

Restoring Rights, Restoring Trust:

Evidence that Reversing Felon Disenfranchisement Penalties Increases Both Trust and Cooperation with Government

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Abstract: Felon disenfranchisement laws restrict the voting rights of more than 6 million US Citizens. Beyond the effects on voter turnout and electoral outcomes, how do these laws affect individual-level attitudes and behaviors? This study implements two field experiments embedded within panel surveys conducted before and after statewide elections in Ohio and Virginia. The survey population is composed of US citizens with felony convictions who were once disenfranchised, but are now either eligible to vote, or to have their voting rights restored. Experimental treatments provide varying assistance with the restoration of voting rights and voter registration. Treated subjects report stronger trust in government and the criminal justice system, and an increased willingness to cooperate with law enforcement. The results suggest that reversing disenfranchisement causes citizens to increase their pro-democratic attitudes and behaviors - all of which are predictors of reduced recidivism.

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More than 6 million United States Citizens are currently denied the right to vote due to state laws that disenfranchise citizens who have been convicted of a felony. More than 75% of these disenfranchised citizens are not in prison, and more than half have completed all terms of parole and probation. The US is unique in the civil consequences it applies to its criminal population. As Ewald and Rottinghaus write “the United States is almost certainly the only country in the world that disenfranchises a significant number of people who are either no longer incarcerated or were never in prison at all” (2009; p 9-10).

Previous research on the effects of felony disenfranchisement laws has focused on the possible racialized motivations behind these laws (Behrens, Uggen, and Manza 2003; Ewald and Rottinghaus 2009), the disproportionate effects of these laws on non-white citizens (Manza and Uggen 2008; Uggen, Larson, and Shannon 2016), understanding the characteristics of public support and opposition to these laws (Pinaire, Heumann, and Bilotta 2002; Manza, Brooks, and Uggen 2004; Manza and Uggen 2008; Wilson, Owens, and Davis 2015; Shineman 2018c), the effects of these laws on voter registration and turnout (Meredith and Morse 2014; Meredith and Morse 2015; Gerber et al 2015; Hjalmarsson and Lopez 2010; Haselswerdt 2009; Miles 2004), and the effects of these laws on electoral outcomes (Uggen and Manza 2002; Manza and Uggen 2008; Burch 2011).

Given the high numbers of citizens being disenfranchised, and the likely partisan consequences of felon disenfranchisement laws, these laws are often decided on a party line. However, members from both sides have begun to find common ground on criminal justice reform. More than 95% of citizens incarcerated in state prisons will eventually be set free, but more than 75% of those released are re-arrested within 5 years (Bureau of Justice Statistics 2018). Given high rates of recidivism, lawmakers have paid increasing attention to policies and

programs that might increase the successful re-entry of formerly incarcerated citizens into society. The re-entry and recidivism literature suggest successful re-entry is best supported by targeting criminogenic characteristics – traits that are dynamic, and directly linked to recidivism – including anti-social attitudes and beliefs. Models of successful re-entry stress the importance of community ties, feeling vested, and prosocial attitudes (Gendreau, Little, and Goggin 1996).

Beyond their effects on voter turnout and electoral outcomes, how do felon disenfranchisement laws affect citizens at the individual level? Does the restoration of voting rights cause citizens to become more trusting, efficacious, and involved in their communities? Early models of participation argued that voting and participation elicit transformative effects, generating more socially-minded, informed, and active citizens (see Pateman 1970, for example). Empirical studies (Shineman 2018a; 2018b; 2018c) have demonstrated that increasing voter turnout increases political information, trust, and efficacy. Beyond the positive effects of voting itself, there are also a number of reasons to expect the restoration of voting rights to directly increase pro-social and pro-democratic attitudes – even if newly eligible citizens choose not to vote. Numerous theorists, policy-makers, and legal scholars have proposed that these relationships exist. Yet no empirical study has properly identified and estimated whether these proposed effects are taking place. This study seeks to fill that gap.

The following sections present the results from two field experiments embedded within panel surveys conducted before and after statewide elections in Ohio and Virginia. Both studies recruit a population of citizens with felony convictions who are either eligible to vote, or to have their voting rights restored. Treatments provide information about either the restoration of voting rights and/or encouragements and assistance with voting in the upcoming election. The design leverages the high degree of misinformation regarding the restoration of rights. By randomly

increasing awareness among some subjects that their right to vote has been restored, the experiment is able to isolate and estimate the effects of having one's votes restored.

The results suggest that both mobilization and the restoration of voting rights (without mobilization) cause subjects to increase their trust in government, and to perceive government as being more fair and representative. Additional analyses also show that restoring voting rights causes subjects to become more trusting of the police and the criminal justice system, and more willing to cooperate with law enforcement. All of these pro-social and pro-democratic attitudes are predictors of successful re-entry. Furthermore, as respect for the rule of law is a predictor for not breaking the law, the results suggest that restoring a citizen's voting rights causes that individual to increase the very types of attitudes and behaviors that make crime – and thus recidivism – less likely.

Section 1 details the theoretical expectations regarding the effects of restoring voting rights and mobilization on different dimensions of political trust, and explains why empirical studies have yet to demonstrate a causal relationship. Section 2 presents the design and results from the first experiment (Akron Ohio, November 2014), and Section 3 presents the design and results from the second experiment (Richmond Virginia, November 2017). Section 4 discusses the results as a whole.

Section 1: Theory and Hypotheses

Political trust is a prerequisite for a stable and efficient democracy. Putnam (1993) describes trust as one of two facets of social capital. Trust can make individuals more likely to invest in public goods (Hetherington 1999), and can increase support for higher levels of government involvement (Hetherington and Husser 2012; Hetherington 2005). The process of democratic government negotiates between divergent preferences, sometimes resulting in winners and

losers. If the government is perceived as legitimate and trustworthy, electoral losers are more likely to accept outcomes without resisting, enabling democracy to continue. Trust in government has been found to affect participation (Levi and Stoker 2000; Hooghe and Marien 2013), political preferences (Hetherington 2005; Abrajano and Alvarez 2010; Hetherington and Rudolph 2015), vote choice (Hooghe, Marien, and Pauwels 2011), and government efficiency (Hetherington and Rudolph 2015; Marien and Hooghe 2011).

Many scholars have endorsed civic engagement and reintegration as key elements of re-entry (Orr 1998; Bazemore, Gordon, and Stinchcomb 2004; Uggen, Manza, and Thompson 2006). Numerous international court decisions have also suggested that disenfranchisement is likely to hurt respect for democracy and the rule of law.¹ Restoring voting rights should increase political trust via two different pathways: (1) through the effect of disenfranchisement on voter turnout; and (2) through the restoration of rights alone.

First Pathway – The Effect of Political Participation on Political Trust: Classical democratic theory contends that political participation plays an “educative role” that promotes civic engagement and democratic values, and creates a self-reinforcing cycle of continuing participation (see Pateman 1970). Deliberative theorists (e.g. Fishkin 1991) argue that involvement in deliberation and decision-making transforms individuals, increasing civic-mindedness and political trust.

Engaging in the act of participation might increase political trust because the act of participation increases the perceived legitimacy of government. Scholars have argued that

¹ For example, see *Sauvé v Canada or Hirst v. United Kingdom*. For further examples, see Hill (2000), Hill (2009), and Ewald and Rottinghaus (2009).

² The complete materials for all experimental treatments can be viewed in the Appendix.

³ Of the 52 subjects who took the first survey, 41 also completed the second survey (78.9%). Attrition was comparable across treatment groups: completion rates were 75% (placebo), 80% (generic GOTV), and 81% (felon specific GOTV).

participating in a system makes an individual more likely to approve of the institutions, norms, and values associated with that regime, leading to an increase in satisfaction with the system as a whole (Finkel 1987; Thompson 1970). Research on public concepts of justice suggest that the notion of legitimacy is linked more to the perceived fairness of the decision-making process, as opposed to approval of particular outcomes (Gamson 1968; Grimes 2006; Tyler 2011; Marien 2011). Individuals who take part in the electoral decision-making process are more likely to view the outcomes as legitimate, increasing acquiescence to government authority (Finkel 1985; 1987; Dryzek 2000; Shineman 2018b). Engaging in electoral participation also increases external efficacy (Finkel 1985; 1987; Shineman 2018c) – which has been found to affect feelings that the system is responsive (Abramson and Aldrich 1982), political trust (Craig 1979), and diffuse system support (Iyengar 1980). If disenfranchisement laws are decreasing voter turnout, then restoring voting rights should not only increase voter turnout – restoring voting rights would also increase political trust and the perceived legitimacy of law.

Does Disenfranchisement Reduce Turnout? There is speculation and some disagreement as to whether – and how frequently – disenfranchised citizens would vote if their rights were restored (Meredith and Morse 2014; Meredith and Morse 2015; Manza and Uggen 2008; Gerber et al 2015; Hjalmarsson and Lopez 2010; Haselswerdt 2009; Miles 2004). Eligible citizens might not vote due to misinformation about their eligibility, lack of political interest, low levels of political socialization, low residential stability, or many other factors. Estimates of the effects of felon disenfranchisement laws on voter turnout are complicated due to the non-random variation of the laws, the decentralized nature of criminal and voting records, and by the potential spillover effects of disenfranchisement into communities. Several studies (Bowers and Preuhs 2009; Lee, Porter, and Comfort; Burch 2014; Uggen and Manza 2002; King and Ericson

2016) suggest that turnout among citizens *without* felony convictions is lower when those citizens live in communities with high rates of disenfranchisement. Similarly, other literature (Weaver and Lerman 2010; Lerman and Weaver 2014) finds that interaction with the carceral state causes citizens to develop an identity of a “carceral citizen”, depressing political interest and participation – even among those *not* convicted of crimes. Although the exact rate of expected participation among currently disenfranchised citizens (if their rights were restored) is unknown, the restoration of voting rights would increase turnout among at least some members of this population (Manza and Uggen 2008; Meredith and Morse 2015; Gerber et al 2015).

Second Pathway – The Effect of Voting Rights on Political Trust: The experience of being disenfranchised also likely causes a direct decrease in trust in government – regardless as to whether the citizen would vote if allowed. A disenfranchised citizen is deliberately disconnected from the democratic process, creating both a psychological stigma and a rational belief that the system is non-inclusive and non-responsive.

