

Bridges to the Ballot: How loved ones can mobilize voters with criminal records

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Abstract

Many Americans are eligible to vote after a felony conviction or other criminal legal contact, but few use that right. We highlight the role that close social ties to active voters can play in helping people with records become politically active. Qualitative interviews establish the strategies family members and other loved ones use to help or pressure individuals with criminal legal contact to register and vote. Administrative data on births, convictions, and voting in Texas illustrate both that these kinds of social connections to voters are widespread among those with records, and that they are highly predictive of voting. Finally, we demonstrate the causal nature of this relationship with field experiments that encourage people to help their loved ones with criminal records register and vote. These findings point to new ways to activate voters with previous convictions, as well as other groups of unlikely or hard-to-reach voters.

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1 Introduction

Although many states disenfranchise people who are serving felony sentences, most people with convictions eventually regain the right to vote (*Felon voting rights*, 2021; Uggen et al., 2020). About 16 million Americans are currently eligible to vote after a felony. Many more who have experienced arrests, felony charges, or misdemeanor convictions—all of whom comprise the group we will call “system-impacted”—never lose the right to vote. Nevertheless, existing literature describes both low rates of participation (Burch, 2011; White and Nguyen, 2022) and high rates of disadvantage and social isolation (Western, 2006; Wildeman and Western, 2010) among members of this group. We ask: how can system-impacted people be (re)incorporated into electoral politics?

People with criminal legal contact often lack the resources thought to drive political participation, and existing literature does not identify many pathways back into political life. We argue, however, that people with criminal records have the potential to become politically active, and that many have access to a crucial resource that can help: close ties to active voters. These politically active family members and loved ones can undertake a range of activities, from providing information about voting eligibility and registration, to exerting social pressure to participate, to offering persuasive personalized arguments about the importance of voting. We establish that some family members already do this sort of political mobilization on their own. We also show that many people who might not think to encourage their loved ones to register can nevertheless be moved to do so. We thus believe that social connections to voters represent an important and partially untapped reservoir for the mobilization of otherwise unlikely voters.

In this paper, we first present a case study of a common type of social tie—siblings—and voting among people with felony convictions in Texas. Using birth, voting, and conviction records, we identify approximately 208,000 people with felonies who are currently eligible to vote, as well as their Texas-born siblings. Though we do not observe whether people are still in contact with their siblings, or other close relationships that could matter, this exercise

teaches us that such ties are widespread and important. We show that many people with criminal records have family connections to the world of voting, a previously unobserved pattern. Over half our sample has a sibling who is registered, and having a voter sibling is highly predictive of an individual’s participation: people with felony convictions were about six percentage points more likely to be registered, and three and a half percentage points more likely to vote in fall 2024, if they had a voter sibling than if their sibling was unregistered. These differences in participation by sibling ties are similar in size to the Black-White racial turnout gap in this sample (Fraga, 2018). The estimates, though observational, suggest that close family relationships could profoundly shape the voting lives of people with convictions.

We then present results from two field experimental tests of the power of social ties. The first takes a relational organizing approach (Green and McClellan, 2020). We worked with a grassroots organization to recruit a group of organizers with deep knowledge of the legal system. These organizers provided lists of system-impacted people in their networks who they thought were unlikely to vote. They then contacted a random selection of the individuals on their lists to encourage them to participate. This approach allows us to assess relational organizing as it occurs in the real world. Collecting rich qualitative data during and after the experimental period allows us to characterize the outreach work that social ties can do and document challenges in implementing the design. We use evidence from interviews with organizers to describe the help they provided to their loved ones, some of which was consistent with existing research on the power of social relationships (Sinclair, 2012; Gerber, Green and Larimer, 2008) and some of which was specific to system-impacted individuals. Then we present experimental treatment effects on voter registration and turnout. Despite unique challenges related to this approach, we find suggestive evidence of the power of this highly-personal method of outreach for registering otherwise hard-to-find voters.

In the second experiment, we use mailers to “nudge” voters to get loved ones with records registered. We begin with a list of voting-eligible people with felony cases and use a variety of data sources to identify their already-registered social ties. We then send a mailer en-

couraging voter registration either directly to the system-impacted person, or to their social tie (Sinclair, McConnell and Michelson, 2013). We find that mobilization through social ties can significantly increase voter registration among people with felony records. These effects are concentrated among individuals for whom we targeted close ties (household-sharing parent/child, spousal, or sibling connections, or those with a shared last name). These experimental results highlight the potential of this mobilization approach, since we show that family members who have presumably not already been doing voter mobilization work can do so if asked.

We find that already-registered social ties can mobilize their system-impacted loved ones and thus represent an important civic resource. This work has important implications for sociological research on family support for people dealing with the criminal legal system, showing that this support can also bolster political engagement. We extend work on the motivating role of social ties in politics, finding that they are widespread and powerful forces even in the lives of people often thought to be cut off from formal political life, or doubtful of the value of voting. Finally, our experimental results point to a set of interventions that we think could be fruitfully applied beyond system-impacted people to bring a range of otherwise hard-to-reach or disaffected voters into political life.

2 Background: Convictions, Voting, and Social Ties

Reading the existing literature on participation among system-impacted people could lead one to conclude that this group is lost to political life. Registration and voting among people with legal contact—from police stops to arrests, jail time, misdemeanor or felony convictions, supervision, or prison—are extraordinarily low (Lerman and Weaver, 2014; White and Nguyen, 2022; McDonough, Enamorado and Mendelberg, 2022). Many were unlikely to vote even before their convictions, due to a range of material and attitudinal barriers (White, 2022; Gerber et al., 2017). And people who have experienced the criminal legal system show

low trust in government, often expressing doubt that their civic voice matters ([Lerman and Weaver, 2014](#); [Weaver, Prowse and Piston, 2019](#)).

Further, contact with the criminal legal system is concentrated in neighborhoods that face a range of other disadvantages ([Soss and Weaver \(2017\)](#) refer to these as “race-class subjugated” communities). Voter turnout in these communities is low, raising questions about whether system-impacted people have *any* social connections to the world of voting ([Burch, 2013](#); [Soss and Weaver, 2017](#); [Lerman, 2013](#)). These studies do not paint a promising picture for the prospect of using social networks to mobilize system-impacted people. We might expect them to be socially isolated, or siloed in homophilous networks characterized by high political disaffection and skepticism about the value of voting.

However, some recent examples point to the potential for organizing among people with criminal records. Formerly incarcerated people have been at the forefront of advocacy efforts to restore voting rights to people with convictions ([Owens, 2014](#); [Morse, 2021](#); [White et al., 2025](#)). And even light-touch informational interventions substantially boost engagement among this group ([Gerber et al., 2015](#); [Harris et al., N.d.](#)). Engagement in their wider communities also varies. While turnout is generally low in heavily-policed areas, scholars observe higher turnout when local candidates campaign on relevant issues like over-policing ([Laniyonu, 2019](#)). And [Walker \(2020\)](#) notes that helping a loved one navigate the system can be politically mobilizing, especially when people view carceral experiences as systemically unjust.

Outside of politics, we know that social ties matter profoundly for people navigating the criminal legal system. Loved ones pay bail and other costs, maintain contact during periods of incarceration, offer a place to stay upon release, and assist with applications for services and jobs to support self sufficiency([Comfort, 2009](#)). Sociologists find that family provides crucial material and emotional support when people re-enter society after incarceration ([Western, 2018](#); [Mowen, Stansfield and Boman IV, 2019](#)). These actions are costly, but widespread among the families of people dealing with the criminal legal system ([Comfort et al., 2016](#)).

We focus on the same sorts of close relationships in this paper, but we ask whether those ties can also help people navigate the political world. We think it is possible that just as people help their loved ones look up information about how to clear a warrant or how to apply for public benefits, they can also help them figure out whether they are eligible to vote and how to register. Family and friends might also exert social pressure, enforcing social norms about the importance of voting, or try to persuade their loved ones that doing so is worthwhile.

In the broader political science literature, the idea that politically active social ties can influence their loved ones to vote is not new. Researchers have long contended that civic engagement is fundamentally social, and that “all turnout is mobilized” (Rolfé, 2012, pg. 99). Evaluations of the social nature of voting have focused on two primary mechanisms by which networks influence turnout: information provision and social pressure (Sinclair, McConnell and Michelson, 2013; Nickerson, 2008). Active voters can share information with their networks, facilitating turnout for eager but uninformed voters, or exert pressure or social sanctions on non-voters. “Put simply,” Sinclair writes, “individuals do not want to disappoint their friends and family, and this is how politics are contagious,” (2013, pg. 7).

However, this literature has largely focused on already-registered voters living in middle-class neighborhoods. In contexts where people have already taken the step of registering to vote, know how to vote, and see themselves as surrounded by people who care about voting, peer pressure can be highly effective. But what about the context we study here, the social lives of system-impacted people, among whom baseline rates of participation are low and political disaffection is high? Can politically active social ties still be effective for this group?

We note several reasons to doubt that existing findings will travel to this population. The first is about the necessary condition of having politically active social ties. Existing research provides reason to think system-impacted people may have very few politically active social ties (Burch, 2013; Soss and Weaver, 2017; Lerman, 2013), but this research has not, to our knowledge, established the extent to which the target population is connected to voters who

can help them navigate the electoral process. Our first empirical task (in the next section), therefore, will be to establish that system-impacted people who are disengaged from politics nevertheless have politically active people in their lives.

The second question is whether these politically-active social ties will be willing to talk with their loved ones about registering and voting. We focus on close relationships between people who are likely in regular contact with one another: family members (biological or otherwise), roommates, close friends, etc. We specifically target such close relationships wherever possible, because we do not expect looser ties (local shopkeepers or service providers) to be as willing to reach out to system-impacted people to encourage voting. Looser ties are not only less likely to have accurate contact information or regular face time, but they are also less likely to feel comfortable broaching the topic. We confirm this intuition in the interviews discussed in Section 4.2. But we proceed with the hypothesis that some close ties are already talking with their system-impacted loved ones about voting, and that many more would do so if prompted. In the field experiments presented below, we provide exactly that encouragement, and we see that many family members respond.

Finally, one might wonder whether social outreach might not work the same way for system-impacted individuals as it does for those without criminal legal contact. People with criminal records might have such deep informational deficits or such skepticism about the value of voting, that the kinds of social pressure or information provision studied in previous work may not suffice for them. However, we think that relationships can matter even in these difficult circumstances. If anyone can shift seemingly intractable attitudes that voting does not matter, social ties are the best situated to do it (Carlson, Abrajano and Bedolla, 2020).

In what follows, we present both observational and experimental evidence that ties to registered voters can increase participation even among people with convictions. We cannot causally identify the precise mechanisms by which they do this, but in section 4.2, we learn about the types of strategies politically-active people use when registering their system-

impacted loved. They include a mix of tactics seen in existing literature (social pressure, providing information about voting and elections) and some that are specific to this group (personalized arguments to persuade people of the value of voting, specific reassurances about legal eligibility). In particular, we note that social ties work hard to persuade people who do not value voting that it is worthwhile, which both extends existing theory and suggests that a relational approach to mobilization could also be effective among other chronic non-voters who are not system-impacted (Sinclair, McConnell and Michelson, 2013; Carlson, Abrajano and Bedolla, 2020; Rolfe, 2012).

3 Observational Data: Sibling Connections to Voting

Our first analysis uses administrative data to examine the presence and importance of social ties to political life among people with felony convictions. Here, we focus on connections with siblings who are registered to vote. Our purpose is two-fold: first, to learn whether people with records have ties to registered voters; and second, to evaluate whether these ties are associated with higher participation, as is true for people without convictions (Sinclair, 2012; Sokhey and Djupe, 2011; Rolfe, 2012). Siblings represent only one kind of social tie, but sibling relationships are illustrative of the kinds of relationships we think could be important for increasing participation among system-impacted people.

We begin with conviction and sentencing records from the state of Texas, which allow us to identify approximately 745,000 people convicted of felonies over the last few decades who now appear to have completed their sentences and are eligible to vote. We then use decades of Texas “birth index” data to link these eligible voters to their Texas-born siblings. The birth index lists the name and date of birth of each baby born in the state (from the 1920’s through the 1990’s) as well as their parents’ names. By assuming that babies born to the same-named pair of parents are siblings, we are able to group Texas-born children into clusters of siblings.¹ We use this mapping of families to identify which of the people

¹This approach is incomplete: it will miss sibling pairs who do not have the same two parent names (or

in our post-felony-conviction sample have siblings born in Texas. We then use the names and birthdays of these siblings to find registered voters in the state’s voter file (snapshot collected June 2024).²

We identify about 200,000 post-conviction eligible voters with at least one sibling born in Texas. Many of these eligible voters have sibling ties to registered voters: of 208,387 people with siblings, 61% have at least one sibling who is registered to vote.³ Given previous findings of low participation both among system-impacted people and their neighbors, such high rates of connectedness to voters may be unexpected, although previous research has not directly estimated the prevalence of political activity in system-impacted networks (Burch, 2013; Lerman and Weaver, 2014; Lee, Porter and Comfort, 2014; White, 2019; Walker, 2020). These estimates, the first of their kind, indicate that despite low participation, many eligible voters with convictions have access to an important civic resource.

Table 1 focuses on people who have at least one Texas-born sibling and estimates the association between having a registered-voter sibling and being registered to vote in December 2024 (columns 1 and 2) and voting in the November 2024 general election (columns 3 and 4). Columns 1 and 3 present bivariate relationships, while columns 2 and 4 include demographic covariates (age in years, sex, and race, constructed using information from the conviction database, plus the total number of siblings in the family). All specifications confirm our expectations: people with a registered sibling are themselves more likely to be registered

names similar enough to be caught by probabilistic matching) listed on their birth certificates (as in the case of single mothers, half siblings, or parent name changes), and pairs where a sibling was born outside of Texas or outside the years covered by our data. It may also introduce some noise by pairing together “siblings” who are not actually related (when both parents have very common first and last names). Such noise should make our estimates of family-tie effects conservative.

²Section A.3 of the SI describes our record linkage process.

³A very conservative alternate approach would be to use 745,000 as our denominator for an estimated rate of 16% with sibling ties to voting, though we note that many people in our broader list could have siblings in other states whose registration status we cannot observe.

(about 6 percentage points) and vote (about 3 and a half percentage points) than people whose siblings are not registered. These estimates represent large differences in participation, given that overall registration is low (40%) and voting in the 2024 general election is below 15%. The estimates are similar in size to the white-Black racial turnout gap in this sample (Fraga, 2018).

We should not over-interpret these associations: people with registered-voter siblings are likely different from those without any such ties in many unobservable ways. But these estimates indicate that many people with previous criminal convictions have family connections to the world of voting, which is a prerequisite for such relationships to affect electoral engagement. We argue that if activated, these relationships could shape participation among a group whose participation scholars and advocates have struggled to understand. We next turn to two field experiments to measure the causal effect of interventions intended to activate those close social relationships.

