Sanctuary policies reduce deportations without increasing crime

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The US government maintains that local sanctuary policies prevent deportations of violent criminals and increase crime. This report tests those claims by combining Immigration and Customs Enforcement (ICE) deportation data and Federal Bureau of Investigation (FBI) crime data with data on the implementation dates of sanctuary policies between 2010 and 2015. Sanctuary policies reduced deportations of people who were fingerprinted by states or counties by about one-third. Those policies also changed the composition of deportations, reducing deportations of people with no criminal convictions by half—without affecting deportations of people with violent convictions. Sanctuary policies also had no detectable effect on crime rates. These findings suggest that sanctuary policies, although effective at reducing deportations, do not threaten public safety.

immigration | crime | sanctuary

Do sanctuary policies hamper deportations of noncitizens who threaten public safety? Do sanctuary policies increase crime? These empirical questions are at the heart of a public debate. US Immigration and Customs Enforcement ("ICE") claims that "in jurisdictions where we are not allowed to assume custody of aliens from jails, . . .sanctuary cities release these criminals back to the street, [increasing] the occurrence of preventable crimes" (1). The government has often formally relied on this empirical assertion, arguing that sanctuary policies "make all of us less safe because they intentionally undermine our laws and protect illegal aliens who have committed crimes" (2). Immigrants' rights advocates, by contrast, maintain that sanctuary policies build trust between immigrant communities and the police (3, 4).

This report brings causal evidence to the public debate. Exploiting the rollout of sanctuary policies—defined as county refusals to cooperate with ICE requests to hold noncitizens beyond their release dates—in different places on different dates, I combine individual-level ICE deportation data with Federal Bureau of Investigation ("FBI") crime data to estimate the effect of sanctuary policies on deportations and on crime. I find that sanctuary policies reduced deportations of people fingerprinted by local authorities by about one-third (relative to the counterfactual trend in those counties) between 2010 and 2015. Such policies had an even larger effect on deportations of people with no convictions, which fell by over half, and had no consistent effect on deportations of people with violent convictions. Moreover, sanctuary policies had no effect on crime rates or clearance rates (the rates at which police arrest people for reported crimes).

This report's main finding—that sanctuary reduces deportations and changes their composition—contributes not only to the public debate over sanctuary, but also to longstanding scholarly debates over the effects of immigration enforcement measures (5–7) and the ability of states and localities to combat those measures (8–12). Most broadly, these findings add to the literature on immigration enforcement and policing in the United States (13–15).

This report's second finding—that sanctuary policies do not increase crime—builds on previous estimates of the effects of immigrant-friendly policies on crime rates. Martinez et al. (ref. 16, p. 9) conclude in a review that "relatively little empirical research examines the impact that local limited cooperation policies have on crime." Wong (17) shows, in cross-section, that crime rates are lower in sanctuary jurisdictions. Amuedo-Dorantes and Arenas-Arroyo (18) find that sanctuary policies make petitions for legal status under the Violence Against Women Act more likely. O'Brien et al. (10) use a matching strategy to compare crime rates in cities with and without sanctuary policies and find little difference between them. Gingeleskie (19) defines sanctuary policies broadly and finds that such policies instituted between 1995 and 2014 in 32 cities caused about a 7% decline in property crime and had no effect on violent crime. Martinez-Schuldt and Martinez (20) find that the adoption of immigrant-friendly policies across cities between 1990 and 2010 reduced robberies (but not murders).

This study improves on those previous studies in two ways. First, I assemble a larger and more comprehensive dataset of county sanctuary policies, as well as monthly (rather than yearly) crime data. Second, I use a more precise definition of sanctuary—whether a county refuses detainer requests—that allows a more precise evaluation of the effect of that policy change. Like those previous studies, I find no evidence of significant effects of sanctuary on crime. These estimates are consistent with estimates from outside the sanctuary context finding that increased immigration enforcement does not reduce crime (21–23).

In sum, this report introduces estimates of the effect of sanctuary policies on the number and composition of deportations and improves on previous estimates of the effect of sanctuary on

Significance

Opponents of sanctuary policies, including the federal government, assert that they harm public safety. This report shows that that claim is not supported by the evidence. This report estimates the effect of sanctuary on deportations, finding that sanctuary policies reduce deportations by one-third, but that those policies do not reduce deportations of people with violent criminal convictions. It also finds that sanctuary has no measurable effect on crime.