The re-entry literature emphasizes the importance of the self-identity of ex-offenders. Early studies on the ritual of criminal justice argue that the process of being convicted and sentenced changes a person’s identity: “a person starts out as one status (presumably a ‘person’ or ‘citizen’), and emerges at the other end as a different entity altogether – an ‘offender’ or ‘criminal’” (Garfinkel 1956, p 420). When ex-offenders see themselves in the role of a criminal, they are more likely to commit further crimes; whereas, if they are able to transition from the stigmatized identity of a criminal to the pro-social identity of an adult law-abiding citizen, they are more likely to successfully re-integrate into society post-release (Maruna 2001; Uggen, Manza, and Behrens 2004; Uggen, Manza, and Thompson 2006; Maruna 2011). As Frederick Douglass stated in his famous essay “What the Black Man Wants”,

“[Men] derive their conviction of their own possibilities largely from the estimate formed of them by others. If nothing is expected of a people, that people will find it difficult to contradict that expectation. By depriving us of suffrage, you affirm our incapacity to form intelligent judgments respecting public men and public measures; you declare before the world that we are unfit to exercise the elective franchise, and by this means lead us to undervalue ourselves, to put a low estimate upon ourselves, and to feel that we have no possibilities like other men”

Social bond theory (Hirschi 1969) argues that attachments to pro-social values, people, and institutions prevent citizens from committing crimes. The “belief” component of social bond theory argues that the more a person embraces the moral validity of laws, the more likely that person is to follow those laws. Applying this logic, one can derive a causal link where reversing disenfranchisement would increase cooperation with government, increase the respect for law, and thus decrease crime.

Hypotheses: Informing citizens that their voting rights have been restored (or assisting citizens whose rights have not yet been restored to request restoration of their voting rights) in advance of a statewide election will cause those individuals to:

(H1) ...Become more trusting of the government elected in that election. Knowing that a person is eligible to vote in an election makes that electoral process feel more fair and legitimate to that person, even if the eligible individual is not mobilized (or does not choose) to register and vote. Treated subjects should view the newly elected government as more trustworthy;

(H2) ...Become more trusting of political actors not elected in that election. Learning that one’s rights have been restored might also increase the perceived trustworthiness of government actors elected in other contests in which the individual is now eligible to vote. The entire political system might appear more fair, inclusive, and representative;

(H3) ...Increase their perceived degree of fairness and representativeness of government.

Questions to test this hypothesis were added in the second study in order to test a proposed

mechanism for why trust is increasing. Citizens might change their trust in government for a number of reasons – including their attitudes toward the individuals and parties who win a given election. Irrespective of the electoral outcomes, political trust might also increase if an individual’s perceptions regarding the fairness and representativeness of the system improve. Learning that one’s right to vote has been restored should make the system feel both more fair and representative;

(H4) ...Become more trusting and willing to cooperate with law enforcement. Trust in the police might increase when one’s right to vote is restored because the penalty imposed based on an arrest from a police officer is being reversed. As ex-offenders transform from an identity of “criminal” to an identity of “citizen”, they will see police as more of a protective force, rather than a threat. Furthermore, willingness to cooperate with law enforcement should increase if the laws being enforced by the police are seen as more legitimate;

(H5) ...Become more trusting of the (criminal) justice system. When one’s right to vote is restored, this might increase their trust in the actors perceived as responsible for taking away that right in the first place. Additionally, again as ex-offenders transition toward an identity of a citizen and active equal member of democratic society, the institutions in that democracy will feel more inclusive, fair, and legitimate;

(H6) ...Become more trusting of non-political actors. Finally, some scholars model trust as a more general feeling, which can spill over and between several dimensions. If the restoration of voting rights causes citizens to become more trusting of government actors, that increase in trust might spillover and also effect other dimensions of trust beyond the electoral realm.

(H1 – H6) Similarly, the combination of restoration along with assistance with voter registration and an encouragement to vote in the upcoming statewide election should also cause

individuals to increase their trust in all the dimensions specified above. The combined treatment should have effects due to both the restoration element (as discussed above), and because it increases the probability of being registered and voting. A comparison of effects between the two treatments can isolate whether information about the restoration of voting rights alone is sufficient to increase political trust, or if restoration is only effective when it is also combined with mobilization.

Existing Empirical Evidence: There is substantial anecdotal evidence where ex-offenders report that disenfranchisement makes them feel excluded, isolated, discouraged, and as if they are not treated like full citizens (Cardinale 2004; Manza and Uggen 2008; Uggen, Manza, and Behrens 2002; Pinkhard 2013; Miller and Spillane 2012; Miller and Agnich 2016). In these focus groups and interviews, citizens with felony convictions repeatedly describe feelings of frustration, exclusion, and anger about being disenfranchised, as well as sadness that they continue to be punished for their past, and are unable to participate in the future of government.

Despite the long-standing and numerous reasons to expect disenfranchisement to decrease pro-social and pro-democratic attitudes, there is no existing empirical evidence demonstrating a causal link. Two observational studies have found a correlation between disenfranchisement and recidivism. Hamilton-Smith et al (2012) find that the strictness of state disenfranchisement laws is correlated with higher recidivism. However, there are numerous differences between the states that allow ex-offenders to vote and those that do not – including demographic differences, as well as differences in other relevant policies relating to prison life, re-entry programs, and social services. Thus a raw comparison between states does not necessarily demonstrate that the disenfranchisement policies are what is causing observed differences. Another study finds that eligible ex-offenders who voted were half as likely to be re-

arrested as eligible ex-offenders who did not vote (Uggen and Manza 2002; Manza and Uggen 2008). However – as the authors themselves recognize – the design cannot account for the endogeneity between voting and crime. The characteristics of ex-offenders who vote are different from the characteristics of ex-offenders who do not vote, and the characteristics that tend to increase turnout (e.g residential stability, education, socioeconomic resources) are also predictors of lower recidivism.

As Uggen, Manza, and Thompson (2006, p 303) point out: “to date, there is little empirical research on civic reintegration and none that would establish a definitive causal relationship between civic participation and desistance from crime”. In a recent review of the literature on the effects of disenfranchisement on recidivism, Whittle (2018, p 520) concludes that though the topic is commonly discussed and researched, existing empirical research on the collateral effects of voter disenfranchisement “[does] not adequately measure the impact such sanctions have on recidivism.”

Empirical Strategy: In order to isolate the effect of restoring voting rights on political attitudes and behaviors (such as trust in government), one must identify an exogenous increase in the restoration of voting rights. Observational studies have been limited because the restoration of voting rights is not randomly assigned, and disenfranchisement is correlated with characteristics that also predict trust.

Felony disenfranchisement laws vary widely by state. Some states never disenfranchise, and citizens retain the right to vote (by mail) even while in prison. Other states disenfranchise citizens while they are in prison, but automatically restore voting rights either upon release from prison, or after the citizens complete their term of parole and/or probation. Some states vary the penalties by crime type. Others disenfranchise upon conviction with no automatic process of

restoration – requiring citizens to appeal for individual restorations of civil rights either from the Governor, or from a State Clemency Board. Given the variation (and frequent changes) in laws across states, there is a high degree of misinformation about the eligibility of citizens with felony convictions – both among the ex-offenders themselves (Drucker and Barreras 2005; Manza and Uggen 2006), and among elites (Ewald 2005; Allen 2011).

In order to isolate random increases in perceived voting rights, this study leverages the high degree of misinformation about eligibility, and randomly assigns some subjects to receive information about restorations that have already occurred. By generating exogenous increases in awareness of the restoration of voting rights, along with varying encouragements to participate, this study is uniquely able to estimate how the restoration of voting rights affects the attitudes and behaviors of those who were formerly disenfranchised.

The following sections present the results from two field experiments embedded within panel surveys conducted before and after statewide elections in Ohio and Virginia. Both experiments mobilize a population of citizens with felony convictions who have either had their voting rights restored – or are eligible to request the restoration of voting rights. The results estimate the effects of notifying citizens of their eligibility on multiple dimensions of political and non-political trust.

Section 2. Experiment #1: Akron, Ohio (November 2014 General Election)

Electoral Setting: A pilot study was designed to mobilize a population of citizens with felony convictions who might not know their voting rights had been restored. The pilot study embedded two mobilization treatments into a panel survey conducted before and after the November 2014 General Election – during which Ohio elected their Governor along with their Congressional

representatives in the US House of Representatives. In the state of Ohio, a citizen loses the right to vote when they are imprisoned for a felony conviction, and any existing voter registration is (supposed to be) purged. The right to vote is automatically restored after release, but citizens who were incarcerated must re-register. Many ex-offenders might not be aware that their right to vote was restored upon release.

Experimental Design: Subjects were invited to participate in a research study, including the completion of two surveys about 5-6 weeks apart. The study recruited a convenience sample, by advertising in places where potential subjects would be likely to see the announcement. All 52 subjects were US Citizens, living in Ohio, with at least one felony conviction – individuals whose right to vote had been restored. Subjects completed the first survey in person at the Akron Urban League, a non-partisan non-profit organization well-known for offering assistance with job training and community development. After completing the pre-election survey, subjects were randomly assigned to receive one of three different treatments in a private face-to-face setting.²

One treatment (Generic GOTV) provided subjects with a generic appeal to register and vote, along with assistance with voter registration, and a reminder of the upcoming election. In the generic treatment, the subject's criminal history was never mentioned during the delivery of treatment – though subjects knew they needed a felony to participate in the study. A second mobilization treatment (Felony-Specific GOTV) added additional felony-specific information. Subjects were provided with a handout displaying the variation in voting rights by state, informed that they did have the right to vote in Ohio, and received a scripted verbal appeal urging subjects to take advantage of their right to vote, to demonstrate that ex-offenders should

² The complete materials for all experimental treatments can be viewed in the Appendix.

have the right to vote, and to influence government and have their voice be heard. This information was intended to correct potential misinformation among subjects who might not know their right to vote had been restored, and to provide subjects with a personalized appeal for why ex-offenders should be involved in politics.

The placebo treatment borrowed and expanded the idea of a recycling placebo, which has been used frequently in the mobilization literature (e.g. Nickerson 2008). Subjects in the placebo group received a packet of information about how to reduce, reuse, and recycle, as well as a guide to composting, and a verbal appeal to take care of the planet. The placebo treatment was designed to provide subjects with an equivalent level of personalized attention and a positive pro-social message, without any reference to elections or voting.

The post-election survey began on November 5th, 2014 – one day after the November 2014 General Election. All subjects who completed both surveys were sent a \$25 postal money order through the mail.³

Treatment Effects on Voter Registration and Turnout: Both treatments generated increases in voter registration and turnout. Many treated subjects opted to fill out a voter registration form for some purpose (e.g. to register for the first time, to update their address, or to request an absentee ballot): 33.3% in the generic treatment, and 42.9% in the felony-specific treatment. Figure 1 displays the rate of voter registration before and after treatments were delivered, as well as voter turnout in the November 2014 election, by treatment group.

Figure 1. Treatment Effects on Voter Registration and Turnout

³ Of the 52 subjects who took the first survey, 41 also completed the second survey (78.9%). Attrition was comparable across treatment groups: completion rates were 75% (placebo), 80% (generic GOTV), and 81% (felon specific GOTV).

