

# Family Matters? Voting Behavior in Households with Criminal Justice Contact

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## Abstract

Contact with the criminal justice system has been shown to reduce individuals' political participation, but its effect on friends and family members is less clear. Do people who see loved ones arrested or incarcerated become mobilized to change the system, or do they withdraw from political life? I address this question using administrative data from one large county, identifying registered voters who live with someone facing misdemeanor charges. Court records and vote histories allow me to accurately measure proximate criminal justice exposure and voting for a broader sample of people than survey data would. Using case timing for arrests shortly before and shortly after the election allows me to avoid bias from omitted variables. I find evidence of a short-term demobilization effect for people who see household members convicted or jailed in the weeks before the election, but no evidence of a lasting turnout effect from these experiences.

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# 1 Introduction

Interactions with the criminal justice system, especially time spent behind bars, have been shown to change the political views and behavior of people who directly experience them (Fairdosi, 2009; Weaver and Lerman, 2010, 2014; White, 2017). But what about the millions of people who have had “proximal contact” (Walker, 2014) with the system through family, friends, or neighbors? Do they become less likely to undertake political action, either because of the practical costs that incarceration imposes on households or because of alienation or distrust in government (Weaver and Lerman, 2014; Lee, Porter and Comfort, 2013; Burch, 2013*b*; Sugie, 2015)? Or do they instead become politically activated (Walker, 2014; Walker and García-Castañon, 2017)?

Existing research suggests several ways that proximal criminal justice contact could shape political participation. Incarceration can place resource stresses on a household: lost work time or lost jobs, exclusion from certain occupations or public benefits, and the financial costs of criminal cases (bail, representation, and sometimes fines or fees) could all make it less likely that household members would find the time and energy to vote (Petersilia, 2000; Comfort, 2008; Boruchowitz, Brink and Dimino, 2009; Western and Pettit, 2010). Beyond the resource effects, a range of direct and indirect “social” effects of proximal contact could affect voting. People may withdraw from political and civic life due to shame or stigma surrounding incarceration, or may change their views of government after seeing the state arrest or incarcerate their loved one (Weaver and Lerman, 2014). Or formerly-incarcerated people may become less likely to vote or follow politics, with household members following their lead (Lee, Porter and Comfort, 2013).

Survey evidence on the political participation of people who see their loved ones arrested, convicted, or incarcerated has been mixed, depending on the type of relationship examined, the kind of carceral experience measured, and the political outcomes collected (Lee, Porter

and Comfort, 2013; Walker, 2014; Sugie, 2015). Relying on self-reported proximal contact has downsides: some people may be uncomfortable reporting that a loved one has been arrested or incarcerated. If people who are politically engaged around mass incarceration were also the most willing to report personal experiences with the system, this reporting bias could cause us to overstate the mobilizing effects of proximal contact. Alternatively, people with different underlying propensities to vote might also perceive different amounts of stigma attached to incarceration, such that unlikely voters would also be more willing to report proximal contact. This pattern could generate estimates of a large demobilizing effect even if there were none.

In this paper, I measure the effect of “proximal contact” with the criminal justice system using administrative data rather than survey responses, and focus on a fairly common experience: having a member of one’s household face misdemeanor charges, for which they might ultimately be jailed.<sup>1</sup> Using a dataset from one large county court system, I geolocate misdemeanor defendants and then rely on the state’s voter file to find registered voters who lived at the same address as a defendant. This allows for a broader sample, as well as more reliable measures of voter turnout and contact than would be available from surveys. Court records allow me to accurately measure whether people have seen a household member arrested but not convicted, or convicted but not jailed, or actually incarcerated.

People who experience proximal contact with misdemeanor cases are less likely to vote than those with no apparent connection to the court system. However, this relationship should not be interpreted causally: households with proximal contact are likely different from those without it, in observable and unobservable ways. To get around this problem,

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<sup>1</sup>I focus on misdemeanor cases because they are common and can have large life impacts, causing employment and housing problems as well as social stigma (Boruchowitz, Brink and Dimino, 2009; Roberts, 2011; Mueller-Smith, 2014). However, in the Online Appendix, I demonstrate that including felony cases does not produce substantively different conclusions.

