Local Immigration Enforcement and Local Economies*

SARAH BOHN and ROBERT SANTILLANO

We examine the impacts of a locally enforced immigration program—287(g)—on private employer reports to the Quarterly Census of Employment and Wages. Using contiguous-county pairs to account for time-varying local economic shocks, we identify impacts on immigrant-intensive industries that are robust to prepolicy time trends, implementation timing, and the exclusion of pairs with large prepolicy differences. Reported employment was 4 percent higher in manufacturing, but 7–10 percent lower in administrative services. These results are consistent with adverse labor-supply shocks, and, to a lesser extent, a decline in labor demand for locally produced goods and services.

Introduction

Comprehensive immigration reform remains a hotly debated political issue in the United States. Since the last immigration overhaul in 1986, the unauthorized population has grown from about three million in 1990 to eleven million in 2013 (Hoefer, Rytina, and Baker 2012; Passel and Cohn 2011; Warren 2014). As in previous policy-reform attempts, debate tends to center around the contentious issues of legalization for unauthorized immigrants and preventing future unauthorized flows.

In this paper, we study the economic impacts of a federal law established by the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) of 1996 aimed at deterring unauthorized immigrants from settling in U.S. localities. The program, referred to as 287(g), allows local authorities to enforce certain immigration laws, such as investigating the documented status

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of individuals during routine police activities. Since 2006, the program has led to more than 1500 participating officers being trained, and more than 300,000 individuals being identified for potential deportation (Immigration and Customs Enforcement 2013). It has long been recognized that economic opportunity is the primary draw for unauthorized immigrants to the United States, but the local economic impacts of their removal are not well understood—despite often being the centerpiece of the motivation. Here we provide direct impact estimates of this type of local enforcement policy on measures of economic activity.

In 2012, the Department of Homeland Security began a phaseout of the 287(g) program by no longer renewing agreements as they expire. Through 2009, the endpoint of our analysis period, more than seventy law enforcement agencies had entered into 287(g) agreements, and as of August 2015, at least thirty agreements were still in effect. Despite these laws having a limited lifespan, they represented an important shift of enforcement activities from federal to local authorities, which is one of the considerations of future reform efforts. For this reason, we believe that identifying the economic impacts of 287(g) can inform the debate on immigration enforcement policy, which continues unabated.

Although the impacts of 287(g) agreements have been the topic of previous studies, their economic consequences have not been documented precisely or comprehensively. Previous research finds mixed results related to demographic responses, with one study showing initial declines of the Mexican immigrant population (Parrado 2012) that go away once large outliers are excluded, others showing more robust population declines of immigrants (O'Neil 2011; Watson 2013), and one identifying both—depending on the location (Capps et al. 2011). Parrado (2012) further finds no relationship between 287(g) and employment for native workers at the metropolitan-area level, while Pham and Van (2010) combine 287(g) with other local laws and show that employment falls when restrictive immigration laws are implemented. Although these studies begin to clarify the role that local immigration laws have played on local economies, those that focus on economic outcomes do not explicitly control for local economic changes that could have dramatically influenced their results.

The main identification challenge of estimating the impacts of 287(g) is nonrandom implementation across localities. For example, passage of local immigration laws could be related to poor economic conditions and high unemployment. This concern is heightened by the fact that the broad economic crisis that started in late 2007 overlaps with the period when many 287(g) agreements were made. The key identifying assumption in our approach is that economic shocks are shared across localities in geographically close regions.

In particular, neighboring localities often have integrated markets, but in many cases do not share the same immigration policies. Thus, neighboring localities, which we define to be counties, can act as comparison groups for one another. Evidence for this assumption has been shown through geographic variation in local economic shocks that diffuse through neighboring counties (Fogli, Hill, and Perri 2012). By matching counties to their neighbors to create contiguous-county pairs, our empirical strategy allows us to estimate an adjusted difference-in-difference model that additionally controls for time-varying local economic shocks. By using this estimation strategy, exploiting the variation in the timing of 287(g) implementation, and controlling for other locally enacted immigration measures, we aim to isolate the economic consequences of implementing 287(g).[1]

To implement this strategy, we create a county-level panel data set covering 2004–2010. Our outcome data come from industry-level reports on employment and wages from private employers to the Quarterly Census of Employment and Wages (QCEW). Our treatment variables are determined by counties that implemented 287(g), while controlling for other locally implemented immigration policy over our period of interest. Before interpreting the findings, we are careful to consider the advantages and disadvantages of using the QCEW—an aggregated employer-reported measure—to address questions related to the employment of undocumented workers. We then explore the impact of the 287(g) program on differences in employer-reported economic activity overall and across industries where we expect undocumented workers to be overrepresented.

We find little evidence that implementation of 287(g) impacted the overall scale of labor-market activity, but do find important differences across industries. Specifically, after 287(g) was implemented, employer-reported employment was 7–10 percent lower in administrative services (e.g., landscaping, janitorial work, and maintenance), relative to bordering counties that did not implement 287(g). Employment fell in construction and accommodation and food-service industries, also big employers of immigrants, but our estimates are imprecise. At the same time, employment in manufacturing increased 4 percent. The difference in the sign of these impacts raises important questions about the labor supply and demand responses to 287(g) implementation. We argue that the primary response is likely to be an adverse supply shock, for which we find some evidence in three of the four immigrant-intensive industries. However, a priori labor demand may also respond, but in either direction —a negative shock due to declines in local goods and service demand or a

[1] This strategy was partially motivated by Dube, Lester, and Reich (2010), who used a similar approach to study the impacts of state-wide minimum wage laws on employment and wages of low-skilled workers.

positive shock due to substituting away from undocumented and unreported immigrant workers toward documented, reported workers. We find some support for this latter mechanism in our results from the manufacturing sector, which is reasonable to expect when relying on employment data reported for official purposes. This is also in line with previous research that has found that employers substitute legal workers for undocumented workers in response to local immigration enforcement (Orrenius and Zavodny 2009).

Although the different findings across industries remains an open puzzle, we conclude that 287(g) had real—but targeted—impacts on local economies. Our results are robust to a number of identification concerns, including county-level time trends, the timing of implementation, and to excluding contiguous-county pairs with large pre-intervention differences. Although the use of 287(g) agreements has faded in recent years, this research is relevant for future immigration law that seeks to use local enforcement of federal laws. However, further research is required to improve our understanding of the exact mechanisms and distributional consequences of similar laws that are enforced at the local level.

Background

Federal immigration laws over the past 30 years have been unsuccessful at curbing growth of the undocumented population. The Immigration Reform and Control Act (IRCA) of 1986 was the last comprehensive overhaul of immigration policy in the United States, and since its passage the unauthorized population has grown nearly monotonically to eleven million in 2013 (Warren 2014). IRCA introduced sanctions intended to curb demand for the employment of unauthorized workers. Importantly, this change meant that federal authorities were tasked with identifying employers who hire undocumented workers and identifying undocumented individuals for deportation. Although sanctions were never strictly enforced, Hispanic wages were immediately depressed following IRCA's passage, but this depression was short-lived (Bansak and Raphael 2001). It is unclear why the depression in wages was not sustained over a longer period, but it is possible that employers learned that sanctions from the federal government were not a credible threat.

