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CEPA Working Paper No. 18-18

Vanished Classmates: The Effects of Local Immigration Enforcement on School Enrollment

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ABSTRACT

Immigration and Customs Enforcement (ICE) is the federal law-enforcement agency with primary responsibility for enforcing immigration laws within the U.S. However, for over a decade, ICE has formed partnerships that also allow local police to enforce immigration law (i.e., identifying and arresting undocumented residents). Prior studies, using survey data with selfreported immigrant and citizenship status, provide mixed evidence on the demographic impact of these controversial partnerships. This study presents new evidence based on the public-school enrollment of Hispanic students. We find that local ICE partnerships reduce the number of Hispanic students by nearly 10 percent within 2 years. We estimate that the local ICE partnerships enacted before 2012 displaced over 300,000 Hispanic students. These effects appear to be concentrated among elementary-school students. We find no corresponding effects on the enrollment of non-Hispanic students. We also find no evidence that ICE partnerships reduced pupil-teacher ratios or the percent of students eligible for the National School Lunch Program (NSLP).

Acknowledgements: We would like to thank seminar participants at the Stanford Center for Education Policy Analysis (CEPA), the Stanford Immigration Policy Lab (IPL), and the Institute for Research in the Social Sciences (iRiSS). We also appreciate comments from participants at the research conference of the Association for Education Finance and Policy. We thank Jacob Rugh and Matthew Hall for sharing their data. We are also grateful for helpful comments from David Plank, Susanna Loeb, Caroline Hoxby, Jens Hainmueller, David Laitin, Duncan Lawrence, Jaime Arellano-Bover, Erika Byun, and Sade Bonilla. This research was supported by the Institute of Education Sciences, U.S. Department of Education, through Grant R305B140009 to the Board of Trustees of the Leland Stanford Junior University. The opinions expressed are those of the authors and do not represent views of the Institute or the U.S. Department of Education or the Board of Trustees of the Leland Stanford Junior University. This research was also supported by the Stanford Immigration Policy Lab.

VERSION

September 2018

Suggested citation: Dee, T.S., & Murphy, M. (2018). Vanished Classmates: The Effects of Local Immigration Enforcement on School Enrollment (CEPA Working Paper No.18-18). Retrieved from Stanford Center for Education Policy Analysis: http://cepa.stanford.edu/wp18-18

1. Introduction

Immigration, both authorized and unauthorized, ranks among the most politically contentious issues of our time. In the U.S., the controversy over immigration focuses not just on the character of border enforcement but also on the interior enforcement of immigration law (e.g., employment verification, worksite enforcement, identifying and arresting undocumented residents). The enforcement of U.S. immigration laws has historically been a federal responsibility. Since the creation of the Department of Homeland Security (DHS) in 2002, Immigration and Customs Enforcement (ICE) has served as the federal law-enforcement agency responsible for enforcing immigration law. In particular, ICE's Enforcement and Removal Operations (ERO) bureau seeks to identify, arrest, and remove undocumented residents in the U.S. However, for more than a decade, ICE has also pursued these objectives through structured partnerships with local law-enforcement agencies (i.e., so-called "287(g) agreements"). These ICE partnerships provide local law-enforcement agencies with the training and authority to enforce federal immigration laws under the supervision of ICE officers.

Advocates for these controversial partnerships have argued that they are an effective way to enforce immigration laws and to deter unauthorized residents, particularly those who have committed crimes. However, critics (e.g., Shahani & Greene, 2009) have questioned their efficacy and charged that the comingling of criminal and civil law enforcement encourages large-scale civil-rights violations and erodes the degree of trust and communication between the police and immigrant communities.[1] Several empirical studies (e.g., Kostandini, Mykerezi, & Escalante, 2014; O'Neil, 2013; Parrado, 2012; Watson, 2013) have examined the impact of these partnerships on the presence of undocumented residents, Hispanics and foreign-born individuals using data with self-reports of immigrant and citizenship status from the American Community Survey (ACS). However, the evidence from these studies is mixed.

This study presents new evidence on the impact of ICE partnerships by focusing on the measured enrollment of Hispanic students in U.S. public schools. We believe this research makes two key contributions. First, the data from universe surveys of school enrollment by Hispanic ethnicity may provide a more reliable indicator of the demographic impact of local immigration enforcement. In our study window, over 80 percent of unauthorized residents originated in

[1] In 2011, the DHS terminated a 287(g) agreement with Maricopa County, Arizona after an investigation found a "pattern or practice of wide-ranging discrimination against Latinos" under the leadership of Sheriff Joe Arpaio.

Mexico and other Latin American countries. Additionally, roughly half of undocumented adults lived with their own children, most of whom were themselves U.S. citizens. The enforcement-induced changes we observe in local Hispanic student enrollment would reflect the displacement of such children due to the outflow of threatened families as well as the inhibited inflow of potential new families.[2] Administrative data on Hispanic enrollment also have unique advantages relative to individual surveys in this context. School districts in the U.S. have strong financial incentives to report all their enrolled students. Furthermore, there is little reason for concern that these aggregate counts place their undocumented students (or those with undocumented parents) at risk. In contrast, there is evidence that the self-reports in individual-level Census surveys do not necessarily provide reliable measures of citizenship status (Passel & Clark, 1997; Van Hook & Bachmeier, 2013). The recent proposal to add a citizenship question to the 2020 Census has drawn new attention to these data-quality concerns.[3]

A second contribution of this study is to provide new evidence on how ICE partnerships may influence students and schools. In particular, the potential effects of ICE partnerships on student mobility are likely to have negative developmental consequences for the affected children (e.g., mental health, student achievement, increased dropout risk). Studies of student mobility suggest that causing "reactive" moves (i.e., those made in response to stressful, adverse events) or inhibiting "strategic" moves (e.g., purposeful moves made to improve a home, school or community situation) can be educationally harmful, particularly for Hispanic students and students who have moved before (Hanushek, Kain, & Rivkin, 2004; Welsh, 2017). Other educational effects of ICE partnerships may be situated in the communities where they are implemented. Specifically, to the extent that ICE partnerships have enrollment effects, they may reduce the diversity in a community's schools. However, these partnerships may also increase the available per-pupil resources for remaining students and influence the socioeconomic status of student peers.

[2] Furthermore, these effects may also exist for Hispanic citizens who dislike living in a community with enhanced enforcement. It is possible that enforcement-induced enrollment declines also reflect students who dropped out of school yet also remained in place (Amuedo-Dorantes & Lopez, 2015, 2017). However, our finding that enforcement effects are concentrated among elementary-school students is more consistent with effects on mobility than on dropout behavior.

[3] A recent study from the Census Bureau's Center for Economic Studies (Brown, Heggeness, Dorinski, Warren, & Yi, 2018) also compares survey-based and administrative data and finds evidence "consistent with noncitizen respondents misreporting their own citizenship status and failing to report that of other household members."

We examine these questions using county-year panel data from 2000 to 2011 when these partnerships first proliferated. Specifically, using data acquired from DHS through Freedom of Information Act (FOIA) requests (Rugh & Hall, 2016), we identify the counties in which a law-enforcement agency applied for an ICE partnership as well as those counties where applications were approved. We estimate the impact of ICE partnerships on Hispanic enrollment and other outcomes in "difference in differences" (DD) specifications. We examine the identifying assumptions of this DD approach through "event study" evidence. We also estimate the impact of ICE partnerships on *non-Hispanic* enrollments as a falsification exercise and we synthesize these results in "difference in difference in differences" (DDD) specifications. We find robust evidence that partnerships between ICE and local law-enforcement agencies led to substantial reductions in Hispanic student enrollment (i.e., a 7.3 percent reduction overall but one that grew to about 10 percent within two years). These reductions in Hispanic student enrollment appear to be concentrated among the youngest students. Based on this evidence, we estimate that, during our study window, ICE partnerships displaced more than 300,000 Hispanic students (i.e., by encouraging them to leave and discouraging them to arrive). In contrast, we find that ICE partnerships did not have statistically significant effects on non-Hispanic enrollments, pupil-teacher ratios, or the percent of remaining students whose household income makes them eligible for the federal National School Lunch Program (NSLP). We conclude by discussing the relevance of our evidence for the recent expansion of local ICE partnerships under the Trump Administration.[4]

2. Immigration and Customs Enforcement (ICE) Partnerships

Section 287(g) of the US Immigration and Nationality Act authorizes the federal government to delegate its authority for immigration enforcement to local law-enforcement entities. However, this statutory authority, which was introduced in 1996, was largely ignored until the terrorist attacks on September 11, 2001 and the subsequent creation of DHS and ICE generated a renewed focus on immigration policy. When communities adopt these ICE partnerships, a joint Memorandum of Agreement (MOA) codifies the delegation of federal

[4] A Presidential Executive Order on January 25, 2017 expressed that "it is the policy of the executive branch to empower State and local law enforcement agencies across the country to perform the functions of an immigration officer in the interior of the United States to the maximum extent permitted by law." (Trump, 2017). The order further cites 287(g) MOA as the method for establishing such partnerships.

immigration-enforcement authority and describes its implementation. In general, 287(g) partnerships allow local law enforcement officers to patrol for immigration-status violations in jailhouses (i.e., the jail enforcement model), during a variety of daily policing activities (i.e., the task force model), or in both capacities (Capps, Rosenblum, Rodriguez, & Chishti, 2011). These MOA also state that, when conducting immigration enforcement, local law-enforcement officers operate under the supervision of ICE officers. Additionally, these agreements require that such "cross designated" local police officers meet ICE's training requirements (i.e., 4 weeks of basic training at a federal facility as well as a one-week refresher program every two years). While DHS pays the ICE trainers, local law-enforcement agencies bear most of the direct costs of program training and operations (e.g., officer salaries and benefits).

