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Deterring Illegal Entry: Migrant Sanctions and Recidivism in Border Apprehensions
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ABSTRACT

Over 2008 to 2012, the U.S. Border Patrol enacted new sanctions on migrants apprehended attempting to enter the U.S. illegally. Using administrative records on apprehensions of Mexican nationals that include fingerprint-based IDs and other details, we detect if an apprehended migrant is subject to penalties and if he is later re-apprehended. Exploiting plausibly random variation in the roll-out of sanctions, we estimate econometrically that exposure to penalties reduced the 18-month re-apprehension rate for males by 4.6 to 6.1 percentage points off of a baseline rate of 24.2%. These magnitudes imply that sanctions can account for 28 to 44 percent of the observed decline in recidivism in apprehensions. Further results suggest that the drop in recidivism was associated with a reduction in attempted illegal entry.

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1 Introduction

In this paper, we examine how penalties on illegal border crossings affect the behavior of undocumented immigrants from Mexico. Two-thirds of these migrants enter the U.S. by crossing the U.S.-Mexico land border (Passel and Cohn, 2016). Until 2005, over 95% of Mexican nationals apprehended while trying to cross the border unlawfully were granted *voluntary return*, under which they were released into Mexico and subject to no further repercussion. In 2008, the U.S. Border Patrol sought to deter new and repeated attempts at illegal entry by replacing voluntary return with sanctions imposed under a Consequence Delivery System (CDS) (Capps et al., 2017). By 2012, the share of apprehended Mexican nationals granted voluntary return was down to 15%. CDS sanctions include *administrative consequences*, which complicate obtaining a legal U.S. entry visa in the future; *programmatic consequences*, which disrupt smuggling networks by relocating a migrant far from the point of capture before release into Mexico; and *criminal consequences,* which entail prosecution in U.S. courts. The CDS rollout was followed by a sharp decline in recidivism: the share of apprehended migrants re-apprehended within the next 18 months fell from 28.1% in 2005 to 17.5% in 2012. We use administrative records from the U.S. Border Patrol on apprehended migrants to estimate the impact of the CDS on recidivism in apprehensions.[1]

The CDS rollout capped 20 years of the U.S. intensifying border enforcement (Roberts et al., 2013). Between 1992 and 2007, the U.S. government quadrupled the number of Border Patrol agents (Figure A1). Despite these efforts, the U.S. population of undocumented immigrants grew from 3.5 million in 1990 to 12.2 million in 2007 (Passel and Cohn, 2016). Measuring border enforcement using Border Patrol manpower (Hanson and Spilimbergo, 1999), previous literature finds that this earlier border buildup had at most modest negative impacts on attempted illegal entry (Gathmann, 2008; Orrenius and Zavodny, 2005; Massey et al., 2016), though impacts are larger for the less-skilled (McKenzie and Rapoport, 2010), and tightened border security did induce undocumented migrants already in the U.S. to remain in the country (Angelucci, 2012).

Since 2007, the scene at the border has changed. Apprehensions are down sharply (Figure A2),[2] and, after decades of growth, the U.S. population of undocumented immigrants from Mexico

[1]These records are maintained by U.S. Customs and Border Protection, the agency of the U.S. Department of Homeland Security that oversees the U.S. Border Patrol.

[2]Mexican nationals were long the majority of those apprehended at the border. Since 2013, the share of Central Americans in apprehensions has grown (Figure A2). In strong contrast to Mexican immigrants, Central Americans are likely to seek asylum, which invokes distinct U.S. immigration procedures (Amuedo-Dorantes et al., 2015).

fell from 6.9 million in 2007 to 5.8 million in 2014 (Passel and Cohn, 2016). Although the Great Recession and demographic shifts in Mexico explain some of these changes (Hanson et al., 2017), the U.S. recovery has not brought a rebound in illegal entry, and population changes are too gradual to account for the late-2000s immigration dropoff. Among recent security measures, Feigenberg (2017) and Allen et al. (2018) find that a border fence constructed along the land portion of the U.S.-Mexico border between 2006 and 2009 has deterred illegal entry along affected crossing routes, while cross-section surveys of apprehended migrants reveal no connection between exposure to the CDS and migrant plans for illegal entry (Amuedo-Dorantes and Pozo, 2014; Martinez et al., 2018).[3]

Because administrative records on apprehensions have been unavailable previously, the literature lacks a longitudinal perspective on how Border Patrol sanctions affect migrant behavior. Our data now permit such an analysis. We face two empirical challenges. The first is that a migrant's characteristics may be correlated both with the sanctions he receives and his re-entry decision. Exploiting the richness of the apprehension records, we control for interactions of migrant age, birthplace, previous apprehensions, and location and timing of capture. Identification is based on differences in recidivism among paired groups of migrants—one of which is sanctioned and one of which is not—in which the two groups share the same birthplace, birth cohort, and apprehension history, and were apprehended in the same location on the same day. Our identifying assumption is that during the CDS rollout, the assignment of penalties was as good as random, conditional on the controls. While implementing the CDS, individual Border Patrol sectors had discretion in delivering sanctions.[4] Capacity in sanctioning was affected by backlogs of migrants awaiting processing and available space in detention facilities (Capps et al., 2017). We estimate treatment effects by exploiting high-frequency variation in capacity constraints.

The second empirical challenge is that, analogous to literature on criminality,[5] we do not observe recidivism in illicit activity (attempted entry) but rather in imprisonment (apprehension). Our estimated impact of the CDS on recidivism thus combines the impact on re-attempted illegal entry and the impact on the probability of capture, conditional on re-attempting entry. We exploit the structure of incentives to avoid capture to help resolve this ambiguity.

[3]Other recent work examines the consequences of changes in immigration enforcement in the U.S. interior (e.g., Orrenius and Zavodny 2009; Bohn et al. 2014; Orrenius and Zavodny 2015; Hoekstra and Orozco-Aleman 2017).

[4]Along the Southwestern border, these sectors are: San Diego and El Centro (CA); Yuma and Tucson (AZ); and El Paso, Big Bend, Del Rio, Laredo, and the Rio Grande Valley (TX).

[5]For recent work on recidivism in criminal arrests, see Bhuller et al. (2016); Heller et al. (2016); Agan and Makowsky (2018). For a review of the previous literature, see Chalfin and McCrary (2017).

Our results imply that the CDS can account for one-third of the reduction in re-apprehension rates between 2008 and 2012. Less-severe administrative sanctions have similar effects on recidivism to those of more-severe programmatic and criminal sanctions. Our results are unaffected by controlling for individual characteristics, as consistent with our identifying assumption that variation in the application of the treatment was due to Border Patrol capacity constraints and not to the identity of the migrant. The stability in our estimates, despite the large number of controls, suggests limited scope for selection-on-unobservables to explain our findings (Altonji et al., 2005; Oster, 2017).

2 Data

2.1 CBP Administrative Data on Border Apprehensions

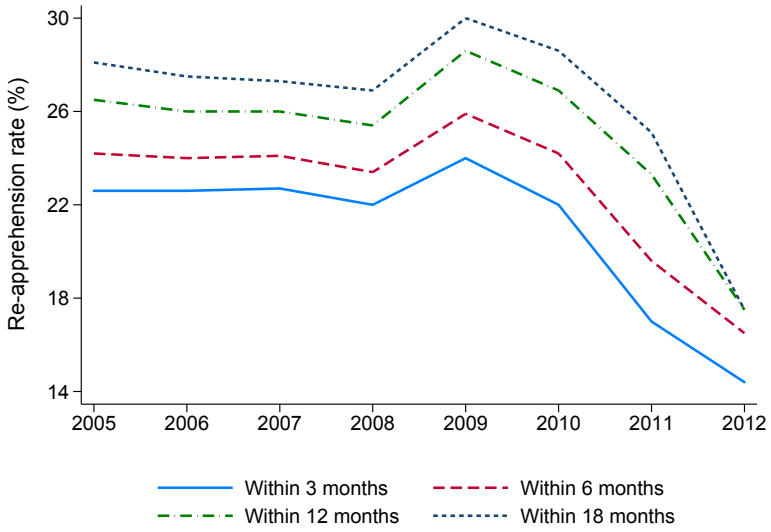
Our data cover all apprehensions of individuals attempting to enter the U.S. without authorization between ports of entry along the U.S.-Mexico border. After apprehension, the Border Patrol fingerprints migrants and takes their biographical information (GAO, 2017a). Fingerprint records allow us to track individual migrants over time. Because Border Patrol policy shields women and minors from some sanctions, reserves voluntary return for Mexican nationals, and imposes severe sanctions on the few migrants with many previous apprehensions (GAO, 2017a), we restrict the sample to male Mexican nationals 16 to 50 years of age with six or fewer previous apprehensions.