More recent federal legislation has allowed for immigration enforcement to be partially shifted from federal to local authorities. The IIRIRA of 1996 established two immigration control programs that could be enforced locally: (1) the E-Verify program for employers to directly check the documented status of newly hired employees (Rosenblum 2011), and (2) the 287(g) program to provide local law enforcement officers the authority to check the documented status of individuals encountered during policing activities (Immigration and Customs

Enforcement 2013). Both of these programs were established as voluntary, but they represent important shifts in federal law that potentially expands the jurisdiction of immigration enforcement to local authorities.

In the absence of new comprehensive legislation, many state and local governments made use of these local enforcement programs, as well as implementing their own laws related to immigrants. These local responses have been tied to rapid demographic changes, poor economic performance, increased political polarization, and general public concern after 9/11 (Hopkins 2010; Orrenius and Zavodny 2009, 2012; Ramakrishnan and Wong 2010). In state and local governments, more than 134 laws were passed between 2005 and 2009 specifically related to the employment of or enforcement against unauthorized immigrants.[2] Some of these were based on implementing available federal programs from IIRIRA. For example, the Legal Arizona Workers Act (LAWA) of 2007 required all employers in the state to use E-Verify and led to a decrease in the share of noncitizen Hispanics in the state (Bohn, Lofstrom, and Raphael 2014). In addition, more than seventy-three law enforcement agencies entered into 287(g) agreements.[3] While varied in scale and scope, nearly all of these actions can be characterized as punitive and are aimed at deterring unauthorized immigration.

This study focuses on the local economic impacts of 287(g) agreements at the county level. We focus on 287(g) agreements because they are more homogeneous than the wide variety of other immigration policies implemented at the state and local level. Although there are two types of 287(g) agreements —jail and task force—Capps et al. (2011) conclude that difference in implementation between the two types of programs were not well characterized.[4] It should be noted that the jail 287(g) program is similar to two additional immigration enforcement programs: Secure Communities and the Criminal Alien Program. Both are based in jails and are aimed at determining the immigration status of serious offenders in the system. Although these are related, the analysis we propose allows us to identify the impacts of 287(g) alone.[5] In 2009, the

[2] We identified seventy-five laws passed in local governments based on a database provided by Kevin O'Neil (2011). In state governments, fifty-nine laws were passed according to our analysis of the National Conference of State Legislatures (2011) database from 2005–2011.

[3] A list of active agreements is available on the Immigration and Customs Enforcement website (ICE 2013). The list of fifty-five agreements we include in this study were all implemented between 2005 and 2009 in counties or within a county's boundaries.

[4] This conclusion is consistent with our findings when we separately estimate the impacts for each type of program. Based on this, we present an analysis of both types of 287(g) combined.

[5] For Secure Communities, 287(g) mostly predates these programs, which began in 2008 and accelerated in number only by 2010, which is largely after our primary analysis period. The Criminal Alien Program has been in place since 2006, and is more in line with the timing of 287(g) adoption. However, CAP is implemented nationally, while we exploit geographic variation in 287(g) to distinguish its effects.

average locality with a 287(g) agreement had 16 trained officers and an average of twenty-five arrests per year (Vaughan and Edwards 2009).

Conceptual Framework

We are interested in the effect of 287(g) on overall labor-market activity, conceptualized in a simple labor supply and demand framework. It is important to note that 287(g) policies target undocumented immigrants directly, rather than their employers. So, unlike programs designed to discourage the hiring of undocumented workers (like E-Verify) by raising employer costs, 287(g) is likely to impact labor demand in an indirect manner, if at all. For this reason, we do not expect a relocation response of businesses if the work is still conducted locally. This logic would then point to shifts in labor supply as the primary impact of 287(g). Indeed, as mentioned earlier, other studies have documented a population response to 287(g). All else equal, a migration response would be reflected in a decline in labor supply, yielding a drop in county employment and increase in the wage rate. We would expect to observe such impacts in industries employing a large share of undocumented workers. Further, the labor supply shock may be smaller than an overall population response, since undocumented workers may accept the trade-off of apprehension versus the reward of plentiful or good-paying job opportunities in a county with a 287(g) program.

Even if we assume the primary mechanism for labor market adjustment in response to 287(g) is through labor supply, there are at least two potential labor-demand responses that must be taken into account. First, a population outflow from a county, if sizeable, could reduce demand for locally produced goods and services, thereby decreasing labor demand. This is more likely to occur in service-oriented industries. And with shocks to both labor supply and demand possible, the net impact of 287(g) on overall employment and wages becomes an empirical question. Second, one must take into account the substitutability of workers. Immigrant workers are not perfect substitutes with native-born workers, and in fact are likely complements in some production (Peri and Sparber 2009). The outmigration of undocumented immigrants could trigger an adverse shock to overall labor demand if the degree of complementarity is high and sufficient substitutable labor is not available.

Related to the issue of how undocumented and documented workers are interdependent in the production process is how their work is reported or regulated. By definition, undocumented work largely happens outside of formal legal controls, and is thus also excluded from some officially reported statistics. This complicates the framework for assessing the impacts of a policy like 287(g) because undocumented employment may not be reflected in some statistics to begin with; the QCEW data we rely on (described below) suffers from this bias. So, for example, if firms employ unreported–undocumented workers and then switch to reported–documented workers, we would observe an increase in employment. If we assume that the wage rates do not suffer the same reporting bias (valid in the case that undocumented and documented workers at the firm are reasonably substitutable), the wage effect of the labor supply and demand shocks concomitant to changes in reporting is indeterminate. There is not much detailed evidence on the scale or nature of unreported work in the United States that can help us isolate this potential mechanism. Bohn and Owens (2012) find evidence of unreported work among immigrants in two narrowly defined industries: residential construction and landscaping. Evidence on other industries that are employers of many unauthorized immigrants is unfortunately lacking. Therefore, we look for patterns of labor-market effects across industries, recognizing that the share of unauthorized immigrants, the nature of work, and the reporting of work can vary substantially across sectors.

In sum, the standard labor market mechanisms operating in response to 287(g) policies may yield employment and wage outcomes in many directions. Our goal is to empirically estimate the net impact of supply and demand shocks rather than isolating each potential mechanism. However, in our discussion below, we revisit the framework outlined here for clues as the nature of the adjustment.

Data

Our analysis focuses on county-level economic outcomes and identification of the impacts is derived from a difference-in-difference strategy using contiguous-county pairs. Here we discuss the data we use to perform the analysis along with advantages and limitations.

State and local immigration efforts. The date and jurisdiction of all 287(g) agreements were publicly available and obtained from the Department of Homeland Security. While we refer to “counties” as the localities studied, 287 (g) agreements may be made by agencies at the city or state level. We apply an adjustment factor to agreements at subcounty levels using the fraction of the county population covered.[6] In the case of 287(g) agreements made with

[6] This is done using geographic correspondence files from the Missouri Census Data Center. By doing this, whenever a policy is not covered by the entire county, we create a dosage indicator ranging from 0 to 1 that represents the fraction of the population that lived in the covered area based on the area’s population in the year 2000. Eighty percent of treated counties have a dosage of 1, and the average dosage is 85 percent.

state-level law enforcement, we control for the timing of such policy, but do not include it as the policy change of interest, because our focus is on local economic impacts. The first county-level 287(g) policy in our study was implemented in 2005 and the last in 2009.