The formation of these voluntary federal-local partnerships begins with an application by local law enforcement agencies to the DHS. The local motivations for submitting these partnership applications vary, but frequently relate to promoting safe communities for citizens and fighting crime (Nowrashteh, 2018). However, it also appears that restrictive immigration policies like these are more likely to be adopted by communities that have low numbers of immigrants but subsequently experience a sharp influx (Boushey & Luedtke, 2011; Shahani & Greene, 2009). To address the potential confounds created by this sort of policy endogeneity, our county-year panel data, which we describe in more detail below, only includes 168 counties in which a local law-enforcement agency submitted a 287(g) application. However, the exact criteria that DHS used for determining which of these applications to approve are not specified publicly. In our data, which were acquired through a Freedom of Information Act (FOIA) request (Rugh & Hall, 2016) and public data sources, roughly a third of counties from which an application originated actually entered a 287(g) MOA.[5] While the evaluative criteria for approving these partnerships are unclear, at least one publicly available rejection notice cited concerns about the fiscal capacity of the local applicant to support immigration enforcement activities. Other factors, such as the estimated number of undocumented individuals residing in the county, also seem likely to have played a role in the DHS's calculations (O'Neil, 2013). In

[5] Most of the remaining applications were denied by DHS. However, some were withdrawn by the applicant or had a pending decision at the end of our study window. One of the robustness checks we present is to limit our sample of non-adopting counties to only those whose applications were denied.

our methodology section below, we discuss strategies for credibly identifying the causal impact of these partnerships in the presence of this uncertain approval process.

During the study window for which FOIA data from DHS are available (i.e., 2000 to 2011), 55 counties included a local law-enforcement agency that introduced a new ICE partnership.[6] Between 2005 and 2006, only 5 counties introduced the first local ICE partnerships. However, the number of counties with new ICE partnerships grew rapidly between 2007 and 2009 (i.e., 49 counties with new agreements). Only 1 new county formed a partnership in 2010 and none were added in 2011. Of the counties with agreements, 30 can be classified as having "jail enforcement" partnerships, 9 as having "task force" partnerships, and an additional 16 having as "hybrid" partnerships that operate as both jail enforcement and task force models. In 2012, ICE discontinued 287(g) agreements under the task-force and hybrid models. However, during the Trump Administration, the number of 287(g) agreements has grown. ICE currently has active partnerships with 78 law enforcement agencies in 20 states, all under the jail-enforcement model. Under the Trump Administration, the DHS has also expressed interest in renewing the task-force model of ICE partnerships (Misra, 2017).

It should be noted that other prominent initiatives also influenced the interior enforcement of immigration policy during our study window (i.e., 2000 to 2011). For example, E-Verify is a DHS website that allows employers to determine an individual's employment eligibility. In 2007, the federal government required all its contractors and vendors to use E-Verify. Similarly, several states also began requiring the use of E-Verify (i.e., by state agencies and vendors but, in some cases, private employers as well). Another initiative introduced towards the end of this period, the "Secure Communities" program, involves sharing the biometric information of those booked into local jails with ICE. If this information matches the available data on non-citizens, ICE officials evaluate the case and may then issue a "detainer" to hold the individual until they can take custody and initiate deportation proceedings. A pilot of this program began in 2008 and expanded during the first Obama Administration, serving as a motivation for discontinuing task-force and hybrid models of 287(g) agreements. DHS

[6] There were also several 287(g) agreements at the state level. However, many of these agreements were not particularly active (i.e., negligibly few identifications) and were often situated in states without the local agreements that are the focus of our study. As a robustness check, we examine our results conditional on a control for state-level 287(g) agreements as well as without county data from the states that also had active state-level 287(g) agreements (i.e., Arizona, Colorado, and Massachusetts).

discontinued the "Secure Communities" program in 2014, but it was restarted in 2017 under an executive order by the Trump Administration. In examining the impact of 287(g) agreements, we also examine data on the expansion of these programs as potential confounds.

3. Undocumented Residents in the U.S.

Because ICE partnerships aimed to strengthen interior enforcement of immigration law, the demographic impact of these partnerships should be understood in the context of what is known about undocumented residents in the U.S. However, directly identifying the size and the characteristics of undocumented U.S. residents is understandably challenging. The Census Bureau does not explicitly collect details about this population (Blau & Mackie, 2016). However, other studies (e.g., Passel, Van Hook, & Bean, 2004) estimate the overall number of undocumented residents by identifying the difference between estimates of the total foreign-born population in the U.S. and the number of immigrants residing in the U.S. with legal visas. These data indicate that, between 2000 and 2007, the number of undocumented residents rose steadily from around 8.4 million to approximately 12.0 million (Passel & Cohn, 2011), roughly 4 percent of the total population in the U.S. Between 2008 and 2011, this population estimate declined to just more than 11 million (Passel, Cohn, & Gonzalez-Barrera, 2012). This number has remained roughly constant since then with an estimated 300,000 to 400,000 undocumented immigrants entering and exiting the country each year (Blau & Mackie, 2016).

Another stylized fact that is critically important to this study concerns the countries of origin for these undocumented residents. Prior to the expansion of ICE partnerships, approximately 81 percent of all undocumented immigrants were from Mexico or other Latin American countries (Passel, 2005). Among the remaining undocumented residents, 9 percent were from Asian countries and 10 percent were from other countries throughout the rest of the world. The overwhelming share of undocumented residents who are likely to report Hispanic ethnicity motivates this study's focus on changes in Hispanic enrollment in K-12 schools.[7]

However, what is known about the family composition of undocumented residents in the U.S. also motivates our focus on Hispanic student enrollment. In 2010, approximately 45 percent

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[7] Given the preponderance of Asian immigrants among the residual population of undocumented residents, we exclude students identified as Asian from our measure of *non-Hispanic* student enrollment. As an aside, we also examined the impact of ICE partnerships on Asian enrollment and did not find consistent evidence of an impact.

of undocumented immigrants lived in households with a spouse and one or more children. Additionally, the vast majority of children whose parents were undocumented - 79 percent - were themselves US citizens, having been born within the U.S. (Passel & Taylor, 2010). The extant literature describes families where both undocumented immigrants and U.S. citizens cohabitate as “mixed-status families” (Passel & Cohn, 2009). In 2008, approximately 4 million children resided in mixed-status families in the U.S., which marked a substantial increase from the 2.7 million children residing in such homes in 2004. Nationally, roughly 7 percent of K-12 students in 2008 had at least one parent who was an undocumented immigrant (Passel & Cohn, 2009), a rate that has remained fairly stable.

Changes in settlement patterns for the undocumented population during the 2000s are also important to note. During this period, undocumented immigrants continued to reside in traditional immigrant communities in the U.S., but also moved to new destinations in the Midwest and Southeast (Blau & Mackie, 2016; Gonzales & Raphael, 2017). States like Georgia and North Carolina as well as others across the South experienced rapid growth in the size of their undocumented population (Passel & Cohn, 2009). Localities with a long history of large foreign-born populations also experienced increases in the number of undocumented residents, though not at as fast a pace (Passel & Cohn, 2009). In summary, the population of undocumented residents generally grew during our study window and then stabilized. This population consisted predominantly of individuals likely to report Hispanic ethnicity. This period also saw an increase in the number of mixed-status families, wherein most children of undocumented residents are themselves citizens, as well as the development of immigrant communities in regions that had rarely experienced such growth.