We study 2008 to 2012, which spans the CDS rollout. Our sample contains 0.97 million apprehensions, which represent 79.5% of Mexican nationals apprehended at the border over 2008-2012. As seen in Appendix Table A1, the Tucson sector of the Border Patrol accounts for 53% of sample apprehensions. When border enforcement intensified in the 1990s, many migrants switched from single-day crossings near major border cities to multi-day crossings in rugged eastern Arizona (Massey et al., 2016), which lies within the Tucson sector. Nearly half of apprehensions occur during the first four months of the year, as migrants arrive for seasonal work in agriculture and construction, and seek to avoid extreme weather. Apprehensions are evenly distributed across days of the week and times of day, reflecting randomness in the timing of apprehensions that results from multi-day crossings. Whereas relatively few apprehended migrants are from Mexican states that border the U.S. (11.5%) or states in Mexico's far south (7.4%), the majority are from central

Mexico (68.0%), consistent with historical patterns (Massey et al., 1994).

Figure 1 plots the fraction of apprehended migrants who were re-apprehended in the following 3, 6, 12 or 18 months. While re-apprehension rates are roughly stable between 2005 and 2009, they drop sharply in ensuing years, which span the CDS rollout. The 2005-to-2012 decline in the re-apprehension rate is 8.2 *p.p.* (percentage point) at the 3-month frequency and 10.6 *p.p.* at the 18-month frequency.[6]

Figure 1: Re-apprehension Rate following Initial Apprehension for Male Mexican Nationals



Note: Data are from CBP administrative records showing the re-apprehension rates for the study population of male Mexican nationals 16 to 50 years of age with six or fewer previous apprehensions.

2.2 The Consequence Delivery System

A foreign national who enters the U.S. without authorization is in violation of U.S. law. The Border Patrol may refer any apprehended migrant for criminal prosecution. For decades, however, standard practice regarding apprehended Mexican nationals was to offer voluntary return (Roberts et al., 2013), under which a migrant forgoes the right to appear before a judge and agrees to depart the U.S. after transport to the border. He avoids *formal removal* and thereby escapes legal repercussions from his offense. Historically, the sheer volume of apprehensions, which averaged 1.2 million annually over 1999 to 2007 (DHS, 2008), in part justified voluntary return. Another

[6]This decline in recidivism is corroborated by the EMIF-Norte (Survey of Migration in the Northern Border), which surveys apprehended migrants returning to Mexico (Appendix Figure A3). The fraction of apprehended Mexican nationals stating they will re-attempt a border crossing within three months fell from 77% in 2009 to 49% in 2013.

motivation was that during its early history, an implicit mandate of the Border Patrol was to help regulate the supply of low-skilled labor in the U.S. border region. Calavita (2010) and Roberts et al. (2013) document that log books of U.S. Border Patrol agents in the 1940s and 1950s cite the demand for farm workers in the Rio Grande Valley of Texas as a factor in determining the intensity of border enforcement.[7] Relatedly, Hanson and Spilimbergo (2001) find that over the period 1970-1997 border enforcement weakened in the months following positive labor demand shocks to U.S. sectors that employed undocumented labor intensively. Under voluntary return, migrants may have interpreted border enforcement as existing to modulate the flow of labor across the border, rather than to prevent it altogether, suggesting that the *illegality* of crossing the border without authorization may not have been foremost in the minds of many prospective migrants.

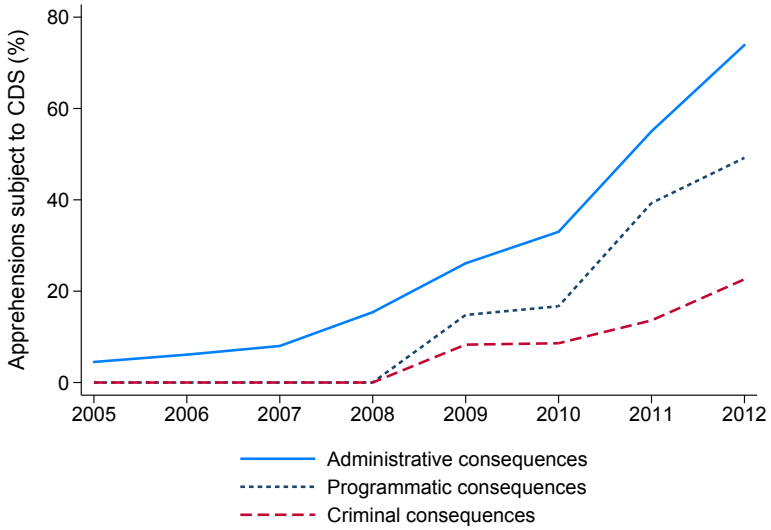
Today, nearly all apprehended migrants are sanctioned under the CDS. Administrative consequences are delivered through a formal removal order (Rosenblum, 2013). Migrants processed for removal are not detained. Instead, they are processed by Border Patrol agents and transferred to the border for release into Mexico. Processing a removal order requires 90 minutes of time, compared to 15 minutes for a voluntary return (Capps et al., 2017). A removal order, which is tied to an individual's fingerprint record, precludes the migrant from applying for a legal entry visa for five years, and counts as a prior infraction when dealing with U.S. law-enforcement authorities in the future. For migrants already under a removal order (i.e., who have been subject to administrative consequences in the past), a subsequent apprehension may extend the time period during which they cannot obtain a legal entry visa to as long as 20 years. Because many undocumented immigrants from Mexico have an application pending for a U.S. green card, this penalty is onerous. In the New Immigrant Survey, Massey and Malone (2002) document that 41% of Mexican nationals who obtained a U.S. green card in 2003 had previously crossed the U.S. border illegally. Administrative consequences thus raise the expected costs to migrants of attempting to enter the U.S. without authorization. The share of apprehended Mexican nationals subject to administrative consequences rose from 15.5% in 2008 to 73.9% in 2012 (Figure 2).

Programmatic consequences are used to disrupt smuggling networks. Given the high probability of apprehension for a Mexican national attempting illegal entry at the border (40-60% during our

[7] As evidence of this history, the Border Patrol would frequently refer apprehended migrants to the Bracero Program (1949-1964), through which the U.S. provided temporary work visas to farm workers.

sample period), the large majority of migrants hire a smuggler.[8] The standard smuggling fee covers the cost of multiple attempts to cross the border, in the event that the first, second, or even third try ends in apprehension (Chávez, 2011). For a given series of attempts to cross the border, migrants are thus tied to a particular smuggler (unless they choose to hire a new smuggler, which requires incurring the expense of a second fee). The main programmatic consequence is the Alien Transfer Exit Program (ATEP), under which an apprehended Mexican national, after being subject to other penalties, is repatriated to Mexico at a location far from his entry point, which complicates reconnecting with the smuggler he has hired. Angelucci (2015) finds that the decision to send a migrant to the U.S. among poor households in Mexico is strongly responsive to random shocks to household income, consistent with financial constraints limiting the ability of households to afford smuggling fees. The adverse shock of being subject to ATEP would limit the ability of some households to finance further attempts to cross the border by their members. The use of ATEP, which only applies to non-minor males, began in 2009 in the four western-most Border Patrol sectors and is now used in seven (of nine) sectors. Programmatic consequences applied to 14.8% of those apprehended in their initial year of 2009 and to 49.2% in 2012 (Figure 2).

Figure 2: Rollout of Consequence Delivery System



Note: Data are from CBP administrative records showing the share of apprehended male Mexican nationals 16 to 50 years of age with six or fewer previous apprehensions subject to sanctions.

Under criminal consequences, an apprehended migrant is subject to prosecution. Most occur

[8]See Amuedo-Dorantes and Pozo (2014), Massey et al. (2016), DHS (2017), and Roberts (2015; 2017).

under Operation Streamline, under which a migrant is tried for misdemeanor unlawful entry and appears with a group of migrants for sentencing. Although sentences may be up to 180 days for a first offense, first-time offenders are typically sentenced to time served (while awaiting a hearing). If the migrant has many previous apprehensions or is suspected of non-immigration crimes, he may face Standard Prosecution in a U.S. federal district court, which involves sentences of up to two years and possibly being tried for a felony offense.[9] The imposition of criminal consequences was intended to signal the seriousness of the Border Patrol regarding border enforcement (Roberts et al., 2013). Analogous to the broken-windows theory of policing (e.g., Corman and Mocan, 2005), subjecting migrants to prosecution in court conveys that further apprehensions could bring harsher penalties. Applying criminal consequences requires the participation of the Federal Judiciary, the U.S. Attorney's Office, the U.S. Marshal's Service, and Immigration and Customs Enforcement (ICE). Criminal consequences were applied to 8.3% of apprehensions in their initial year of use in 2009 and to 22.6% by 2012 (Figure 2). Operation Streamline accounted for 83.5% of these cases.

Administrative and criminal consequences, aside from their legal repercussions, may impose additional psychic costs on migrants. The sanctions communicate that border crossing is illegal, perhaps changing migrant perceptions of procedural justice surrounding border enforcement (e.g., Sunshine and Tyler, 2003; Tyler, 2004). Whereas the voluntary-return regime did not emphasize the criminality of illegal border crossing, the CDS regime does so strongly. Emphasizing the criminality of the act may have raised the disutility associated with being apprehended.