As mentioned above, there are a variety of other policy levers utilized by localities to curb illegal immigration that we must also control for. We do so by using a database that is—to the best of our knowledge—comprehensive of local and state immigration-related legislation over 2005–2009. State immigration laws were collected using reports from the National Conference of State Legislatures (NCSL), which has identified all state-level immigration laws since 2005. We use the NCSL summaries of such legislation, when available, or the text of state legislation, to determine which policies fit our criterion of being employment- or enforcement-related. Local laws are more numerous and more varied than state laws, and thus are very difficult to compile. For these laws, we take advantage of a database shared by Kevin O'Neil (2011).[7] As with coverage of 287(g) agreements, when local legislation did not pertain to an entire county, we apply a population-based adjustment factor to these policy variables.

Our final analysis sample includes the 274 counties that either entered into a 287(g) agreement or bordered a county implementing an agreement between the years 2005–2009. We choose these dates to be consistent with the database of other immigration policy we have available, even though a handful of 287 (g) agreements were implemented before or after this period. We also restrict our sample to county pairs with full data from the QCEW from 2004–2010, a full year before through a full year after the policy sample period, so that we can test leads and lags of the policy changes. The prevalence of these laws in the 274 counties of interest is given in Table 1. Fifty-five counties (20 percent) implemented 287(g) locally during this time, but a good number of additional laws were passed or implemented as well. For example, 71 percent of the counties were in a state with an immigration law. Using the fifty-five local 287(g) implementers, a total of 292 contiguous-county pairs were formed that experienced differential implementation of 287(g) from 2005 through 2010. Few agreements were made from 2005 through 2006, but nearly a third came in each of 2007 and 2008 before leveling off.

[7] O'Neil used a variety of sources to identify local laws. Primary identification came from searches of U.S. newspapers and lists that were compiled from immigrant advocacy organizations, such as Fair Immigration Reform Movement (FIRM), Latino Justice PRLDEF, American Civil Liberties Union (ACLU), Mexican American Legal Defense and Education Fund (MALDEF), the Immigration Reform Law Institute, U.S. English, and Pro English.

TABLE 1

Summary Statistics

Panel A: Local Immigration Policies	Counties	Share w/Policy	First Policy
Local 287(g)	274	.20	2005, Q1
Local employment/enforcement law	274	.09	2005, Q2
State 287(g)	274	.26	2004, Q1
State employment/enforcement law	274	.70	2004, Q2

Panel B: Monthly Employment ('000), by Local 287(g) Implementation	County-Quarters	Mean	Std. Dev.
Never 287(g)	6132	91	195
Ever 287(g)	1540	340	568
All	7672	141	324

Panel C: Weekly Wages, by Local 287(g) Implementation	County-Quarters	Mean	Std. Dev.
Never 287(g)	6132	672	210
Ever 287(g)	1540	800	229
All	7672	698	220

Sources: 287(g) agreements from Immigration and Customs Enforcement, local employment, and enforcement laws from Kevin O'Neil (see O'Neil 2011), and state laws from National Conference of State Legislatures (2005–2010). Employment and quarterly wage data come from the QCEW.

Notes: The analysis sample consists of counties initiating a local 287(g) policy before 2009 along with their bordering counties. Counties were further limited to those with complete reporting of QCEW data from Q1, 2004 through Q4, 2010, leading to a balanced sample of 274 counties over 28 quarters. Employment and wage data are based on all private sector employers reporting to the QCEW and represent monthly averages within each quarter. Differences across never implementers and ever implementers are statistically significant at a 99-percent level on reported employment and wages.

Quarterly Census of Employment and Wages. Our outcome data come from the QCEW, which provides distinct advantages for addressing our research questions. The QCEW is based on employers' quarterly reporting to states of total employment and wages paid for the purposes of the Unemployment Insurance program.[8] Being a census, these reports aim to capture the universe of private-sector employers at a small level of geography (county), and at a high frequency (quarter). Because these reports contain the most comprehensive reporting by employers in the country, the list of reporting employers is used as the sample frame from which the Bureau of Labor Statistics draws national surveys to characterize the U.S. labor market. For this reason, we have high confidence in the QCEW's ability to provide the most accurate picture of formally reported jobs and wages. Further, the high-frequency reporting

[8] It is important to note that the QCEW does not accurately capture the agricultural sector, an industry where undocumented workers are overrepresented. Although this precludes an analysis of impacts for this industry, we are still able to study other immigrant-intensive industries.

at a small geographic level allow us to precisely attribute the local laws to their appropriate area of coverage and time.

As outcomes, we use average monthly employment and average weekly wages paid to employees by private-sector employers within any given quarter. Employers actually report on employment by month, but we use the average to provide a more natural quarterly measure. Employers also report total wages paid, but we use average weekly wages for a more direct measure of marginal factor productivity. When analyzing these outcomes, we start by studying the entire private sector before focusing on immigrant-relevant industries (using 2-digit North American Industry Classification System [NAICS] codes).

The QCEW has some disadvantages that need to be considered when interpreting results. The QCEW is best understood as a measure of formally reported workers. Therefore, unauthorized workers should only be incorporated in the QCEW when working under false pretenses. As discussed above, we are unable to distinguish the rate at which they show up and how that varies across industries (Bohn and Owens 2012). Further, self-employed, agriculture, informal, and sometimes contracting workers are excluded from the QCEW.[9] Because unauthorized immigrants are more likely to be working in less formal arrangements, the QCEW is likely less representative of unauthorized workers relative to authorized workers, but data limitations preclude our ability to know for sure. Finally, employment in the QCEW does not represent the number of unique employees in a county, but more closely reflects the number of jobs that exist. To the extent that this leads to differences across industries in how work is reported, such as for part-time work, is not completely known—although we return to this in the results below.

An immediate challenge for this study is identifying industries relevant to the immigrant population. One drawback of the QCEW is that there is no information on individual workers, so these data cannot be used to control for individual characteristics or to determine industries that are particularly relevant for undocumented workers. Passel and Cohn (2009) estimate that as of 2008, the industries that employed the largest share of unauthorized immigrants were: construction (21.2 percent); leisure and hospitality (16.7 percent), which includes accommodation and food services; manufacturing (13.4 percent); and professional, business, and other services (13.3 percent), which

[9] The laws on reporting of contract work to the QCEW vary by state. This is relevant for undocumented workers if some firms are contracting services to avoid hiring undocumented workers directly. Upon creation of the NAICS industrial codes, a specific code was created for professional employer organizations, which are firms that provide laborers under contract. Thirty-four states require these agencies to report their workers to the QCEW by the industry of the contract work, in which case they would be appropriately assigned to industry in the QCEW (Dey, Houseman, and Polivka 2010). This is true for 80 percent of the counties in our analysis sample. Our main results were robust when limiting the analysis to counties from these states.

includes administrative services such as janitorial work, landscaping, and maintenance. Based on this, we focus on the following 2-digit NAICS industries: construction, manufacturing, administrative services, and accommodation and food services.[10]

One final data concern with the QCEW is that reporting is censored when there are few firms. However, our checks confirm that this is not a significant issue for the industries of interest in the counties included in our analysis sample. In the cases where a county's measures are censored for any quarter, we exclude them throughout, leaving a balanced sample of counties from quarter 1 in 2004 to quarter 4 in 2010.

