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ORIGINAL ARTICLE

Border Walls and Crime: Evidence From the Secure Fence Act

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Abstract
Despite a lack of rigorous empirical evidence, reduced crime is often touted as a potential benefit in the debate over increasing border infrastructure (i.e., border walls). This paper examines the effect of the Secure Fence Act of 2006, which led to unprecedented barrier construction along the US–Mexico border, on local crime using geospatial data on dates and locations of border wall construction. Synthetic control estimates across twelve border counties find no systematic evidence that border infrastructure reduced property or violent crime rates in the counties in which it was built. Further analysis using matched panel models indicates no effect on property crime rates and that observed declines in violent crime rates precede barrier construction, not the other way around. Taken together, this paper finds that potential crime reductions are not a compelling argument toward the benefits of expanding border infrastructure.

Keywords Border wall · Crime · Migration · Synthetic control

JEL Classification K42 · H54 · F22

"The border city of El Paso, Texas, used to have extremely high rates of violent crime – one of the highest in the country, and considered one of our nation's most dangerous cities. Now, immediately upon its building, with a powerful barrier in place, El Paso is one of the safest cities in our country."
Former President Donald Trump
State of the Union address
February 5, 2019

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Introduction

In the last two decades, the total number of global migrants has increased by nearly 50%, to 258 million (United Nations Population Division 2017). In response to this increase, many countries hosting migrants have focused on increasing border security and regulating migration. In the USA, home to around 50 million migrants, increased border security was a key focus of the Trump administration.[1] Between 2004 and 2017, the US Customs and Border Patrol's enacted budget increased from \$6.0 to \$14.3 billion, a 138% increase (US Department of Homeland Security 2019). Since the passage of the 2006 Secure Fence Act, a key element of efforts to increase border enforcement has been the construction of a wall along the US–Mexico border.[2] While the costs of such infrastructure investments are known (and economically significant), the benefits of border wall expansion are difficult to quantify. Proposed benefits by proponents of border wall expansion can be classified in terms of labor market effects (wage and employment effects on native populations from reduced migration), public expenditure effects (changes in public program spending from changes in migrant flows), and effects on crime and safety. Allen et al. (2019) estimate that expansion of border walls along the US–Mexico border between 2007 and 2010 harmed both Mexican workers and US high-skill workers. US low-skill workers had welfare gains equivalent to a \$0.28 increase in per capita income, far below the \$7 per capita cost of border wall construction that they estimate.

Beyond the labor market effects of border walls, recent public discourse has focused on the crime and safety effects of these barriers, especially after former President Trump made the unsubstantiated claim that the border wall had greatly reduced crime in the city of El Paso during his State of the Union address in 2019. While there is no shortage of opinions on crime in border regions, there is little rigorous empirical evidence on the potential impact of border infrastructure on local crime.

The present study addresses this gap in the literature and in the political discourse. We examine the unprecedented expansion of border infrastructure built along the US–Mexico border following the passage of the 2006 Secure Fence Act on county-level property and violent crime rates. In order to overcome differences in both crime rate levels and trends in border counties (compared to non-border counties) prior to 2006, we utilize synthetic control matching to assess the effect of constructing a border wall on crime across twelve border counties. Overall, we find that construction of border fencing has no appreciable effects on either property or violent crime rates. Across 24 estimates, we find only one that is significant (violent crime in Santa Cruz County, Arizona) – all others are not significant (nor even of consistent direction). Leveraging spatial and temporal data on completion timing, we also examine whether our overall effects may be masking heterogeneity

[1] Advocacy for border walls continued beyond the end of the Trump presidency, as evidenced by Texas Governor Greg Abbot's push to use state funds to build border walls in Texas.

[2] Throughout this study, we use the terms "wall," "barrier," and "fence" interchangeably.

from construction periods and post-construction periods and conclude that our lack of results is not driven by offsetting effects.

From our synthetic weights, we create matched panels to test whether changes in crime rates correspond to the timing and extent of barrier completion. Across a variety of specifications, we find no significant reduction in property crimes as a function of barrier construction. While we do observe declines in violent crime rates in some specifications, these declines actually precede construction, rather than following it. After a barrier is built, we find no evidence of sustained declines in either property or violent crime rates.

We also conduct our synthetic control analysis on arrest rates for all crimes and for drug crimes in particular. Proponents of barriers along the US–Mexico border often argue that they would be effective at reducing drug trafficking. We find very little evidence that barrier construction has led to reduced drug arrests. We also provide evidence that our findings are not attenuated by local crime displacement (e.g., crime spilling over to adjacent non-border counties). Taken together, we conclude that the post-2006 border wall expansion did not lead to sustained crime reductions in border counties.

Our paper contributes to a growing literature studying various effects of increased border enforcement and border barrier construction in particular.[3] One recent study by Sandner and Wassman (2018) evaluates whether the abolition of border controls at the eastern German and Austrian borders accompanying the implementation of the Schengen Treaty in December 2007 increased crime rates in the border areas of these countries. They find that the elimination of these border controls led to an increase in burglaries, with other forms of crime unaffected by loosening border controls. Miles and Cox (2014) considers the effect of the Secure Communities policy that allows the federal government to check the immigration status of every person arrested by local police and to deport migrant arrestees. Utilizing the staggered rollout of this program across the USA, they find no observable impact of this policy on crime rates.

A related literature considers the effect of migration on crime. This is relevant to our research question if increased border enforcement affects migration. A recent study by Feigenberg (2019) provides evidence that the addition of fencing along the US–Mexico border has in fact led to 26–39% reduction in migration in counties where fences were constructed.This supports earlier work by Carrión-Flores and Sorensen (2006), who find that increasing the intensity of enforcement by the US Border Patrol in specific locations not only reduces migration overall, but also diverts migrants toward locations with less enforcement. Another study by Hoekstra and Orozco-Aleman (2017) estimates that the passage of Arizona's SB 1070, one of the most restrictive state immigration laws ever passed, reduced the flow of undocumented migrants to Arizona by 30–70%.[4]

[3] For example, Allen et al. (2019) look at the labor market effects of building walls along the US–Mexico border.

[4] An advantage of using county-level data in our analysis is that we can effectively control for immigration policies such as SB 1070 that are enacted at the federal and state level, as our analysis focuses on individual counties within states.

Though increased border enforcement may reduce migration, the existing literature has not established a clear relationship between migration and crime. Several studies have found that increased migration is associated with higher rates of property crime, but no appreciable change in violent crime. For example, Bianchi et al. (2012) look at Italian provinces and find that only burglary rates rise with immigration. Similar results are found by Bell et al. (2013) for the UK, by Alonso-Borrego et al. (2012) for Spain, and by Spenkuch (2014) for the USA. Other studies have found no relationship between migration and crime. Butcher and Piehl (1998) look at US metropolitan areas in the 1980s and find that new immigrant inflows did not significantly increase crime. In a later study, Butcher and Piehl (2007) find that recent immigrants actually have lower incarceration rates than natives. Chaflin (2014) addresses the potential endogeneity of migrant location choice by leveraging spatial and temporal variation in rainfall in Mexico with the persistence of regional Mexico–US migration networks. Even after controlling for the fact that migrants are more likely to go to areas with less crime, Chaflin finds no evidence of any links between migration and either property or violent crime.

The rest of the paper proceeds as follows. The next section discusses the history of enforcement along the US–Mexico border. This is followed by a theoretical model linking the construction of a border barriers to crime. We then present the data used in our analysis and follow with a discussion of our empirical methodology. Estimates are presented and discussed, and we conclude with a discussion of the policy relevance of our results.

A History of US–Mexico Border Policy

The border between Mexico and the USA is the most frequently crossed in the world, with nearly 200 million border crossings in 2018 alone.[5] At a length of 1,954 miles, the border runs through four US states (Arizona, California, New Mexico, and Texas) with a total of 23 counties adjacent to the border. There are officially 50 legal ports of entry along the border and about a third of the border currently has some sort of barrier in place.

Restrictions on migration from Mexico to the USA are a fairly recent phenomenon. From 1942 to 1964, migration was encouraged under the Bracero program. This program was designed to facilitate Mexican migrants to work in the USA on short term, primarily agricultural labor contracts. During this period, over 4.6 million contracts were signed, with many individuals returning several times on different contracts.[6]

While the Bracero program ended with the 1965 Immigration and Nationality Act, it was not until the 1986 Immigration Reform and Control Act (IRCA) that the

[5] Authors' calculations based on total crossings of bus passengers, pedestrians, and vehicle passengers across 27 different legal ports of entry along the US–Mexico border for the year 2018. Data source is US Department of Transportation (2019).

[6] UCLA Labor Center (2019).

federal government enacted legislation actively seeking to curb illegal immigration. This law made knowingly hiring undocumented immigrants illegal and authorized a significant expansion of Border Patrol activities. During the early 1990s, there was an increase in the number of border patrol agents in the El Paso and San Diego sectors. Fencing also began to be erected in these areas, though there was limited work completed (Nuñez-Neto and Viña 2006). In 1996, the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) authorized the US Department of Justice to construct fencing along the border and mandated additional construction in the San Diego sector. However, only nine of the mandated fourteen miles of fencing were completed (Hanson and Spilimbergo 1999). By late 2005, only a total of 78 miles of pedestrian fencing had been constructed along the entire border (Stana et al. 2009).