Author contributions: D.K.H. designed research, performed research, analyzed data, and wrote the paper.

Competing interest statement: Until July 2019, I was an attorney at the American Civil Liberties Union's (ACLU's) Immigrants' Rights Project, and I continue to consult occasionally for the ACLU and other immigrants' rights organizations. This study is unrelated to that consulting work, but both concern immigration enforcement.

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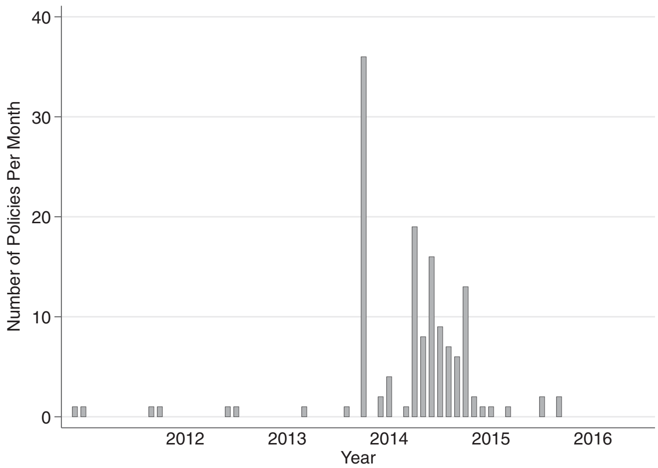

Fig. 1. Policy enactment over time.

Table 1. Summary statistics: Deportations by criminal convictions per county–month

	Total	Category 1	None	Violent
Mean	20	7	3	1
Median	5	2	1	0
SD	57	22	11	4
Maximum	969	384	257	76
N	18,299	18,299	18,299	18,299

crime. I find no evidence that sanctuary policies threaten public safety.

Materials and Methods

When a local law enforcement agency arrests someone, it takes that person's fingerprints and transmits them to the FBI. The FBI then shares data with ICE, which conducts a search of its immigration databases. If ICE determines that the arrestee could be deported—most commonly because the person is undocumented or because the crime with which the person is charged might invalidate a lawful immigration status—it issues a detainer request. That request asks the local law enforcement authority to notify ICE of when the person is going to be released and to continue to detain the person up to 48 hours beyond when the person would otherwise be released to allow ICE to make an arrest.

Since 2010, hundreds of counties have implemented sanctuary policies. The key feature of sanctuary policies is a refusal to cooperate with at least some ICE detainer requests, and I use that feature to code policies, although many policies include additional measures that might increase the cost to ICE of making arrests in jails and prisons. Common measures include, among others, decisions not to notify ICE about when noncitizens will be released, decisions not to allow ICE access to county jails, and policies barring local officials from asking arrestees about their immigration status. For detail on the variety of sanctuary policies, see refs. 11 and 24.

The principal dataset contains every ICE deportation in which an individual was fingerprinted by a county from November 2008 through December 2015. More precisely, the dataset includes all removals of individuals identified through the link between ICE and FBI databases. The dataset includes returns and voluntary departures; it does not include detentions of individuals who later obtained relief and avoided deportation. This report's analysis relies on a small number of variables that the dataset reliably captures: the county in which the individual was fingerprinted, the date of removal, the ICE-coded criminal threat level (marked "NA" when ICE records no convictions), and the individual's most serious criminal conviction as of the time of removal. (Note that the most serious criminal conviction variable includes convictions from any point in an individual's life; this may not always be the conviction that led to the individual's removal. Failure to record any conviction does indicate the lack of a conviction: For all but 8 of 71,868 deportations without a recorded conviction, ICE records a non crime-related reason for prioritization for deportation, and ICE records such a non crime-related reason in only 82 of 382,606 deportations for which a crime is recorded. For details on the data, including a copy of the raw data released by ICE, see replication archive folder [https://doi.org/10.7910/DVN/NX0SAI (25)].) The dataset includes only deportations of people who had contact with local law enforcement; people deported after at-large arrests by ICE are not included unless they previously were fingerprinted by a local authority. Because sanctuary policies make the transfer from local authorities to ICE more difficult, the dataset includes all deportations that I expect to be affected by sanctuary policies. And local-fingerprint deportations are important in their own right: In 2014 and 2015—the key period in which sanctuary policies took effect—these local-fingerprint deportations made up about 55% of all deportations from the interior of the United States (e.g., deportations not at the border). (See *SI Appendix* for more details.) This study relies only on publicly available, anonymized data, and did not involve any human subject research.