Descriptive statistics on outcomes of interest by local 287(g) implementation are provided in Table 1. Comparing the rows, counties that have implemented a 287(g) policy employed nearly 250,000 more workers in an average month and earned nearly $130 more per week, on average. Given the statistical and economic magnitude of these differences, below we discuss additional exercises used to assess the sensitivity of our findings to pre-intervention differences.

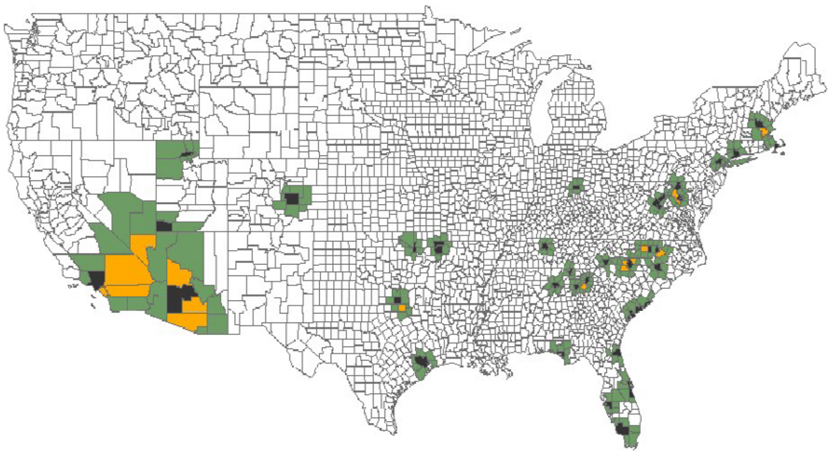
Contiguous-county pairs. We identify contiguous counties using ArcGIS software. Two counties are considered contiguous if they share a land border or, if divided by a waterway, share a bridge. Each contiguous-county pair where at least one county has implemented a local 287(g) agreement from 2005 through 2009 is included in our analysis. Overall, we identified a total of 292 contiguous-county pairs that experienced differential exposure to local 287 (g) enforcement. Although the first pairs were created in 2005, approximately a third of these pairs were created in each of 2007 and 2008, and stopped being created after 2009. Given our focus on pairs, counties in multiple pairs will be duplicated in our analysis. We describe the method used to control for precision bias due to this duplication in the empirical strategy section below.

Figure 1 presents the geographic spread of 287(g) agreements across the United States along with their contiguous-county pairs. There are three groups represented in the figure: "first-mover" counties that implemented 287(g) (treated), neighboring counties that subsequently adopted 287(g) (sometimes treated), and neighboring counties that never adopted 287(g) (comparison). The figure reveals that 287(g) adopters are spread throughout the country,

[10] For completeness, we estimated impacts for professional services as well, but exclude it from the reporting below even though it was identified by Passel and Cohn's (2009) larger aggregation of "professional and administrative services." This is because it is a white-collar industry and would only serve as a robustness check where we would not expect to find any impacts—a hypothesis confirmed by the analysis.

FIGURE 1

CONTIGUOUS-COUNTY PAIRS WITH VARYING EXPOSURE TO LOCAL 287(G) AGREEMENTS



NOTES: Authors' mapping of data from Immigration and Customs Enforcement (2013). Counties are shaded by colors to represent the relative timing of their 287(g) agreements, or proximity to such a county. Black represents those counties that were the first to implement a 287(g) policy among all of their contiguous-county pairs. Orange represents counties that subsequently adopted 287(g) policies. Green represents counties that have never implemented a 287(g) policy, but are bordering a county that has. Unshaded counties represent the remaining counties in the United States.

particularly in the southern half, though not exclusively. Large areas of Arizona and southern California appear to be in either treated or comparison counties, but this is partly due to the fact that the geographic size of counties in these areas tend to be larger.

Empirical Strategy

We run a number of difference-in-difference models with a focus on a preferred specification that allows us to control for time-varying shocks shared by a pair of contiguous counties. We start by first running a traditional difference-in-difference (DD) specification:

$$\ln(\text{Outcome}_{ct}) = \alpha + \beta \times 287\text{g}_{ct} + \theta \times \text{ImmigrationLaws}_{ct} + \varphi_c + \tau_t + \varepsilon_{ct}, \tag{1}$$

where Outcome_{ct} is either average monthly employment or average weekly wages in county c, at time t. $\text{ImmigrationLaws}_{ct}$ represents a vector of dosage-indicator controls for the three non-287g immigration policies that are also implemented in each county: (1) local employment or enforcement ordinance, (2) state 287(g) agreement, or (3) state employment or enforcement policy. φ_c is a county-level fixed effect, and τ_t is a quarter fixed effect. We augment this specification by taking advantage of our contiguous-county database to estimate the following regression:

$$\ln\left(\text{Outcome}_{cpt}\right) = \alpha' + \beta' \times 287\text{g}_{cpt} + \theta' \times \text{ImmigrationLaws}_{cpt} + \varphi'_c + \tau'_{pt} + \varepsilon'_{cpt}, \tag{2}$$

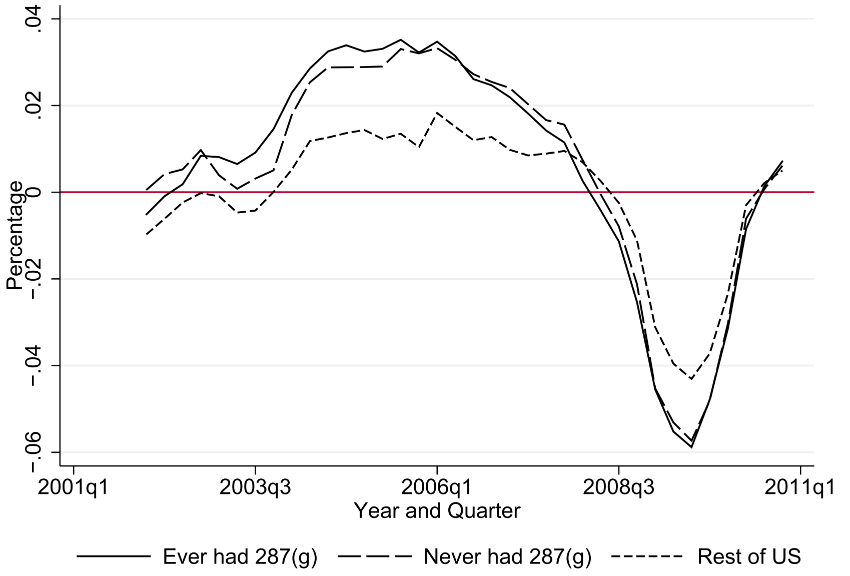
for county-pair p. The primary difference here is that τ'_{pt} is now a time-varying contiguous-county-pair fixed effect, and a county re-enters the specification each time it is in a pair. For the reasons stated above, we believe this is a critical advantage given the economic shocks experienced during the Great Recession.