Expansion of border fencing began with the Real ID Act of 2005 that authorized the Department of Homeland Security (DHS) to waive any laws (such as environmental regulations) that impeded the construction of security barriers along the border. This was followed by the 2006 Secure Fence Act, which called for the construction of 700 miles of double-layered pedestrian fencing along specifically designated segments of the border. Interestingly, the legislation was quickly amended to require that a minimum of 700 miles of fencing be constructed where it would be "most practical and effective." These relaxed restrictions on the location of fencing were driven in part by concerns that DHS would not meet the timeline set out in the original legislation, as only 71 miles of new fencing had been completed by September 2007 (Stana et al. 2009). With relaxed restrictions on where the fence could be built, construction proceeded rapidly. By April 2010, 262 miles of pedestrian fence and 227 miles of vehicle fence had been constructed. Thus, the majority of border fencing was completed during the relatively short period between 2006 and 2010. A minimal amount of fencing has been built since 2010, though the Trump administration sought out funding to significantly increase construction of border walls.

According to de-classified government documents discussed in Feigenberg (2019), the fence construction locations were chosen based as much on ease and cost than on security concerns. For example, the US CPB expressed frustration that fencing was being built not in areas that were their operational priorities, but rather in areas that were advantageous to meet construction deadlines. This suggests that endogeneity of fence location is less of a concern. Had fencing been built in response to crime, then we would be worried about reverse causality. However, it appears that fencing decisions were based more on ease of construction, so we feel more confident about treating the construction of fencing as an exogenous shock.

A Model of Border Walls and Crime

To analyze the link between border enforcement and crime, we begin with the standard rational choice model of crime participation introduced by Becker (1968) and revised by Ehrlich (1973). The basic idea of this model is that individuals will engage in criminal activity if the returns from committing a crime exceed those from participating in the legal market. Crime is risky, so we must calculate the returns to criminal activity relative to the probability of getting caught and the expected sanction if caught. This relationship is summarized as

$$(1 - p_j)U(Crime_j) - p_j U(S_j) > U(Legal_j) \tag{1}$$

We use the subscript j to denote the particular location (US county in our analysis). The probability of being caught in the crime is p_j, and the monetary value of committing the crime is $Crime_j$. The monetary equivalent of the sanction if caught is S_j, and the amount that could have been earned in the legal job market is $Legal_j$. Crime will therefore increase if the probability of capture falls, if the value of crime rises, if the sanction falls, or if the value of legal activity falls. Following Soares (2004), crime rates will be higher in regions with more conditions that make crime attractive.

Increased border enforcement may influence each component of this model. Increased enforcement could cause the probability of capture to rise since there is a larger policing presence at the border. There could be positive spillover effects that increase the ability to detect crime. For example, building a border wall could allow local police departments to divert resources away from the border and toward detecting criminal activity. On the other hand, if resources are funneled into the construction of a border wall, this may come at the expense of local policing. There may also be an element of moral hazard in which people may incorrectly assume that building a wall will reduce crime and as a result be less vigilant about detecting crimes unrelated to the border. Dudley (2018) points to trust as another potential reason why increased border enforcement may reduce the effectiveness of local policing. If increased enforcement is done in an adversarial way, there will be less trust between authorities and migrant communities. As a result, migrant communities will be less willing to work with law enforcement against organized crime. In addition, there is likely to be less reporting of crimes such as domestic abuse by those vulnerable to immigration enforcement.[7]

The value of committing a crime is likely to rise with increased border enforcement. Roberts et al. (2010) find that smuggling costs along the border have increased alongside enforcement. Thus, the construction of a border barrier will make that particular location a more lucrative crossing point for smugglers. This in turn strengthens criminal organizations that actively participate in human smuggling. Once a human smuggling network is set up, then there is an infrastructure for trafficking other illegal goods and services. That increased border enforcement likely strengthens criminal organizations is supported by evidence from Orrenius and Coronado (2015), who find that violence along the border has increased alongside greater enforcement efforts. Laughlin (2019) provides evidence that enforcement may have spillover effects in nearby areas. Following the construction of a border fence, there is no significant increase in violence in Mexican localities near the fence. However,

[7] For example, Amuedo-Dorantes and Arenas-Arroyo (2019) find that immigrants are more likely to report domestic violence in "sanctuary cities," locations with policies that limit the cooperation of local law enforcement with Immigration and Customs Enforcement.

there is an estimated increase of 1000 deaths in localities that provide alternative smuggling routes into the USA.

The effect of increased border enforcement on the value of participating in the legal market is less clear. On the one hand, increased enforcement has been shown to reduce migration. With less migration, there is likely to be less competition for jobs in the unskilled labor market. As this is the segment of the population more likely to engage in criminal activity, we are likely to observe a reduction in crime as there are more legal opportunities for would-be criminals. In addition, increased enforcement may provide a short-run local stimulus to the economy with greater federal spending on border barriers and patrols. As economic opportunities rise in the legal market, we should observe a drop in criminal activity.

However, the reduction in migration could also have negative effects on the local economy. With an economic downturn, legal opportunities could diminish. Furthermore, increased border enforcement could serve to simply keep undocumented migrants on the US side of the border, whereas they would have been more likely to cross back and forth with changes in seasonal employment before. With more undocumented migrants staying on the US side after a barrier is built, there will be more people pushed into informal labor markets. Those in informal markets lose out on legal labor protections and are more susceptible to extortion by criminal enterprises. Finally, increased border enforcement tends to lead to more deportations and family separations. Youth that are exposed to this kind of trauma are themselves more likely to participate in organized criminal activity (Mok et al. 2018).

Thus, there are several theoretical mechanisms through which increased border enforcement could affect crime, but we cannot make a definitive prediction about the direction of the effect. If increased border enforcement leads to improved local policing and better opportunities in the legal market, crime should fall. However, increased enforcement could also deteriorate local policing, increase the value of criminal activity in border counties, and possibly even reduce legal opportunities. In this case, border enforcement is likely to increase crime. Furthermore, there may be regional differences in these effects, with some areas experiencing changes that are conducive to more crime and others with those leading to less. Our analysis considers both the average treatment effect across the entire border and the effect in individual counties, allowing local effects to be a function of a complex set of factors.

Data on Border Construction, Crime, and County Characteristics

This paper utilizes data across a number of different sources. Our data on border construction come from Castañeda and Quester (2017). These spatially explicit data provide details on the location, type, and completion dates of individual fence segments for the entire border fence. The fence types consist of pedestrian barriers (primary, secondary, and tertiary) and vehicle barriers (temporary and permanent). We focus on primary pedestrian barriers (as secondary and tertiary barriers are built behind primary barriers) and both types of vehicle barriers. We relate segments to counties based on their spatial location and aggregate fence segments based on type, year completed, and county to which they pertain. Figure 1 presents the cumulative

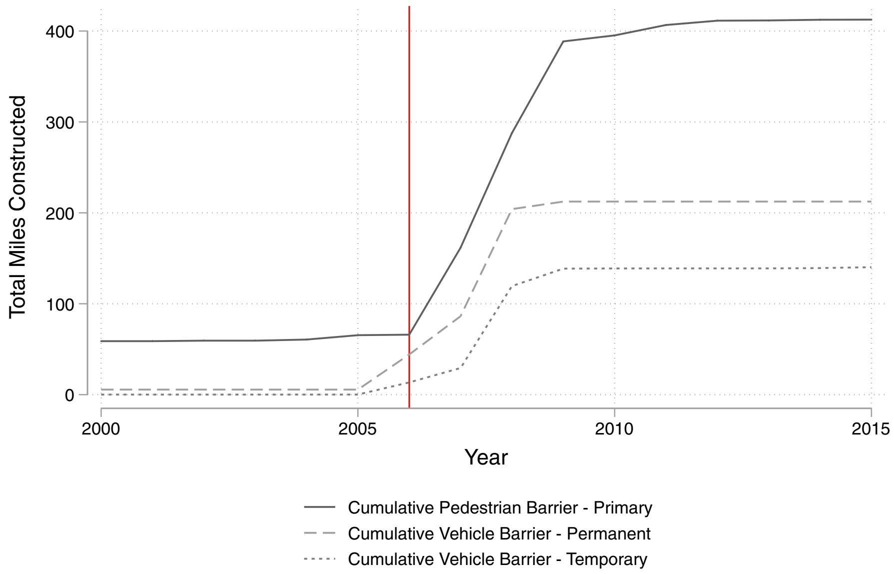

Fig. 1 Cumulative barrier construction by type. This figure presents the cumulative barrier construction by year for the US–Mexico border. The red line indicates the passage of the Secure Fence Act in 2006. (Color figure online)

barrier construction over time. Prior to 2005, little new construction took place and there existed minimal vehicle barrier and approximately 65 total miles of primary pedestrian barrier. After the Secure Fence Act of 2006, construction dramatically increased. By 2015, the total primary pedestrian barrier exceeded 400 miles and the combined vehicle barrier extended over 350 miles. This construction was not evenly spread across border counties. Figure 2 presents construction over time as a share of a county's border with Mexico. Across the 16 counties that experienced border construction over this time, the total construction ranged dramatically from over 100 miles (Pima and Yuma counties) to less than 3 miles (Maverick, Val Verde, and Webb counties).

We collect data on crime rates from the Federal Bureau of Investigation's Uniform Crime Reports (UCR) from 2001 through 2014. We gather statistics on both property crime and violent crime for all counties in states along the US–Mexico border. The crime rate is calculated as the total number of crimes committed (property or violent) per 1000 inhabitants in a particular county. Across the entire sample period, the average violent crime rate was 2.6 per 1000 residents, while the average property crime rate was 19.6 per 1000. For counties adjacent to the border, these rates were 3.5 and 27.0 per 1000 for violent and property crime, respectively. Thus, crime rates in general are higher for border counties than non-border counties in the four states we examine. We have a total of 300 counties in our sample, of which 256 have data on crime for every year in the sample (Fig. 3).

