

Letter

Family Matters? Voting Behavior in Households with Criminal Justice Contact

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Contact with the criminal legal 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.

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, Forthcoming). 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 (Burch 2013a; Lee, Porter and Comfort 2013; Sugie 2015; Weaver and Lerman 2014)? Or do they instead become politically activated (Walker 2014; Walker and García-Castañón 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 (Boruchowitz, Brink, and Dimino 2009; Comfort 2008; Petersilia 2000; Western 2006; Western and Pettit

2010). Beyond these resource effects, a range of direct and indirect “social” effects of proximal contact could affect voting. People may withdraw from political and civic lives 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; Sugie 2015; Walker 2014). Relying on self-reports has downsides: some people may be uncomfortable reporting proximal contact. 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.¹ Using a dataset from one large county court system, I geolocate defendants and

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¹ 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; Mueller-Smith 2018; Roberts 2011). However, in the Online Appendix, I demonstrate that including felony cases does not produce substantively different conclusions.

then rely on the state's voter file to find registered voters who lived at the same address. 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.

Descriptively, people who experience proximal contact with misdemeanor cases are less likely to vote than those with no apparent connection to the court system. However, households with proximal contact are likely different from those without it, in observable and unobservable ways. To get around this selection problem, 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 afterward.

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 does not appear that voters change their behavior in the long run, as we might expect from accounts of political socialization. The effects of this type of proximal contact (misdemeanor arrest, conviction, or jail) on this type of political participation (voting) are limited in scope.

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).² Common case types for these courtrooms include theft, possession of small amounts of marijuana, and certain types of (nonaggravated) assault. I focus on first-time misdemeanor cases filed in 2012–13, in which the defendants have valid addresses 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 contains the addresses of all registered voters.³ 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.⁴

This yields a sample of 19,192 registered voters who lived with someone charged with a misdemeanor in 2012–13. Section 1 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.

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 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, *Forthcoming*). This could allow for larger demobilizing effects among family members than among 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.

MEASURING THE EFFECTS OF PROXIMAL CONTACT

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 4). But people who are sentenced to jail (and their families) are likely different from those who are not. 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. 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 afterward 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

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

³ I use 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.

⁴ See Section 2 of the Online Appendix for more details of sample construction.

TABLE 1. Comparing the Sample Used in This Paper to All Registered Voters in Harris County

	Proximal contact sample	All voters
Voter turnout 2012	0.49	0.55
Prior voter turnout (2010)	0.32	0.42
Prior voter turnout (2008)	0.52	0.61
Prior voter turnout (2006)	0.21	0.30
Prior voter turnout (2004)	0.41	0.49
Mean age (years)	46.28	48.76
Proportion male	0.44	0.46
Mean time registered (years)	13.16	14.05

FIGURE 1. Pretreatment Covariates During the Pre-/Post-Election Periods, Demonstrating Balance and No Large Discontinuities