Table 1: Registration and Voting by Sibling Voter Status

	<i>Dependent variable:</i>			
	registered		voted24	
	(1)	(2)	(3)	(4)
hasVoterSibling	0.059*** (0.002)	0.057*** (0.002)	0.036*** (0.002)	0.036*** (0.002)
Constant	0.398*** (0.002)	0.468*** (0.006)	0.125*** (0.001)	−0.008** (0.004)
Covariates (Age, Sex, Race, Total Sibs)	No	Yes	No	Yes
Observations	208,387	208,387	208,387	208,387
R ²	0.003	0.014	0.002	0.016
Adjusted R ²	0.003	0.013	0.002	0.016
<i>Note:</i>		*p<0.1; **p<0.05; ***p<0.01		

4 A Relational-Organizing Experiment

To measure the effectiveness of social ties in getting system-impacted people involved in politics, we fielded two experiments in 2024. In the first, we worked with a grassroots organization to recruit politically active people to serve as “mobilization captains” who would reach out to their system-impacted loved ones and get them registered. As described below, we worked closely with the organizers to train them and learn about the strategies they used in the field, yielding both qualitative and quantitative evidence about how social ties can help people become engaged. This study’s focus on carefully-selected organizers allowed us to implement an intensive, personalized, realistic design. The approach also has limitations, including sample size and composition, concerns we address with the mailer study presented in Section 5.

4.1 Relational Organizing Experiment Design

In summer 2024, we worked with a partner organization in Texas (Grassroots Leadership, or “GRL”) to recruit 48 volunteers embedded in system-impacted communities to serve as “mobilization captains.” Captains attended trainings on voting eligibility, voter registration, and research methods (details on recruitment and training can be found in SI Sections B.1 and B.3). Trainings focused heavily on voting laws in Texas, to ensure that efforts would be consistent with the state’s policies, and on research methods, to ensure proper treatment implementation. Captains also built lists of people in their lives who had had criminal legal contact and who were unlikely voters. The research team cleaned the lists (described in SI Section B.2) and then randomly assigned the individuals on them to “control” and “treatment” groups with equal probability, blocking on captain. In the few weeks before the state’s October registration deadline, captains reached out to their “treatment” list via any means they liked, and were instructed not to contact individuals on their “control” lists. The research team met individually with captains several times to discuss their progress

and encourage persistence, and held one group meeting to facilitate peer support. After the election, we merged the experimental list with September and December snapshots of the voter file to measure outcomes: voter registration and turnout.⁴

4.2 Learning About Treatment From Captain Interviews

We collected data on the captains’ activities during the experimental period. One PI conducted open-ended interviews with as many captains as possible, asking them to recount how they reached out to each person on their treatment list and how it went (see SI Section B.4 for details). These interviews taught us about both treatment delivery (e.g. whether contact was made with a list-member and how), and the tactics social ties used when mobilizing their loved ones. We were able to learn about what happened in this specific experiment and observe a range of tactics loved ones might use in other experimental or real-world contexts.

4.2.1 Information Provision and Material Support

Captains frequently reported providing information about, or material assistance with, the registration process. Some of the information provision was specific to system-impacted people: many captains reported encountering misperceptions about voting eligibility after a criminal case. One organizer reflected on a conversation she had with her son about his rights, where her intimate knowledge of his status allowed her to correct misinformation he received elsewhere:

[My son said] ‘You know, I can’t vote. I’m a felon.’ and I say, ‘who told you that?’ because see, his girlfriend told him that. I say, ‘who told you that?’... and then I explained to him everything else, like, ‘well, you know, just ‘cause you a felon and you already did your time...’ He wasn’t on parole or anything.

⁴See SI Section C for our pre-analysis plan filed before the outcomes were collected, and SI Section B.5 for a discussion of the merge to the voter file.

So. And he’s not on probation. I said, ‘You’re not on probation. So. There’s nothing stopping you, if you want to vote, all you got to do is go register.

Complicated and geographically varying felony disenfranchisement laws leave many people believing they are ineligible to vote long after they have had their rights restored ([Drucker and Barreras, 2005](#); [McCahon, N.d.](#); [Wood and Bloom, 2008](#)). Social ties can provide education, drawing on their civic literacy to look up and interpret voting policies for their loved ones. They may also have personal knowledge about individuals’ cases that allows them to correct eligibility confusion, as exemplified by the mother of the misinformed son quoted above. And their close relationship can help them provide information as a trusted source ([White et al., 2025](#); [Sugie et al., 2024](#)).

Active voters can also use their civic resources to help loved ones navigate material challenges. One organizer described helping people with limited literacy decipher the registration form. Another reported that he helped someone facing housing instability who lacked a physical address. Still another assisted a friend with transportation issues. Though people with criminal records have unique questions about legal eligibility, the need for information and help around voting is not specific to this group. Many unregistered voters are uncertain about the process or face material barriers to voting and could use support from loved ones to overcome them ([Bennion and Nickerson, 2016](#)).

4.2.2 Social Pressure

Captains also reported using “social pressure”: enforcing norms about voting or implicitly threatening disapproval of those who do not vote. These approaches resemble those depicted in previous work on social pressure, though they targeted a very different group ([Sinclair, 2012](#); [Cormack, 2019](#)). For example, one interviewee referenced voting as a necessary step toward change, and the value of voting to the community:

That’s my brother, the only brother I have. And I talked to him about it because he, he knew that I was doing it because he know about the work that I do in the

community. So he just had this... wild thing about why he didn't want to vote. And I'm like, well, if we don't create change ... we talk about doing positive things and uplifting our community. Your vote does matter. It count for you to be out, you know, to make a difference? So he did his registration.

Other organizers applied pressure through implied social sanctions, saying, for example: "My thing is, well, we've got to get out and vote. You can't complain if you never tried to do anything about it. Use your voice!" Thus, organizers relied on values they thought their loved ones held around the value of voting to pressure them to take an action.

4.2.3 Persuasion

Finally, we saw organizers work to persuade skeptical loved ones of the value of voting. Many system-impacted people express alienation and doubt that their voice matters ([Lerman and Weaver, 2014](#); [Weaver, Prowse and Piston, 2020](#); [Justice and Meares, 2014](#)). But captains drew on personal knowledge of these individuals to craft appeals about the importance of voting. One organizer connected the election to a subject's child's education:

She hates politicians in general. So with her I hit the school board because she has a daughter in school and her and I have previously talked before about how I can't go help my nephew [at] school. Like, it's very frustrating... we have a man in [redacted] County that's running for the state school board. And I showed her his Facebook. And it's because I know what she believes and doesn't believe. And I said this is the kind of thing that he wants in our whole state... Like, it's not just like about the president. And if you honestly didn't even care about that, that's whatever. But there are other things that we've got to be concerned with for our kids, you know?

Another organizer summed up the persuasion strategy succinctly: "Basically you have to find out what the person is interested in to get them to vote and sign up." Unlike the use of

social pressure, where organizers pointed to a norm of voting, this tactic sought to persuade people to view voting as valuable— at least situationally— through personalized arguments linking the act to specific issues.

Organizers also encountered immovable attitudes. In those instances, they often told us they would benefit from more time to engage in conversations about the value of voting. Indeed, this is a long-term strategy: research suggests that people can change their minds about the value of voting through exposure over time to social connections who themselves value voting (Sinclair, 2012). Persuasion emerged as a particularly important mechanism by which social ties attempted to mobilize system-impacted loved ones who did not yet adhere to a norm of voting, often by drawing on deep personal knowledge of the mobilization target.

4.3 Organizing Treatment Effects

After the 2024 election, we collected outcome data on voter registration and turnout for people in our sample. Table 2 presents estimates of the effect of the relational organizing experiment on voter registration among contacts in the sample.⁵ We present three different specifications. Column 1 of the table presents the simplest difference-in-means estimate. It indicates a positive effect of our treatment on voter registration (1.4 percentage points’ increase), but it is statistically indistinguishable from zero.

The next two specifications account for an unexpected feature of the experimental sample. After cleaning the captain-provided lists to correct issues including name misspellings and incorrect birthdates, we found that over two thirds of our sample were already registered.⁶ Given this issue, including a measure of pre-treatment registration greatly increases

⁵Treatment assignment was block-randomized within captain lists, with equal probability of assignment to treatment and control. Consistent with the pre-registration we filed, we present regressions of outcomes onto treatment assignment using inverse propensity weighting (IPW) to account for small variations in probability of treatment from varying captain-list sizes.

⁶For a similar set of problems in a different context, see Cohen and Green (2023). In retrospect, this

the precision of the estimates: Column 2 includes this covariate and reports a statistically significant treatment effect of 3.4 percentage points ($p < .05$).⁷ Finally, Column 3 presents exploratory analysis of the effect of our treatment among the one third of our sample who were not already registered and thus could respond to treatment. We did not pre-register this analysis as we did not expect so much of the sample to be registered already. Here, we see about a 7 percentage point treatment effect on this group ($p < .05$). This represents a substantial increase in registration: registration rates in the control group for this part of the sample were below two percent. SI Figure B.5 presents estimated treatment effects on voter turnout as well as a measure of registration updating; our preferred specification suggests small positive effects on both outcome measures, but neither is distinguishable from zero.

The results from this experiment provide suggestive evidence that a relational approach to voter mobilization can increase engagement among system-impacted people. This high-touch intervention, where we worked with organizers to build a list of their loved ones and experimentally evaluated their efforts to mobilize them, closely approximates how social ties operate in the real world. Organizers routinely engage in this type of mobilization (Green and McClellan, 2020). But there is little social science research on the effectiveness of such approaches, in part because they are difficult to study.

We have indicated some of the challenges we faced implementing the design. Chief among them was building lists of organizers’ contacts. People with intimate knowledge of their friends and family may nevertheless be unwilling to share the official details (full legal pattern makes sense: the close contacts of our volunteer mobilization captains have already been “treated” by their political conversations over the years, as some captains have been doing this mobilization work in their social circles since before our study prompted them to do it in 2024.

⁷These estimates should be interpreted cautiously: the experiment was designed (and powered) with the assumption of lower baseline registration rates and a larger sample. And though we pre-registered the inclusion of pre-treatment covariates for precision, we did not specifically pre-register the inclusion of previous registration as a covariate, so these estimates are best viewed as exploratory.

Table 2: Effects of Relational Outreach on Voter Registration (by Previous Registration Status)

	<i>Dependent variable:</i>		
	Registered (December 2024)		
	(1)	(2)	(3)
Relational Treatment	0.014 (0.038)	0.034*** (0.013)	0.070*** (0.022)
Already Registered		0.933*** (0.014)	0.962*** (0.019)
Relational Treatment * Already Registered			−0.055** (0.027)
Constant	0.660*** (0.027)	0.038*** (0.013)	0.019 (0.016)
Observations	621	621	621
R ²	0.0002	0.885	0.886
Adjusted R ²	−0.001	0.884	0.885
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01		

names, dates of birth) that are needed to find people in government records. The challenge of eliciting captains’ contacts and linking them to state criminal records contributed to a smaller than anticipated sample.⁸ We planned for a sample of 1,000 people (50 captains with lists of 20 people each), and we were ultimately able to recruit 48 captains and build lists containing a total of 621 people. The small sample size compounded power issues stemming from a much higher-than-anticipated rate of prior registration.

We also faced compliance issues: some enthusiastic captains contacted people who were not on their “treatment” list, sometimes including individuals in the control group. Communication in group settings also led to list contamination. Still another captain reported that while she did not speak with an individual in her control group during the treatment implementation, she had encouraged that person to register just prior to the start of the study. Such difficulties come with the realism of an experiment implemented as part of people’s everyday lives, and bias us against finding experimental effects.

Despite these challenges, our results suggest exciting promise for relational approaches to bringing unlikely voters into the electorate. Our volunteers were willing and able to find people in their lives with previous criminal-legal-system contact and help them register. To address limitations of the relational organizing design, including lack of researcher control over treatment administration and limits to generalizability, we turn to a concurrent experiment that allowed us to more precisely control both the treatment and the sample.

⁸When a captain reported that someone on their list had some direct personal contact with the legal system (as opposed to proximal contact through a family member), we then attempted to find the associated cases to confirm their voting eligibility. Given the legal risks to people who vote while ineligible in Texas, we erred on the side of excluding people with unclear eligibility from the sample.

5 A Mail-Based Experiment

Our second field experiment used mailers to reach a much larger sample of Texans with previous felony cases (and their loved ones). We built a list of over half a million eligible post-felony voters, and randomly assigned a mailer treatment in which some people were directly sent information about registration and voting, and in other cases we sent the same information to an already-registered voter connected to the person in our sample. This approach lets us learn about the potential for social ties—even those who are not already political activists—to get their loved ones registered.

Experimental Sample Prior to the 2024 general election we used the Texas conviction database (requested from the state Department of Public Safety) to identify a large number of Texans who appeared eligible to vote after a felony. Section A.1 of the SI describes this process in detail. After dropping people with unclear voting eligibility, those who appeared to have died or moved out of state, and those for whom we saw existing voter registration records,⁹ we were left with a list of 596,120 post-felony eligible registrants. Not all had felony convictions resulting from these charges: in some cases, people had pled down to misdemeanors, and others served sentences that ultimately allowed them to avoid having a conviction on their record via Texas’ “deferred adjudication” process. We think this inclusive approach allows us to learn about the behavior of a broad set of people impacted by felony cases; in Section A.7 of the SI we demonstrate that restricting the analysis to people with felony convictions yields similar results to those presented here.

⁹The experiment was focused on registering people to vote, so we sought to exclude already-registered people. As Table SI1 in the SI shows, our initial voter file merge did not catch all registrations in the sample, as the primary name listed in the conviction database did not always correspond to a person’s legal name that they would use to register and vote. Using aliases associated with the records allowed us to see that about 16% of our ultimate study sample had already been registered to vote when we went into the field; we thus include pre-treatment registration as a covariate in many of our analyses to improve precision.

This list of 596,120 people represented the “focal individuals” targeted in our study. We tested the efficacy of enlisting the help of an already-registered social tie to register the focal individuals. We tested this indirect approach alongside a mailer sent directly to the focal individual to benchmark our effects. To implement these treatments, we needed to figure out how to send mail either to these focal individuals or to voters with close relationships to them. To find the mailing addresses of the focal individuals, we contracted with a commercial address vendor, which was able to match 79% of our list to a current mailing address in Texas.

To identify target individuals’ social ties to voters, we went through a multi-step process, looking first for close familial relationships and, when we could not find those, expanding our search to include looser ties. Figure 1 shows the various types of ties included in our sample and the share of the sample linked to them; Section A.2 in the SI provides more detail on each step of the process of identifying social ties. Starting at the top of the diagram, we see that for 12% of the sample list, we were able to use public records to find a close family member (spouse, sibling, or someone who shares the person’s last name) who was registered to vote at the same address we had on file for the focal individual. These are exactly the sort of closely connected social ties we expect to be effective at helping people register and vote, so we rely on these when they are available. For people without a close in-home family member, we look for any registered-voter family ties who are not co-resident with the focal individual; this kind of tie is the best we can do for about 11% of people on our list. Next, we turn to ties without a clear family link: for 20% of our sample, we can see that someone who lives at the same address as our focal individual is registered to vote, and for 18% of our sample the closest tie we find is a neighbor (next-door or same-block) who is registered. Finally, for 38% of the sample, we cannot identify any close social or geographic ties to registered voters. This does not mean that this group has no ties to voters, simply that we are not able to identify any via available public and commercial records. About half of the individuals in this group have no mailing address in the commercial data we use, so we have no address data with which to locate registered voters who might live nearby.