4. Prior Literature

Several panel-based studies have directly examined the demographic impact of 287(g) agreements. All of these studies have relied on data from the American Community Survey (ACS) to measure the population for whom this immigration enforcement may be most salient. Most found that these partnerships had no effects or limited effects on the population. For example, Parrado (2012) examined metropolitan-area panel data on the estimated size of the prime-age, male, Mexican-born population and finds that ICE partnerships did not appear to have an effect (i.e., outside of 4 influential outlier locations). Similarly, O’Neil (2013) studied annual county-level panel data from 2005 to 2010 using three different population measures that might be influenced by ICE partnerships (i.e., Hispanic non-citizens, all Hispanics, and the foreign-born population). O'Neil (2013) finds no evidence of statistically significant effects and concludes that "local immigration enforcement has been ineffective in controlling growth of the unauthorized immigrant population." In another study based on ACS data, Watson (2013) finds that, while the jail-enforcement model of 287(g) agreements had no apparent effects, the task-force model appeared to make undocumented residents twice as likely to relocate to another Census region within the U.S. Kostandini et al. (2014) finds that the existence of 287(g) agreements reduced the share of self-reported immigrant non-citizens by approximately 0.5 percentage points each year.

A fundamental concern that motivates this study is that survey-based measures like those available in the ACS may, in general, provide a noisy and unreliable proxy for the number of undocumented residents. Indeed, an earlier study by Passel and Clark (1997) concludes that Census data overestimate the number of naturalized citizens, likely because non-citizens sometimes misreport as citizens. In a more recent update to this study, Van Hook and Bachmeier (2014) find that similar reporting biases exist in the American Community Survey (ACS) and among respondents for whom immigration enforcement is most likely to be salient (i.e., Mexican men and immigrants with fewer than 5 years of residency). Self-reported data on citizenship status may also be biased by the introduction of more intense local immigration enforcement. For example, Kostandini et al. (2014) acknowledge that their results could reflect a reporting bias if undocumented residents become less likely to report their citizenship status truthfully after the implementation of an ICE partnership. To address this concern, they also estimate the impact of 287(g) agreements on the estimated foreign-born population and find a statistically insignificant effect (i.e., except among those with fewer than 20 years in the U.S.).

These measurement concerns are a key motivation for this study's focus on Hispanic (and non-Hispanic) K-12 enrollment data. There are several reasons to believe that the changes in Hispanic enrollment may provide a more valid measure of the demographic impact of ICE partnerships. School districts in the U.S. complete universe surveys of their enrollments (i.e., both overall counts and counts by race/ethnicity) on an annual basis. Furthermore, school districts have high-powered financial incentives to report all of their students because it relates to state and federal funding streams. And, because these data are aggregated, the perceived risk from including counts of undocumented students or the children from mixed-status families is likely to be comparatively modest (i.e., in contrast to individual participation in surveys like the ACS). Another important advantage of school-enrollment data relative to ACS-based measures concerns external validity. Kostandini et al. (2014) report that they exclude from their analysis roughly 20 percent of counties with an ICE partnership because these locations are too lightly populated to have county-identified data in the ACS. In contrast, with few exceptions, school-enrollment data are universally available.

A related literature also suggests indirectly that the prevalence of null findings in ACS-based studies of the direct demographic effects of ICE partnerships is misleading. That is, studies that examine *other* potential social and economic effects of ICE partnerships without relying on self-reported data tend to find effects, which suggests these partnerships were behaviorally potent. For example, there is evidence that 287(g) agreements reduce overall employment and create labor shortages in the agricultural sector (Kostandini et al., 2013; Pham & Van, 2010) while increasing Hispanic housing foreclosures rates (Rugh & Hall, 2016). Using vital statistics data from North Carolina, Rhodes et al. (2015) also find evidence that ICE partnerships caused delays in Hispanic women receiving prenatal care.[8] Studies that instead rely on individual survey data and possibly unreliable self-reports of citizenship status also find effects on a diverse set of outcomes. For example, evaluations based on ACS and Current Population Survey (CPS) data find that ICE partnerships increase poverty and food insecurity while decreasing the use of food stamps (Amuedo-Dorantes, Arenas-Arroyo, & Sevilla, 2018; Potochnick, Chen, & Perreira, 2016) in households that are likely to have an undocumented parent. Similarly, using self-reported data to create a proxy for undocumented residents in the CPS, Amuedo-Dorantes and Lopez (2015, 2017) find that local immigration enforcement increases the observed grade repetition of younger children and the dropout rates of teens residing in the adopting communities.

While school-enrollment data by Hispanic ethnicity may be less subject to misreporting and external-validity concerns, they do not, of course, necessarily represent the entire population that might be influenced by ICE partnerships. We nonetheless view our focus on students and schools as a *second* key contribution of this study. As noted earlier, nearly half of undocumented

[8] Another study focusing on the 287(g) agreements in North Carolina (Forrester & Nowrasteh, 2018) indicates that these partnerships actually had no effect on crime rates.

residents live in a household with children, the overwhelming majority of whom are themselves U.S. citizens. An ICE partnership that meaningfully increased immigration enforcement and catalyzed family and student mobility could shape the educational opportunities of these children in multiple, policy-relevant ways. For example, the presence of stress caused by an unanticipated family relocation or fear of deportation could harm the developmental trajectory of these children.[9] It may also be true that ICE partnerships influence a student's educational opportunities if they inhibit the in-migration of immigrant families seeking better economic opportunities.

An extensive body of literature has focused on the impact of student mobility in the context of K-12 education in the U.S. (Rumberger, 2015; Welsh, 2017). In general, this literature suggests that the impact of mobility depends on several dimensions of the student's context. For example, "reactive" moves represent school changes that are primarily "to escape a bad situation" (Rumberger, Larson, Ream, & Palardy, 1999) such as parental job loss, changed family structure, or behavioral problems. This type of mobility tends to occur during the school year, rather than during the summer break, and has frequently been linked to dropout risk and to negative effects on student achievement, especially for Black and Hispanic students and for students experiencing multiple moves (Beatty, 2010; Hanushek et al., 2004; Xu, Hannaway, & D'Souza, 2009). For example, Xu et al. (2009) estimate that Hispanic students in grades 3 through 8 lose 0.052 SD of math achievement due to a single reactive move and up to 0.37 SD of math achievement after four or more moves.[10] In contrast, strategic moves are purposeful ones linked to seeking better opportunities and have been found to have no overall effect or a slightly positive effect on student achievement (Hanushek et al., 2004; Rumberger, 2015; Xu et al., 2009).

This literature implies that, if ICE partnerships led to meaningful reductions in Hispanic enrollment, it had pejorative social-welfare implications through some combination of increasing stress, catalyzing harmful reactive student mobility and inhibiting potentially beneficial strategic moves. ICE partnerships may have other relevant consequences for a community's schools and

[9] Recent evidence indicates that stress exposure can negatively affect cognitive functioning and student test performance (Heissel, Levy, & Adam, 2017).
[10] Given that the math achievement of students in these grades grows annually by 0.39 SD on average (Hill, Bloom, Black, & Lipsey, 2008), the negative impact of a single reactive move amounts to roughly 1.5 months of lost learning while four or more moves implies losing almost an entire year of learning.

students. For example, a variety of evidence (e.g., Bowen et al., 2016) argues for the social and educational benefits of diversity. And, in most of the counties that adopted ICE partnerships, Hispanic students were not a majority of the students. Therefore, these partnerships may cause further harm through reducing a community's experiences with diversity. However, there may also be resource benefits for remaining students in a community that adopts an ICE partnership. In particular, it may increase the per-pupil funding available to remaining students and or improve the socioeconomic levels of the remaining student peers.[11] We investigate these questions by examining two additional outcome variables: pupil-teacher ratios and the percent of students whose household income makes them eligible for the National School Lunch Program (NSLP). Eligibility for the NSLP, though extensively used in research studies, is a crude proxy for household income (Domina et al., 2018; Michelmore & Dynarski, 2017). However, NSLP eligibility is more strongly associated with student test performance (Domina et al., 2018) suggesting it reflects other dimensions of educationally relevant disadvantage.

5. Data

Our study is based on county-year panel data for the period from 2000 through 2011. Data acquired through multiple FOIA requests to DHS identify the 168 counties in which a law-enforcement agency applied for an ICE partnership as well as the subset of 55 counties in which an application was approved.[12] County-level law-enforcement agencies actually submitted the large majority of these applications. However, these data also identify counties in which a municipal or other sub-county agency applied. We identified the exact date on which these 287(g) agreements were officially approved as well as the enforcement type by relying on a variety of supplemental data sources (i.e., Capps et al., 2010; Gelatt, Bernstein, & Koball, 2017). As noted earlier, the first agreements were approved in 5 counties in 2005 and 2006. Between 2007 and 2009, 49 additional counties had ICE partnerships approved. And, in the final two

[11] ICE partnerships may also have consequences for communities that receive mobile students. However, it is not clear how to examine this because the location choices of undocumented residents who move are uncertain. The best available evidence (Watson, 2013) suggests they tend to move out of state or even to another Census region within the U.S.. In a relevant study of Hurricane Katrina refugees, Imberman et al. (2012) find no evidence that their arrival harmed the achievement of students in receiving schools.

