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## 2 **Supplementary Information for**

### 3 **Sanctuary Policies Reduce Deportations Without Increasing Crime**

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#### 6 **This PDF file includes:**

- 7     Supplementary text
- 8     Figs. S1 to S15
- 9     Tables S1 to S2
- 10    SI References

## 11 Supporting Information Text

12 In this SI Appendix, I offer more information on the data, further background on immigration enforcement during the period in  
13 which sanctuary policies took effect, and a series of robustness tests, including event study results for balanced panels as well as  
14 results using linear rather than negative binomial regressions. Finally, I show tests of additional mechanisms driving the results.

15 **Collecting Sanctuary Policy Information.** In order to collect sanctuary policy information, I relied on a variety of sources. I  
16 began with the dates of the sanctuary policies collected and publicized by ICE itself in four public reports on declined detainer  
17 requests. In order to collect a policy implementation month for each county, I took the following simplifying steps:

- 18 • I used the first date of any relevant policy. For example, Los Angeles implemented a sanctuary policy in June, 2014, but  
19 California had already passed the Trust Act in October 2013, establishing a no-detainer policy statewide. As a result,  
20 October 2013 is the date I use for the policy in Los Angeles.
- 21 • The large majority of policies were at the county level. Only two states passed sanctuary laws that regulated local officials'  
22 behavior between 2012 and 2015: California in October 2013 (changing policies in 36 of the 37 California counties in the  
23 sample; Santa Clara county had already adopted a policy before that date) and Connecticut in January 2014 (changing  
24 policies in 4 counties in the sample). I do not distinguish between policies adopted by the county sheriff and the county  
25 jail. Where a city within a county passed a sanctuary policy, I treat that as the policy for the relevant county if the  
26 city made up more than half the population of the county. This coding rule affected six counties in the sample: Pima,  
27 AZ, Middlesex, MA, Hartford, CT, Essex, NJ and Providence, RI (city policy excluded) and Boston, MA (city policy  
28 included for Suffolk County).
- 29 • I drop the city and county of Baltimore because ICE's data does not distinguish between the two, and the city is not a  
30 part of the county.
- 31 • As noted in the text above, I combine the five New York City counties (boroughs) into a single jurisdiction.
- 32 • Where a state enacted a policy only affecting its own prisons—and not regulating county or city officials—I do not  
33 consider that policy, even though it may have had some impact on deportations of people fingerprinted in the counties in  
34 that state. This affects only Rhode Island; a similar statute in North Dakota has no effect here because no North Dakota  
35 county has a large enough Hispanic population to be part of the sample.
- 36 • Where sources offer conflicting dates of policy onset, I rely first on the written policy, if I have it, then on ICE records,  
37 and finally on third-party lists.

38 The “policies.dta” dataset in the replication archive (on file with the author, to be posted upon publication) indicates the  
39 document I have relied on for each county.

40 **Detainer Data.** The ICE detainer data, unlike the ICE removals data, does not include a county variable, but it does include  
41 the name of the jail or prison to which the detainer request was made. I was able to match many of these jails to counties  
42 using sanctuary policy information from the Immigrant Legal Resources Center; I matched the remaining jails by searching for  
43 the location of each detention center manually. I excluded detainer requests made to state or federal prisons from the analysis,  
44 since county-level policies should have no effect on those requests. I also excluded local jails serving multiple counties, where  
45 the relevant policy was ambiguous. For more details, including the full original dataset and the steps taken to reduce the  
46 sample, see the replication folder.

47 **Sample Restrictions.** Table S1 shows the steps by which I limited the sample used in the analysis. I concentrated on a sample  
48 of counties with the largest Hispanic populations in order to capture a majority of local-origin deportations nationwide while  
49 feasibly collecting implementation dates for nearly all counties in the sample. Ultimately, I was forced to exclude 12 of 314  
50 counties because, although ICE data from the Immigrant Legal Resources Center indicated that they had sanctuary policies,  
51 I could not find the dates of those policies. These counties accounted for relatively few deportations, and their exclusion is  
52 unlikely to have biased the results. In all of the analysis, I excluded county-months before each county had its first Secure  
53 Communities deportation in order to avoid categorizing county-months as including zero fingerprint-match deportations when  
54 the Secure Communities program—which made such deportations possible—had not yet begun.

55 For the early and late balanced panels discussed below, I further restricted the sample to include only counties that  
56 (a) implemented sanctuary policies during the study period and (b) had at least 10 months of data before and after the  
57 implementation of the policy. Because some counties had imposed sanctuary policies before others entered the sample, I  
58 use two different balanced panels, one with more early data and the other with more late data. The early balanced panel  
59 includes 68 counties from December 2010 through December 2015 ( $N = 4,148$  county-months, accounting for 130,685 local-  
60 fingerprint deportations). The late balanced panel includes 127 counties from August 2012 through December 2015 ( $N = 5,207$   
61 county-months, accounting for 77,790 local-fingerprint deportations).

**Table S1. Sample Restrictions (Combined Census and Removals Data)**

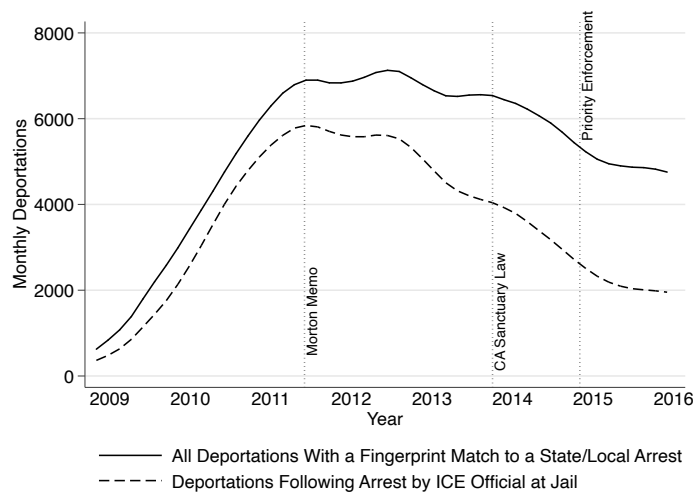
Restriction	Counties	Removals	Hispanic Pop.
All	3140	454,474	50,477,156
Top 10% Hispanic Population	314	373,994	44,253,940
Matching Counties with Sanctuary Policy Information Through 2015	296	369,388	43,511,744

*Data:* Sanctuary Policy, 2010 Census, and ICE Secure Communities Removals Data (Nov. 2008 - Dec. 2015)  
*Note:* The 296 counties account for 300 original counties; New York City is collapsed into one.