Our final data source is the US Census Bureau, from which we collect county-level data on the labor force, unemployment rate, per capita income, and

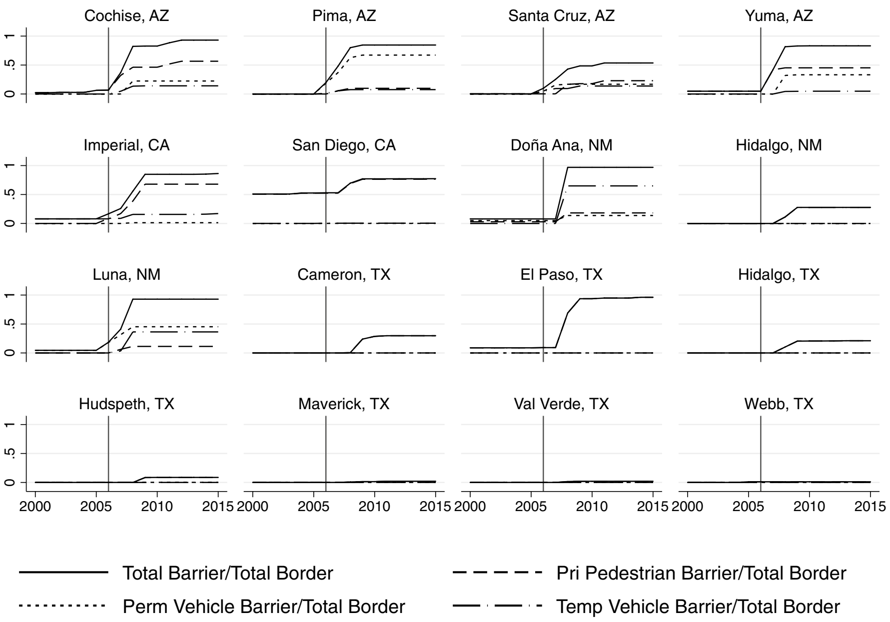

Fig. 2 County-level ratio of barrier to US–Mexico border by barrier type. *Notes* This figure presents the cumulative barrier construction by year for counties along the US–Mexico border. Each plot represents the ratio of barrier length to length of border potted for each year in our sample. The solid line is for the sum of primary pedestrian border, temporary and permanent vehicle barriers; the different dotted lines present the individual categories separately. Vertical lines indicate the passage of the Secure Fence Act in 2006

employment shares in agriculture, government, and manufacturing. These variables are used as pre-treatment matching characteristics for our synthetic control model. For example, we include labor share in agriculture as undocumented migration from Mexico is likely to have different labor market outcomes (and thus effects on crime) in counties that have higher shares of employment in agriculture. Pre-2006 county characteristics are summarized in Table 1.

Empirical Approach

Given the nature of our data, with annual construction measures, annual crime rates, and controls across counties that experience fence construction and those that do not, a natural empirical approach would be a difference-in-differences model using the passage of the 2006 Secure Fence act as the treatment date. Such an approach, however, relies on the assumption that the trends in crime rates for counties that received border infrastructure expansion would, in the absence of such investments, follow a parallel trend to counties that did not receive these investments. While this assumption is fundamentally untestable, we plot trends in average crime rates across

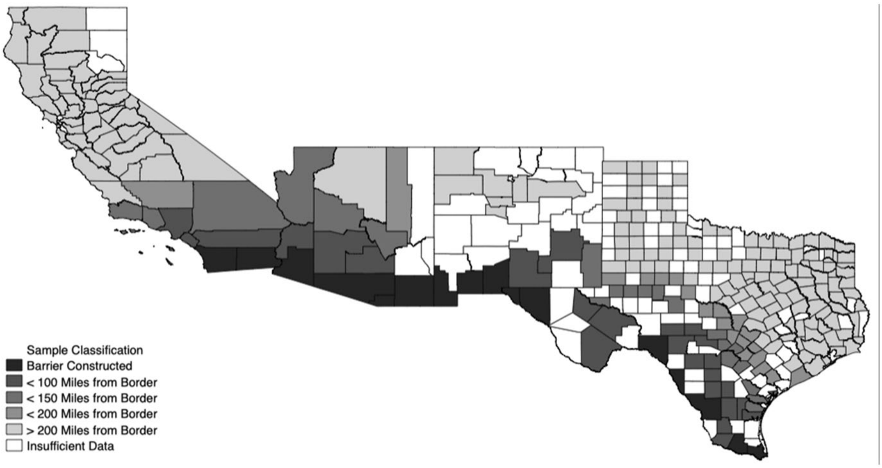

Fig. 3 Map of counties in analysis. *Notes* This map presents the county classification in our sample. Counties shaded in black are border counties that have any border construction by 2015. Shades of gray differentiate minimum distances to the US-Mexico border. Counties shaded white are missing one or more outcome or control variables. (Color figure online)

Table 1 Pre-2006 summary statistics

	Treatment counties		Control counties	
	Mean	SD	Mean	SD
Property crime rate (per 1000)	33.88	15.36	22.88	16.88
Violent crime rate (per 1000)	4.20	1.63	2.86	2.32
Total arrests (per 1000)*	49.75	28.6	44.42	18.99
Drug arrests (per 1000)*	11.2	19.36	5.67	3.93
Unemployment rate	.0919	.0423	.0593	.0182
Population	515,712	781,204	235,380	776,961
Personal income per capita	22,662	5,868	27,509	7,644
Labor force/total population	.425	.0407	.480	.0590
Population density (pop/KM^2)	79.39	100.43	92.61	435.61
Farm employment share	.044	.062	.091	.089
Manufacturing employment share	.056	.033	.069	.053
Government employment share	.225	.047	.179	.069

This table presents means and standard deviations of our crime measures as well as our matching variables for 2001–2005 across our 12 treatment counties and the pool of 242 donor counties. Asterisks (*) indicate that statistics for treatment groups omit Luna County due to missing arrest data

the 16 counties that received border infrastructure and compare them to trends for counties that received no border infrastructure that lie within 100 miles of the US–Mexico border, counties that lie within 100–200 miles of the US–Mexico border, and other counties in these states that are located more than 200 miles of the US–Mexico border. Figure 4 indicates that both property crime rates and violent

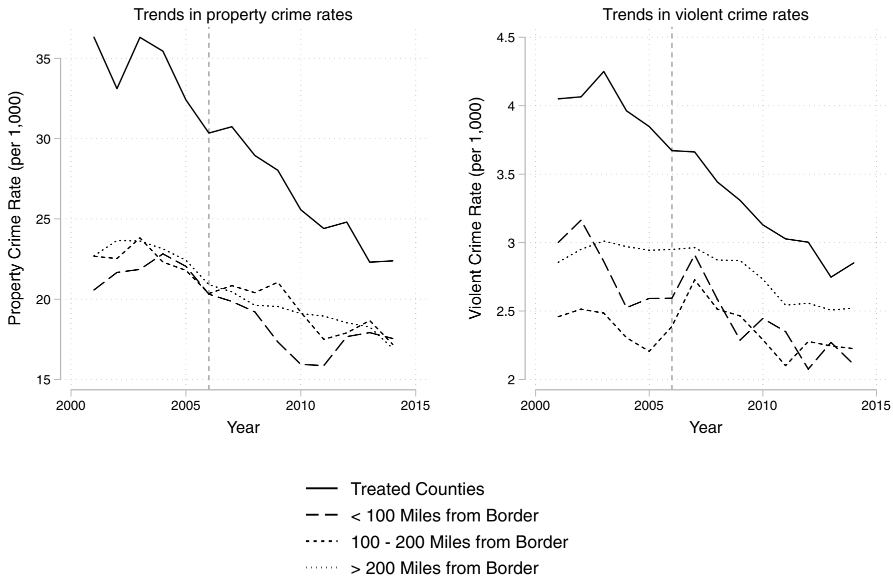

Fig. 4 Trends in crime rates across treatment and control counties. *Notes* This figure presents trends in average crime rates (for both property and violent crimes) for counties that received border infrastructure (treated counties), for control counties within 100 miles of the US–Mexico border, for counties within 100–200 miles of the US–Mexico border, and for counties in border states that lie more than 200 miles from the US–Mexico border

crime rates are much higher at the start of our sample in the treatment counties compared to other averages in our control group and, more importantly from the perspective of the difference-in-differences identifying assumption, seem to be trending downward prior to treatment and at a faster rate than our control counties. We take this as evidence against the parallel trends assumption – the control group averages will not provide suitable counterfactuals for the treated counties and estimates from difference-in-differences will likely be biased. This motivates our use of both synthetic control matching and matched panel data approaches described in the following subsections.

Synthetic Control Analysis

Synthetic control matching provides a data-driven approach to choosing appropriate counterfactuals (Abadie and Gardeazabal 2003; Abadie et al. 2010, 2015). Rather than assuming all untreated counties are suitable counterfactuals, synthetic control matching creates a counterfactual for every treated county by constructing a set of time-invariant, cross-sectional weights across untreated counties in order to best match the treated county on pre-treatment outcomes. These weights are applied to observed outcomes in the untreated group over the post-treatment period to construct a synthetic control unit for the treated county in every year. A formal presentation of synthetic control matching follows.

Consider the case of $N + 1$ units in a sample (indexed by i) observed over T time periods (indexed by t). The sample consists of one treated unit ($i = 1$ for simplicity) receiving treatment at some time T_0 (with $1 < T_0 < T$) over the sample period. We observe k different pre-treatment characteristics for each of the $N + 1$ units. Let Y_{it}^1 denote unit i's outcome in period t having received the treatment and Y_{it}^0 denote unit i's outcome in period t having not received the treatment. We can represent the difference between the treated unit and the unobservable counterfactual in the post-treatment period as

$$\alpha_{1,t} = Y_{1t}^1 - Y_{1t}^0 \tag{2}$$

The fundamental problem of causal inference implies that we cannot observe both Y_{1t}^1 and Y_{1t}^0. Instead, when estimating the treatment effect for $t \geq T_0$, we rely on our constructed counterfactual $\hat{Y}_{1t}^0$ to estimate $\alpha_{1,t}$:

$$\hat{\alpha}_{1,t} = Y_{1t}^1 - \hat{Y}_{1t}^0 \tag{3}$$

We construct $\hat{Y}_{1t}^0$ using a two-step procedure. The objective of the procedure is to match our treated unit to untreated units so as to minimize the root mean squared prediction error (RMSPE) over the pre-treatment period, that is $\sqrt{\frac{1}{T_0-1}\sum_{t=1}^{T_0-1}(Y_{1t}^1 - \hat{Y}_{1t}^0)^2}$.

