies that begin with lists of registered voters whom scholars then try to turn out through various means (Mann and Bryant, 2020; Jackman and Spahn, 2021). This focus on registered people will miss many people with convictions, who are already much less likely to be registered compared to the overall population. People with felony convictions may also be uniquely unlikely to participate in electoral politics, as confusion around eligibility, cumulative disadvantage that precedes and follows carceral contact, and declining trust in the government all contribute to low levels of electoral engagement among justice-impacted people (Meredith and Morse, 2015; White, 2019; Pettit and Western, 2004). Crucially, for all of these reasons, justice-impacted

people are almost never the target of traditional "get-out-the-vote" (GOTV) outreach efforts (Owens and Walker, 2018). Institutional neglect likely compounds chronic non-voting among this group. Accurate samples of returning citizens[2] are hard to construct, further deterring scholars and advocates from pursuing mobilization efforts among justice-impacted people (Gerber et al., 2015; Burch, 2011). We therefore know very little about how to identify and reach unregistered people with felony convictions who retain the right to vote, how to characterize the differences between registered and unregistered people, and whether traditional methods of voter mobilization—mailers, phone calls, or in-person efforts—are effective.

In this study, we ask how to identify and reach people with convictions who are eligible voters, and whether traditional outreach strategies can increase participation among this group. We identify people with felony convictions who are eligible but unregistered by bringing together administrative records on sentencing and conviction with voter registration files. This, itself, is a major contribution, as we demonstrate a method for approaching registration mobilization in the absence of statewide lists of unregistered voters. We then work with a commercial data vendor to obtain contact information for eligible but unregistered people with convictions. This constitutes our list of individuals to target for outreach efforts. After several pilot studies refining our approach, we develop a mail-based intervention informing people of their right to vote and how to register that we evaluate against an uncontacted control group in an experimental setting. Ultimately, we are able to find and contact approximately one in three eligible North Carolinians with a past felony conviction, and we see substantively large effects of these mailers on registration and turnout in this sample. On average, the mailers sent as part of the main experiment in fall 2020 increased voter registration by 0.8 percentage points ($p < 0.05$), an overall increase in registration of 12 percent, and voter turnout by 0.5 percentage points ($p < 0.10$), an overall increase in registration of 12 percent.

Although we tested variations in message wording, we find no differences of note across

[2]We use "returning citizens" to refer to people who have completed custodial or supervisory sentences.

treatment arms, leading us to conclude that the simple provision of information about eligibility to register and the means to do so is sufficient to increase baseline engagement among justice-impacted people. We find no differences by party of registration that follow from our mailers. We find some evidence that these mailers may be less effective at mobilizing Black individuals, relative to their white counterparts. Supplemental tests suggest that this pattern is not due to differences in database address quality, but could be driven by differences in background exposure to other forms of outreach. Finally, a parallel study targeted to similarly-situated individuals who do not have a felony conviction had no impact on turnout among the non-conviction group, suggesting that informational efforts targeted to justice-impacted people hold unique promise.

One major contribution of this project is methodological. We develop a process for reaching difficult-to-find populations, and we contact a much broader sample of people with convictions (an estimated 30%) than the most relevant previous registration study focused on this population (an estimated 5%, Gerber et al., 2015).[3] Further, we develop this list without working in collaboration with a state agency (as scholars have done in the past), a choice that both expands the pool of justice-impacted people we can target through our effort, and one that may be important for a population whose experiences with the state lead to declining trust in government (Lerman and Weaver, 2014). Our alternative method of constructing a list is important because it not only expands the pool of individuals we can contact, but potentially alters the composition of our list in important ways, relative to past efforts – in particular, we are able to target individuals who may have been incarcerated and/or released (two factors likely to impact voting) for a much longer time period. We show that not only can we find and contact individuals who have been released for a significant amount of time, but also that our treatment is about as effective at mobilizing this group as it is for more recently released people. Moreover, the various benchmarks we provide

[3]The details of these coverage estimates appear in the "Sample Construction" section of the Supporting Information (SI).

suggest that barriers to targeting unregistered citizens for mobilization can be overcome. Since returning citizens are often on the margins of a variety of systems and institutions (employment, education, social services) – in part because of their records – it has been unclear how to identify them 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 similar to the underlying population.

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. Simply reaching out, providing information about eligibility and inviting participation can increase voter registration for a group scholars have largely seen as lacking in civic capacity. Our treatments appear particularly meaningful for those who have who experienced incarceration. 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). There are democratic and potential practical benefits (for mobilization efforts) to re-integrating people into political life after criminal convictions, and this study contributes to our understanding of how to do so.

Background

Despite fairly widespread rights post-custodial voting rights restoration, scholars have documented very low rates of registration and voting among individuals with felony convictions, even among those who are eligible (Lerman and Weaver, 2014; Gerber et al., 2015; White, 2019; White and Nguyen, 2022). 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 just 24% turned out in that election.[4]

There are many reasons that people with felony convictions are unlikely to vote. Custodial citizens often contend with multiple sources of material disadvantage that predate criminal justice contact, and which criminal legal involvement exacerbates (Lerman and Weaver, 2014; Gerber et al., 2017). Contact with the criminal justice system can erode attitudes important to participation, like trust in government and a belief in the value of one's political voice (Lerman and Weaver, 2014; Weaver and Lerman, 2010; Weaver, Prowse and Piston, 2020). That returning citizens are unlikely to vote makes them undesirable targets for mobilization-focused organizations, who largely direct their resources toward more likely voters. This institutional neglect compounds chronic non-voting in this group, further inhibiting their full incorporation into the polity (Owens, 2014).

There is some evidence, however, that not only are returning citizens able to return to political life, but that contact itself may sometimes spur participation. Owens (2014) highlights that a felony conviction can make people stakeholders in new policy areas, since they may lose access to important social welfare goods as a consequence of a conviction. Walker (2020) argues that contact can lead to a politicized identity, and that when they view their experiences as unjust or inhumane, individuals may be compelled to protest and engage in community organizing. In terms of voting, Laniyonu (2019) notes that when issues related to criminal justice are on the ballot those in highly policed communities turn out at higher rates, and Ang and Tebes (2021) find that exposure to apparently-unjust police violence in one's neighborhood also promotes turnout. It is unclear from this work whether people with convictions themselves participate – it may be that their loved ones or neighbors vote at higher rates. These findings are nevertheless encouraging, especially alongside Meredith and

[4]By comparison, 70% of the full adult citizen population in North Carolina was registered to vote as of the November 2020 election (Current Population Survey, 2021).

Morse (2015), who found that informing returning citizens that they were eligible to vote after the law changed in Iowa increased their overall participation. Thus, a small collection of work suggests that many justice-impacted people are ripe for mobilization, and other kinds of barriers, including neglect by organizations traditionally engaged in voter mobilization efforts, prevent them from becoming active members of the electorate.

Researchers recognize, moreover, that requirements that one register in order to vote place an additional burden on voters, and that those who have overcome this precondition are also more likely to turn out.[5] Yet, despite the recognition that getting registered is a major step often out of reach for marginalized people, most research around voter mobilization still focuses on convincing already-registered voters to turn up at the polls (Mann and Bryant, 2020). Evaluating how to encourage registration is more difficult, because there is no universal database of unregistered people (Mann and Bryant, 2020). Researchers interested in voter registration must, instead, rely on other readily-available lists of individuals, often targeting known groups, like college students, or engage in a more general, neighborhood-focused door-knocking approach (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, be listed on utility bills or issued credit cards, or 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 large 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 increased registration by 1.8

[5]Some of the only electoral reforms researchers have identified that effectively enhance turnout among low-propensity voters after the passage of the Voting Rights Act are those that ease or eliminate the burden of registration, including same-day registration and automatic voter registration (Grumbach and Hill, 2022; McGhee, Hill and Romero, 2021; Kim, 2023).

percentage points relative to the control group, suggesting that people can be re-incorporated into political life if they can be found and asked. This study, however, focused on a narrow subgroup of recently-released, non-violent offenders who served relatively short stints in prison. The researchers partnered with the state government to develop and distribute mailers (sending the mailers on official Secretary of State letterhead), and to obtain address information for targeted individuals. The choice to obtain contact information from the state necessarily limits the pool of individuals they were able to target to those who had been released from prison for only a handful of years, since states maintain records of the address to which the individual was initially released. In short, the intervention focused on an easy-to-identify, easy-to-contact group that is not necessarily representative of the larger population of people with felony convictions who are eligible to vote.

We estimate that Gerber et al. (2015) reached about 5% of the total population of individuals with convictions who are also eligible voters.[6] This work is groundbreaking insofar as it demonstrates that simply asking custodial citizens to participate can boost their engagement. Yet, Gerber et al. (2015) use a strategy that: 1) cannot be applied to identify and contact 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; 2) may not work outside of Connecticut, in places where state agencies may be reluctant to cooperate with researchers for this purpose; and 3) may be less effective than strategies that do not work with state institutions, since increased fear and skepticism towards the state is often a direct consequence of experiences with the system for justice-impacted people (Lerman and Weaver, 2014). Moreover, whether the narrow group targeted by Gerber et al. (2015) is relatively easy (or difficult) to mobilize is an outstanding question, as no other study of which we are aware has attempted to mobilize justice impacted people in an experimental setting. Finally, the restricted and relatively small sample precludes analysis of the intervention's effectiveness among population subgroups.

[6] See SI Section A2 for the details of this calculation.

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). 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.

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. Our first task is to develop an effective method for building a list that is broadly representative of people with felony convictions who are eligible to vote. We therefore take great care to evaluate the quality of our constructed sample, who we reach, and the eligible voters we successfully register and turn out. Our second task is to see whether we can increase registration and turnout in this broader group, and if we succeed, to identify any subgroup variation of note. Recognizing the attitudinal obstacles to participation people with felony convictions face, we partner with a well-known North Carolina nonprofit organization focused on voter mobilization. Acknowledging the material and informational barriers to voting that returning citizens face, we aim to reduce barriers to registration with the provision of information about eligibility and the means to register. We describe our data, interventions and analytic strategy in detail below.