62 **Additional Background and Descriptive Statistics.** Sanctuary policies took effect during a period of overall decline in interior  
63 immigration enforcement. Figure S1 shows overall trends in deportations (including all deportations, not just those in the  
64 large counties in the eventual sample): there was a large increase from 2009 to 2011, as the Secure Communities program  
65 (which gave ICE access to the information that local authorities shared with the FBI after arrests) was rolled out, followed by  
66 a decrease from 2013 through 2015. The ICE dataset identifies deportations that followed an arrest within a jail or prison:  
67 those deportations in which the person’s most recent arrest took place through the so-called Criminal Alien Program (“CAP”)  
68 or a cooperative (section 287(g)) agreement between the federal government and the local authority.\* This is the subset of  
69 deportations that I expect sanctuary policies to affect directly. I call these *local-arrest deportations*; the dashed line in Figure S1  
70 shows how their numbers changed over time. The variable tracking whether a deportation originated with an arrest in a jail or  
71 prison is only available through December 2015, which explains why the study period ends then.

72 The three vertical dotted lines in Figure S1 show three important events. First, in June 2011, ICE issued a memo, known as  
73 the Morton Memo, which instructed ICE to exercise individualized prosecutorial discretion to decline to institute deportation  
74 proceedings against noncitizens on the basis of a range of factors, including lack of criminal history. That memo was issued  
75 at the peak of removals following local arrests (the dashed line in Figure 3), which began to fall soon after. Second, the  
76 California sanctuary law, passed in October 2013, marked the beginning of a period in which many large counties adopted  
77 sanctuary policies. And finally, in November 2014, ICE ceased its Secure Communities program and replaced it with the  
78 Priority Enforcement Program. That new program addressed a few of the concerns of sanctuary jurisdictions; it required ICE  
79 officers to stop arresting, in jails, noncitizens not convicted of crimes, and it reduced the use of detainer requests (2).

**Fig. S1. Local-Fingerprint Deportations Over Time**



Lines from kernel-weighted local polynomial smoothing.

80 Table S2 shows summary statistics on local-origin deportations, together with the dates of sanctuary policies, for the fifteen  
81 counties with the most deportations during the study period (November 2008 to December 2015). Figure S2, meanwhile, shows  
82 a map of all counties included in the sample (except three counties in Hawaii and Alaska).

83 In order to show that the effect of sanctuary on deportations does not depend on model choice, I show two basic descriptive  
84 figures below. Figure S3 shows trends over time in counties that eventually adopted a sanctuary policy during this period,

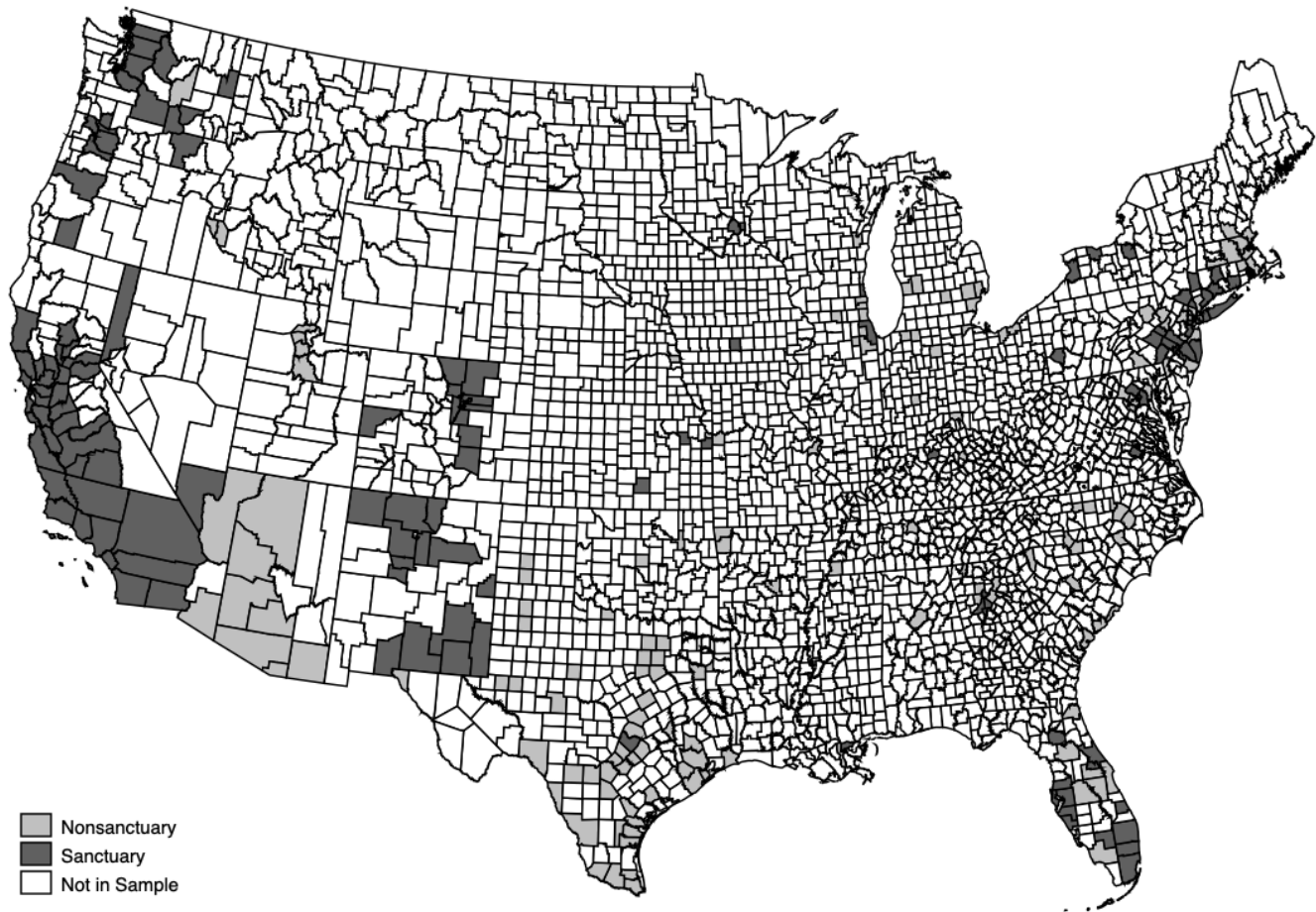
\*Apprehensions are marked as having taken place through CAP or 287(g) when ICE the officer codes the arrest that way, and encounters with noncitizens in local jails are generally coded that way. See (1) at 44-46 (“Q. Okay. What does it mean to be a CAP officer now? A. It means that you’re conducting Criminal Alien Program duties, either within a jail or dealing with the criminal justice system.”), 59 (“Q. So from 2006 to the present is any encounter between a CAP officer and an individual incarcerated a CAP encounter? A. It would be considered a CAP encounter.”). There remains some uncertainty about the consistency with which ICE codes detainer-related arrests as CAP arrests; to the extent that the coding contains measurement error, that error likely biases the CAP results downward.