Our minimization of the pre-treatment RMSPE is achieved by choosing two sets of weights. The first is a set of control weights, $\mathbf{V}$, a $k \times k$ symmetric, positive semidefinite matrix that applies relative weight of each of the control variables for the pre-treatment period. The second set of weights, $\mathbf{W} = (\omega_2, \omega_3, \ldots, \omega_{N+1})'$, is a $N \times 1$ vector that applies unit weights on the untreated observations. The elements in $\mathbf{W}$ are constrained to be nonnegative and must sum up to 1. The unit weights are chosen so as to minimize the difference in pre-treatment observable characteristics between the treated unit and the weighted average of control units. Let $\mathbf{X_1}$ be the $k \times 1$ vector of pre-treatment characteristics for the treated unit and $\mathbf{X_0}$ be the $k \times N$ matrix of pre-treatment characteristics for the N untreated units in the sample. Values of $\mathbf{W}$ are chosen to minimize the following pre-treatment covariate distance measure.

$$||\mathbf{X_1} - \mathbf{X_0}\mathbf{W}|| = \sqrt{(\mathbf{X_1} - \mathbf{X_0}\mathbf{W})'\mathbf{V}(\mathbf{X_1} - \mathbf{X_0}\mathbf{W})} \tag{4}$$

Applying the time-invariant unit weights to the outcomes of the untreated units yields our counterfactual outcome for any given year.

$$\hat{Y}_{1t}^0 = \sum_{i=2}^{N+1} \omega_i Y_{it} \tag{5}$$

Because $\mathbf{W}$ will be a function of $\mathbf{V}$, we follow Abadie et al. (2010) in choosing $\mathbf{V}$ to minimize the pre-treatment RMSPE. Once we have our unit weights, we can estimate the treatment effect for the treated unit in any given year as

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{i=2}^{N+1} \omega_i Y_{it} \quad \forall \quad t \geq T_0. \tag{6}$$

Traditional inference cannot be conducted on the estimates detailed above, and magnitudes alone are not informative regarding significance of the estimated effect. To this end, we follow Abadie et al. (2010) and many others in running placebo models by estimating treatment effects for every untreated unit. We then compare the ratio of the pre-treatment RMSPE with the post-treatment RMSPE for both the treated unit and the untreated units.[8] This ratio measure is increased by an improved pre-treatment fit and by a larger estimated effect in the post-treatment period. We report "quasi-p values" as the share of placebo post-/pre-RMSPE ratios at least as large as that of the treated unit.[9] We also present graphical evidence by plotting $\hat{\alpha}_{it}$s from $t = 1, \dots, T$ for all placebo estimates and overlay the treatment estimates. These figures can be found in the online appendix.[10]

In our context, we use 2006 as the treatment year (T_0) as this corresponds to the passage of the Secure Fence Act and the earliest initiation of the subsequent infrastructure construction. Our units are counties in the border states of California, New Mexico, Arizona, and Texas with treatment counties consisting of border counties receiving infrastructure investment during this period. We estimate a synthetic control model for each county (dropping all other treated counties).

We use a variety of pre-treatment characteristics for our matching: county crime rates from 2001 through 2005, labor force participation rate, unemployment rate, total population, population density, annual per capita personal income, average employment shares in farming, manufacturing, and government over 2001–2005, indicators for county location within 100, 150, and 200 miles of the US–Mexico border. As noted by Ferman et al. (2020), there is not yet a consensus on how pre-treatment outcomes should be included as matching variables. In our main analysis, we follow Dustmann et al. (2017), Gobillon and Magnac (2016), Bohn et al. (2014), Billmeier and Nannicini (2013), and Hinrichs (2012) – among many others – and include all pre-treatment outcomes (crime rates) as matching variables to obtain the best possible fit. Doing so removes the importance weight placed on any non-outcome matching variables. However, due to the concerns raised by Kaul et al. (2015) we also re-estimate our synthetic control models using *average* pre-treatment crime rates and 2005 crime rates (in addition to other pre-treatment county characteristics) in lieu of all annual pre-treatment period crime rates and present these results in the online appendix. Also, as is common in the literature, we apply different donor weights for our different outcomes as the underlying drivers of property crime and

[8] The RMSPE for county i across periods $1 - s$ is calculated as $\sqrt{\frac{1}{s}\sum_{t=1}^{s}\hat{\alpha}_{it}^2}$.

[9] For example, if there are 10 placebo estimates with post-/pre-RMSPE ratios larger than the treated unit, and $N + 1 = 243$, the quasi-p value would be $11/243 = 0.045$. Also note that, because we are agnostic on the direction of the effect, our ranking is only on the RMSPE, not the sign of the estimated average effect.

[10] The online appendix may be accessed at https://sites.google.com/sdsu.edu/hishamfoad/research/border-walls.

violent crime differ. Thus, for the same county, we select counterfactuals using separate weights by crime type.

We omit three counties from Texas from our analysis (Maverick, Val Verde, and Webb) because they each received less than 3 miles of total border infrastructure over our sample window. We also omit the county of Hidalgo, New Mexico, due to insufficient crime data. We note that we do not have 2009 crime data for Luna County, so all synthetic control matching estimates for that county omit 2009 to preserve the required balanced panel for synthetic control analysis. Table 1 presents pre-2006 summary statistics for our outcomes and matching characteristics for our 12 treated counties and our 242 donor counties.

Identification in synthetic control models requires the assumption that the matched control units provide a suitable counterfactual to the treated unit. This assumption may be violated if outcomes in control units are affected by the treatment status of the treated unit – i.e., in the context of spatial spillovers. If crime is diverted to neighboring counties, synthetic control estimates could be biased (if the displacement adversely affects untreated units in the donor pool selected by the synthetic control matching). We believe this is of little concern in this context for three reasons. First, we have limited observations of untreated border counties in our sample. Second, if such displacement of crime to nearby counties did occur, it would cause us to overestimate crime reductions. If we were to find significant reductions from our synthetic control estimates, we might be concerned that these findings were a product of bias from spatial spillovers. As we find no effects in general, we believe that such bias is not a primary concern. Finally, we observe limited internal mobility of migrants after international migration. Using a data from the American Community Survey, we create a sample of migrants from Mexico and Central America to the USA who have resided in the USA for at least 5 years. In this sample, 9.3% initially migrated to border counties and remained there and 89.3% initially migrated to non-border counties and remained there. Only 1% of the sample of migrants first moved to a border county and were living in a non-border county 5 years later. These shares are also consistent when restricting the sample to those who migrated in the pre- or post-construction periods. That said, we also re-estimate our main synthetic control matching models omitting all untreated counties within 100 miles of the US–Mexico border from the donor pool, thus ensuring that our synthetic estimates are not biased by displacement of criminal activity.

Matched Panel Approach

The key limitation of the synthetic control method in this context is that it restricts analysis to binary treatment designations. While using the passage of the 2006 Secure Fence Act on border counties receiving subsequent infrastructure expansion captures a broad treatment period for counties that are ever treated, it prevents us from utilizing our more detailed data on timing, type, and length of actual border wall construction. However, as discussed above, differential trends in crime rates prior to 2006 suggest that the use of a simple, two-way fixed effects model might falsely attribute downward trends in crime rates to border expansion using our full sample of 242 untreated counties.

To overcome this issue, we construct a matched control group from the synthetic matching approach described above. For each of our crime types (property crime and violent crime), we sum all donor weights received by each donor county from the 12 treated counties. We estimate two-way fixed-effects models as follows:

$$Crime\_rt_{i,t} = \beta_1 Barrier_{i,t} + \beta_2 X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t} \tag{7}$$

where $Crime\_rt_{i,t}$ is our crime rate outcome (for both property and violent crime) and $Barrier_{i,t}$ is one of three different measures we use for border distance (the inverse hyperbolic sine of barrier miles, the ratio of barrier miles to US–Mexico border miles in the county, and simply the number of border miles). We include time-varying county controls (in this case, the unemployment rate) and county and year fixed effects (given by α_i and γ_t, respectively). We also present results using state-by-year fixed effects, although they typically have a negligible effect on coefficient estimates.

We estimate the equation above using a weighted model, whereby each control county receives the sum of their synthetic weights and treatment counties receive a weight of unity. Our identifying assumption in this context is analogous to that of the parallel trends assumption required in most panel data models – i.e., we assume that, absent the border construction, crime rates in treated border counties would have evolved over time parallel to crime rates in our matched sample. While this fundamental assumption remains untestable, we are able to do look for evidence for or against this assumption. First, we can plot year-by-year estimated differences between our treatment and control samples to see if differences in crime rates are evolving distinctly in these groups prior to 2006. Furthermore, we can estimate model (7) with a set of leads and lags of border construction to test whether any potential crime effects lead or follow barrier construction.[11]

Estimated Results

Evidence from Synthetic Control Methods

We present our main results in Table 2 for both property crime and violent crime outcomes. The estimated effect we report is the average of annual differences between the observed crime in the treated county and synthetic counterfactual crime rates for 2006–2014 ($\frac{1}{9}\sum_{t=2006}^{2014}\hat{\alpha}_{i,t}$). As discussed above, traditional inference is not available in this context. For each estimate, we present "quasi-*p* values" which correspond to the share of the 242 placebo estimates from untreated donor counties that have a larger ratio of post-treatment root-mean-square prediction error over the

[11] Figure A8 in the online appendix plots average crime rates across our treated counties and the weighted average of control counties. Comparing this plot to Fig. 4, the matched sample does is better able to align the pre-period crime rates than the full sample.