About 20% of treated subjects filled out a registration form because they said they did not think they were registered to vote – but about half of those subjects were already registered (and did not know it). Actual registrations increased by 13.3 percentage points (pp) in the Generic GOTV treatment ($p = 0.04$), and by 9.5 pp in the Felon GOTV treatment ($p = 0.05$), increasing by 11.1 pp overall across both treatments ($p = 0.04$). For the other 9% of the treated sample, although they were already registered – filling out the new form likely updated their address, so they will now receive election-related mail. Voter turnout was also higher in both treatment groups – by 20.4 pp in the generic treatment ($p = 0.03$), by 8.0 pp in the felon-specific treatment ($p = 0.11$), and by 13.2 pp overall ($p = 0.06$).

Results – Treatment Effects on Political Trust: Both the pre-treatment and the post-election survey asked subjects to indicate their level of trust regarding various institutions and groups of people. All estimates of trust are re-scaled from 0-100, with higher numbers indicating higher trust. All models are run through OLS regression, and include a set of covariates intended to further decrease noise and increase the accuracy and precision of the estimates (Pocock, Assmann, Enos, and Kasten 2002). The magnitude of the estimated effects of each GOTV treatment were remarkably similar. The figures below display estimated levels of pre and post-election trust for a typical subject in the sample (a nonwhite unemployed male with a high school diploma), comparing the control group to a pooled estimate of receiving either GOTV treatment.

The pilot study provides initial support for the hypotheses. The mobilization treatments significantly increased trust in the local government (+19.5, $p = 0.02$) and state government (+14.7, $p = 0.03$), and marginally increased trust in the federal government (+16.8, $p = 0.07$). Although trust in government in a general sense increased, trust in specific government actors (the President and the Congress) did not.

Figure 2. Treatment Effects on Trust in Government

Although subjects increased their trust in government, trust in other non-political actors (Fire Department, Media, Neighbors, and Co-workers) was not affected. This selective pattern of increases in trust lends support to the theory that mobilization affects political trust in particular, rather than overall trust in all types of actors.

Figure 3. Treatment Effects on Trust in Non-Political Actors

The only non-political groups whose trust level increased in response to mobilization were actors in the criminal justice system. Specifically, mobilization increased trust in the police (+36.8, $p = 0.00$) and the criminal courts (+30.1, $p = 0.01$), and trust in the Supreme Court was higher on average – though not statistically significant (+13.1, $p = 0.13$).

Figure 4. Effects of Pooled Mobilization Treatment on Trust in Police and Courts

Although the effects of mobilization on trust in the criminal justice system were not originally expected, the effects seem intuitive in retrospect. Being told that one's rights have been restored, and being encouraged to participate, is effectually reversing some of the penalty assigned to each subject by the criminal justice system. As these effects were discovered during exploratory analysis, but seem theoretically justifiable, a second study is designed to deliberately test this relationship as a predicted effect. The second experiment is also intended to test the robustness of all other observed effects, and to further identify the effects of restoring voting rights and mobilization on different dimensions of trust in government and the criminal justice system.

Limitations of Experiment #1: The felony-specific GOTV treatment was designed to engage the subject's criminal history in a positive light – to clarify misinformation, and to provide a positively-framed felony-specific appeal to participate. However, the felony-specific treatment might have accidentally introduced negative elements as well, both by raising the

saliency of the subject's status as an ex-offender (increasing stigma), and by possibly correcting misinformation in the opposite direction. Moreover, the generic GOTV treatment, which was intended to be devoid of references to the subject's criminal record, was likely associated with felon disenfranchisement anyway. However, the recruitment materials (and several survey questions) mentioned the subject's felony status. As such, even though the treatment did not specify that it was trying to mobilize ex-offenders, this connection might have been otherwise obvious to subjects. These patterns can help explain the similarity between the treatments – as the felony-specific treatment was weaker than intended, and the generic GOTV treatment was stronger than intended.

Given the promising results from the pilot study and the overlapping boundaries between the two initial mobilization treatments, a second experiment is warranted. The second experiment was designed to clearly differentiate between the effects of knowing one's rights were restored and the effects of being mobilized. The survey was also lengthened, to allow further exploration into the different dimensions of political trust, and how each is affected by the restoration of voting rights. Finally, the 2nd experiment identifies a population of citizens who are even more likely to be unaware that their voting rights have recently been restored.

3. Experimental #2: Richmond, Virginia (November 2017 Statewide Election)

Electoral Setting: The experiment was conducted during the November 2017 Virginia (VA) Statewide General Election, during which the state of Virginia elected its Governor, Attorney General, and all 100 members of the State Legislative Assembly. Virginia was an ideal setting to conduct the experiment because of recent changes in voter eligibility. In the state of Virginia, a citizen loses the right to vote when they are convicted of a felony. There is no automatic process

for the restoration of rights. In order to restore one's voting rights in Virginia, a citizen with a felony conviction must complete their full term of supervision (e.g. probation), and also receive a personalized exemption from the Governor of Virginia via Executive Order. Historically, VA Governors have waited for eligible applicants to request the restoration of their voting rights, and evaluated each application as it was submitted.

However, in 2016, VA Governor Terry McAuliffe decided to proactively restore voting rights to anyone who was eligible to have their rights restored by the Governor (e.g. all US Citizens with a felony conviction who had completed their probation). McAuliffe originally tried to restore voting rights to all 206,000 eligible citizens in a single executive order, signed April 22nd 2016. Newly eligible citizens began registering to vote immediately. However, the executive order was challenged by the Republican Party, and overturned by the VA Supreme Court in July 2016. The Court ruled (in a 4-3 split decision) that McAuliffe had the right to restore voting rights to all 206,000 citizens – but he was not allowed to do so in a single executive order. Instead, the Court ruled that McAuliffe would have to process 206,000 separate executive orders – one for each individual. The executive order was overturned, and all 13,000 new voter registrations that had been processed for citizens whose rights had been restored were canceled.

McAuliffe announced he would process the individual orders one-at-a-time, as quickly as possible – starting with those who had already tried to register. He restored voting rights to 70,000 citizens before the deadline to register and vote in the November 2016 National Presidential Election, and to more than 150,000 citizens before the deadline to register and vote in the November 2017 VA Statewide General Election. The Governor's office sent a certificate

of restoration to each citizen after their rights were restored, based on the last known address for each individual. However, ex-offenders are a particularly transient population. Because many of these last known addresses were no longer current, many citizens did not receive their notification letter, and were unaware that their rights had been restored.

Given these circumstances, the Virginia 2017 election presents an ideal setting for estimating the effects of the restoration of voting rights. Given the strict state laws which disenfranchised all citizens with felony convictions after their release, and the former policy from the Governor's Office which limited the restoration of voting rights to a small number of cases, citizens with felonies in Virginia are highly likely to know that they were disenfranchised (whereas in Ohio some citizens might not have even been aware that they were not allowed to vote while in prison). More than 150,000 citizens had their voting rights restored within the last year, with more than half of these restorations occurring after the deadline to register and vote in the previous high-profile 2016 Presidential Election. Many of these newly-eligible voters did not know that their rights had been restored. Given the initial restoration order being reversed by the VA Supreme Court, the 13,000 new registrations that were processed and then canceled, the gradual process through which individual restorations were processed over time, and the inconsistency with which citizens received notification of the restoration of their rights – there was a high degree of confusion, misinformation, and suspicion regarding the restoration process. The experiment intentionally leverages this misinformation in order to capture the experience of having one's rights restored. Treatments randomly assign some subjects to receive information about restorations that might have already happened.

Experimental Design: The experiment embedded two mobilization treatments into a panel survey conducted before and after the November 2017 Virginia Statewide Gubernatorial

Election. Recruited subjects (n = 98) were US Citizens with at least one felony conviction, currently living in Virginia, who had completed their term of supervision – individuals who are eligible to have their voting rights restored by the Governor. The first survey took place about a month before the November 2017 election. A convenience sample was recruited by advertising the study in places potential subjects would be likely to see the invitation to participate.⁴ All subjects completed the first survey in person in a private office suite located in downtown Richmond, VA. After completing the first survey, subjects were randomly assigned to receive one of three treatments. All treatments were delivered one-on-one in a private face-to-face setting, after subjects completed the first survey.⁵ In order to avoid accidentally activating the stigma-ridden identity of a criminal, all treatments in the second experiment were tailored to avoid specific references to the subject's status as an ex-offender, and instead to focus on the subject's completion of probation as a success story (rather than their original conviction as a stigma). Subjects were referred to as "citizens", none of the pre-treatment survey questions asked about criminal history or behavior, and none of the treatments used the word "felon" as a noun.⁶

(Treatment #1) Restoration Only: Subjects were provided with information about the Governor's current policy and initiative regarding the restoration of voting rights. The researcher offered to look up the subject's restoration status (which only takes about 15 seconds through a government website), and did so if the subject agreed. If the subject agreed to let the researcher look up their voting rights, the researcher would then either confirm the date on which the

⁴ Recruitment materials were placed online and in print in a variety of newspapers and job search websites, and were also distributed through flyers posted at outdoor street festivals, public transportation hubs, bodegas, and in offices and organizations that provided public services. Recruitment materials did not mention voting rights or any other political content.

⁵ The complete materials for all experimental treatments can be viewed in the Appendix

⁶ Subjects completed a short "eligibility questionnaire" before being checked in, which confirmed that they were a US Citizen, had at least one felony conviction, and had completed their probation.

subject's voting rights had been restored, or – if the subject's rights had not yet been restored – the researcher would offer to assist the subject in submitting a request to restore their voting rights. Such requests were typically processed within about 3 weeks.

(Treatment #2) Restoration + Mobilization: Subjects were provided with all elements of the *Restoration Only* Treatment. Additionally, subjects were informed that a statewide general election was coming up on November 7th, and were provided with information about how to register, locate their polling place, and look up other information about the election. The researcher also offered to look up the subject's registration status (which only takes about 15 seconds through a government website), and did so only if the subject agreed. If the subject was not registered, the researcher offered to assist the subject in registering to vote.

Placebo Treatment: The “Placebo” treatment provided subjects with an intensive personalized appeal to volunteer in their communities, along with a list of 14 upcoming volunteer opportunities in the area.⁷ The goal of the placebo treatment was to mirror the level of personal contact and connection provided in the two voting rights treatments, and to include a similar pro-participatory message encouraging community involvement, without any reference to voting rights or the upcoming election.⁸

The post-election survey began two days after the election, and could be completed by phone, online, or through the postal mail. Attrition was extremely low: of the 98 subjects who

⁷ All volunteer opportunities were for events taking place between October 14th (the voter registration deadline) and November 7th (Election Day)

⁸ The placebo treatment was modified from the original recycling template in order to make it feel more believable that the placebo message was the intended purpose of the study. The pre-treatment survey also ended with a series of questions about volunteering, to further increase the naturalness of the placebo treatment. A pro-volunteer message also better mimics the pro-participatory content of the treatments, allowing direct isolation of the voting rights content.

completed the first survey, 93 also completed the 2nd survey. All subjects who completed both surveys were provided with a \$25 gift card to a location of their choice.

