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

The mandate of the U.S. Border Patrol is, understandably enough, to safeguard U.S. borders.[1] Substantial and persistent undocumented immigration from Mexico in recent decades has been taken as *prima facie* evidence of borders that are less than fully controlled and therefore as a challenge for policy makers to address. To reduce illegal labor inflows, the government uses its enforcement resources—expenditures on patrol manpower, physical barriers, surveillance technology, and punitive actions—to prevent and deter illegal entry. In addition to interdicting migrants, the intent is to influence behavior both behind the border—by discouraging prospective migrants from attempting an illegal crossing—and at the border—by discouraging the substantial number of migrants apprehended during illegal crossings from trying again.[2] In this paper, we examine how penalties on illegal border crossings affect the behavior of recently apprehended immigrants from Mexico and thereby further the government objective of at-the-border deterrence.

Approximately two-thirds of undocumented migrants from Mexico 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 migrants caught at the border to estimate the impact of the CDS on recidivism in apprehensions.

The CDS rollout occurred amid 20 years of intensifying U.S. border enforcement (Roberts et al., 2013). Between 1992 and 2007, the U.S. government quadrupled the number of Border Patrol agents

[1] The *Performance and Accountability Report* of U.S. Customs and Border Protection, which oversees the Border Patrol, describes its mission as "managing, securing, and controlling our Nation's borders" (USCBP, 2012).

[2] On the evolution of deterrence strategies for border enforcement, see Roberts et al. (2013) and Alden (2017).

(Figure A1), which pushed the Border Patrol's budget from $551 million in 1992 to $2.5 billion in 2008 (2015 USD).[3] 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 impacts on attempted illegal entry (Gathmann, 2008; Orrenius and Zavodny, 2005; Massey et al., 2016), though impacts were 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),[4] and, after decades of growth, the U.S. population of undocumented immigrants from Mexico fell from 6.9 million in 2007 to 5.8 million in 2014 and then to 4.9 million in 2017 (Gonzalez-Barrera and Krogstad, 2019). Although the Great Recession and demographic shifts in Mexico explain some of these changes (Hanson et al., 2017), the post-2010 U.S. economic recovery did not engender a rebound in illegal entry, and population changes have been too gradual to account for the speed of 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).[5]

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, which cover all adult Mexican nationals apprehended at the Southwestern border between 2005 to 2012, permit such an analysis (U.S. Customs and Border Protection Agency, 2015). When a migrant is apprehended near the border, he is processed by the Border Patrol, which involves taking fingerprints and recording details of the apprehension, including whether the migrant is subject to

[3]Over 1992, to 2008, the 9.4% annual growth in the Border Patrol's budget compares to 4.8% annual growth in total federal government outlays. During the 2008-2012 period that we study, the Border Patrol's budget grew by 9.7% annually, compared to 4.3% growth in total federal outlays.

[4]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 often seek asylum when confronting U.S. immigration authorities, which invokes distinct immigration procedures and necessarily involves multiple government agencies to adjudicate their request (Amuedo-Dorantes et al., 2015).

[5]Other work examines consequences of changing 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), and how altering U.S. visa policies affects incentives for unauthorized immigration (Liu, 2019; Kovak and Lessem, 2020).

the CDS. By matching fingerprints across records, we determine if a migrant apprehended today is re-apprehended at a later date. Re-apprehensions are common,[6] with most occurring within a few months of the initial capture.[7]

In conducting our study, 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. To remove any mechanical connection between migrant characteristics and sanctions, we restrict the sample to adult men who have been apprehended six or fewer times, which represents 80% of apprehensions during our sample period. The Border Patrol does not release information on the apprehension of children, leaving their data out of our sample. Excluding women, who frequently migrate with children, avoids the confounding effects of Border Patrol actions that shield women with children from certain sanctions. For their part, the small number of migrants with many prior apprehensions are typically treated as potential smugglers and automatically subject to sanctions. In the analysis, we exploit the volume and richness of the apprehension records by controlling for the complete set of interactions among migrant age, birthplace, apprehension history, 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 birth state, birth cohort, and apprehension history, and were apprehended in the same location on the same day, though at different times. Our identifying assumption is that during the CDS rollout, the assignment of penalties was as good as random, conditional on the controls.

Our approach exploits hour-by-hour variation in the ability of the Border Patrol to deliver sanctions. During the implementation of the CDS, individual Border Patrol sectors had discretion in when and how to deliver penalties.[8] Sanctions capacity is constrained by the supply of Border Patrol officer time—officers have multiple duties to fulfill and imposing sanctions quadruples time required to process an apprehension—and resources needed from other government agencies—such as space in immigration courts or detention facilities (Capps et al., 2017). Because crossing the border illegally is typically a multi-day affair through rugged terrain distant from population centers, there

[6]Retrospective surveys of Mexican migrants also find that a high fraction of those apprehended at the border re-attempt illegal entry (Orrenius and Zavodny, 2005; Massey et al., 2016).

[7]Migrants re-apprehended within a few weeks or months of their initial capture are likely cases in which the individual is seeking to enter the U.S. during a given seasonal labor-market cycle; those re-apprehended a year or so hence are cases in which the migrant may be seeking to enter for the following year's cycle.

[8]Along the Southwestern border, these sectors are: San Diego and El Centro (California); Yuma and Tucson (Arizona); and El Paso, Big Bend, Del Rio, Laredo, and the Rio Grande Valley (Texas).

is little predictability in the exact time or location when a migrant may encounter the Border Patrol during a crossing attempt. This unpredictability, combined with the large volume of attempted crossings, creates high-frequency volatility in apprehensions. Whereas some migrants in our sample were captured following a temporary lull in apprehensions, others were captured following a spike in apprehensions. Relative to the latter group, the former group would be more likely to encounter slack constraints in sanctions capacity, and more likely, all else equal, to be subject to sanctions. Consistent with this reasoning, we find that conditional on the set of controls, the probability that a migrant is sanctioned is strongly negatively correlated with the regional volume of apprehensions around the time of his capture. And, consistent with our assumption that sanctions are driven more by capacity constraints than by migrant characteristics, once we control for date-by-apprehension location interactions, further controlling for individual characteristics leaves our estimates of the impact of sanctions on recidivism in apprehensions materially unchanged.

The second empirical challenge we confront is that, analogous to research on criminality,[9] we do not observe recidivism in illicit activity (attempted entry) but in imprisonment (apprehension). Our estimated impact of the CDS on recidivism combines its impact on the probability of re-attempting illegal entry and its impact on the probability of capture, conditional on re-attempting entry. If exposure to the CDS has no impact on the future probability of re-apprehension, our estimated impact on recidivism in apprehensions will understate its impact on attempted illegal entry. If, instead, CDS exposure induces migrants to intensify efforts to avoid capture during subsequent crossings, we may over-estimate the impact. To resolve this ambiguity, consider two migrants, each of whom is apprehended on his initial crossing attempt, where the first is subject to sanctions and the second is not. Because the sanctioned migrant has lost the option of seeking a legal immigration visa for some period of time, he may have a weaker incentive to avoid re-capture on a second crossing, relative to the unsanctioned migrant. Any tendency to over-estimate the CDS treatment effect is thus likely to be weakest for migrants with a single prior apprehension. Restricting the estimation to migrants with one previous apprehension produces little change in our results, suggesting that risk of over-estimating impacts of CDS exposure may not be an overriding concern.