Table 2 Synthetic control results including all pre-treatment outcomes as matching variables

County	Property crime				Violent crime			
	Estimated effect	Quasi-p value	Pre-RMSPE	Post-/Pre-RMSPE	Estimated effect	Quasi-p value	Pre-RMSPE	Post-/Pre-RMSPE
Cochise, AZ	− 1.79	.72	0	1.55e+10	− .4	.88	.18	6.53
Pima, AZ	− 39.02	.88	3.29	11.92	− 1.13	.13	0	2.70e+12
Santa Cruz, AZ	5.93	.82	.04	166.26	− .5***	0	0	3.25e+13
Yuma, AZ	− 1.95	.18	0	8.67e+11	− .68	.29	0	6.80e+11
Imperial, CA	2.86	.37	0	2.39e+11	− 1.26	.22	0	1.24e+12
San Diego, CA	− 4.36	.18	0	8.61e+11	− .56	.42	0	3.12e+11
Doña Ana, NM	8.29	.99	7.73	1.14	1.39	.9	.35	5.03
Luna, NM	− 2.69	.11	0	1.19e+12	.84	.28	0	7.07e+11
Cameron, TX	.7	.79	0	2.78e+09	− .84	.4	0	3.61e+11
El Paso, TX	− 1.76	.31	0	3.16e+11	.34	.66	0	3.11e+10
Hidalgo, TX	− 1.73	.72	0	1.68e+10	− 1.09	.16	0	2.08e+12
Hudspeth, TX	− 1.23	.47	0	1.29e+11	− .98	.82	.09	14.11

This table presents that our main synthetic control estimates of the secure fence act on both property and violent crime rates across our twelve counties including all pre-treatment outcomes as matching variables. The estimated effect corresponds to the average difference between the observed crime rate in the particular county and the weighted average of control counties that comprise the county's synthetic control for 2006–2014. The quasi-p value is the share of placebo county estimates that have a RMSPE ratio greater than that of the treated county. The post-/pre-RMSPE presents the ratio of post-treatment RMSPE to pre-treatment RMSPE for each county. The significance is denoted as follows: $^{***}p < 0.01$

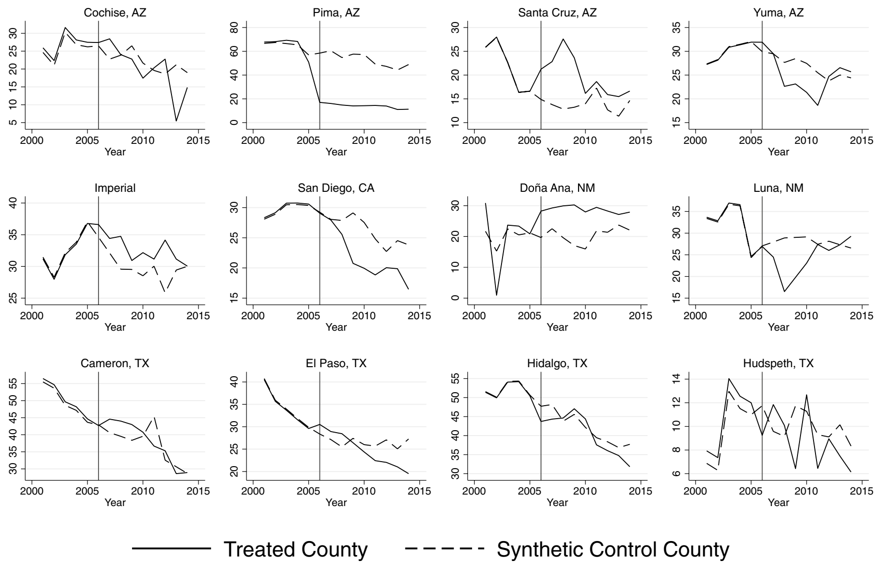

Fig. 5 Observed and synthetic property crime rates. *Notes* This figure presents the time series of property crime rates for each treated county and the weighted average of control counties that comprise the synthetic control. The vertical line indicates 2006 – the year of the passage of the Secure Fence Act

pre-treatment root-mean-square prediction error. We also present the pre-treatment RMSPE as well as the RMSPE ratio for each crime outcome.

Taken together, the results do not indicate any systematic relationship between border infrastructure investment and crime rates. Of the 12 estimates on property crime, 8 are negative and 4 are positive and none of these estimates appear significant using the quasi-*p* value metric described above (meaning that the post-treatment differences we observe in our treated counties are similarly likely to be observed in non-treated, control counties). It is worth noting that the quasi-*p* values are not proportional to the estimated effect – Pima County, for example, has an estimated effect of − 39.02 but a quasi-*p* value of 0.88. Because the quasi-*p* value corresponds to the ratio of the post-RMSPE to the pre-RMSPE, counties with poor pre-treatment fit with their synthetic controls will receive higher *p* values (Pima County's pre-treatment RMSPE is 3.29, orders of magnitude larger than that most treated counties).

Results for violent crime also lack a clear, significant relationship. Although 9 of the 12 are negative, only one estimate achieves significance in terms of the quasi-*p* value which has an estimated decrease of 0.5 violent crimes per 1000 residents in Santa Cruz County, Arizona. Of note regarding the political rhetoric previously cited, El Paso county shows no sign of an effect from border construction with small magnitude estimates that change signs across property and violent crime, with neither RMSPE ratio exceeding that of 31 percent our placebo ratios.

We present the individual time series in crime rates for each treated county overlaid with the time series for the county's synthetic control in Figs. 5 (property crime) and 6 (violent crime). Consistent with our estimates in Table 2, we see no systematic

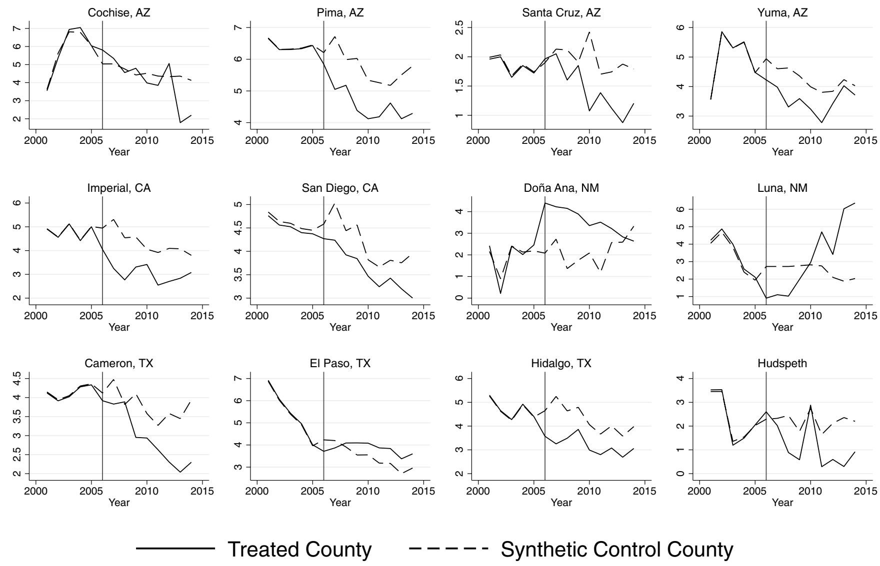

Fig. 6 Observed and synthetic violent crime rates. *Notes* This figure presents the time series of violent crime rates for each treated county and the weighted average of control counties that comprise the synthetic control. The vertical line indicates 2006 – the year of the passage of the Secure Fence Act

relationship in these figures. Generally, counties that diverge from their synthetic control after 2006 have fairly poor fits in the pre-treatment period. Pima County, for example, displays a dramatic drop in property crime rates, but this drop begins prior to the passing of the Secure Fence Act and is largely driven with a well-documented drop in crime rates for the city of Tuscon beginning in 2005. This explains the large pre-treatment RMSPE observed for this county. We present figures of the year-by-year estimated effects for each county with the year-by-year placebo estimates for all donor counties for both property and violent crime rates in the online appendix as Figures A4–A7.

The results presented in Table 2 correspond to an average treatment effect from the passage of the Secure Fence Act in 2006 until 2014. We separate each county's treatment period into a construction period and a post-construction period to assess whether our overall effects may be masking heterogenous effects that differ across these periods. We define the construction period for each county as 2006 until the completion of 97.5% of the total border wall built by 2014.[12] Completion dates range from 2008 in Yuma, Doña Ana, and Luna counties to 2012 in Cochise County.

Table 3 presents the results for both property and violent crime for the construction and post-construction periods. If county i completes construction in

[12] We use 97.5% rather than 100% only because there are a large number of counties that show very small sections completed only in the last year. As such, we believe that 97.5% provides a more meaningful end date of the construction.