Experimental Design

The List

We use a combination of state administrative data and commercial data to identify unregistered, voting-eligible people with past criminal convictions. This project focuses on North Carolina due to its representativeness with respect to election and penal policies, as well as data availability.[7] As of 2020 (when the intervention was fielded), people who had been convicted of felonies in North Carolina were temporarily ineligible to vote, but their eligibility was automatically restored after completion of their sentences (including probation or parole). This legal framework, shared with 17 other states, is slightly more punitive than the next most common framework, which bars people from voting only while incarcerated (The Sentencing Project, 2020). North Carolina ranked slightly below the national mean in its rate of incarceration (Uggen et al., 2020), but is among the top when it comes to the rate of Black incarceration (The Sentencing Project, 2021).

People in North Carolina can register by mail, online, or in person. In this respect, North Carolina is in the majority – 38 states and the District of Columbia allow individuals to register online (Martin, 2020). While access to the ballot box has recently been contested in the wake of *Shelby County v. Holder* (Herron and Smith, 2015; Shepherd et al., 2021), scholars who have developed summary indices measuring ease of access to the vote place North Carolina directly in the middle of such rankings (Schraufnagel, Pomante and Li, 2022). Thus, North Carolina's election rules around access to the vote for people with convictions and around registration are similar to those in other states. Perhaps the most notable aspect of the state's political context during the 2020 election cycle was the fact that it was competitive, with Trump winning the state by less than a percentage point (Cilliza, 2020).

[7] We also attempted to include a Texas component of the experiment, but faced implementation challenges as described in SI section A.10.

For all of these reasons, North Carolina is an appropriate site for our study, and we anticipate that findings from here are likely generalizable to states with similar electoral frameworks. Moreover, we can easily obtain both the criminal justice records necessary to identify many people with felony convictions who are eligible to vote and the voter registration files that allow us to identify eligible voters with convictions who are not already registered. North Carolina is therefore a useful site for understanding how to build a representative list of people with convictions who are eligible to vote and establishing effective strategies for mobilizing returning citizens.

To build our list of voting-eligible individuals with felony convictions, we started with 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 had completed the terms of their sentence.[8] We also used the state's publicly available voter file to identify people who appeared to be registered to vote already and omitted them from the study sample. We then contracted with a commercial data vendor to find current mailing addresses for as many people from our list of eligible voters as possible.

In the interest of providing benchmarks for future research and validating the demographic composition of our final list, we provide a brief overview of the technical details of the data construction process (described in full in the SI). We removed the following groups of people from the sentencing records data: those who were still under supervision at the

[8]States vary in the availability of custodial data. While North Carolina stands out in terms of the amount of data accessible from its DPS website, we confirmed at least 18 states have offender lookup tools, most of which are state-run. Scholars and advocates can request bulk data from the databases that support these tools, often for a fee. Public records requests are another strategy to obtain these data, including in states without web-based offender lookup tools. We do not mean to imply that working with non-state actors to obtain relevant data and construct a list to facilitate mobilization efforts is without obstacles, only that it may be a more practical approach for non-profit organizations than working with states, and a more appealing strategy for justice impacted people.

time of the study (9% of the sentencing records); those who did not clearly have a felony conviction and thus may never have lost the right to vote (70%); individuals who were deceased or over 70 years old (almost 9%); individuals who were non-citizens and those who appeared to be duplicate entries (roughly 1%). We then compared the resulting list to the voter file, in order to remove anyone who might already be registered, and obtained contact information for those individuals not found in the voter file for purposes of randomization within the context of the experiment.[9] Post-treatment, we merged our list back to the voter registration file in order to identify individuals who did in fact register and vote.

In merging our experimental dataset back to the voter file to measure our outcome variables of registration and turnout, we used names, years of birth (as North Carolina does not include exact dates of birth in its voter file), and addresses. Given the difficulty of matching across databases without exact dates of birth, our matching strategy proceeded across several stages. We first performed an exact match on first/middle/last names, address and year of birth. We then identified additional cases that matched on name, allowing for variation on whether a match was found on middle name or initial, year of birth and county of residence. Finally, we allowed for variation in how names are recorded by finding cases where a newly-registered voter has the exact same address and year of birth as someone from the sample, and where their names were similar (using the Jaro-Winkler string distance metric and a cutoff of .25 for either the full name or both the first and last name fields). This combined, multi-stage approach allowed us to identify matches even when names varied across databases, while still limiting the problem of false positives posed by the lack of exact dates of birth on the voter file.

We streamlined this approach to building the list via three pilot studies conducted during

[9]We obtained address information from a third-party data vendor, based on names and dates of birth. The vendor's address sources and matching approach is proprietary. Nevertheless, this is a common method for obtaining contact information for individuals when doing things like building a list for the purposes of fielding a web or phone-based survey of people not registered to vote (Barreto et al., 2018).

2020. Ultimately, we developed a list of over 70,000 people who we determined were eligible to vote and for whom we obtained address information. We estimate that this represents approximately one-third of all voting-eligible unregistered North Carolinians with a felony conviction (as of 2020). Readers may wonder why we could not find or treat the other two-thirds of this population, and whether that coverage could be improved. We note several sources of dropoff, some of them unique to North Carolina. First, as noted above, we excluded people over age 70, or for whom we could not with full confidence determine that they were eligible to vote due to incomplete information about their sentence. Because the North Carolina voter files only include year of birth, we also conservatively omitted anyone who appeared to match to a record in the voter registration file using this limited information. That means that there were potentially unregistered but still eligible people with convictions who were not included in our final list because we could not distinguish them from other registered voters (a problem that would be less prominent in states with exact dates of birth on the voter file). That, together with the fact that justice impacted people are a hard-to-find and highly transient group, inhibiting our ability to find accurate address information for them, accounts in large part for the loss of two-thirds of the target group. Nevertheless, this approach represents a dramatic increase from the estimated 5% reached by Gerber et al. (2015). We included 35,249 records in the main experiment, presented here, and the remainder were used in the pilot experiments.

The NC DPS data includes individual-level covariates we can use to benchmark the final sample of individuals for whom we found addresses against the overall list of individuals sentenced in North Carolina, including race, gender, age and time since release. More detail on this comparison appears in SI section A2. We do not observe much difference in the demographic composition of the final mailable list compared to the full set of people sentenced. Slightly less than half of the full list is Black, and the same is true of the final list of people for whom we obtained addresses. About 77% of individuals sentenced overall are men, relative to 75% in our list of those with addresses. The mean age of the sample of those

for whom we obtained addresses is slightly lower than in the sentencing records overall (44 compared to 51) but this is likely due to the omission of those who are deceased or over the age of 70. The removal of over-70 records likely also contributes to a notable difference in time since release. Individuals in the full list of sentencing records had been released for 17 years on average, relative to nine years among those for whom we obtained addresses. This pattern may also have to do with residential mobility and the potential that more recently released individuals may have served time on supervision, which tethered them to the state.

The Intervention

Over the course of 2020, we ran three pilot studies (described in the SI) and one main study (described here). Building on the pilots, we designed a large field experiment to be run in fall 2020. As noted above, it is not clear from the literature how to effectively mobilize returning citizens into the electorate. A large body of research has investigated why individuals do not participate, identifying such factors as resource barriers to participation, lack of knowledge about eligibility, and alienation from the state. A newer set of studies finds that people with criminal justice contact still vote sometimes, especially when they view themselves as policy constituents. Additionally, a small collection of studies have made inroads into understanding how to mobilize other groups of marginalized people – highlighting, for example, the value of delivering messages in Spanish when targeting Latino eligible voters (Michelson, 2006; Bedolla and Michelson, 2012), and of working with community-based organizations (Sinclair, McConnell and Michelson, 2013; Kammerer and Michelson, 2022; Grumbach, Han and Warren, 2022). Following from these findings, which highlight the importance of employing culturally competent methods when mobilizing hard-to-reach constituents, we partnered with the organization *You Can Vote*, a well-known non-profit in North Carolina focused on voter mobilization (Michelson, 2006; Valenzuela and Michelson, 2016).[10]

[10]In earlier pilots, we observed a slightly larger registration effect when mailers came from a non-profit rather than a generic project name, although the effect was not distinguishable from zero.

We then crafted a mailer that provided information about eligibility and instructions on how to register. We produced several variants of the mailer intended to address specific theories of non-participation. In order to test the premise that it is important to help people overcome logistical barriers to registration, we randomized whether individuals received a registration form and a pre-addressed, pre-stamped return envelope with their mailer. We also varied two other pieces: whether the informational mailer included introductory language explicitly focused on eligibility and felony convictions (several sentences noting that one is eligible once *off papers*), and the inclusion of language connecting the current election to civil rights and related issues faced by returning citizens. In the first case, our expectation is that the inclusion of extra language about eligibility among people with convictions may increase the likelihood of voting because it quickly addresses a knowledge barrier to participation. Without this information being highlighted at the top of the letter, people with convictions may still incorrectly assume that they are not eligible to vote, a particular concern for our partner organization. In the second case, we also anticipate that connecting the right to vote to issues important to people with convictions may increase registration and turnout. This intuition develops from research observing that otherwise low propensity voters can be mobilized when issues that connect to their marginalized status are relevant in a given electoral context (Owens, 2014; Pérez, 2015; Walker, 2020; Laniyonu, 2019). In sum, we randomly assigned people to one of five treatment conditions:

1. a control group that was not contacted at all;

2. a group sent an informational letter about eligibility to vote signed by a local non-profit organization, along with a registration form with pre-addressed, postage-paid envelope ("basic mailer");

3. a group sent the basic mailer package, omitting the highlighted information about eligibility among people with felony convictions ("no criminal record framing");[11]

[11]Note that these mailers still included a list of eligibility criteria, including information relevant to those

4. a group sent the basic mailer, with no registration form or pre-addressed, postage-paid envelope included ("no registration form"); and

5. a group sent 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").[12]

All mailers were developed in conjunction with *You Can Vote*, and are shown in Section A.3 of the Supporting Information. We randomly assigned individuals in the sample across these five groups with equal probability. The SI presents descriptive statistics for this sample and tests illustrating covariate balance across treatment arms, as well as a discussion of ethical considerations we took into account in sample construction and treatment design. Related to our ethical considerations, we chose not to include a social pressure arm, which has shown to be effective in GOTV experiments (Gerber, Green and Larimer, 2008). We make this choice because members of the target population, given their history, may be particularly wary of surveillance.