[12] Rugh & Hall (2016) acquired these data and generously shared them with us. In a separate analysis not reported here, we examined the effect of ICE partnerships on geographically adjacent counties as well. While we found suggestive evidence for similar effects in neighboring counties, the corresponding estimates were generally smaller and statistically insignificant.

years of our study window, only 1 county had a newly approved application. In most of these counties (n=30), the 287(g) agreements were the "jail enforcement" model. Only 9 counties exclusively had a task-force model of enforcement, while 16 counties had both jail-enforcement and task-force models approved (Table 1).

The source for our outcome data (e.g., enrollment by race/ethnicity) is the National Center for Education Statistics' (NCES) annual Common Core of Data (CCD). The CCD includes an annual universe survey of enrollment and staffing in all K-12 public schools. The CCD intends for the enrollment counts in a given school year to be defined as of October 1st. Therefore, we coded a 287(g) agreement as being in effect for a given school year if it had final approval by the first of October. We also identified enrollment counts separately for Hispanic and non-Hispanic students (Table 1).[13] Additionally, we identified enrollment data for three different grade spans (i.e., elementary, middle, and high schools) because of the likelihood that enforcement-induced mobility of families with children may vary by the age of the child (Table 1).

Most counties in our sample have complete data across the 12 years of the panel. However, for a subset of 49 counties, there was at least one year when data were not available. When possible, we identified missing values by relying on other data sources (e.g., state departments of education) or simple interpolations based on the leading and lagging longitudinal data.[14] The missingness that remains implies our analytical sample consists of an unbalanced county-year panel of 1,862 observations (i.e., 168 counties observed annually over as many as 12 years). Most of the remaining missingness (i.e., roughly 88 percent) within counties occurs between 2000 and 2004 (i.e., *before* the first ICE partnerships) and none occurs after 2008. Critically, auxiliary regressions using the fixed-effect specifications we describe below indicate that the adoption of an ICE partnership has small and statistically insignificant effects on the

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[13] Prior to 2009, the CCD reported enrollment in five mutually exclusive categories: Hispanic, white, Black, Asian, or American Indian/Alaska Native. Beginning in 2009, some states began using seven mutually exclusive categories: Hispanic, white, Black, Asian, American Indian/Alaska Native, Hawaiian/Pacific Islander or two or more races. We consistently define non-Hispanic as the sum of white, Black, American Indian/Alaska Native and Hawaiian/Pacific Islander. As noted earlier, our measure excludes Asian because this population was susceptible to the policy impact. We also exclude the modestly sized "Two or More Races" category, which often includes individuals who report an Asian race as well as those who report "Some Other Race," a category also chosen by some respondents with an Hispanic identity (Jones & Bullock, 2012). Notably, our regressions condition on a binary indicator for counties in years when the 7 categories were used.

[14] A data appendix provides a detailed description. We also discuss our examination of these data for outlier values that suggest coding errors.

probability of missingness. Furthermore, we find that restricting our focus to a balanced panel for which data are not missing leads to results quite similar to those we report (Appendix Table 2).

We also included additional variables identifying the economic circumstances in each county and year by drawing on two additional sources. First, using data from the Bureau of Labor Statistics (BLS), we identified the unemployment rate in each county and year. Similarly, we added a county-year measure of median household income using data from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) program. These variables provide controls for potentially confounding economic circumstances that are changing within counties over time. However, because ICE partnerships may influence measured economic activity, we also report results that exclude these control variables. Table 1 reports descriptive statistics on all the key variables used in this study.

6. Methods

Our panel-based research design estimates the impact of ICE partnerships by effectively comparing the changes in the outcomes of adopting counties to the contemporaneous changes in counties that never or had not yet implemented such agreements. Specifically, we estimate variants of the following basic "differences in differences" (DD) specification:

$$Y_{ct} = \alpha_c + \gamma_t + \theta D_{ct} + \boldsymbol{\beta X_{ct}} + \epsilon_{ct} \tag{1}$$

where Y_{ct} is our dependent variable of interest (e.g., the natural log of Hispanic or Non-Hispanic student enrollment), α_c represents county fixed effects, γ_t are year fixed effects and ϵ_{ct} is a mean-zero error term that accommodates clustering at the county level (Bertrand, Duflo, & Mullainathan, 2004). These fixed effects account for time-invariant observable and unobservable characteristics specific to each county and common shocks across all counties over time. Our coefficient of interest, θ, represents the effect of an ICE partnership. This variable, D_{ct}, takes on a value of one when an observation is from an adopting county in a year when a 287(g) MOA was active. Finally, $\boldsymbol{X_{ct}}$ is a vector of covariates that varies within counties over time. This includes the natural log of median household income, the unemployment rate, and an indicator for whether race and ethnicity data were reported in seven categories instead of five.

Equation (1) embeds a variety of important assumptions that merit scrutiny. For example, this static DD specification implies that the effect of 287(g) will be a constant one. However, there are a variety of reasons to believe that the effects of these policies will vary over time.

Most obviously, the implementation of immigration enforcement as well as the public awareness of their existence (and the resulting behavioral change) is likely to grow following their adoption, implying that their effects become larger over time. We accommodate the possibility of such time-varying treatment effects in flexible semi-dynamic specifications that allow the policy to have distinct effects in the adoption year, in the first year after adoption, and 2 or more years after adoption:

$$Y_{ct} = \alpha_c + \gamma_t + \sum_{\tau=0}^{2+} \delta_\tau D_{c,t+\tau} + \boldsymbol{\beta X_{ct}} + \epsilon_{ct} \tag{2}$$

We also use these specifications to test directly the null hypothesis of a constant treatment effect (i.e., $H_0: \delta_0 = \delta_1 = \delta_{2+}$).

Our analysis also relies on the critical identifying assumption that the time-varying changes in "control" counties (i.e., those that never or hadn't yet formed ICE partnerships) provide a valid counterfactual for the changes that would have occurred in adopting counties if they had never adopted these ICE partnerships (i.e., a "parallel trends" assumption). This assumption is fundamental to interpreting our estimates as causal effects. However, it may be invalid. For example, our estimate of the impact of ICE partnerships would be biased downward if the successful adoption of this initiative were preceded by a comparative increase in Hispanic enrollments (i.e., differential population growth that motivated the design of a *successful* 287(g) application). We examine the empirical validity of this critical assumption in a variety of ways. For example, we explore the robustness of our findings by sometimes conditioning on a variety of possibly confounding county-year variables (including potentially endogenous ones) as well as by excluding various counties (e.g., Los Angeles) that may be influential observations. We also present two other types of direct evidence that speaks to the causal warrant of the inferences based on this approach.

First, we present evidence on the presence of parallel trends across counties that do and do not adopt ICE partnerships through estimating "event study" DD specifications (Angrist & Pischke, 2009). These specifications include an unrestrictive set of dummy variables that identify leads and lags of the policy change. Specifically, they take the following form:

$$Y_{ct} = \alpha_c + \gamma_t + \sum_{\tau=1}^{5} \delta_{-\tau} D_{c,t-\tau} + \sum_{\tau=0}^{2+} \delta_\tau D_{c,t+\tau} + \epsilon_{ct} \tag{3}$$

where Y_{ct} is again our dependent variable of interest and α_c and γ_t represent our county and year fixed effects. In this specification, we are particularly interested in estimates of the parameters, $\delta_{-\tau}$, which identify the "effect" of being τ years *prior* to the adoption of an ICE partnership (i.e., relative to a reference category of not adopting the policy or being 6+ years prior to adoption). Evidence that adopting and non-adopting counties have similar time-varying changes prior to the adoption of the policy would be consistent with the parallel trends assumption.[15] We report these point estimates directly and test their joint significance using F-tests.

A second and important source of evidence on the internal validity of our inferences comes from an intuitive falsification exercise. We estimate versions of equations (1) and (2) in which the dependent variable is the natural log of *non-Hispanic* school enrollment in each county and year. The logic of this test is straightforward. As noted earlier, very few undocumented residents are in the non-Hispanic category. Therefore, we expect local ICE partnerships to have no (or sharply attenuated) relevance for these groups. It follows that, if our DD specifications are generating reliable inferences, we would expect to find no (or substantially smaller) estimated effects on non-Hispanic enrollment. Conversely, if ICE partnerships appear to have large effects on non-Hispanic enrollment, it would suggest our panel-based inferences are biased by the presence of unobserved variables that are related to the policy adoption.