After apprehension, Border Patrol officers propose the combination of CDS sanctions a migrant receives. Of the 35.4% of sample migrants from 2008 to 2012 subject to administrative consequences, 49.0% were further subject to programmatic or criminal consequences (Table A2). Programmatic consequences, applied in 19.5% of sample apprehensions, also brought administrative consequences in 51.0% of cases. Because programmatic consequences only involve transport before release into Mexico, they do not mandate formal removal. Criminal consequences, applied in 8.8% of apprehensions, entailed administrative consequences in 94.5% of cases, as it is standard to issue removal orders for migrants facing criminal prosecution. Not all U.S. federal court districts permit Streamline prosecutions or Standard Prosecution of offenses tied to illegal entry. Because Border Patrol

[9] In the early 2010s, the average sentence under Standard Prosecution was 18 months (USSC, 2015). Long sentences may reduce recidivism mechanically by incapacitating the migrant. However, the use of Standard Prosecution remains uncommon, accounting for only 1.5% of cases in our data.

sectors in these districts tend to use programmatic consequences in lieu of criminal consequences, we combine programmatic and criminal consequences when analyzing these penalties.

2.3 Application and Rollout of the CDS

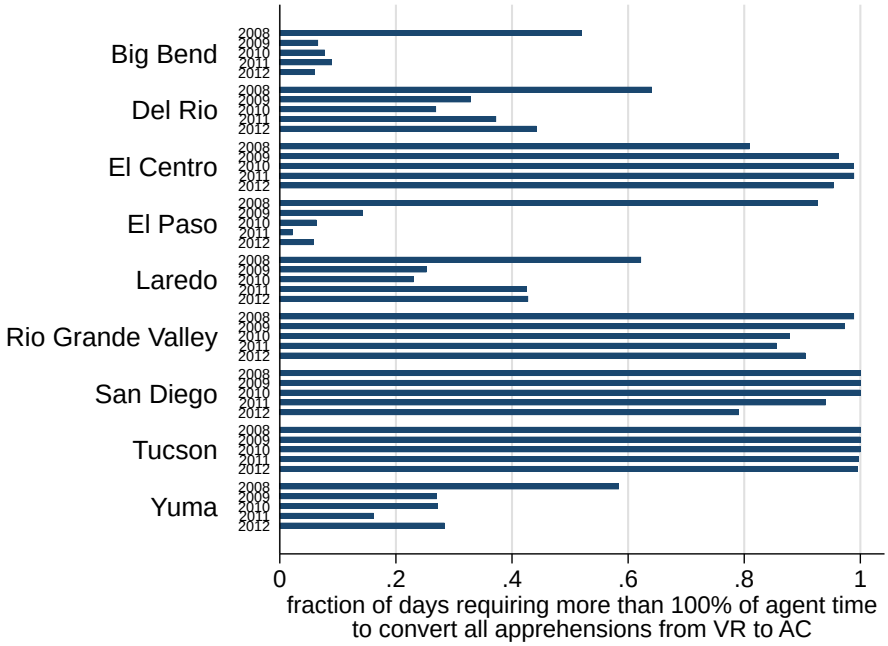
Historically, the Border Patrol was a decentralized organization, with sector chiefs having autonomy in setting enforcement strategy (Calavita, 2010). The CDS originated in Border Patrol sectors whose leaders perceived that it might be effective in deterring illegal entry. It was not implemented across all sectors until late 2012 (Simanksi, 2013). During the CDS rollout, variation in its application arose in part from sector-level differences in capacities for processing migrants. Applying sanctions is time intensive, and staffing levels were initially insufficient to impose penalties on all those apprehended. Because some sanctions require assistance from other government entities, local resource availability in these entities also affected the delivery of penalties. We examine how these sources of variation in the application of the CDS—along with daily variation in the number of migrants attempting illegal entry—helped generate plausibly exogenous assignment of sanctions to apprehended migrants, conditional on their observable characteristics and the conditions of their apprehension.

Discretion in application of consequence programs. During the CDS rollout, decision rules for applying sanctions were based on (1) the origin country of the migrant, (2) the migrant's apprehension history, and (3) whether the migrant was traveling with family members (GAO, 2017a). The highest priority for sanctions was migrants from countries other than Mexico with many previous apprehensions (or a record of criminality). The lowest priority was Mexican nationals with no previous apprehensions, and the next lowest priority was Mexican nationals with a few previous apprehensions. Our sample migrants were therefore relatively low priority for sanctions and ones for whom the Border Patrol would have maximal discretion in assigning penalties.

Capacity constraints at the level of Border Patrol sectors. Costs in imposing sanctions meant that on high-traffic days, agents may have been unable to impose sanctions on all apprehendees. Consider officer time that would have been required to transition fully from a system of voluntary removal to one based on administrative consequences (AC) in each year of the CDS rollout. For each Border Patrol sector, we compute the share of officer time that would be absorbed by applying AC to all apprehended migrants on each day from 2008 to 2012 (see Appendix B for details). We use sector–day observations on the total number of apprehensions, sector–year

observations on the total number of agents, and an in-depth analysis of time use by Border Patrol agents in the early 2010s (GAO, 2017b). Figure 3 plots the results across Border Patrol sectors during the CDS rollout. In the four busiest sectors—El Centro, Rio Grande Valley, San Diego, and Tucson, which account for 86.7% of sample apprehensions—most days would have required close to or more than 100% of agent time to apply AC to all apprehended migrants. Such constraints created substantial day-to-day variation in the fraction of migrants receiving sanctions. From 2008 to 2012, the standard deviation of the daily fraction of apprehensions subject to AC was 0.19 in El Centro (mean of 0.25), 0.32 in the Rio Grande Valley (mean of 0.38), 0.08 in San Diego (mean of 0.19), and 0.39 in Tucson (mean of 0.51).

Figure 3: Capacity Constraints in Switching to Administrative Consequences

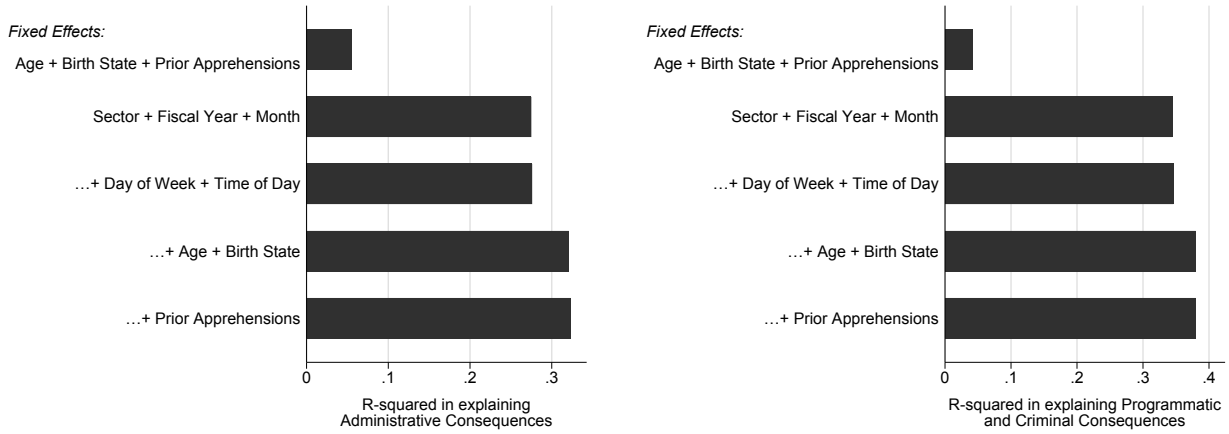


Note: Each row shows the fraction of days in which Border Patrol agent time required to impose AC on 100% of those apprehended (up from the 2007 baseline of 8%) would exceed all available agent hours for a Border Patrol sector. Estimates are based on equation (4) in Appendix B.

Capacity constraints in partner agencies. While the Border Patrol applies administrative consequences, it relies on other agencies to deliver programmatic and criminal consequences. Applying ATEP may require ICE buses and drivers. With criminal consequences, the Border Patrol requires the U.S. Marshal's Service to transport migrants to court, ICE to hold migrants in detention, the U.S. Attorney's Office to prosecute cases, and the U.S. federal judiciary to hear cases (GAO, 2017a).

Because these agencies face many demands on their resources, they may sometimes lack the capacity to address matters related to border apprehensions. In the early 2010s, federal courts requested that the Tucson sector, which first launched Streamline prosecutions, limit Streamline cases to approximately 70 per day (Capps et al., 2017). From 2008 to 2012, daily apprehensions in Tucson hit this cap sometime during the day on 99.3% of days.

Figure 4: Determinants of which Migrants Receive Sanctions



(A) LPM for AC (B) LPM for PC/CC

Notes: Each row of these figures reports the R^2 from an OLS regression of a binary indicator for the given CDS sanction on the covariates listed in the row title. Each covariate is an exhaustive set of dummy variables for the given category; the ... indicate the addition of covariates in the given row to covariates in the prior row with the ultimate row including the full set of controls.