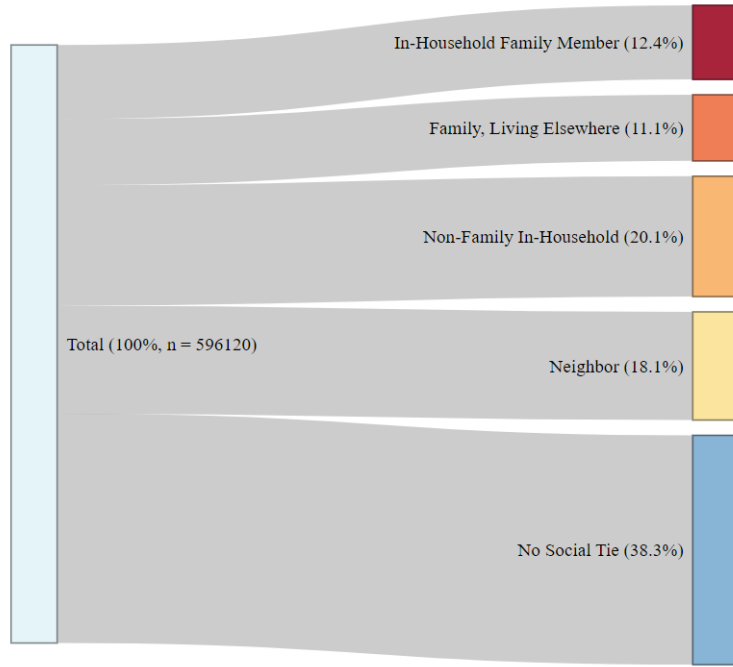


Figure 1: Linking the mailer-experiment study sample to registered voters via social ties

After identifying as many mailing addresses and social ties as possible, we randomly assigned focal individuals in the study sample to treatment conditions. Note that we did not restrict the sample to those with a mailing address or with available social ties, so all analyses presented will be of “intent-to-treat” effects: we assign people to an intended treatment condition, and then analyze the experiment regardless of whether people actually receive the treatment. Such estimates can teach us about how effective real-world interventions might be among the full population of interest.

The design included four experimental conditions: control, two “individual mailer” conditions, and a “social-tie mailer” condition. People assigned to the control group were not contacted at all. The two “individual mailer” treatments consisted of a letter from Grassroots Leadership encouraging the recipient to register and vote, information about eligibility and the registration process, and a paper registration form and return envelope. Both “individual mailer” treatments were targeted directly to the focal individual in our sample: if

we had a mailing address on file for the individual, we sent the mailer packet directly to them. The two individual-mailer treatments differed only in the wording and attribution of the opening paragraph of the letter; the main individual mailer letter (shown on the left side of Figure 2) was signed by the leadership of our partner organization, while an alternative “credible-messenger” version of the letter (shown in SI Section SI1) was written from the perspective of and signed by a formerly incarcerated GRL staff member. For parsimony, we combine the two individual treatments here (see SI Section A.7 for separate analyses).

Finally, the “social-ties” condition tests the effectiveness of outreach sent to registered voters connected to people in the sample. This treatment arm consisted of the same information about registration and voting (and the same registration form/envelope) as in the individual mailer arms above, but the enclosed letter (shown on the right side of Figure 2) was addressed to the already-registered voter, encouraging them to help the system-impacted person in their life register. When a focal individual in our sample was assigned to this condition, we sent the letter to the closest registered-voter tie we could find for them (as discussed above and shown in Figure 1).

We built the sample list in summer 2024. We then randomly assigned the sample to the four treatment arms using the R package `randomizR`, with 59% assigned to control, 5.5% to each of the individual-mailer arms, and 30% to the social-ties arm.¹⁰ In September 2024, treatment letters were printed and mailed in time for recipients to register before Texas’ October voter registration deadline for the November general election. After the election, we collected the state’s voter registration and history files and merged them to the experimental list using names and birthdays.¹¹

¹⁰We constrained the randomization such that if two focal individuals in the sample lived at the same mailing address, they would be assigned to the same treatment condition. As such, we had 565,012 household clusters to assign to treatment; when analyzing the experiment, we cluster standard errors by household to account for this feature of the design.

¹¹See section A.3 of the SI for a full discussion of the voter-file merge and validation thereof.



helping people gain power

Do you or someone you know have a criminal record? You may still have the right to vote. Know your rights! The 2024 Election is coming up and you have the chance to make your voice heard. Citizens are eligible to vote as soon as they have completed the terms of their felony conviction, including any period of probation or parole. If you are off paper, you may be able to register and vote.

Annette Price, Executive Director
Dianna Williams, Deputy Director
Grassroots Leadership

WHO IS ELIGIBLE TO REGISTER AND VOTE IN TEXAS? YOU MUST BE:

- ☐ At least 18 years old by Election Day
- ☐ A US Citizen
- ☐ A resident of the Texas county where you apply for registration
- ☐ Not currently incarcerated or on probation or parole for a felony conviction
- ☐ Not determined by a court to be ineligible to vote due to mental incapacitation

USE THE STEPS 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 blue) and provide EITHER your driver's license number OR the last four digits of your social security number.
2. If you don't get mail where you live, enter your valid mailing address in Section 5.
3. Make sure to sign and date the form and include a phone number.
4. Mail back this form or deliver it to your county Elections Office by October 7, 2024.

You can't register to vote online in Texas. Visit grassrootsleadership.org/vote to check your status or request another free application in the mail. Visit your county's elections website for more information about voting.

STEP 2: AFTER REGISTERING, CHOOSE THE BEST VOTING OPTION FOR YOU & VOTE. Find your polling place, information on early voting and vote-by-mail options at votetexas.gov.

See how you can get involved even if you are not a voter! Scan this QR code to learn more about civic engagement with Grassroots Leadership



Grassroots Leadership | 7910 Cameron Rd Austin, TX 78754 | grassrootsleadership.org | 512-499-8111



helping people gain power

As an active voter, you do so much for your community! There may be people in your community who are eligible to vote but are not registered. For example, we think {NAME INSERT} may not be registered to vote. If you know them, can you share the information below and help them make their voice heard in the 2024 election? **Help them know their rights!**

Annette Price, Executive Director
Dianna Williams, Deputy Director
Grassroots Leadership

WHO IS ELIGIBLE TO REGISTER AND VOTE IN TEXAS? YOU MUST BE:

- ☐ At least 18 years old by Election Day
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See how you can get involved even if you are not a voter! Scan this QR code to learn more about civic engagement with Grassroots Leadership



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Figure 2: Example letters from mailer field experiment (additional individual-targeted version included in SI Section SI1)

5.1 Mailer Experiment Main Findings

We begin by examining the overall effects of the treatments on registration and voting in Figure 3. The figure reports three outcome measures. “Registered” indicates that the person was found on the voter rolls in December 2024, “Voted 2024 General” indicates they voted in the November 2024 election, and “Updated or New Registration” is a measure of whether the individual newly appeared on the voter file between September and December 2024, or changed some information on their record during that period.¹² Figure 3 presents estimates comparing both our treatment conditions to the uncontacted control group, including pre-treatment registration status as a covariate for precision. SI Section A.6 shows the underlying regressions, both with and without pre-treatment covariates, as well as a version separately analyzing individual treatment letters.

Figure 3 demonstrates that both our individual and social-tie-targeted outreach efforts increased voter registration. Both treatments have significant effects ($p < .05$) on the voter registration outcome (bottom row of the figure) and the “new-or-updated registration” outcome (middle row of figure). They also appear to have increased voting in the 2024 general election, though these estimates are noisier and the effect of the social ties treatment on turnout is not statistically distinguishable from zero.

Our core hypothesis is that close social ties (family members and other loved ones) can help people navigate the process of registering and voting, especially when nudged to do so. Thus, we are especially interested in the effects of the “social-ties” treatment. Recall that this was an “intent-to-treat” design with substantial non-compliance: over one-third of the sample did not have a clearly connected social tie for us to contact, so we were only

¹² “Registered” and “Voted 2024” were the main outcome measures described in our pre-analysis plan (see SI for the full PAP and a (currently-redacted) link to the online preregistration), while the “Updated or New Registration” outcome is exploratory and was included to capture the behavior of people in the sample who were already registered but might have responded to treatment by updating their registration (see Footnote 8 above).

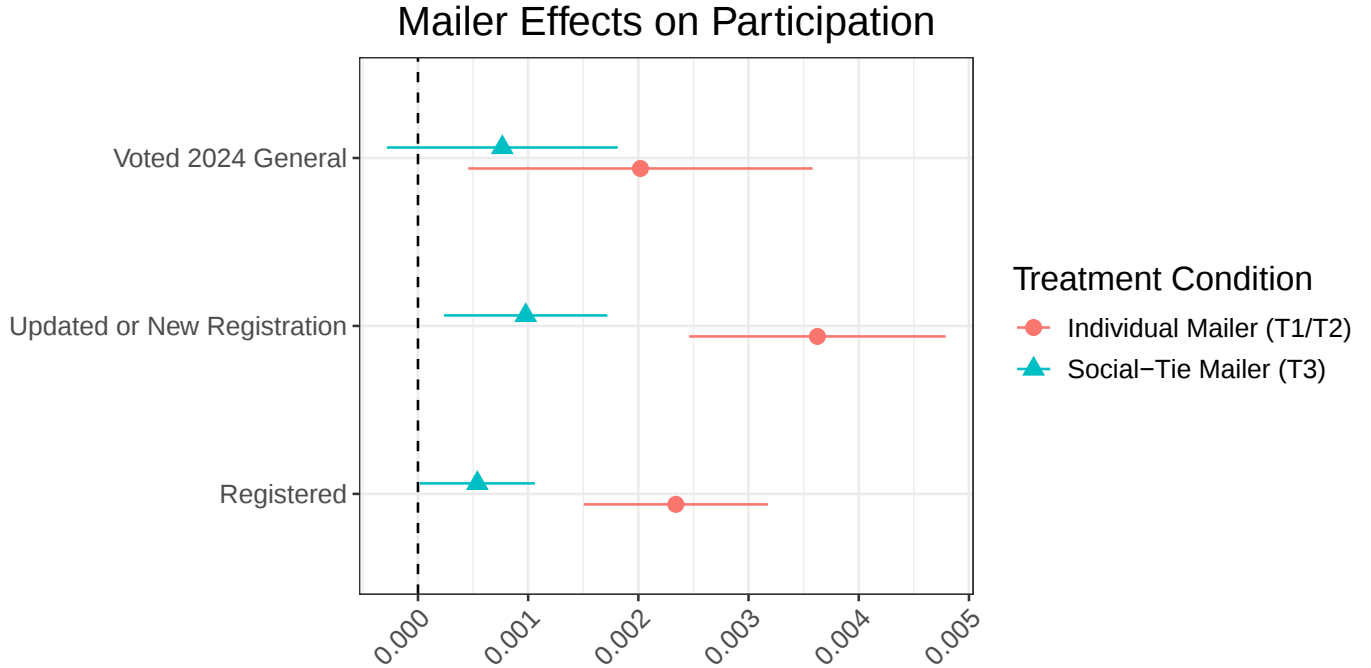


Figure 3: Comparing all treatment conditions to uncontacted control (x-axis is 0-1, so .0025 is one-quarter of a percentage point)

able to send mailers to approximately two-thirds of the people assigned to the “social-ties” condition. Even when we did observe a social tie, many were only loosely connected to the focal individual. There was also no guarantee that contacted social ties would open their mail or help focal individuals register. After all, our sample was constructed to include mainly unregistered people, meaning no one in their lives had successfully encouraged them to register yet. This experiment provided a relatively hard test of the idea that loved ones can help people with previous criminal cases register and vote.

Given these sources of non-compliance, we find the effects shown in Figure 3 notable. The social-ties mailer increased total registration rates by close to one-tenth of one percent (bottom row of Figure 3), and increased new or updated registrations by about one-tenth of one percent (middle row of Figure 3). Given low baseline rates of overall registration and registration updating, these shifts are noticeable. In the control group, just 1.7% of people took some registration-related action between September and December, so an increase of

one-tenth of a percentage point represents a 6% increase in this behavior, from a single attempt to send mail to someone in their life.

Mail is a useful medium for causal inference, but not the best for relational mobilization. These estimates do not represent the possible size of social-tie effects in all settings. Rather, they are a clean test of the idea that even ordinary voters (not just organizers like those in the “captains” study above) can mobilize their loved ones to register. The effects of our direct, individual mailers (about one-quarter of a percentage point increase in registration) demonstrate that we should not expect large, GOTV-style treatment effects in this setting. In our intent-to-treat design, not everyone was sent a mailer—especially in the social-ties arm—and many more did not receive or open them.¹³ Texas is also a difficult place to register, requiring registrants to submit paper forms weeks before election day. We view these mail-based estimates as a proof of concept. Although mailers may work in some contexts,¹⁴ we expect that most real-world interventions building on them will use hands-on, personal approaches similar to those described in Section 4.

5.2 Mailer Experiment: Exploring the Effectiveness of Ties

Next, we ask whether the effects of our “social-tie” mailers are concentrated, as our theory would predict, among focal individuals for whom we were able to find a close social tie. In Figure 4, we explore the effectiveness of our treatments among subsets of the sample defined by the type of social tie to whom we sent a mailer. Here, the outcome is new or updated registration. The “full sample” estimates are the same as those in the middle row of Figure 3 and are included for reference. Each circular point shows the effects of the “social-ties”

¹³A follow-up test described in SI Section A.10 indicated that many of our mailers likely went unopened.

Under fairly strict assumptions, we can rescale our estimates to produce a treatment-on-treated estimate of about three-quarters of a percentage point increase in registration for our social-ties arm.

¹⁴For more evidence on the relative costs of mail sent to individuals versus their family members, see SI section A8.

mailer (compared to uncontacted control)¹⁵ for that subset of the sample: people with no voter ties, those for whom we found an apparently unrelated voter living at the same address or nearby, those for whom we found a non-co-resident family member (mainly siblings), and those for whom we found a close family member registered at the same address.