Fortunately, in this scenario, the data on non-Hispanic enrollment would then provide a compelling way to control for the unobserved determinants of enrollment that may be specific to county-year observations. We present such evidence through estimating "difference in difference in differences" (DDD) specifications based on stacked school enrollment data at the county-ethnicity-year level. These DDD specifications take the following form:

$$Y_{cgt} = \alpha_{ct} + \gamma_{tg} + \delta_{cg} + \theta D_{cgt} + \epsilon_{cgt} \tag{4}$$

This approach conditions on an unrestrictive set of fixed effects for each possible two-way interaction: county-year (i.e., α_{ct}), year-ethnicity (i.e., γ_{tg}), and county-ethnicity (i.e., δ_{cg}). The parameter of interest is then the estimated impact of the three-way interaction (i.e., D_{cgt}) that identifies observations of Hispanic enrollment in counties and years where there are active ICE partnerships. Notably, this approach allows us to estimate the enrollment effects of ICE partnerships in specifications that control unambiguously for the unobserved determinants of

[15] A recent working paper (Borusyak & Jaravel, 2017) explains that, in event studies where all cross-sectional units are treated at some point, the dynamic effects can only be identified up to a linear trend. However, it should be noted that, in our context, roughly two-thirds of the counties in our data *never* adopted an ICE partnership.

enrollment unique to each county-by-year observation. We also report results from a semi-dynamic DDD specification similar to equation (2).

A remaining methodological issue merits some discussion. An established (but little discussed) property of regression-based fixed-effect estimators is that, when treatment effects are heterogeneous, OLS produces an overall estimate that is weighted by the conditional variance in treatment (e.g., Angrist & Pischke, 2009). In DD applications like ours, this implies that the early-adopting counties (i.e., those that were adopted in the middle rather than towards the end of our study period) effectively have higher weights (Goodman-Bacon, 2018). A recent study by Gibbons et al. (2018) underscores the potential empirical relevance of this issue. They examined eight influential studies that report fixed-effects estimates and finds that the conventional fixed-effect estimates often differ quite meaningfully from the relevant average treatment effect (ATE). They also developed an unbiased and consistent estimator of the ATE that effectively reweights observations to undo the weighting implied by OLS in the presence of fixed effects. We implemented this regression-weighted estimator (RWE) in this context and found it generated point estimates quite similar to those we report. Also, using the Wald test introduced by Gibbons et al. (2018), we find that we cannot reject the null hypothesis of equivalence between our fixed-effect estimates and the corresponding ATE. As an additional robustness check, we also examine our results in samples that exclude the early-adopting counties.

7. Results

Table 2 presents the main results from DD specifications that estimate the impact of ICE partnerships on Hispanic student enrollment. The results in column (1) suggest that ICE partnerships reduced Hispanic student enrollments by a statistically significant 7.3 percent (i.e., exp(-0.076) - 1). The semi-dynamic results in column (2) suggest that this negative effect grew monotonically over time from roughly 5 percent in the adoption year to nearly 10 percent two or more years after adoption. However, an F-test based on the results in column (2) fails to reject the null hypothesis of a common treatment effect over time (p-value = 0.1107). The remaining columns in Table 2 report the results when controls for county-year economic conditions are added. The results are effectively unchanged with one notable exception. In the semi-dynamic specification reported in column (4), the null hypothesis of a common treatment effect can be rejected at the 90 percent significance level (p-value = 0.0895). To place these large impact estimates into perspective, we identified the total number of K-12 Hispanic students in the counties that adopted ICE partnerships in 2005 just prior to the onset of the policy as roughly 3.2 million. A 10-percent reduction from this base implies that these ICE partnerships eventually displaced around 320,000 students. As noted earlier, the displacement observed in our enrollment measure can operate through encouraging threatened families to leave a community, dropping out of school, and discouraging other families from entering.

In Table 3, we begin exploring the robustness of our main results, which are repeated in columns (1) and (2). Specifically, in columns (3) and (4), we report DD estimates of the impact of ICE partnerships on *non-Hispanic* enrollment. These results consistently indicate small and statistically insignificant effects. The lack of an impact on non-Hispanic student enrollment is consistent with the hypothesis that our findings do not reflect the confounding influence of unobserved determinants of student enrollment. In columns (5) and (6), we synthesize the results from these comparative DD specifications by reporting DDD estimates. These results similarly suggest an impact of roughly 7 percent and that appears to increase monotonically over time, though these estimates are less precise. We also examined our findings through estimating event-study DD specifications (equation 3) for both Hispanic and non-Hispanic enrollment. We summarize these results in Figure 1. The results for non-Hispanic enrollment indicate that, both prior to and after the adoption of an ICE partnership, this outcome was trending similarly in both adopting and non-adopting counties. Additionally, prior to the adoption of an ICE partnership, Hispanic enrollment in treated and untreated counties had quite similar trends. However, these point estimates indicate that Hispanic enrollment began falling sharply as the ICE partnerships were introduced. These point estimates are not statistically precise (see Appendix Table 1). Nonetheless, the patterns in both of these measures are consistent with the identifying assumptions of the DD and DDD specifications.[16]

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[16] One interesting feature to note in Figure 1 is that treatment counties saw small year-to-year enrollment declines of approximately 1 percent just *prior* to the final approval of their ICE partnership. However, these year-to-year changes jumped to 3 percent with the onset of the policy. A slight drop in Hispanic enrollment before the official policy adoption could reflect a community's firm awareness (or an informed anticipation) of an imminent ICE partnership. We used the date of the final signature approving the MOA to date the policy change. However, negotiations and communication between local police and ICE indicate that DHS approval was often known ahead of final MOA completion. This knowledge along with any possible changes in immigration-related enforcement could have influenced enrollment. As an additional robustness check, we examined our main findings in models that moved the treatment adoption one year earlier and found it only attenuated our results modestly (i.e., a 6.1 percent reduction with a p-value of 0.077).

We also explored the robustness of our results through a variety of sample restrictions and changes to the control variables. We report the key results of all these DD specifications, both for Hispanic and non-Hispanic enrollment, in Appendix Table 2. For example, we restricted the control counties in our study to those that had their 287(g) applications denied. This excludes counties that voluntarily withdrew their application or had an application pending at the close of our study window. The overall impact on Hispanic enrollment in this sample is larger (i.e., roughly 10 percent) while the estimated effect on non-Hispanic enrollment remains small and statistically insignificant. We also examined our results in samples that excluded the earliest adopting counties (i.e., 2005 and 2006) and in samples that excluded the counties that Parrado (2012) identified as influential outliers (Los Angeles, CA; Maricopa, AZ; Riverside, CA, and Dallas County, TX). In addition, we examined our results in models that excluded data from states that had seemingly active state-level 287(g) agreements (AZ, CO, MA) and, in separate specifications, included a control for this state-level policy. We then report our results in specifications that conditioned on other immigration-enforcement policies (i.e., E-Verify and Secure Communities). We also examine the results from focusing on a balanced panel of counties. Across all of these varied samples and specifications, Appendix Table 2 reports statistically significant and negative effects of ICE partnerships on Hispanic enrollment and small, statistically insignificant effects on non-Hispanic enrollment. One notable exception is that population-weighted estimates indicate that ICE partnerships led to statistically significant reductions in non-Hispanic enrollment. However, weighted estimates also suggest that the estimated impact on Hispanic enrollments is particularly large (i.e., nearly a 20 percent reduction). So, the DDD estimates by these DD estimates are similar to those we report.

These results provide strikingly consistent and robust evidence that ICE partnerships led to meaningful reduction in Hispanic enrollment in public schools. In Tables 4 and 5, we present evidence on the heterogeneity in these results. Table 4 reports the estimated effects of ICE partnerships on Hispanic and non-Hispanic enrollment defined separately for grades K-5 (elementary schools), 6-8 (middle schools), and 9-12 (high schools). The results in columns (1) and (2) consistently indicate that ICE partnerships had negative effects. However, the point estimates are particularly large and statistically significant for elementary-school enrollment (i.e., more than a 9 percent reduction). In contrast, the estimated effects on non-Hispanic enrollment are small and statistically insignificant for all school levels. Mixed-status families with young children may be uniquely likely to move in response to an enforcement threat for a variety of reasons. Parents often see moving younger children as easier to move and undocumented parents of younger children may be particularly concerned about the fate of their younger children if they are apprehended for an immigration violation.

In Table 5, we report the estimated effects of ICE partnerships on our enrollment measures by enforcement type (i.e., jail enforcement, task force, or a hybrid of both). The point estimates in columns (1) and (2) are consistently negative for all enforcement types. The estimated effects of the more common jail-enforcement model are particularly and statistically significant. This pattern is somewhat surprising given that the task-force model seems more severe in that it encourages police officers to enforce immigration violations in their regular duties. However, the differences in the impact of these enforcement models do not appear to be statistically meaningful. F-tests indicate that we cannot reject the hypothesis of a common effect across these enforcement models. Notably, the estimated effects on the non-Hispanic enrollment measure (i.e., columns (3) and (4)) are consistently small and statistically insignificant.