If variation in the application of the CDS was due more to prevailing conditions in a given Border Patrol sector than to migrant characteristics, these characteristics should play little role in determining whether a migrant received sanctions. Figure 4 reports R^2 from a linear probability model (LPM) regressing an indicator for whether an apprehended migrant is subject to sanctions on controls for the location and the time of the apprehension, the demographic characteristics of the migrant, and the migrant's previous apprehensions. In panel A of Figure 4, the dependent variable indicates whether a migrant is subject to administrative consequences (AC). In row 1, we include only dummies for age, birth state, and previous apprehensions, which yields an R^2 of 0.055.[10] When we replace these dummies with indicators for the sector, year, month, day of week, and time of day of the apprehension in row 3, the R^2 jumps to 0.276. Adding back in dummies for

[10] Gronau (1998) shows that the R^2 in an LPM equals the difference between the average predicted probability in the two groups (i.e., how much the covariates differentiate CDS sanctioned migrants from non-CDS sanctioned migrants) and clarifies why the R^2 in an LPM is less likely to approach 1 than in the case of a continuous outcome.

migrant birth state, age, and previous apprehensions in row 5 raises the R^2 modestly to 0.323. Panel B repeats these regressions for programmatic/criminal consequences (PC/CC). Again, age, birth state, and previous apprehension dummies have little explanatory power, whereas the sector and timing of the apprehension have substantial explanatory power. During the CDS rollout, where and when a migrant was apprehended played a dominant role in determining whether he was subject to sanctions.

3 Empirical Specification

3.1 Regression framework

We evaluate how being subject to the CDS affects the likelihood that an apprehended migrant is re-apprehended in the future. Our specification is

$$y_{ist+\tau} = \beta \times CDS_{ist} + f\left(X_{it}, \alpha_s, \alpha_t\right) + \epsilon_{ist} \tag{1}$$

where $y_{ist+\tau}$ is an indicator for whether migrant i who is apprehended in Border Patrol sector s at time t is re-apprehended anywhere along the border within τ periods, for $\tau = 3$, 6, 12, or 18 months; CDS_{ist} is defined alternatively as an indicator for whether the migrant was subject to administrative consequences (AC) at apprehension, an indicator for whether the migrant was subject to any consequences at apprehension, or a vector that includes the AC indicator and an indicator for whether the migrant was subject to programmatic/criminal consequences (PC/CC) at apprehension; X_{it} includes indicators for the migrant's age cohort at apprehension, birth state in Mexico, and number of previous apprehensions; α_s indicates the Border Patrol sector of apprehension; α_t describes the timing of apprehension (year, month, day of week, and time of day); $f(\cdot)$ characterizes the manner in which we interact the control variables; and ϵ_{ist} is a disturbance term than captures unobserved variables that affect the likelihood of re-apprehension.

To control for variables that may be related both to whether the Border Patrol sanctions a migrant and to whether he re-attempts illegal entry, we define $f(\cdot)$ to generate interactions among X_{it}, α_s, and α_t. We first interact only the sector, year, and month of apprehension, then add interactions for day of week and time of day of apprehension, and then add migrant characteristics. Under the most complete set of interactions, we allow sanctions to be correlated with shocks that affect the

migrant's re-entry decision, as long as these can be modeled by sector×calendar-date×age×birth-state×previous-apprehension interactions. Our approach to identification would be invalid if there are additional characteristics of a migrant that affect his decision to re-attempt illegal entry and the decision of the Border Patrol to impose sanctions on him. Because agents decide whether to impose sanctions on a migrant in a matter of minutes, our assumption that, conditional on the controls, the assignment of consequences to a migrant was as good as random may not be unreasonable.

Comparing estimates of β across specifications reveals the sensitivity of the treatment effect to increasingly more-expansive controls for the characteristics of the migrant. We formally evaluate possible bias due to unobservables using the approach proposed by Altonji et al. (2005) and generalized by Oster (2017).

3.2 Interpreting the CDS treatment effect

In (1), we estimate how the imposition of sanctions affects the probability that a migrant is re-apprehended in the future. This probability can be written as

$$P(R_1) = P(E_1|C_0) \times P(A_1|C_0, E_1), \tag{2}$$

where $P(R_1)$ is the probability that a migrant apprehended at $t = 0$ is re-apprehended at $t = 1$; $P(E_1|C_0)$ is the probability that the migrant re-attempts illegal entry at $t = 1$, conditional on having been apprehended and having faced consequence $C_0 = \{0, 1\}$ at $t = 0$, where 0 indicates no sanction and 1 indicates a sanction; and $P(A_1|C_0, E_1)$ is the probability that the migrant is apprehended at $t = 1$, conditional on having been apprehended and faced consequence C_0 at $t = 0$ and on re-attempting illegal entry at $t = 1$. The treatment effect that we estimate in (1) is

$$\hat{\beta} = P(E_1|1) \times P(A_1|1) - P(E_1|0) \times P(A_1|0). \tag{3}$$

We (weakly) underestimate the impact of the CDS on the probability that a migrant re-attempts illegal entry if $P(A_1|1, E) \geq P(A_1|0, E_1)$. How would sanctions affect the incentive for a migrant to reduce his apprehension probability (e.g., by hiring a higher quality smuggler)? Consider two migrants who each have been apprehended once, where one was subject to sanctions and the other was not. Under administrative or criminal consequences, the sanctioned migrant has lost the ability to seek a legal entry visa for five years or more (and voided any visa application under review).

Because the risk of felony prosecution for a second apprehension is low, he may have less at stake in a subsequent crossing than the non-sanctioned migrant, who can still apply for a legal visa (or pursue a visa application under consideration). After a single apprehension, the sanctioned migrant may have a weaker incentive to reduce his probability of apprehension on a subsequent entry attempt, when compared to the non-sanctioned migrant, in which case $P(A_1|1,E) \geq P(A_1|0,E_1)$. Accumulating apprehensions exposes a previously sanctioned migrant to risk of felony conviction. Although standard prosecution is rare within our sample, applying to just 1.5% of apprehended migrants, a sanctioned migrant with multiple previous apprehensions may have a relatively strong incentive to avoid apprehension in a next crossing attempt, when compared to a non-sanctioned migrant with the same number of previous apprehensions. This discussion suggests that one way to gauge the sensitivity of our results to the confounding effects of changes in the apprehension probability is to allow the treatment effect to vary with the number of previous apprehensions. Estimating the CDS treatment effect for migrants with a single previous apprehensions is where it seems most plausible that $P(A_1|1,E) \geq P(A_1|0,E_1)$ and that we weakly underestimate the impact of the CDS on the probability that a migrant re-attempts illegal entry.[11]

4 Empirical Results

4.1 Administrative Consequences

Figure 5 reports estimation results for (1) using our sample of male Mexican nationals with six or fewer previous apprehensions who were apprehended between 2008 and 2012. The outcome variables, organized in panels A-D, are indicators for whether the migrant was re-apprehended in the following 3 or 18 months (with results at 6 and 12 month horizons reported in Appendix A). The first treatment we consider is whether the migrant was subject to administrative consequences (AC) at apprehension, shown in panels A and B. In row 1, the control variables are complete interactions among dummy variables for the Border Patrol sector, the fiscal year, and the month in which the apprehension occurred. In row 2, we add interactions with indicators for the time

[11] A factor affecting the ability of a migrant to finance multiple border-crossing attempts—and therefore also affecting the impact of the CDS on the apprehensions probability—is credit constraints (Angelucci, 2012). If successive attempts to cross the border each end in apprehension, a migrant may be progressively less able to marshal the resources to pay smuggling fees on each subsequent crossing. Consistent with this reasoning, data from the EMIF-Norte (see note 6) reveal that the likelihood that a recently apprehended migrant used a coyote (smuggler) on his most recent crossing attempt is negatively correlated with the number of times he has been apprehended in the recent past (Roberts, 2015).

of day and day of week of the apprehension; in row 3, we add interactions with indicators for the migrant's age and birth state; and in row 4, we add interactions with indicators for the migrant's number of previous apprehensions. In row 5, we go further and introduce fixed effects for the sector and calendar date of the apprehension (e.g., January 12, 2010 in Tucson); and in row 6, we interact those dummy variables with migrant age, birth state, and number of previous apprehensions. The figures include point estimates and 95% confidence intervals. Tables A3 and A4 present the full regression output including sample size, which varies across specifications, as interactive fixed-effect cells without variation in sanctions across migrants are omitted. We cluster standard errors by the sector-year-month combination (270 clusters), to account for the common exposure of migrants to policies defined at the sector level at a given time.[12]

Consider the estimate of the AC treatment on the 3-month re-apprehension rate in row 1 of panel A. The value of −0.064, which is very precisely estimated, indicates that migrants subject to administrative consequences were 6.4 *p.p.* less likely to be re-apprehended in the next 3 months (compared to a 2008 re-apprehension probability of 0.226). As we allow for more interactions between time and location of apprehension and migrant characteristics, there is essentially no change in the estimated treatment effect. In row 4, with the full set of controls, the estimate is −0.063. The more-exhaustive sector-date fixed effects in rows 5 and 6 do not materially change this point estimate, which is −0.066 in row 6. Table A3 explores impacts on the likelihood of re-apprehension at longer time horizons, and again the estimated treatment effects are insensitive to expanding the set of controls. The treatment effect diminishes modestly as we expand the re-apprehension time horizon. For results with exhaustive fixed effects in row 6 of Figure 5 (panels A and B), the AC treatment effect falls from −0.066 at 3 months to −0.046 at 18 months (2008 re-apprehension probability of 0.269). This attenuation could indicate that some of the sanction impact is psychological, where the trauma diminishes with time. Alternatively, it may take migrants time to build up the resources to undertake a second crossing, meaning impacts are lower at longer horizons.