I exploit the timing of criminal cases that fall before and after the election. Rather than comparing people who see their family member arrested to (likely-incomparable) people who have not had this experience, I compare them to people who have not *yet* seen a family member arrested as of the election, but who will have that experience shortly afterwards.

I find no evidence that proximal contact (seeing a household member charged, convicted, or jailed) reduces voting in the long run. People who see a household member convicted in the few weeks immediately preceding the election vote at lower rates than they would otherwise, but this effect fades out quickly. The life chaos induced by a criminal case might be causing people not to vote in the immediate aftermath, but it doesn't appear that voters change their behavior in the long run, as we might expect from accounts of political socialization. I conclude that the effects of this type of proximal contact (misdemeanor arrest, conviction, or jail) on this type of political participation (voting) are limited.

## 2 Using Government Data to Measure Contact

I begin with public records from the Harris County (Texas) criminal courts at law, which hear all misdemeanor cases in the county (including those from the city of Houston).<sup>2</sup> Common case types for these courtrooms include driving while intoxicated, theft, possession of small amounts of marijuana, and certain types of (non-aggravated) assault. I focus on first-time misdemeanor cases filed in 2012-2013, in which the defendant has a valid address within Harris County on file. I then use defendants' addresses to precisely geocode their homes. Next, I find household members of these defendants within the Texas state voter file, which

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<sup>2</sup>Records were requested from the District Clerk's office and are also publicly searchable at <http://www.hcdistrictclerk.com>

contains the addresses of all registered voters.<sup>3</sup> I geocode all voters within Harris County and then find any registered voters who live within five meters of one of the geocoded misdemeanor defendants. To avoid including all residents of large apartment complexes or housing projects as “household members” of a given defendant, I omit addresses with more than 10 registered voters at that address. I also exclude misdemeanor defendants that are themselves registered voters.<sup>4</sup>

This yields a sample of 19,192 registered voters who lived with a person charged with a misdemeanor in 2012-2013. Section [SII](#) in the Online Appendix examines whether these voters genuinely have some relationship with the defendants to whom they’ve been matched; many are close family members.

	Proximal Contact Sample	All Voters
Voter Turnout 2012	0.49	0.55
Prior Voter Turnout (2010)	0.33	0.42
Prior Voter Turnout (2008)	0.53	0.61
Prior Voter Turnout (2006)	0.22	0.30
Prior Voter Turnout (2004)	0.42	0.49
Mean Age (Years)	46.22	48.76
Proportion Male	0.45	0.46
Mean Time Registered (Years)	13.10	14.05

Table 1: Comparing the sample used in this paper to all registered voters in Harris County.

Table [1](#) compares these voters’ characteristics to the universe of registered voters in Harris county. Compared to all registered voters, those living with defendants are slightly younger and have been registered to vote for less time. They are also less likely to have voted in prior elections, even before their household member was arrested. Still, turnout

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<sup>3</sup>The voting data used here are 2012 and 2014 snapshots of the state voter file provided by Nationbuilder. I match defendants to voters that were registered at an address as of August 2012, then find those voters in the 2014 file to discover their 2012 turnout.

<sup>4</sup>See Section 2 of the Online Appendix for more details of sample construction.

rates for this group are far higher than turnout found among people with direct criminal justice contact in prior studies (Burch, 2010; Haselswerdt, 2009; White, 2017). This could allow for much larger demobilizing effects among family members than have been seen for incarcerated people themselves. Descriptively, they are also less likely to have voted in the 2012 election than voters at large. Next, I try to establish the causal effect of proximal contact on 2012 voting.