**Table S2. Fifteen Counties with Highest Local-Fingerprint Deportation Counts (by Criminal Conviction), November 2008 Through December 2015**

County	Total	Level 1	None	Violent	Policy Date
Dallas, TX	11599	3752	1833	896	–
El Paso, TX	15314	3378	1353	338	–
Harris, TX	27196	9103	2014	1622	–
Hidalgo, TX	8995	3383	1120	584	–
Imperial, CA	6426	3664	395	576	Oct. 2013
Kern, CA	4888	2053	491	393	Oct. 2013
Los Angeles, CA	37058	15915	6728	3383	Oct. 2013
Maricopa, AZ	29519	11892	4753	1597	Feb. 2017
Miami-Dade, FL	5139	1631	2045	506	Dec. 2013
Orange, CA	13862	6180	1946	1283	Oct. 2013
Pima, AZ	4767	1474	461	211	–
Riverside, CA	4448	1489	1326	320	Oct. 2013
San Bernardino, CA	5697	2363	1278	491	Oct. 2013
San Diego, CA	17632	6119	3706	933	Oct. 2013
Travis, TX	5931	2085	634	670	June 2014

Source: ICE Secure Communities Deportations Data

Fig. S2. Map Of Large Sanctuary and Nonsanctuary Counties



Sanctuary status is measured as of December 2015. Honolulu, Hawaii, and Anchorage counties not included. Boundaries from Hughes (3).

85 centered around the date when they adopted that policy. The vertical line at zero shows the month of policy enactment. In the  
86 4-5 months after sanctuary policies took effect, deportation counts fell steeply, then stabilized at a level around 30% lower than  
87 before the policies.

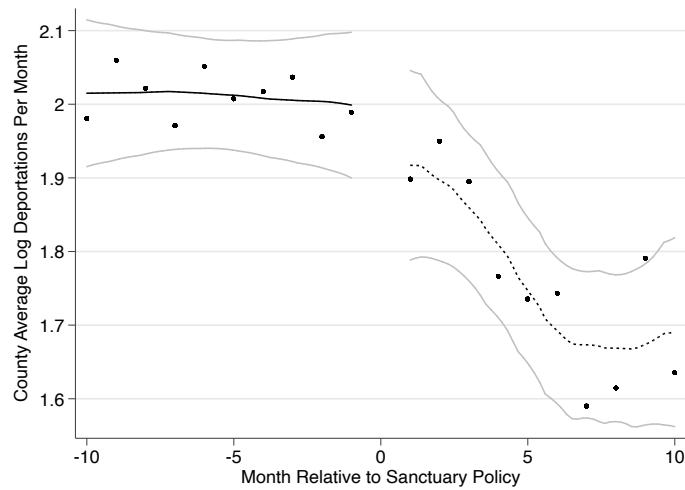
88 Next, Figure S4 compares average log deportation counts over time for California counties to the same average log counts  
89 for counties that never adopted a sanctuary policy during this period. As in Figure S3, deportations fell steeply in California  
90 after the introduction of the statewide sanctuary policy. Although trends in California and nonsanctuary jurisdictions look  
91 parallel in the year just before California's law, they are not parallel before that; the balanced panel event study specifications  
92 below address that concern by testing whether the results persist in a sample that includes only jurisdictions that eventually  
93 adopted sanctuary policies.

94 Figures S3 and S4 show descriptive evidence of the effect of sanctuary on deportations, with no need for modeling.

95 **Overall Trends in Deportations.** There were 89,661 total interior local fingerprint removals during this period and 161,536 total  
96 interior removals. Note that there were 128,304 *total* local-fingerprint removals during this period, mostly because many people  
97 apprehended at the border had had their fingerprints taken by a local jurisdiction.

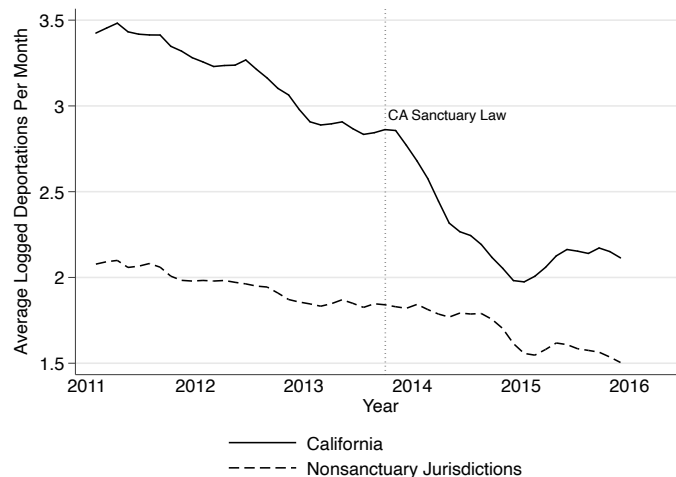
98 It is surprisingly difficult to determine how many total interior removals took place during the study period. In order to try  
99 to do so, I use a separate ICE dataset—also obtained by FOIA request—of all ICE deportations. In that dataset, ICE seems  
100 to have changed how it categorized border vs. interior removals between 2010 and 2013. Because I am uncertain about the  
101 reliability of ICE's overall count of interior removals between 2010 and 2013, I explain above only that interior local-fingerprint  
102 removals made up 55% of interior removals in 2014 and 2015. (Note that this uncertainty does not affect the main results of  
103 the study, which rely on where an individual was fingerprinted, not on how ICE categorized his or her removal.) Figures S5  
104 and S6 show the reasons for skepticism about the pre-2014 categorization. Figure S5 shows that interior deportations fell  
105 steeply between 2011 and 2013 at the same time that border deportations rose. Border deportations have a strong seasonal  
106 pattern—the dashed line forms annual spikes. Interior deportations mimic this pattern, to a lesser degree, in 2010 and 2011  
107 (and possibly slightly in 2012), but not after 2013.