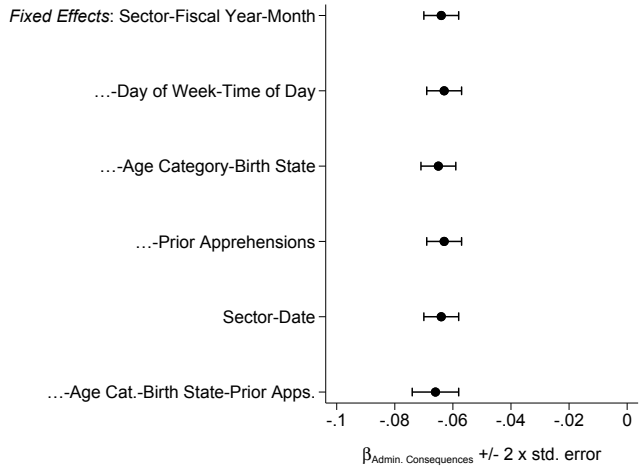
Overall, the stability of coefficients across fixed-effect specifications provides *prima facie* evidence against a large role for selection on unobservables to explain our findings. Consider panel A, where the R^2 increases from 0.076 in row 5 with sector-date fixed effects to 0.409 in row 6 when adding

[12] Table A3 also reports more conservative confidence intervals and p-values based on clustering at the level of the nine border patrol sectors. Given the small number of clusters, we apply the wild cluster bootstrap of Cameron et al. (2008). All point estimates remain significant at the 1% or 5% level.

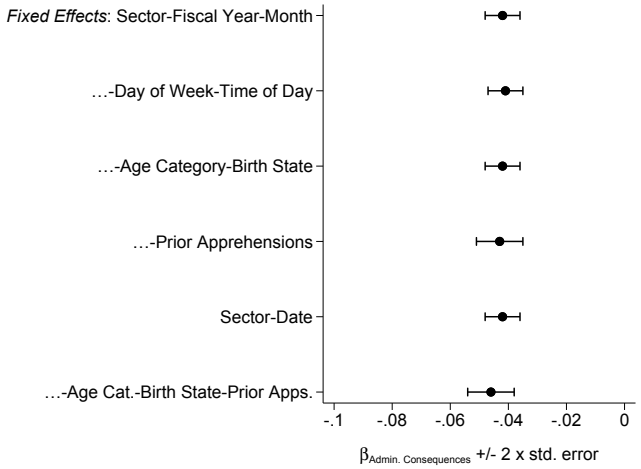
Figure 5: Impact of Exposure to CDS Sanctions on Probability of Re-apprehension

Administrative Consequences

(A) Re-apprehension within 3 months

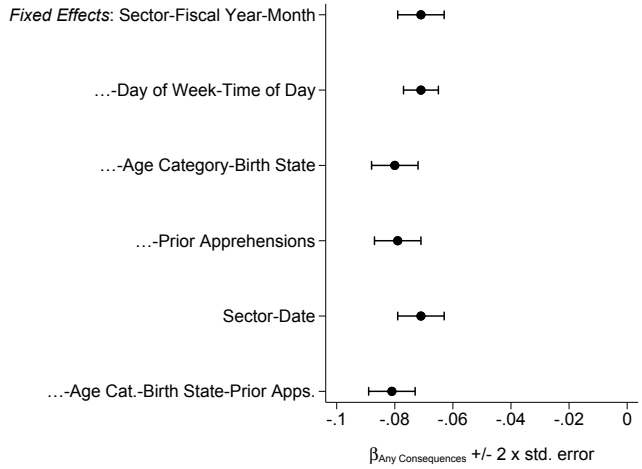


(B) Re-apprehension within 18 months

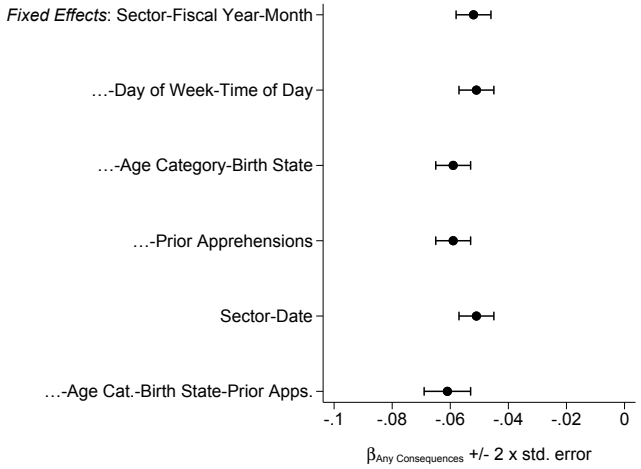


Any Consequences

(C) Re-apprehension within 3 months



(D) Re-apprehension within 18 months



Notes: Each row of these figures reports the point estimate and 95% confidence interval on the dummy variable for administrative consequences, in panels (A) and (B), or any consequences, in panels (C) and (D), in an OLS regression for re-apprehension within the next 3 months (panels A and C) or 18 months (panels B and D). Each row is a separate regression controlling for the fixed effects (FE) listed in the row title. These FE enter interactively where the ... indicate the addition of FE in the given row to FE in the prior rows. Standard errors are clustered by sector-fiscal year-month (270 clusters).

interactions with age, birth place, and number of previous apprehensions.[13] Despite this large increase in explanatory power, the treatment effect of AC sanctions remains effectively unchanged, going from -0.064 to -0.066. In other words, adding a large number of observable determinants of re-apprehension does not change the observed impact of AC. This pattern holds across our findings (see Table A3) and points to very limited selection on unobservables based on the Oster (2017) test.[14] The calculation here (comparing rows 5 and 6) suggests that selection on unobservables would have to be 482 times larger than selection on observables, which far exceeds the rule-of-thumb cutoff of 1 for observational studies. A similarly large Oster δ-statistic of 178.6 arises when comparing the specifications in rows 4 and 2. In short, selection on unobservables would have to be implausibly large to explain the effects on recidivism that we find.

4.2 Other Consequence Programs

In panels C and D of Figure 5, we repeat the analysis in the upper two panels, redefining the treatment as an indicator for any consequence, including administrative (AC), programmatic (PC) or criminal consequences (CC). This broader definition of any CDS sanction implies a larger reduction in recidivism than the AC treatment alone. For the most demanding specification in row 6, the estimated effect of treatment increases from -0.064 for AC alone to -0.081 for any consequence (AC, PC, and/or CC) at the 3-month horizon and from -0.046 to -0.061 at the 18-month horizon. Like the AC treatment effect, the any-consequence effect is stable across fixed-effect specifications.

Although the any-consequence treatment implies a larger reduction in recidivism, there is little difference in the effects of AC versus PC/CC. This can be seen in panel A of Table A5, which shows that when entered as separate indicators, the AC and PC/CC treatments have statistically indistinguishable effects across most time horizons, with the distinct effects of each being slightly smaller than the any-consequence treatment. At the 3-month horizon, for example, the AC coefficient is -0.066, which we fail to reject being different from the PC/CC coefficient of -0.060 (p-value of 0.36). We find analogous patterns for the 18-month horizon (see panel A of Table A6).

[13] This increase in R^2 is understated insomuch as the fixed effects fully absorb cells of observations within which there is a single migrant. These singleton cells do not contribute to the identifying variation in the point estimates, are omitted from the sample size in Table A3, and thus do not add to the R^2.

[14] The Oster (2017) δ-statistics are computed as $\delta = \left(\frac{\beta_c}{\beta_u - \beta_c}\right) \times \left(\frac{R_c^2 - R_u^2}{0.3 \times R_c^2}\right)$, where β_c is the coefficient estimate with additional controls, β_u is the reference coefficient estimate without those controls, and R_c^2 and R_u^2 are the corresponding R^2 from the respective regressions. δ is infinite (or undefined) when $\beta_c = \beta_u$ and $R_c^2 > R_u^2$.

Given the uneven application of PC and CC across sectors and time (see Section 2.3), we exploit different sources of spatial and temporal identifying variation when comparing across consequences. This prevents evaluating the conceptually ideal comparison across AC and PC/CC *within-person*. These different sources of identifying variation may explain why the combined treatment effect of AC and PC/CC is less than two times the AC or the PC/CC treatment alone (see panel B of Tables A5 and A6). Alternatively, the positive interaction between the AC and PC/CC treatments may be evidence of diminishing returns to additional sanctions, such that the combined effects of the full set of sanctions is less than two times the effect of AC or PC/CC alone.