All mailers were sent in fall 2020, landing in mailboxes in early October (before the state registration deadline). After the November 2020 presidential election, we used the state's publicly-available voter file to observe voter registration and turnout among people in the sample. All analyses presented are of intent-to-treat effects based on assignment to each treatment arm, as we cannot observe who actually opened or read the mail we sent them.

with criminal records. But they do not include an opening paragraph highlighting this information.

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

Results

Table 1 presents regression estimates of the experimental treatment effects on voter registration and turnout in the 2020 general election. Overall, it appears these mailers substantially increased both registration and turnout, though the estimates vary somewhat in magnitude and significance.

We focus on Columns 1 and 4 (estimates not including the background covariates age, gender, race, and previous incarceration), but estimates with controls (Columns 2 and 5) are nearly identical. Column 1 shows effects on voter registration. The basic mailer increased voter registration by an estimated 0.8 percentage points ($p < 0.10$). The mailer with no criminal record framing increased registration by 1.1 percentage points ($p < 0.01$). The mailer with no registration form increased registration by 0.8 percentage points ($p < 0.10$). And the mailer with extra civil rights framing increased registration by 0.6 percentage points (n.s.). Column 3 shows the estimated effect of being sent any mailer relative to control: .8 percentage points, or a 12% overall increase.

We cannot statistically distinguish the effects of the different treatment arms from each other, partly because this study is only powered to detect relatively large differences across arms (on the order of a percentage point). However, the similarity across arms is also consistent with findings from Green and Gerber (2019), who observe that subtle differences in treatment wording rarely yield large voter-mobilization effects. Even the treatment condition that omitted the paper registration form did not produce a substantially-smaller treatment effect, a pattern that we note could be due to North Carolina's online voter registration platform; in states requiring paper forms, the inclusion of a form could be more important. Though none of the treatment arms are statistically distinguishable, the point estimates suggest that a simply-worded informational mailer that does not lead with language about felony convictions can be as effective (and possibly more effective) than mailers that begin with a focus on past criminal legal contact. This contrasts with our expectations, and raises the possibility that extra framing around criminal legal contact is stigmatizing, depressing

turnout. This is an area for further research.

Column 4 of Table 1 shows the mailers' effects on voter turnout in the November 2020 general election. The basic mailer increased voter turnout by 0.8 percentage points ($p < 0.05$). The mailer with no criminal record framing increased voter turnout by a noisily-estimated 0.7 percentage points ($p < 0.10$). The mailer without a registration form may have increased turnout by 0.3 percentage points (n.s.), and the mailer with extra civil rights framing appears to have increased voter turnout by 0.4 percentage points (n.s.). Again, we cannot reject the null hypothesis that these mailers all had equivalent effects. In Column 6, we estimate that sending any mailer increased voter turnout by 0.5 percentage points ($p < 0.10$), an overall increase of 11 percent. This estimate implies that a substantial fraction of people who were induced to register by our treatment mailers ultimately voted in the next election.

Given ongoing debates about the partisan implications of expanding the electorate, we also consider party registration among those who registered to vote. Table A4 in the Supporting Information shows the number of people registering with each party. Overall, the distribution of party registrations appears similar for the treatment and control groups in each sample, with nearly equal proportions of Democratic and Republican registrants as well as many non-party registrants in both groups. That is, it appears that our intervention was not disproportionately effective among people inclined to vote for one party over another. This is in keeping with past research demonstrating that enfranchising people with felony convictions is unlikely to give either party an electoral edge (Burch, 2011, 2012).

On the whole, it appears that sending mailers with basic information about voter eligibility can substantially increase voter registration and turnout among this group of people with low propensity to register and vote. In the next few sections, we consider heterogeneity in these effects and compare them to effects seen in other populations and in previous work.

Table 1: Effects on Voter Registration and Turnout (Percentage Points)

	Dependent variable:					
	Voter Registration			Voted November 2020		
	(1)	(2)	(3)	(4)	(5)	(6)
Basic mailer	0.8*	0.8*		0.8**	0.7**	
	(0.4)	(0.4)		(0.4)	(0.4)	
No crim. record framing	1.1**	1.1**		0.7*	0.7*	
	(0.4)	(0.4)		(0.4)	(0.4)	
No registration form	0.8*	0.8*		0.3	0.3	
	(0.4)	(0.4)		(0.4)	(0.4)	
Extra civil rights framing	0.6	0.6		0.4	0.4	
	(0.4)	(0.4)		(0.4)	(0.4)	
Any Treatment Mailer			0.8**			0.5*
			(0.3)			(0.3)
Constant	6.6***	4.3***	4.3***	4.6***	1.4**	1.4**
	(0.3)	(0.7)	(0.7)	(0.3)	(0.6)	(0.6)
Covariates		X	X		X	X
Observations	35,245	35,245	35,245	35,245	35,245	35,245

Note: *p<0.1; **p<0.05; ***p<0.01
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.

Effect Heterogeneity

Given Black Americans' disproportionate exposure to the criminal legal system, we are curious about whether this intervention is equally effective for potential voters who are Black. Table 2 thus presents regression results showing how the experimental treatment effects vary across race. Columns 1 and 2 present the effects for subgroups of Black and white individuals. Column 3 combines these groups and interacts "Black" with the treatment indicator to formally test for differential effects by race. Given the power limitations discussed above, we focus here on the effect of receiving any treatment mailer compared to control, rather than splitting out individual treatment arms by race.

These estimates suggest differences in the effectiveness of the mailers across racial groups. We see consistently large and positive treatment effect estimates for white mailer recipients, but not for Black mailer recipients. There is no evidence that our mailers increased participation for Black recipients; non-Black recipients appear to account for the positive turnout effects shown in Table 1. Whether looking at the analyses that subset to Black voters (columns one and four) or summing the interaction terms with the main treatment effects to get treatment-effect estimates for Black voters from the models in columns three and six, the point estimates are substantively small and near zero. The voter-turnout point estimates indicate a reduction in turnout of about one-fifth of one percentage point for Black voters who received the mailer, while the voter-registration point estimates indicate a positive effect of the same size. We cannot distinguish either of these effects from zero. Observed group differences in the effects of the mailer are not simply due to lower statistical power or higher baseline rates of registration among Black mailer recipients: Columns 1 and 2 contain almost the same number of observations, and the control-group registration rates differ by less than one percentage point.

We further wished to learn about address quality and experimental noncompliance in the form of undelivered mailers. We therefore ran a followup study, sending a postcard that invited people to take a survey – although the primary purpose of the study was to assess

Table 2: Racial Heterogeneity in Mailer Effects (Percentage Points)

	Dependent variable:					
	Voter Registration			Voted November 2020		
	Black	White	Both	Black	White	Both
	(1)	(2)	(3)	(4)	(5)	(6)
Any Treatment Mailer	0.3	1.3**	1.3***	−0.2	1.0**	1.0**
	(0.5)	(0.5)	(0.5)	(0.4)	(0.4)	(0.4)
Black			−0.8			−0.6
			(0.6)			(0.5)
Any Treatment Mailer * Black			−1.0			−1.2*
			(0.7)			(0.6)
Constant	6.3***	7.1***	7.1***	4.4***	5.1***	5.1***
	(0.5)	(0.5)	(0.4)	(0.4)	(0.4)	(0.4)
Observations	15,280	17,694	32,974	15,280	17,694	32,974
Note:	*p<0.1; **p<0.05; ***p<0.01 This table shows the effect of the treatment (sending a mailer) on voter registration/turnout by race.					

whether postcards landed in individuals' mailboxes and did so differentially by observable characteristics such as race. This study is described in detail in the SI. Postcards were more likely to bounce when sent to younger people, and less likely to bounce among those who had been released for longer periods of time. This makes some sense: it may be harder to obtain addresses for people who have been out for long periods of time, but with a visible address are more residentially stable. Crucially, there are no differences in observed bounce rate by race or gender. It does not appear that racial differences in the effect of the mailer were driven by lower-quality address data for Black individuals relative to their white counterparts.

An additional factor that could explain subgroup variation in the effect of our mailers is mobilization by other entities. In order to assess whether individuals included in our study also received mailers from other organizations, we worked together with the Voter Participation Center (VPC), a large voter-registration organization that blanketed North Carolina with registration-focused mailers in 2020. VPC compared their mailing lists to the list used for our main experiment and found that fewer than one-fourth of the people in

our sample were included in their list of unregistered people to contact. This comparison gives us information about one major organization, not all outreach activities in the state, but the pattern seen here suggests that many people in our study sample may not have received other eligibility information during the 2020 cycle. Moreover, voter mobilization organizations often target select groups of voters based on characteristics such as race or age, and many people included in this study do not fall into the categories often targeted. Indeed, further investigation suggests that about four in 10 Black individuals received VPC mailers in addition to ours, relative to one in 10 White individuals for whom the same is true. This disparity in outreach may explain the differential effectiveness of our mailer by race for the same reason our mailer is most effective for people with felony convictions: when individuals are not contacted by organizations traditionally engaged in outreach efforts, a single light-touch mailer can be particularly effective at mobilizing them. We therefore suspect that concurrent outreach efforts targeted to people of color in North Carolina in 2020 attenuated the impact of the mailers among Black individuals in our sample. However, other possibilities include that experiences with racism make Black recipients skeptical of this type of voter outreach. Further research is needed to understand how to effectively mobilize different racial groups into the electorate.

Finally, we test for heterogeneous effects across a number of dimensions using a machine-learning approach developed by Wager and Athey (2018).[13] This approach allows us to identify subgroups with larger treatment effects in a principled way that minimizes concerns about data mining. The full results of this analysis appear in Table A10 in the Supporting Information. Briefly, 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 (as opposed to supervision). We do not observe such clear patterns when it comes to age and time since release, although individuals in the highest

[13]Both the explicit test for racial heterogeneity and this machine-learning-based approach assessing heterogeneity across several dimensions were pre-registered.

treatment effect bin are less likely to be over the age of 55.