To preview our findings, migrants subject to administrative consequences are 6.6 percentage points (p.p.) less likely to be re-apprehended in the next 3 months (off of a 2008 rate of 22.0%)

[9] 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).

and 4.6 *p.p.* less likely to be re-apprehended in the next 18 months (off of a 2008 rate of 26.9%). Migrants subject to any sanctions are 8.1 *p.p.* and 6.1 *p.p.* less likely to be re-apprehended at these horizons. These estimates are unaffected by controlling for individual characteristics, limiting the sample to migrants with one prior apprehension, or restricting the sample to time periods when capacity constraints on sanctions appear to bind. The stability in our estimates, despite the large number of fixed effects, suggests limited scope for selection-on-unobservables to explain our findings (Altonji et al., 2005; Oster, 2017), which stands in contrast to other recent work on recidivism (Bhuller et al., 2016). Our estimated magnitudes imply that, holding constant the impact of the CDS on the number of migrants who attempt illegal entry, the rollout of the CDS can account for 28% to 44% of the reduction in re-apprehension rates over 2008 to 2012.

In Section 2, we describe the apprehensions data and the Consequence Delivery System. In Section 3, we present our estimating equation and consider challenges to identification. In Section 4, we provide the empirical results. And in Section 5, we discuss the implications of our findings.

2 Data and Policy Context

U.S. immigration enforcement falls under the purview of the Department of State, which screens applicants for immigration visas, the Department of Homeland Security, which regulates U.S. borders and ports of entry and investigates infractions of immigration law in the interior U.S., and the Department of Justice, which prosecutes violations of U.S. immigration statutes. Within Homeland Security, U.S. Customs and Border Protection (CBP) manages and monitors U.S. borders, with responsibilities divided between the Office of Field Operations, which handles inspections at U.S. entry ports, and the Border Patrol, which takes responsibility for impeding the illegal inflow of goods or people between ports of entry (Roberts, 2015). The job of the Border Patrol is both to interdict unlawful entrants and to deter unlawful entry. Some enforcement activities—such as patrolling the border, maintaining physical border barriers, and monitoring the border using surveillance technology—enhance both interdiction and deterrence. Other enforcement activities, including sanctioning undocumented immigrants, are primarily for deterrence and therefore suitably evaluated in terms of their impact on reducing attempts at illegal entry. CBP uses the recidivism rate (the fraction of first-time apprehendees who are later re-apprehended) and the re-apprehension rate (the number of

total apprehensions per apprehended migrant) as primary measures of the effectiveness of at-the-border deterrence, with the stated goal of driving both rates toward zero (Roberts, 2015; Capps et al., 2017; GAO, 2017a). We study deterrence at the border itself: whether sanctions discourage migrants apprehended during an illegal crossing attempt from attempting to cross again.

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 (U.S. Customs and Border Protection Agency, 2015).[10] After apprehension, the Border Patrol fingerprints migrants and takes 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 the period 2008 to 2012, which spans the CDS rollout. Our sample contains 973,171 apprehensions, which represent 79.5% of Mexican nationals apprehended over 2008-2012.[11] The Tucson sector of the Border Patrol accounts for 53% of apprehensions (Appendix Table A1). When border enforcement intensified in the 1990s, many migrants switched from single-day crossings near border cities to multi-day crossings in remote 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. 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 border crossings. Whereas relatively few apprehended migrants are from Mexican states that border the U.S. (11.5%) or states in Mexico's south (7.4%), a majority are from central Mexico (68.0%), as consistent with historical patterns (Massey et al., 1994).

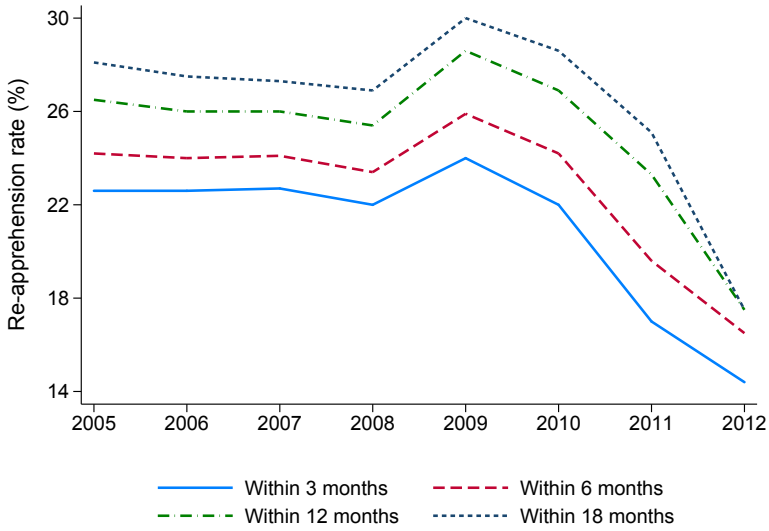
Figure 1 plots the fraction of apprehended migrants who were re-apprehended in the following 3, 6, 12 or 18 months. While re-apprehension rates are roughly stable between 2005 and 2009,

[10] Most apprehensions of deportable aliens occur between ports of entry at the U.S.-Mexico border and therefore are the result of operations by the U.S. Border Patrol. A deportable alien is someone present in the U.S. without having been admitted, who is encountered by an immigration officer within 100 air miles of the border, and who has not been present in the U.S. for a 14-day period immediately before their arrest.

[11] We requested apprehension records covering the CDS rollout and were given data for adult Mexican nationals over this time period. To our knowledge, this is the first time that CBP has released such data for academic research.

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.* at the 3-month frequency and 10.6 *p.p.* at the 18-month frequency. This decline in recidivism is corroborated by the EMIF-Norte (Survey of Migration in the Northern Border, El Colegio de la Frontera Norte, 2015), which surveys apprehended migrants returning to Mexico (see Appendix Figure A3). Its data show that 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.[12]

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 population of male Mexican nationals 16 to 50 years of age with six or fewer previous apprehensions.