Treatment Effects on Restoration of Civil Rights, Voter Registration, and Voter Turnout: During the delivery of the treatments, the researcher recorded whether the subject thought their voting rights had been restored, whether they allowed the researcher to look up their restoration status, whether their rights had been restored, and if not – whether they completed a request to have their rights restored. About 65% of subjects said they thought their rights has already been restored, and about 43% of treated subjects allowed the researcher to look up the status of their voting rights.⁹ Among all treated subjects, 21.4% learned that their right to vote had been restored during the treatment delivery (22.2% in the restoration treatment, and 20.7% in the treatment combining restoration with mobilization). Just over 16% of subjects verified that their right to vote had *not* yet been restored, and then also filled out a request to have their rights restored during the treatment (18.5% and 13.8% across each treatment). Lastly, 20.7% of subjects who received the mobilization treatment filled out a voter registration form – 17.2% to register for the first time, and 3.5% to update their address.¹⁰

In addition to these on the spot estimates, official data regarding whether each subject had their rights restored, was registered to vote, and voted was attained through official government sources. Figure 5 displays the percent of subjects whose right to vote had been restored in each treatment group, before and after the treatment.¹¹ As the baseline rates varied

⁹ Those that refused typically either insisted they knew their rights were already restored, or expressed disinterest in voting or distrust about the restoration process.

¹⁰ By default, all these values were zero in the placebo treatment, as those subjects were not offered these options.

¹¹ Post-treatment restoration rates were measured in January 2018. The original intent specified in the pre-analysis plan was to estimate post-treatment restorations in November. Requests for a new restoration of voting rights were typically processed in about three weeks. By that standard,

substantially between treatments, the most accurate estimate of the effect estimates the effect of each treatment on *new* restorations (or the increase in restorations). In comparison to the placebo – where one new subject had their rights restored during this time (+2.7), the restoration treatment increased restorations by 14.8 percentage points ($p = 0.04$). The treatment combining restoration and mobilization increased restorations by 6.9 pp ($p = 0.212$). Compared to the placebo, the pooled effects of both treatments increased restorations by 10.7 pp ($p = 0.08$).

Figure 5. Percent of Subjects Whose Voting Rights Have Been Restored, by Treatment

Figure 6 displays the percent of subjects who were registered to vote in each treatment group, before and after the treatment. The voter registration rate was also unequal by chance before the treatments were administered, so the analysis estimates the treatment effects on new registrations. No new subjects registered to vote in the placebo group. Though subjects in the restoration only treatment were not offered assistance with voter registration, 14.8 percent of the sample registered on their own ($p = 0.01$) – though notably only one of these subjects registered before the deadline to vote in the November 2017 election. Restoration with mobilization increased voter registrations by 13.8 pp ($p = 0.01$) – with one subject registering after the deadline had passed. Overall, the treatments increased *new* registrations by 14.3 pp ($p = 0.01$).

Figure 6. Percent of Subjects Who Are Registered to Vote, By Treatment

any subject who requested their rights be restored during the treatment would have received their restoration by the second survey, and surely by the end of November. However, the Governor’s Office decided to delay processing of new restoration requests during the three weeks just before the election. When asked, they replied that they did not want to confuse people by restoring voting rights in between the voter registration deadline and the election. They worried that citizens who received a restoration letter after the voter registration deadline might then try to register and vote in the upcoming election, and would be confused as to why they were not eligible – potentially causing them to not believe their rights had really been restored. To prevent this possible confusion, restorations were temporarily paused. This unexpected delay mean that subjects who requested new restorations completed the second survey before their rights were restored. In order to better estimate the effect of the treatments on the total number of people whose rights have been restored, voting rights were verified in January instead.

Finally, Figure 7 displays the percent of subjects who voted in the November 2017 election, by treatment group. The results are inconsistent. Although the rate of restorations and voter registration across the three treatment groups was unequal before the treatments were administered, the rate of voter turnout was fairly comparable across treatment groups in previous elections. However, the pattern in 2017 is inconsistent. Not only did restoration alone not increase turnout, a 2-tailed model would suggest it actually decreased turnout. This effect is most likely due to chance. We would not expect substantial turnout effects from a treatment that did not help subjects register. However, a seemingly negative effect is puzzling. The negative effect is likely caused by random error – but it’s worth noting that perhaps some element of only providing restoration information had an unexpected demobilizing effect. When restoration and mobilization were combined, the effect on turnout was positive (+9.7).

Figure 7. Validated Voter Turnout in November 2017 Election, by Treatment

Overall, both treatments clearly caused increases in knowledge about restored voting rights, new restorations, and new registrations – although the effects on turnout are difficult to estimate.

Methods for Analysis: Both the pre and post-election surveys asked 26 different questions estimating multiple dimensions of trust in a variety of government and non-government actors. The analysis estimates the effects of each treatment (as well as the pooled effects of both treatments) on each estimate of political (and non-political) trust. The trust estimates are presented individually, and clustered into 6 categories: (1) Trust in the government actors elected in the November 2017 Election;¹² (2) Trust in government actors not associated

¹² Trust in... “The newly elected Governor of Virginia, who will take office in January 2018”; “The newly elected Virginia State General Assembly, that will take office in January 2018”; “The VA State Government as a whole”

with the November 2017 Election;¹³ (3) Perceptions regarding the fairness and representativeness of government;¹⁴ (4) Trust and cooperation with the police;¹⁵ (5) Trust in the (Criminal) Justice System;¹⁶ and (6) Trust in Non-Political Actors.¹⁷ To easily facilitate comparisons of magnitude, all estimates of trust are re-scaled to range from 0-100, with higher numbers indicating higher levels of trust.

The analysis estimates the effects of each treatment (as well as the pooled effects of both treatments) on each estimate of political (and non-political) trust. As in the previous experiment, all models are run including a set of covariates that are likely to affect trust,¹⁸ as well as the pre-treatment value of each dependent variable¹⁹ – in order to further decrease noise and increase the accuracy and precision of the estimates (Pocock, Assmann, Enos, and Kasten 2002). Given the directional nature of all hypotheses, all models are evaluated with a 1-tailed (directional) significance test within a 95% confidence interval. Effects within a 90% confidence interval are noted as marginally significant.

¹³ Trust in... “The previously elected Governor of Virginia, who is currently in office”; “The previously elected Virginia State General Assembly, who is currently in office”; “The President of the United States”; “The US Congress”; “The US Federal Government as a Whole”

¹⁴ “I generally trust the [Virginia State Government / US Federal Government / Richmond City Government] to make decisions in a fair way”; “The [Virginia State Government / US Federal Government / Richmond City Government] represents people like me”

¹⁵ “Trust in: The Police”; “If I was in danger, I would feel comfortable calling the police for help”; “If I saw a crime being committed that harmed someone else, I would feel comfortable calling the police for help”

¹⁶ Trust in... “The Criminal Courts”; “The [Virginia State / US] Supreme Court”

¹⁷ Trust in... “The Fire Department”; “The Media / Press”; “Your Neighbors”; “Your Co-Workers”; “Citizens with Felony Convictions”

¹⁸ As specified in the pre-analysis plan, covariates include age, age², gender, race, education, employment, and number of years at current address.

¹⁹ In the interests of space (and as specified in the pre-analysis plan), the body of the manuscript only presents the estimates from models including covariates. Coefficients for all models are included in the Appendix. Models estimating the change (pre v. post treatment) are also presented in the Appendix. The results are robust across models, and the substantive interpretation of the results is the same across all specifications.

Results – Treatment Effects on Political Trust: Figure 8 displays the estimated effect of each treatment, along with 95% confidence interval bars for each estimated effect. Model A estimates the average effects of the “Restoration Only” treatment. Model B estimates the average effects of the treatment providing both restoration and a “get out the vote” (mobilization) message (Restoration + GOTV). Model C pools both treatments together, and estimates the average effects of receiving either treatment, compared to receiving the placebo. Each interval bar indicates the lower-level cutoff for a 95% confidence interval for a directional (one-tailed) hypothesis test: each model estimates that the effect of receiving that treatment is at least as high as the lower bar. The upper bar for a one-tailed hypothesis test is technically unbound; hence, the upper end of each interval bar points toward the maximum value. The point predictions of each model are also specified.

Figure 8: Estimated Effects of Restoration and GOTV Treatments on 26 Estimates of Political Trust

As both treatments performed comparably (in all but one case), the discussion focuses on the pooled results. To place the magnitude of each estimate within the context of varying baseline values, the figures below display predicted values for pre and post-treatment levels of political trust for a typical subject (a nonwhite male of average age and length of residency, with some college education, and a full-time job), comparing the predicted value in the placebo group to the predicted value among subjects who received either treatment (Model C from Figure 8).

The first hypothesis predicts that both treatments will cause subjects to become more trusting of the government elected in that election. Models 1A–1C find strong support for these hypotheses. The treatments generated a significant increase in trust in the newly elected Governor (+15.1) the newly elected State Assembly (+15.5), and the VA government overall (+16.9).

**Figure 9. Predicted Values of Trust in the Government
Elected in November 2017 Election, by Treatment Group**

The second hypothesis predicts that each treatment will increase trust in political actors who were not elected in the November 2017 Virginia statewide election. Overall, this hypothesis is not supported. Although trust in the old Governor (McAuliffe) increases significantly (+18.2), this is likely because subjects learned that McAuliffe was the one who restored their voting rights. Trust in the previous State Assembly, the local government, the President, the US Congress, and the US Federal Government are not affected.

**Figure 10. Predicted Values of Trust in Government
NOT Elected in November 2017 Election, by Treatment Group**

The third hypothesis targets a likely mechanism causing trust in government to increase, and predicts that the treatments will increase the perceived fairness and representativeness of government. There is strong support for this hypothesis. The treatments caused a significant increase in the perceived fairness (+24.6) and representativeness (+19.8) of the VA State Government. Perceived fairness of the federal government also increased (+14.9), as did the perceived representativeness of the local government (+12.2). The remaining two categories increased on average, but were not within a 95% confidence interval. Overall, the pattern of results reflects those observed in the first two hypotheses. Treated subjects increased their evaluations of the government that was elected during the course of the experiment, but experienced fewer increases in evaluations of government at the local or federal level. If the experiment were conducted during a local (or federal) contest, we might expect to see similar patterns focused on those levels of government. It is also possible that subjects will later increase their evaluations of the local and federal government, after they experience their first local / federal election as an eligible voter.

**Figure 11. Predicted Values of Perceived Fairness
and Representativeness of Government, by Treatment Group**

The fourth hypothesis predicts that the treatments will increase trust and cooperation with the police, and there is strong support for this hypothesis. The treatments significantly increased trust in the police (+19.2). Questions also asked subjects if they would be willing to contact the police for help to protect themselves or to protect someone else. Reported willingness to cooperate with the police significantly increased when evaluating oneself (+17.6), and also when evaluating a scenario where one was protecting another person (+11.0).