### 3 Measuring the Effects of Proximal Contact

#### 3.1 Exploiting Case Timing

A simple bivariate regression of 2012 voting onto “saw household member arrested/convicted/jailed” suggests that proximal contact has substantial demobilizing effects (results in Online Appendix Section [SI4](#)). But people who are sentenced to jail (and their families) are likely different from those who aren’t, in both observable and unobservable ways. Indeed, adding in available covariates shrinks the effect estimates dramatically, suggesting that a simple comparison of jailed to unjailed yields biased results (Gerber et al., 2017).

Rather than simply comparing registered voters who see a household member arrested to those who do not, I exploit the timing of criminal cases to get better estimates of proximal contact’s effect on voting. The intuition here is that people who see a loved one sent to jail in October are not substantially different from people who see their loved one sent to jail in December, except that the October cases fall before the election and can impact people’s decision to vote, and the December cases don’t. If we believe that arrest, charge, and sentencing behavior are fairly consistent across time (particularly around the election), we should be able to compare people who experience a given type of proximal contact before the election to those who experience it afterwards to obtain a causal effect of contact on voter turnout. This is similar to approaches used by Weaver and Lerman (2010) and Testa (2016)

to estimate the individual effects of contact and by Burch (2013*b*) to estimate neighborhood effects of incarceration, and shares intuition with the approaches described in Elwert and Pfeffer (2016).

As a first check of the assumption that cases from before and after the election are fairly similar and affect similar households, Figure 1 plots binned means for a number of pre-treatment covariates for the weeks before and after the November 2012 presidential election. These background characteristics look fairly consistent over time, both for characteristics of the voters (age, gender, prior turnout in the 2008 election) and the matched defendants (gender, race, and charge severity). Similarly, Figure SI1 in the Online Appendix explores voting in the four prior elections rather than just 2008. If we saw large trends or discontinuities in these pre-treatment variables, we would worry that comparing across time would introduce bias, as we would be comparing voters with different underlying turnout propensities (and the differences on observable covariates might also suggest differences on unobservable characteristics). It doesn't appear that arrests shortly before the election affected different types of households than those shortly after.<sup>5</sup>

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<sup>5</sup>The most likely direction of bias, if we feared strategic behavior by police or judges, would be negative: cases falling after an election would be more likely to be matched to high-propensity voters, which would make the demobilizing effect of contact look larger than it truly is. Based on the voting rates shown in Figure SI1, this does not appear to be happening. But if it were, it should generate larger negative results, not the mainly-null results presented here.