To make data collection on sanctuary policy dates feasible and to study a set of counties that regularly experienced deportations, I restricted the dataset to the largest 10% of US counties by Hispanic population (according to the 2010 census). The result is a dataset of 296 large counties that account for more than 80% of all local-fingerprint deportations from 2009 to 2018. For the crime results, I combined this dataset with FBI Uniform Crime Report data at the county–month level (26).

Of the 296 counties in the sample, 140 adopted sanctuary policies between 2010 and 2015. These 296 jurisdictions account for 300 large counties because the ICE data do not distinguish among New York City's five boroughs (counties); of the 314 counties that made up the top 10% by Hispanic population, I excluded 12 for which I was unable to find sanctuary onset dates and 2 for which census and ICE jurisdictional boundaries

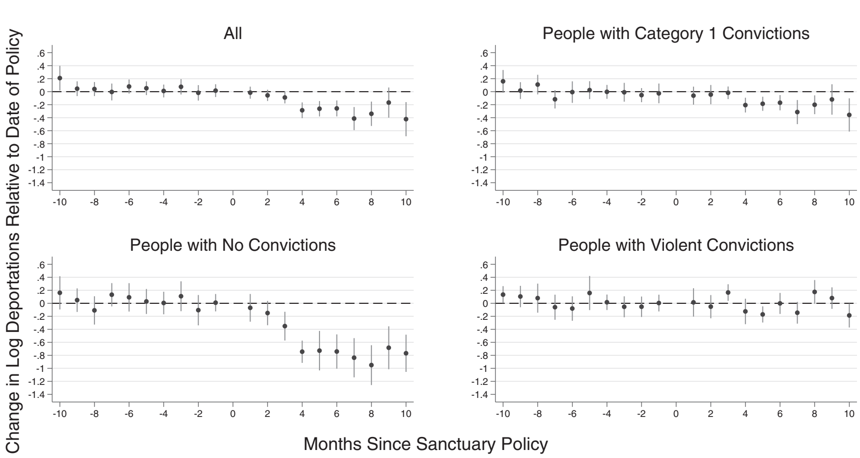

Fig. 2. Effect of sanctuary policies on local-fingerprint removals: Event study results. The plotted coefficients are from an event study specification; the *y* axis shows the change, relative to the month in which a sanctuary policy was implemented, in log deportations. Coefficients are from negative 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.

did not match (see *SI Appendix* for more details). Fig. 1 shows when the sanctuary policies took effect: They clustered in late 2013 and in 2014, with a large spike in October 2013, when California passed its statewide sanctuary act.

I ask not only whether sanctuary policies affected the number of deportations, but also how those policies affected the composition of deportations. The overall composition of deportations offers an important starting point. Table 1 shows descriptive statistics for four different dependent variables: the counts of 1) all local-fingerprint deportations, 2) local-fingerprint deportations of people convicted of ICE category 1 crimes [two crimes punishable by more than 1 y in prison or one crime categorized as an aggravated felony under the Immigration and Nationality Act (25)], 3) local-fingerprint deportations of people with no criminal convictions, and 4) local-fingerprint deportations of people with convictions for violent crimes (note that these are also included in the category 1 crimes). (I categorized the following crimes as serious violent crimes: aggravated assault, sex assaults and offenses, homicide, rape, carjacking, arson, kidnapping, lewd or lascivious acts, molestation of minor, robbery, terror related crimes, and manslaughter. I did not include nonaggravated assault or battery. This definition largely tracks the FBI index of violent crimes used in the crime results below.)

The most important lesson from Table 1 is that deportations of people with violent convictions—and people with no convictions—are relatively rare. Around half of all deportations are in a category not separately listed: people convicted of crimes, but crimes that did not rise to the level of ICE's category 1.