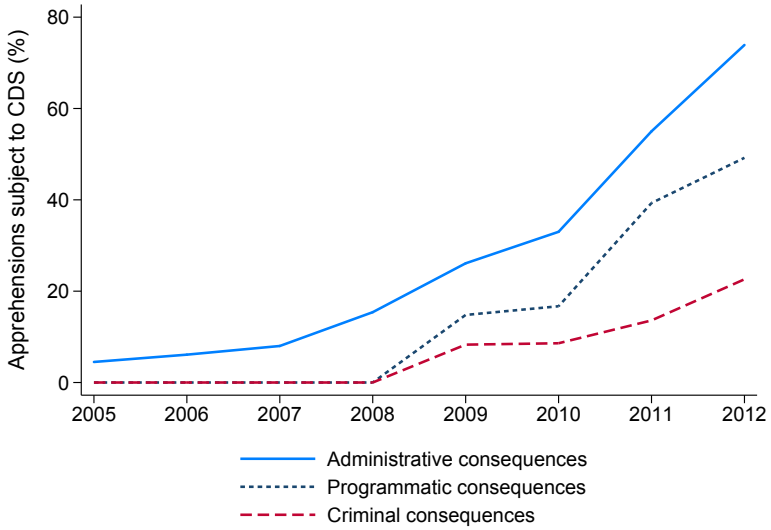
2.2 The Consequence Delivery System

A foreign national who enters the U.S. without authorization is in violation of U.S. law. Although the Border Patrol may refer any apprehended migrant for criminal prosecution, for decades standard practice was to offer apprehended Mexican nationals 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

[12]The population of migrants in EMIF-Norte surveys is reweighted to match the age, gender, and birth region of migrants apprehended by the Border Patrol. See Roberts (2017) for further discussion. By design, EMIF-Norte surveys do not track individual migrants over time, which complicate their use in the analysis of recidivism.

1999 to 2007 (DHS, 2008), in part justified voluntary return. Another 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; Roberts et al., 2013).[13] Under voluntary return, migrants may have reasonably interpreted border enforcement as serving to modulate the flow of labor across the border, rather than to prevent it altogether.

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.

Today, nearly all apprehended migrants face sanctions under the CDS.[14] Administrative consequences take the form of an order of expedited removal, for those who have never been formally removed from the U.S., or an order of reinstatement of removal, for those under an existing removal order (Rosenblum, 2013).[15] Migrants subject to removal are not detained. Instead, they are processed by the Border Patrol and transferred to the border for release into Mexico. Processing a

[13] Hanson and Spilimbergo (2001) find that over the period 1970 to 1997 border enforcement weakened in the months following positive labor demand shocks to U.S. sectors that employ undocumented labor intensively, suggesting that the Border Patrol may have adjusted enforcement intensity to accommodate changes in U.S. labor market conditions.

[14] The CDS, as part of the Border Patrol's strategy of at-the-border deterrence, was intended to reduce recidivism in apprehensions. Customs and Border Protection saw the decline in recidivism following the CDS rollout as a sign of its success (Roberts, 2017; DHS, 2017). Around the same time, U.S. Immigration and Customs Enforcement (ICE) expanded its operations to locate and deport undocumented immigrants in the interior U.S. Whereas the Border Patrol apprehends those in the act of entering the U.S. unlawfully, ICE, by virtue of its purview over interior enforcement, concentrates on the established undocumented population. (For research on interior enforcement, see Orrenius and Zavodny 2009; Bohn et al. 2014; Orrenius and Zavodny 2015; Hoekstra and Orozco-Aleman 2017.)

[15] Expedited removal and reinstatement of removal were introduced in the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 (https://www.congress.gov/104/crpt/hrpt828/CRPT-104hrpt828.pdf).

removal order requires 90 minutes of officer time, compared to 15 minutes for a voluntary return (Capps et al., 2017). A removal order, which is tied to an individual's fingerprints, precludes the migrant from applying for a legal entry visa for five years, or longer, and may count as a prior infraction in any future dealings with U.S. law enforcement. Because many undocumented immigrants from Mexico have applied for a U.S. green card and are awaiting adjudication of their request (which can take several years), 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 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—which ranges from 40% to 60% during our sample period—the large majority of migrants hire a smuggler.[16] The smuggling fee may cover the cost of multiple attempts to cross the border, in the event an initial try ends in apprehension (Chávez, 2011). For a given series of attempts to cross the border, migrants are thus tied to a particular smuggler. 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 at a location far from his entry point, which complicates reconnecting with his smuggler. Angelucci (2015) finds that among households in Mexico the decision to send a migrant to the U.S. responds strongly to income shocks, consistent with financial constraints limiting the ability of households to afford smuggling fees. Being subject to ATEP thus may impede some households from being able 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).

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

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

the migrant has many prior apprehensions or is suspected of other 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.[17] 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.[18]

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.[19] Because Border Patrol 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

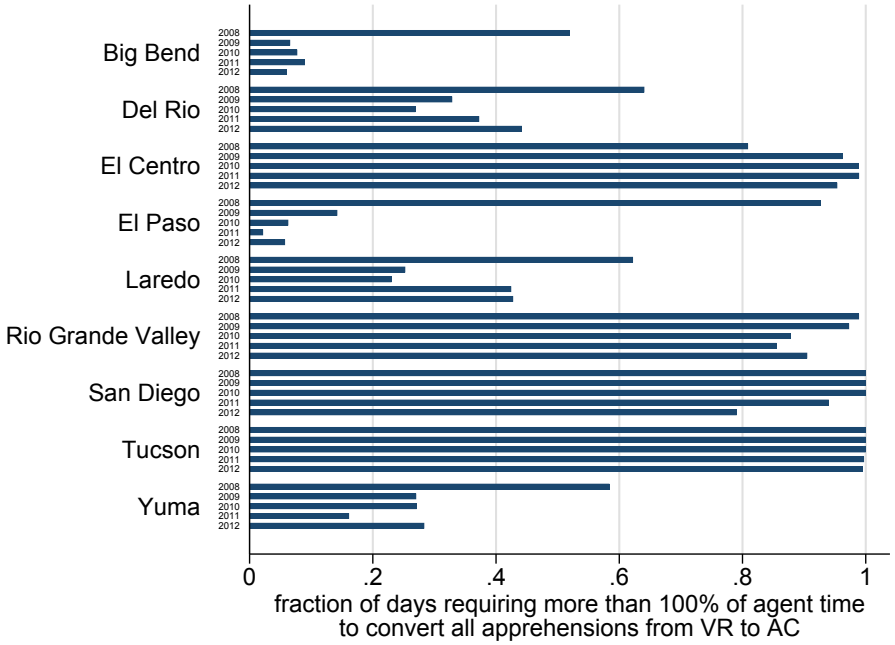
[17] In the early 2010s, the average sentence under Standard Prosecution was 18 months (USSC, 2015). Long sentences may reduce recidivism mechanically by incapacitating the migrant. However, the application of Standard Prosecution remains uncommon for border apprehensions, accounting for just 1.5% of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

[18] Administrative and criminal consequences may also impose additional psychic costs on migrants. Sanctions may change migrant perceptions of procedural justice surrounding border enforcement (e.g., Sunshine and Tyler, 2003; Tyler, 2004). Whereas voluntary return did not emphasize the criminality of illegal border crossing, the CDS does so explicitly. Emphasizing the criminality of the act may have raised the disutility associated with being apprehended.

[19] Over 2008-2012, Streamline prosecutions were disallowed by the Southern District of California GAO (2017a), which spans the El Centro and San Diego Border Patrol stations; they were also eschewed in the Big Bend sector.

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.

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.