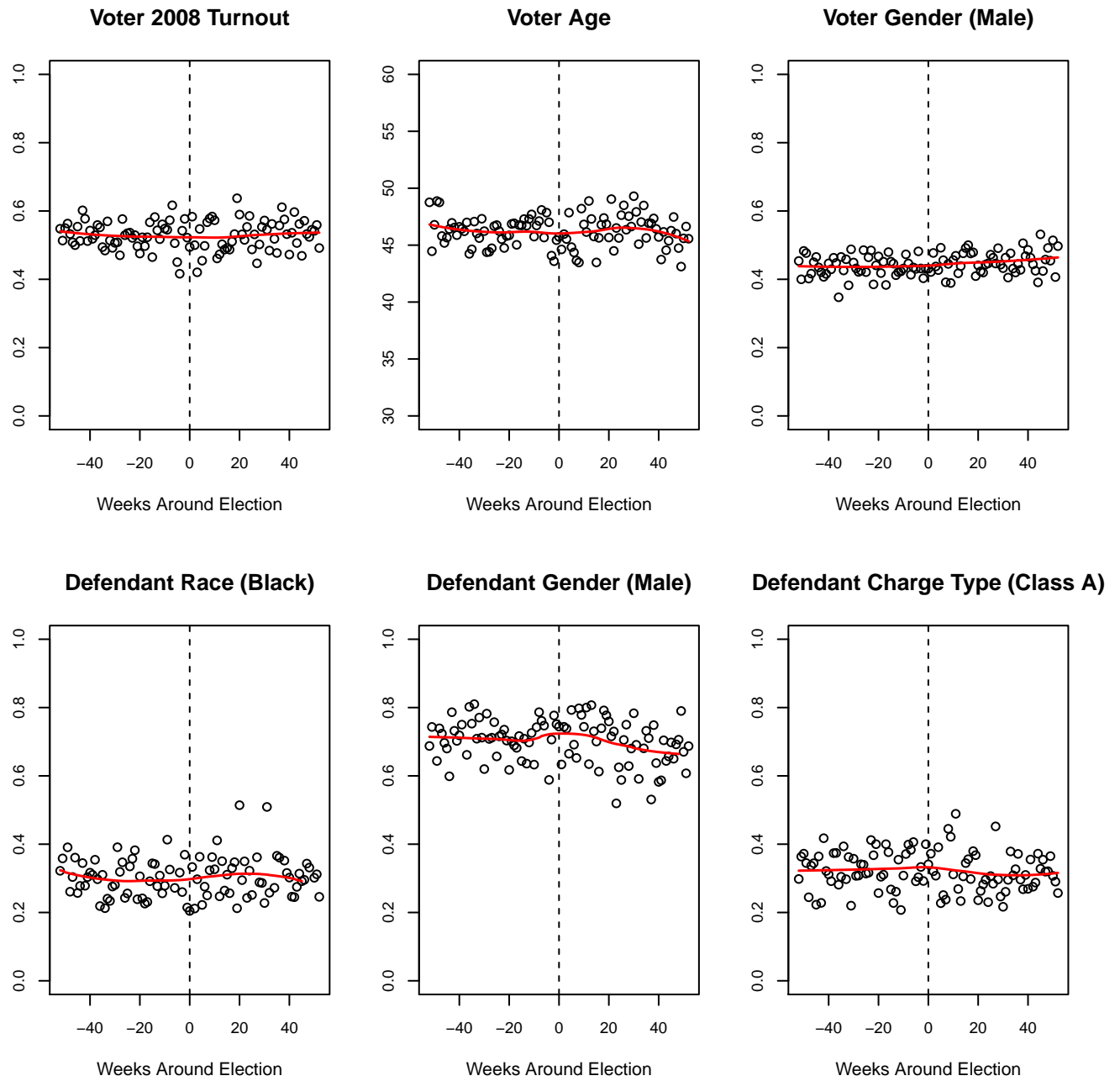


Figure 1: Pre-treatment covariates during the pre/post-election periods, demonstrating balance and no large discontinuities.



Table 2: Proximal Contact on Voting (Using Case Timing)

	Voted 2012	
	(1)	(2)
Case Before Election	0.033 (0.048)	0.027 (0.036)
HH Member Convicted	0.080 (0.055)	0.055 (0.044)
HH Member Jailed	-0.024 (0.047)	-0.011 (0.036)
HH Member Convicted * Before Election	-0.172* (0.080)	-0.156* (0.062)
HH Member Jailed * Before Election	-0.125 (0.067)	-0.121* (0.051)
Voter Male		-0.008 (0.019)
Voter Age (Years)		-0.0004 (0.001)
HH Member Male		0.024 (0.024)
HH Member Black		0.054* (0.025)
2006 Turnout		0.032 (0.032)
2008 Turnout		0.339* (0.030)
2010 Turnout		0.312* (0.032)
Constant	0.493* (0.036)	0.210* (0.044)
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Observations	1,558	1,541
R <sup>2</sup>	0.015	0.338
Adjusted R <sup>2</sup>	0.011	0.332

*Note:*

\*p<0.05

I begin with cases from one month around the election. Table 2 presents OLS regression results from a model that regresses a voter's 2012 turnout onto an indicator variable for whether their household member's case fell before or after the election, and a factor variable indicating how extreme their level of proximal contact was (charge but no conviction, conviction but no jail, or a jail term), as well as the interaction between the two. These results suggest that seeing a household member charged but not convicted of a misdemeanor before the 2012 election had little impact on voter turnout (estimate of +3 percentage points) compared to having that experience after the election. Seeing a household member convicted of a misdemeanor crime (but not jailed) before the election rather than after appears to have a larger demobilizing effect (-6 percentage points), as does seeing a household member sent to jail (-12 percentage points).<sup>6</sup>

These results from the 30 days immediately before and after the election suggest that proximal contact can demobilize voters. But do these effects persist, or are they limited to the relatively small number of people who are dealing with the immediate aftermath of cases when election day comes around?