Table 3 Estimates by construction timing including all pre-treatment outcomes as matching variables

County	Comp.	Property crime				Violent crime			
		Construction		Post-construction		Construction		Post-construction	
	Year	Effect	*P* value	Effect	*P* value	Effect	*P* value	Effect	*P* value
Cochise, AZ	2012	.53	.74	− 9.93	.67	.13	.94	− 2.25	.82
Pima, AZ	2009	− 42.32	.86	− 36.38	.88	− 1.12	.11	− 1.15	.14
Santa Cruz, AZ	2011	7.34	.82	3.11	.83	− .38***	0	− .74***	0
Yuma, AZ	2008	− 1.01	.16	− 2.42	.17	− .89	.22	− .58	.29
Imperial, CA	2009	2.72	.38	2.97	.35	− 1.5	.18	− 1.07	.22
San Diego, CA	2009	− 2.74	.16	− 5.66	.16	− .59	.37	− .53	.43
Doña Ana, NM	2008	8.54	.98	8.17	.99	2.2	.87	.99	.91
Luna, NM	2008	− 5.34*	.07	− 1.1	.21	− 1.71	.29	2.37	.26
Cameron, TX	2011	.89	.78	.32	.8	− .54	.44	− 1.44	.31
El Paso, TX	2011	.17	.39	− 5.61	.23	.18	.67	.66	.64
Hidalgo, TX	2010	− .67	.71	− 3.05	.71	− 1.24	.13	− .9	.18
Hudspeth, TX	2009	− 1.16	.41	− 1.29	.5	− .69	.82	− 1.22	.81

This table presents that synthetic control estimates (including all pre-treatment outcomes) of the secure fence act on both property and violent crime rates and separately examine the construction period and the post-construction period. The first column presents the year in which 97.5% of the 2014 border extent is completed. The construction period for each county is defined as 2006 to the specific completion year, and the post-construction period is defined as the year following the completion year through 2014. The effects for each period are the average differences between the observed crime rates and the synthetic crime rates over the time in question. The p value is the share of placebo county estimates that have a RMSPE ratio greater than that of the treated county. The RMSPE ratio is the RMSPE ratio of the relevant treatment period (construction or post-construction) divided by the RMSPE prior to 2006. The significance is denoted as follows: $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$

year T, then the construction period estimate will be $\frac{1}{T-2006+1}\sum_{t=2006}^{T}\hat{\alpha}_{i,t}$ and the post-construction estimate will be $\frac{1}{2014-T}\sum_{t=T+1}^{2014}\hat{\alpha}_{i,t}$. We construct the quasi-p values analogous to those presented earlier, but use the post-RMSPE from the corresponding treatment period. Thus, the quasi-p value on the construction period will be the share of placebo estimates that have a larger ratio of RMSPE for 2006 to T divided by the pre-2006 RMSPE and the post-construction quasi-p value will be the share of placebo estimates that have a larger ratio of RMSPE for $T + 1$ to 2014 divided by the pre-2006 RMSPE.

Separating the effects across construction and post-construction periods also fails to yield systematic results. We find a small but significant ($p < 0.1$) reduction in property crime in Luna County during the construction period (2006–2008) that then attenuates and loses significance upon completion of construction. For violent crime, the 0.5 reduction in Santa Cruz, AZ, observed above seems comprised of a 0.4 and 0.7 reduction in the construction and post-construction phases, respectively. No other effects are significant at conventional levels using the quasi-p value for violent crime.

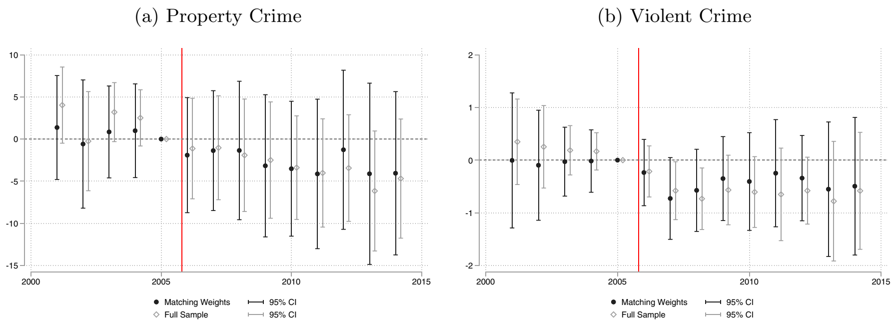

Fig. 7 Estimated treatment effects by year for matched sample. *Notes* These figures present year-by-year estimated differences between the border counties receiving barrier construction after the Secure Fence Act and control counties. These year-by-year differences are estimated via a two-way fixed effects model. Unweighted estimates are presented in gray, and weighted estimates (whereby control observations receive the sum of their synthetic weights and treated observations receive a weight of unity) are presented in black. All estimated differences between treatment and control counties are normalized to the 2005 difference (the year prior to the passage of the Secure Fence Act). Panel **a** presents the estimates for property crime rates, and panel **b** presents the estimates for violent crime rates

Matched Panel Analysis

While our synthetic control analysis has the advantage of allowing us to estimate treatment effects for each individual treated county, apart from comparing effects in the table, it is difficult to look for systematic effects from treatment. Using the matched panel constructed from the synthetic donor weights, we can examine changes in differences over time across treatment and control groups both to assess the potential validity of our parallel trends assumption and whether we see divergence in crime rates that coincide with treatment timing.

We begin by estimating year-by-year differences in crime rates across treatment and control groups to see if they deviate relative to their 2005 difference. Figure 7 presents the estimated differences for each year (omitting the 2005 difference) via a model of county and year fixed effects. Panel 7a presents the estimates of year-by-year differences in property crime rates using the sum of synthetic control weights on control counties (black lines) and the estimates of the year-by-year differences using all control counties without weighting (gray lines), and Panel 7b presents the same for violent crime rates. Estimates in these models measure differences in crime rates in a given year relative to the 2005 difference. Across both figures, we see no significant estimated differences in either pre-2006 or post-2006 periods. The absence of significant estimates prior to 2005 suggests that trends in crime rates are not diverging prior to the 2006 Secure Fence Act between our weighted counterfactual group and our treated group of counties, while the absence of significant estimates after 2006 suggests that there are no strong, systematic crime reductions in our treated counties that we can be attributable to border construction. This contrasts with the year-by-year coefficients from the full sample model without weighting in which the leads display more pronounced pre-trends, though individual leading coefficients are not significantly different from zero. We take this as evidence

Table 4 Matched difference-in differences-estimates of crime on pedestrian barrier construction

	Property crime		Violent crime	
	(1)	(2)	(3)	(4)
IHS barrier miles	− 2.017	− 2.007	− 0.128	− 0.147*
	(1.954)	(1.659)	(0.0782)	(0.0808)
No Obs	685	685	685	685
R^2	0.209	0.300	0.209	0.300
	(1)	(2)	(3)	(4)
Barrier/border miles	− 3.930	− 5.120	− 0.108	− 0.0954
	(8.545)	(6.747)	(0.617)	(0.576)
No Obs	587	587	685	685
R^2	0.290	0.400	0.199	0.289
	(1)	(2)	(3)	(4)
Barrier miles	− 0.128	− 0.115	− 0.00844*	− 0.00749*
	(0.106)	(0.0939)	(0.00427)	(0.00435)
No Obs	587	587	685	685
R^2	0.340	0.429	0.213	0.298
Year FEs	✓		✓	
State-by− year FEs		✓		✓

This table presents matched two-way fixed effects models of crime on three measures of pedestrian fence construction: the inverse hyperbolic sine of pedestrian fence miles, the ratio of pedestrian fence miles to the US–Mexico border, and the total miles of pedestrian fencing. Outcome variables in Columns (1)–(2) are property crime rates, and Columns (3)–(4) are violent crime rates. Odd-numbered columns include year fixed effects, and even-numbered columns include state-by-year fixed effects. All models are estimated using weighted regressions with control observations receiving the sum of their synthetic weights from the synthetic matching exercise. All models control for the local unemployment rate. Standard errors are clustered at the county level with significance denoted as follows: $^*p < 0.10$

that our weighted approach better controls for diverging pre-trends that might otherwise bias our results. We note a transitory (though not statistically significant) dip in crime violent crime rates for the years 2007 and 2008 in our weighted model, but no difference in 2009 and beyond. We return to this point later in the analysis.

We present estimates of Eq. (7) in Table 4. We focus our analysis on pedestrian barrier construction (rather than vehicle barrier construction) for two reasons. First, pedestrian barrier represents the most costly portion of border infrastructure. According to a 2009 US Government Accountability Office report, pedestrian barriers cost an estimated $6.5 million per mile as compared to vehicle barriers, which cost an estimated \$1.7 million per mile.[13] Second, pedestrian barriers are built closer to population centers and are likely to be more effective at restricting movements across the border. Because vehicle barriers were likely to be put up in places where it was easier and cheaper to build, estimates combining pedestrian and vehicle barriers could potentially attenuate results from looking at pedestrian barriers alone. As such, we present estimates of pedestrian barriers to ensure any null findings are not due to attenuation from vehicle barrier construction.

Table 4 presents the coefficient estimates of β_1 from Eq. (7). The three panels correspond to different measures of pedestrian barrier construction; the inverse hyperbolic sine transformation of the miles of pedestrian barrier (top panel), the ratio of pedestrian barrier miles to total US–Mexico border miles in the county (middle panel), and the number of constructed pedestrian barrier miles (bottom panel). Estimates for property crime rates are on the left two columns, and estimates for violent crime rates are on the right two columns. For each crime measure and barrier measure, we estimate models with and without state-by-year fixed effects.

For property crime, we find no significant estimates in any of the six specifications. Estimates are consistently negative, but statistically insignificant. Using the inverse hyperbolic sine of barrier miles, the estimates indicate that a 10 percent increase in pedestrian barrier is associated with an approximate reduction of 0.2 property crimes per 1000 people per year. This would represent a 0.6 percent reduction in pre-2006 crime rates in treated counties. Again, these are not estimated to be significantly different from zero. Taken together, these findings present strong evidence that barrier construction (length and timing) had no effect on reducing property crime rates in these border counties.