Comparison to People Without Felony Convictions

Readers may wonder whether there is added value to specifically targeting justice-impacted people for mobilization efforts. We have argued that several things are likely true of many returning citizens: 1) they are overlooked by other agents traditionally engaged in voter mobilization efforts (Owens and Walker, 2018); 2) they may not be certain of their eligibility to vote; and 3) they can be as interested in politics as other people, and perhaps even more so given their experiences with the criminal legal system (Owens, 2014; Walker, 2020). If any of these things are true, there should be added value to targeting them specifically for outreach. If not, we should not see much difference in the effectiveness of outreach between justice-impacted people and similarly situated people without criminal legal involvement. In order to evaluate this argument, we constructed a comparison group of people without felony convictions and fielded a parallel experiment in North Carolina during the 2020 election. We contracted with the same commercial data vendor who provided the addresses for the main study, this time to purchase a list of people from their address database who were not registered to vote and who did not appear in the sentencing records.

We focused this list on the six North Carolina zipcodes with the largest numbers of people from our main study sample, to limit the possibility that differences between the two samples were due to socioeconomic differences or different mobilization efforts across neighborhoods. Because we asked the data vendor to return a list of individuals who were both unregistered and not found in the sentencing records, we do not have any kind of demographic indicators for this comparison group. Instead, we rely on geographic indicators to construct a sample that is plausibly comparable to the sample of individuals with conviction records. Zipcode is itself a relatively small geographic unit, and neighborhoods are notably segregated by race and class. It may be that individuals in the comparison group have other kinds of criminal legal involvement of which we are unaware, such as misdemeanor convictions. In any case,

building a comparison group in this way allows us to examine variation in whether one has served a felony sentence. This process yielded a sample of 35,708 people.

We randomly assigned people in this second, no-felony-conviction sample to either an uncontacted control group or a treatment group that would be sent a basic mailer without extra conviction framing (the same mailer as those in the "no criminal record framing" treatment arm of our main study). In other words, individuals in the treatment group were simply provided with information about eligibility and how to register, including a registration form and pre-addressed, pre-paid return envelope.

The results of this parallel experiment are shown in Table A15 in the SI. They are precise null effects that are statistically distinguishable from the treatment effects observed in our main study sample (restricting the main study to the control group and the group that received the same treatment). It appears that our experimental treatment was uniquely effective among justice-impacted people, compared even to people living in the same neighborhoods during the same election season. Again, this pattern could be due to differences in existing mobilization efforts: if people in our main sample have been missed by other outreach efforts, the information we offer them about eligibility may have an especially large effect, whereas people without felony convictions who remain unregistered in a competitive state like North Carolina may have already received this information about registration in the past and nevertheless chosen to remain unregistered. Given that, as noted above, only a quarter of our sample received mailers from VPC, it is plausible that our outreach to unregistered people with past convictions is finding those who have not been contacted in the past, and is targeted to a group especially receptive to mobilization efforts.

Comparison to Prior Work

On average, our mailers increased registration and turnout by .8 and .5 percentage points.[14] The size of these effects may appear modest relative to the effects – 1.8 and .9 percentage point increases in registration and turnout – of a previous intervention targeting those with felony convictions (Gerber et al., 2015). However, that study targeted a narrow and relatively easy-to-find group of recently released non-violent offenders who had served minimal time (recall that we estimate it reached about 5% of voting-eligible former felons in Connecticut), and is thus not directly comparable to the sample we target here. Indeed, our motivation was not to replicate Gerber et al. (2015), but to develop a method to reach the much broader group of people with felony convictions who are eligible to vote. Readers may wonder how to understand the estimates derived from the present study relative to those observed in previous work. To better facilitate this comparison, we constructed a subset of our sample that closely approximates the narrower sample used by Gerber et al. (2015) and evaluated the impact of our treatment among that subgroup. We present the full details of this subsample and our analyses in SI section A6.

While the analyses in this section were not preregistered and are thus strictly exploratory, they shed some light on the conditions under which our mailers were most effective. Notably, the effect of our mailers on registration among the Gerber-comparison sample is very similar to that observed in the earlier study, increasing registration by 1.7 percentage points relative to the uncontacted control. The size of the effect on voting is larger than that observed in the earlier study, increasing turnout by 1.2 percentage points relative to the uncontacted control. Further probing suggests that the primary factor that attenuates the size of the main effects

[14] It would also be useful to provide effect-size benchmarks from mailer-based registration studies focused on the general public, but unfortunately such studies are vanishingly rare, with most work prioritizing voter turnout among already registered people. However, the handful of extant studies find effects comparable to those shown here, with turnout effects ranging from .8 to 2 percentage points (Mann and Bryant, 2020; Bryant et al., 2022)

of the present study is our inclusion of people who have never been incarcerated, even as they incurred a felony conviction. Among those who were never incarcerated, the impact of receiving any mailer on registration and voting is near zero. Among those who have experienced incarceration, the size of the impact of receiving a mailer on registration and voting grows to 1.4 and .9 percentage points respectively.[15] The seriousness of one's offense and time since release appears to matter little for the overall impact of receiving a mailer. While these findings are preliminary, they suggest that previously-incarcerated people may benefit particularly from political outreach. Further, they help contextualize the size of the main experimental effects.

Conclusion

Millions of people with felony convictions are eligible to vote (Manza and Uggen, 2008). In fact, people with felony convictions regain their rights at some point in the vast majority of states – and this is true for more and more people every day, as states continue to pass expansive reforms around felony disenfranchisement. Yet, participation rates for those with felony convictions are low and traditional mobilization campaigns tend not to prioritize returning citizens, because 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, especially 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,

[15] These conclusions mirror the finding of our pre-registered heterogeneity analysis described above, which noted that people in the bins with the highest average treatment effects were more likely to have been incarcerated.

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 randomized control trials that it is possible to increase registration and voting among this population. Importantly, even as justice impacted people may remain low-propensity voters, once registered they become visible to all manner of organizations interested in strategic get-out-the-vote campaigns, and repeated voter mobilization contacts may further increase their likelihood of turning out over time.

One contribution of this project is the method by which we constructed the sample. Departing from strategies employed in previous research which relied on state agencies, we used 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. In sum, we have found a way to construct a list that endeavors to be broader and more representative of the full population of eligible voters with felony convictions than previous work. Moreover, previous methods relied on a privileged relationship with the state – a relationship not necessarily available to the kinds of organizations interested in mobilizing justice-impacted people. The choice to rely on the state also constrained the pool of people scholars could reasonably target for mobilization efforts, since the state maintains records of the address to which an individual was initially released, and the likelihood that it is outdated increases with time since release. We obtained contact information from a private data vendor and worked together with a non-state actor to field the intervention. We have offered, at every stage in the process, benchmarks for both the sample and the impact (or lack thereof) of the various interventions (including those fielded in the pilots). We have developed a process that could be replicated by non-state actors and a roadmap to effective interventions for those interested in broadening the electorate. In so doing, we have tried to reorient questions of mobilization among people with felony convictions around service to the marginalized, and we can imagine this approach being useful for research on mobilizing other difficult-to-reach

or under-mobilized populations.

The second 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. This confirms findings from previous work, but a much larger sample and several additional tests for robustness and scope conditions generate new knowledge around how and when such mailers are effective. We find that such mailers appear most effective among white individuals. Our research suggests that this is not because of differences in address quality between white and Black individuals, but that it may be attributable to field saturation with mobilization efforts during the 2020 election often targeted directly to Black voters. We also find that our mailers work better among people who have incurred a felony conviction than among a comparison group of those who have not, and that they work best among people who have been incarcerated, relative to those who incurred a conviction but were never imprisoned. Finally, the effectiveness of the treatment was non-partisan.

Readers may wonder about the extraordinary circumstances of the 2020 election, and whether the findings here are generalizeable to other contexts. When we evaluate the impact of the mailers in the present study on the subset of individuals targeted in the 2012 general election in a different state by different researchers, we find our mailers had a remarkably similar impact on registration and turnout to the previous study. We therefore think the findings are generalizeable beyond North Carolina in 2020. Justice-impacted people are not lost to political life. Instead, they hold a stake in politics and many retain an interest in participating, and are thus ripe for mobilization.

The findings presented in this paper also suggest several areas for future research. While we test several variations in message wording, we do not find substantial differences across mailers. We conclude that the simple provision of information and resources to register is enough to increase registration among returning citizens. We do not interpret these findings to mean that justice-impacted people cannot be persuaded to engage in electoral politics,

nor that simply reminding people about the election is sufficient to reintegrate them into the electorate (as scholars have found elsewhere (Dale and Strauss, 2009)). Instead, mail-based messaging seems to be effective via the provision of information about eligibility and how to register. We consider these baseline estimates about the potential for voter mobilization campaigns to reach and reintegrate justice-impacted people into electoral politics, but we anticipate that other modes of contact may yield even higher participatory gains than can be achieved through a light-touch mail-based campaign.

In this spirit, it may be that a more tailored form of contact would further increase registration. Research about mobilizing marginalized people emphasizes the importance of trusted messengers grounded in the communities targeted for mobilization (Michelson, 2006; Valenzuela and Michelson, 2016), and based on this intuition, we worked together with a known organization in North Carolina, *You Can Vote*. However, we were unable to test the effectiveness of mailers branded by a non-profit organization relative to those branded by a state agency (as used by Gerber et al. (2015)). Future research might consider explicitly testing messages delivered by a state authority relative to a non-state actor, and should also consider employing a trusted messenger more deeply embedded in heavily policed communities. With respect to mode of contact, previous research suggests that members of marginalized groups may be more receptive to strategies that establish a personal connection through conversation. Our pilot studies suggest that phone and text message outreach are difficult to implement with this population due to out-of-date numbers being common, but other high-touch strategies such as neighborhood-intensive door-knocking efforts hold promise.

This study also raises questions about how to recover electoral participation among chronic non-voters across different electoral regimes. Increasingly, states are reducing the burden of registration through automatic voter registration schemes (this is now true for 23 states). In a context where registration is not an issue, the question for scholars becomes how to turn people out in a given electoral cycle – and this suggests more complex answers

that may be tied to local electoral contexts. Finally, important questions remain about how to more effectively mobilize members of different racial groups.