Fig. S3. Pattern Before and After Policy Implementation in All Counties that Adopted Sanctuary Policies During the Study Period



Lines and 95% confidence intervals from kernel-weighted local polynomial smoothing; scatterplot shows average log deportation counts binned by month. Only eventual sanctuary jurisdictions are included.

Fig. S4. Trends Over Time In California vs. Nonsanctuary Jurisdictions

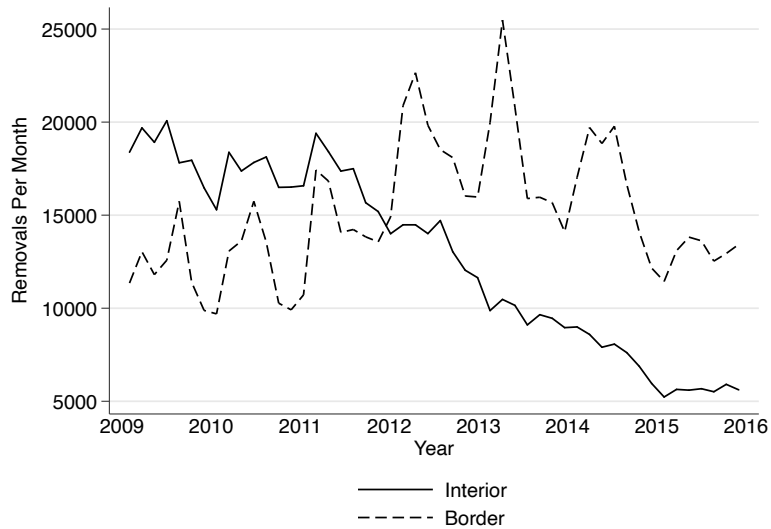


Lines from kernel-weighted local polynomial smoothing.

108 That change makes me suspect that some border deportations were miscategorized as interior deportations before 2013.  
 109 Figure S6 cements that suspicion. At the same time as interior deportations were falling, “final order - inadmissible” deportations  
 110 were falling as well. As those deportations on the basis of inadmissibility were falling, deportations through the expedited  
 111 removal process were increasing to replace them. Although these categories of deportation are not identical—inadmissibility  
 112 grounds are often at issue in normal immigration court proceedings that end with interior deportations—the trends over time  
 113 suggest that ICE began to treat more inadmissibility cases as expedited removal cases. I am unsure whether this change  
 114 was only a data-keeping change or whether it reflected an increasing use of expedited removal, but either way, it resulted  
 115 in some cases previously counted as interior removals beginning to be counted as border removals. At the same time, the  
 116 number of voluntary returns—many of which were counted as interior deportations but almost certainly occurred near the  
 117 border—declined as well. Together, these changes make it very likely that the 2011 to 2013 decline in interior removals was at  
 118 least partly illusory.

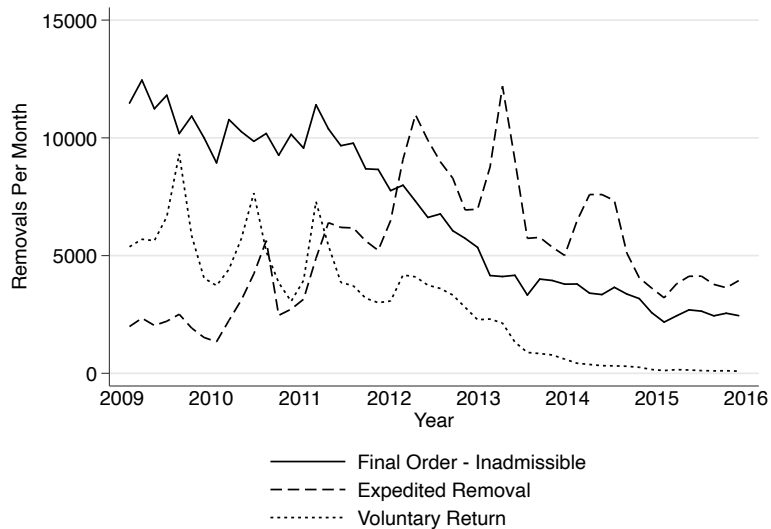
119 **Balanced Panel Results.** Although trends in California (see Figure S4) look parallel for sanctuary and nonsanctuary counties  
 120 in the nine months before policies took effect, the two sets of counties look slightly different further in the past: the coefficient  
 121 on month -10 is positive and statistically significant. One might worry that differential trends in the two sets of counties are

Fig. S5. Border vs. Interior Removals Over Time



Lines from kernel-weighted local polynomial smoothing.

Fig. S6. Types of Removals Over Time



Lines from kernel-weighted local polynomial smoothing.

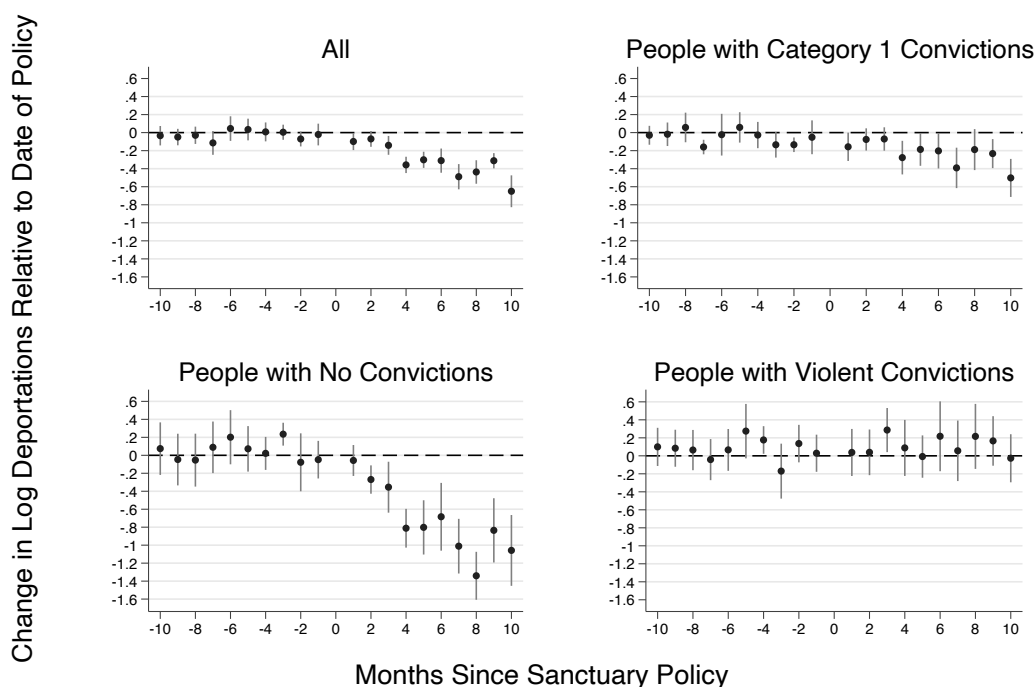
122 partly driving the results. To address this possibility, I show results for balanced panels, including only counties that eventually  
123 adopted sanctuary policies. Because some counties put in place sanctuary policies before others had become part of the Secure  
124 Communities program, I show results for two different balanced panels (see above for details on these panels). The results in  
125 Figures S7 and S8 are broadly consistent with those from Fig. 2 in the main text, and they offer evidence that the results are  
126 not driven by differing trends in counties that adopted policies and those that did not.