Figure 12. Predicted Values of Trust and Cooperation with Police, by Treatment Group

The fifth hypothesis predicts that the treatments will increase trust in the (criminal) justice system. As with all previous models, Figure 13 displays the predicted values for a typical subject in the placebo group versus a typical subject receiving either restoration treatment. The pooled effects are positive. Trust in the criminal courts increases (+12.0), as does trust in the US Supreme Court (+14.6) and the VA State Supreme Court (+14.0).

**Figure 13. Predicted Values of Trust in (Criminal) Justice System, by Treatment Group
(Placebo v. Pooled Effects)**

However – unlike all the other models where the two treatments performed remarkably similar to each other, the effects of the “Restoration Only” treatment were different from the effects of the “Restoration and GOTV” treatment in the area of criminal justice. Because of this difference, reporting pooled outcomes is less accurate. Figures 14 and 15 display the predicted values for typical subjects in the placebo group compared to typical subjects in each treatment group individually. Although the pooled effects are positive, we can see that the pooled effect is driven – in this case – by the effect among subjects who received the Restoration and GOTV treatment.

**Figure 14. Predicted Values of Trust in (Criminal) Justice System, by Treatment Group
(Placebo v. Restoration Only)**

Figure 15. Predicted Values of Trust in (Criminal) Justice System, by Treatment Group (Placebo v. Restoration + GOTV)

Finally, the sixth hypothesis predicts that both treatments will increase trust in non-political actors. This hypothesis is not supported. Neither treatment caused a significant increase in trust in the fire department, media, one's neighbors, one's co-workers, or the subject's peer group – citizens with felony convictions. Again – as in the Ohio experiment – the selective pattern of increases in trust serves to bolster support that restoring voting rights causes increases in trust in the actors and institutions associated with the denial of rights along with the actors involved in the election in which citizens are now eligible to vote.

Figure 16. Predicted Values of Trust in Non-Political Actors, by Treatment Group

Section 4. Discussion of Results

Overall, this experiment provides strong evidence that restoring voting rights to citizens with felony convictions will cause those citizens to develop stronger levels of trust in government, as well as stronger trust and cooperation with the police and (criminal) justice system more generally speaking.

Previous research has demonstrated that mobilizing a sample of citizens without felony convictions to participate in politics causes trust in government to increase (Shineman 2018b). The current study adds further confidence to this result, by replicating this previous finding with two different subject populations in two different settings. As such, the current study adds support to the overall theory that increasing participation increases trust. Identifying a strategy for increasing trust among ex-offenders also offers new normative implications for this effect.

This study also makes two additional novel contributions. First, both experiments find evidence that mobilizing citizens with felony convictions to participate in politics causes them to

become more trusting of not just the government actors elected in that contest – but to also become more trusting of the police and the (criminal) justice system more generally speaking. These results have striking implications, as the ex-offender population has notoriously low levels of trust in the criminal justice system.

The final novel contribution of this study is providing evidence that the restoration of voting rights alone (without mobilization) increases trust in both government and law enforcement. Subjects who received only information about the restoration of their voting rights – with no assistance with voter registration or mention of the upcoming election – also became significantly more trusting of the newly elected Virginia State Government, more trusting of the police, more willing to cooperate with law enforcement, and they perceived government as more fair and representative. These results suggest that felon disenfranchisement laws are directly decreasing trust through the act of disenfranchisement – not just through their effect on turnout. Some argue that the effects of disenfranchisement are less severe because of the low voter turnout among eligible ex-offenders (e.g. Miles 2004). While other estimates of the electoral power of the ex-offender population refute this critique (e.g. Manza and Uggen 2008), the current study offers an additional response. Regardless as to whether citizens choose to exercise their voting rights, the act of restoring rights alone causes citizens to feel more trusting of the democratic system.

On the Duration and Magnitude of the Effects: The treatment effects on trust are estimated in the short-term, about 2-7 days after the election took place (and 4-6 weeks after treatments were delivered). Subjects were not contacted again after the post-election survey, so there is no formal estimate regarding the long-term duration of these effects. While the initial magnitude of the effect size is likely to decrease in the weeks and months following the election

– as many short-term effects have been found to do – there is also reason to believe that the positive effects of the treatments will be both enduring and reinforcing over time. Subjects whose rights were restored will always continue to know that their rights have been restored, and the feelings generated from this knowledge will likely be re-activated during future elections and political discussions. As such, although the magnitude of the treatment effects is likely to fall in the short-term, these effects should not diminish completely, and should also rise again during political events. Michelitch and Utych (2018) find that the strength of partisan identity rises and falls with electoral cycles. Similarly, the effects of being eligible to vote will likely rise and fall with electoral cycles – with a gradual increase over time, as the identity as a citizen and represented member of society continues to be reinforced.

Furthermore, as we know that participation and turnout are habit forming (Gerber, Green, and Shachar 2003; Meredith 2009; Davenport et al. 2010), the effects on voter registration and voter turnout are likely to persist, continuing to cause subjects to feel more included and represented in the political system. As noted in the analysis of the Virginia experiment, treated subjects continued to increase their rate of both new restorations of civil rights and new voter registrations after the 2nd survey was conducted. These delayed effects suggest that not only will registration and turnout effects continue to persist where they were observed, but we might also see other new subjects register and vote in response to the treatments in future elections.

There are also a few reasons to suspect that observed treatment effects are an underestimate of the true full effect of having one's votes restored. Firstly, when asked post-treatment, 70.3% of subjects in the control group said they thought their voting rights had been restored. Similarly, about 65% of subjects in the treatment groups reported that they thought their voting rights had been restored before they received the treatment. Several subjects described receiving

the official restoration order from the Governor, talked about how they had it framed, or said they had been excited they were able to vote in the most recent Presidential election. These subjects had already experienced the positive effects of learning their rights had been restored – and sometimes also the positive effects of being registered and voting. As such, the experiment only estimates the full increases in trust among the subset of the subjects who received new information during the treatments (about 21% who learned their rights had been restored, 16% whose rights were not yet restored but requested restoration, and about 20% - in the mobilization treatment only – who either registered to vote or updated their address for the first time). Additionally, among the 21% of treated subjects whose rights had not yet been restored that requested the restoration of voting rights – although voting rights were eventually restored to nearly all of these subjects, all of the restorations were processed on or after November 15th (after the 2nd survey had been completed). Thus although these subjects had been told that their rights *would* be restored, they had not received confirmation that this restoration had actually been processed until after they completed the second survey. For example, one subject sent a text message on November 30th (weeks after he completed the 2nd survey), after finding out that his rights had been restored, saying: “Thank you guys very so much..Now I feel as a American citizens.. [sic] Thanks again..” It is likely that this subject’s trust levels would have been higher, had he been surveyed after the restoration of his voting rights had been processed.

Generalizability: One might naturally ask if the effects demonstrated in these studies can be generalized to all citizens with felony convictions who might have their rights restored. The replication of the effects across two different samples in different states is promising, and provides robustness to the overall argument. All four treatments generated significant increases in trust in government and the police. Future studies can verify if these effects persist in all

states, and across all types of citizens. Future work can replicate this template provided in this study with different samples in other elections and other states – in order to test the robustness of the effects. Furthermore, there might be interesting variations in the treatment effects across types of citizens (e.g. by age, by gender, by race). Although the sample sizes in these two experiments are not large enough to allow estimation of heterogeneous treatment effects, future studies could increase the sample size in order to estimate which types of citizens experience the highest increases in trust when their rights are restored.

CONCLUSION

Felon disenfranchisement laws restrict the voting rights of over 6 million US Citizens, altering electoral outcomes, and generating unknown effects at the individual level. Scholars and policymakers have long suspected that disenfranchisement causes a sense of otherness, ripples into disengagement beyond the electoral arena, and can affect overall political efficacy and trust in government, as well as recidivism. Though these theories are long-standing, the best existing evidence in support of these effects was based on observational data highly subject to endogeneity. This study presents the first empirical evidence of a causal relationship between the restoration of voting rights and political trust.

The potential policy implications of this study are widespread. A separate paper (Shineman 2018d) finds that attitudes toward felon disenfranchisement are largely racially motivated. However, that study also finds that public opposition to felon voting rights would be reduced if evidence suggests that allowing citizens with felony convictions to participate would help them reintegrate into society, and also if restoration was found to decrease recidivism. Thus the results of this study might influence public opinion on the topic. Several states are currently

debating reforms in felon disenfranchisement policies – including the state of Florida, which is about to vote on a ballot measure that would provide automatic restoration of voting rights when citizens complete their probation – a change that would restore voting rights to more than one million US Citizens who are currently disenfranchised. Although felon disenfranchisement has traditionally been a partisan issue, bipartisan alliances have recently found common ground on concerns regarding successful re-entry programs. Given ex-offenders’ historically low trust in government and police – and the links between pro-social attitudes and recidivism – demonstrating that the restoration of voting rights increases political trust could have a wide impact on our discussion of disenfranchisement policies nationwide.