Results

Effect on Deportations. I expect sanctuary policies to reduce deportations. The hypothesis is simple: If jails do not hold people on detainers for ICE to make arrests, ICE will arrest and deport fewer people. Although this hypothesis is plausible, it might not be true. For example, ICE often has an informal presence at county jails and may thereby learn noncitizens' release dates and arrest them without first lodging a detainer. Or local sheriffs may post detainees' arrest dates to allow ICE to see those dates: In fact, many California sheriffs have done exactly that to evade California's sanctuary law (27). It is not obvious that sanctuary policies matter at all, and certainly not obvious how much they matter.

I also expect sanctuary policies to decrease removals more among noncitizens who have no criminal convictions or minor criminal convictions. Why? First, many sanctuary policies make exceptions for people convicted of serious crimes, allowing sheriffs to hold them until ICE arrests them. Second, throughout this period, ICE policy required that officers prioritize people with serious convictions for deportation. If sanctuary policies make deportations more costly, ICE might concentrate its resources on deportations of people with serious convictions. Third, people with serious convictions are often released only after serving a sentence (rather than after charges are dropped, for example), and ICE therefore has more time to anticipate when they will be released.

I use a difference-in-difference approach to test whether sanctuary policies reduce deportations and change their composition.[*] That research design exploits the fact that counties adopted sanctuary policies at different times, comparing the change in deportations in the months after those policies were adopted to the change in deportations in the same months in counties that did not change their policies.

The key assumption underlying a difference-in-difference research design is that the posttreatment change in the untreated units offers an estimate of the counterfactual change for the treated units—in other words, that treated and untreated units would have experienced the same trend in the absence of the treatment. Here, that assumption means that, to interpret these results causally, I must assume that absent a sanctuary policy, nonsanctuary jurisdictions would have experienced the same trend as sanctuary jurisdictions. The assumption is not testable, but one can evaluate its plausibility by examining whether treated and control units experienced parallel trends before the treatment. I check for parallel pretreatment trends in Fig. 2 and show additional checks, including event study results from balanced panels, in *SI Appendix*.

In panel regressions, I find that sanctuary policies reduced local-fingerprint deportations overall by about one-third in the counties that adopted them, relative to their counterfactual trend. (See Table 2; the percentage effect is equal to one minus the exponentiated coefficient.) But the effects vary for different groups of noncitizens. Sanctuary policies reduced deportations of people without convictions by about half (−0.68 log reduction) and appeared to reduce deportations of people convicted of violent crimes by around 16% (−0.175 log reduction). The effect on people convicted of violent crimes is an artifact of model specification, however: it does not hold up in the event study analysis in Fig. 2.

These results imply that there would have been over 22,300 more deportations nationwide from 2013 to 2015 had sanctuary policies not taken effect, which would have meant about a 15% larger overall number of local-fingerprint deportations. Among those 22,300 deportations would have been about 3,300 deportations of noncitizens without convictions—about 24% more than in fact took place.

So far, I have shown the results of simple panel regressions. But it is also possible to test how quickly the policies had an effect

Table 2. Difference-in-difference estimates: Effect of sanctuary on deportations

	All	None	Category 1	Violent
Sanctuary	−0.427**	−0.676***	−0.326**	−0.175*
	(0.135)	(0.146)	(0.114)	(0.066)
Intercept	−0.730***	−0.749***	−3.170***	−5.019***
	(0.135)	(0.153)	(0.094)	(0.069)
N	18,299	18,299	18,299	18,299

Negative binomial regression is shown; standard errors are in parentheses. Regressions include county and month fixed effects, with standard errors clustered on state. Coefficients show effects on log counts. *$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Table 3. Summary statistics on crime

	Violent	Property	Total	Clearance rate
Mean	403.05	2,891.07	3,294.12	0.23
Median	342.92	2,678.44	3,048.57	0.22
SD	266.10	1,260.35	1,458.69	0.08
Maximum	666.31	10,479.17	11,478.14	0.86
N	13,427	13,427	13,427	13,427

Statistics used are annualized rates per 100,000 population; FBI Uniform Crime Report data, 2010 to 2015 (224 large counties) (26); and annual county population data from American Community Survey (29).