The coefficient estimates of pedestrian barrier construction on violent crime rates seem to indicate a stronger relationship. Estimated coefficients are consistently negative across our three measures, and half of the specifications are marginally significant ($p < 0.1$). The inverse hyperbolic sine-transformed barrier miles suggest that a 10 percent increase in pedestrian barrier construction is associated with a 0.013–0.014 decrease in violent crimes per 1000 people per year. This would be a 3 percent reduction in the pre-2006 violent crime rates. Other measures also suggest economically meaningful reductions associated with barrier construction.

Comparing these findings on violent crime rates to the observed transitory drop observed in Fig. 7b, we next test to see whether drops in crime follow barrier construction or precede it. To do so, we estimate a version of Eq. (7) including three-year leads and lags of barrier construction. Our model is as follows:

$$Crime\_rt_{i,t} = \sum_{s=-3}^{3} \beta_s Border_{i,t+s} + \beta_2 X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t} \tag{8}$$

[13] These numbers are in 2008 dollars. In terms of today's prices, the same figures would be \$7.8 and \$2.0 million per mile, respectively. Source: US Government Accountability Office (2009) For a side-by-side visual comparison of the two types of barriers, see Figure A1 in the online Appendix.

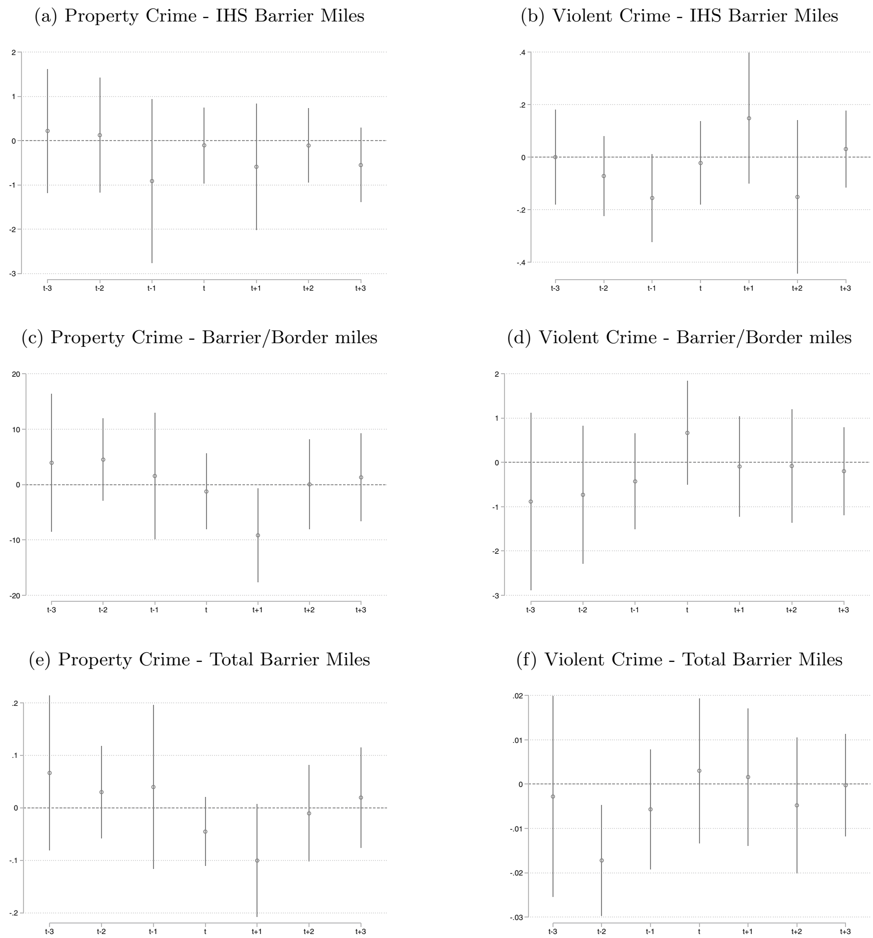

Fig. 8 Coefficient estimates on 3-year lead/lag models of crime rates on pedestrian border barrier measures. *Notes* The figures above present coefficient estimates of leads and lags of different measures of pedestrian border fence on property and violent crime rates. Coefficients are estimated from a weighted two-way fixed effects model whereby estimated control counties are weighted by the sum of their synthetic weights and treated counties receive a weight of unity. Individual coefficients refer to the effect of the amount of constructed barrier in the given year relative to the current year. For example, $t-3$ captures the effect of barrier constructed 3 years prior on crime rates in this year, t captures contemporaneous effects of barrier constructed in the present year on crime in this year, while $t+2$ captures the lagged effects of previously constructed barrier 2 years ago on crime rates in this year

In this model, we estimate seven different coefficients to examine the timing of potential crime reductions associated with timing of pedestrian barrier construction. In this equation, s denotes the lead/lag. For example, β_{-3} estimates the effect of the amount of barrier constructed in year t on crime in year $t-3$ (i.e., how does future barrier construction affect current crime rates). β_0 estimates the contemporaneous effect of barrier constructed by year t on crime rates in year t, and β_3 estimates the lagged effect of barrier constructed in year t on crime in year $t+3$ (i.e., how does past barrier construction affect current rime rates). If it is the barrier itself that reduces crime, we should expect to see effects concentrated in $s \geq 0$. However, if the effect is concentrated in $s < 0$, this would indicate crime reductions actually preceded the construction of the barrier and could not be attributed to the barrier itself. Rather, reductions in crime could be induced by the economic stimulus of border construction, which would go away once construction is complete.

We estimate Eq. (8) for both types of crime using our matched sample and the weighted matched sample models described above. Figure 8 presents the coefficient plots of estimates of $\beta_{-3} - \beta_{+3}$ from both outcomes (property and violent crime rates) on all three different measures of pedestrian barrier. For property crime, we see little systematic relationship in how the timing of barrier construction affects crime rates (consistent with our null findings discussed above). For violent crime, the effects are concentrated in the leading coefficients, meaning that the observed reduction in violent crime is occurring prior to the construction of the barrier, not after. In the IHS model, the effect is strongest in the year before; in the barrier-to-border mile share, all three leading coefficients are negative (though not individually significant) and contemporaneous and lagged coefficients are less consistent. In the linear model using barrier miles, the two-year lead is significant in both models with contemporaneous and lagged coefficients around zero.

The observed reductions in violent crime observed in Table 4 in fact precede barrier construction. Additionally, when three-year leads are included in model (7), the coefficient on barrier miles is consistently rendered insignificant across all models. If the border wall did lead to a reduction in violent crime through deterring migration patterns, we would expect to see these effects begin and/or follow the completion of the barrier construction, not precede it.

Further Analysis

We conduct a series of additional analyses related to the main synthetic control analysis detailed above. County-level annual arrest data are collected from the Federal Bureau of Investigation's Uniform Crime Reporting Program (UCR) on arrests. We compute the total number of arrests for all crimes as well as arrests for drug-related crimes for the counties in our sample. These county arrest numbers are normalized by population to create arrest rates per 1000 people for all arrests and drug arrest rates per 1000 people. Due to missing observations, we omit Luna County from this portion of the analysis. We present averages in treated counties and untreated counties in Table 1.

The benefit of arrest data is that it allows us to identify drug arrests separately. The UCR crime data only have categories for violent and property crime, not crimes directly related to illegal drugs. Identifying the potential impact of border walls on drug arrests is important given that one of the stated justifications for constructing border walls is to reduce drug trafficking across the US–Mexico border.

We conduct our same synthetic control analysis as above, using these different arrest rate measures as our outcomes. We present our overall results for the 11 treated counties in our sample in the online appendix, but discuss our results here.[14]. Our results indicate no significant relationship between passing the 2006 Secure Fence Act and arrest rates. Across our 22 estimated effects, only Cochise County yields an effect deemed statistically significant in its quasi-*p* value, a reduction of 1.13 drug arrests per 1000 after 2006. All other estimates have quasi-*p* values ranging from 0.124 to 0.955. The estimates are evenly split between positive and negative effects, and most have very small mean squared prediction error in the pre-treatment period, indicating that our lack of findings are not exclusively driven by poor model fit. One exception is Hudspeth County, whereby the synthetic control model fit is quite poor in the pre-2006 period across both arrest rate measures.

Furthermore, we find no evidence that differences in construction timing mask overall effects on arrest rates. The estimate for drug arrest rates found in Cochise County above remains similar across construction and post-construction phases. Aside from an estimated increase in drug arrest rates during the construction phase in Pima County, all other estimates are not significant (as measured by quasi-*p* values). Taken together, we believe that these estimates indicate little to no relationship between border construction and arrest rates.

In order to assuage concerns regarding potential spillovers of criminal activity to neighboring counties, we re-estimate our synthetic control models on property and violent crime rates dropping counties within 100 miles of the US–Mexico border from the donor pool. While crime displacement from counties receiving border infrastructure investments to similar counties nearby should overstate reductions in crime from such investments, we concede that we cannot rule out such spillovers ex ante. By dropping control counties within 100 miles of the US–Mexico border, we greatly reduce the risk that our synthetic counterfactual crime rates are being affected by spillovers from border construction in treatment counties. We find no qualitative changes to our main estimates suggesting that our null findings were not driven by spatial spillovers from border crime. The results are given in online appendix Tables A15 and A16.

We re-estimate our main model including only pre-treatment average and year-prior-to-treatment outcomes in our matching. Such a restriction forces our matches to put more weight on other county characteristics (rather than all weight on pre-treatment outcomes). Due to differing opinions on the correct way to leverage pre-treatment information, we estimate this alternative model to demonstrate that our lack of systematic findings is not driven due to decisions over the inclusions or omission of pre-treatment outcomes in our matching process. Our estimates remain qualitatively similar. Our overall model results as well as our disaggregated timing model results are given in the online appendix as Tables A17 and A18.