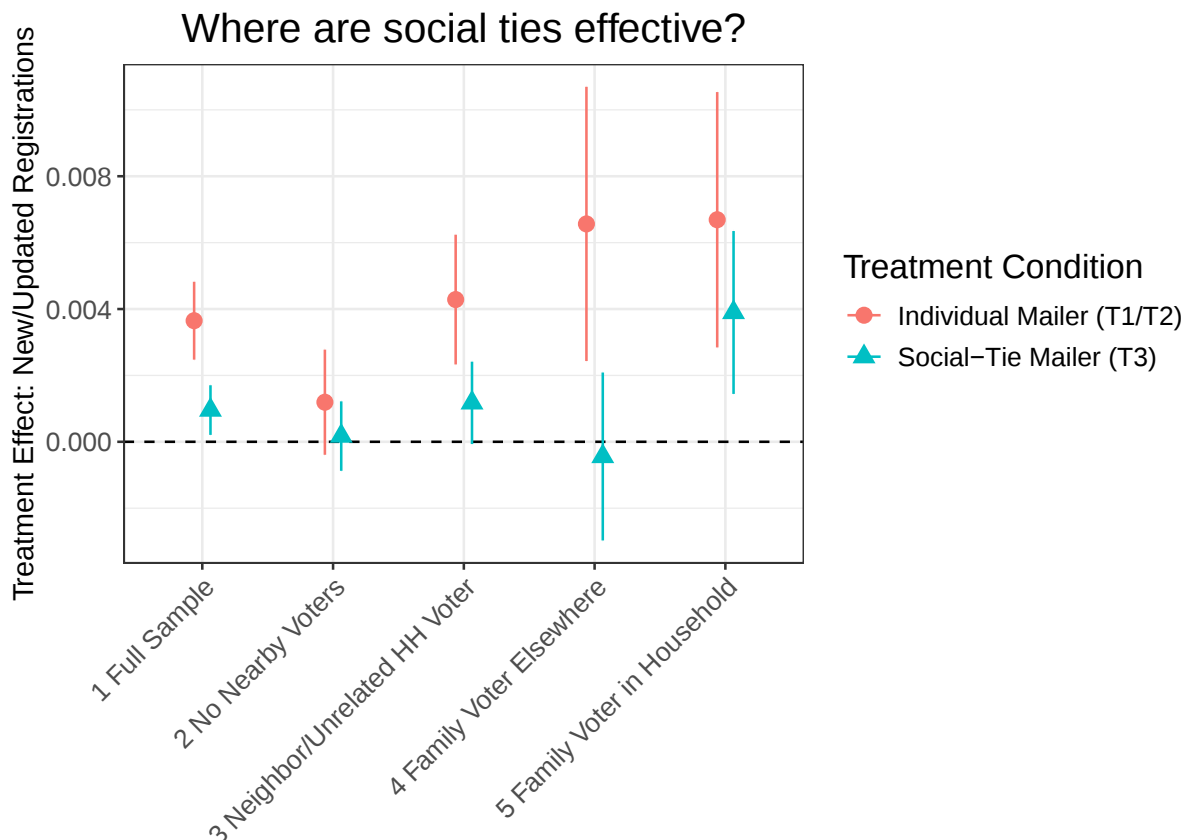


Figure 4: Treatment effects by types of available social connection

Figure 4 shows that the effects of our social-ties mailer on registration are largely concentrated where we would expect. There are no effects among people for whom we could not find any social ties to voters. There are small and marginally-significant registration effects

¹⁵We fully subset the sample for this analysis, based on our pre-treatment assessment of the availability of social ties. For example, when focusing on people with no social ties to voters, we are comparing people in the control group with no identified social ties to voters to people in the social-ties-mailer group with no identified social ties to voters.

among people for whom we reached out to a geographically close but unrelated voter. The social-ties mailer did not affect registration when it was directed to a family member who did not live with the focal individual (group 4 on the plot). When we sent the mailer to a co-resident family member (group 5 on the plot) however, it had a clear, positive effect on registration. The point estimate of four-tenths of a percentage point is indistinguishable from the effect of the direct mailer, and it is much larger than the effect on the full sample. We also find a positive, significant effect on turnout among people for whom we contacted a close household tie (see Figure SI2).

We note that these subsets of the overall sample are not determined at random: people who live with close family may differ from those with no social connections to voters in a variety of ways that could make them more or less responsive to outreach efforts in general. But the individual-mailer conditions give us useful insight into the question of whether people with close in-home family ties are uniquely responsive to voting outreach. Looking at the two groups on the far right side of Figure 4, we can see that those for whom we found a non-co-resident family member (group 4) and those for whom we found a co-resident family member (group 5) are almost equally responsive to our individual mailer treatments. When we sent simple informational mailers directly to them at their addresses, people in these subsamples responded in exactly the same way. But when we instead sought to work through their social connections, our social-tie mailers were much more effective when targeted to family members who have close contact with the person in question.

As we discuss in Section 2, people may find it more convenient and comfortable to discuss registration and voting with close ties that they see regularly.¹⁶ Seeing smaller effects of our social-ties mailer in other subsamples does not mean that the social ties who received our

¹⁶Indeed, we saw this pattern in our interviews with mobilization captains: captains who included good friends and family members on their lists were more persistent than those whose lists included acquaintances or social media contacts. These captains made more attempts to contact subjects and were more likely to make phone calls or arrange in-person meetings.

letters were completely unwilling to help people in their lives register to vote. Instead, we think many of them may have responded to our letter by registering other, closer connections rather than the “focal individual” our study had in mind. Section A.8 presents exploratory analyses of an intermediate experimental outcome: whether the pre-addressed envelopes included in our mailers were actually used to return a registration form, even for someone other than our focal individual. We see similar rates of envelope usage across the individual and social-ties mailer conditions, overall and for nearly all subsamples, suggesting that our social-ties mailers may have yielded many more voter registrations than our experimental design allows us to observe.

This finding—that our social-ties condition saw roughly the same rate of return-envelope usage as our individual-mailer conditions—has implications for the cost of voter outreach approaches that rely on social ties. Targeting already-registered voters who might know unregistered people is logistically simpler than trying to find unregistered people directly, and thus it could be a powerful way of expanding voter registration. If our social-ties mailers, despite being sent to fewer people (because we did not send mailers in cases where we could not find a relevant tie), generated the same rates of envelope returns and potentially new voter registrations, then such social-tie-focused outreach could be a cost-effective approach to registering new voters.

Taken together, the findings derived from this well-powered, light-touch, mail-based intervention bolster those from the more direct, real-world relational organizing version described above: even social ties who are not active organizers can be prompted to help their system-impacted loved ones register. The results in this section come from a statewide sample in Texas and, therefore, generalize beyond the urban settings from which we sampled for the relational organizing experiment. Politically active social ties are a valuable civic resource for system-impacted voters, who are otherwise unlikely to participate.

6 Conclusion

We have demonstrated that close social ties can help people re-enter political life after contact with the criminal legal system. Our qualitative interviews describe the range of actions loved ones can take, from providing information and support navigating the voting process to exerting social pressure to making personalized persuasive arguments about the importance of voting. These patterns mirror sociological findings on the importance of family ties in re-entry after incarceration, but here they represent a path back into political life. Section 3 suggests that some family members may already be doing this sort of “civic re-entry” work, as people with active-voter siblings are more likely to register and vote after a conviction than those without such family ties to voting.

Many voters are likely (and understandably) more focused on helping their loved ones navigate material challenges like finding housing or work, or dealing with court cases or sentencing, than on helping them register and vote. But we believe they can also effectively take on civic re-entry work if they are prompted to do so. Section 5 demonstrates that even a light-touch mailer directed at the social ties of people with felony cases translates into an appreciable increase in their voter registration. And though mail provides a low-cost medium for testing our theory, it is by no means the only way to implement this sort of intervention. Section 4 reports findings from a smaller study of the sort of intensive “relational organizing” approach that we believe holds promise for finding and activating otherwise-hard-to-reach voters. Though we faced implementation challenges and statistical power limitations, evidence suggests that this approach can bring difficult-to-reach voters onto the rolls. And getting people newly registered is a first step towards long-term engagement in voting, rendering them visible to future mobilization efforts that take place around elections.

Our findings point to a new path into political life for system-impacted people, and they broaden our understanding of the importance of social connections for political behavior, pointing to a wide array of potential interventions to expand the electorate. Scholars

note that loved ones play an important political mobilization role, but we demonstrate that they can also be a powerful force for activating very unlikely or disconnected voters. As such, campaigns and organizations pursuing relational approaches to getting out the vote (McKenna and Han, 2014; Schein et al., 2021) might consider broadening those efforts to registration, especially in low-participation areas. Registered voters there have connections to otherwise-hard-to-find people and can help bring them into political life.

We have demonstrated this process for a particularly disengaged and hard-to-contact group, but we think much of what we show here may apply to other low-propensity groups as well. Unregistered people across the country share some of the same characteristics—limited information about voting processes, limited formal education and civic skills, skepticism about elections—as system-impacted people. Though we do not expect eligibility information to play as large a role for other groups, social pressure, persuasion and material assistance should be useful to other chronic nonvoters.

Future research could build on these studies in a number of useful ways. A relational-organizing approach could be tested without constraining the sample to system-impacted people, which would allow for easier implementation and larger samples, demonstrating the approach’s broad applicability (Jackman and Spahn, 2021). Cluster-randomized designs that assigned captains, rather than contacts, to treatment or control could avoid the contamination we faced and allow captains to fully engage their social networks. And work on how best to choose nodes in a network for interventions could allow for more efficient approaches to volunteer recruitment (Banerjee et al., 2013; Akbarpour, Malladi and Saberi, 2020). Finally, there is room for more description of the family and friends who do this kind of political work: who is willing and able to promote civic re-entry, and how can they best be enlisted to do it?

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A Additional Detail on Mailer Experiment

A.1 Identifying eligible post-felony voters

To build the sample for our mailer experiment, we turned to a series of administrative datasets. We started with the Texas Computerized Criminal History (CCH) database, provided by the state’s Department of Public Safety. However, the CCH does not directly indicate that an individual that has been convicted of a felony and that they are currently ‘off-paper;’ that is, that the individual has completed their sentence, probation, or parole. We used a conservative approach to determining voting eligibility, as we did not want to expose anyone to legal consequences for voting while ineligible. Briefly, we kept individuals from the CCH that had at least one felony offense that was not marked as having been reduced to a misdemeanor where that felony offense had a corresponding court record that did not result in a non-conviction (such as dismissal). For all court records for an individual that met those two conditions, we further checked to see if the records were either marked closed or any associated probation period had ended before the experiment. If the individual had any open court records, we then checked the custody records. Based on those records, we removed anyone from our sample who had any ongoing custody stints.

Using names and dates of birth, we matched the gathered set of individuals (N=1,058,524) to a late May 2024 snapshot of the Texas voter registration file and to a 2013 copy of the Social Security Death Master Files.¹⁷ We filtered any matches to either file, and thus removed individuals who appear to already be registered voters or deceased. Additionally, we removed any individuals older than 75. For each person remaining in our list (N=741,845), we provided name, date of birth, sex, and race to a commercial data vendor, which in turn attempted to provide us an up-to-date address. We then removed any individual that had an address outside of Texas (since we knew they lived out-of-state and thus were not eligible to vote in Texas), but kept anyone not matched to an address. At this point, we had constructed our input list of 596,120 individuals for randomization in the mailer experiment.

A.2 Identifying social ties

For everyone in our mailer experiment list, we searched for identifiable social ties who were registered voters in Texas as of May 2024, using a series of additional data sources. This section briefly describes that search.

To identify spouses of focal individuals on our list, at least for those who were married within Texas, we acquired marriage records from the Texas Department of State Health Services covering 1966 to 2016.¹⁸ Using name and DOB, we matched our target individuals to the marriage records. If there were multiple matches, the most recent marriage was prioritized. We then used the corresponding spouse’s name and DOB to match to the Texas voter registration file. To identify the parents and siblings of individuals on our list, we gathered the Texas birth indexes from the years 1926 to 1997. After matching Texas-born target individuals as children in the birth records, we used the names of their parents and the address provided by the data vendor to find any shared-household parents who are active

¹⁷2013 is the most recent vintage available, due to changes in SSA data access policies.

¹⁸More recent records exist, but no longer include DOB for each spouse.

registered voters. To find siblings, we used probabilistic fuzzy matching across birth records using the names of parents in order to group records into families.¹⁹ For matched target individuals, we then searched within these family groupings to locate active voter siblings, preferring siblings closer in age to the target individual and removing any over 25 years older/younger than the target.

We also searched for geographically proximate active voters. To find shared-household voters, we used the vendor-provided address and matched all voters at the same address. For the purposes of finding shared-household voters, we discard any addresses with more than 5 registered voters, or with more than 2 of our target individuals. From our inspection, many of the locations where one or both are true are homeless shelters, hospitals, half-way houses, and so on; these are places our target individuals pass through but do not seem like strong candidates for finding their social ties. Within this set of shared-household voters, we further searched for any spouses or siblings we have previously identified, as well as any voters that share the target individual’s surname. To find next-door neighbors who are active voters, we used the address and the Texas voter registration file. For the addresses of target individuals without unit information (e.g., an apartment identifier), we offset the street address number by up to 2 when searching (to account for typical even/odd street address numbering); for addresses with unit information, we offset the unit marker by up to 1. In the case of multiple active voter neighbors, we kept the closest one. To find active voters within 50 meters of the target individual’s address, we mapped each of the addresses for our target individuals and each of the addresses for registered voters to geographical coordinates. We then used GIS software to search within ZIP code for matches within 50 meters. If there were more than 10 voters within 50 meters, we dropped this as a potential social tie.

Because individuals could have multiple social ties, we prioritized them in the following descending order: 1) spouse within household, 2) sibling within household, 3) same-surname individual within household, 4) parent outside the household, 5) sibling outside the household, 6) different-surname individual within household, 7) next-door neighbor, and 8) neighbor within 50 meters. See Figure 1 for a visual depiction of each broad category.

After identifying the best social tie for each of our focal individuals, we found that some people (1.6% of the individuals on our list) shared social ties with someone else: the same registered voter had been identified as a social tie for multiple people in our sample. To avoid confusion from these voters receiving multiple letters or falling into multiple treatment conditions, we randomly took any duplicate-tie individuals and searched for a looser social tie based on our ‘hierarchy’ of social ties. We performed this procedure twice, reducing the remaining duplicates to 1.0%. After randomization, we removed all remaining duplicate social ties within the social tie arm, setting 3,116 social ties in the social tie treatment arm of 169,504 to missing. This prevented any registered voter that we marked as a social tie from receiving a letter for more than one individual.

¹⁹This process of using parent names to group children’s birth records into family clusters also forms the basis of our identification of sibling pairs for the observational analysis described in Section 3, though there we do not restrict to registered-voter siblings or unregistered focal individuals.

A.3 Record Linkage (Voting Outcomes)

To measure voter outcomes, we acquired two voter registration snapshots from the Texas Secretary of State: one from mid- September 2024, and the other from early December 2024. We then performed two sets of matches between our list of target individuals and the voter registration snapshots, which we describe here.

For our list of individuals (see A.1), we gathered up to five names and two dates of birth from the Texas Computerized Criminal History (CCH). These include the primary, ‘base’ entry for each of these pieces of identifying information, as well as alternatives (e.g. aliases). For each combination of these in the CCH, we merged to the voter registration file using the name and birthdate for each voter record. We then matched iteratively between the files using different combinations, prioritizing higher quality matches as well as prioritizing those using the base entry in the CCH. For each of these matches, we used an exact match between the CCH and voter registration on DOB. We then used combinations of first name, middle initial or name, and last name, as well as ‘close-enough’ matches to allow differences in spellings. We also further switch in former last names and middle names for individuals marked female in some matches to account for our expectations (relative to men) of name changes for women following marriage and other life events. For validation, we also reran the match using permuted birthdate information on one side of the match by adding 35 days to CCH-derived DOB (Meredith and Morse, 2015). We found 2,652 matches in the voter registration snapshot dated early December 2024 using the permuted DOB relative to the 99,738 matches using the original DOB. This suggests a false-positive rate of 2.66%.

We also acquired a voter history file detailing participation in the 2024 general election. Using the unique voter ID, we linked the set of matched voters from the early December 2024 voter registration file to the voter history file. Lastly, we create outcomes based on differences between the match records in the pre-experimental voter registration file dated to September and the post-experimental snapshot dated to December. Based on differences in the registration status, name, or address, we consider a voter to have updated their registration. If there was no corresponding registration in the September file for a registered voter post-election, we consider them newly registered.