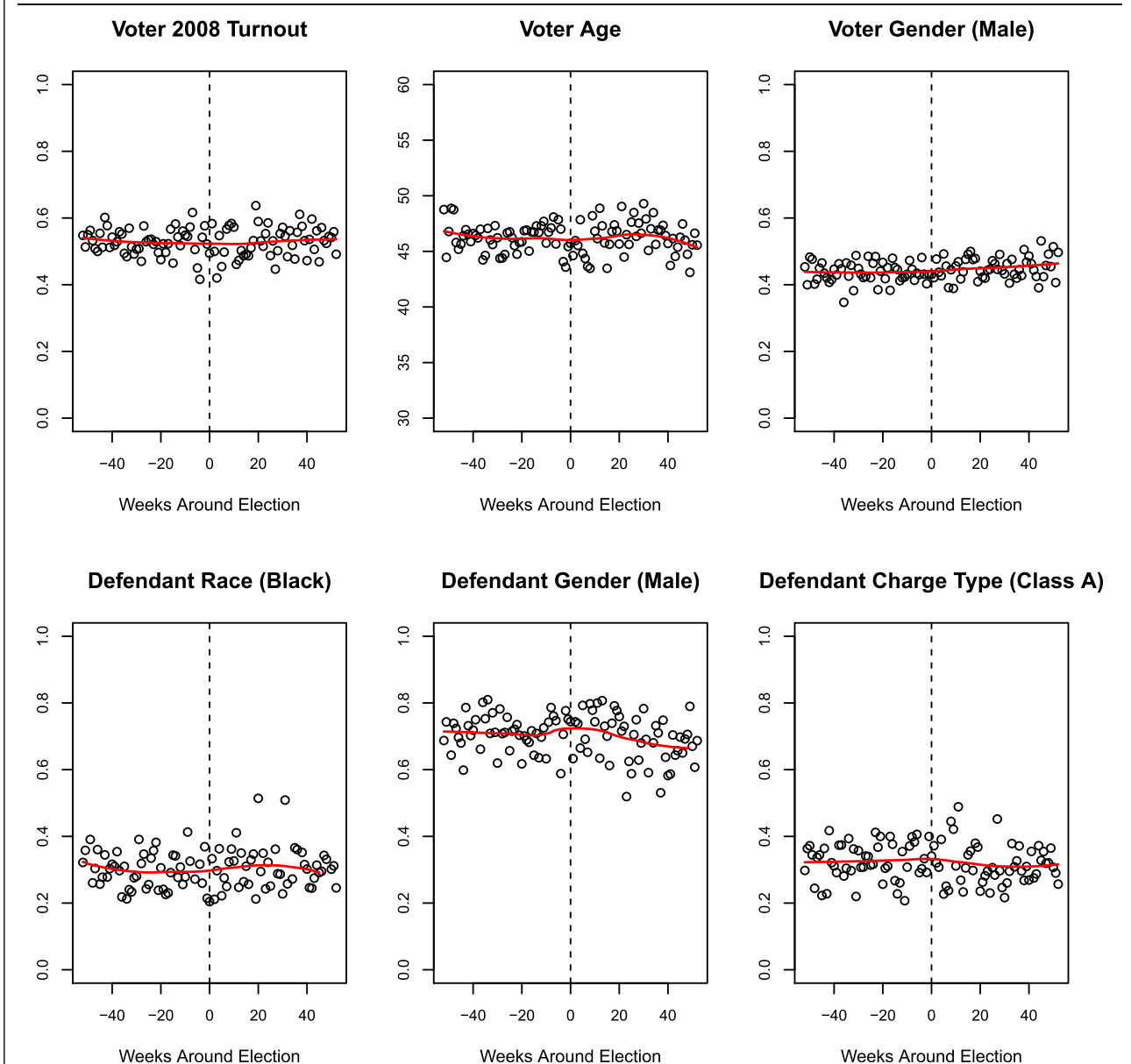


TABLE 2. Proximal Contact on Voting (Using Case Timing)

	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)
Observations	1,558	1,541
R^2	0.015	0.338
Adjusted R^2	0.011	0.332

Note: * $p < 0.05$.

and by Burch (2013a) 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 pretreatment 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 1 in the Online Appendix explores voting in the four prior elections rather than just 2008. If we saw large trends or discontinuities in these pretreatment 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 does not appear that arrests shortly before the election affected different types of households than those shortly after.

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).⁵

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?

⁵ 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.