To gauge the economic significance of the estimates, consider the impact of the CDS on recidivism in apprehensions 18 months after capture, as shown in row 6, panel B of Figure 5 for administrative consequences and row 6, panel D of Figure 5 for any consequences. Between 2008 and 2012, recidivism in apprehensions declined by 9.6 *p.p.* at the 18-month horizon (Figure 1). With the 2008-to-2012 increase in the incidence of the AC treatment of 58.5 *p.p.* $(73.9 - 15.4)$ and of the any-consequence treatment by 69.8 *p.p.* $(85.2 - 15.4)$ (see Table A2), the AC treatment effect is a reduction in recidivism equivalent to 28.0% $([.046 \times .585]/[.269 - .175])$ of the observed decline, and the any-consequence treatment effect is a reduction in recidivism equivalent to 44.4% $([.061 \times .698]/[.269 - .175])$ of the observed decline. CDS sanctions thus account for a substantial share of the observed decline in recidivism in apprehensions.

4.3 Recidivism in Apprehensions versus Recidivism in Attempted Illegal Entry

Following Section 3.2, we connect our results on recidivism in apprehensions to recidivism in attempted illegal entry by allowing the CDS treatment effect to vary with the migrant's number of previous apprehensions. A sanctioned migrant with a single previous apprehension may have a weaker incentive to avoid apprehension on a subsequent crossing than a non-sanctioned migrant with a single apprehension, in which case our estimated impact of sanctions on recidivism in apprehensions may weakly understate the impact of sanctions on recidivism in attempted illegal entry.

Table A7 reports extended regression results for the row 4 specifications in Figure 5. Panel A shows treatment effects for administrative consequences, and panel B does so for any consequences, where we allow these effects to vary according to whether a migrant has one, two, three, or four to six previous apprehensions. For re-apprehension within 18 months, shown in column

4, the estimated any-consequence treatment effect for migrants with a single previous apprehension (-0.056) is smaller in absolute value than that for migrants with two previous apprehensions ($-0.075 = -0.056 - 0.019$), where this difference is statistically significant. This pattern holds for both administrative consequences and PC/CC, and results are comparable at other time horizons for re-apprehension.

Taking the CDS treatment effect for migrants with a single previous apprehension as our most conservative estimate, we obtain magnitudes that are only slightly smaller than in Figure 5. In going from the full sample to the single-apprehension sample, the reduction in re-apprehension rates induced by any consequences falls from -0.079 to -0.074, at the 3-month horizon, and from -0.059 to -0.056, at the 18-month horizon. These results suggest limited scope for the impact of sanctions on the probability of apprehension to confound our estimates, indicating that the CDS treatment effect on recidivism in apprehensions is informative about the (more-policy-relevant) CDS treatment effect on recidivism in attempted illegal entry.

5 Discussion

Undocumented immigration is a highly contentious issue in the U.S. Some critique the government's success in securing borders against illegal entry, while others object to the treatment of immigrants by authorities. Early research on border enforcement inspired pessimism about government efforts to deter undocumented immigration. As the number of Border Patrol agents grew in the 1990s and early 2000s, so too did illegal entry. Since the late 2000s, the Border Patrol has carried out another personnel buildup, while it has augmented its enforcement strategy by constructing additional physical barriers, deploying new detection technologies, and imposing tougher sanctions on apprehended migrants. Sanctions work by reducing the viability of legal immigration in the future and by raising the risk of incarceration. We find that sanctions have large negative impacts on recidivism in apprehensions and, plausibly, on recidivism in illegal entry. The crucial next step in designing viable immigration policies is to determine the relative cost effectiveness of alternative strategies for deterring illegal entry. Such analysis is not yet feasible but would be with expanded access to data on U.S. border enforcement efforts.

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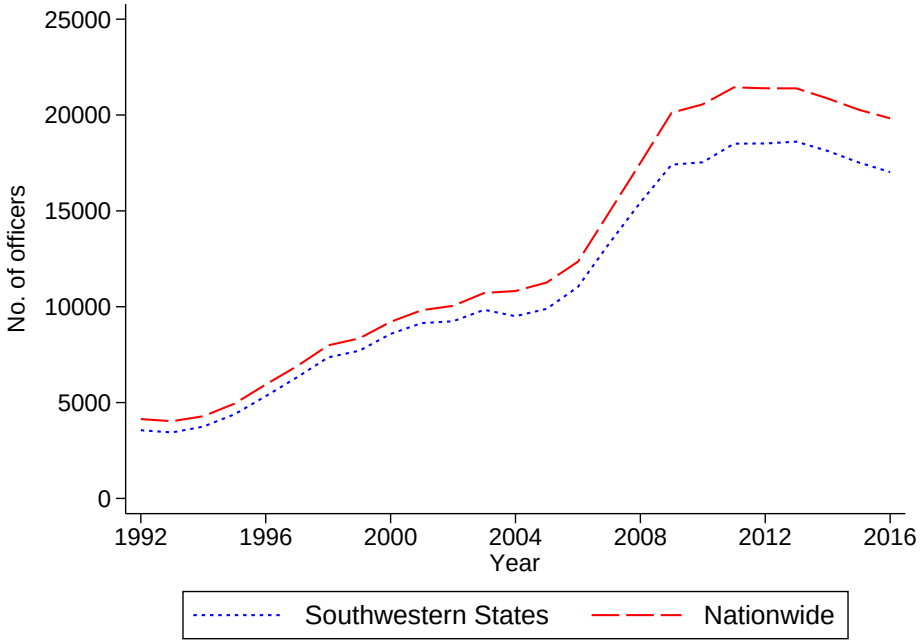
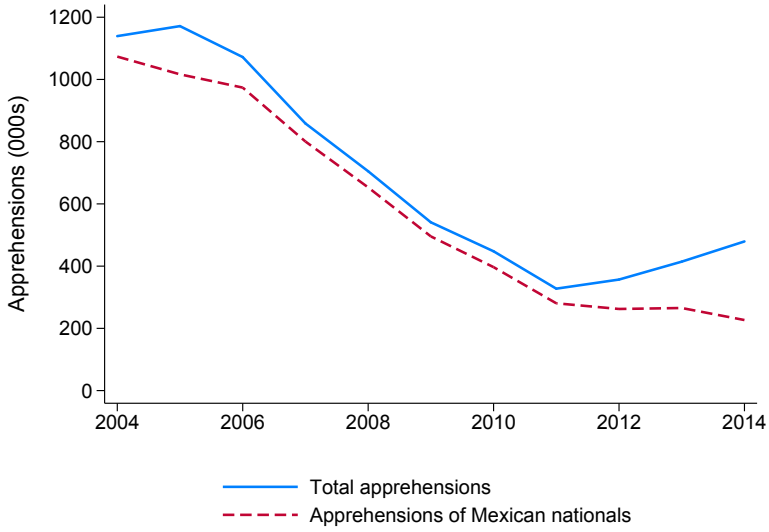
Appendix A: Figures and Tables

Figure A1: Number of Border Patrol Officers along Southwestern Border and Nationwide



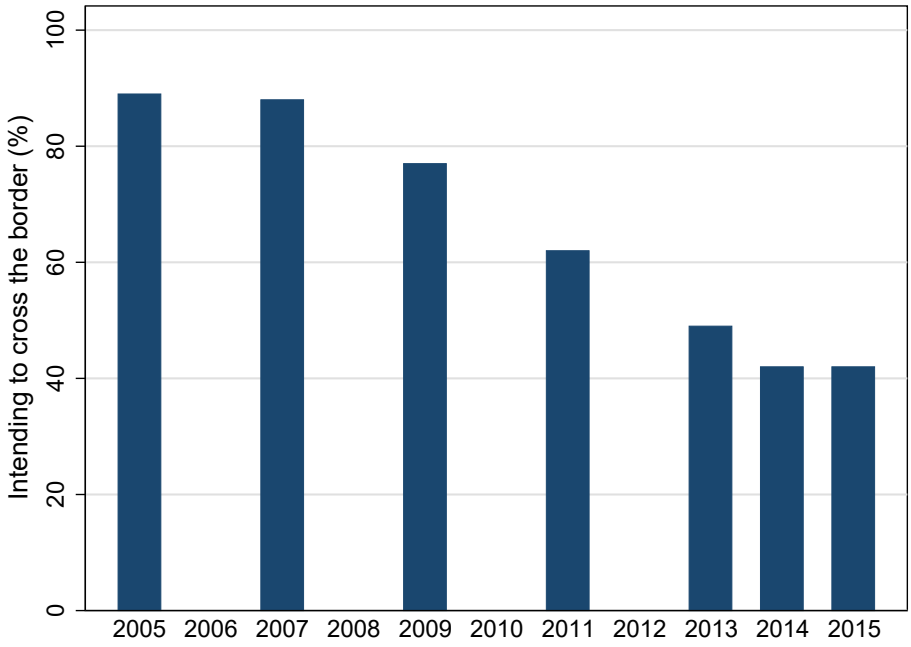
Note: Data are from the U.S. Customs and Border Protection, *CBP Border Security Report, FY2017*.