It is also common to consider the inclusion of county-specific time trends in both of the above specifications, but making the parametric assumption of a linear time trend may not provide an appropriate specification given the volatility of the economy. Further, if the impacts are dynamic, then assuming a linear trend can bias the coefficient of interest, β (Wolfers 2006). Although (2) is our preferred specification, to err on the side of caution, we estimate two additional models that de-trend each outcome according to a linear time trend established in the pre-intervention periods. By de-trending the outcome before estimating models (1) and (2), we are controlling for county-specific trends in addition to the remaining controls. Results for all four models are presented below.

The analysis sample includes counties that have implemented a 287(g) policy, as well as all bordering counties. Bordering counties, we argue, provide the best counterfactual for treated counties in that they are more likely to share local economic shocks. To provide evidence for this, we plotted yearly changes in employment in each quarter for counties that have ever had a 287(g) law, contiguous counties that have never had a 287(g) law, as well as the remaining counties in the United States in Figure 2. The effects of the Great Recession are clearly seen in this figure, but what is also clear is that overall employment trends are much more similar amongst the bordering counties (i.e., the comparison sample) relative to the rest of the United States. Our strategy to identify the impacts of 287(g) is appropriate as long as time-varying economic shocks that relate to the presence of 287(g) are shared by each pair.

FIGURE 2

PERCENTAGE CHANGE IN EMPLOYMENT FROM PREVIOUS QUARTER



SOURCE: Authors' analysis of Quarterly Census of Employment and Wages for all industries.
Notes: Percentage change in employment is based on yearly change from quarter to the same quarter in order to remove seasonal variation.

One potential concern with this strategy is that neighboring counties may be affected through population spillovers. If immigrants leave a 287(g) county to live (or work) in neighboring counties, then the labor supply of the treated county may appear artificially low relative to its neighbors. Two facts help to assuage this concern. First, targeted immigrants would be leaving from a single county potentially into multiple surrounding counties; so in each bordering county the impacts are likely to be dispersed. Second, although Watson (2013) finds demographic impacts from the 287(g) program, the Capps et al. (2011) case studies in seven sites found that neighboring counties experienced no population change coincident with the adoption of 287(g), suggesting immigrants are not simply moving to bordering counties. These types of spillover effects were also not found in other well-known local immigration enforcement settings, such as LAWA (Bohn, Lofstrom, and Raphael 2014).

Another potential concern involves large baseline differences for each pair, making their comparisons less credible. As noted above, counties with a 287(g) policy were much larger than those not implementing a policy. Although using logs and the standard difference-in-difference strategy ameliorates some of these concerns, the magnitude of these differences warrants further caution. We address this by performing two robustness checks. In the first, we calculate the difference in outcomes between each treated and nontreated pair of counties in 2003—before any of our sample counties implemented the law. Next, we re-estimate our primary specification after sequentially excluding those pairs with the largest absolute baseline difference in groups of twenty-five. Even after excluding one hundred pairs of counties, we found our results to be generally robust. In the second check, we performed an analogous exercise, but with respect to the share of the reported workforce in each industry. Specifically, we calculated the share of workers reported in each industry, took the difference between treated and nontreated counties within each pair, and, again, estimated our primary specification while excluding those with the largest difference. Again, we found our results to be robust after excluding as many as one hundred county pairs.

Last, to better understand the timing of the impacts, we estimate a version of equation (2) that replaces a single β' estimate with an impact for each of the eight quarters preceding the agreement (leads) and eight quarters after the agreement (lags). There are two purposes for this. First, estimating impacts before the policy agreement acts as a falsification test to provide evidence that the strategy is working as intended. Specifically, we would not expect there to be impacts before the policy change. Second, by focusing on an impact by quarter after the policy is implemented, we will be able to assess whether or not the impacts were dynamic.

Correction for repeated county observations. We now revisit the issue of correcting for repeated county observations based on each of its pairs for equation (2). There are two primary issues that need to be addressed: (1) counties with more pairs will have larger impacts on coefficient estimates, and (2) standard errors are biased downward due to additional observations whose error terms are correlated. To deal with the first concern, we weight our estimates by the inverse of the number of its county pairs. This will effectively give each county equal weight when estimating the impacts.[11] To deal with the second concern, we performed a multi-way cluster correction suggested by Cameron, Gelbach, and Miller (2011). Specifically, following the guidance of Bertrand, Duflo, and Mullainathan (2004), given that we are including a set of

[11] Estimating the models with no weights did not alter the results, but we chose this as our primary specification to assuage any concerns.

pair-time fixed effects in a multiperiod panel, we are clustering our standard errors by pair in order to further safeguard against serial correlation shared within a pair. Second, because counties can be in multiple pairs, it is also necessary to cluster the error terms of each county–time observation because they are related across the different pairs. Because these two clusters are nonnested, it is necessary to adjust the standard errors using a multiway cluster correction (Cameron, Gelbach, and Miller 2011).[12]

Results

Here we present results from each of the separate models including a naïve model that simply regresses each outcome on a constant and an indicator for 287(g) implementation. Table 2 presents impact estimates of local 287 (g) on employment, and Table 3 presents analogous results on weekly wages. In each table, Panel A presents estimates on all private sector industries, while the remaining panels present results for specific industries of interest: construction, manufacturing, administrative services, and accommodation and food services. Column (1) presents the naïve regression, column (2) presents a traditional DD specification from equation (1), column (3) presents a traditional DD with a de-trended outcome (to control for a pre-intervention county-trend), column (4) presents our preferred contiguous-county DD specification from equation (2), and column (5) presents our preferred specification, again, with the de-trended outcome to reflect a pre-intervention county trend.

Impacts for all private-sector industries. When including all industries, Panel A shows no impact on aggregate outcomes reported by private-sector employers in either table. The naïve regressions in column (1) shows that counties that implemented 287(g) programs had higher employment and wages, on average, than counties with no such policy, but these differences disappear in even the most basic DD strategy. The remainder of the results in Panel A shows that regardless of how we control for county-specific and time factors, there is no discernible difference in county-level labor-market outcomes due to 287(g). This is not surprising given that a large local

[12] This correction was implemented in other studies that have used this strategy—such as Dube, Lester, and Reich (2010)—and can be understood as an application of the inclusion–exclusion principle. In this case, multiway clustering is performed by adding the two variance–covariance (VCV) matrices obtained from estimating the models based on one clustering at a time and then subtracting the VCV matrix obtained when clustering on the intersection of the two cluster directions.