Figure 1. Treatment Effects on Voter Registration and Turnout

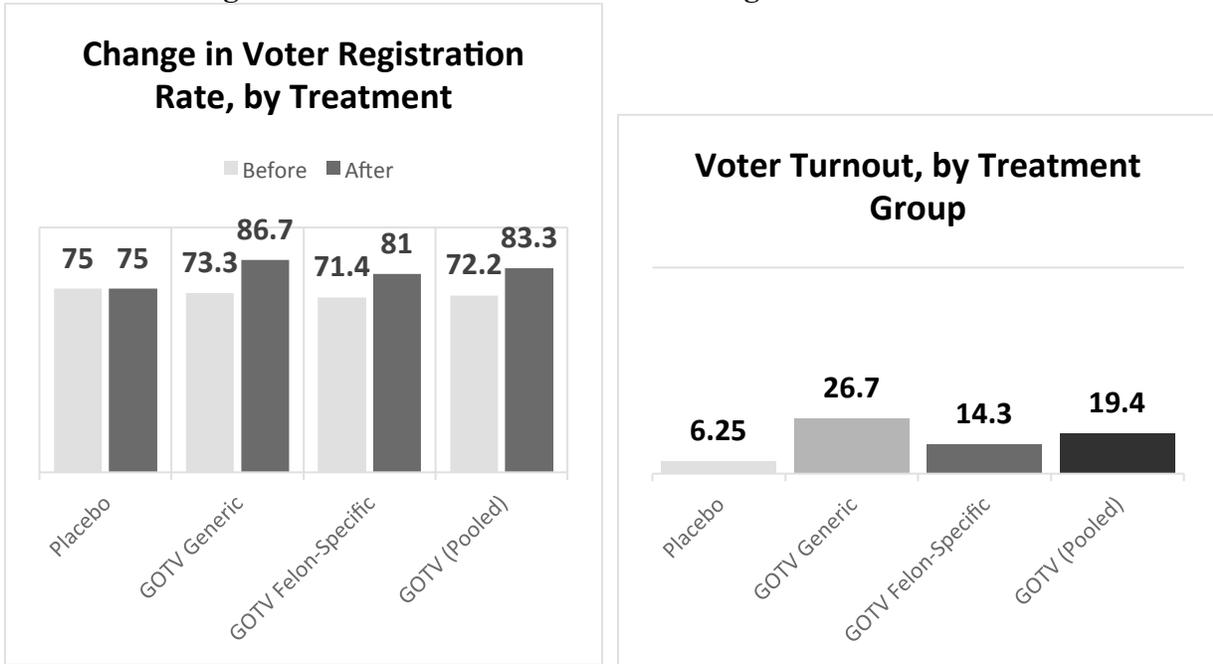


Figure 2. Treatment Effects on Trust in Government

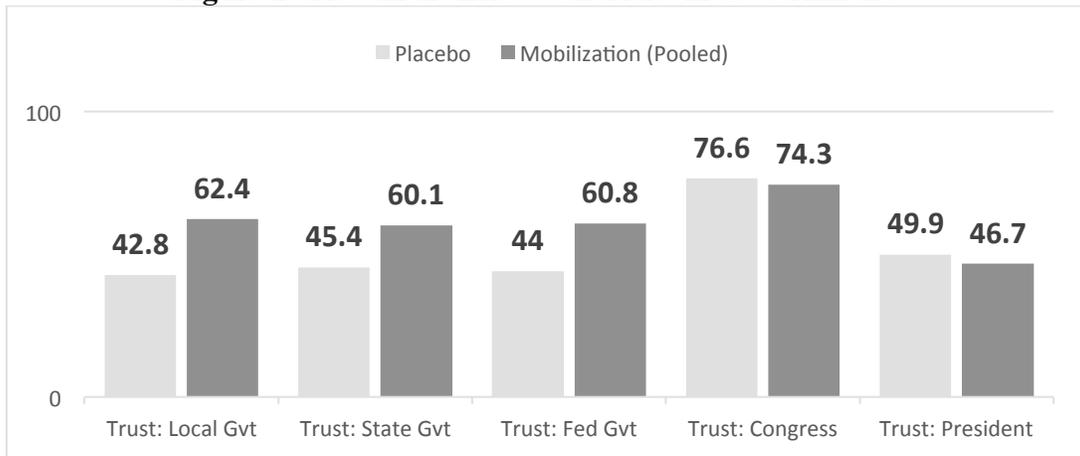


Figure 3. Treatment Effects on Trust in Non-Political Actors

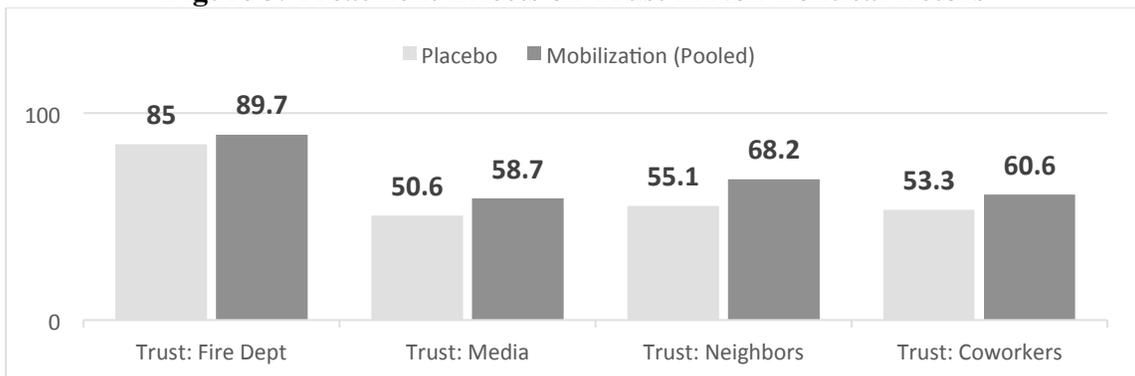


Figure 4. Effects of Pooled Mobilization Treatment on Trust in Police and Courts

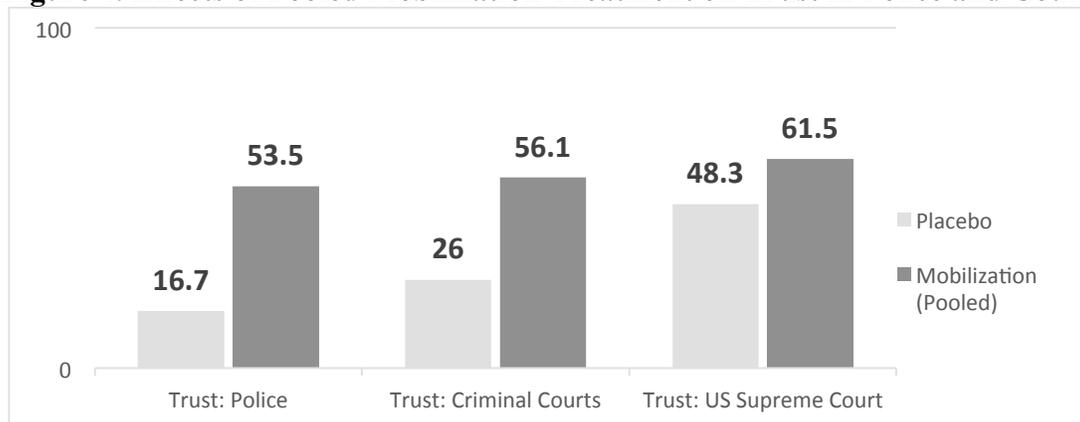


Figure 5. Percent of Subjects Whose Voting Rights Have Been Restored, by Treatment

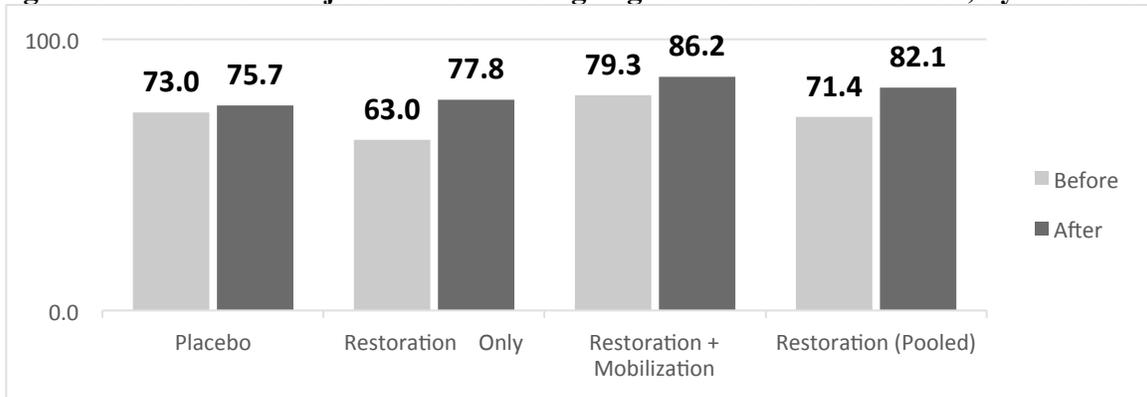


Figure 6. Percent of Subjects Who Are Registered to Vote, By Treatment

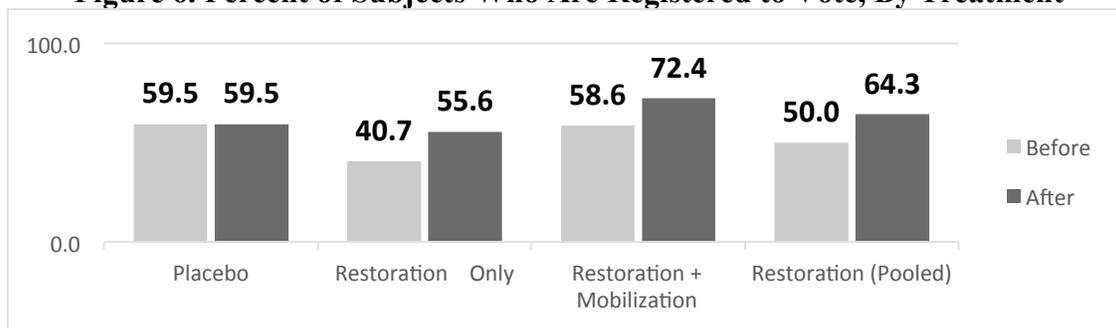
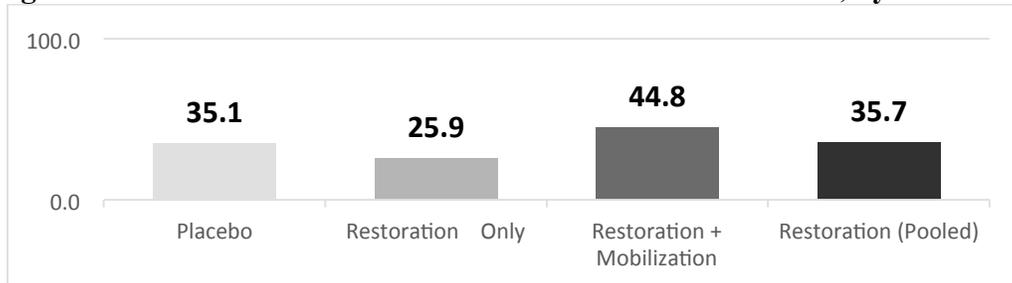