The literature on student mobility, in combination with the results we report here, suggest that ICE partnerships are harmful to Hispanic children (i.e., causing reactive mobility and inhibiting strategic mobility). However, in theory, there might be some benefits to the communities that introduce this policy. For example, the reduction in student enrollments may result in more resources for remaining students. Furthermore, the reduction in enrollment may raise the socioeconomic status of the remaining students' peers. We provide evidence on these questions in Table 6. Specifically, we report DD estimates of the impact of ICE partnerships on pupil-teacher ratios and on the percent of students who are NSLP-eligible. For both outcomes, these results indicate that ICE partnerships had small and statistically insignificant effects. In Appendix Table 3, we interrogate these results further through event-study specifications. In general, these findings are consistent with the null results reported in Table 6 though there is some qualified evidence that ICE partnerships *increased* the share of remaining students whose low household income qualified them for the NSLP.

8. Discussion

The heated political controversies around immigration policies in the U.S. provide a compelling motivation for the large body of research on this topic. However, credibly examining the outcomes of undocumented residents using conventional survey data is difficult because of the documented propensity for individuals to misreport their immigrant and citizenship status. One fundamental motivation for this study of controversial partnerships between ICE and local law-enforcement agencies turns on the potential benefits of relying instead on administrative data. Specifically, we provide new evidence on the most proximate demographic impact of these ICE partnerships using panel data on school enrollment by Hispanic ethnicity. We find quite robust, quasi-experimental evidence that ICE partnerships led to a substantial reduction in the enrollment of Hispanic students in public schools. Specifically, our results indicate that local ICE partnerships reduced a county's Hispanic enrollment by 7.3 percent overall or roughly 10 percent within 2 years.

At the most basic level, the evidence from these unique administrative data indicates that local partnerships with ICE seemed to create highly unattractive environments for undocumented residents (and perhaps Hispanic citizens as well). However, a second key feature of our enrollment-based results is to underscore the presumably unintended consequences of ICE partnerships for students and schools. In particular, the literature clearly suggests that causing "reactive" mobility (i.e., moves under duress) or dropping out of school harms students, while inhibiting moves towards economic opportunity can also be detrimental. Based on our results and the pre-treatment levels of Hispanic enrollment in the adopting counties, we estimate that ICE partnerships displaced more than 300,000 Hispanic students in this manner. These impacts are likely to be concentrated among younger students and, it should be noted, that most of the students with an undocumented parent are themselves U.S. citizens. At the same time, while ICE partnerships reduced the Hispanic presence in public schools, we find no evidence that they lowered pupil-teacher ratios or the share of remaining public-school students who are disadvantaged (i.e., NSLP eligible).

Given the expansion of ICE partnerships under the Trump Administration, these results have contemporary policy relevance. In particular, this study's findings along with evidence on the pejorative economic consequences of local ICE partnerships (Kostandini et al., 2013; Rugh & Hall, 2016) can inform the public consideration of these policies. Other relevant considerations include the allegations that the implementation of these partnerships harms the capacity of local police to serve immigrant communities by encouraging discriminatory practices and eroding trust (e.g., discouraging victims of crime and witnesses from coming forward).

Additionally, our findings also have immediate relevance for the varied entities that serve and support children in communities effected by ICE partnerships (e.g., counselors, teachers, doctors). This study's findings suggest that these entities can better serve these children if they are aware of the potential developmental consequences of interior immigration enforcement and if they can identify and implement promising programs and practices (e.g., Baker et al., 2014; U.S. Department of Education, 2012).

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Table 1 - Descriptive Statistics

Variables	Mean	SD	Min	Max
Hispanic Enrollment	21,800	93,172	15	1,069,267
Elementary School (K-5)	10,981	45,588	6	529,998
Middle School (6-8)	4,923	21,107	2	247,333
High School (9-12)	5,746	25,612	3	319,500
Non-Hispanic Enrollment	47,022	62,046	854	511,027
Elementary School (K-5)	21,148	27,439	372	234,625
Middle School (6-8)	11,120	14,584	198	121,062
High School (9-12)	14,568	19,786	279	156,764
Active 287(g) MOA	0.09	0.29	0	1
Type of Active MOA: Jail	0.05	0.22	0	1
Type of Active MOA: Task Force	0.01	0.11	0	1
Type of Active MOA: Jail & Task Force	0.03	0.17	0	1
% NSLP-Eligible	37.15	14.77	5.20	76.74
Pupil-Teacher Ratio	16.0	2.3	11.0	26.2
Median Household Income	50,922	14,227	26,666	119,525
Unemployment Rate	5.9	2.6	1.8	16.9

Notes: Our analytical sample is a panel of 168 counties observed annually from 2000 to 2011 (N=1,862). Each included county applied for a 287(g) agreement and 55 counties implemented an agreement during this period. The student enrollment and educational data are from the National Center for Education Statistics' Public Elementary and Secondary School Universe Survey reported through the Elementary and Secondary Information System, 2000-2011. The immigration enforcement data are from the Immigration and Customs Enforcement division of the Department of Homeland Security, 2000-2011. The economic data are from the Local Area Unemployment Statistics published by the Bureau of Labor Statistics and the Small Area Income and Poverty Estimates reported by the US Census Bureau, 2000-2011.

Table 2 - The Estimated Effect of a 287(g) MOA on Hispanic Student Enrollment

Independent Variable	(1)	(2)	(3)	(4)
Active 287(g) MOA	-0.076**	-	-0.075**	-
	(0.035)		(0.034)	
Adoption Year	-	-0.049*	-	-0.045*
		(0.027)		(0.026)
1 Year Lag	-	-0.079**	-	-0.075**
		(0.034)		(0.033)
2+ Year Lag	-	-0.102**	-	-0.104**
		(0.047)		(0.046)
County-year controls?	no	no	yes	yes
R^2	0.9932	0.9932	0.9942	0.9942
p-value: (H_0: $\beta_1 = \beta_2 = \beta_3$)	-	0.1107	-	0.0895

Notes: The dependent variable is the natural log of Hispanic enrollment; standard errors, clustered at the county level, are in parentheses. All models include county FE, year FE and an indicator for whether race and ethnicity data were reported in seven categories instead of five (coefficients suppressed). Results reported in columns (3) and (4) also include the following county-year controls: the natural log of median household income and the unemployment rate. The county-year data are based on 168 counties observed annually from 2000 to 2011 (N=1,862).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3 - The Estimated Effect of a 287(g) MOA on Student Enrollment by Hispanic Ethnicity, DD and DDD

	DD				DDD	
	Hispanic		Non-Hispanic			
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
Active 287(g) MOA	-0.076**	-	-0.005	-	-0.070*	-
	(0.035)		(0.016)		(0.039)	
Adoption Year	-	-0.049*	-	-0.003	-	-0.043
		(0.027)		(0.013)		(0.032)
1 Year Lag	-	-0.079**	-	-0.007	-	-0.064
		(0.034)		(0.016)		(0.039)
2+ Year Lag	-	-0.102**	-	-0.006	-	-0.103**
		(0.047)		(0.022)		(0.051)
R^2	0.9932	0.9932	0.9972	0.9972	0.9987	0.9987
p-value: (H_0: $\beta_1 = \beta_2 = \beta_3$)	-	0.1107	-	0.6992	-	0.1733

Notes: The dependent variable is the natural log of enrollment; standard errors, clustered at the county level, are in parentheses. Models (1) - (4) include county FE, year FE and an indicator for whether race and ethnicity data were reported in seven categories instead of five (coefficients suppressed). The county-year data are based on 168 counties observed annually from 2000 to 2011 (N=1,862). Models (5) and (6) are based on county-year-ethnicity data (N=3,724) and condition on county-year FE, county-ethnicity FE, and year-ethnicity FE.

* p < 0.1, ** p < 0.05, *** p < 0.01

Table 4 - The Estimated Effects of a 287(g) MOA on Student Enrollment by School Level

	DD				DDD
	Hispanic		Non-Hispanic		
Sample	(1)	(2)	(3)	(4)	(5)
Elementary School	-0.099***	-0.097***	-0.002	-0.001	-0.096**
	(0.036)	(0.036)	(0.018)	(0.018)	(0.040)
Middle School	-0.056	-0.055	0.003	0.003	-0.059
	(0.035)	(0.034)	(0.017)	(0.017)	(0.041)
High School	-0.057	-0.057	-0.013	-0.014	-0.043
	(0.037)	(0.036)	(0.015)	(0.015)	(0.044)
County-year controls?	no	yes	no	yes	n/a

Notes: The dependent variable is the natural log of enrollment; standard errors, clustered at the county level, are in parentheses. Students in grades K-5 are categorized as elementary school students, grades 6-8 as middle school students, and grades 9-12 as high school students. Each cell and corresponding standard error represents a separate regression. Models reported in columns (1)-(4) include county FE, year FE and an indicator for whether race and ethnicity data were reported in seven categories instead of five (coefficients suppressed). Results reported in columns (2) and (4) also include the following county-year controls: the natural log of median household income and the unemployment rate. The county-year data are based on 168 counties observed annually from 2000 to 2011 (N=1,862). Model (5) is based on county-year-ethnicity data (N=3,724) and conditions on county-year FE, county-ethnicity FE, and year-ethnicity FE.