[*] I estimate a standard difference-in-difference panel regression of the form

$$Y_{cm} = D_{cm} + \Gamma_c + \Lambda_m + \epsilon_{cm}. \quad [1]$$

In this model, Y_{cm} is the count of local-origin deportations by county c and month m, D_{cm} is an indicator variable for the presence of a sanctuary policy in a given county and month, Γ_c represents county fixed effects, and Λ_m represents month fixed effects. Because the dependent variable contains a skewed count distribution and many county–months include zero deportations, I use negative binomial regression to estimate the model. Following Allison and Waterman (28), I use unconditional fixed effects. I also follow Allison and Waterman's (28) suggested adjustment for the standard errors generated by these regressions. Because this is an unusual specification, in *SI Appendix* I also present results of linear regression where the dependent variable is the log of the count of deportations plus one. Those results exhibit the same overall pattern, but likely underestimate the effect of sanctuary on the types of deportations for which many or most county–months had zero counts. I cluster standard errors at the state level.

Table 4. Difference-in-difference estimates: Effect of sanctuary on crime

	Property crime	Violent crime	Clearance
Sanctuary	85.145	3.500	0.001
	(55.627)	(6.437)	(0.004)
Intercept	3123.121***	411.401***	0.225***
	(86.287)	(8.564)	(0.007)
N	13,427	13,427	13,427

Standard errors are in parentheses. Estimates include county and month fixed effects; standard errors are clustered on state. Crime rate effects are in terms of crimes per 100,000/y. ***$P < 0.001$.

and to verify that sanctuary and nonsanctuary counties were on parallel paths before sanctuary counties adopted their policies. To do so, I use an event study model—a nonparametric model that shows the effect at each month before and after policies took effect.[†]

Fig. 2 shows the results. For all deportations except those of people with violent convictions, there is a strong and consistent pattern: Sanctuary counties experience a steep drop in deportations a few months after their policies take effect. The slightly delayed effect makes sense, since there is some delay from arrest to deportation. These results also confirm that trends before the onset of sanctuary policies were relatively parallel: The coefficients before policies take effect hover around zero. As a further test of the reasonableness of the parallel trends assumption, in *SI Appendix* I estimate similar models for balanced panels including only counties that eventually adopted sanctuary policies. Results are similar. In every event study model, sanctuary policies have no effect on deportations of people convicted of violent crimes, and sanctuary policies have the largest effect on deportations of people who were never convicted of any crime.

Effect on Crime. I use FBI Uniform Crime Report data (26) to test whether sanctuary policies affected crime rates. See Table 3 for summary statistics on property crime, violent crime, and the clearance rate from 2010 to 2015 in 224 large counties. I drop 72 of the 296 counties used in the deportations analysis because they did not consistently report monthly crime data throughout this period. (I first dropped all months before the month of a county's first local fingerprint deportation [as in the removals analysis in *Effect on Deportations*]. I then dropped all counties in which agencies that reported in every month throughout the period made up fewer than 90% of all crimes. In the remaining 224 counties, agencies reporting in every month accounted for over 98% of all crimes.)

Sanctuary policies might increase crime by increasing the number of noncitizens with (mostly minor) criminal convictions present in a county. They might also encourage noncitizens to cooperate with the police, thereby increasing the clearance rate—the rate at which police solve crimes—and making police deterrence more effective. Or such policies, if they increase trust in the police, might cause reporting of crime to increase, leading to a higher reported crime rate. Finally, the policies might simply have little effect either way.

The results offer no reason to think that sanctuary policies had any effect on crime or clearance rates. Table 4 shows the results of difference-in-difference panel regressions. The estimated effect of sanctuary on violent crime is close to zero, and the estimated effect on property crime is similarly small and not significant—about 6% of 1 SD and under 3% of the median rate. Even if the actual effects were at the upper bound of the confidence interval, that would suggest an increase in property crime of about 6% over the median and an increase in violent crime of 5% over the median. Similarly, the point estimate for the effect on clearance rates is close to zero. In sum, all effects on crime are statistically indistinguishable from zero. In *SI Appendix*, I investigate trends in crime further, using descriptive figures and event study models to test the parallel trends assumption; results are consistent with these main results.