This project, which establishes a means of identifying and contacting returning citizens, lays the logistical groundwork for future studies. We have shown that simply reaching out and asking individuals to participate and giving them the means to do so can increase the baseline electoral engagement of a group that scholars and campaigns have, otherwise, largely treated as lacking in civic capacity. Many aspects of the findings presented are exploratory, and deserving of further scholarly attention. Nevertheless, when taken together with innovations in the method for identifying and contacting justice impacted people, we offer new insight into an increasingly relevant question in American politics: how to expand the size and composition of the electorate through recovering the engagement of chronic nonvoters – and in particular, the large group of justice impacted people who nevertheless retain the right to vote.

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Appendices

Supplemental Information

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A More Details on Main Experiment

A.1 Descriptive Statistics and Balance

Table A1 below shows descriptive statistics and tests of covariate balance for the main study described in the paper. 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.

A.2 Sample Construction

This study sought to find and contact people in North Carolina who were eligible to vote after a past felony conviction, but who were not registered to vote. Available estimates put this group of people at approximately 167,000-216,000 in North Carolina.[16] Across the pilots and main experiment, we found a sample of 72,213 people for whom we could determine voting eligibility and send mail to a current mailing address, representing over one-third of the population we sought to target in NC.

This coverage is remarkably high given the difficulty of finding and contacting this population. A previous study that collaborated with Connecticut's secretary of state's office to send out official letters informing people of their eligibility to vote reached a much smaller share of the state's eligible unregistered population.[17] That study reached approximately one of every twenty eligible Connecticut voters with past felonies[18], both because of government decisions to carve out people with specific conviction types as well as limitations of government address records: because that study relied on state address records, it was

[16]The full population of people with past felony convictions still living in North Carolina in 2010, who had completed their custodial sentences and supervision (and thus were eligible to vote) was about 295,000-335,000 [(Shannon et al., 2017) Online Appendix Table S9]. Burch estimated that 35.5% of eligible voters in North Carolina with past felony convictions were registered to vote, implying a 64.5% non-registration rate (Burch, 2011). Thus, we estimate that the target population in North Carolina is between approximately 295,000*.645 = 167,055 and 335,000*.645 = 216,075 people. Shannon, Sarah KS, Christopher Uggen, Jason Schnittker, Melissa Thompson, Sara Wakefield and Michael Massoglia. 2017. "The growth, scope, and spatial distribution of people with felony records in the United States, 1948–2010." Demography 54(5):1795–1818; Burch, Traci. 2011. "Turnout and party registration among criminal offenders in the 2008 general election." Law & Society Review 45(3):699–730.

[17]Gerber, Alan S, Gregory A Huber, Marc Meredith, Daniel R Biggers and David J Hendry. 2015. "Can incarcerated felons be (Re) integrated into the political system? Results from a field experiment." American Journal of Political Science 59(4):912–926.

[18]The full population of people in Connecticut with felony convictions (including people who were ineligible to vote while in custody or on parole) was estimated to be 223,000-266,000 in 2010 (Shannon et al., 2017). The ineligible population in CT in 2010 was estimated at 22,215, putting the range of eligible voters with past felonies at 200,785-243,785 (Uggen, Shannon and Manza, 2012). Gerber et al. (2015) estimate that 40.2 % of a sample of eligible voters with past felonies in Connecticut were registered, implying a 59.8% non-registration rate (Gerber et al., 2017). Thus, we estimate that the target population in Connecticut was between approximately 120,069 and 145,783. Uggen, Christopher, Sarah Shannon and Jeff Manza. 2012. State-level estimates of felon disenfranchisement in the United State, 2010. Sentencing Project Washington, DC; Gerber, Alan S, Gregory A Huber, Marc Meredith, Daniel R Biggers and David J Hendry. 2017. "Does incarceration reduce voting? Evidence about the political consequences of spending time in prison." The Journal of Politics 79(4):1130–1146.

constrained to contacting recently-released people for whom those address records were up-to-date. Our approach allows us to find and contact people who may have been convicted years or even decades earlier, thus reaching a much broader swath of the relevant population. It also provides a roadmap to organizations that may seek to help people register in states where government is not interested in collaborating in registration efforts.

A.2.1 Building the List for Main Experiment

As described in the main paper, we began with a database from NCDPS (North Carolina Department of Public Safety) of records for people who had been sentenced to DPS custody in North Carolina since the 1970's. This 1.2-million-row file contained many duplicates, along with many people who were not in our target population either because they were not eligible voters or because they had not been convicted of a felony. Figure A1 illustrates the process of paring the DPS dataset down to a list of people who appeared to be eligible voters who should be included in our study. We omitted people who were still serving sentences or who were known to be deceased, as well as people without clear felony records (denoted on the figure as "felony status unclear": some of this is due to unclear record keeping, but for most people this is because they only had misdemeanor convictions). We also omitted apparently duplicated records and those missing name information, as well as people over 70 (out of concern that many people would be deceased) and those whose records indicated they were not citizens. This left us with about a quarter-million people who we believed to be eligible voters in NC with past felony convictions; about half of this list had already been used in our pilot studies earlier in the year (this number is larger than our eventual pilot sample size because many people were already registered or did not match to addresses). Removing data that had been used for pilots left us with a list of 136,268 potential voters to look for. We worked with a commercial data vendor to find addresses for as many people on this list as possible while omitting people who were potentially already registered to vote, measuring existing registration quite conservatively.[19] This process yielded a main experimental sample of 35,245 people in North Carolina.

The process of paring down the DPS records and finding address matches involved trimming down the dataset in multiple stages, and we were curious about how the resulting final sample compared to the original DPS records or the full list of eligible voters we sought to find and contact. Figure A2 below traces the racial and gender composition of the dataset at various stages of sample construction. They are remarkably stable: the composition of the final sample is not dramatically different from the original DPS list or the list of eligible voters for whom we sought addresses. There are several expected differences based on our design choices: the decision to omit people over 70 means that the age composition of our final sample is slightly younger than the original DPS dataset and also that the median length of time since release is slightly shorter in our sample than in the full DPS dataset. But on the whole, this approach appears to yield a surprisingly representative sample of the

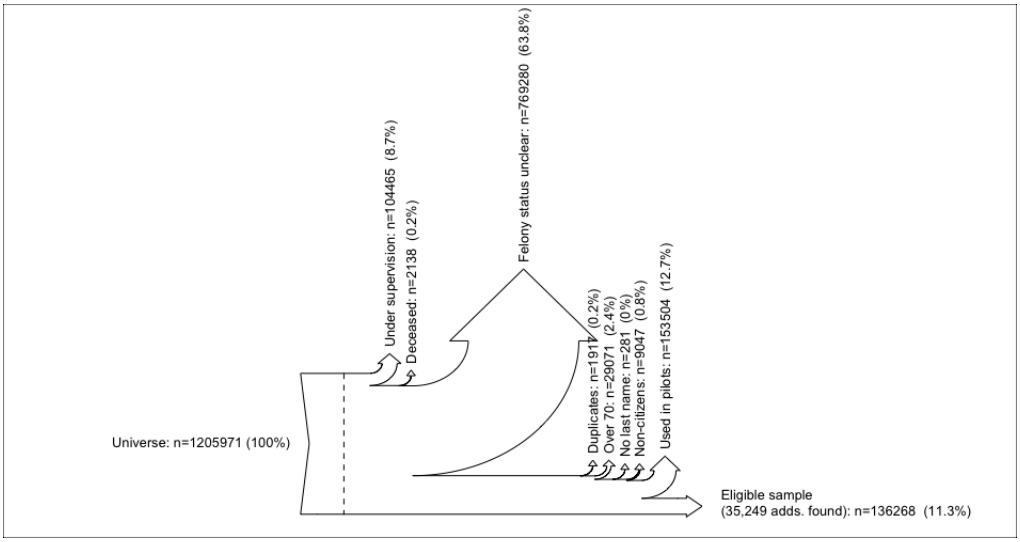
[19] Because the state's publicly-available voter files only include voter year of birth rather than exact date of birth, it can be difficult to be sure whether an apparent voter-file match (say, "John Smith" born in 1950) actually represents a voter registration for the person in question or a false positive. We erred on the side of being conservative and excluded from our study sample anyone for whom it appeared that they could possibly be registered to vote already, in order to avoid confusing people about their registration status.

overall population, at least on visible characteristics.

Table A1: Main Study: Covariate balance across treatment arms

		Difference from Control				
	Control Mean	Basic Mailer	No CR Framing	No Reg. Form	Civil Rights Framing	Joint F-test p-val
Male	0.75	0.00	-0.00	0.00	-0.00	0.89
		(0.01)	(0.01)	(0.01)	(0.01)	
Age	43.75	0.25	0.18	0.23	0.38	0.40
		(0.19)	(0.19)	(0.19)	(0.19)	
Black	0.43	0.01	0.00	0.00	0.02	0.18
		(0.01)	(0.01)	(0.01)	(0.01)	
Past Incarc.	0.57	0.00	-0.01	-0.01	-0.01	0.56
		(0.01)	(0.01)	(0.01)	(0.01)	
Days since release	3474.65	3.27	58.59	-49.04	16.84	0.73
		(75.51)	(75.84)	(75.83)	(75.72)	
Observations	7049	7049	7049	7049	7049	

Figure A1: Construction of Sample

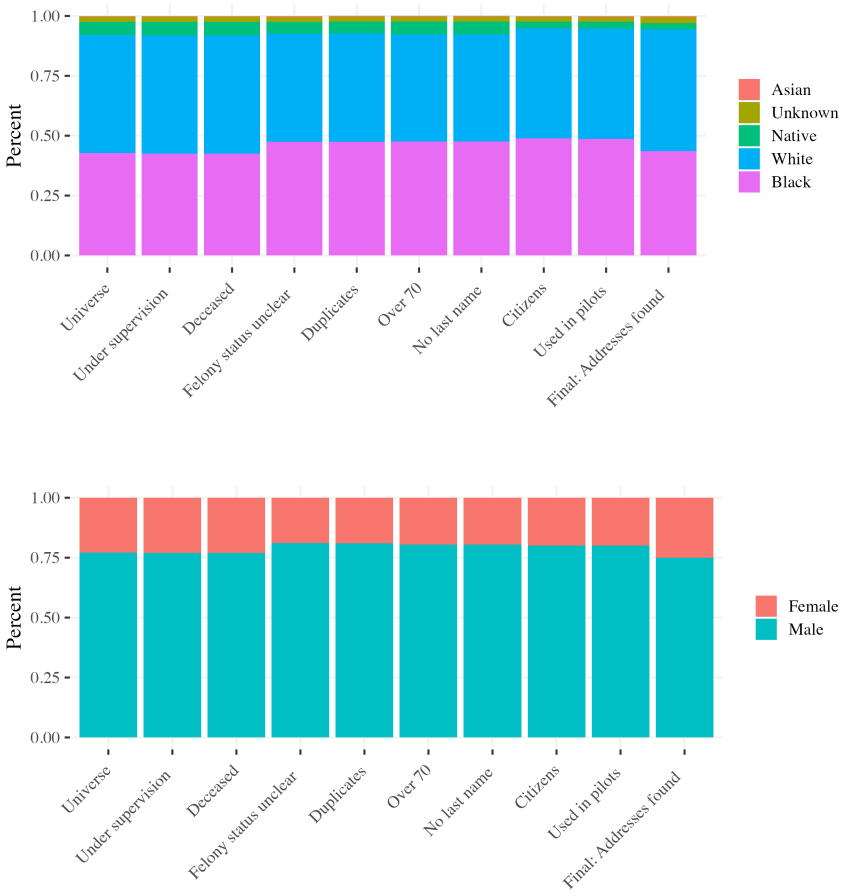


Notes: This figure shows the process of paring down from North Carolina's DPS database to the sample of voting-eligible unregistered people with past felony convictions for the main study sample.