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 or 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 discretion in assigning penalties.[20]

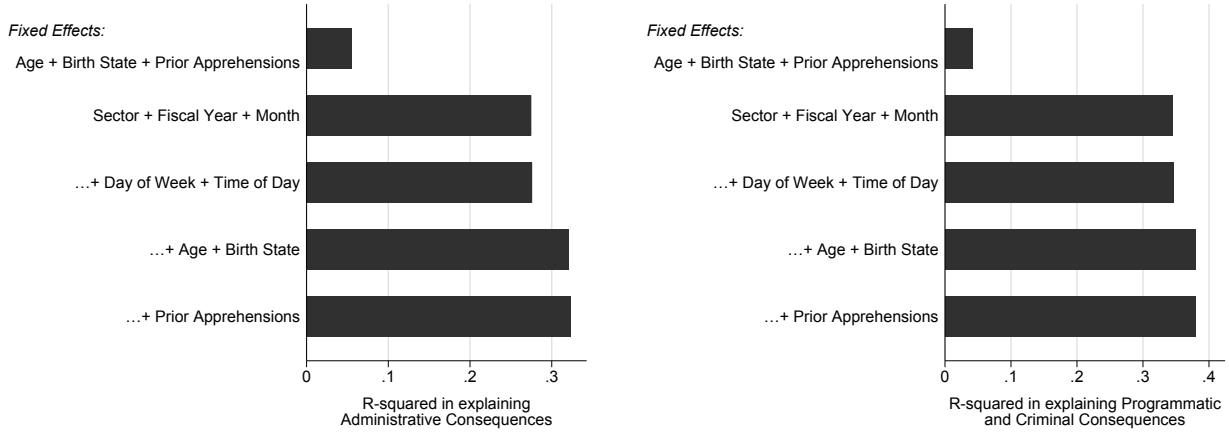
Capacity constraints at the level of Border Patrol sectors. Costs in imposing sanctions meant that on high-traffic days, agents may have been unable to impose sanctions on all apprehendees. Consider officer time that would have been required to transition fully from a system of voluntary removal to one 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 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).

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

[20]Border Patrol officers have clear guidelines for how to process apprehended migrants, where specific details are set at the sector level (and therefore may vary across sectors). Migrants from countries other than Mexico (or Canada) are ineligible for voluntary return and, because they are subject to deportation to their home countries, require greater time in processing. By the end of the CDS rollout in 2012, non-Mexican nationals accounted for 26.5% of those apprehended at the U.S.-Mexico border. Though relatively few in number, Mexican nationals under suspicion of smuggling or with prior criminal activity or extensive apprehension histories are in most cases subject to prosecution and by necessity require greater officer attention. If those awaiting processing include minors, officers must determine whether the minors have family members among those recently apprehended, such that they can be removed with their relatives. (Although migrants frequently travel in groups and are commonly apprehended in groups, not all those apprehended in a given operation are necessarily traveling together. Sanctions apply to the individual and not to the group.) It is only for adult male Mexican nationals without a record of prior criminal activity or extensive previous apprehensions—which constitute our sample—that Border Patrol officers have discretion over the application of the CDS. The factors that determine whether such migrants receive sanctions include (1) whether the obligatory processing of other apprehended migrants exceeds capacity for assigning discretionary sanctions, (2) whether the sector has reached the daily limit for prosecutions, (3) whether bed space is available in detention facilities, and (4) whether transport vehicles are available for application of ATEP or MIRP. See GAO (2017a) for further discussion.

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 Operation 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. Standard errors are clustered by sector-fiscal year-month (270 clusters). See Table A1 for categories used to define dummy variables.

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.[21] When

[21] Gronau (1998) shows that the R^2 in an LPM equals the difference between the average predicted probability

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 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 whether he was subject to sanctions.

To evaluate the relationship between capacity constraints and the probability of receiving sanctions more directly, we expand the regression analysis in Figure 4 by adding measures of the number of total apprehensions that occurred in the same Border Patrol sector in which a migrant was caught, either during the same six-hour period of his capture or the preceding six-hour period. Our maintained hypothesis is that the higher the volume of apprehensions, the less likely it will be that the Border Patrol has spare officer time to process additional removal orders, as needed to deliver administrative consequences, or that cooperating government agencies have the capacity to accommodate additional detentions, transport of migrants, or court hearings, as needed to deliver programmatic or criminal consequences. All specifications include controls for migrant age, birth state, number of previous apprehensions, and the Border Patrol sector, year, month, day of week, and time of day of the apprehension, as in the most exhaustive specifications in Figure 4.

Table 1 reports the results. The probability of receiving administrative sanctions, in column (1), is strongly negatively correlated with the current volume of apprehensions in a given sector, with this relationship being statistically significant at the one-percent level. The magnitude of the coefficient estimate implies that increasing the number of apprehensions in the current six-hour period by one standard deviation, or 41.9 total captures, sizably reduces the probability that a migrant apprehended in that sector will receive administrative consequences by 5.6 *p.p.* In column (2), we add the number of apprehensions during the preceding six-hour time block. Here again, there is a strong negative relationship between the volume of apprehensions at the sector level and the probability that a migrant receives sanctions. We obtain similar results when we examine the probability of receiving programmatic or criminal consequences, as seen in columns (3) and (4). These results suggest that migrants apprehended during or after a spike in captures are less likely

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.

to face sanctions, as consistent with local-level capacity constraints in delivering the CDS.

Table 1: Impact of Apprehensions Volume on Probability of Receiving CDS Sanctions

	(1)	(2)	(3)	(4)
	Dependent Variable = 1 if migrant subject to CDS			
	AC	AC	PC/CC	PC/CC
Border Sector Apprehensions:	-0.133	-0.089	-0.095	-0.062
Current Time Block / 100	(0.023)	(0.015)	(0.027)	(0.019)
Border Sector Apprehensions:		-0.096		-0.075
Previous Time Block / 100		(0.016)		(0.020)
Number of Individuals	925,380	889,800	925,380	889,800
Dep. Var. Mean	0.345	0.339	0.270	0.268
Adjusted R-squared	0.324	0.324	0.410	0.412

Notes: This table reports OLS regressions of a binary indicator for a given CDS sanction (administrative consequences (AC) in columns 1 and 2; programmatic or criminal consequences (PC/CC) in columns 3 and 4) on covariates that include total apprehensions in the sector in which a migrant was apprehended during the same six-hour period as his capture (all columns), total apprehensions in the sector during the previous six-hour period (columns 2 and 4), and controls for migrant age, birth state, number of prior apprehensions, and Border Patrol sector, fiscal year, month, day of week, and time of day of apprehension (all columns). Standard errors are clustered by sector-fiscal year-month (270 clusters). See Table A1 for details on the categories used to define the dummy variables used in the analysis.