Figure 2 expands the time frame for Table 2's analysis, looking at windows ranging from two weeks around the election to forty weeks. The top panel presents estimates of the effect of seeing a household member charged with a misdemeanor (but not convicted) before the election, the middle panel estimates of the effect of conviction (but not jail), and the bottom panel estimates of pre-election jail sentencing's effect on household member voting. These plots suggest that Table 2's estimates, of large negative effects on voting from proximal contact, are more the exception than the rule. As the window broadens to include cases that fell months rather than weeks before the election, the estimated effects of proximal

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<sup>6</sup>Results do not change substantially if I omit cases that fall in the few days immediately before the election, out of concern that people may not receive the treatment in time for it to affect voting.

contact approach zero. It does not appear that the types of proximal contact measured here (misdemeanor charges, conviction, or jail time) have lasting effects on household members' voter turnout beyond a few weeks. It may appear that conviction (middle panel of Figure 2) still exercises some negative influence on turnout as we look at cases months from the election: the estimates are negative and suggest a 2-4 point drop in turnout, though they can't be statistically distinguished from zero. But recall that these estimates are based on a dataset that combines voters who experienced conviction very near the election with those who experienced it earlier. If we drop the month (30 days) immediately around the election and generate the same estimates, we reach a different conclusion, as shown in Figure 3. Once we omit voters who could have a very short-term effect from proximal contact (because their household member is charged and convicted in the few weeks immediately before the election), the pattern of null results becomes even clearer. People who see their household member convicted of a misdemeanor two or three or eight months before the election do not seem less likely to vote than people who don't see their family member convicted until afterwards.

The introduction to this paper highlighted two broad types of mechanisms by which proximal contact could demobilize: roughly, "resource" mechanisms, and "social" ones. These results are consistent with only the resource story. Having a household member convicted or jailed in the weeks before an election can send a household into chaos, as household members scramble to pay legal costs or fines, to find childcare, and to keep the household financially afloat during one member's absence (Howell, 2009). Further, that household member's return from jail or lockup can bring additional turmoil (Comfort, 2015). These experiences are serious, but many of them are short-term, consistent with the pattern of demobilization seen here.