Figure 7. Validated Voter Turnout in November 2017 Election, by Treatment



Estimated Effects of Restoration and GOTV Treatments on 26 Estimates of Trust

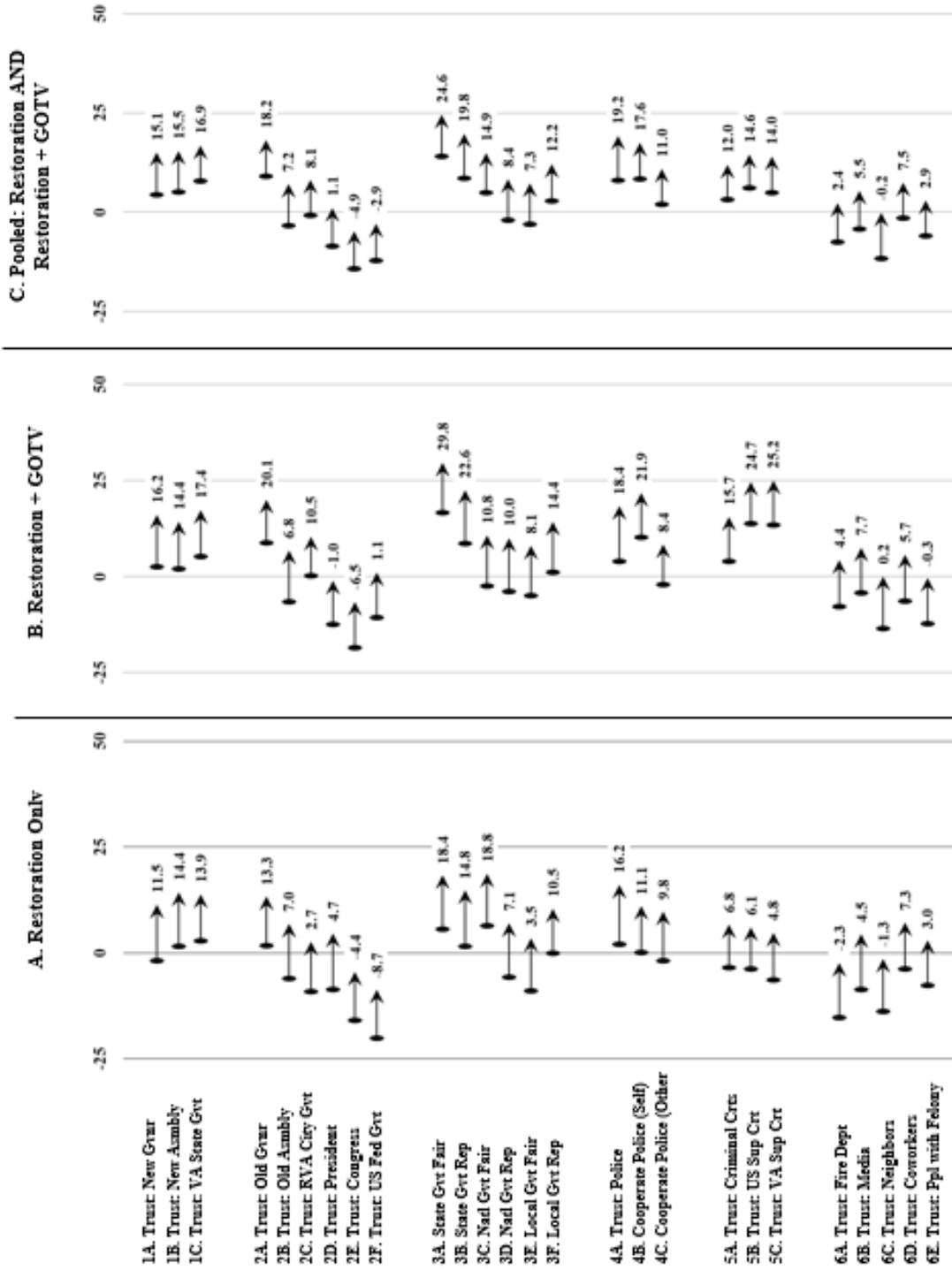


Figure 9. Predicted Values of Trust in the Government Elected in November 2017 Election, by Treatment Group

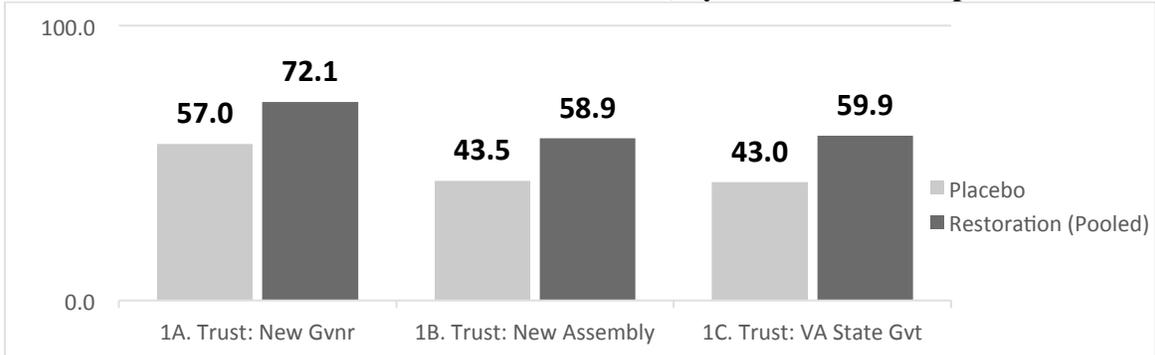


Figure 10. Predicted Values of Trust in Government NOT Elected in November 2017 Election, by Treatment Group

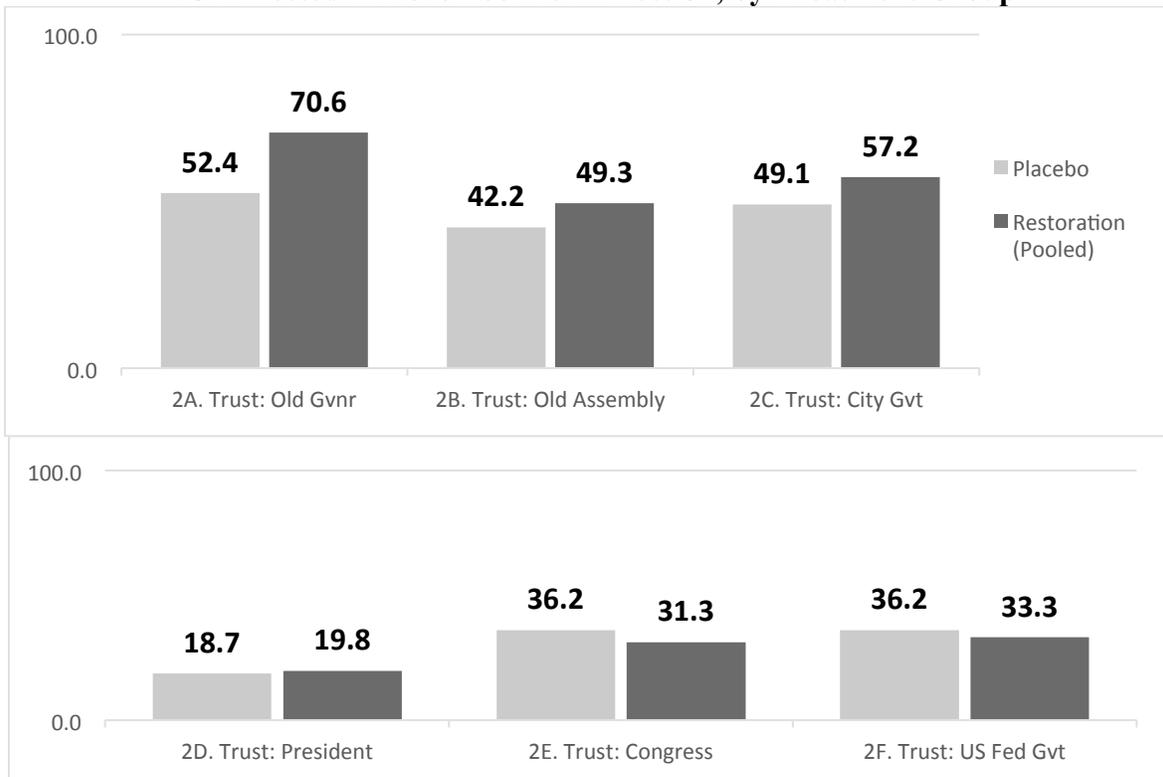


Figure 11. Predicted Values of Perceived Fairness and Representativeness of Government, by Treatment Group

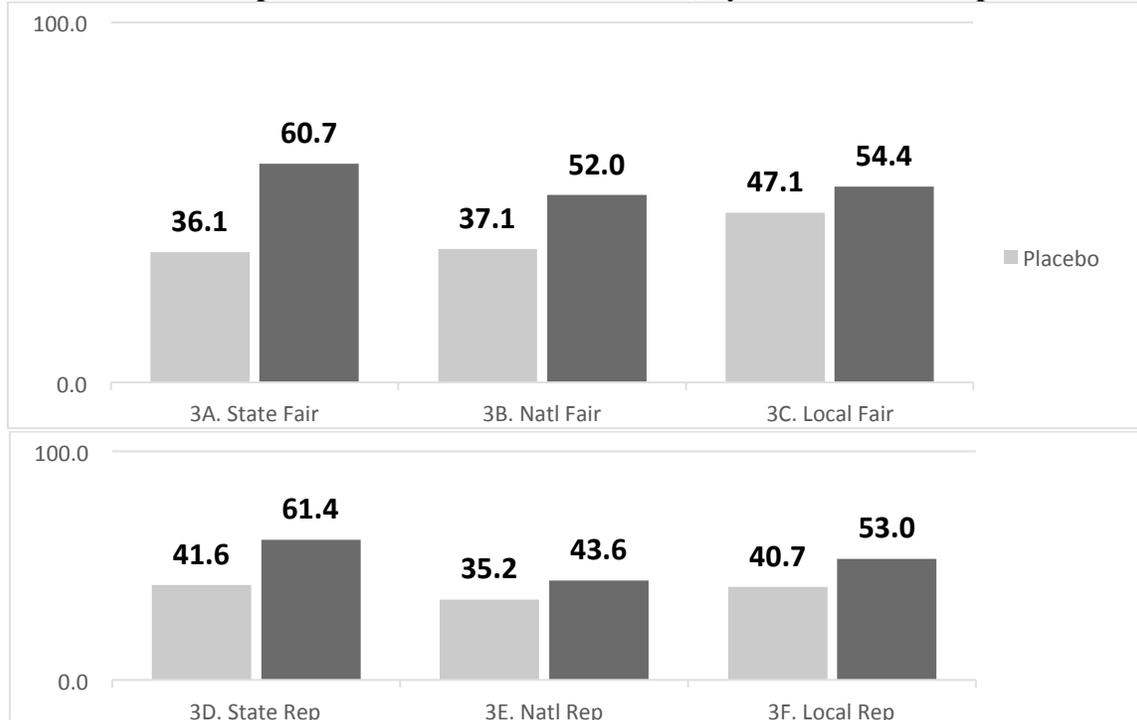


Figure 12. Predicted Values of Trust and Cooperation with Police, by Treatment Group

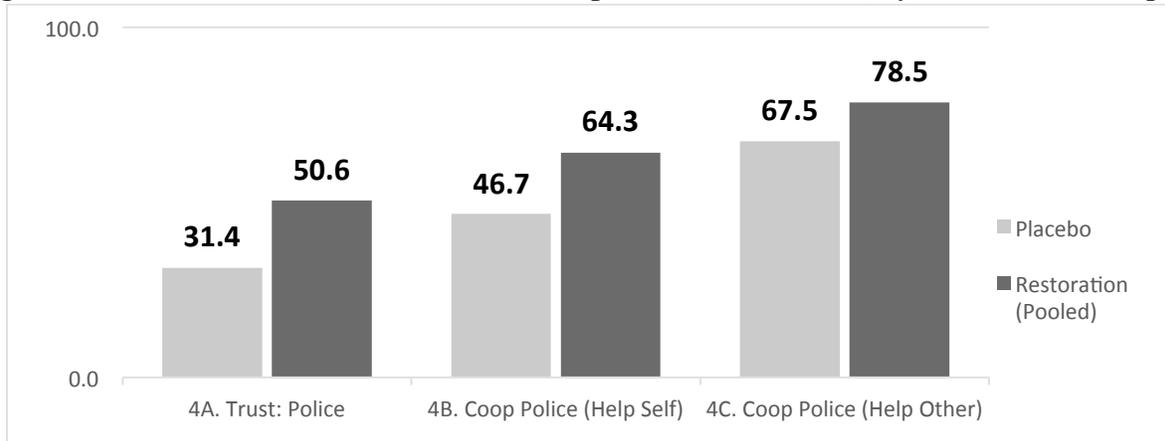


Figure 13. Predicted Values of Trust in (Criminal) Justice System, by Treatment Group (Placebo v. Pooled Effects)