3 Empirical Specification

In our main analysis, 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 factors that may relate both to whether the Border Patrol decides to sanction a migrant and to whether he elects to re-attempt 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 interactions with migrant characteristics. The first specification, with sector×year×month interactions, controls for time and location-specific labor-market shocks that may have affected a migrant's original decision to attempt illegal entry. For instance, if particularly fair weather affects the demand for construction or farm labor, such events may provoke an increase in the flow of itinerant workers to the border, who may be less determined to cross the border than individuals who have decided to move to the U.S. permanently. In neutralizing such variation, we seek to compare migrants who chose to cross the border under similar expectations about labor-market conditions.

The second specification, which additionally controls for day of week×time of day interactions, absorbs any non-randomness in the timing of the initial apprehension, which may reflect differential knowledge across migrants about the intensity of border enforcement. The third and most complete set of interactions allows sanctions to be correlated with shocks that affect the migrant's reentry decision, as long as these can be modeled by sector×calendar-date×age×birth-state×previous-apprehension interactions, which allows perceptions of labor-market shocks and border enforcement to be specific to migrant demographic characteristics and apprehensions history. Our approach to identification would be invalid if there are additional characteristics of a migrant that affect both 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—and under the constraint that they have spare sanctions capacity after prioritizing more pressing apprehension cases—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 and his apprehension. We formally evaluate possible bias due to selection on unobservables using the approach proposed by

Altonji et al. (2005) and generalized by Oster (2017).

4 Empirical Results

In presenting our results on how the CDS affects recidivism in apprehensions, we first consider the impact of administrative consequences, then examine other consequences, and finally examine how our findings are affected by imposing sample restrictions that either limit observations to those in time periods for which capacity constraints on delivering sanctions appear to bind or segment observations according to a migrant's prior number of apprehensions (to assess how impacts on recidivism in apprehensions may compare to those on recidivism in attempts at illegal entry).

4.1 Administrative Consequences

Figure 5 reports estimation results for (1) using our sample of adult male Mexican nationals with six or fewer prior apprehensions who were apprehended between 2008 and 2012. The outcome variables, organized in panels A to D, are indicators for whether the migrant was re-apprehended in the following 3 or 18 months. These results, along with those at 6 and 12 month horizons, appear in Table 2 (for administrative consequences) and Appendix Table A3 (for any consequences).

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 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 prior 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 prior apprehensions. The plots in Figure 5 include point estimates and 95% confidence intervals. Tables 2 and A3 present the full regression output including sample size, which varies modestly across specifications owing to the fact that 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.[22]

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 statistically significant at the one-percent level, 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). This specification controls for Border Patrol sector×fiscal year×month interactions, which one may think of as absorbing location and time-specific expectations about labor-market conditions and enforcement patterns among migrants prior to their initial attempt. As we allow for more interactions between time and location of apprehension and migrant characteristics—adding day of week×time of day interactions in row 2, age category×birth state interactions in row 3, and prior apprehension interactions in row 4—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 row 5—which allows for Border Patrol sector×calendar date interactions—and row 6—which allows for the full set of interactions between migrant characteristics, Border Patrol sector, and calendar date—do not materially change this point estimate, which is −0.066 in row 6.

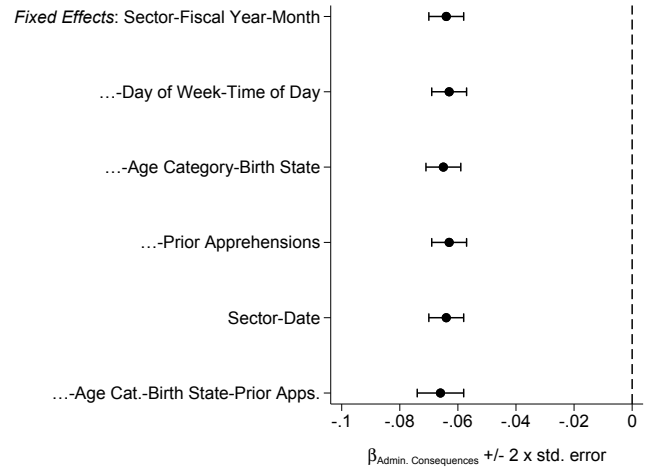
Table 2 explores CDS impacts on the likelihood of re-apprehension at the intermediate time horizons of 6 and 12 months. Again, the estimated treatment effects are insensitive to expanding the set of controls. The estimated impacts vary across specifications at the six-month horizon from −0.054 to −0.058 and at the 12-month horizon from −0.046 to −0.050, with all coefficients very precisely estimated. We do see that the treatment effect diminishes modestly as we expand the re-apprehension time horizon. Returning to Figure 5, if we compare the row 6 specifications with the most exhaustive fixed effects in panels A and B, the AC treatment effect falls from −0.066 at the 3-month horizon to −0.046 at the 18-month horizon (2008 re-apprehension probability of 0.269). This attenuation could indicate that some of the impact of sanctions is psychological—due, e.g., to the emotional shock of receiving a removal order or appearing before an immigration judge for individuals from poor communities with limited prior interaction with formal justice systems—where the trauma of the experience diminishes with time. Alternatively, it may take time for a migrant or his family to build up the financial resources needed to undertake a second border-crossing attempt,

[22]Table A7 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.

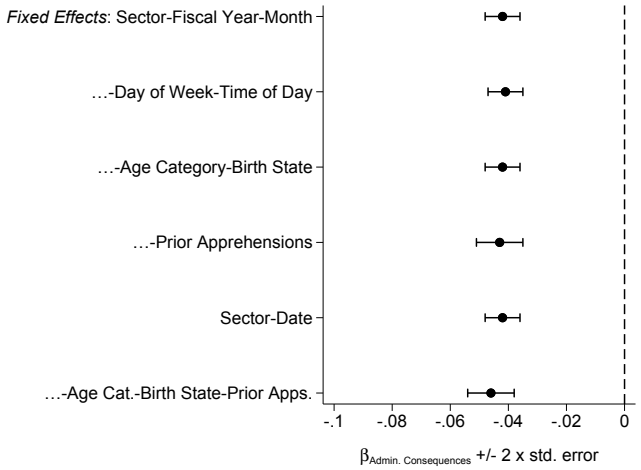
Figure 5: Impact of Exposure to CDS Sanctions on Probability of Re-apprehension