A.4 Other Individual Treatment Letter

Figure 2 presents two of the three letter versions from the mail-based RCT. Figure SI1 shows the final letter version, the “credible messenger” letter that was sent to focal individuals in treatment condition 2.

A.5 Balance and summary statistics

A.6 Regression tables from paper figures

Tables SI2 and SI3 present the underlying regression tables for the figures in the main paper. Figure ?? follows the same structure as Figure 3 in the main paper but shows voter turnout as an outcome rather than registration.



helping people gain power

When I went to prison, I felt like I was cast out of society. Since I got out, I've been educating, advocating and organizing to improve my community. But unfortunately, I cannot vote. You see, I am on parole until 2050. I'll be 81 years of age before I can cast a ballot. If you've finished your sentence, you CAN vote. Where my political voice has been silenced, you have a chance to make yours heard. **Can you join our community in voting this fall?**

*Dylan Martin, Community Organizer
Grassroots Leadership*

WHO IS ELIGIBLE TO REGISTER AND VOTE IN TEXAS? YOU MUST BE:

- ☐ At least 18 years old by Election Day
- ☐ A US Citizen
- ☐ A resident of the Texas county where you apply for registration
- ☐ Not currently incarcerated, or on probation, or parole for a felony conviction
- ☐ Not determined by a court to be ineligible to vote due to mental incapacitation

USE THE STEPS 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 blue) and provide EITHER your driver's license number OR the last four digits of your social security number.
2. If you don't get mail where you live, enter your valid mailing address in Section 5.
3. Make sure to sign and date the form and include a phone number.
4. Mail back this form or deliver it to your county Elections Office by October 7, 2024.

You can't register to vote online in Texas. Visit grassrootsleadership.org/vote to check your status or request another free application in the mail. Visit your county's elections website for more information about voting.

STEP 2: AFTER REGISTERING, CHOOSE THE BEST VOTING OPTION FOR YOU & VOTE. Find your polling place, information on early voting and vote-by-mail options at votetexas.gov.

See how you can get involved even if you are not a voter! Scan this QR code to learn more about civic engagement with Grassroots Leadership



Grassroots Leadership | 7910 Cameron Rd Austin, TX 78754 | grassrootsleadership.org | 512-499-8111

Figure SI1: Treatment Arm 2: "credible messenger" letter version

	Control	T2 (Ind. 1)	T3 (Ind. 2)	T4 (Social)	Joint F-test p-val
Black	0.221	0.002 (0.002)	-0.002 (0.002)	-0.002 (0.001)	0.293
White	0.3	0.003 (0.003)	0.003 (0.003)	0 (0.001)	0.336
Hispanic	0.472	-0.005 (0.003)	-0.001 (0.003)	0.002 (0.001)	0.11
Male	0.757	-0.001 (0.002)	0.002 (0.002)	0.002 (0.001)	0.393
Has Social Tie	0.622	-0.002 (0.003)	-0.002 (0.003)	-0.001 (0.001)	0.716
Has Ind. Address	0.787	-0.004 (0.002)	-0.002 (0.002)	-0.001 (0.001)	0.348
Already Registered	0.159	0.002 (0.002)	-0.001 (0.002)	0 (0.001)	0.632
Has Felony Conviction	0.616	0.002 (0.003)	0.007 (0.003)	0 (0.001)	0.088

Table SI1: Summary Statistics and Balance Testing, Mailer Experiment

Where are social ties effective? (DV: Voter Turnout)

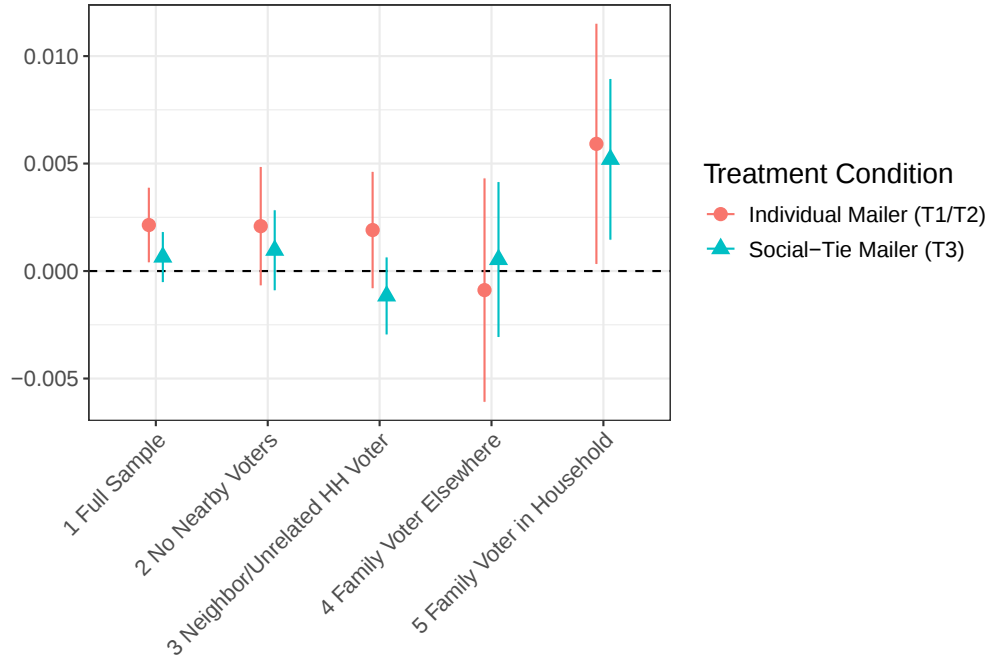


Figure SI2: Treatment effects by types of available social connection

Table SI2: Mailer Effects on Registration/Voting

	Registered	Registered	New/Re-Reg	New/Re-Reg	Voted	Voted
(Intercept)	0.167*** (0.001)	0.010*** (0.000)	0.017*** (0.000)	0.009*** (0.000)	0.043*** (0.000)	0.004*** (0.000)
T1/T2: Ind. Mail	0.003+ (0.002)	0.002*** (0.000)	0.004*** (0.001)	0.004*** (0.001)	0.002* (0.001)	0.002* (0.001)
T3: Social-tie Mail	0.000 (0.001)	0.001* (0.000)	0.001* (0.000)	0.001** (0.000)	0.001 (0.001)	0.001 (0.001)
Already Registered		0.990*** (0.000)		0.047*** (0.001)		0.244*** (0.001)
Num.Obs.	596 120	596 120	596 120	596 120	596 120	596 120
R2	0.000	0.939	0.000	0.017	0.000	0.191
R2 Adj.	0.000	0.939	0.000	0.017	0.000	0.191
AIC	516 772.5	-1 153 694.1	-727 631.0	-737 817.6	-203 896.3	-330 040.2
BIC	516 817.7	-1 153 637.6	-727 585.8	-737 761.1	-203 851.1	-329 983.7
RMSE	0.37	0.09	0.13	0.13	0.20	0.18
Std.Errors	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table SI3: Mailer Effects on New/Updated Registrations, by Type of Tie

	All	No Tie	NonFamily	Family(non-HH)	Family(HH)
(Intercept)	0.017*** (0.000)	0.013*** (0.000)	0.018*** (0.000)	0.023*** (0.001)	0.021*** (0.001)
T1/T2: Ind. Mail	0.004*** (0.001)	0.001 (0.001)	0.004*** (0.001)	0.007** (0.002)	0.007*** (0.002)
T3: Social-tie Mail	0.001* (0.000)	0.000 (0.001)	0.001+ (0.001)	0.000 (0.001)	0.004** (0.001)
Num.Obs.	596 120	225 424	230 213	66 256	74 227
R2	0.000	0.000	0.000	0.000	0.000
R2 Adj.	0.000	0.000	0.000	0.000	0.000
AIC	-727 631.0	-342 914.4	-267 533.6	-62 523.4	-70 596.5
BIC	-727 585.8	-342 873.1	-267 492.3	-62 487.0	-70 559.6
RMSE	0.13	0.11	0.14	0.15	0.15
Std.Errors	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table SI4: Mailer Effects on Registration/Voting (Subset: felony convictions)

	Registered	Registered	New/Re-Reg	New/Re-Reg	Voted	Voted
(Intercept)	0.142*** (0.001)	0.009*** (0.000)	0.015*** (0.000)	0.009*** (0.000)	0.034*** (0.000)	0.004*** (0.000)
T1/T2: Ind. Mail	0.002 (0.002)	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.002* (0.001)	0.002** (0.001)
T3: Social-tie Mail	0.001 (0.001)	0.001** (0.000)	0.001** (0.000)	0.001** (0.000)	-0.001 (0.001)	-0.001 (0.001)
Already Registered		0.990*** (0.000)		0.044*** (0.001)		0.225*** (0.002)
Num.Obs.	367 563	367 563	367 563	367 563	367 563	367 563
R2	0.000	0.929	0.000	0.014	0.000	0.175
R2 Adj.	0.000	0.929	0.000	0.014	0.000	0.175
AIC	269 356.2	-704 035.3	-484 322.9	-489 632.2	-206 755.3	-277 440.6
BIC	269 399.5	-703 981.2	-484 279.7	-489 578.2	-206 712.0	-277 386.5
RMSE	0.35	0.09	0.13	0.12	0.18	0.17
Std.Errors	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

A.7 Robustness

This section presents additional specifications. Table [SI4](#) subsets to only people in the sample with confirmed felony convictions (as opposed to felony cases that did not lead to a felony conviction, as in the case of people successfully completing the “deferred adjudication” process), while Table [SI5](#) treats the two individual-mailer arms separately rather than collapsing them as in the main paper.

Table SI5: Mailer Effects on Registration/Voting (all arms)

	Registered	Registered	New/Re-Reg	New/Re-Reg	Voted	Voted
(Intercept)	0.167*** (0.001)	0.010*** (0.000)	0.017*** (0.000)	0.009*** (0.000)	0.043*** (0.000)	0.004*** (0.000)
TreatmentT1	0.005* (0.002)	0.003*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.003* (0.001)	0.003* (0.001)
TreatmentT2	0.001 (0.002)	0.002** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.001 (0.001)	0.001 (0.001)
TreatmentT3	0.000 (0.001)	0.001* (0.000)	0.001* (0.000)	0.001** (0.000)	0.001 (0.001)	0.001 (0.001)
Already Registered		0.990*** (0.000)		0.047*** (0.001)		0.244*** (0.001)
Num.Obs.	596 120	596 120	596 120	596 120	596 120	596 120
R2	0.000	0.939	0.000	0.017	0.000	0.191
R2 Adj.	0.000	0.939	0.000	0.017	0.000	0.191
AIC	516 772.3	-1 153 694.4	-727 630.5	-737 816.7	-203 895.8	-330 038.8
BIC	516 828.8	-1 153 626.6	-727 574.0	-737 748.9	-203 839.3	-329 971.0
RMSE	0.37	0.09	0.13	0.13	0.20	0.18
Std.Errors	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID	by: HH_ID

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

A.8 Envelope Usage

This section presents exploratory analysis of an intermediate measure of responsiveness to our mailers: the use of the enclosed return envelope from the mailer packet. Each mailer contained a paper copy of the Texas voter registration form and a postage-paid, pre-addressed envelope that could be used to return the completed form. Using the postal service’s “Share-mail” feature, our mail vendor is able to track which of these return envelopes were actually used. While we don’t know who used the envelope (and, indeed, many of these envelope uses do not result in the eventual voter registration of the targeted individual), these uses indicate that someone used the materials in the mailer to try to get registered to vote. We use this additional outcome measure to learn a bit more about how and when our social-ties mailers worked.

Figure SI3 compares each experimental treatment arm to the uncontacted control group (which is, by construction, 0) on this outcome. It shows that our social-ties mailer yielded similar rates of envelope usage to the individual-mailer arms. This similarity is notable, since Figure 3 shows the social-ties arm had smaller effects on the voter registration of the “focal individuals” in the sample, compared to the effects of the individual mailer. That we see such similar rates of envelope usage, but different registration effects, suggests to us that our social-ties mailers may often be getting other people (besides the focal individuals in our sample) registered to vote.

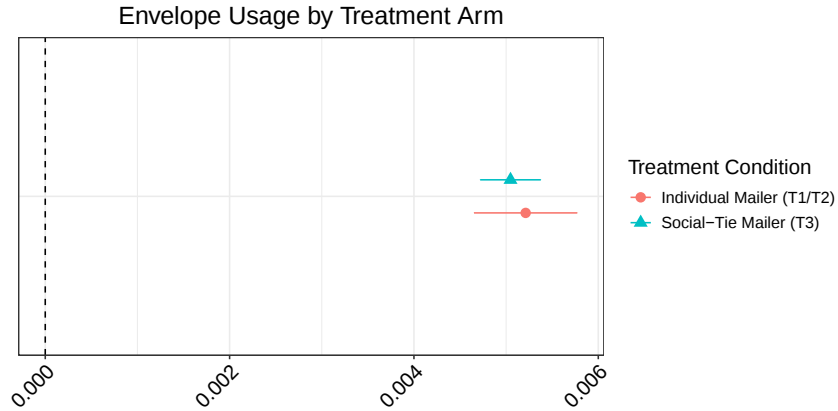


Figure SI3

Figure SI4 then examines envelope usage among subsets of the sample with different types of available social ties, analogous to Figure 4 in the main paper. Again, within all subgroups save the first (the one where we did not send any social-ties mailers and thus no envelopes could be used), we see social-ties mailers yielding similar rates of envelope usage to the individual mailers, and in some cases higher.