Discussion: Mechanisms and Limitations

These findings—that sanctuary decreased deportations, particularly among noncitizens with no convictions or minor convictions, and that it had no detectable effect on crime—leave

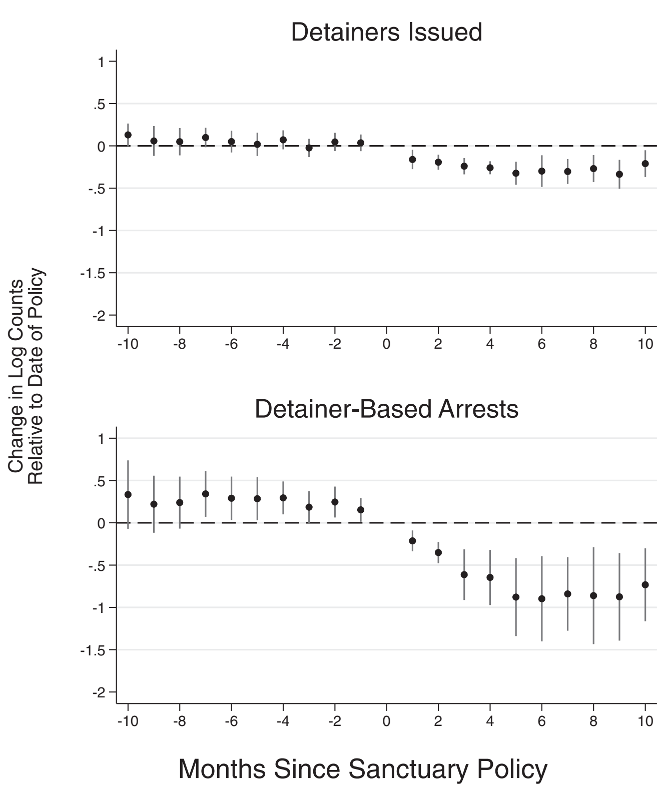

Fig. 3. Detainer and detainer-based arrests: Event study results. The plotted coefficients are from an event study specification; the y axis shows the change, relative to the month in which a sanctuary policy was implemented, in log counts of detainers issued and arrests made on the basis of detainers. 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 contribute only to the estimation of the month fixed effects. N = 18,187 county–months. Standard errors are clustered on state; months -10/10 include all previous/subsequent months.

[†] I estimate event study models of the form

$$Y_{cm} = \sum_{t=-10, t\neq 0}^{t=10} \delta_t D_{cm}^t + \Gamma_c + \Lambda_m + \epsilon_{cm}. \quad [2]$$

In this model, Y_{cm} is the count of local-fingerprint deportations by county c and month m, Γ_c represents county fixed effects, Λ_m represents month fixed effects, and D_{cm}^t is a set of dummy variables in which each variable represents a number of months t before or after the onset of a sanctuary policy (e.g., one dummy variable represents 1 mo before treatment, another one represents 1 mo after treatment, and so forth; the month of treatment itself is the omitted level). In counties that never adopted a sanctuary policy between November 2008 and December 2015, $D_{cm}^t = 0$ for all t—these counties contribute only to the estimation of the month fixed effects. Again, standard errors are clustered at the state level, and again, I estimate the model with negative binomial regression but include, in *SI Appendix*, results from a linear model with a logged dependent variable.

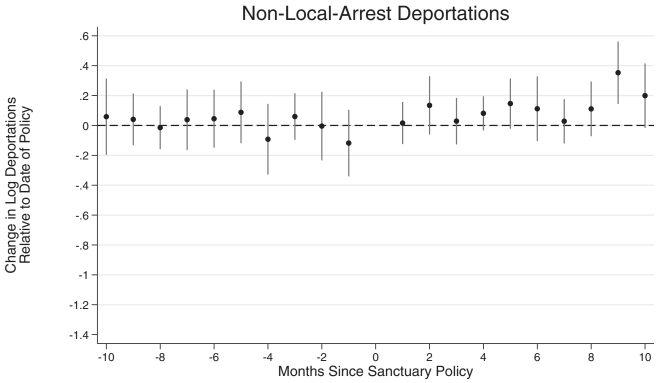

Fig. 4. Effect of sanctuary policies on removals not following local arrests: Event study results. The plotted coefficients are from an event study specification; the *y* axis shows the change, relative to the month in which a sanctuary policy was implemented, in log local-fingerprint deportations not 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 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.

open two questions. First, did deportations decrease because counties began refusing to comply with detainer requests, or did deportations decrease for some other reason (for example, that sanctuary counties became generally more hostile to ICE)? Second, as ICE reduced deportations from local jails, did it substitute other types of deportations?