Administrative Consequences

(A) Re-apprehension within 3 months

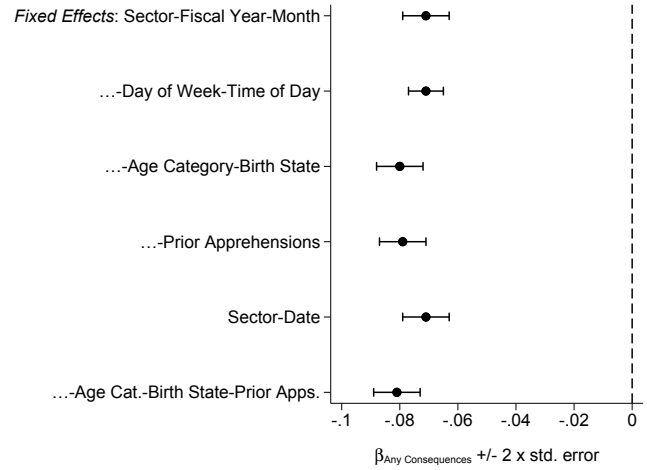


(B) Re-apprehension within 18 months

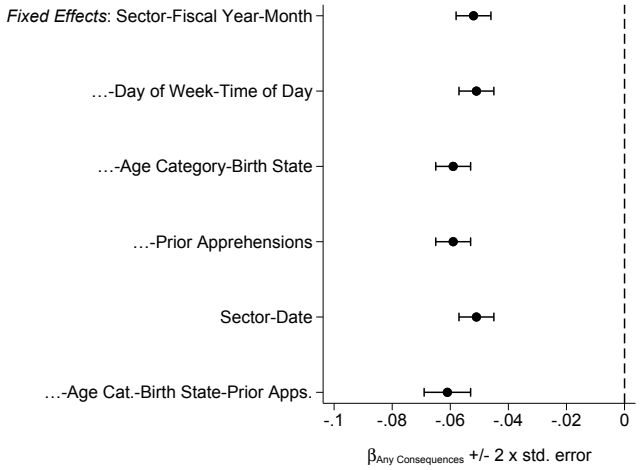


Any Consequences

(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). Table 2 and Appendix Table A3 present the full regression output (for panels A and B and panels C and D, respectively).

such that CDS impacts are lower at longer time horizons.

Table 2: 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)
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
Dep. Var. Mean	0.206	0.206	0.217	0.214	0.206	0.214
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)
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
Dep. Var. Mean	0.226	0.226	0.237	0.232	0.226	0.232
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)
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
Dep. Var. Mean	0.250	0.250	0.261	0.256	0.250	0.255
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)
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
Dep. Var. Mean	0.264	0.264	0.276	0.269	0.264	0.269
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. 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. (See Appendix Table A7 for p-values based on wild bootstrap standard errors.)

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 in Table 2, where the R^2 increases from 0.076 in column 5, with sector-date fixed effects, to 0.409 in column 6, when adding interactions with age, birth place, and number of previous apprehensions.[23] 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 the probability of re-apprehension does not change the observed impact of administrative consequences. This pattern holds across our findings by time horizon in Table 2—as seen when comparing columns 5 and 6 in panels B, C, or D—and points to very limited potential for selection on unobservables based on the Oster (2017) test.[24] The calculation here, comparing columns 5 and 6 of panel A, suggests that to explain our results 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 208.4 arises when comparing the specifications in columns 5 and 6 in panel D at the 18-month horizon. In short, selection on unobservables would have to be implausibly large to explain the CDS impact on recidivism in apprehensions 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

[23] 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 2, and thus do not add to the R^2.

[24] 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 $R_c^2 > R_u^2$.

difference in the effects of AC versus PC/CC. This can be seen in panel A of Table A4, 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 A5).

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, which prevents comparing across AC and PC/CC *within-person*. Different sources of identifying variation may explain why the combined effect of AC and PC/CC is less than two times the AC or the PC/CC treatment alone (panel B of Tables A4 and A5). Alternatively, the positive interaction between the AC and PC/CC treatments may indicate diminishing returns to sanctions, making the combined effects of the sanctions 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.

In performing this calculation, we hold constant any impact of the CDS on the number of migrants who make a first attempt at illegal entry—i.e., the CDS impact on behind-the-border deterrence—which is absorbed by our Border Patrol sector-by-time fixed effects. That is, any impact of sanctions on behind-the-border deterrence would affect the number of migrants potentially exposed to the CDS treatment. If sanctions reduce the number of migrants who choose to make a first crossing attempt, there may be less pressure on border enforcement resources and the Border Patrol may find it possible to sanction a larger fraction of the now smaller number of migrants that it is likely

to apprehend, thereby expanding its capacity to deter post-apprehension illegal entry. Such an outcome would indicate that the total impact of the CDS on the number of individuals who are deterred from attempting unauthorized entry is larger than that implied by our estimates of the marginal treatment effect of the CDS on at-the-border deterrence.

4.3 Restricting the Sample to Time Periods with Low Sanctions Capacity

Our identification strategy is based on the idea that during the CDS rollout the Border Patrol agents who decided whether to impose sanctions on apprehended migrants did so in an environment in which factors external to the migrant largely dictated the sanction decision, at least for the migrants in our sample. As adult male Mexican nationals with relatively few prior apprehensions, these migrants both account for the large majority of apprehensions during our sample period and represent cases in which agents had maximal discretion over the disposition of migrants. Still, because we do not have an instrumental variable for the sanctions decision, we are left to present evidence that substantiates our claim that the Border Patrol was frequently subject to capacity constraints in delivering sanctions, which on given days would have prevented them from applying the CDS to all of those captured and forced agents to effectively randomize over who was subject to the CDS (e.g., as by assigning sanctions on a first-come, first-served basis).

As a check on the robustness of our results, we narrow the sample to days in which the data suggest that constraints on sanctions capacity would likely have been binding. These days are those outlined in Section 2.3, for which according to our calculations delivering administrative sanctions to all those apprehended would have more than absorbed the available time of Border Patrol agents on duty (even without considering the resource requirements of additionally imposing programmatic or criminal sanctions). In Table 3, we re-estimate the specifications in panels A and D of Table 2, for the impact of administrative consequences on recidivism in apprehensions at the 3 and 18-month horizons, and in panels A and D of Table A3, regarding the impact of any consequences on the re-apprehension probability, also at 3 and 18-month horizons. Because imputed capacity constraints register as binding on the majority of days, these sample restrictions reduce the sample size only modestly, by 5.9*p.p.* (916,032 versus 973,171 observations) in the column 1 regressions with sector×year×month interactions and by 2.1*p.p.* (485,052 versus 495,668 observations) in the column 6 regressions with the most exhaustive set of interactions.

Table 3: Restricting Sample to Days when Capacity Constraints Bind

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Pr(Re-Apprehension within 3 months)					
Administrative Consequences	-0.065	-0.064	-0.065	-0.064	-0.065	-0.067
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Number of Observations	916,032	915,791	690,393	498,048	916,032	485,052
Dep. Var. Mean	0.215	0.215	0.222	0.219	0.215	0.217
R-squared	0.053	0.067	0.321	0.397	0.067	0.407
Adjusted R-squared	0.052	0.055	0.078	0.098	0.057	0.102
	Panel B: Pr(Re-Apprehension within 3 months)					
Any Consequences	-0.073	-0.073	-0.081	-0.080	-0.073	-0.082
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Number of Observations	916,032	915,791	690,393	498,048	916,032	485,052
Dep. Var. Mean	0.215	0.215	0.222	0.219	0.215	0.217
R-squared	0.054	0.068	0.322	0.399	0.068	0.408
Adjusted R-squared	0.053	0.057	0.080	0.099	0.058	0.104
	Panel C: Pr(Re-Apprehension within 18 months)					
Administrative Consequences	-0.043	-0.042	-0.042	-0.043	-0.042	-0.046
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Number of Observations	916,032	915,791	690,393	498,048	916,032	485,052
Dep. Var. Mean	0.272	0.272	0.281	0.274	0.272	0.272
R-squared	0.040	0.053	0.308	0.387	0.054	0.396
Adjusted R-squared	0.040	0.042	0.061	0.083	0.043	0.085
	Panel D: Pr(Re-Apprehension within 18 months)					
Any Consequences	-0.052	-0.052	-0.059	-0.059	-0.051	-0.062
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Number of Observations	916,032	915,791	690,393	498,048	916,032	485,052
Dep. Var. Mean	0.272	0.272	0.281	0.274	0.272	0.272
R-squared	0.041	0.054	0.309	0.388	0.054	0.396
Adjusted R-squared	0.040	0.043	0.062	0.084	0.044	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 or any consequences on the probability of re-apprehension within 3 and 18 months after the initial apprehension. 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.