127 **Log Regression Results.** In this section, I show the results of linear regressions using the natural log of each county-month's  
128 count of deportations (plus one, in order to avoid omitting county-months with zero deportations). Figure S9 shows these  
129 results. Figure S9 corresponds to Table 2 in the main text; the only difference is that it employs linear regression with a logged  
130 dependent variable (counts plus 1) instead of negative binomial regression.

131 I also replicate the event study specification with logged counts (plus one) and linear regression. Figure S10, below,  
132 corresponds to Fig. 2 in the main text. Again, the only difference is that it employs linear regression with a logged dependent



Fig. S7. Effect of Sanctuary Policies on Local-Fingerprint Removals: Event Study Results, Early Balanced Panel



The plotted coefficients are from an event study specification; the y-axis shows the change, relative to the month in which a sanctuary policy was implemented, in log deportations. Coefficients are from negative binomial regression with county and month fixed effects; counties that never instituted sanctuary policies are omitted, as are counties with a first deportation after December 2010, and counties that experienced fewer than 10 months before and after the onset of their sanctuary policies during the December 2010 to December 2015 timeframe.  $N = 4,087$  county-months. Standard errors are clustered on state; months -10/10 include all previous/subsequent months.

133 variable instead of negative binomial regression. As expected, these results are most similar to the results from negative  
 134 binomial regression for the dependent variables with relatively few county-months with zero counts. Many county-months  
 135 included zero deportations of people with no convictions or violent convictions; the linear specification appears to underestimate  
 136 the effect for people with no convictions (for people with violent convictions, both linear and negative binomial regression yield  
 137 no evidence of an effect). The same basic pattern holds regardless of the type of regression: sanctuary policies led to fewer  
 138 deportations, especially of people convicted of no crimes or minor crimes.

139 **Event Study Results: Crime.** Finally, I also provide additional descriptive plots and robustness checks for the crime results. As  
 140 a first look at trends in crime across sanctuary and nonsanctuary jurisdictions, Figure S11 plots the crime rate in California  
 141 against the crime rate in jurisdictions that never adopted sanctuary policies during this period. There is no obvious pattern  
 142 around the time of the passage of the sanctuary law.

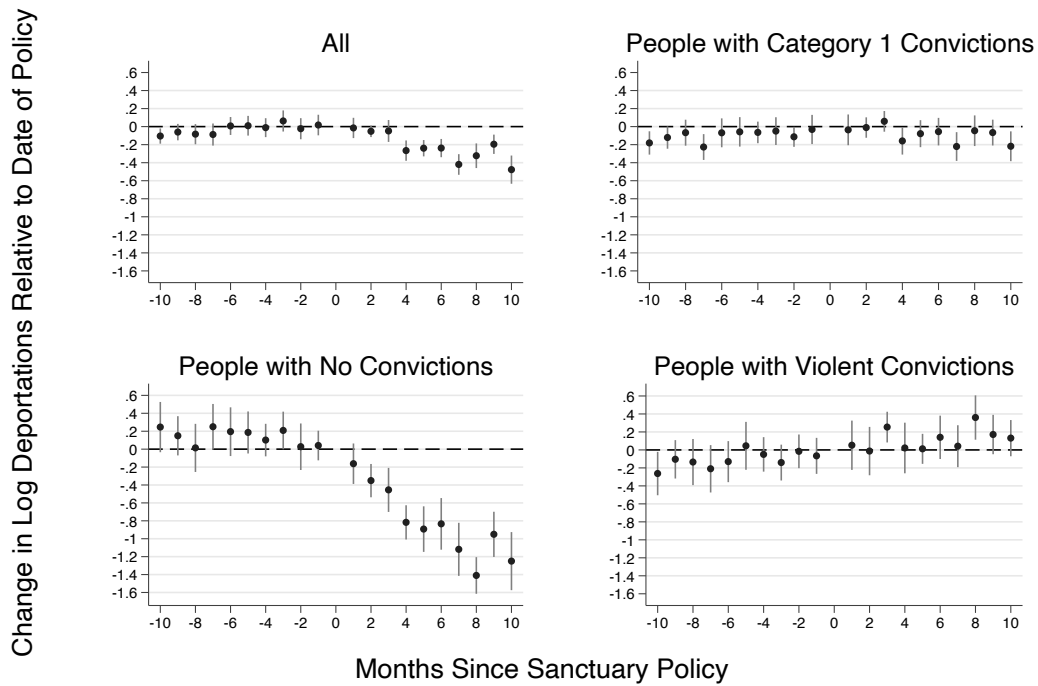
143 To check the robustness of the panel regressions presented in the main text, I show the results of an event study model  
 144 (Figure S12). The results are consistent with the panel regression results: there is no detectable effect. The confidence intervals  
 145 in the event study model displayed in Figure S12 suggest that sanctuary policies did not increase or decrease violent crime by  
 146 more than about 30 per 100,000—just over one tenth of one standard deviation and around one tenth of the median violent  
 147 crime rate (see Table 3).

148 The event study for property crime is harder to interpret; it depicts what looks like a cyclical difference in trends between  
 149 sanctuary and nonsanctuary jurisdictions. That cyclical pattern seems to repeat itself after about 12 months, suggesting that it  
 150 might be driven by seasonality. Given that California accounts for 35 of the counties in the sample, all of which implemented  
 151 sanctuary policies on the same date, and that California experiences relatively little seasonal variation in weather and crime  
 152 (see Figure S11), California might be driving this trend. The bottom panel of Figure S12 therefore shows the same results but  
 153 omits California. The differential pre-trends are less pronounced, and there is still no evidence of an effect on property crime.  
 154 The confidence intervals suggest that any effect was smaller than 150 crimes per 100,000—just over one tenth of one standard  
 155 deviation and around 5% of the median property crime rate (see Table 3). In sum, these results are consistent with the  
 156 main results presented in the text.