Figure A2: Apprehensions by the U.S. Border Patrol at the Southwestern Border



Note: Data are from the U.S. Department of Homeland Security, *Yearbook of Immigration Statistics*, various years.

Figure A3: Apprehended Migrants Intending to Cross the Border within Next 3 Months



Note: Data are from EMIF-Norte Surveys (Surveys of Migration in the Northern Border of Mexico) 2005 to 2015 and Roberts (2017).

Table A1: Summary Statistics

Characteristics of migrant		Fraction	Location, time of apprehension		Fraction
Age	16-17	0.049	Border Patrol	San Diego	0.173
	18-20	0.159	sector	El Centro	0.059
	21-24	0.189		Yuma	0.013
	25-28	0.175		Tucson	0.531
	29-33	0.178		El Paso	0.035
	34-40	0.163		Big Bend	0.007
	41-50	0.087		Del Rio	0.039
				Laredo	0.043
Birth region	Border	0.115		Rio Grande Valley	0.104
in Mexico	North	0.125			
	Center North	0.180	Fiscal year	2008	0.282
	Center	0.198		2009	0.233
	Center South	0.314		2010	0.198
	South	0.067		2011	0.145
				2012	0.143
Number of prior	1	0.458			
apprehensions	2	0.254	Month	January	0.083
	3	0.140		February	0.102
	4	0.077		March	0.143
	5	0.044		April	0.130
	6	0.027		May	0.100
				June	0.077
Re-apprehended	Within 3 mos.	0.206		July	0.064
	Within 6 mos.	0.226		August	0.064
	Within 12 mos.	0.250		September	0.058
	Within 18 mos.	0.264		October	0.075
				November	0.059
Administrative	Removal order	0.571		December	0.045
consequences	Reinstatement order	0.429			
	Total	344,974	Day of week	Sunday	0.134
				Monday	0.142
Programmatic	ATEP	0.862		Tuesday	0.149
consequences	MIRP	0.138		Wednesday	0.149
	Total	189,532		Thursday	0.149
				Friday	0.142
Criminal	Streamline	0.835		Saturday	0.135
consequences	Standard Prosecution	0.165	Time of day	12am-7am	0.258
	Total	85,683		7am-12pm	0.222
				12pm-6pm	0.297
				6pm-12am	0.223

Note: This table provides summary statistics on our sample of apprehensions of male Mexican nationals, ages 16 to 50, with six or fewer previous apprehensions, where the apprehension in question occurred between ports of entry along the Southwester border between 2008 and 2012. The re-apprehension statistics are cumulative rather than mutually exclusive. For those apprehended in 2005, we track whether they had been apprehended during the 18 months back into 2003; for those apprehended in 2012, we track whether they were apprehended in the 18 months out into 2014. The full data cover 2,824,776 apprehensions of Mexican nationals between 2005 and 2012. Restricting the sample to men drops 437,618 apprehensions of women, to ages 16 to 50 drops 71,519 apprehensions of younger and older males, and to those with fewer than seven previous apprehensions drops another 102,704 apprehensions. The final sample contains 973,171 apprehensions.

Table A2: Details on CDS Rollout

Consequence Type	2008	2009	2010	2011	2012	2008-12
Administrative	0.154	0.261	0.330	0.550	0.739	0.354
Programmatic		0.148	0.167	0.393	0.492	0.195
Criminal		0.083	0.086	0.136	0.226	0.088
Programmatic or Criminal		0.229	0.249	0.510	0.680	0.273
Administrative & Programmatic		0.004	0.043	0.242	0.385	0.100
Administrative & Criminal		0.072	0.081	0.132	0.218	0.083
Administrative & Programmatic/Criminal		0.076	0.120	0.355	0.567	0.174
Any	0.154	0.414	0.458	0.705	0.852	0.454

Notes: Fraction of sample apprehended migrants (male Mexican nationals, ages 16-50, with 6 or fewer prior apprehensions) subject to given consequence programs during rollout period for CDS.

Table A3: Impact of Administrative Consequences on Probability of Re-Apprehension

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Pr(Re-Apprehension within 3 months)					
Administrative Consequences	-0.064***	-0.063***	-0.065***	-0.063***	-0.064***	-0.066***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
	[0.010]	[0.014]	[0.022]	[0.015]	[0.011]	[0.005]
Oster |δ| Statistic			83.7	∞		482
Relative to Column …			2	2		5
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
R-squared	0.060	0.074	0.326	0.401	0.076	0.409
Adjusted R-squared	0.059	0.061	0.079	0.099	0.061	0.103
	Panel B: Pr(Re-Apprehension within 6 months)					
Administrative Consequences	-0.055***	-0.054***	-0.055***	-0.054***	-0.055***	-0.058***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
	[0.019]	[0.020]	[0.048]	[0.036]	[0.020]	[0.013]
Oster |δ| Statistic			144.4	∞		308
Relative to Column …			2	2		5
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
R-squared	0.053	0.068	0.320	0.396	0.070	0.405
Adjusted R-squared	0.053	0.054	0.072	0.092	0.055	0.096
	Panel C: Pr(Re-Apprehension within 12 months)					
Administrative Consequences	-0.047***	-0.046***	-0.047***	-0.047***	-0.047***	-0.050***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
	[0.025]	[0.033]	[0.083]	[0.063]	[0.032]	[0.046]
Oster |δ| Statistic			125.8	131.9		291.7
Relative to Column …			2	2		5
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
R-squared	0.047	0.062	0.315	0.392	0.064	0.400
Adjusted R-squared	0.047	0.048	0.065	0.086	0.049	0.089
	Panel D: Pr(Re-Apprehension within 18 months)					
Administrative Consequences	-0.042***	-0.041***	-0.042***	-0.043***	-0.042***	-0.046***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
	[0.035]	[0.042]	[0.098]	[0.068]	[0.039]	[0.054]
Oster |δ| Statistic			113.2	60.7		208.4
Relative to Column …			2	2		5
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
R-squared	0.046	0.060	0.313	0.391	0.062	0.399
Adjusted R-squared	0.045	0.047	0.063	0.084	0.047	0.086
Interactive Fixed Effects						
Sector x Fiscal Year x Month	✓	✓	✓	✓		
… x Day of Week x Time of Day		✓	✓	✓		
… x Age Category x Birth State			✓	✓		
… x Number of Prior Apprehensions				✓		
Sector x Calendar Date					✓	✓
… x Age Category x Birth State x Prior Apprehensions						✓

Note: This table reports estimates of equation (1) for the effect of administrative consequences on the probability of re-apprehension within 3, 6, 12, and 18 months after the initial apprehension. Coefficients and standard errors are those shown in Figure 5. Sample sizes decline with the inclusion of additional interactive fixed effects because we omit the singleton cells for which there is one observation. Standard errors (clustered by sector-year-month) are in parentheses. Stars indicate significance at 10% *, 5% **, and 1% *** level for those standard errors. Within brackets, we report the p-values based on a wild bootstrap procedure clustering at the sector level (of which there are 9).

Table A4: Impact of Any CDS Sanction on Probability of Re-Apprehension

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Pr(Re-Apprehension within 3 months)					
Any Consequences	-0.071***	-0.071***	-0.080***	-0.079***	-0.071***	-0.081***
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
	[0.009]	[0.009]	[0.028]	[0.028]	[0.009]	[0.034]
Oster |δ| Statistic			22.8	26.8		116.8
Relative to Column …			2	2		5
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
R-squared	0.061	0.075	0.327	0.402	0.077	0.410
Adjusted R-squared	0.060	0.062	0.081	0.101	0.062	0.104
	Panel B: Pr(Re-Apprehension within 6 months)					
Any Consequences	-0.064***	-0.064***	-0.072***	-0.071***	-0.064***	-0.074***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)
	[0.008]	[0.006]	[0.023]	[0.024]	[0.007]	[0.034]
Oster |δ| Statistic			23.5	2.79		118.4
Relative to Column …			2	2		5
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
R-squared	0.054	0.069	0.321	0.397	0.070	0.406
Adjusted R-squared	0.054	0.055	0.074	0.094	0.056	0.098
	Panel C: Pr(Re-Apprehension within 12 months)					
Any Consequences	-0.056***	-0.056***	-0.064***	-0.064***	-0.055***	-0.066***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
	[0.005]	[0.005]	[0.014]	[0.014]	[0.004]	[0.032]
Oster |δ| Statistic			21.4	22.4		103.4
Relative to Column …			2	2		5
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
R-squared	0.048	0.063	0.316	0.393	0.065	0.401
Adjusted R-squared	0.048	0.049	0.066	0.088	0.050	0.090
	Panel D: Pr(Re-Apprehension within 18 months)					
Any Consequences	-0.052***	-0.051***	-0.059***	-0.059***	-0.051***	-0.061***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
	[0.005]	[0.004]	[0.012]	[0.019]	[0.004]	[0.028]
Oster |δ| Statistic			19.8	20.7		110.5
Relative to Column …			2	2		5
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
R-squared	0.046	0.061	0.314	0.391	0.062	0.399
Adjusted R-squared	0.046	0.047	0.064	0.085	0.048	0.087
Interactive Fixed Effects						
Sector x Fiscal Year x Month	✓	✓	✓	✓		
… x Day of Week x Time of Day		✓	✓	✓		
… x Age Category x Birth State			✓	✓		
… x Number of Prior Apprehensions				✓		
Sector x Calendar Date					✓	✓
… x Age Category x Birth State x Prior Apprehensions						✓

Note: This table replaces administrative consequences with any consequences (administrative, programmatic, and (or) or criminal) and re-estimates the specifications in Table A3. Coefficients and standard errors are those shown in Figure 5. Standard errors (clustered by sector-year-month) are in parentheses. Stars indicate significance at 10% *, 5% **, and 1% *** level for those standard errors. Within brackets, we report the p-values based on a wild bootstrap procedure clustering at the sector level (of which there are 9).