A.3 Experimental Mailers

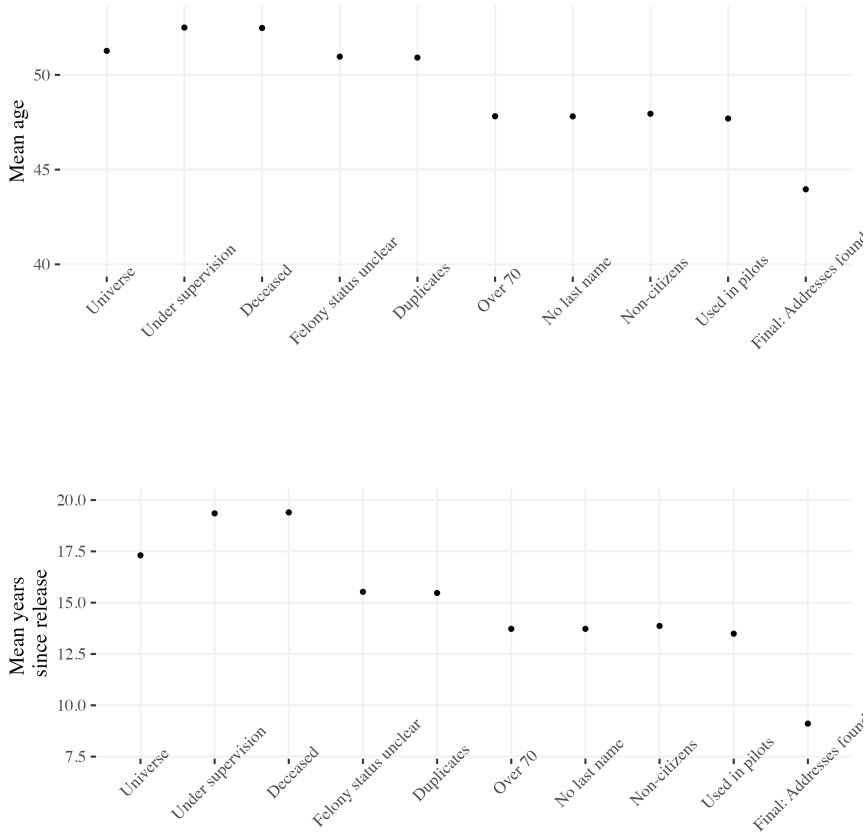
Figure A4 below presents all four letter variations associated with the treatment arms described in the main paper.

Figure A2: Describing Data Loss: Race and Gender



Notes: This figure shows how the racial (top panel) and gender (bottom panel) composition of the dataset changed as we omitted DPS records to arrive at our final experimental sample.

Figure A3: Describing Data Loss: Age and Time Since Release



Notes: This figure shows how the age (top panel) and time-since-release (bottom panel) composition of the dataset changed as we omitted DPS records to arrive at our final experimental sample.

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/register** 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,

Kate Fellman
Executive Director

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

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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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.)

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!

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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.**

Thank you,

Kate Fellman
Executive Director

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Figure A4: All Mailer Variations from Fall 2020 Experiment

A.4 Partisanship

Table A2 below shows the number of people registering with each party among those who registered to vote. Column 1 shows the number of new registrants by party, for those assigned to a treatment group from our main study sample. Column 2 shows the equivalent numbers for those assigned to the control group in the main study. Columns 3 and 4 show the numbers for people from our no-criminal-record comparison group (parallel experiment described below). 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 in 2020, 36% of new registrants registered as Democrats, 35% registered as Republicans, 0.7% registered as Libertarians, and 28% registered as unaffiliated.

Table A2: 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

A.5 Comparison to Gerber et al. (2015)

In order to get a clearer sense of how our intervention compared to that of Gerber et al. (2015), we constructed a narrower subset of our experimental sample that closely resembled the sample used for that prior study. There are four ways in which the sample in Gerber et al. (2015) is narrower than the one used in this experiment: 1) they filtered on conviction type to omit people who had committed specific types of violent offenses; 2) they omitted anyone who was not incarcerated; 3) they target a set of dates between which individuals must have been both convicted and released – functionally, this means that they omitted anyone who had been out for longer than three years, and who had been sentenced to more than three years; and 4) they omitted additional offenses for which only one or two people are listed as having been convicted. In our sample, we can determine the following: time since release, seriousness of offense, and whether one was incarcerated. We cannot cleanly determine the length of their sentence. Our reconstruction therefore approximates the strategy taken by Gerber et al. (2015) as closely as possible given available data. Then, we run our main analysis within this comparable subset to see how the treatment effects in this experiment compare to those in Gerber et al. (2015).

The analysis that follows is exploratory and was not preregistered. Nevertheless, it sheds some insight into the value of targeting a broad swath of individuals with convictions who are eligible to vote, irrespective of time since release or offense (for example). Table A3 displays the sample size for each category for which we have information. Our full sample

includes 35,245. Only 6,089 individuals qualify as part of the approximate Gerber et al. (2015), having been incarcerated, been out for three years or less, and having been convicted of a non-violent offense. This is about similar to the size of Gerber et al. (2015)'s sample. Tables A4-A5 display models evaluating the impact of receiving any mailer on registration (Table A4) and voting (Table A5). Tables A6-A7 display models evaluating the impact of each treatment arm on registration (Table A6) and voting (Table A7). Column 1 of each table reports the main findings from the experiment. Column 2 reports the impact of our treatments among only those who experienced incarceration. Column 3 reports the impact of our treatments among those who were not convicted of violent offenses. Column 4 reports the impact of our treatments among those who have been released for three years or less. These three columns do not omit people successively. Instead, Column 5 combines all these filters and evaluates the impact of our treatments among the narrow sample that approximates Gerber et al. (2015).

Across both registration and voting, the size of the coefficient increases as the sample is constricted in various ways in keeping with choices made by previous scholars. Notably, the size of the effect among those who most closely approximate the sample included in Gerber et al. (2015) is almost identical to what the authors found (an increase in registration of 1.8 percentage points among those who received a mailer relative to an uncontacted control). The size of the effect of our treatments on voter turnout exceeded those observed by previous scholars – 1.2 percentage points in the present study, relative to an increase of .9 percentage points observed in previous work.

Further investigation suggests that in the present study, the main factor attenuating the size of the effect is the inclusion of individuals with felony convictions *who never experienced incarceration*. In our study, about 40 percent of the sample has a felony conviction, but was never sentenced to incarceration. Among those who were never incarcerated, the mean registration absent any treatment was 7.5 percent. Among those who experienced incarceration, the mean registration among the uncontacted control group was 5.8 percent. Similarly, those who experienced incarceration and were not treated turned out at a rate of 5.6 percent, relative to 3.7 percent among those in the control group who were incarcerated. We do not observe meaningful differences along any other dimension used by Gerber et al. (2015) to delineate inclusion in their sample. Time since release does attenuate the size of the effects somewhat, but not such that it renders the impact of the treatment null. While these findings are preliminary, they do suggest that there is particular value in targeting formerly incarcerated individuals who are eligible to vote for mobilization efforts.

Table A3: Doleac et al. vs. Gerber et al. Samples

	N value
Total sample	35245
Never incarcerated	15196
Omitted Offenses	3228
Out for 3+ years	13421
Gerber comparable sample	6089

Table A4: Impact of receiving any mailer on registration, by sample

	Total sample	Incarcerated (only)	Violent offenses (omitted)	Out for 3+ (omitted)	Gerber comparable
	(1)	(2)	(3)	(4)	(5)
Any mailer	0.8**	1.4***	1.2**	1.7**	1.7**
	(0.3)	(0.4)	(0.5)	(0.8)	(0.8)
Constant	6.6***	5.9***	5.9***	5.9***	5.5***
	(0.3)	(0.4)	(0.4)	(0.7)	(0.7)
Observations	35,245	20,049	16,813	6,620	6,089
R^2	0.000	0.000	0.000	0.001	0.001
Adjusted R^2	0.000	0.000	0.000	0.001	0.001
Residual Std. Error	25.9 (df = 35243)	25.5 (df = 20047)	25.3 (df = 16811)	25.9 (df = 6618)	25.2 (df = 6087)

Note: *p<0.1; **p<0.05; ***p<0.01

Table A5: Impact of receiving any mailer on voting in 2020, by sample

	Total sample	Incarcerated (only)	Violent offenses (omitted)	Out for 3+ (omitted)	Gerber comparable
	(1)	(2)	(3)	(4)	(5)
Any mailer	0.5*	0.9**	0.8**	1.1*	1.2**
	(0.3)	(0.4)	(0.4)	(0.6)	(0.6)
Constant	4.6***	3.7***	3.6***	3.3***	2.9***
	(0.3)	(0.3)	(0.3)	(0.5)	(0.5)
Observations	35,245	20,049	16,813	6,620	6,089
R^2	0.000	0.000	0.000	0.000	0.001
Adjusted R^2	0.000	0.000	0.000	0.000	0.001
Residual Std. Error	21.8 (df = 35243)	20.7 (df = 20047)	20.3 (df = 16811)	20.1 (df = 6618)	19.3 (df = 6087)