Where are social ties effective? (DV: Envelope Usage)

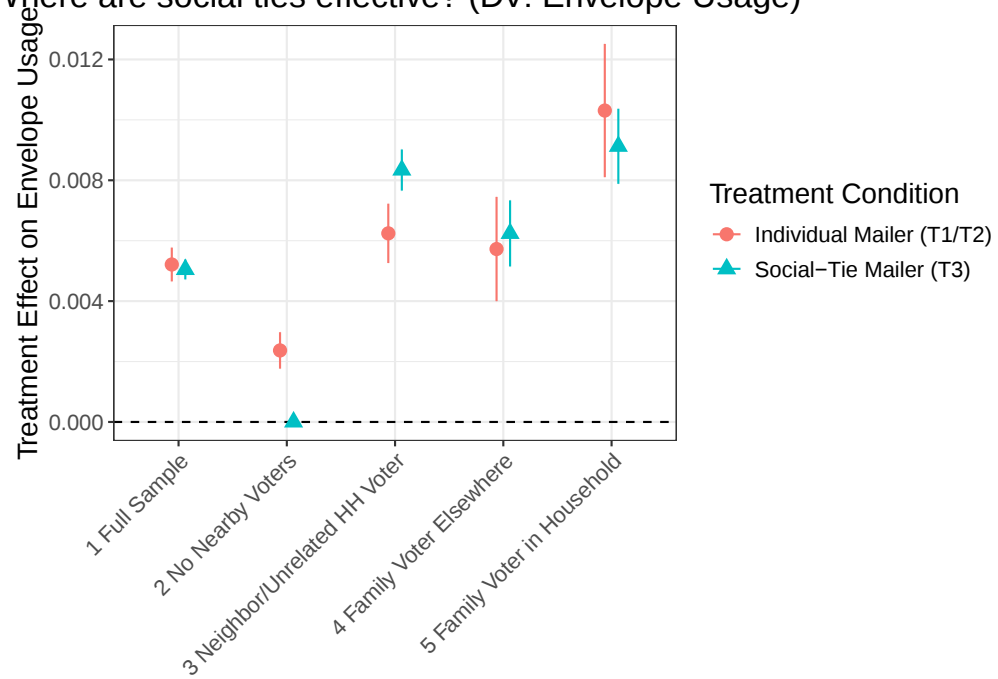


Figure SI4

A.9 Other hypotheses

In this “overflow results” section, we present additional analyses described in our pre-analysis plan. Our preregistration noted the following comparisons/hypotheses for the mailer study:

1. Each treatment condition will increase turnout and registration relative to the uncontacted control - *This hypothesis is addressed in Section 5 of the main paper.*
2. The credible-messenger condition will increase registration and turnout more than the simple individual-mailer condition - *This hypothesis is about the two different individual-mailer arms, which are analyzed separately in Table SI5 above. Though treatment arm T1 appears to have slightly larger effects on all three outcomes, those differences are never statistically distinguishable from 0 (coefficient test appears in replication code).*
3. All treatments will be more effective at increasing voting/registration among people with a history of incarceration - *We present this analysis in Table SI6, including an interaction term between “Ever in Prison” and our treatment conditions. Briefly, we see no evidence that our treatments were more effective among people who had been to prison: the interaction terms are a mix of small positive and negative estimates, with one statistically-significant estimate on the social-ties mailer and voter turnout that we are reluctant to overinterpret given the number of tests run.*
4. We also planned to test whether the effects of the individual and social-ties mailer arms were distinguishable from one another, in the overall sample and also by social-tie availability. - *In the replication code, we conduct tests for the difference between the individual-mail and social-ties-mail coefficients for all three outcomes shown in Figure 3. The individual-mailer effects are distinguishable from the social-ties mailer effects on the two registration-focused outcomes but not turnout (though we note the effects aren’t really directly comparable given different compliance rates as discussed in Section A.10). For people with a close social tie for us to target (a family member within the household), the effects of the individual-mailer and social-tie mailers are similar in size and indistinguishable from each other on all three outcomes (shown in replication code).*
5. Finally, we planned to examine racial heterogeneity by interacting treatment indicators with race variables from the conviction records. *Figure SI5 below splits out our main figure from the paper by racial group. In some cases it looks like the treatment was especially effective for white people in the sample (particularly the individual mailers for registration-updating), while in most cases there is no substantive or statistical difference across groups.*
6. We also described an analysis based on QR codes: all our letters included personalized QR codes that we can track to see if recipients used the codes to seek out more information. We do not have this information for the control group (rather, it is mechanically 0 for anyone not sent a letter), so as described in the PAP, we simply examine QR code usage across the treatment conditions. *Briefly, very few people*

Table SI6: Mailer Effects on Registration/Voting, by Prison Experience

	Registered	New/Re-Reg	Voted
(Intercept)	0.010*** (0.000)	0.009*** (0.000)	0.009*** (0.000)
T1/T2: Ind. Mail	0.002*** (0.001)	0.004*** (0.001)	0.002+ (0.001)
T3: Social-tie Mail	0.000 (0.000)	0.001* (0.000)	0.002** (0.001)
Ever in Prison	0.000 (0.000)	0.000 (0.000)	-0.013*** (0.001)
Already Registered	0.990*** (0.000)	0.047*** (0.001)	0.243*** (0.001)
Ind. Mail * Ever in Prison	0.000 (0.001)	-0.001 (0.001)	0.000 (0.002)
Social-tie Mail * Ever in Prison	0.000 (0.001)	-0.001 (0.001)	-0.003** (0.001)
Num.Obs.	596 120	596 120	596 120
R2	0.939	0.017	0.192
R2 Adj.	0.939	0.017	0.192
AIC	-1 153 691.9	-737 814.2	-330 822.0
BIC	-1 153 601.6	-737 723.8	-330 731.6
RMSE	0.09	0.13	0.18
Std.Errors	by: $HH_I D$	by: $HH_I D$	by: $HH_I D$

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

scanned the QR code in any condition: only 328 rows in our dataset had a QR-code scan recorded. With such low rates of usage, we cannot statistically distinguish the treatment arms’ usage rates from each other (test shown in replication code).

7. We also said we would analyze the data subsetting to people for whom we found residential addresses. - As anticipated, reproducing the main figure in the paper within this subset yields slightly larger treatment effect estimates (code included in replication package). We omit it here due to space limitations.

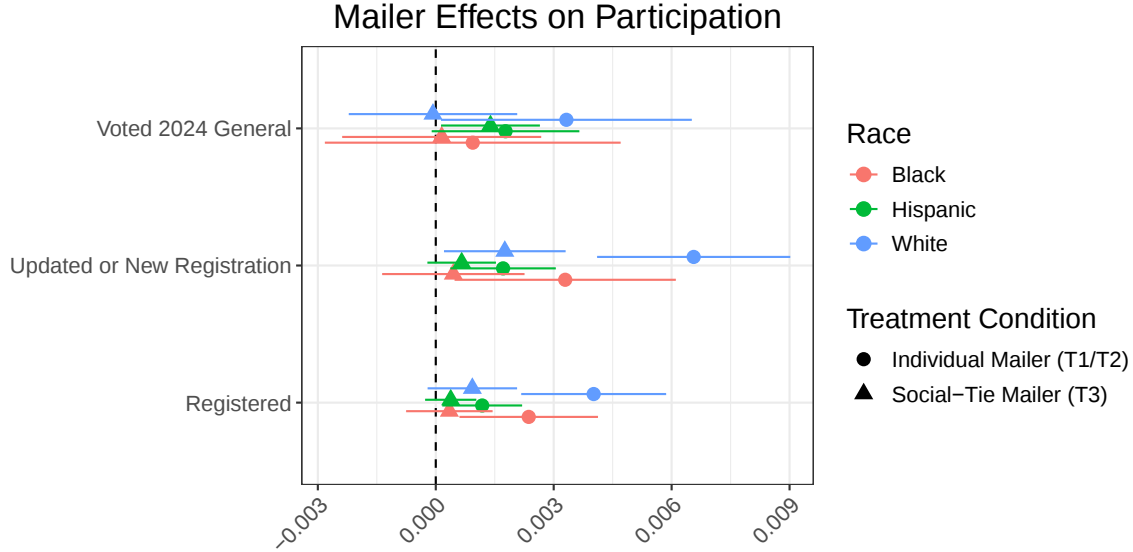


Figure SI5: Mailer treatment effect by race

A.10 Follow-up Mail-Open-Rate Test

As noted in the main text, many of our mailers may not have reached or been opened by their intended targets. Our intent-to-treat estimates, presented in the main paper, give a realistic sense of how a mail-based intervention like this could be expected to work in the real world if targeted in the exact same way. But we might wonder not about “what happens when you try to send mailers to people’s loved ones?” but “what happens when people’s loved ones *receive* mailers?”. This is a harder question to answer given experimental “non-compliance” (meaning, in this case, that we weren’t able to send a mailer to everyone and not all the mailers we sent were delivered or opened. We set aside the additional question of whether family members who receive our mailers actually try to do anything in response to them.). In this section, we try to estimate about how many of our mailers were likely delivered and opened, and then roughly rescale our main estimates based on that rate.

We conducted a small followup study in January 2025 (shortly after our main mailer study) to measure mail open rates. We randomly sampled 100 addresses from each of our individual-mailer and social-ties samples from the main mailer experiment list, for a total of

200 addresses. We then sent 199 of them a mailer intended to learn about their propensity to open mail.²⁰ Each mailer consisted of a plain envelope addressed to the same person who’d been contacted for our main mailer study, with the intention of having the piece of mail look quite similar to the main study mail (and thus having people be equally likely to open it). Inside was a brief letter encouraging them to take a short survey on current events, with a personalized QR code to access the survey (as well as a short, nonpersonalized link as another option), and a \$5 pre-paid gift card as an expression of our thanks. Our intent was to measure whether people opened the mail by tracking whether they used the gift card, or alternatively whether they scanned the QR code for the survey; either action would tell us that the mail had landed and been opened (though neither would let us learn about whether it had actually reached its intended recipient).

Ultimately, 26 of the mailers had some affirmative indication of having been opened ($26/199 = 13\%$). This 13% open-rate estimate is a possible underestimate in the sense that some people may have opened the mail but decided not to use the included gift card; it is a possible overestimate in the sense that some of those mail-open actions may have been taken by someone other than the intended recipient of the letter (and so may still not tell us about whether our actual treatment in the main study landed as intended). Still, we use it as a rough estimate of treatment delivery. Recall also that not all of the people assigned to treatment in our main study even had a letter mailed out; in our social-ties condition, for example, we were only able to send letters for about 62% of the sample due to missingness in addresses or social ties. So we estimate that about eight percent ($.62 * .13 = .08$) of the sample actually saw our treatment get appropriately delivered to the social tie we had identified.

Scaling the main intent-to-treat estimates requires some strong assumptions; we urge caution given that people who could not be reached by our mailers might be different in some way from people who could. But if we assume homogeneity, we can scale the main social-ties treatment effect estimates accordingly: for the measure of new or updated registration, this is $.001 * (1/.08) = .0125$, or one and one-quarter percentage points’ increase as a treatment-on-treated effect. For the slightly smaller estimates of voter registration effects, it is about three-quarters of a percentage point increase.

B Relational Organizing Study

The relational organizing study was implemented between June - mid-November of 2024. The research team recruited Grassroots Leadership (GRL) to serve as our community partner in the spring of 2024. GRL is a community based organization headquartered in Austin, and with a chapter in Houston. Recruitment of captains took place during the summer of 2024. Captains were trained and their lists collected and vetted during the first two weeks of September. The captains implemented the treatment itself between September 20 - October 7, 2024 (the deadline to register and be eligible to vote in the 2024 general election). Following the conclusion of the treatment implementation period, captains completed exit

²⁰We lost one row of the sample to a test of our gift-card tracking process: we had ordered 200 gift cards and needed to use one to ensure we’d be able to track them as they were used.

interviews with the research team. Exit interviews were conducted between early October and mid-November.

B.1 Recruiting the Mobilization Captains

We recruited the mobilization captains through a series of informational events between June and August. In total, we recruited 48 mobilization captains. Captains had a variety of relevant experience. Some were system-impacted people interested in organizing their community. Others were not system-impacted, but interested in doing voting rights work. Some captains were seasoned organizers, and some were new to politics and voter education efforts. We recruited 25 captains who primarily worked through the Houston location, and 23 who worked through the Austin location. In practice, however, captains were located in cities across the Central and East Texas regions, including in San Antonio, Dallas-Fort Worth and Texarkana. Captains received a \$500 stipend for their contribution to the project.

B.2 Sample Construction, Cleaning, Record Linkage

To build the experimental list, we asked the mobilization captains to provide us with a list of the names and birthdays of 20 individuals in their networks who they thought were system-impacted and unlikely to vote. From these initial lists, we had 1,113 contacts. As was to be expected in a relational organizing model, there was variance in each captain’s list: list length varied from 2 contacts to 43 contacts, as did quality in terms of contacts’ information (e.g. incomplete DOB), and—found through processing these individuals as discussed below—eligibility to be included in our experiment.

First, we searched for all possible matches to the contacts in the Texas Computerized Criminal History (CCH) files based on last name and DOB, accounting for any aliases or incomplete DOB information. We then determined whether there were one or more good matches if there was also a close first name match. Using the information in the CCH, we also determined whether a person was eligible if they were ‘off-paper’ for every felony conviction they may have or if they alternatively had a misdemeanor but had never been convicted of a felony. This automated process gave us singular, strong, eligible matches for a portion of our contacts. But other contacts remained unmatched, or the matches we had were multiple, or those matches were ambiguous as to their eligibility. For these reasons, we turned to a manual approach, deciding on a case-by-case basis a person’s contact with the criminal justice system and if so, their matched institutional information. As part of our process of making manual determinations, we: 1) considered the names, dates of birth, race, and more from the CCH in the matches found above, 2) performed further manual lookups using internet-based searches of offenders and inmates via the Texas Department of Public Safety’s Criminal History Conviction Name Search, 3) searched the records of county jails and local courts, and 4) requested clarifying information from captains. During this we not only marked individuals as eligible and ineligible, as above, but also broadened our categorization to capture the three additional ways the contacts could be system-impacted (beyond having a conviction). We categorized a person as ‘proximal’ if we confirmed that a family member (which could include the captain) had direct contact with the criminal justice system. We categorized a person as ‘unconvicted’ if they had been arrested but

had not convicted; e.g. we confirmed their appearance in county jail records. Lastly, we categorized person as ‘open’ if they were in the justice pipeline but eligible to vote; e.g. awaiting trial.

The kinds of social ties included in the experimental list varied. Many people included close friends and family members, the kinds of relationships we expected to be most effective at encouraging registration and voting. Several captains worked with other organizations that address needs that are common among those who are justice-impacted, including peer-to-peer mentorship for people in recovery, support for people facing houselessness, and anti-poverty services. Some captains included people they served through this work on their lists. Other captains included people with whom their relationships were less personal and active.

Once we had completed our exhaustive search through the available information, we prepared the data for randomization, including removing any contacts that were ineligible as well as some duplicate observations that existed within individual captains. At this point in sample construction, we had 626 contacts. We then performed block-level randomization at the level of our captains, splitting each captain’s remaining list into roughly equal parts. We shared captains’ contact lists with them at the second training, via email or via text. We also did further cleaning post-randomization focused on harmonizing the disparate threads of information we had for each contact and updating contacts with any information learned after the relational organizing experiment’s rollout in mid-September 2024.

B.3 Treatment Implementation

The mobilization captains completed two training sessions. The first training focused on research ethics and design. Academic institutional requirements and best practices for community engaged research classify individuals engaged in implementing the treatment (as the mobilization captains were) as project staff who should be provided training around ethical research practices. It was also necessary to brief the captains on the overall research design and the nature of the research protocols we would ask them to follow. With respect to the implementation of the research design itself, the training emphasized the importance of randomization, and the importance of not contacting individuals in the control group.

The second training focused on the rules and regulations governing who is eligible to vote in Texas, how to get registered to vote, and who can register voters. The goal was to equip captains with clear information and resources to aid them in their mobilization, and to help them act within the law while talking with their loved ones. The second half of the training was devoted to practicing strategies for engaging people in conversation about their right to vote. Captains walked through conversational scenarios and were provided with suggested prompts to help them feel more confident when approaching their peers about registration and voting.

The implementation period, during which captains were reaching out to individuals on their contact lists, lasted from September 20, 2024 - October 7, 2024 (the registration deadline to be eligible to vote in the November 5th general election). Throughout this period the research team followed up with organizers via email and text message to check in and discuss any problems they might be having. The research team also held one in-person meeting at each site during the implementation period. The purpose of this meeting was to check in with the organizers, share stories, and problem solve. Organizers were instructed to

try to make contact with individuals on their treatment lists at least three times during the implementation period. Organizers were free to use whatever contact method they preferred, whether in person, over the phone, or via social media. During exit interviews, we collected data on the specific conversations captains had with each list member.