TABLE 2

Impact of Local 287(g) Agreements on Private-Sector Employment

	Dependent Variable: Log Average Monthly Employment				
	(1)	(2)	(3)	(4)	(5)
Panel A: All Industries					
287(g) dosage	1.607***	–0.006	–0.015	0.001	0.007
Standard error	(0.061)	(0.009)	(0.012)	(0.009)	(0.009)
p-value	[0.000]	[0.483]	[0.192]	[0.909]	[0.452]
Panel B: Construction (NAICS 23)					
287(g) dosage	1.419***	–0.050**	–0.094**	–0.020	0.002
Standard error	(0.062)	(0.023)	(0.039)	(0.016)	(0.026)
p-value	[0.000]	[0.034]	[0.016]	[0.203]	[0.928]
Panel C: Manufacturing (NAICS 31-33)					
287(g) dosage	1.285***	0.020	0.015	0.042**	0.042*
Standard error	(0.062)	(0.017)	(0.024)	(0.020	(0.024)
p-value	[0.000]	[0.237]	[0.529]	[0.033]	[0.086]
Panel D: Administrative Services (NAICS 56)					
287(g) dosage	1.379***	–0.066**	–0.107**	–0.100**	–0.071
Standard error	(0.070)	(0.030)	(0.047)	(0.030)	(0.061)
p-value	[0.000]	[0.028]	[0.025]	[0.001]	[0.245]
Panel E: Accommodation and Food Services (NAICS 72)					
287(g) dosage	1.304***	–0.015	–0.005	–0.021	0.007
Standard error	(0.060)	(0.013)	(0.013)	(0.014)	(0.016)
p-value	[0.000]	[0.250]	[0.715]	[0.149]	[0.655]
County FE		X	X	X	X
Quarter FE		X	X		
County trend			X		X
Pair–quarter FE				X	X
N: Counties	187–274	187–274	187–274	187–274	187–274
N: Pairs	NA	NA	NA	192–292	192–292
N: 287(g) counties	50–55	50–55	50–55	50–55	50–55
R^2	0.07–0.10	0.99	0.99	0.99	0.99

Source: QCEW and local immigration laws. *, **, *** = 10%, 5%, 1% levels of statistical significance.

Notes: Each panel and column presents results from separate regressions on a balanced sample of counties that includes 28 quarters from Q1 of 2004 through Q4 of 2010. All regressions include dosage indicators for other local immigration policies including: (1) local ordinances covering employment or enforcement of undocumented individuals, (2) state 287(g) agreements, and (3) state laws covering employment or enforcement of undocumented individuals. Regressions in columns (1), (2), and (3) provide cluster-robust standard errors at the county level. Regressions in columns (4) and (5) include multiway cluster-robust standard errors from both county–time and pair clusters. FE, fixed effect; NA, not applicable.

unauthorized population is still a small share of the labor force. Further, even if these policies had population consequences, it is unlikely all unauthorized immigrants would have responded.

Impacts by industries. We next estimate impacts for the immigrant-intensive industries identified by Passel and Cohn (2009). The pattern of results varies substantially across the four industries we study, and is shown in Table 2 (employment) and Table 3 (wages), Panels B–E. We

TABLE 3

Impact of Local 287(g) Agreements on Private-Sector Weekly Wages

	Dependent Variable: Log Average Weekly Wages				
	(1)	(2)	(3)	(4)	(5)
Panel A: All Industries					
287(g) dosage	0.154***	–0.002	–0.008	0.000	–0.004
Standard error	(0.010)	(0.005)	(0.007)	(0.006)	(0.006)
p-value	[0.000]	[0.728]	[0.232]	[0.932]	[0.493]
Panel B: Construction (NAICS 23)					
287(g) dosage	0.114***	–0.020**	–0.019*	–0.023**	–0.008
Standard error	(0.009)	(0.009)	(0.010)	(0.009)	(0.011)
p-value	[0.000]	[0.031]	[0.046]	[0.010]	[0.455]
Panel C: Manufacturing (NAICS 31-33)					
287(g) dosage	0.123***	–0.001	–0.002	–0.001	–0.004
Standard error	(0.011)	(0.008)	(0.011)	(0.009)	(0.010)
p-value	[0.000]	[0.904]	[0.842]	[0.944]	[0.692]
Panel D: Administrative Services (NAICS 56)					
287(g) dosage	0.068***	0.011	0.032*	0.014	0.047*
Standard error	(0.010)	(0.012)	(0.018)	(0.016)	(0.026)
p-value	[0.000]	[0.369]	[0.079]	[0.384]	[0.071]
Panel E: Accommodation and Food Services (NAICS 72)					
287(g) dosage	0.124***	–0.006	–0.016*	–0.009	–0.008
Standard error	(0.008)	(0.006)	(0.008)	(0.007)	(0.007)
p-value	[0.000]	[0.317]	[0.055]	[0.166]	[0.306]
County FE		X	X	X	X
Quarter FE		X	X		
County trend			X		X
Pair–quarter FE				X	X
N: Counties	187–274	187–274	187–274	187–274	187–274
N: Pairs	NA	NA	NA	192–292	192–292
N: 287(g) counties	50–55	50–55	50–55	50–55	50–55
R^2	0.01–0.03	0.90–0.97	0.93–0.98	0.98–0.99	0.99

Source: QCEW and local immigration laws. *, **, *** = 10%, 5%, 1% levels of statistical significance.

Notes: Each panel and column presents results from separate regressions on a balanced sample of counties that includes 28 quarters from Q1 of 2004 through Q4 of 2010. All regressions include dosage indicators for other local immigration policies including: (1) local ordinances covering employment or enforcement of undocumented individuals, (2) state 287(g) agreements, and (3) state laws covering employment or enforcement of undocumented individuals. Regressions in columns (1), (2), and (3) provide cluster-robust standard errors at the county. Regressions in columns (4) and (5) include multiway cluster-robust standard errors from both county–time and pair clusters. FE, fixed effect; NA, not applicable.

detect negative employment and wage effects in the construction and accommodation and food service industries, but our estimates are imprecise, particularly in our preferred specification in Column (4). At the same time, these negative impacts are consistent with adverse labor-supply and labor-demand shocks.

In the administrative services industry (Panels D), we find negative employment effects and positive wage effects, with statistical significance and robustness primarily on the employment side. These estimates suggest that following the 287(g) agreement, employment was 7–10 percent lower in administrative services and wages were about 1–4 percent higher. These results are consistent with an adverse labor-supply shock in response to 287(g) implementation.

Last, in the manufacturing sector (Panels C), we find positive employment effects, which are significant in our preferred model, and remain even when controlling for pre-intervention county trends (column 5). These estimates suggest that manufacturing employment rose 4 percent in 287(g) counties after implementation relative to neighboring counties. We find consistently very small and statistically insignificant wage effects. These estimates suggest a net increase in labor demand following 287(g) implementation and are robust to inclusion of a linear county-level time trend. A priori, an increase in labor demand is the least likely response to 287(g) given previous evidence on the population response. We thus find the most likely explanation for increased employment to be an increase in officially reported workers in this industry, as employers shift away from undocumented workers. This would be consistent with other research that has focused on local immigration enforcement on employers (Orrenius and Zavodny 2009). However, data limitations preclude us from checking this more directly.