To answer the first question—whether refusals to comply with detainer requests caused the decrease in deportations—I test directly whether sanctuary policies reduced the number of detainer requests and the frequency of arrests following detainer requests. (For these results, I turn to a separate dataset, described in more detail in *SI Appendix*, tracking every detainer request issued by ICE and indicating whether that request resulted in a person being booked in to ICE custody.) Fig. 3 shows that sanctuary policies led ICE to issue about 20% fewer detainers in sanctuary counties. Fig. 3 also suggests that sanctuary policies reduced the number of book-ins after detainer requests by around two-thirds. Fig. 3, *Bottom*, however, also shows differential pretreatment trends: In the 2 mo before the 0 mo, when the policies took effect, detainer-based arrests were already decreasing significantly in sanctuary counties. (If pretreatment trends were parallel, we would expect to see the pretreatment coefficients hover around zero, as in Fig. 3, *Top*.) These pretreatment trends are less troubling than they seem, however. The only date available in the detainers data is the date on which the detainer was issued, which can be months before an arrest actually takes place. Suppose, for example, that ICE issued a detainer the month before a sanctuary policy took effect, but the noncitizen was released a month after the policy took effect and therefore benefited from the policy. That detainer request was affected by the sanctuary policy even though the detainer issuance date was before the policy effective date. As a result, the pretreatment trend might be an artifact of the delay between detainer issuance and arrest. Still, that trend complicates the interpretation of the detainer-based arrest results. Together with the effect on detainer issuance, however, there is clear evidence that sanctuary policies reduced the effectiveness of detainers.

In sum, these results show the same patterns as the deportation results: Sanctuary policies reduced the rate at which ICE detainer requests resulted in arrests and caused ICE to reduce how many detainer requests it issued. The importance of detainer requests also helps explain why the effect of sanctuary policies appears to have decreased over time (*SI Appendix*), as the Obama administration reduced its use of detainer requests.

To answer the second question—whether ICE substituted other forms of deportations for those from detainers—I divide the dataset into two parts. The first part is a set of deportations that occurred following arrests in jails or prisons, and the second part is a set of deportations that occurred as a result of any other type of arrest. If ICE substituted non–detainer-related deportations for those using detainers, one would expect an increase in the number of deportations of people not arrested in jails or prisons. Fig. 4 shows the results: Deportations not following local arrests may have increased slightly following the introduction of sanctuary policies, but if the effect exists at all, it is much smaller than the treatment effect.

Here, however, a limitation of the dataset bears emphasis: The deportations dataset includes only deportations of people fingerprinted by a county. That means that the dataset is missing deportations of people who were never fingerprinted by a county and therefore does not include most deportations of people arrested outside a jail or prison. The dataset does include some of those deportations, however—many noncitizens were fingerprinted by a county and then arrested by ICE in a separate incident. It is this subset of deportations that did not increase significantly after sanctuary policies took effect, suggesting little if any substitution.

A final limitation of these results is that the lack of a detectable effect on crime does not rule out a small effect. Even a small effect is unlikely, however, simply because of the relative scale of crime and deportations. If we were to assume (unreasonably) that every one of the approximately 22,300 people whose deportations were prevented by sanctuary between 2013 and 2015 went on to commit a property crime, that would have led to an additional approximately 22,300 property crimes in sanctuary jurisdictions in that period. But there were at least 4,391,667 property crimes in those counties during this period. (I say "at least" because the FBI lacks consistent crime data for 72 of the 296 counties during this period, and crimes in those counties are therefore not counted.) As a result, even if that extreme assumption were correct, sanctuary would have increased property crime by only 0.5%.

The debate over sanctuary has depended more on assertions than on facts. This report demonstrates 1) that sanctuary policies work, reducing local-fingerprint deportations by one-third; 2) that those policies have the largest proportional effect on deportations of people with no convictions or minor convictions; and 3) that sanctuary policies have little if any effect on crime.

Data Availability. All data and code necessary to replicate these findings have been deposited at Harvard Dataverse, https://doi.org/10.7910/DVN/NX0SAI (25).

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