157 As a further robustness check, I also show event study results for crime for balanced panels of jurisdictions that eventually  
 158 adopted sanctuary policies (see Figure S13). I use the same balanced panels as those for the deportation results above, but  
 159 because not all counties consistently reported monthly crime data, the sample is smaller here. The early balanced panel includes  
 160 52 counties from December 2010 through December 2015 ( $N = 3,172$  county-months). The late balanced panel includes 95

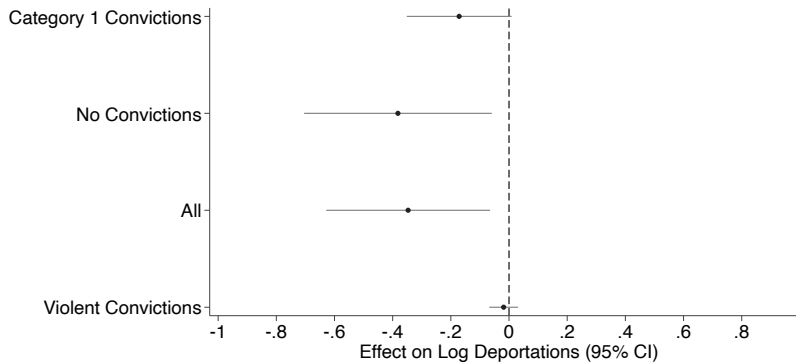


Fig. S8. Effect of Sanctuary Policies on Local-Fingerprint Removals: Event Study Results, Late Balanced Panel



The plotted coefficients are from an event study specification; the y-axis shows the change, relative to the month in which a sanctuary policy was implemented, in log deportations. Coefficients are from negative binomial regression with county and month fixed effects; counties that never instituted sanctuary policies are omitted, as are counties with a first deportation after August 2012, and counties that experienced fewer than 10 months before and after the onset of their sanctuary policies during the August 2012 to December 2015 timeframe.  $N = 5,084$  county-months. Standard errors are clustered on state; months -10/10 include all previous/subsequent months.

Fig. S9. Effect of No-Detainer Policies on Removals: Difference-in-Difference Results (Linear Regression)



Coefficients from negative binomial regression with county and month fixed effects.  $N = 18,299$  county-months. Standard errors clustered on state.

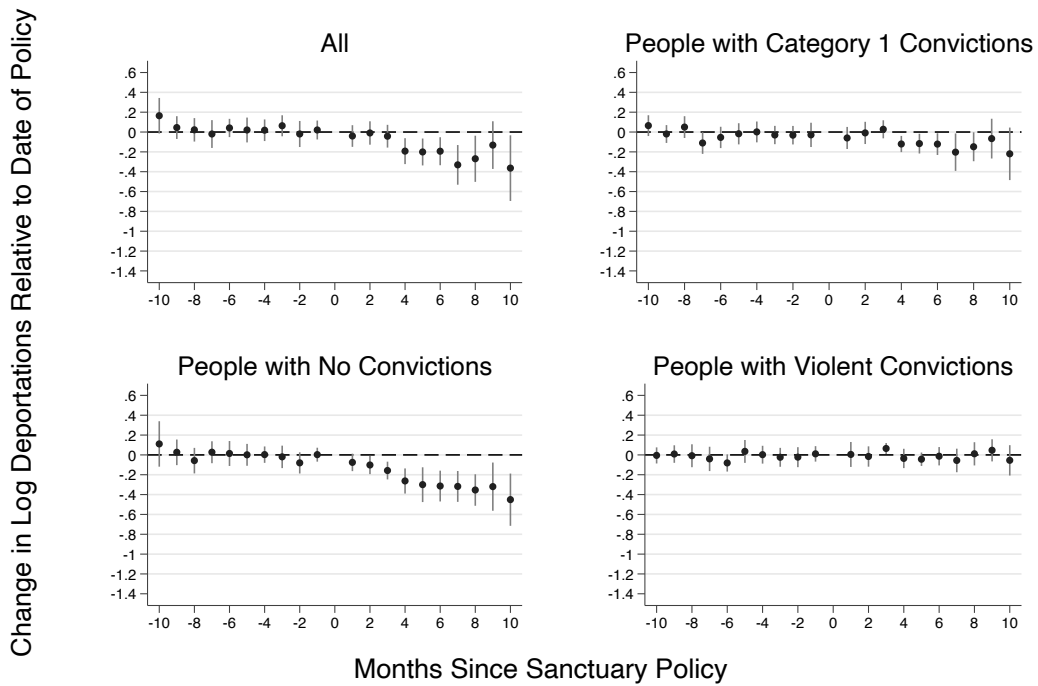
161 counties from August 2012 through December 2015 ( $N = 3,895$  county-months).

162 Generally the results are consistent with those from the main specification (see Figure S12 above). Although the late panel  
 163 results might suggest a slight decline in property crime (consistent with previous city-level estimates (4)), that panel also shows  
 164 a differential pre-treatment trend for property crime. Overall, I interpret these findings as null results.

165 **Mechanisms.** The number of deportations goes down when sanctuary policies take effect, and the number goes down more for  
 166 people without criminal convictions than for people with serious convictions. What drives these effects? In this section, I offer  
 167 evidence that sanctuary policies worked by reducing ICE arrests at jails, and that sanctuary policies had smaller effects when  
 168 they were introduced later.

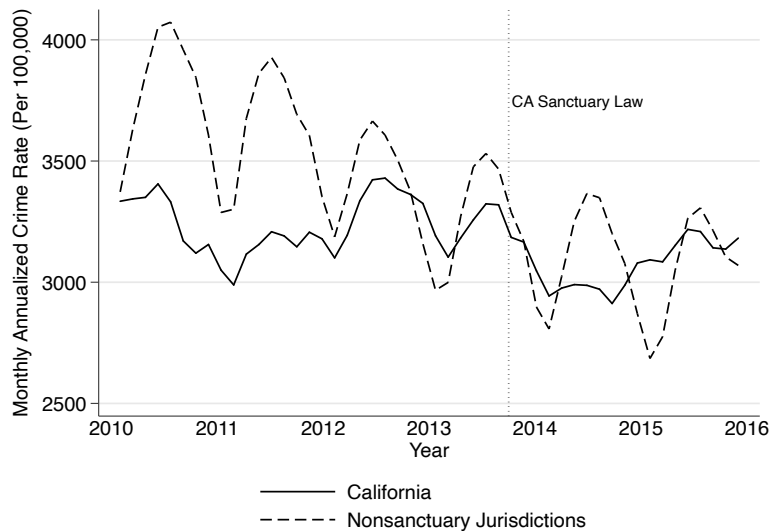
169 If sanctuary policies work by making it harder for ICE to arrest people when they're released from jails, then such policies

**Fig. S10.** Effect of Sanctuary Policies on Removals: Event Study Results (Linear Regression)



Coefficients from an event study specification using linear regression with county and month fixed effects. The dependent variable is the log of the county-month count of deportations (plus one, to include zero-count months).  $N = 18,299$  county-months. Standard errors clustered on state. Months -10/10 include all previous/subsequent months.