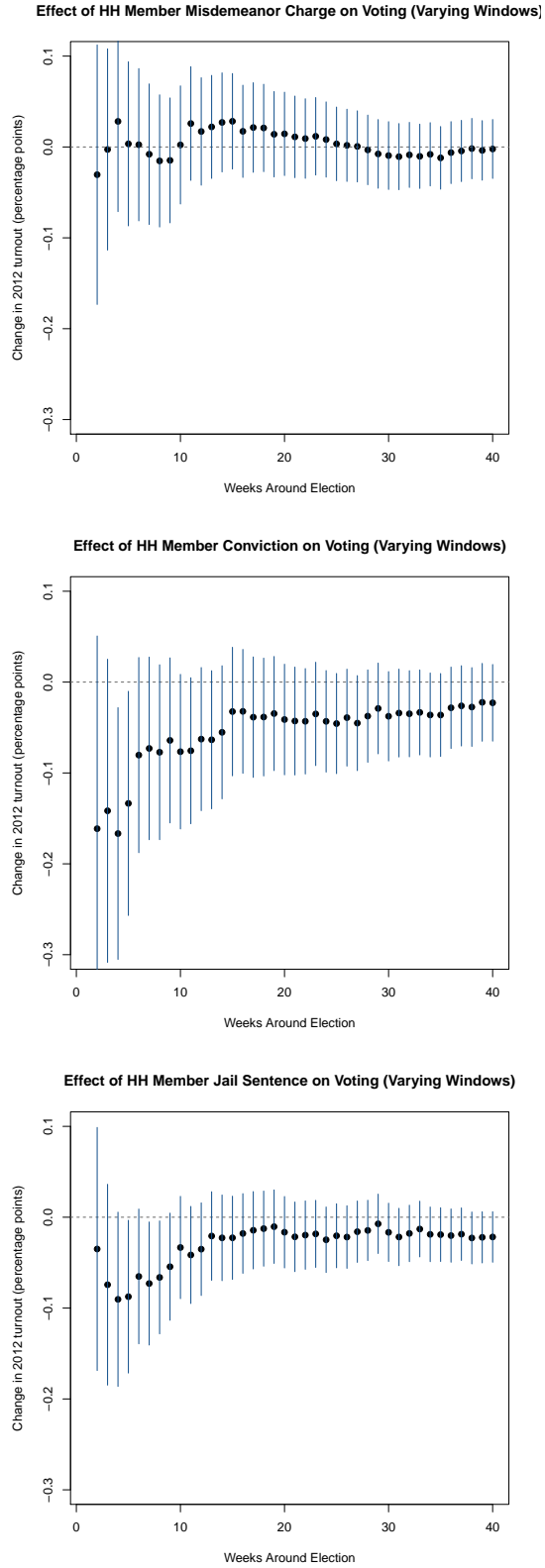


Figure 2: Estimates of the effect of proximal contact (charge, conviction, jail) on turnout using cases that fall before and after the election, varying the time window used.

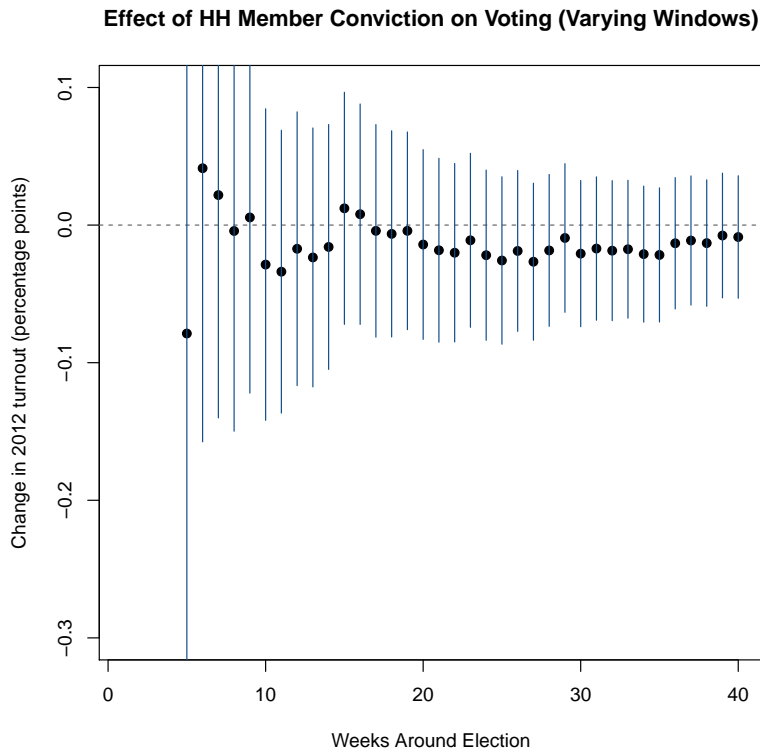


Figure 3: Estimated effects of household member misdemeanor conviction on 2012 voting, as in the center panel of Figure 2, dropping cases from the 30 days immediately around the election.