Note: *p<0.1; **p<0.05; ***p<0.01

Table A6: Impact of each treatment on registration, by sample

	Total sample	Incarcerated (only)	Violent offenses (omitted)	Out for 3+ (omitted)	Gerber comparable
	(1)	(2)	(3)	(4)	(5)
Basic mailer	0.8*	1.2**	1.4**	1.3	1.5
	(0.4)	(0.6)	(0.6)	(1.0)	(1.0)
No criminal record framing	1.1**	1.7***	1.2*	2.2**	1.8*
	(0.4)	(0.6)	(0.6)	(1.0)	(1.0)
No registration form	0.8*	1.6***	1.6***	1.4	1.7*
	(0.4)	(0.6)	(0.6)	(1.0)	(1.0)
Extra civil rights framing	0.6	1.2**	0.8	2.0**	1.6
	(0.4)	(0.6)	(0.6)	(1.0)	(1.0)
Constant	6.6***	5.9***	5.9***	5.9***	5.5***
	(0.3)	(0.4)	(0.4)	(0.7)	(0.7)
Observations	35,245	20,049	16,813	6,620	6,089
R^2	0.000	0.001	0.000	0.001	0.001
Adjusted R^2	0.000	0.000	0.000	0.000	0.000
Residual Std. Error	25.9 (df = 35240)	25.5 (df = 20044)	25.3 (df = 16808)	25.9 (df = 6615)	25.2 (df = 6084)

Note: *p<0.1; **p<0.05; ***p<0.01

Table A7: Impact of each treatment on voting in 2020, by sample

	Total sample	Incarcerated (only)	Violent offenses (omitted)	Out for 3+ (omitted)	Gerber comparable
	(1)	(2)	(3)	(4)	(5)
Basic mailer	0.8**	1.0**	1.1**	1.7**	2.0***
	(0.4)	(0.5)	(0.5)	(0.8)	(0.8)
No criminal record framing	0.7*	1.2***	0.8	1.0	0.8
	(0.4)	(0.5)	(0.5)	(0.8)	(0.8)
No registration form	0.3	0.8*	1.0**	0.6	1.1
	(0.4)	(0.5)	(0.5)	(0.8)	(0.8)
Extra civil rights framing	0.4	0.7	0.4	1.1	0.9
	(0.4)	(0.5)	(0.5)	(0.8)	(0.8)
Constant	4.6***	3.7***	3.6***	3.3***	2.9***
	(0.3)	(0.3)	(0.3)	(0.5)	(0.5)
Observations	35,245	20,049	16,813	6,620	6,089
R^2	0.000	0.000	0.000	0.001	0.001
Adjusted R^2	0.000	0.000	0.000	0.000	0.001
Residual Std. Error	21.8 (df = 35240)	20.7 (df = 20044)	20.3 (df = 16808)	20.1 (df = 6615)	19.3 (df = 6084)

Note: *p<0.1; **p<0.05; ***p<0.01

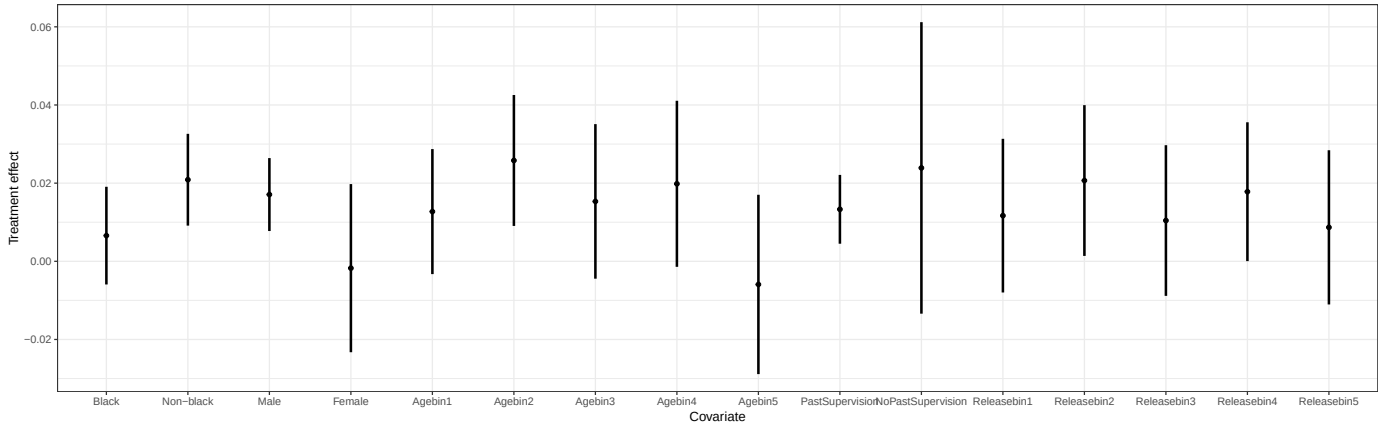

Figure A5: Subgroup treatment effect estimates from ML approach

A.6 Effect Heterogeneity

As discussed in the main paper, we examine effect heterogeneity in two ways, both pre-registered. First, we examine possible differences in treatment effects by the race of the person contacted, shown in the main paper.

In addition to these differential effects by race, we test for heterogeneous effects using a machine-learning approach developed by Wager and Athey (2018) and applied in Davis and Heller (2017).[20] 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.

We use the following characteristics to examine heterogeneity: gender, race/ethnicity, past supervision, age (binned into quintiles), and time since release (binned into quintiles; dropping people never incarcerated).

There are numerous possible ways of visualizing the results of this approach. Table **??** presents variable importance from this exercise, highlighting that gender as well as certain age groupings were particularly predictive of variation. Figure A5 presents subgroup estimates of treatment effects on registration, illustrating both the racial differences presented in the paper as well as some potential heterogeneity by age (bin 5 includes people above age 55, who are less likely to show large treatment effects)

[20] Wager, Stefan and Susan Athey. 2018. "Estimation and inference of heterogeneous treatment effects using random forests." Journal of the American Statistical Association 113(523):1228–1242; Davis, Jonathan and Sara B Heller. 2017. "Using causal forests to predict treatment hetero- geneity: An application to summer jobs." American Economic Review 107(5):546–50.

variable	importance
male1	0.20
agebin3	0.19
wrublack1	0.11
agebin4	0.11
agebin5	0.07
releasebin2	0.06
releasebin3	0.06
releasebin5	0.06
agebin2	0.05
releasebin4	0.05
pastsupervision1	0.04
wruhispanic1	0.00

Table A8: Variable importance from causal-forest approach to effect heterogeneity

A.7 Address Quality Followup Study

As noted in the main paper, we observed suggestive evidence of racial differences in the effectiveness of the mailer treatments. One possible reason for such differences could be address quality differences: if the addresses we found for Black residents in the sample were especially likely to be out-of-date or simply wrong, we could see smaller effects for Black than white letter recipients even if the letters were equally effective once delivered. We explore this possible explanation with a followup study fielded in summer 2021.

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 A8 shows how demographic characteristics correlated with whether the postcard "bounced" (whether it was not successfully delivered). We interpret a bounce as an indicator of having an incorrect address. 86% of the postcards were successfully delivered — a high success rate for a relatively hard to reach population. Postcards were less likely to bounce if they were sent to men, more likely to bounce as the intended recipient aged, and less likely to bounce as time since release increased. Race does not predict whether a postcard bounced. It thus appears that the racial disparities in our estimates are not driven by racial differences in address quality.

Table A9: Postcard Followup: Predicting Bounced Mailers

	Dependent variable:
	Bounced
Male	-0.012^{*}
	(0.007)
Black	0.004
	(0.005)
Age	0.001^{***}
	(0.0003)
Time since Release	-0.002^{***}
	(0.0003)
Constant	0.127^{***}
	(0.013)
Observations	20,041

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

A.8 Texas Sample

The study 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. Primarily, we faced issues with mail delays 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 some sample construction problems, [21] we are extremely doubtful about the treatment implementation. It seems highly likely that the mailers arrived too late to meaningfully affect registration in time for participation in the November 2020 election. This makes the results of the Texas arm of the experiment unhelpful for determining whether our intervention affects those outcomes. Nevertheless, Table A9 below presents estimates from Texas side-by-side with the main paper's estimates from NC.

A.9 Ethical Considerations

We made numerous decisions throughout the design and implementation of this study aimed at minimizing potential harms that could be caused by our study. We knew that some jurisdictions are imposing criminal penalties on people who are ineligible to vote but mistakenly register. This informed several decisions.

First, we avoided states where either we or potential registrants might find it difficult to assess their eligibility status. For example, in Florida, voters who have completed their sentences and paid all fines and fees were eligible to vote – but the state provided no mechanism for researchers, voter registration organizations, or even voters themselves to check whether they had unpaid fines and fees. We did not conduct research in Florida because we did not want to risk encouraging someone to register when we could not be sure of their eligibility, and they might be criminally penalized for erroneously registering.[22]

Second, we were extremely conservative in selecting our sample. We excluded people with incomplete information, because we did not want to risk encouraging someone to register when they were not eligible. Third, we recognized that there would always be a degree of uncertainty. We therefore designed our mailers to avoid telling people that they personally

[21] There were errors in the processing of the administrative data. Due to these issues, we inadvertently included some individuals whose status was unclear and thus may have not been eligible to vote (28% of the Texas sample), who were still under supervision and were definitely not eligible to vote (2.5% of the Texas sample), or alternatively who were already registered and thus outside the target population of the study (at least 11% of the Texas sample).

[22] Indeed, the state of Florida has recently begun prosecuting people who inadvertently voted while still owing legal financial obligations, including some who had been told by local election officials that they were eligible.

were or were not eligible to register and vote. Instead, we informed them about the eligibility requirements for registration, including the requirements related to criminal legal system involvement.