**Fig. S11.** Crime in California vs. Nonsanctuary Jurisdictions



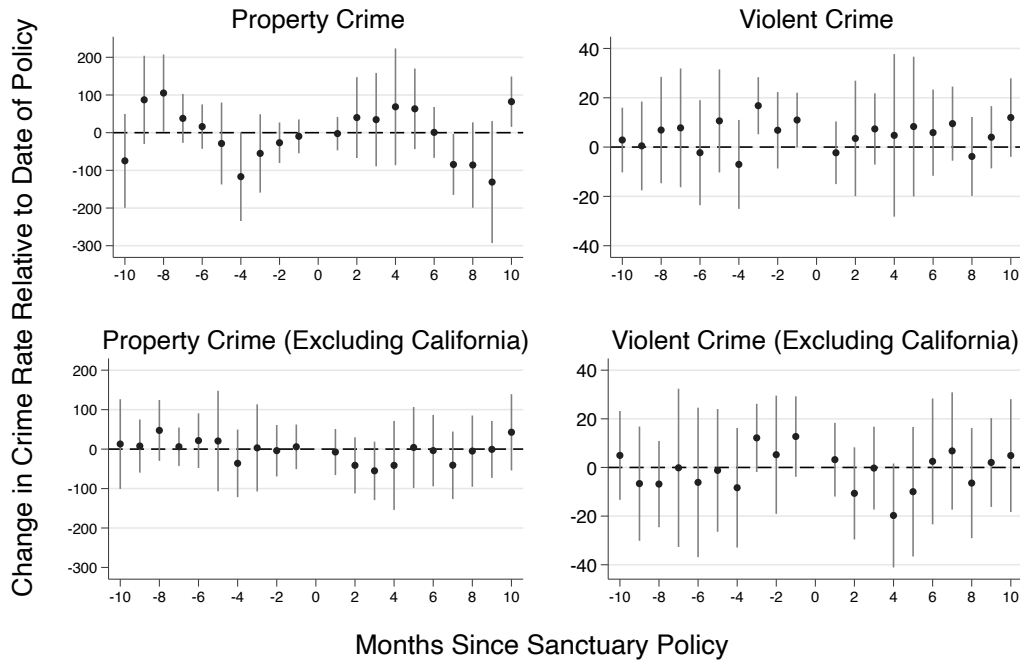
Lines from kernel-weighted local polynomial smoothing.

170 should particularly reduce the number of local-arrest deportations—those beginning with arrests in jails (i.e. arrests made by  
 171 an ICE officer associated with the section 287(g) or Criminal Alien Program).<sup>†</sup>

172 Figure S14 shows event study results where the dependent variable is the number of deportations that resulted from arrests

<sup>†</sup> Recall that the full dataset of local-origin deportations includes deportations of anyone who was ever fingerprinted by a state or local authority, even if the deportation did not follow immediately from that arrest.

Fig. S12. Event Study Estimates of Effect of Sanctuary on Crime



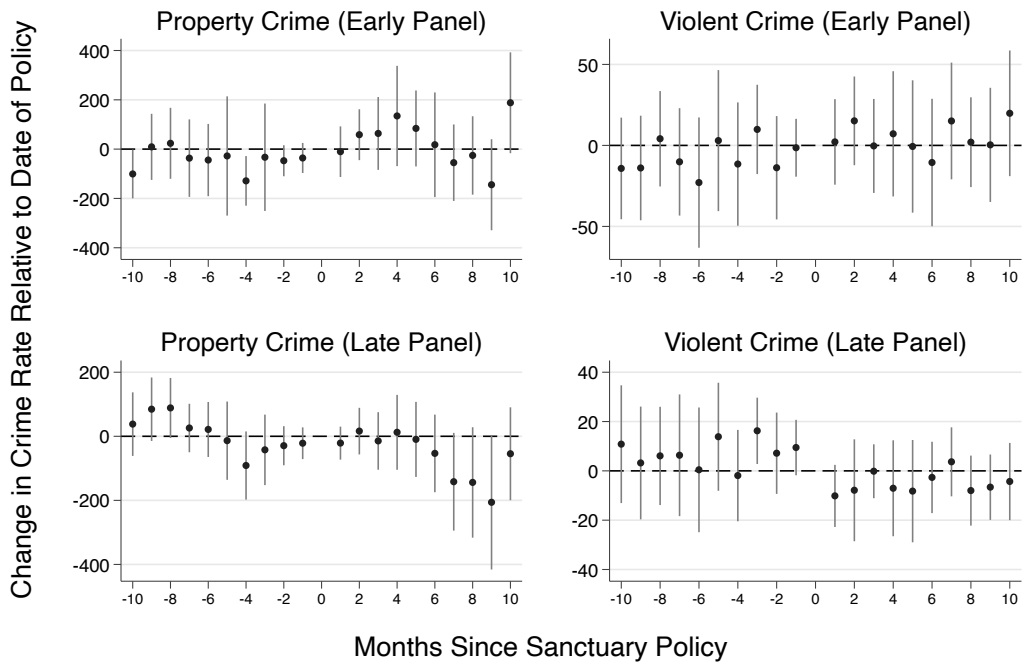
The plotted coefficients are from an event study specification; the y-axis shows the change, relative to the month in which a sanctuary policy was implemented, in the annualized crime rate. Coefficients are from linear regression with county and month fixed effects; counties that never instituted sanctuary policies are included, but their lead and lag dummy variables are set to zero, and they therefore contribute only to the estimation of the month fixed effects. Standard errors are clustered on state.  $N=13,427$  (all), 11,071 (excluding California). Months -10/10 include all previous/subsequent months.