But many of the social mechanisms by which proximal contact has been hypothesized to reduce turnout occur on a longer time frame. Social stigma or embarrassment at a loved one’s arrest and conviction would be expected to cause families to withdraw from social and civic life for more than just a few weeks. Political socialization experienced by families of people incarcerated, such as that described by Weaver and Lerman (2014), is usually thought to persist for months or years after the initial experience. If such processes were happening in this case, we should continue to see effects for households whose cases fell months or even years away from Election Day. That we do not see such long-lasting effects casts doubt on whether these mechanisms are occurring here. As noted below, the absence of these long-run social effects is not likely due to this paper’s focus on misdemeanor cases (possibly considered

a weaker “treatment” for families); similar patterns appear in felony cases as well. Instead, it seems that processes like social withdrawal, while important in many realms of life, do not shape voting patterns among households with proximal contact. Section 4 considers why.

### **3.2 Robustness**

Section [SI5](#) of the Online Appendix presents additional evidence to address several concerns: match quality (generally or over time), imbalance around the election, and the generalizability of these findings. A placebo test using only 2013 cases rules out certain types of bias due to seasonal patterns. I also estimate the effects of household members’ felony cases (these results, like the main results for misdemeanor cases, do not suggest a lasting demobilization effect).

I also check whether the apparent null effects shown here are masking effect heterogeneity: some household members being demobilized by proximal contact while others are mobilized, with these effects obscuring each other. In the Online Appendix, I explore the possibility of effect heterogeneity on several dimensions: race, neighborhood, wealth, family relationships, and past voting. Across many different subsets of the population, I find very little evidence of persistent demobilization (or mobilization). Similarly, Section 2.3 of the Online Appendix discusses what underlying differences between Harris County and the rest of the US could mean for generalizability.

## **4 Implications**

I find no evidence of a medium- or long-run effect on turnout among registered voters who see their household member entangled with misdemeanor courts. What broader conclusions can we draw from these findings?

First, I do not think these findings contradict or invalidate ethnographic work that traces

the demobilizing effects of proximal contact (Comfort, 2008; Lee, Porter and Comfort, 2013), nor the rich qualitative literature on activism by the families of incarcerated people (Gilmore, 2007; Miller, 2008; Owens, 2014). Some people clearly become demobilized or activated by proximal contact. But if those effects extended to the many millions of Americans who have seen a family member charged with, convicted of, or jailed for a misdemeanor crime, the impact on political participation would be staggeringly large. The present findings do not minimize the importance of experiences with the criminal legal system or suggest that mass incarceration is irrelevant or morally acceptable; they suggest limits to the extent of its effects on a particular political behavior.

And this is a particularly large caveat, as this paper focuses solely on voter turnout despite cautions by Walker (2014) and others that voting may not respond in the same way as protest, group membership, or other political behaviors. That said, voting is one of the most common forms of political participation, so there is value in knowing how it is shaped by proximal contact.

This piece also focuses on adult household members who have already registered to vote, neglecting possible long-run effects on children or unregistered adult household members. This is a limitation of the project; I have no way of estimating such long-run effects. However, focusing on already-registered voters has value not only as a data-collection decision (though it does make it much more feasible to find people who may not want to volunteer to survey-takers that they have experienced proximal contact), but also as a substantive one. Many people who see their household members arrested may not have voted regardless—for them, non-voting is overdetermined. People who have already registered to vote, and in many cases have voted in past elections, arguably provide the easiest test of the hypothesis that proximal contact affects participation.

But acknowledging that many people who experience proximal contact are already non-voters raises more questions about how we think about the effects of criminal justice con-

tact. Burch (2013*a*) notes that disadvantaged neighborhoods experience incarceration at rates many times higher than advantaged ones. Perhaps the marginal effect of seeing one's household member arrested or convicted is minor, because people in the sample have already learned a great deal about the criminal legal system through other proximal contact (seeing neighborhood police stops, or extended family or friends' incarceration), or even through national media. But if that is the case, it suggests that the effects of proximal contact are more complex than previous theories have suggested, and that we should fine-tune our predictions for when people will be demobilized (Sampson, 2014).

This paper has investigated voting behavior by a set of people who might reasonably have been expected to be deterred from voting by proximal contact, and has found no lasting effects. This should provide a starting point for further discussion of when and whom proximal contact demobilizes.



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