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

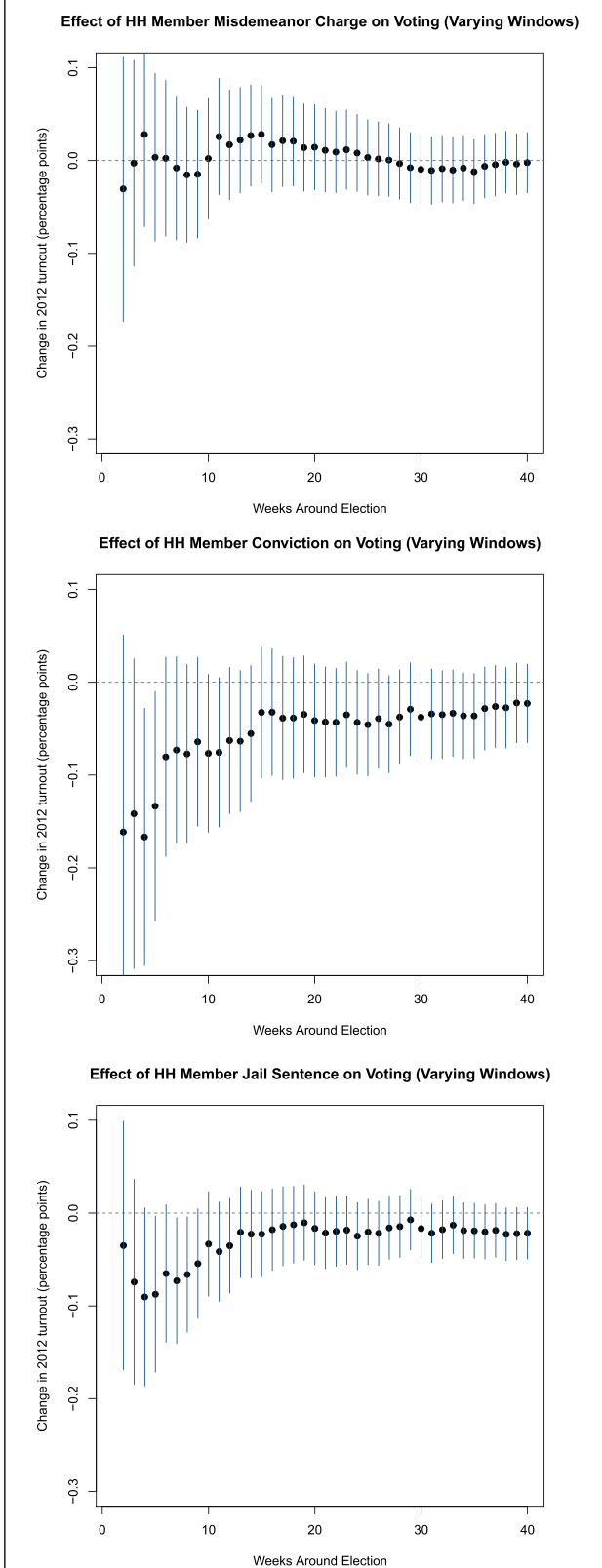


FIGURE 3. Estimated Effects of Household Member Misdemeanor Conviction on 2012 Voting, as in the Center Panel of Figure 2, Dropping Cases From the 30 Days Immediately Around the Election

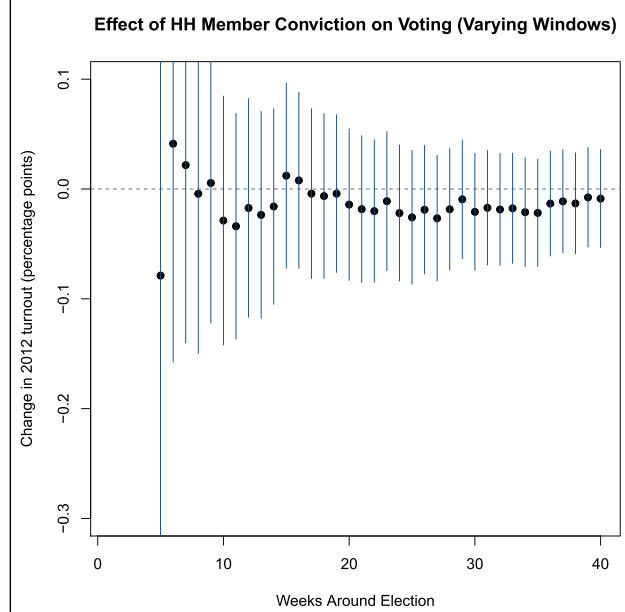


Figure 2 expands the time frame for Table 2's analysis, looking at windows ranging from two weeks around the election to forty weeks. The top panel presents estimates of the effect of seeing a household member charged with a misdemeanor (but not convicted) before the election, the middle panel estimates 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 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, although they cannot 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 do not see their family member convicted until afterward.

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 2016). These experiences are serious, but many of them are short term, consistent with the pattern of demobilization seen here.

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; similar patterns appear in felony cases as well (Section SI5). 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. The concluding section considers why this is.

Robustness

Section 5 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 (Maltby 2017). 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 USA could mean for generalizability. I consider characteristics such as court processes, local demographics, and voter turnout and conclude that it is unlikely that a similar study run elsewhere would find substantially larger or longer-lasting effects.

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. These estimates are well identified, and they are consistent with a story about short-run demobilization arising from life chaos, but not with longer-term mechanisms such as political socialization. What broader conclusions can we draw from these findings?

First, my approach to measuring proximal contact with administrative data rather than survey self-reports confirms past descriptive findings that people who experience proximal contact are less likely to vote than the general public. This difference in voter turnout does not carry a causal interpretation, but it is still worth knowing. Some of the people most affected by carceral policies are unlikely to be engaged in the political process that generates those policies.

Second, 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 an important caveat, as this paper focuses 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 are 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, but also as a substantive one. Many people who see their household members

arrested may not have voted regardless—for them, nonvoting is overdetermined. People who have already registered, 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 nonvoters raises more questions about how we think about the effects of criminal justice contact. Burch (2013b) 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.

SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit <https://doi.org/10.1017/S0003055418000862>.

Replication materials can be found on Dataverse at: <https://doi.org/10.7910/DVN/5CYWWF>.

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