This final ethical choice – designing our mailers to avoid determinations of individual status – also addressed privacy concerns for people who received the mailers. We knew that mail addressed to one person might end up in other hands. Thus, we designed our mailers to avoid disclosing any information about the recipient's criminal legal system involvement. Taken in total, these choices reflect our commitment to foreseeing and avoiding unintentional harm to mailer recipients. They also highlight the role of state sanctions in the ethics of voter registration for returning citizens. Our results are conservative: if states provided clear individual eligibility determinations and removed criminal punishments for good faith errors, the reachable population would likely be significantly larger.

Table A10: Effects on Voter Registration and Turnout in NC and TX

	Dependent variable:					
	Voter Registration			Voted November 2020		
	NC	TX	All	NC	TX	All
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Arms Combined						
Any Treatment	0.8**	−0.2	0.1	0.5*	0.1	0.2
	(0.3)	(0.3)	(0.2)	(0.3)	(0.2)	(0.2)
Panel B: Separate Treatment Arms						
Basic mailer	0.8*	0.1	0.3	0.8**	0.3	0.4*
	(0.4)	(0.4)	(0.3)	(0.4)	(0.3)	(0.2)
No crim. record framing	1.1**	−0.2	0.2	0.7*	0.2	0.4
	(0.4)	(0.4)	(0.3)	(0.4)	(0.3)	(0.2)
No registration form	0.8*	−0.5	−0.2	0.3	−0.2	−0.1
	(0.4)	(0.4)	(0.3)	(0.4)	(0.3)	(0.2)
Extra civil rights framing	0.6	−0.1	0.1	0.4	−0.03	0.1
	(0.4)	(0.4)	(0.3)	(0.4)	(0.3)	(0.2)
Control Mean	6.6	14.61	12.35	4.55	7.86	6.92
Observations	35,245	89,750	124,995	35,245	89,750	124,995

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, both in our main NC sample and in a sample in TX where we encountered implementation problems. *p<0.1; **p<0.05; ***p<0.01

A.10 Pilot Studies

Prior to our main experiment in fall 2020, we conducted three smaller pilot experiments earlier in 2020 to test out various components of the design. All the pilots followed a similar structure to the main experiment in terms of sample construction (using administrative data to find eligible unregistered voters, then commercial address data to find current contact information) and random assignment to treatment, but they vary in the exact treatment conditions included. Those pilots are described briefly here.

Pilot 1 took place in January 2020, in time for letter recipients to register and vote in the March 2020 primary election. With a sample of 8,621 people, we randomly assigned people with equal probability to an uncontacted control condition or to a control group that would be mailed a simple informational mailer about registration and a registration form with postage-paid reply envelope.[23]

Pilot 2 was fielded in March 2020. We assigned a sample of 6,584 people to three treatment arms: (1) an uncontacted 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 inclusion of a text-message followup made this sample slightly different from those used in Pilot 1 since we restricted not only to eligible people for whom our data vendor could find a mailing address but also to those with available telephone numbers. Readers will notice that Pilot 2 took place just as most of the country went into lockdown due to the COVID pandemic, potentially explaining the null effects seen in this pilot relative to the other three studies reported here.

Pilot 3 was fielded in June 2020, with the goal of testing 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.[24] 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 assigned a sample of 21,763 across four treatment arms: (1) a control group, (2) a group who received a basic mailer without YCV branding (again replicating the first study), (3) a group who received the mailer with YCV branding, and (4) a group who received both the YCV-branded mailer and a follow-up call from YCV staff and volunteers.

Table A10 presents estimates of each pilot study's effects on voter registration as of 30 days after the mailers were sent. Panel A presents effects of assignment to any treatment arm (relative to control), while Panel B separates out each treatment arm for studies with multiple arms. Columns 1-3 present each pilot study separately, while Column 4 pools all three studies together.

On the whole, the pilot studies showed the effectiveness of this mailer-based approach to registration, with the exception of pilot 2 (which was fielded during the COVID lockdown

[23]The text of this mailer was relatively similar to the "basic mailer" in our main study, but included information about the primary and was not branded with the logo of the nonprofit organization with whom we collaborated on the main study.

[24]Sinclair, Betsy, Margaret McConnell and Melissa R Michelson. 2013. "Local canvassing: The efficacy of grassroots voter mobilization." Political Communication 30(1):42–57; Michelson, Melissa R. 2006. "Mobilizing the Latino youth vote: Some experimental results." Social Science Quarterly 87(5):1188–1206.

period, when we imagine people may have had other things on their minds besides voter registration[25]). While we hesitate to make much of small differences in the point estimates across studies given the sample sizes and differences in list-building approach, Pilot 1 appears to have had the largest effect, potentially because it fell just before a high-profile primary election rather than after it as the other two pilots did. In Pilots 2 and 3, we did not see any indication that adding a followup text message or phone call increased mailer effectiveness, potentially because many of the phone numbers used seemed to be out of date (based on reports from the volunteers doing followup calls in Pilot 3, they no longer belonged to the person we were trying to reach), so we did not include phone or text followups in the main study design.[26] In Pilot 3, we could not statistically distinguish between the different treatment arms, but the point estimates suggested that the mailer produced in collaboration with a non-profit partner organization (and bearing their logo) might be more effective than a simple mailer without that branding. We continued this collaboration for the main experiment (as seen in the mailer images below).

Table A11 presents estimates of each pilot study's effects on voter turnout. Column 1 examines Pilot 1's effect on March 2020 primary voting, since that pilot occurred before the primary. Columns 2-4 look at each pilot's effect on eventual turnout in the November 2020 general election, and Column 5 pools all three studies together to look at November turnout. Pilot 1 had a small but significant effect on primary turnout shortly after the mailers went out. Long-run effects of the pilot studies on eventual general-election turnout are less clear, with largely positive but non-significant point estimates. The pooled estimate in Panel A Column 5 suggests a combined effect of about .3 percentage points' increase in turnout, slightly smaller than the .5 percentage-point effect seen in our main experiment and not statistically distinguishable from zero. These pilot studies were not designed or powered to measure long-run turnout effects, and we interpret these with caution: it is hard to know whether the effects of the intervention are short-run effects that "wash out" with time as other outreach efforts find some people in the control group, or whether these are simply noisily-estimated positive effects.

[25]Indeed, the Center for Election Innovation & Research found that new voter registrations in all twelve states it examined, including North Carolina, dropped off dramatically in March 2020: https://electioninnovation.org/research/new-voter-registrations-in-2020/

[26]Readers may wonder whether the use of phone numbers in the pilot and not in the main study shifts the sample and thus address quality of the main study: did we use all the "good records" with phone numbers in the pilot and then the remainder in the main study? The short answer is no: for the pilots, we drew random samples of the full list of eligible post-conviction people in the state, then pared down (for example, to those with both address matches and telephone records, for pilot 3) within those random samples. So the main study sample did not disproportionately include people without telephone record matches.

Table A11: Studies 1-3: Effects on Voter Registration

	Dependent variable:			
	Registration			
	Pilot 1	Pilot 2	Pilot 3	All
	(1)	(2)	(3)	(4)
Panel A: All Arms Combined				
Treatment (Any mailer)	1.3***	0.2	0.9***	0.9***
	(0.3)	(0.4)	(0.2)	(0.2)
Panel B: Separate Treatment Arms				
Basic Mailer	1.3***	−0.05	0.7***	0.8***
	(0.3)	(0.4)	(0.3)	(0.2)
Basic Mailer + Text		0.5		0.9**
		(0.4)		(0.4)
Branded Mailer			1.0***	1.1***
			(0.3)	(0.2)
Branded Mailer + Call			1.0***	1.0***
			(0.3)	(0.2)
Study Fixed Effects				X
Control Group Mean	0.74	2.05	1.14	0.96
Observations	8,621	6,584	21,763	36,968
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A12: Studies 1-3: Effects on Voter Turnout

	Dependent variable:				
	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)	0.2*	−0.02	0.7	0.4	0.3
	(0.1)	(0.5)	(0.6)	(0.4)	(0.3)
Panel B: Separate Treatment Arms					
Basic Mailer	0.2*	−0.02	0.7	0.7	0.4
	(0.1)	(0.5)	(0.7)	(0.5)	(0.3)
Basic Mailer + Text			0.8		0.7
			(0.7)		(0.6)
Branded Mailer				0.1	−0.03
				(0.5)	(0.4)
Branded Mailer + Call				0.4	0.3
				(0.5)	(0.4)
Study fixed effects					X
Control Group Mean	0.21	5.64	5.47	6.03	5.45
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

A.11 Comparison-Group Study Fall 2020

We fielded a parallel experiment simultaneously with our main study in fall 2020, this one focused on unregistered people who did not have past felony convictions. Table A12 presents estimated treatment effects from that experiment and compares them to the main experimental effects. Column 1 shows the effect of our treatment on registration for the no-felony-record comparison group. The coefficient is near-zero and statistically insignificant. Column 2 shows the treatment effect from the comparable treatment arm in the main study; our mailers increased voter registration among people with felony convictions by 1.1 percentage points (16%; $p < 0.05$). Column 3 combines both experimental datasets to test for different effects across the two samples, and the treatment is significantly more effective among the main-study sample of people with past records. Columns 5-8 of Table A12 show the effects on turnout. Column 4 indicates that our treatment had no effect on turnout among the comparison group, while Column 5 shows the comparable treatment effect from the main study, for individuals with a felony conviction, is 0.7 percentage points (15%; $p < 0.10$). The difference in the effects across these groups is marginally significant.

Table A13: Treatment Effects for Comparison Group Versus Main Study Group

	Dependent variable:					
	Voter Registration			Voter Turnout		
	Comparison	Main Study	All	Comparison	Main Study	All
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment mailer	−0.03	1.1**	−0.03	−0.03	0.7*	−0.03
	(0.2)	(0.4)	(0.2)	(0.2)	(0.4)	(0.2)
Criminal record group			2.1***			1.1***
			(0.3)			(0.3)
Treatment * Record			1.1**			0.8*
			(0.5)			(0.4)
Control Mean	4.77	6.87	5.37	3.658	4.77	3.972
Observations	35,708	14,098	49,806	35,708	14,098	49,806
Note:						*p<0.1; **p<0.05; ***p<0.01