* $p < 0.1$, ** $p< 0.05$, *** $p < 0.01$

Table 5 - The Estimated Effects of a 287(g) MOA on Student Enrollment by MOA Type

	DD				DDD
	Hispanic		Non-Hispanic		
Sample	(1)	(2)	(3)	(4)	(5)
Jail	-0.099**	-0.099**	-0.002	-0.001	-0.099*
	(0.049)	(0.048)	(0.021)	(0.020)	(0.051)
Task Force	-0.050	-0.038	0.001	0.001	-0.047
	(0.063)	(0.065)	(0.036)	(0.036)	(0.070)
Jail & Task Force	-0.047	-0.049	-0.014	-0.014	-0.030
	(0.047)	(0.049)	(0.028)	(0.028)	(0.063)
County-year controls?	no	yes	no	yes	n/a
R^2	0.9932	0.9932	0.9972	0.9972	0.9987
p-value: (H_0: $\beta_1 = \beta_2 = \beta_3$)	0.6837	0.9256	0.9256	0.9050	0.6346

Notes: The dependent variable is the natural log of enrollment; standard errors, clustered at the county level, are in parentheses. Each column represents a separate regression. Models reported in columns (1)-(4) include county FE, year FE and an indicator for whether race and ethnicity data were reported in seven categories instead of five (coefficients suppressed). Results reported in columns (2) and (4) also include the following county-year controls: the natural log of median household income and the unemployment rate. The county-year data are based on 168 counties observed annually from 2000 to 2011 (N=1,862). Model (5) is based on county-year-ethnicity data (N=3,724) and conditions on county-year FE, county-ethnicity FE, and year-ethnicity FE.

* $p < 0.1$, ** $p< 0.05$, *** $p < 0.01$

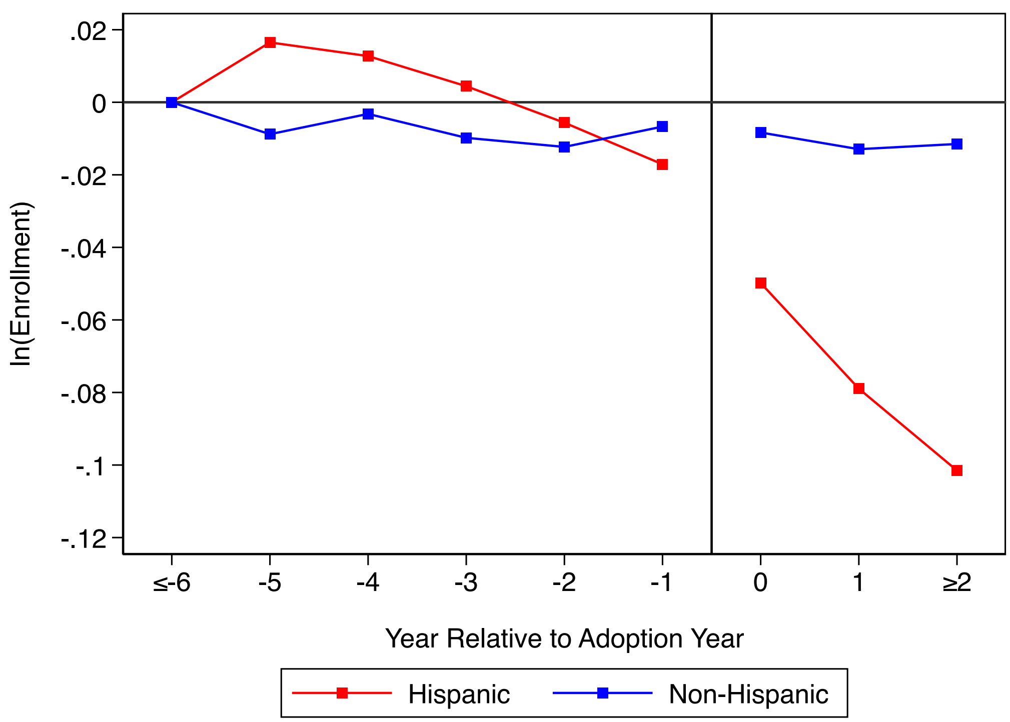
Table 6 - The Estimated Effects of a 287(g) MOA on the Pupil-Teacher Ratio and the Percent NSLP-Eligible

	Dependent Variables			
	Pupil-Teacher Ratio		% NSLP-Eligible	
Independent variable	(1)	(2)	(3)	(4)
Active 287(g) MOA	0.267	0.277	0.671	0.742
	(0.198)	(0.195)	(0.608)	(0.536)
County-year controls?	no	yes	no	yes
R^2	0.8656	0.8671	0.9659	0.9703

Notes: The dependent variable in Models (1)-(2) is the pupil-teacher ratio and in Models (3)-(4) is the percent of NSLP-eligible students; standard errors, clustered at the county level, are in parentheses. All models include county FE, year FE and an indicator for whether race and ethnicity data were reported in seven categories instead of five (coefficients suppressed). Results reported in columns (2) and (4) also include the following county-year controls: the natural log of median household income and the unemployment rate. The county-year data are based on 168 counties observed annually from 2000 to 2011 (N=1,862).

* p < 0.1, ** p < 0.05, *** p < 0.01

Figure 1 – The Estimated Effects of the Adoption of a 287(g) MOA on Student Enrollment by Hispanic Ethnicity Relative to Adoption Year



Appendix

Appendix Table 1 reports the key estimation results from the full event-study DD for both Hispanic and Non-Hispanic student enrollment. Appendix Table 2 reports robustness checks based on multiple sample restrictions (e.g., the balanced v. unbalanced panel; with and without early adopters; using weighted least squares regression, etc.). Appendix Table 3 presents event-study results that correspond to the results presented in Table 6 for the pupil-teacher ratio and the percent of students who are NSLP-eligible.

We also provide here additional details on the construction of our analytical sample. Our county-year sample construction begins with the 168 counties from which a law-enforcement agency submitted a 287(g) application during our study window. Given our 12-year study window (i.e., 2000 to 2011), this implies a panel data set potentially with 2,016 observations. We then examined these 2,016 observations for missing values in any of our outcome variables (i.e., student enrollment counts by race/ethnicity and school level, percent NSLP-eligible, and the pupil-teacher ratio). For 12 of these county-year observations, the only missing value was the pupil-teacher ratio, which we constructed using the overall student membership and teacher FTE variables. After this correction, 244 of our county-year observations had a missing value for at least one variable. For the remaining county-year observations, we first sought other data sources to replace the missing value for any of our key analytical variables. These data sources included state education agencies (e.g., the Texas Education Agency), local education agencies (e.g., Washington County, Utah Office of Education) or NCES's Elementary and Secondary Information System data reported at different levels (i.e., district level reports). In 45 instances, we were able to obtain valid data and rely solely on these to replace the missing values in a particular county-year observation. When a county-year observation had missing values but external data were *not* available, we implemented a simple imputation based on the available longitudinal data. Specifically, if valid data for a given variable existed both prior to and after the missing value, we conducted a linear interpolation to generate an estimate of the missing value (i.e., a simple average of the prior and subsequent values). For 49 county-year observations, conducting these linear interpolations enabled us to remove all missing values from the observation. In 5 county-year observations, we both replaced a value and conducted a linear interpolation to obtain complete data. Following this, we were left with a total of 145 county-year observations with missing data.

We also examined our key variables for misreported or erroneous data. We did this by assessing the within-county, year-to-year percent change. We flagged observations that experienced a change by nine percent or more in the membership variable for further scrutiny. For the more volatile pupil-teacher ratio and percent NSLP-eligible variables, we flagged observations with a change by fifteen percent or more. For each flagged observation, we first sought to verify the accuracy of the data points through external sources. If external data confirmed the accuracy of the data, we retained the existing value and removed the flag for that observation. When the external data suggested that the flagged data point was inaccurate, we replaced this data point with the available external data and removed the flag. In some cases (i.e., most notably for counties in Texas, Illinois and Delaware), this review suggested other variables in a county's longitudinal profile were miscoded and we replaced them using other data sources. In total, we conducted this type of replacement in 376 of the county-year observations in the sample. For 213 of these observations, the change was solely to the percent NSLP-eligible variable; the pupil-teacher ratio variable, or both. When external data were not available, we assessed whether the flagged observation was accurate by comparing it against other data points in that county in prior or following years. If the outlier flag was judged to be correct, we replaced the outlying data with missing values. When these missing values were surrounded by valid data points, we conducted a linear interpolation and removed the flag. In total, we conducted at least one linear interpolation for data points in 177 county-year observations. For 133 of these observations, the interpolation was solely to the percent NSLP-eligible variable, the pupil-teacher ratio, or both. In 61 county-year observations, we conducted both an external replacement and a linear interpolation for different outlying values. In some instances, however, the missing values were from the earliest or latest years of the panel. In these cases, a linear interpolation was not feasible and the missing values were retained. After replacing outlying data points as missing if external data points were unavailable and a linear interpolation could not be conducted, 9 additional county-year observations were flagged as including missing data.