The impacts on employment for manufacturing and administrative services are sizable, but their opposing directions raise questions about distributional consequences. Specifically, these impacts may reflect both direct and indirect responses to population shifts of workers influenced by 287(g) policing policy. Part of this may be explained by industry practices in how undocumented workers are included in official reports where reporting differences may exist across industries (Bohn and Owens 2012). Further, in separate checks, we do detect differences in reporting across industries. Specifically, we studied the ratio of household-reported employment in an industry from the 2005–2007 pooled American Community Survey (ACS) relative to the employer-reported number of jobs in the QCEW. We found this ratio was just above unity for manufacturing and construction, around 0.75 for accommodation and food services, and 0.5 for administrative services.[13] This implies that employers report twice as many QCEW jobs in the administrative sector as individuals report working in those sectors from the ACS, which suggests the sector has individuals with more than one

[13] This exercise used the 2005–2007 ACS sample to count the number of employed individuals by NAICS industry at the state level. We then divided this number by the average reported jobs across quarters from the QCEW for these three years to create the ratios.

job. Our identification strategy does not allow us to take more advantage of the ACS, but it does confirm reporting differences across industries.

We conclude that in three of the four largest industry-level employers of unauthorized immigrants, there is suggestive evidence that the 287(g) policy produced a decline in labor supply, strongly consistent with previous research documenting population responses to the policy. Although not strongly statistically significant, the wage impacts suggest adverse shocks to labor demand may also have been present. These are the exact industries—construction, accommodation and food services, and administrative services—in which we would expect a decline in demand for locally produced goods and services in response to an outflow of immigrants. In the fourth industry we examine, manufacturing, we found a different pattern of results, and we argue that an increase in employment likely reflects a shift from unreported to reported employment. In sum, we find there are real labor-market responses to the implementation of 287(g), concentrated in immigrant-intensive industries.

Impacts from timing of implementation. To study the impacts from timing of implementation, we made a simple modification to the main specification (equation 2) by including 287(g) treatment leads and lags for each of the eight quarters preceding implementation of the policy and eight quarters after implementation of the policy. We do this for two reasons: (1) we view this as a specification test to see if our model correctly predicts no impacts preceding the implementation of the policy, and (2) we wanted to understand if there were dynamic influences that may attenuate the impacts after the policy was implemented over time. To proceed, we drop all observations that are not relevant to the seventeen-quarter window of interest. As we interpret the estimates, it is important to keep in mind that the reduction in both sample and increase in model parameters will decrease the precision of the point estimates.

Results from the timing of implementation are generally as expected. Specifically, for industries with no impacts, estimates were consistent both before and after 287(g) implementation. For those with impacts, we saw little evidence of impacts preceding the law, but impacts in the expected direction after the law for construction, manufacturing, and administrative services. To provide a better sense of this, Figure 3 presents the quarter-specific results for manufacturing and administrative services, where we found opposite-signed impacts on employment.[14] For manufacturing, the figure shows evidence of a slight upward trend in weekly wages, no trend in employment before the law,

[14] We do not include the analogous figure for construction, but it is as expected. For weekly wages, there is no evidence of a pre-intervention trend, but a negative impact after 287(g) was implemented—although the impacts are not significant by quarter.

and an increase in employment after the law that stabilizes around 4 percent—although impacts across quarter cannot be differentiated statistically. The results are less satisfying when looking at administrative services. There does appear to be a slight downward trend in both employment and wages preceding the law, but the impact becomes pronounced once the law is implemented, and stabilizes at around 9 percent for employment six quarters after implementation. Of all industries, administrative services showed the strongest pre-intervention trend, and when including the specification with the pre-intervention trend, the magnitude of the result stays the same, but the estimate is no longer significant. As previously argued, we do not consider the inclusion of county-specific time trends to be necessary, but we present them to show that the results for administrative services are not as robust. At the same time, the consistent pre-implementation clustering of impacts around zero for the other industries assuages our concerns that there were no real trends preceding implementation of the policy.

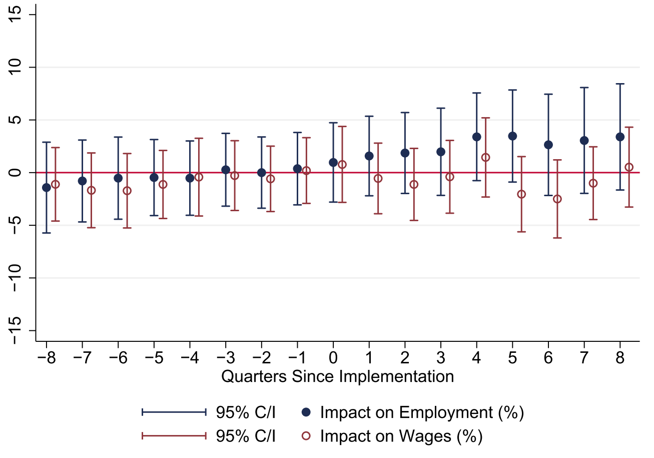
Impacts after excluding pairs with large pre-intervention differences. Given large outcome differences across counties in each contiguous-county pair, we adjusted our sample according to two types of baseline differences in 2003. Specifically, based on two different criteria, we excluded pairs in multiples of twenty-five in a stepwise fashion to study whether or not our results were being driven by contiguous-county pairs with large pre-intervention differences. The two criteria were (1) large differences in the outcome of interest, and (2) specific to industry-level results, large differences in the share of workers in each respective industry. After re-estimating our preferred specification with these exclusions, we found our results to be robust. In presenting these results, we again focus on manufacturing and administrative services—the industries with the significant findings. Figure 4 presents the results when excluding pairs by differences in outcomes and Figure 5 presents results when excluding pairs by difference in industrial share. In each figure, the first estimates that exclude zero pairs represent the primary specification estimated on the full sample. Across each panel, after excluding one hundred pairs, the coefficient estimates appear to be stable. Although the confidence intervals start to increase as pairs are dropped, we take the stability of the estimates as evidence that the results are robust to these outliers.

Additional robustness checks. We performed two final robustness checks related to pairs residing in the same state, as well as the intensity of one well-known case—Arizona. For the first check, we limited our sample to county pairs from the same state. For the second check, given the intensity of 287(g) implementation in Arizona, we limited the sample of pairs to those exclusively