Finally, we re-estimate our main model using total crimes rather than crime rates as our outcome variables. We present tables analogous to Tables 2 and 3 in the online appendix. Our findings are remarkably consistent with our findings presented in the main paper on crime rates. For property crime, we find no significant

[14] Table A21 presents the estimates from these models, and Table A22 presents the results split by construction completion date.

effects with half of our estimates positive and half negative. For violent crime, Pima County displays the same reduction as in crime rates (with the timing coinciding with the police reforms discussed above). Cameron County does exhibit a statistically significant ($p < 0.1$) effect not present in the crime rate analysis.

Why Border Walls May Affect Crime

Our estimated results suggest no overall effect on border wall construction on crime rates. As discussed earlier, this may reflect the countervailing ways through which border walls affect crime, with various channels suggesting both increases and decreases in crime. In this section, we explore four potential channels through which border walls may affect crime: construction activity as an economic stimulus, border walls enriching criminal smuggling organizations, a shift in the demographic makeup of migrants, and border walls "trapping" seasonal migrants on the US side of the border.

Construction Activity

One possible channel through which border walls could affect crime is through the economic stimulus brought on by construction of these barriers. We may expect crime rates to decline with increased construction activity given greater opportunities in the legal labor market, especially for low-skilled males who are most prone to criminal activity. Implicit in this hypothesis is that border counties experience increased construction employment during the border wall construction period. Using data from the Bureau of Economic Analysis on construction employment over the period 2001–2019, we see no deviations from the trend in border counties compared to non-border counties following 2006.[15] This suggests that border wall construction did not have a significant causal effect on construction employment in border counties relative to non-border counties. As such, we do not believe that border wall construction affected crime rates through this economic stimulus channel.

Increases in Smuggling Costs

Another potential mechanism through which border walls may affect crime is by changing the value of human smuggling operations across the border. The fees charged by "coyotes" for taking people across the border illegally will likely increase as the costs of clandestine border crossings increase. As border walls should make these illegal crossings more difficult, we would expect coyote fees to rise in response to their construction. Higher fees may lead to increased criminal activity as organized crime groups in Mexico and the USA fight over more lucrative

[15] We present the graph of averages for these two groups in the online appendix. We also estimated a difference-in-differences model using county income and population as controls with both county and year fixed effects. The average treatment effect of border wall construction (defined as counties experiencing construction activity during the period 2006–2011) is statistically insignificant.

human smuggling routes. To evaluate this hypothesis, we gather data from the Mexican Migration Project. This dataset surveys Mexican migrants to the USA and includes data on the timing/location of migrant crossing, whether migrants utilized a coyote, and if so, the fees paid to the coyote. We then use these data to consider two questions: did the fees paid to coyotes increase after border walls were constructed and were increases larger in areas that saw greater border wall construction? Between 2000 and 2005, we estimate that the average fee paid to a coyote for illegally crossing the border was \$2153. This increased by 23% to \$2649 on average for the period 2006–2017. Coyote fees increased after border walls were built, though there are a number of explanations for this other than border walls themselves. To shed more light on a possible causal effect of border wall construction on coyote fees, online Appendix Table A1 gives average coyote fees for the pre- and post-construction periods for US counties experiencing the largest number of crossings facilitated by a coyote. While we do not have a large enough sample to conduct a more rigorous empirical analysis, the numbers in the table suggest that while coyote fees have increased overall, border wall construction is likely not the main cause of this increase. The largest increase in fees is for migrants crossing from Piedras Negra in Coahuila, Mexico to Maverick County in Texas—a county that did not experience any border wall construction activity. Similarly, migrants crossing from Juarez to El Paso actually saw coyote fees decline by 24% despite 47.2 miles of additional border barriers being constructed over this period. There is not enough evidence here to suggest that border walls have increased crime through enriching criminal organization involved in human smuggling.

The Demographic Makeup of Migrants

While there is some evidence that border walls have reduced migration overall, one channel through which border walls could affect criminal activity is by selecting for migrants who have a different proclivity to engage in criminal behavior. Criminal activity tends to be higher for groups that are on average younger and more male. If border wall construction led to a shift in migration that is younger and more male, we may observe an increase in crime. To evaluate this hypothesis, we compare the demographic profiles of migrants from Mexico and Central America who arrived in the USA between 2004 and 2006 and those arriving between 2007 and 2015. We further separate these groups into those residing in border counties and those residing in non-border counties.[16] To evaluate whether border wall construction led to a change in the demographic profile of migrants, we compare these characteristics for migrants between periods and between border and non-border counties in online Appendix Table A2. We limit the analysis to migrants who resided in the same county as they migrated to 5 years later to control for any spillovers between border and non-border counties. Looking at migrants who moved to border counties (and stayed there for at least 5 years), we see that migrants in the post-border wall period

[16] Our earlier analysis of these data suggests limited internal mobility of migrants, implying that those migrating to border counties tend to stay in border counties.

were on average older, less male, more likely to be married, had greater English language proficiency, and more education. All of these factors are correlated with lower crime rates. However, we also observe the same demographic shifts when looking at non-border counties (with the exception of marriage rates). As such, it appears that migrants from Mexico and Central America did experience demographic shifts over this period associated with lower crime rates, but it is unlikely that border wall construction was driving these shifts.

Trapping Seasonal Migrant Labor

Another channel through which border walls could affect crime is by "trapping" seasonal migrant workers on the US side of the border. The idea here is that by increasing migration costs, border walls may induce seasonal migrants to remain in the US during off-season periods of low labor demand. This may lead to higher unemployment during off-season months, which is associated with higher crime rates. To evaluate this hypothesis, we collected data on monthly unemployment rates by county from 2000 to 2015 from the Bureau of Labor Statistics. We then compute the annual standard deviation in unemployment rates by county as a measure of unemployment volatility. If border walls are "trapping" seasonal migrant labor on the US side of the border, we would expect there to be an increase in border county unemployment volatility relative to unemployment volatility in non-border counties in the post-construction period. Prior to 2006, the average annual standard deviation in unemployment rates in non-border counties is 0.617, while that in border counties is 1.586. After 2006, unemployment volatility in non-border counties is 0.655 and volatility in border counties is 1.198. Thus, it appears that unemployment volatility in border counties actually declined relative to that in non-border counties. This decline may be due to the documented decrease in unauthorized migration following border wall construction. In any event, the hypothesis that border walls are "trapping" migrant labor on the US side of the border does not hold up to this cursory analysis.

Final Remarks

Estimates suggest that completing a physical barrier along the entire US–Mexico border would cost around $60 billion (Nowraseth 2019). Proponents of expanding border fencing often point to the crime deterrent effects of this fence. While we acknowledge that completing a physical barrier across the entire border may have certain general equilibrium effects that our setting does not capture, our estimates suggest that crime deterrence is not a suitable justification for a border wall given that the existing wall has done little to reduce crime. In general, we find little evidence of any overall effects of border wall construction on crime rates.

We find very limited evidence that border walls have reduced crime. For example, from our synthetic control analysis on crime rates in Table 2, the only significant effect we find is 0.5 per 1000 fewer violent crimes in Santa Cruz County, AZ. This is a very small effect in terms of magnitude when compared to the cost of border wall construction. For example, there are about 15 miles of primary pedestrian border fencing in Santa Cruz County. Using the GAO construction cost estimates of $6.5 million per mile of pedestrian fence, a rough estimate is that it cost nearly $97.5 million to build Santa Cruz County's pedestrian border wall with Mexico.[17] Can this expense be justified by the 0.5 fewer violent crimes per 1000 we estimate as a result of the border wall?

To put this into context, we estimate the average cost of a violent crime in Santa Cruz County, AZ, using data from Chaflin and McCrary (2018). We estimate that each violent crime has a social cost of roughly $321,323.[18] Given that Santa Cruz County has a population of around 46,000 people, a decline of 0.5 violent crimes per 1000 translates to about 23 fewer violent crimes as a result of border wall construction. Using our estimated cost measure, these 23 fewer violent crimes represent a social value of about $7.4 million. Given that the border wall in Santa Cruz County cost $97.5 million, it is difficult to justify its expense using a crime reduction argument.

Similarly, from our synthetic control analysis on drug arrests in Table 5, the only significant effect is a reduction of 1.13 per 1000 drug arrests in Cochise County, AZ. The 55 miles of primary pedestrian border walls in Cochise County cost $357.5 million to build. Again, this is a massive expense for a relatively minor reduction in drug arrests. Would it have been better to direct border wall expenses toward more effective crime reduction strategies such as increased policing, spending on education, and targeted interventions?

Our matched panel estimates do find evidence of a negative relationship between border walls and violent crime rates. However, when including leads and lags of borders wall construction, we observe that the crime deterrent effects of construction on violent crime are all occurring before the border walls are complete. This suggests that constructing a border wall may lead to a temporary reduction in violent crime, but any reductions are most likely being driven by the short-run economic stimulus effect of construction. Once construction is complete, we do not observe any lasting crime deterrent effects of border walls. Further, the estimated temporary reductions in violent crime may be difficult to justify given the small magnitude of crime reduction, the exorbitant costs of border wall construction, and the temporary nature of these crime reductions.

[17] Construction cost estimates reported in US Government Accountability Office (2009). Miles of border wall constructed include all construction from 2006 to 2014 based on authors' calculations. Vehicle barriers are not included so as to estimate a lower bound of construction costs.

[18] This was computed by first looking at the breakdown of violent crimes in Santa Cruz County over the sample period as 4% murder, 12% rape, 32% robbery, and 52% aggravated assault. We then multiplied these weights by the costs in Chaflin and McCrary (2018) of $7,000,000 per murder, $142,000 per rape, $12,624 per robbery, and $38,924 per assault.

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