After completing these steps, a total of 154 county-year observations from 49 different counties still had at least one key variable (i.e., enrollment counts, percent NSLP-eligible, or the pupil-teacher ratio) with missing data. The other 1,862 county-year observations (92.4 percent of the sample) had complete data for all variables of interest. The county-year observations without these data were highly concentrated in certain states (e.g., in certain instances the reporting from certain states did not include enrollment counts by grade and race/ethnicity) and during the early years of our study window (i.e., years prior to the first local 287(g) partnerships). Specifically, county-year observations are missing for counties in the following states during the following years:

- Arizona between 2000-2003 (N=24);
- Illinois between 2000-2001 (N=10);
- Massachusetts between 2000-2003 (N=20);
- Nevada in 2000 (N=1);
- New Hampshire in 2000 (N=3);
- Tennessee between 2000-2005 (N=60);
- Virginia in 2000 (N=16); and
- Washington between 2000-2001 (N=2).

Additionally, a few remaining counties from Arizona (N=8), North Carolina (N=4), Virginia (N=3), Tennessee (N=2) and Utah (N=1) had missing data in other years as well.

To ascertain whether these 154 observations appeared to be missing at random, we examined an auxiliary DD regression in which an indicator for missing observations was the dependent variable (N = 2,016). The results indicated that ICE partnerships had a small and statistically insignificant effect on missingness. Given this evidence of missingness at random, we privilege the unbalanced panel of 1,862 county-year observations, which incorporates data from all 168 counties in our sample. However, in Appendix Table 2, we also report results based only the subset of counties that have complete panel data in all twelve years of the panel (N=1,428).

Appendix Table 1 - The Estimated Effects of a 287(g) MOA on Student Enrollment Over Time

	Hispanic		Non-Hispanic	
Independent variable	(1)	(2)	(3)	(4)
5 Year Lead	0.016	0.016	-0.009	-0.009
	(0.027)	(0.026)	(0.011)	(0.011)
4 Year Lead	0.013	0.011	-0.003	-0.004
	(0.034)	(0.033)	(0.012)	(0.012)
3 Year Lead	0.004	0.004	-0.010	-0.010
	(0.040)	(0.039)	(0.015)	(0.015)
2 Year Lead	-0.006	-0.004	-0.012	-0.012
	(0.046)	(0.046)	(0.018)	(0.018)
1 Year Lead	-0.017	-0.018	-0.007	-0.007
	(0.051)	(0.050)	(0.020)	(0.020)
Adoption Year	-0.050	-0.046	-0.008	-0.008
	(0.053)	(0.053)	(0.022)	(0.022)
1 Year Lag	-0.079	-0.076	-0.013	-0.013
	(0.057)	(0.056)	(0.024)	(0.024)
2+ Year Lag	-0.102	-0.105	-0.011	-0.012
	(0.066)	(0.066)	(0.029)	(0.029)
County-year controls?	no	yes	no	yes
R^2	0.9932	0.9942	0.9972	0.9972
p-value: (H_0: $\beta_1 = \beta_2 = \beta_3 = \beta_4 = \beta_5$)	0.7175	0.6399	0.3178	0.4092
p-value: (H_0: $\beta_6 = \beta_7 = \beta_8$)	0.1151	0.0929	0.6935	0.6809

Notes: The dependent variable is the natural log of enrollment; standard errors, clustered at the county level, are in parentheses. All models include county FE, year FE and an indicator for whether race and ethnicity data were reported in seven categories instead of five (coefficients suppressed). Results reported in columns (2) and (4) also include the following county-year controls: the natural log of median household income and the unemployment rate. The reference category is relative to not adopting the policy or being 6+ years prior to adoption. The county-year data are based on 168 counties observed annually from 2000 to 2011 (N=1,862).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2 - The Estimated Effects of a 287(g) MOA by Sample Restrictions

	Hispanic		Non-Hispanic		
Sample Restriction	(1)	(2)	(3)	(4)	N
Full Sample	-0.076**	-0.075**	-0.005	-0.005	1862
	(0.035)	(0.034)	(0.016)	(0.016)	
Deniers Only	-0.105***	-0.105***	-0.003	-0.003	1366
	(0.036)	(0.036)	(0.017)	(0.017)	
No Early Adopters	-0.059*	-0.059*	0.004	0.004	1814
	(0.033)	(0.033)	(0.016)	(0.016)	
No Early Adopters & Deniers Only	-0.088**	-0.088**	0.005	0.007	1318
	(0.035)	(0.035)	(0.017)	(0.017)	
Exclude LA County	-0.062*	-0.062*	-0.001	-0.000	1850
	(0.033)	(0.033)	(0.016)	(0.016)	
Exclude Maricopa County	-0.074**	-0.072**	-0.005	-0.005	1854
	(0.035)	(0.035)	(0.016)	(0.016)	
Exclude Riverside County	-0.073**	-0.072**	-0.004	-0.004	1850
	(0.036)	(0.035)	(0.016)	(0.016)	
Exclude Dallas County	-0.070**	-0.069**	-0.002	-0.001	1850
	(0.035)	(0.034)	(0.016)	(0.016)	
Exclude AZ & CO & MA	-0.070*	-0.068*	-0.008	-0.008	1770
	(0.037)	(0.036)	(0.017)	(0.017)	
287(g) State Partnership Control	-0.075**	-0.074**	-0.004	-0.003	1862
	(0.035)	(0.034)	(0.016)	(0.016)	
Secure Communities Control	-0.076**	-0.075**	-0.005	-0.005	1862
	(0.035)	(0.034)	(0.016)	(0.016)	
E-Verify Control	-0.081**	-0.080**	-0.007	-0.007	1862
	(0.034)	(0.033)	(0.016)	(0.016)	
Weighted Least Squares	-0.199***	-0.143***	-0.065***	-0.047***	1862
	(0.054)	(0.037)	(0.021)	(0.017)	
Balanced Panel	-0.084*	-0.082*	-0.017	-0.016	1428
	(0.044)	(0.042)	(0.019)	(0.018)	
County-year controls?	no	yes	no	yes	

Notes: Standard errors are in parentheses and clustered at the county level. All models include county FE, year FE and an indicator for whether race and ethnicity data were reported in seven categories instead of five (coefficients suppressed). Results reported in columns (2) and (4) also include the following county-year controls: the natural log of median household income and the unemployment rate. Weighted least squares weight by enrollment in 2004-05.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3 - The Estimated Effects of a 287(g) MOA on the Pupil-Teacher Ratio and the Percent NSLP-Eligible

	Dependent Variables			
	Pupil-Teacher Ratio		% NSLP-Eligible	
Independent variable	(1)	(2)	(3)	(4)
5 Year Lead	-0.253**	-0.249*	0.619	0.545
	(0.125)	(0.126)	(0.521)	(0.469)
4 Year Lead	-0.045	-0.048	1.027	0.897
	(0.159)	(0.162)	(0.648)	(0.573)
3 Year Lead	-0.158	-0.169	0.793	0.797
	(0.176)	(0.181)	(0.760)	(0.645)
2 Year Lead	0.034	0.013	0.747	0.917
	(0.210)	(0.211)	(0.892)	(0.762)
1 Year Lead	0.113	0.108	0.945	0.874
	(0.245)	(0.242)	(0.955)	(0.811)
Adoption Year	0.249	0.246	1.292	1.535*
	(0.293)	(0.290)	(1.017)	(0.870)
1 Year Lag	0.143	0.132	1.296	1.519*
	(0.284)	(0.278)	(1.023)	(0.857)
2+ Year Lag	0.309	0.332	1.179	0.931
	(0.294)	(0.290)	(1.045)	(0.897)
County-year controls?	no	yes	no	yes
R^2	0.8662	0.8676	0.9660	0.9705
p-value: (H_0: $\beta_1 = \beta_2 = \beta_3 = \beta_4 = \beta_5$)	0.1321	0.1372	0.5311	0.7990
p-value: (H_0: $\beta_6 = \beta_7 = \beta_8$)	0.4051	0.0747	0.2004	0.4292

Notes: The dependent variable in Models (1)-(2) is the pupil-teacher ratio and in Models (3)-(4) is the percent of students NSLP-eligible; standard errors, clustered at the county level, are in parentheses. All models include county FE, year FE and an indicator for whether race and ethnicity data were reported in seven categories instead of five (coefficients suppressed). Results reported in columns (2) and (4) also include the following county-year controls: the natural log of median household income and the unemployment rate. The reference category is relative to not adopting the policy or being 6+ years prior to adoption. The county-year data are based on 168 counties observed annually from 2000 to 2011 (N=1,862).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$