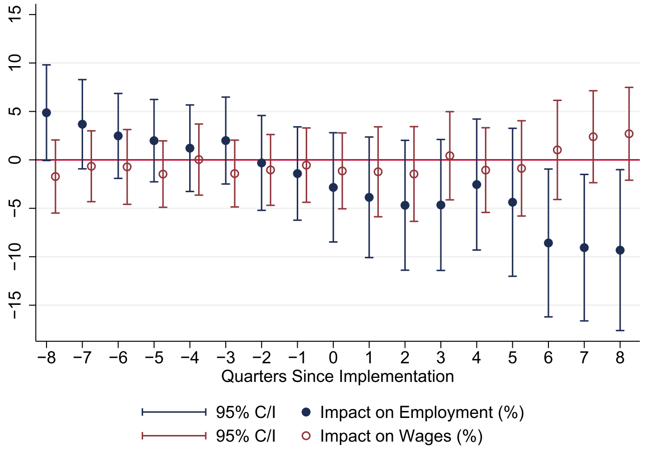
FIGURE 3

Timing of 287(g) Impacts by Quarterr

Panel A: Manufacturing



Panel B: Administrative Services

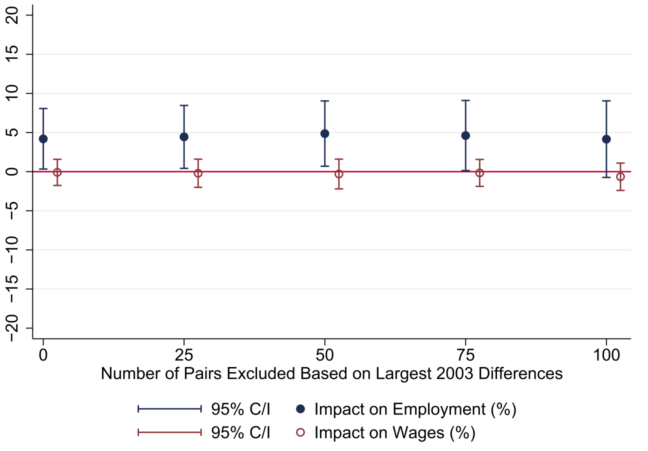


Notes: Employment = log of average monthly employment in quarter; wages = log of total quarterly wages. Impact from 287(g) dosage is presented as a percentage change impact. Regressions include dosage indicators for other local immigration policies including: (1) local ordinances covering employment or enforcement of undocumented individuals, (2) state 287(g) agreements, and (3) state laws covering employment or enforcement of undocumented individuals. Regressions include county and pair–time fixed effects, with robust standard errors, and multiway clustering based on county–time and pair clusters.

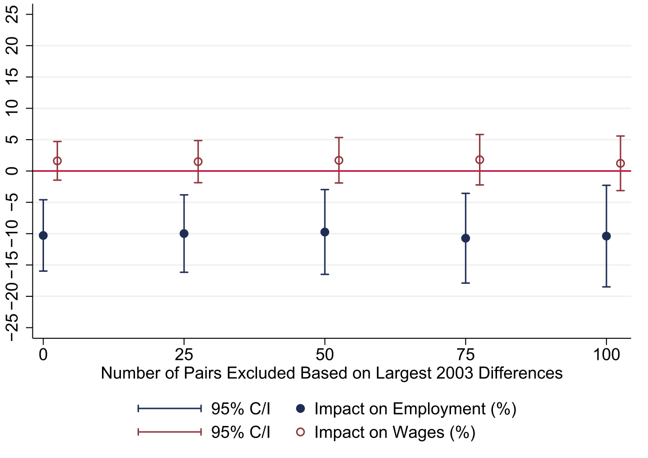
FIGURE 4

Impacts of 287(g) When Excluding Largest Pre-Intervention Differences in Outcomes

Panel A: Manufacturing



Panel B: Administrative Services

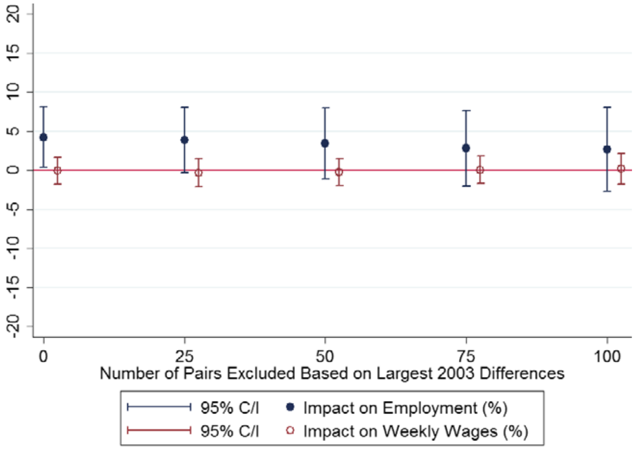


Notes: Employment = log of average monthly employment in quarter; wages = log of total quarterly wages. Impact from 287(g) dosage is presented as a percentage change impact. Regressions include dosage indicators for other local immigration policies including: (1) local ordinances covering employment or enforcement of undocumented individuals, (2) state 287(g) agreements, and (3) state laws covering employment or enforcement of undocumented individuals. Regressions include county and pair–time fixed effects, with robust standard errors, and multiway clustering based on county–time and pair clusters.

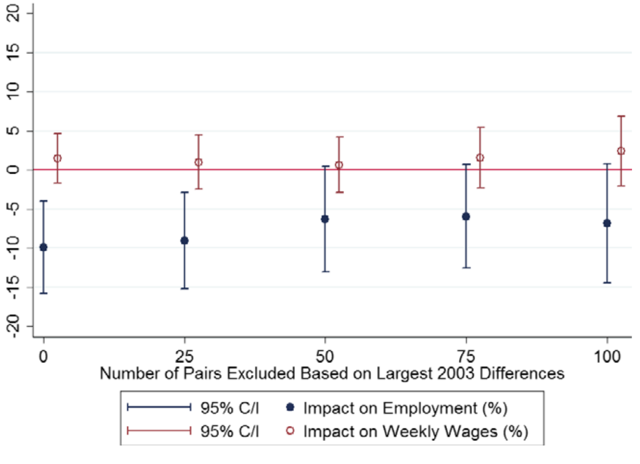
FIGURE 5

IMPACTS OF 287(G) WHEN EXCLUDING LARGEST PRE-INTERVENTION DIFFERENCES IN SHARE OF INDUSTRY EMPLOYMENT

Panel A: Manufacturing



Panel B: Administrative Services



NOTES: Employment = log of average monthly employment in quarter; wages = log of total quarterly wages. Impact from 287(g) dosage is presented as a percentage change impact. Regressions include dosage indicators for other local immigration policies including: (1) local ordinances covering employment or enforcement of undocumented individuals, (2) state 287(g) agreements, and (3) state laws covering employment or enforcement of undocumented individuals. Regressions include county and pair–time fixed effects, with robust standard errors, and multiway clustering based on county–time and pair clusters.

outside of Arizona. In both cases, the results were unchanged across both outcomes and all industries.

Conclusion

Local immigration policies have become some of the key policy tools used to address unauthorized immigration over the past decade. At the same time, the economic consequences of these policies have not been well documented. Overall, we find little evidence that local 287(g) policies led to large county-wide labor-market changes. Counties that implemented 287(g) programs do not have noticeably larger declines in employment or wages than their neighboring counties that did not adopt 287(g) programs. However, across industries with a substantial share of unauthorized workers, we find a pattern of results suggesting 287(g) programs induced a decline in labor supply and—to a lesser extent—some labor-demand attenuation as well. In administrative services, construction, and accommodation and food service industries, employment was lower and wages were higher than neighboring counties following the passage of 287(g). Only in administrative services, however, do we find robust and precise estimates for employment, on the order of 7–10 percent lower. In manufacturing, reported employment was actually higher by about 4 percent.

These findings suggest distributional impacts, which may be an important consideration for certain local governments with a particular industrial composition. We find robust evidence that 287(g) does have distributional consequences, but we are reluctant to draw strong policy conclusions on the economic consequences given the limitations of data on undocumented workers. Specifically, given the illicit nature of work for the unauthorized population, we are not able to perfectly measure economic well-being at the county level because our measures are based on formally reported employment only. Although it is clear that there are economic ramifications to these laws, our findings point to the need to improve our understanding of the unauthorized workforce in the formal labor market, as well as the significance of the informal market in each industry.

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