B.4 Exit Interviews and Analysis

After the conclusion of the treatment implementation period, the captains were debriefed as a group. One-on-one exit interviews were conducted between mid-October and mid-November of 2024. Not all captains were able to complete the exit interviews. Interviews were completed with 38 out of 48 mobilization captains.

Exit interviews consisted of two parts. In the first part, the interviewer went through each person included on the captain’s contact list and asked a series of questions about the strength of the captain’s relationship with the mobilization target, the frequency and mode of contact, and details about the conversation overall. In the second part of the interview, mobilization captains were asked about their own experiences and their reasons for wanting to become involved in the voter mobilization project. These interviews provided insight into how the treatment was implemented in the field, and when providing people with information about their eligibility and how to vote successfully improved registration and turnout.

The interview transcripts were transcribed using NVivo. A research assistant then listened to the recordings and compared them with the transcriptions, correcting the transcriptions where necessary. NVivo was then used to take a first pass at identifying themes present in the interview transcripts. From this, the researchers then devised a coding scheme, identifying the primary themes present in the interviews that focused on the nature of the relationship with the mobilization target; the mechanisms by which the relational treatment worked; and the barriers faced by captains to convincing the mobilization target to turn out. After these themes were identified, two research assistants were enlisted to code the interviews by hand, identifying when a given theme was present in an interview, and extracting quotes that exemplified the theme. The research team then compared the results and identified areas where coding by the research assistants were in agreement, and where they disagreed. Places where the two coders agreed were taken as the final thematic coding for a passage from a given interview. PIs discussed instances where the coders determinations diverged to make a final decision.

We draw several conclusions from the qualitative effort, which we discuss in brief in the body of the manuscript. Reports from organizers suggest that the quality of their relationship with a mobilization target was related to the success of their efforts. Relationships that were relatively close, where the individuals spoke frequently, yielded more high quality conversations than more distant relationships. Close contacts were open to speaking about politics and comfortable talking about their experiences with the criminal justice system, which provided a pathway for captains to educate them about their right to vote.

In contrast, when organizers were not in regular contact with the individual they were targeting, even if they felt they had a strong relationship with that person, they often faced challenges contacting the target. Several organizers reported instances when they were only connected to individuals via social media, and they were never able to successfully have an exchange with the target of their efforts. Interviews suggested that face-to-face contact was

the best strategy for having a meaningful conversation about the right to vote. In some instances, organizers got creative and enlisted the help of other people in their networks to reach mobilization targets. This was successful when the organizer had planted seeds about the value of voting. For example, Monique recounted an interaction with someone targeted for mobilization efforts who ultimately relied on their loved one to register:

He was very, very excited to know that he could vote. And he said the same thing. Kind of like, ‘Can you. Can you help me figure out the process? Sign me up,’ and so we talked. I want to say maybe three times... I just followed up and said, okay, you know, you can register... his wife, I think, was helping him figure out the process or something. They were working through it.

There are three primary ways that mobilization captains worked to mobilize the people on their list: through providing information, applying social pressure, and persuading individuals that voting could be valuable. Many individuals targeted for outreach were already interested in politics and held the norm that voting was valuable. In this instance, organizers were most positive about the conversations they had, because they were able to provide a valuable good to the person they were targeting - information and civic literacy.

A second approach mobilization captains described as successful was the application of social pressure. In this instance, captains would make arguments like, “if you don’t vote, you can’t complain,” indicating that the social sanction of loss of respect for one’s opinion would follow from declining to vote. Captains would also appeal to the relationship itself, imploring targets otherwise uninterested in voting to do it simply because the captain asked. They would also make group-based appeals along the lines of, “our ancestors fought for this right, it is our responsibility to vote.”

The biggest challenge captains faced was convincing more alienated individuals that their political voices mattered and that voting is valuable. For example, Carl spoke with an individual with whom he had a strong relationship, and who had expressed interest in voting in the past. But this individual was frustrated with politics, and Carl worried he would not vote this cycle:

You know, he is married, he has kids. And the fact that they trying to take out... the public school system and he’s like, ‘okay if our votes mean something, why is this even an issue – like just common sense of we need to have the public school system and common sense for women to have their rights. But is it truly that our vote is mattering? Or is our vote just something to make us feel good, and they’re making their own decisions?’ ... It’s like, well, you should still just vote just in case our rights matter. I think our rights matter. He’s just like, ‘Yeah, but if our rights matter. Why does this keep happening over and over and over? So.’

Some captains reported that they were able to overcome disaffection towards voting when they engaged people about local political races and connected voting to issues they cared about. In these instances, it wasn’t so much that the captains convinced people who did not think voting was valuable to suddenly believe in voting as a civic virtue. Rather, they

convinced individuals that under some circumstances voting could matter (such as in local elections) and that voting in those elections could impact things they cared about (such as voting for the local DA or in their children’s school board election). This may provide the foundation for convincing the individual that voting is generally a good thing over the long term.

In sum, qualitative interviews helped the researchers understand how the treatment was implemented in the field, and the conditions under which the treatment could work. As discussed in the manuscript, the treatment was most successful when the relationship was active and captains were able to have face-to-face conversations. While research elsewhere has found that social media can sometimes be an effective tool, for the system-impacted population close relationships with communication that happened offline is more effective. Likewise, while previous research on social ties suggests that social pressure is most effective in social groups who already believe voting is valuable, our research suggests that social ties can step in even when the mobilization target does not hold this belief. Social ties are best suited to convincing people to change their perspective on voting.

B.5 Linkage to the Voter Records

Our process of linking the contacts in our captains’ lists to the voter records is similar to the one used for the mailer study (see A.3), but differs in several key ways. In the relational organizing experiment, unlike the mailer study, a portion of the individuals—132 out of the total 626—did not have information in the state conviction database (CCH). (This is because we included people with various types of contact in the sample, so not all of them had felony cases.) For those 132 contacts, our identifying information was generally worse; we lacked middle names for almost all individuals, and many had incomplete or misreported birthdates. Another key difference is that the sample size is much smaller; thus, we decided it was both feasible and prudent to manually check each match to the voter records. With manual checking, we were also able to include much looser matches, which in the context of the mailer experiment would have introduced too high a mismatch rate.

For the set of contacts with CCH information, up to five names and two dates of birth from the file were used in combinations when attempting to find a match. These include the primary, ‘base’ entry for each of these pieces of identifying information, as well as alternatives (e.g. aliases). In the file not using convictions info, the contact name provided by the captain is used, in the form we arrived at after any clarifications they communicated to us or that we found. In addition, we used up to three alternative names and one alternative date of birth we had found. In both sets of contacts, we first searched for high quality candidate matches to the list of voters with active registrations as of our December 2024 voter registration snapshot. For the set with CCH information, this was the same process as that used in the mailer experiment with the additional allowance for the day, month, or year information to differ. For the set without that information, we attempted a variety of exact and inexact matches across first name, last name, and DOB. We then manually reviewed these candidates, marking any that were a reasonable match based on name. After removing any matched contacts, we iteratively considered candidates that were: 1) an exact match on last name and DOB, 2) any of the previously described match types extended to the cancelled and suspended voter records, and 3) an exact match on first name and DOB. We then referred

to other public records (birth/marriage records, social media, people search) to confirm or rule out these looser candidate matches.

Confirming our final set of matches between the contacted individuals in the relational organizing experiment and the December 2024 Texas voter registration snapshot provided our main outcome for the study: voter registration. As in the mailer experiment, we used the unique voter ID to link to the voter history file and thus mark our contacts’ participation in the 2024 general election. Also like the mailer experiment, we link the voters longitudinally to the pre-experiment September 2024 voter registration snapshot to identify any new or updated registrations.

B.6 Additional Estimates

Table SI7: Additional Outcomes, Relational-Organizing Experiment

	<i>Dependent variable:</i>			
	New or Updated Registration		Voted Nov 2024	
	(1)	(2)	(3)	(4)
Relational Treatment	0.015 (0.017)	0.015 (0.017)	−0.004 (0.034)	0.003 (0.031)
Already Registered		0.001 (0.018)		0.338*** (0.033)
Constant	0.039*** (0.012)	0.038** (0.017)	0.229*** (0.024)	0.003 (0.031)
Observations	621	621	621	621
R ²	0.001	0.001	0.00002	0.147
Adjusted R ²	−0.0003	−0.002	−0.002	0.145

Note:

*p<0.1; **p<0.05; ***p<0.01

C Pre-Analysis Plan and Discussion

Below, we include the pre-analysis plan for both the mailer experiment and the relational-organizing experiment described in the main paper, filed with OSF in September 2024.²¹

We adhere to the PAP as closely as possible. Here we note a handful of places where we diverge from the planned analysis:

²¹Link redacted for blind review.

- The PAP listed two main electoral outcomes for the mailer study: being registered to vote post-election, and voting in the November 2024 election. We expected these simple measures to capture all actions unregistered people could take, and we intended for our sample to be unregistered since we excluded everyone with a name/DOB match to the voter file. However, as noted in the main paper, about 16% of the sample turned out to have a previous voter registration under a name other than the primary name on their conviction record. As such, we include a third outcome measure, “new or updated registration” that captures whether someone either newly registered (possible for unregistered people but not the already registered) *or* updated something about their existing registration record, such as their name or address (possible for the already-registered). We also include pre-treatment (September 2024) voter registration as a pretreatment covariate for statistical precision in some specifications (see Table SI2 above for versions with and without pre-treatment registration as a covariate).
- As noted in section 4 of the main text, many of the people in the sample for the relational/“captains” experiment turned out to already be registered, so in the main paper we present an exploratory look at our treatment effects among people who were not already registered (we had not preregistered this specification because we expected most of the sample would be unregistered, as our initial voter file merge had indicated before we did more cleanup of the captain-provided lists).
- At the time we pre-registered the study, we had identified 626 people to include in the sample. Additional manual record searches conducted after treatment assignment identified five people we believed were ineligible to vote due to ongoing sentences, so we followed up with captains where needed to ask them not to treat those individuals and dropped those five people from the sample, yielding the sample size of 621 shown in the main analysis tables. We further note that there are eight rows in the sample where subsequent name checks/cleanup led us to suspect they were duplicated (that is, four people seem to have appeared twice on our list because multiple captains added them to the list under slightly different names). Reproducing the main table dropping all eight rows yields nearly identical estimates.

Testing Relational Approaches to Political Reincorporation

September 20, 2024

1 Overview

This is a research design investigating how eligible but unregistered voters might make their way back into political life after a past criminal conviction. It involves two experimental components: a large-scale mailer-based study design and a person-to-person “relational” design, both to be fielded in Texas during the 2024 election season.

The mailer study focuses on people with felony records in Texas who are legally eligible to vote but are not registered, and traces possible ways of finding and contacting them either directly or indirectly through social ties. We will describe the landscape of people with past convictions more thoroughly than past work, including their ties to politically-engaged people, and will then experimentally test several outreach approaches encouraging them to register and vote: an informational letter from a grassroots civic organization, the same type of letter introduced/signed by a person with a criminal conviction (a “credible messenger”), and letters sent not to the focal individual but to already-registered voters with some sort of family or social tie to the unregistered person.

The “relational” design will begin with a team of mobilization “captains” recruited by a partner organization in Texas. These captains will build potential-outreach lists of people in their social networks with some past criminal legal contact. After some list vetting to ensure people on the contact lists are part of the target population and are eligible to vote, the lists will be randomized and captains will reach out personally to a fraction of their contact lists to encourage them to register and vote.

We view these two designs as complementary: the mailer study provides descriptive information and experimental estimates for the full population of interest, while the relational model gives a stronger and more realistic treatment and the opportunity to trace some specific mechanisms. We describe them in turn.

This document is being filed after randomization and before collection of any outcome data, as treatment is beginning to be implemented (mailers have been sent out and mobilization captains have just received their contact lists).

2 Mailer Study

2.1 Mailer Study Data Setup

We put together a sample of people with felony convictions in Texas believed to be eligible to vote but currently unregistered. We begin with the Texas convictions database, collected from the state, and clean it to omit people who do not appear to be eligible to vote due to active (or not clearly ended) sentences, known non-citizenship, or death while in custody or on supervision, as well as a few thousand people with incomplete name information.

This process leaves us with a list of about 1 million people, which we then merge to the state voter file of Texas (snapshot from May 2024) to determine which people on the list are already registered to vote and thus should be excluded from the study. We use name components and dates of birth in varying combination to allow for different approaches to recording names across databases and time: most people matched have exact matches on first/last name and date of birth, but we also capture some matches where a female voter’s “former last name” on the voter file matches the last name in conviction records, etc.¹ Excluding everyone who appears to be registered, as well as (at this point) excluding people who were recorded as having died (using a 2013 copy of the SSA death master file) or who were under age 18 or over age 75 in August 2024, yields a list of about 740,000 people who appear to be eligible to vote in Texas after a previous felony conviction (if still living in the state).

We then send this list to a commercial address vendor for matching. About four-fifths of the list matches to a recent address, though some of those addresses are out of state. We do not drop non-matched people from the sample (since we are interested in an ITT among eligible voters, regardless of whether we are actually able to reach them with treatment), but we do exclude just under 146,000 people who, based on address matches, appear to live out of state and thus to be ineligible to register and vote in Texas. This leaves us with a list of 596,120 people that we believe to be unregistered-but-eligible voters in Texas after past felony convictions; this is the list we will use as the experimental sample.

¹Permuting dates of birth by adding 35 days to the DOBs in the conviction records and re-matching yields a rough guess of 1.7% false positive match rate from this approach.

2.2 Mailer Study Experimental Design

2.2.1 Experimental Treatments

We assign members of the sample to one of four conditions with the following intended treatments:

1. An uncontacted control group (“Control”)
2. A group mailed a letter from a grassroots civic organization providing information about eligibility and registration/voting and encouraging them to register (“Individual Mailer”)
3. A group mailed a letter with the same information and group logo, but with an introductory message from a person with a criminal conviction (“Credible Messenger Mailer”)
4. A group not directly sent mail, but instead contacted indirectly via social ties: someone connected to them (via family ties or geographic closeness) is sent a letter with the same information about eligibility as the other treatments, encouraging that person to help the focal individual register and vote. (“Social-Ties Mailer”)

Compliance with these treatments may vary depending on data availability, as we discuss below.

2.2.2 Randomization and Treatment Implementation

“Focal individuals” from our list of eligible but unregistered people are assigned to the four treatment arms as follows. First, for pairs of focal individuals who are known to live at the same address as one another, we constrain the randomization to ensure that both people in the household are assigned to the same treatment condition rather than potentially receiving different letters. Thus, we have 565,012 household clusters to be assigned to treatment, containing 596,120 individuals.² We then assign those clusters to the four arms using the R package `randomizr`, with 59% to control, 30% to social-ties arm, and 5.5% to each of the two individual-mailer arms. This yields a final sample with treatment arm counts and covariate balance as displayed in the balance table at the end of the document.

²In a small number of cases where more than two focal individuals (in some cases, several dozen) were matched to the exact same address, we assumed that those addresses were not likely to be valid (such as homeless shelters or halfway houses, places where people rarely remain for long and keep receiving mail) and thus we treated people at those high-volume addresses as not actually having an address match.