The estimated treatment effects are insensitive to imposing the sample restrictions. Compare panel A in Tables 2 and 3. For the column 6 specification, which controls for the full set of interactions between migrant characteristics, Border Patrol sector, and calendar date, the AC treatment effect is -0.067 at the 3-month horizon with sample restrictions (Table 3), and -0.066 at the 3-month horizon without sample restrictions (Table 2), where both coefficients are statistically significant at the one-percent level. We also find similarly estimated treatment effects without and without sample restrictions for administrative consequences at the 18-month horizon (panel C in Table 3 versus panel D in Table 2), and for any consequences at either the 3-month (panel B in Table 3 versus panel A in Table A3) or 18-month (panel D in Table 3 versus panel D in Table A3) horizons. We interpret the insensitivity of the results to restricting the sample to days on which capacity constraints appeared to bind as support for our identification strategy.

4.4 Recidivism in Apprehensions versus Recidivism in Attempted Illegal Entry

As a final exercise, we consider how changes in recidivism in apprehensions—which we observe—relate to changes in recidivism in attempted illegal immigration—which we do not observe. First, we discuss analytically how the CDS treatment effect on recidivism in apprehensions relates to the CDS treatment effect on recidivism in illegal entry; second, we explore empirically whether our estimates of the first treatment effect under or overstate the second treatment effect.

4.4.1 Interpreting the CDS treatment effect

In equation (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

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

In striving to enhance at-the-border deterrence, the objective of the Border Patrol in delivering sanctions through the CDS would be to reduce the probability that an apprehended migrant seeks to reattempt to enter the U.S. illegally, or to engender the outcome that $P(E_1|1) - P(E_1|0) < 0$. Equation (3) makes clear that our ability to evaluate this impact is obfuscated by the re-apprehension probability and its possible change in response to sanctions.

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 continue to pursue a visa application currently under consideration). After a single apprehension, the sanctioned migrant thus appears to 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.[25]

[25] 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

4.4.2 Allowing the CDS Treatment Effect to Vary with Previous Apprehensions

We now allow the CDS treatment effect to vary with the number of previous apprehensions. A sanctioned migrant with a single apprehension (who has lost the right to seek a legal immigration visa) may have a weaker incentive to avoid apprehension on a subsequent crossing than a non-sanctioned migrant with a single apprehension (who has not yet lost this right but may if apprehended again), in which case by equation (3) our estimated impact of sanctions on recidivism in apprehensions may weakly understate the impact of sanctions on recidivism in attempted illegal entry.

Table A6 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 programmatic/criminal consequences, and results are similarly 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. The CDS treatment effect on recidivism in apprehensions thus appears to be informative about the more-policy-relevant CDS treatment effect on recidivism in attempted illegal entry, whose reduction is a prime objective of the Border Patrol's at-the-border deterrence strategy.

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 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).

5 Discussion

The job of the U.S. Border Patrol includes interdicting migrants who attempt to enter the country without authorization and deterring either first-time or repeat border crossers from attempting entry. Undocumented immigration is a highly contentious issue in the U.S., which makes understanding the effectiveness of Border Patrol enforcement strategies important both for helping inject dispassion into the policy debate and for aiding the government in navigating a landscape in which it must secure the nation's borders while respecting public sensitivities.

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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Online Appendix:

Deterring Illegal Entry:

Migrant Sanctions and Recidivism in Border Apprehensions

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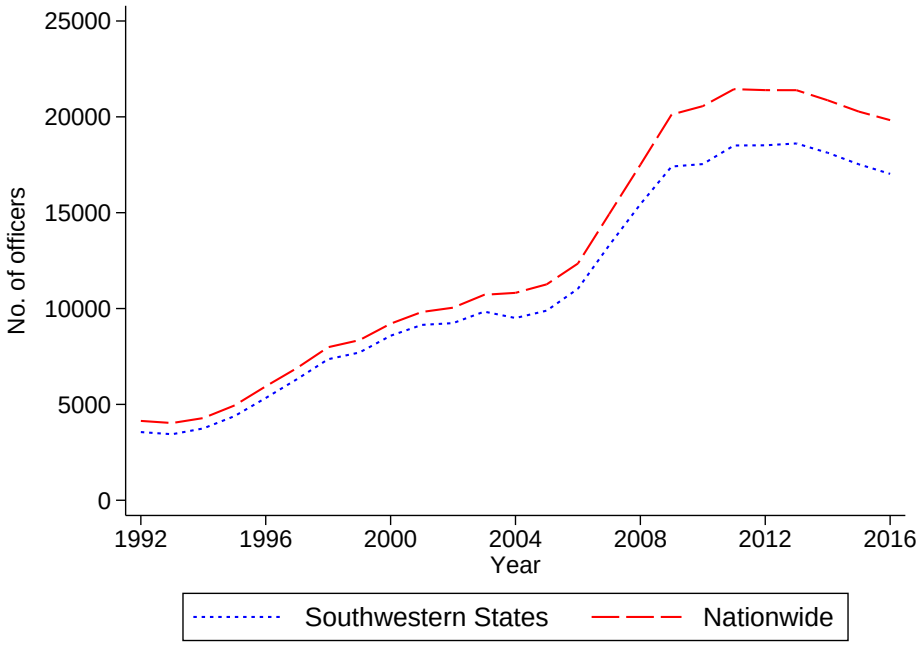
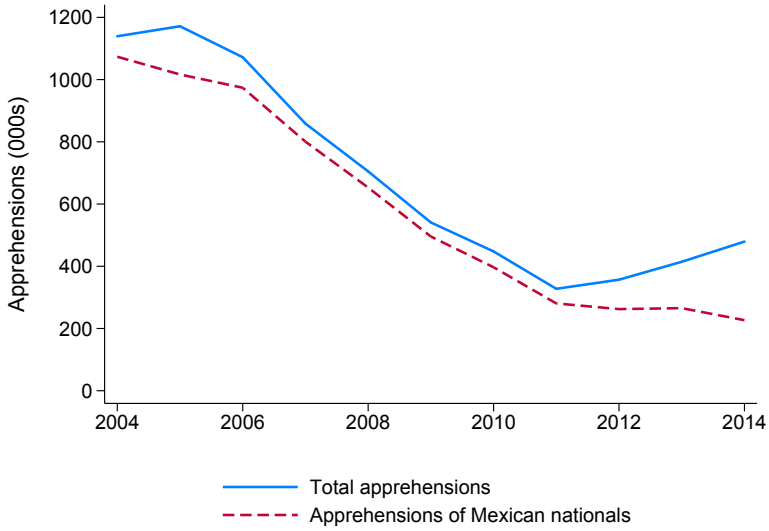
Appendix A: Figures and Tables