Table A5: Comparing Administrative and Other Consequences, 3-Month Horizon

	Dep. Var.: Pr(Re-Apprehension within 3 months)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel (A)					
Admin. Conseq.	-0.060***	-0.060***	-0.065***	-0.060***	-0.063***	-0.066***
(AC)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Program or Crim. Conseq.	-0.043***	-0.042***	-0.059***	-0.059***	-0.042***	-0.060***
(PC/CC)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
Adjusted R-squared	0.060	0.062	0.081	0.101	0.062	0.105
	Panel (B)					
Admin. Conseq.	-0.070***	-0.070***	-0.075***	-0.075***	-0.070***	-0.076***
(AC)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Program or Crim. Conseq.	-0.059***	-0.060***	-0.076***	-0.077***	-0.058***	-0.077***
(PC/CC)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)
AC x PC/CC	0.034***	0.036***	0.039***	0.043***	0.033***	0.039***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
Adjusted R-squared	0.061	0.062	0.081	0.101	0.063	0.105
Interactive Fixed Effects						
Sector x Fiscal Year x Month	✓	✓	✓	✓		
… x Day of Week x Time of Day		✓	✓	✓		
… x Age Category x Birth State			✓	✓		
… x Number of Prior Apprehensions				✓		
Sector x Calendar Date					✓	✓
… x Age Category x Birth State x Prior Apprehensions						✓

Note: This table reports estimates of equation (1) for the probability of re-apprehension within 3 months after the initial apprehension, allowing administrative and programmatic/criminal consequences to have different effects on recidivism in apprehensions. Panel A enters the two consequences separately; panel B allows for their interaction. Standard errors are clustered by sector-year-month. Stars indicate significance at 10% *, 5% **, and 1% *** level.

Table A6: Comparing Administrative and Other Consequences, 18-Month Horizon

	Dep. Var.: Pr(Re-Apprehension within 18 months)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel (A)					
Admin. Conseq.	-0.039***	-0.039***	-0.042***	-0.043***	-0.039***	-0.046***
(AC)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Program or Crim. Conseq.	-0.037***	-0.036***	-0.049***	-0.048***	-0.036***	-0.050***
(PC/CC)	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
Adjusted R-squared	0.046	0.047	0.064	0.085	0.048	0.088
	Panel (B)					
Admin. Conseq.	-0.047***	-0.047***	-0.052***	-0.053***	-0.047***	-0.055***
(AC)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Program or Crim. Conseq.	-0.050***	-0.050***	-0.065***	-0.065***	-0.049***	-0.065***
(PC/CC)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
AC x PC/CC	0.028***	0.030***	0.036***	0.038***	0.027***	0.033***
	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
Adjusted R-squared	0.046	0.048	0.064	0.085	0.048	0.088
Interactive Fixed Effects						
Sector x Fiscal Year x Month	✓	✓	✓	✓		
… x Day of Week x Time of Day		✓	✓	✓		
… x Age Category x Birth State			✓	✓		
… x Number of Prior Apprehensions				✓		
Sector x Calendar Date					✓	✓
… x Age Category x Birth State x Prior Apprehensions						✓

Note: This table reports estimates of equation (1) for the probability of re-apprehension within 18 months after the initial apprehension, allowing administrative and programmatic/criminal consequences to have different effects on recidivism in apprehensions. Panel A enters the two consequences separately; panel B allows for their interaction. Standard errors are clustered by sector-year-month. Stars indicate significance at 10% *, 5% **, and 1% *** level.

Table A7: Heterogeneous Impacts of Consequence Programs by No. of Previous Apprehensions

(A) Administrative Consequences

	(1)	(2)	(3)	(4)
Pr(Re-apprehension within … months)	3	6	12	18
Administrative Consequences	-0.058***	-0.049***	-0.043***	-0.039***
	(0.003)	(0.003)	(0.003)	(0.003)
Administrative Consequences x 2 Prior Apprehensions	-0.025***	-0.024***	-0.022***	-0.021***
	(0.005)	(0.005)	(0.005)	(0.005)
Administrative Consequences x 3 Prior Apprehensions	-0.029***	-0.027***	-0.015	-0.015
	(0.010)	(0.010)	(0.011)	(0.011)
Administrative Consequences x 4-6 Prior Apprehensions	-0.011	-0.007	-0.000	0.009
	(0.017)	(0.018)	(0.018)	(0.020)
Number of Observations	512,727	512,727	512,727	512,727
R-squared	0.401	0.396	0.392	0.391
Adjusted R-squared	0.099	0.092	0.086	0.084

(B) Any Consequences

	(1)	(2)	(3)	(4)
Pr(Re-apprehension within … months)	3	6	12	18
Any Consequences	-0.074***	-0.067***	-0.060***	-0.056***
	(0.004)	(0.004)	(0.004)	(0.004)
Any Consequences x 2 Prior Apprehensions	-0.024***	-0.022***	-0.019***	-0.019***
	(0.005)	(0.006)	(0.006)	(0.006)
Any Consequences x 3 Prior Apprehensions	-0.019**	-0.014	-0.002	-0.001
	(0.009)	(0.009)	(0.010)	(0.011)
Any Consequences x 4-6 Prior Apprehensions	-0.004	-0.003	0.003	0.011
	(0.018)	(0.019)	(0.019)	(0.020)
Number of Observations	512,727	512,727	512,727	512,727
R-squared	0.402	0.397	0.393	0.391
Adjusted R-squared	0.101	0.094	0.088	0.085
Interactive Fixed Effects				
Sector x Fiscal Year x Month	✓	✓	✓	✓
… x Day of Week x Time of Day	✓	✓	✓	✓
… x Age Category x Birth State	✓	✓	✓	✓
… x Number of Prior Apprehensions	✓	✓	✓	✓

Note: This table reports estimates of the regressions in column 4 of Table A3, shown in panel A, and in column 4 of Table A4, shown in panel B, in which we allow the impact of consequence programs on the probability of re-apprehension to vary with the number of previous apprehensions for an individual. Standard errors are clustered by sector-year-month. Stars indicate significance at 10% *, 5% **, and 1% *** level.

Appendix B: Estimating Capacity Constraints in Figure 3

In Section 2.3, we discuss the results in Figure 3 demonstrating the staffing constraints in moving from voluntary return to administrative consequences under the CDS. For each Border Patrol sector s, we compute the share of officer time that would be absorbed by applying AC to all apprehended migrants on a given day d from 2008 to 2012 based on the equation:

$$agent\ time_{sd} = 100 \times \left[\frac{(1.5 - 0.25) \times (0.92 \times apprehensions_{sd})}{(agents_{sd} - (0.8 \times agents_{s,2007})) \times 8 \times 0.51 \times 0.916} \right] \tag{4}$$

where (1.5-0.25) captures the increase in agent man-hours to go from processing one VR to processing one AC; 0.92 is the share of apprehensions that were not already subject to AC as of 2008 (i.e, 92% of migrants received VR in 2007); $(agents_{sd} - (0.8 \times agents_{s,2007}))$ is agent time available after subtracting the fraction needed for essential operations (e.g., patrolling the border, making apprehensions) which is set to 80% of the level of 2007 agent activities; 8 is the number of potential hours available per agent per day; 0.51 is the fraction of each hour that agents work in operations after accounting for reported time not on duty, on breaks, in training, or performing administrative tasks; and 0.916 is the fraction of operations time not spent on traffic checkpoints (which occur relatively far from the border itself, impeding agents who man check points from performing other duties). These parameter values in equation (4) are based on an in-depth analysis by the U.S. Government Accountability Office of time use by Border Patrol agents along the U.S.-Mexico border in the early 2010s (GAO, 2017b). Figure 3 plots the resulting variation using sector–day observations on the total number of apprehensions, and sector–year observations on the total number of agents. Note that the number of apprehensions used in equation (4) is based on our sample and, hence, likely understates demands on agent time, as it excludes minors, serious criminals, and non-Mexican nationals, which account for 15% of total apprehensions during 2008-2012.