173 in jails or prisons. Figure S14 shows a significantly larger effect of sanctuary on this more limited set of deportations than on  
 174 the full set of local-fingerprint deportations: sanctuary policies reduced all local-arrest deportations by about 40% (compared  
 175 to about a third for for all local-fingerprint deportations), and reduced local-arrest deportations of people without convictions  
 176 by around two thirds (compared to just over half for all local-fingerprint deportations).

177 It makes sense that sanctuary policies had a larger effect on deportations following arrests in jails or prisons; those policies  
 178 break the link between local custody and ICE custody.

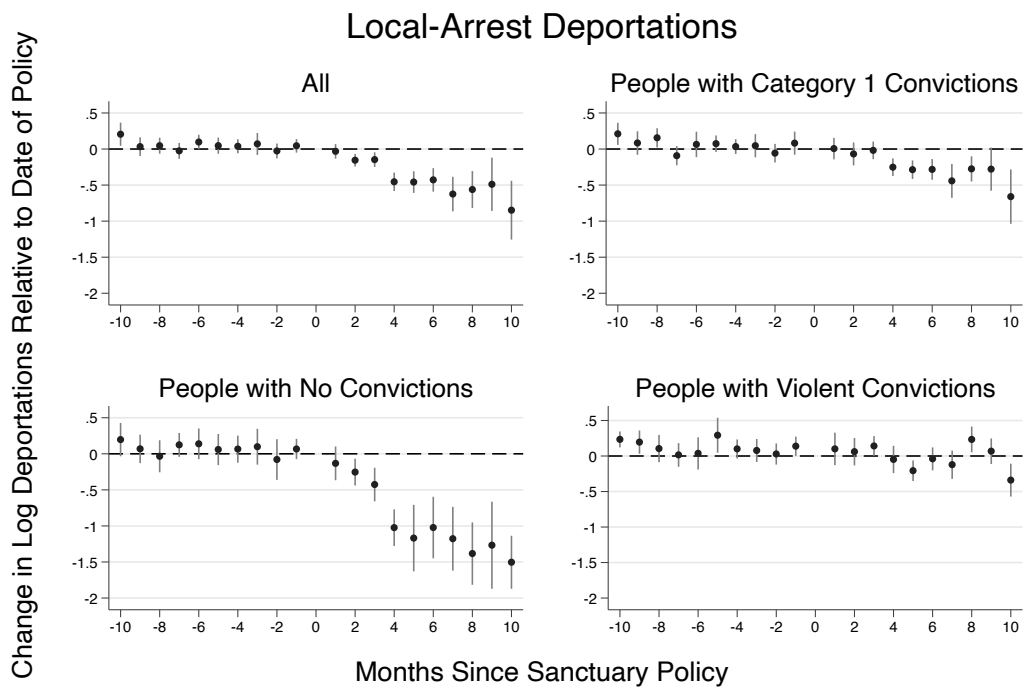
179 Finally, was the effect of sanctuary policies uniform over time? As sanctuary policies took effect in 2013 and 2014,  
 180 local-fingerprint deportations were declining everywhere, not only in sanctuary counties. And in November 2014, the federal  
 181 government changed its policy, ceasing detainer requests for people without convictions and reducing its reliance on detainees  
 182 overall. Given this changing policy environment, one would expect counties that adopted sanctuary policies earlier to have  
 183 reduced deportations more: earlier on, there were more deportations to prevent. Figure S15 compares the size of the effect  
 184 on local-arrest deportations for counties that introduced policies before vs. after June 2014. The effect for earlier-enacting  
 185 counties was about twice as large as for later ones.

Fig. S13. Event Study Estimates of Effect of Sanctuary on Crime, Balanced Panels



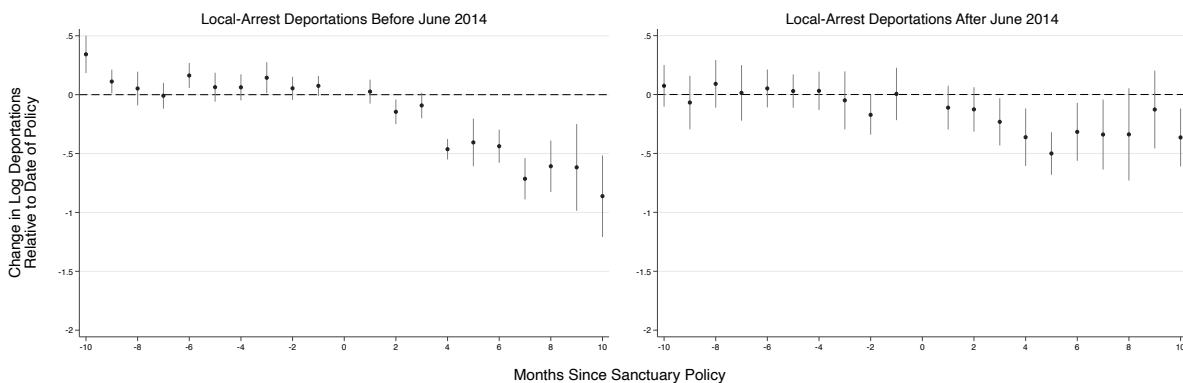
The plotted coefficients are from an event study specification; the y-axis shows the change, relative to the month in which a sanctuary policy was implemented, in the annualized crime rate. Coefficients are from linear regression with county and month fixed effects; counties that never instituted sanctuary policies are omitted. Standard errors are clustered on state.  $N=3,172$  (early), 3,895 (late). Months -10/10 include all previous/subsequent months.

Fig. S14. Effect of Sanctuary Policies on Local-Arrest Removals: Event Study Results



The plotted coefficients are from an event study specification; the y-axis shows the change, relative to the month in which a sanctuary policy was implemented, in log deportations following arrests in jails or prisons. Coefficients are from negative binomial regression with county and month fixed effects; counties that never instituted sanctuary policies are included, but their lead and lag dummy variables are set to zero, and they therefore contribute only to the estimation of the month fixed effects.  $N = 18,299$  county-months. Standard errors are clustered on state; months -10/10 include all previous/subsequent months.

**Fig. S15.** Effect of Early vs. Late Sanctuary Policies



The plotted coefficients are from an event study specification; the y-axis shows the change, relative to the month in which a sanctuary policy was implemented, in log deportations following arrests in jails or prisons. Coefficients are from negative binomial regression with county and month fixed effects; counties that never instituted sanctuary policies are included, but their lead and lag dummy variables are set to zero, and they therefore contribute only to the estimation of the month fixed effects. Standard errors are clustered on state.  $N=14,744$  (early), 13,589 (late). Months -10/10 include all previous/subsequent months.

186 **References**

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