For each treatment arm, we then treat as many of the people assigned to that arm as possible. For each of the two individual-mailer arms, that means that we send out individual mailers to everyone with a valid mailing address (approximately 80% of the sample) who has been assigned to that treatment condition. For the social-ties arm, it means that we send out a mailer to a social tie for anyone with an available social tie. We prioritize closer social ties where available, so if someone lives with a family member who is a registered voter (sibling or parent, as found from birth records, or a spouse as found from marriage records, or someone with the same last name), we send a mailer to that person rather than to an apparently-unrelated person in their household or to a neighbor. But in the absence of close familial ties, we then proceed to use looser ties: co-residence at a household address without other apparent linkages, or a registered voter who lives next door to the focal individual or within 50m of them.³

On the final mailer list, there are 159,477 individuals. Of these, 51,427 out of the 65,612 assigned to one of the individual treatment arms (78.4% compliance) are set to receive one of the two types of individual mailers, and 108,050 out of the 178,906 individuals assigned to the social ties treatment arm (60.4% compliance) have a voter who was marked as a valid social tie to the target individual who are set to receive the remaining 108,050 mailers.

Mailers are to be printed and mailed in early September, for in-home dates in mid-September. This timeline allows for people to receive the mailers and return voter registration forms before the October registration deadline for the November 2024 general election.

2.3 Mailer Study Analysis

2.3.1 Descriptive Analysis

We view the descriptive portion of this project as one of the main contributions of the paper. Beginning with a nearly-comprehensive list of people in Texas who are eligible to vote after a felony conviction, and combining it with various other public and commercial records, allows us to characterize the behavior and connections of this large group of unregistered voters

³In a small number of cases, we found that the same social tie was matched to multiple focal individuals, sometimes because a registered-voter parent lived with multiple focal-individual children or other cases of this sort. In order to avoid recipient confusion and SUTVA violations from sending multiple letters to the same registered voter, we constrained the treatment to ensure we did not send multiple letters about different people to the same registered voter. In most cases, we were able to simply choose the next-best social tie for one of the household members and use them as the mailer recipient for that focal person. In a small number of cases (several thousand, out of over 170,000 people), we were not able to find alternative social ties and thus we did not send out a mailer about one of the focal individuals who had “shared” a social tie.

with a level of detail that has previously not been possible. We will describe not only the baseline rates of registration among eligible voters with past convictions in the state, in line with past work, but will also be able to flag how many people appearing in administrative records are actually not eligible voters due to out-of-state moves or death (thus suggesting a reconsideration of past estimates of registration and voting among this group). Then, we use administrative records of births and marriages, as well as commercial address data, to uncover how many people in this population are currently reachable via mail, as well as how many have visible connections to already-registered voters. This description, including description of specific types of family and social ties held by various subsets of the sample, will give a richer picture of the political lives and networks of this population than has previously been available.

2.3.2 Main Experimental Analysis

Our main analysis of the mailer experiment will examine several outcome measures for two main subsamples.

Outcome measures We will examine voter registration (measured by merging the experimental list to a copy of the Texas voter file collected after the general election, voter turnout in the 2024 general election (measured by merging the experimental list to a copy of the Texas voter history records requested from the state in late 2024 or early 2025, once all vote-history data is updated), and mailer QR-code usage via the API of the service we used to generate the QR codes.⁴

Subsamples We will conduct our main analyses on two different subsets of the data. First, we will use the full list of unregistered, voting-eligible Texans with past convictions that we used for randomization. This is our best guess at the full target population, though it may be slightly under- or over-inclusive in some ways (out-of-state moves not captured by commercial records, noncitizens not flagged as such in conviction records, people with convictions from other states). As such, it provides a useful understanding of the “intent-to-treat” effect of a mailer campaign of this sort on the full population of interest. The ITT effects are expected to be small, since we will be unable to treat some fraction of the list due to missing address data, but this compliance issue is useful to understand. For example,

⁴Each mailer contains a QR code that recipients can scan to go to a page on our partner organization’s website to “See how you can get involved even if you are not a voter”. Thus, we use QR scans as a measure of (interest in) other civic engagement.

this could be informative to policymakers or nonprofit organizations seeking to contact this population with information about programs or public benefits.

Second, we will also subset our data to the portion of the list for which we have been able to find current address data and rerun our analyses for this group. This is where we should expect to see clearer effects, since we are largely unable to contact people not in this subset (with the exception of a small number of people in the social-ties arm), and so this subsetting should allow us to see effects among the set of people who would be likely to actually be included in a mail campaign of this sort. We note that this is not an instrumental-variables analysis of the sort often used to deal with non-compliance, because we actually know address-match status for everyone in the sample (including the uncontacted control group) and thus can just subset on it and rerun our main analyses.⁵

Comparisons For both our voting and registration outcome measures, we will compare across our treatment arms within both subsamples (the full list and the address-match list). This analysis will take the form of an OLS regression with indicators for treatment condition; we will present estimates both with and without available pre-treatment covariates (such as race and gender) included. Standard errors will be clustered on household to account for cases where two focal individuals live at the same address and their treatment assignment is determined jointly.

We will test the following hypotheses:

- Each treatment condition (social-ties, individual, credible-messenger) will increase turnout and registration relative to the uncontacted control.
- The credible-messenger condition will increase registration and turnout more than the simple individual mailer condition.
- All treatments will be more effective at increasing voting/registration among people with a history of incarceration (as opposed to people convicted but not sentenced to incarceration); we will test this hypothesis by interacting an indicator variable for past incarceration with treatment.
- We will also test whether the effects of the social-ties treatment and the two individual mailers are statistically distinguishable (i.e. does the social-ties approach have larger or smaller effects than the individual-mailer arms in this study), both overall and also

⁵There is likely additional, unobservable noncompliance in the form of some people not receiving or opening the mail we send them, but we do not have information to incorporate on this issue.

by social-tie availability (an indicator for whether we see any high-quality social ties for that individual, based on administrative records of family ties or co-residence plus shared last name).

- Because previous work has found mixed evidence on racial heterogeneity, we will also examine racial heterogeneity in this study, interacting our treatment arms (or potentially an indicator for any treatment, depending on similarity and power concerns) with available race measures from the conviction records. We do not have clear directional hypotheses here given previous findings, but we will use the larger/broader sample of this study to look again at this question.

As our last outcome measure, we will also collect data on non-voting civic engagement via a QR code provided on both letters (discussed above). Our main comparison for this measure will be between the individual- and social-ties mailer arms; we are curious about whether the social-ties approach yields more uptake of nonvoting participation. We may also present a comparison of the treatment arms to the uncontacted control group, but we note the limits of this analysis given the way this measure is constructed: since the uncontacted control group, by definition, has no access to the QR code that we use to measure engagement, it mechanically has a value of 0 on this outcome measure.

3 Relational Experiment

3.1 Relational Study Experimental Design

This second design is intended to complement the above mailer-study design, targeting a smaller number of people with a much stronger treatment (personalized contact from someone who knows you, rather than simply a mailer). It is based on a model in which we and our partner organization recruit community members to serve as “captains” who will then reach out to people in their social networks to encourage them to register and vote.

The first step is recruiting and training captains. We and our partner organization focus recruitment efforts in two major Texas cities, aiming to recruit at least 50 mobilization captains across the two cities.⁶ Captains come to initial training/orientation sessions, then further sessions throughout the study period to check in on their outreach efforts. They

⁶Several other captains with connections to the organization or its partners also join from other parts of the state.

receive payment of several hundred dollars for their time at these sessions, disbursed in multiple increments through the fall.

Once captains have been recruited, we work with them to build lists of potential contacts. We elicit from them information on people in their lives who they believe to be eligible to vote after some previous contact with the criminal legal system, and then check their names/dates of birth against public records to confirm that they are eligible for inclusion in the sample. We include people with a range of types of contact, though most people in our sample are those with past criminal convictions in the state of Texas. As we seek to include only people who are actually eligible to register and vote⁷, we take several steps to ensure we can exclude people who are ineligible due to a current sentence⁸ and we code people in the sample as having one of several categories of contact:

- “Eligible” people are those who have a previous conviction that appears in state conviction records and have now completed their sentence(s). We determine previous conviction status using a mix of automated and manual lookups in the state’s conviction records, in order to find as many people as possible given variations in name spellings and birthdate information provided by the captains. This group constitutes the bulk of our sample.
- “Unconvicted” people are those with some past legal contact that we can verify through public records, but no conviction on their record. This group largely consists of people whose cases were handled (and ultimately dismissed) via the deferred adjudication process, and whose case records we observe via county court records searches.⁹
- “Open” describes people with current open cases and no previous disenfranchising convictions, observed via a mix of county records searches and the state’s conviction records.
- “Proximal” describes people with proximal contact: captains explicitly confirm that they do not believe this person has a past conviction, but that they are impacted by the

⁷Although our project is explicitly a voter-education project and does not tell any individual person that they are eligible (just what the rules are for eligibility), we nevertheless do not want to expose anyone to legal risk by encouraging them to register if we can figure out that they are ineligible.

⁸People convicted of felonies regain the right to vote in Texas after they have served their full sentence, including any probation or parole.

⁹It is important that we find records of these cases rather than relying on captain’s reports, as mobilization captains may not know all the details of their friends’ cases and might misunderstand their eligibility. But also, finding court records allows us to verify the correct name and birthdate that each person uses when interacting with the state, which lets us properly search for them in the state’s conviction records to exclude the possibility that they have another case that could be disenfranchising.

legal experiences of others in their lives (such as a parent or spouse being incarcerated). We also double-check these names against conviction records.

People who are confirmed to be ineligible (i.e. we can see that they have a conviction with an ongoing sentence, including probation or parole), or for whom we cannot confirm one of the above statuses (we can't find them in the records and we also can't confirm with the captains that they are included due to proximal, not personal, contact) are excluded from the sample and not randomized into treatment.¹⁰

This list-collection and record-checking process yields a list of 626 contacts across 48 captains. We then randomly assign people from the contacts list to be contacted, or not, by the captains who put them on the list. We block-randomize within captain (50-50 treatment and uncontacted control), such that each captain reaches out to some people on their list and not others.¹¹ We then generate lists of the treated units for each captain and return those contact lists to the captains.

The treatment here consists of personalized outreach by the captains, which can take various forms: some people may meet up in person, while others text or call their contacts. Captains receive lists the week of 9/16/2024 and receive detailed training on voter eligibility and registration processes, and are then instructed to begin contacting and registering their treatment lists on 9/20/2024, in advance of the Texas voter registration deadline in early October.

In addition to being in regular contact with the captains at check-in sessions, the research team will conduct interviews with some captains to collect further qualitative information about their experiences and actions in the program. This additional data collection will not provide quantitative measures of implementation processes (i.e. we will not observe individual compliance for each person on the contact list), but will give us a sense of how implementation went and what kinds of efforts captains undertook and felt were successful when doing outreach.

¹⁰Our reason for excluding people who do not match to the conviction records (unless we can either verify a case dismissal, or have explicit confirmation from the captain that this is someone without personal criminal legal contact) is that a non-match to the conviction records could be because someone has no records, but it could also be because we have inaccurate information about them, such as a name misspelling. Thus, we err on the side of caution in order to avoid encouraging ineligible people to register.

¹¹In practice, we conducted this blocked randomization in three waves, because individual captains submitted their lists (and responded to our requests for additional information) at various times.

3.2 Relational Study Analysis

Our main outcome measures for this study are voter registration, registration updating¹², and voter turnout, measured (as in the mailer study above) using state voter-file records. Our main analysis is a comparison of voter registration and (November 2024) voter turnout between contacted and uncontacted list members.

This is a block-randomized design with blocks defined by the mobilization captains: each captain produces their own list and we randomize contact within lists. This means that slight variations in list size across captains can yield different assignment probabilities across captains, and the analysis must account for these varying assignment probabilities as well as the clustered nature of the data. As such, we follow Green and McLellan (2020)¹³ in analyzing this as a blocked experiment with inverse probability weights to handle varying assignment probabilities. Thus we regress our outcome variables (turnout or registration) onto an indicator for treatment assignment and an indicator for each block (captain), applying IP weights to that regression. This yields an unbiased estimate of the Intent-to-Treat effect. We will present these estimates without covariates and may also—if we are successfully able to collect these using public records—include background covariates (age, race, gender, legal history) for precision.

Other outcomes if feasible We will also attempt to collect information about other actions people could take, such as signing up for our partner organization’s mailing list or (as in the mailer study above) scanning a QR code to learn more informations about voting or other civic-engagement opportunities, and if possible will analyze them as described above.¹⁴ Similarly, if we are able to collect later voting data on long-run effects (turnout in future elections), we will look at the persistence of experimental effects, though this possibility also depends on the future actions of our organizational partners and captains. (If we find

¹²Because we do not restrict the sample to unregistered people, some people in our study may already be registered to vote and thus cannot be induced to register. But some of these people may have outdated voter registrations (such as those at an old address) and would need to update their registration in order to actually vote. Our measure of registration updating captures both new registrations and registration changes that occur after our first captain lists are distributed 9/17/2024. If someone wasn’t registered and then gets registered, this variable will be set to 1, and if someone was registered already but changes something about their registration, like updating their address, it will also be set to 1.

¹³https://www.turnoutnation.org/_files/ugd/c2b504_c6eacb6fed514da29335e35d1ac7e079.pdf

¹⁴We will try to collect mailing-list signups at the individual level in order to run an analysis analogous to the ones we run for registration and turnout. The QR code measure is likely to be visible only at the captain level (one QR code per captain, not per contact), so it will mainly give us contextual information about captains’ mobilization efforts rather than an experimental outcome measure.

that people generally go back and get everyone on their list registered eventually, a long-run analysis won't make sense).

Compliance Our main analysis is focused on the intent-to-treat effect: we report outcomes for everyone assigned to contact (treatment) or control (no contact), regardless of whether the mobilization captain actually reached out to them. Given the novelty of the design, we do not know how widespread non-compliance will be. Because we have blocked on mobilization captain, we could potentially also report results for only the people included on the lists of “active” captains, subsetting to drop the entire lists of captains who drop out of contact and disappear from the project during the treatment period. If we are able to collect individual compliance data (i.e. did a specific person get contacted by their mobilization captain), we could try to do a 2SLS analysis, but we are unsure about whether we will be able to observe individual-level compliance.

Contact type Depending on group size and statistical power, we may conduct subgroup analyses by type of contact with the criminal legal system. If the other groups are too small for analysis, we may bin them for comparison to the largest (“eligible” people with past convictions), given that we expect the largest effects among that “eligible” group, or may simply report estimates among that largest subset if nothing else is feasible.

4 Appendix: Balance Table for Mailer Study

	Control	T2 (Ind. 1)	T3 (Ind. 2)	T4 (Social)	Joint F-test p-val
Black	0.242	-0.004 (0.003)	-0.003 (0.003)	-0.001 (0.002)	0.55
White	0.748	0.004 (0.003)	0.002 (0.003)	0.001 (0.003)	0.581
Hispanic	0.319	0.008 (0.004)	0.009 (0.003)	0.009 (0.003)	0.011
Male	0.757	0.003 (0.003)	0.003 (0.003)	0.001 (0.002)	0.393
Has Social Tie	0.621	-0.001 (0.004)	0.001 (0.003)	0.002 (0.003)	0.716
Has Ind. Address	0.783	0.001 (0.003)	0.003 (0.002)	0.004 (0.002)	0.348