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



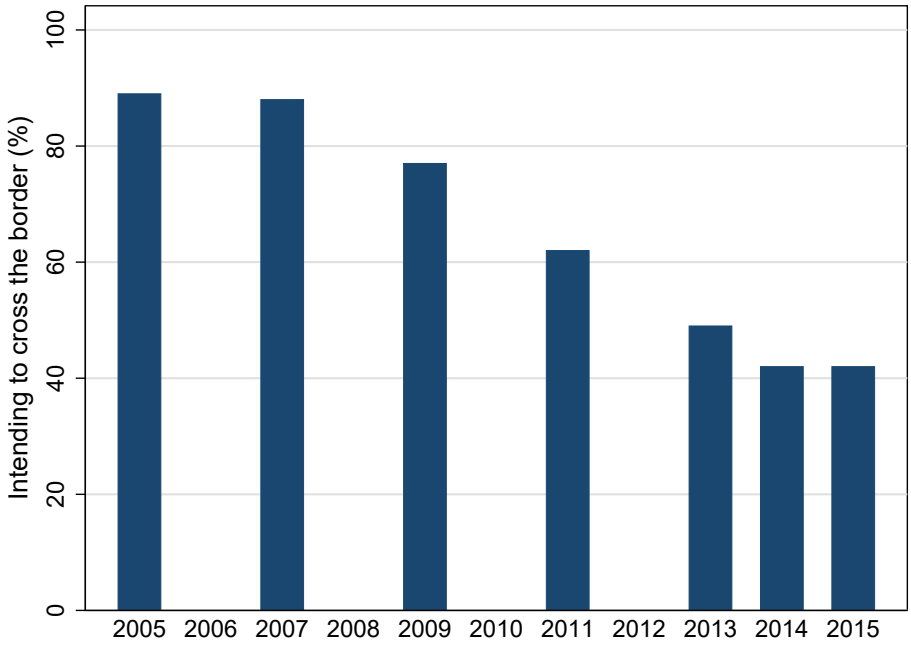
Note: Data are from the Customs and Border Protection (2017).

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



Note: Data are from the DHS (2008, 2009, 2010, 2011, 2012, 2013, 2014, 2015)

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



Note: Data are from El Colegio de la Frontera Norte (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

Note: 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. For programmatic consequences, the Border Patrol briefly supplemented ATEP with a second consequence, the Mexican Interior Relocation Program (MIRP), under which apprehended Mexican nationals were flown to Guadalajara or Mexico City before their release. MIRP was rolled out in 2009 and discontinued in 2011 due to its high cost. We include MIRP under programmatic consequences. In our sample, 163,440 migrants were subject to ATEP, whereas 26,092 were subject to MIRP.

Table A3: 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)
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
Dep. Var. Mean	0.206	0.206	0.217	0.214	0.206	0.214
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)
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
Dep. Var. Mean	0.226	0.226	0.237	0.232	0.226	0.232
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)
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
Dep. Var. Mean	0.250	0.250	0.261	0.256	0.250	0.255
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)
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
Dep. Var. Mean	0.264	0.264	0.276	0.269	0.264	0.269
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 2. Coefficients and standard errors are those shown in Figure 5. Standard errors (clustered by sector-year-month) are in parentheses. (See Appendix Table A8 for p-values based on wild bootstrap standard errors.)

Table A4: 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
Dep. Var. Mean	0.206	0.206	0.217	0.214	0.206	0.214
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
Dep. Var. Mean	0.206	0.206	0.217	0.214	0.206	0.214
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.

Table A5: 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
Dep. Var. Mean	0.264	0.264	0.276	0.269	0.264	0.269
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
Dep. Var. Mean	0.264	0.264	0.276	0.269	0.264	0.269
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.

Table A6: Heterogeneous Impacts of Consequence Programs by No. of Previous Apprehensions

(A) Administrative Consequences

	(1)	(2)	(3)	(4)
	Panel (A)			
	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
Dep. Var. Mean	0.214	0.214	0.214	0.214
R-squared	0.401	0.396	0.392	0.391
Adjusted R-squared	0.099	0.092	0.086	0.084

(B) Any Consequences

	Panel (B)			
	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
Dep. Var. Mean	0.214	0.214	0.214	0.214
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 2, shown in panel A, and in column 4 of Table A3, 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.

Table A7: Impact of Administrative Consequences on Probability of Re-Apprehension (p-values based on wild bootstrap)

	(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
	[0.010]	[0.014]	[0.022]	[0.015]	[0.011]	[0.005]
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
	Panel B: Pr(Re-Apprehension within 6 months)					
Administrative Consequences	-0.055	-0.054	-0.055	-0.054	-0.055	-0.058
	[0.019]	[0.020]	[0.048]	[0.036]	[0.020]	[0.013]
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
	Panel C: Pr(Re-Apprehension within 12 months)					
Administrative Consequences	-0.047	-0.046	-0.047	-0.047	-0.047	-0.050
	[0.025]	[0.033]	[0.083]	[0.063]	[0.032]	[0.046]
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
	Panel D: Pr(Re-Apprehension within 18 months)					
Administrative Consequences	-0.042	-0.041	-0.042	-0.043	-0.042	-0.046
	[0.035]	[0.042]	[0.098]	[0.068]	[0.039]	[0.054]
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
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 replicates Table 2, showing within brackets the p-values based on a wild bootstrap procedure clustering at the sector level (of which there are 9).

Table A8: Impact of Any Consequences on Probability of Re-Apprehension (p-values based on wild bootstrap)

	(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
	[0.009]	[0.009]	[0.028]	[0.028]	[0.009]	[0.034]
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
	Panel B: Pr(Re-Apprehension within 6 months)					
Any Consequences	-0.064	-0.064	-0.072	-0.071	-0.064	-0.074
	[0.008]	[0.006]	[0.023]	[0.024]	[0.007]	[0.034]
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
	Panel C: Pr(Re-Apprehension within 12 months)					
Any Consequences	-0.056	-0.056	-0.064	-0.064	-0.055	-0.066
	[0.005]	[0.005]	[0.014]	[0.014]	[0.004]	[0.032]
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
	Panel D: Pr(Re-Apprehension within 18 months)					
Any Consequences	-0.052	-0.051	-0.059	-0.059	-0.051	-0.061
	[0.005]	[0.004]	[0.012]	[0.019]	[0.004]	[0.028]
Number of Observations	973,171	972,754	713,528	512,727	972,721	495,668
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 replicates Appendix Table A3, showing within brackets the p-values based on a wild bootstrap procedure clustering at the sector level (of which there are 9).

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.