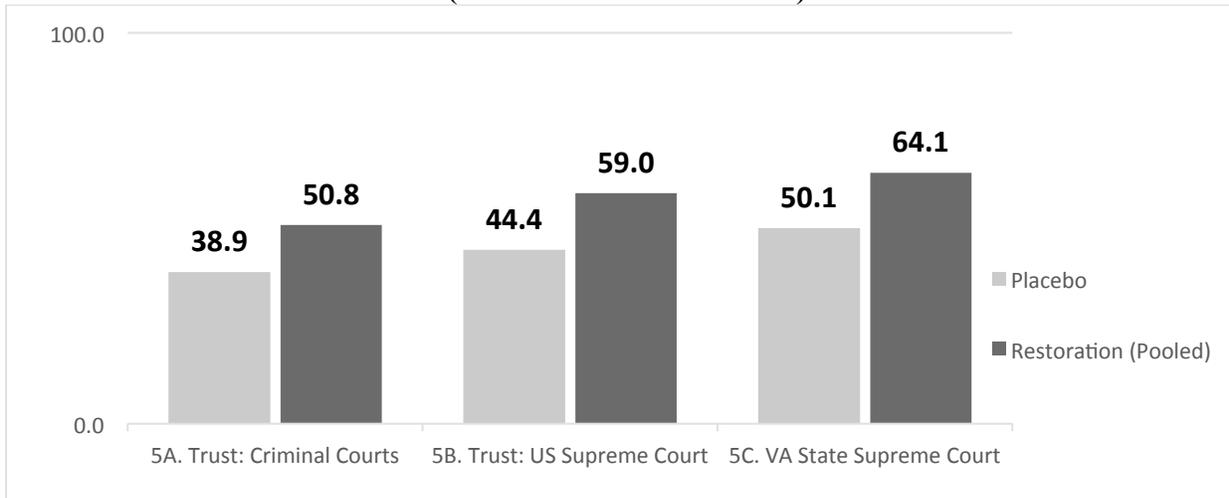


Figure 14. Predicted Values of Trust in (Criminal) Justice System, by Treatment Group (Placebo v. Restoration Only)

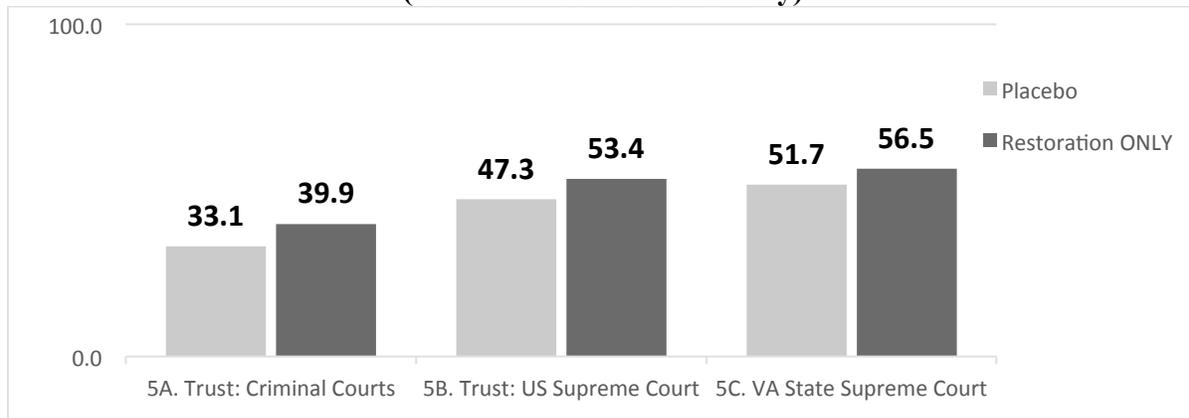


Figure 15. Predicted Values of Trust in (Criminal) Justice System, by Treatment Group (Placebo v. Restoration + GOTV)

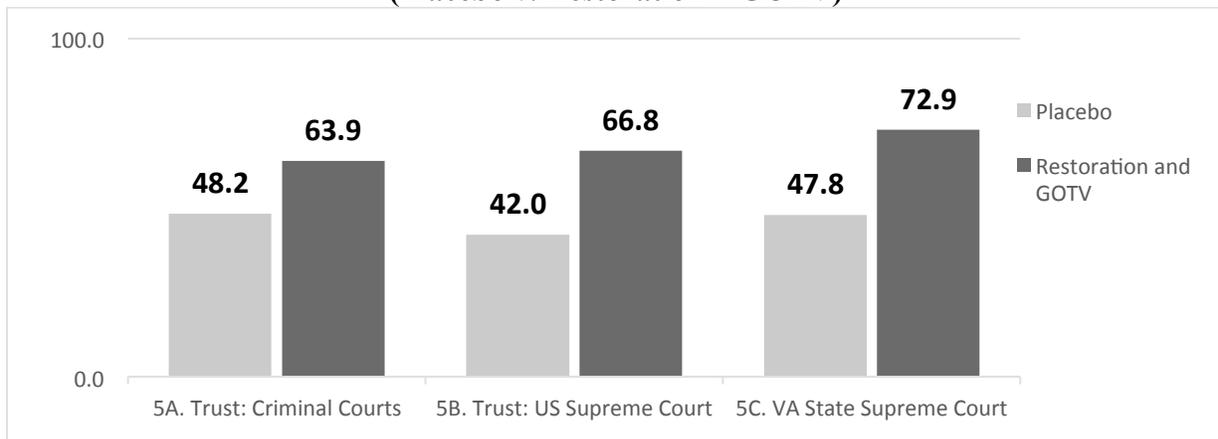
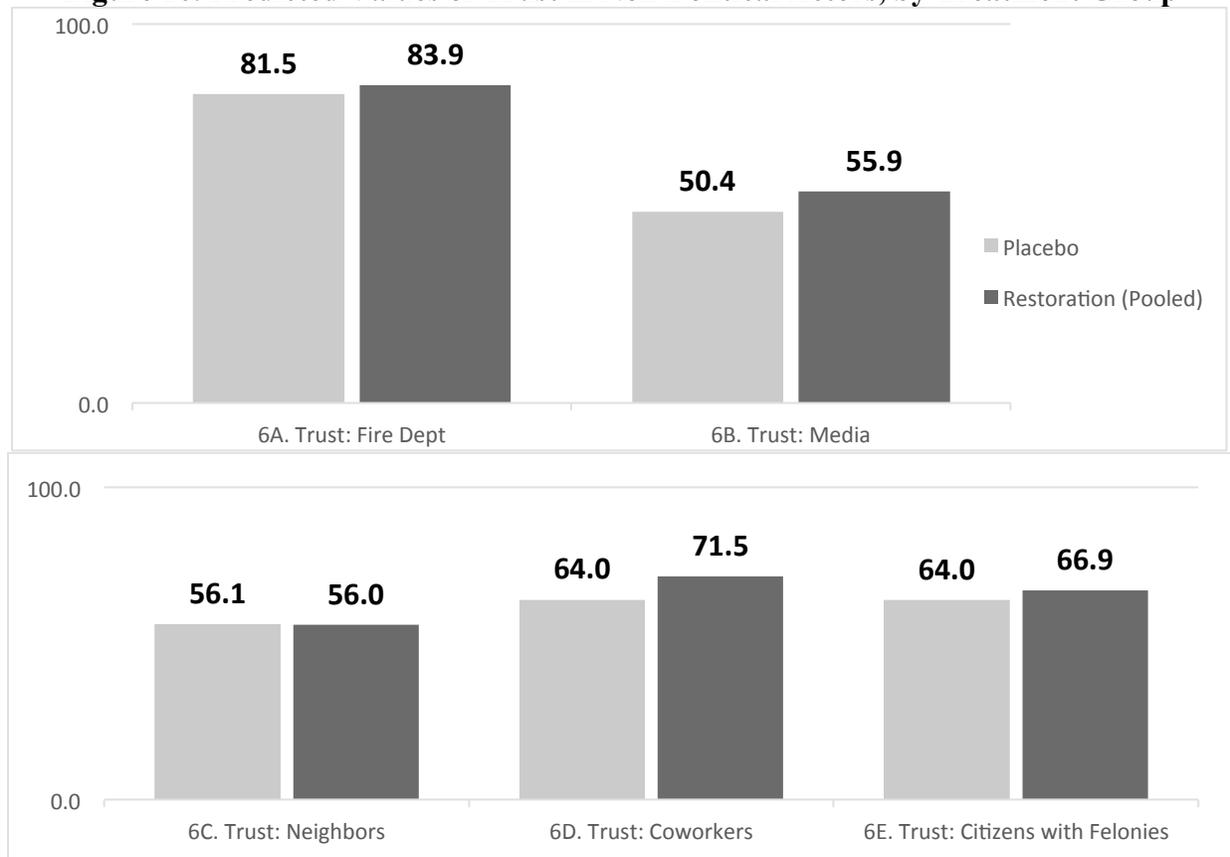


Figure 16. Predicted Values of Trust in Non-Political Actors, by Treatment Group



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Appendixes:

All Appendixes for Akron, Ohio Experiment

<https://drive.google.com/file/d/0B8ncWLLzADEac1h1SUs3R1FnVTQ/view?usp=sharing>

Appendixes for the Richmond, VA Experiment:

Pre-Election Survey:

https://drive.google.com/file/d/1IRGD9zrRlwmpGmWSD_o5IPNL0kn0-ceR/view?usp=sharing

Post-Election Survey Questions:

<https://drive.google.com/file/d/16cjOiesrITAWukhmAAXrDyiL7JuVSLy/view?usp=sharing>

Placebo Treatment:

<https://drive.google.com/file/d/13LhSGZFjplG4GXdxH1ZD5jtck9stTvIF/view?usp=sharing>

Restoration ONLY Treatment:

https://drive.google.com/file/d/1e1yCugbhA__h0hNqSCtzf1BtvU0bWHog/view?usp=sharing

Restoration + Mobilization Treatment:

<https://drive.google.com/file/d/1CdqGcV0ckF6JPWhCUd-TBvwZwsukK1ws/view?usp=sharing>

Pre-Analysis Plan (Registered on EGAP #20171109AA)

<https://drive.google.com/file/d/1xs1hZwHmUYbb-6tT6Y7d42sll10M6zxA/view?usp=sharing>

Note: I am still building tables and figures for the appendixes detailing the results from alternative specifications. The substantive interpretation of the results is unchanged, as the results are robust across model specifications. Additional appendixes with these details will be added soon. Replication files are available upon request and will be posted publicly when the manuscript is published.