Is ICE Freezing US Agriculture? Impacts of Local Immigration Enforcement on Farm Profitability

Jennifer Ifft
Margaret Jodlowski

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Cornell University
Charles H. Dyson School of
Applied Economics and Management

Abstract

This study provides one of the first causal estimates of the impact of immigration policy on U.S. firms. An unbalanced panel of confidential, national farm survey data and aggregated county-level farm census data are used to test the impact of enhanced county-level immigration enforcement on various indicators of labor supply shocks and firm profitability. The endogeneity of enhanced enforcement through the 287(g) program is addressed by using pre-policy county jail occupancy as an instrumental variable. Our results are consistent with labor supply shocks and suggest a decline in farm profitability. Farms in counties adjacent to those participating in 287(g) appear to have benefited from a positive labor supply shock. These results suggest that that labor-saving technology and native workers are at best partial substitutes for immigrant farm workers.

1 Introduction

Unauthorized workers make up a large share of the labor force for several domestic industries, and the debate on U.S. immigration policy is wide-ranging, covering the economic impacts for workers and businesses, as well as national fiscal implications. However, very little is actually known about the impacts of immigration policies on firms, especially the degree to which immigrant labor can be substituted. Studies on the economy-wide effects of immigration have largely focused on wage and employment impacts for both immigrant and native workers (e.g. [Chiswick, 1978], [Borjas, 1987], [Ottaviano and Peri, 2012]). Changes to wage levels and employment rates are firm-level decisions, so estimating how firms are influenced by changes in the supply of immigrant labor is critical to understanding the broader economic implications of immigration policy. However, the costs of current immigration policy for U.S. businesses are difficult to measure due to the need for firm-level data, as well as the challenge of disentangling effects of immigration policy from broader economic trends.

This paper explores how U.S. farms were affected by a labor supply shock induced by a change in local immigration enforcement levels. Our study provides a detailed, firm-level analysis of adaptation to immigration policy changes. We contribute to an ongoing national policy debate that is supported by thin empirical evidence on how firms' non-wage decisions are affected. Further, our approach allows us to provide evidence on the substitutabilty of native workers and capital for immigrant labor in a sector heavily dependent on an undocumented labor force. Foreign-born workers tend to specialize in manual labor [Peri and

Sparber, 2009], which limits their substitutability. Whether this substitutability exists, and if so, its extent, is a crucial point of difference in many arguments on both sides of the immigration debate.

If production technology can always adapt to the available labor supply, different industries will be largely unaffected by policy changes that change the relative availability of a "type" of worker. In examining sector-level response of labor supply shocks, Lewis [2003] finds that they do not affect the local sector mix. Instead, increases in the supply of a type of labor, such as manual immigrant labor, tend to increase relative factor intensity of that type, with no effect on wages. Consistent with theoretical models of endogenous technical change, a decrease in the supply of one type of labor causes substitution away from that type. The underlying assumption of adaptive technological change may be violated in industries, like agriculture, that rely on manual labor. The labor supplied by and demanded of immigrant workers in agriculture may also differ from that of native workers. In addition to potentially not having 'competing' workers, other factors of production, like machinery, may only partially substitute for labor. Therefore, industries with limited substitutabilty of native workers and capital may undergo fundamental changes: in addition to shifting the wage rate and the relative amounts of labor and mechanization, firm structure and profitability may be impacted.

Regardless of the outcome considered, causal identification of labor supply shocks remains the foremost challenge to understanding the impact of immigration. Borjas [2003] summarizes the difficulty of overcoming this challenge: "practically all empirical studies in the literature" explain the impact of immigration using a spatial correlation that relates the native wage in a metropolitan area with the relative number of immigrants there. Borjas [2003] says that this method ignores the endogeneity between local conditions and the supply of immigrants. This has been overcome in two ways: either by exploiting an unexpected exogenous shock, like the Mariel boatlift [Card, 1990], or by instrumenting for the current immigrant population, using the past populations of immigrants from the same place of origin, relying on the enclaving tendency of immigrants to settle in the same place over time [Card, 2009]. The former occurs infrequently and typically only in one place, affecting external validity, while the latter is really only effective for urban areas, where enclaves are able to form.

Give these challenges, the few existing studies of the impact of immigration policy on workers have mixed results. There is evidence that employment impacts of local immigration enforcement may vary between industries [Bohn and Santillano, 2017] and that U.S.-born, non-Hispanic whites were unaffected by E-Verify [Orrenius and Zavodny, 2015]. Dustmann et al. [2013] finds that immigration does depress native wages below the 20th percentile, but increases wages at the higher end of the distribution, with an overall slightly positive effect of immigration on wages. In contrast, Borjas [2003] finds a small, but significant, negative impact of immigration on wages: an increase in the labor supply of immigrant workers reduces wages for native *competing* workers by 3-4%.

This study takes advantage of a natural experiment provided by the spatial and temporal variation of 287(g) program adoption to estimate the impact of increased immigration enforcement on the profitability and structure of U.S. farm businesses, which are only weakly linked to broader economic conditions. We first describe the 287(g) program and farm labor use in U.S. agriculture. We then take advantage of access to two different sets of agricultural survey data, one at the farm level and one county-level, to implement a unique and robust instrumental variables strategy for local immigration enforcement decisions. This approach allows us to provide one the first causal estimates of the economic impact of immigration policy on U.S. firms. Our findings suggest that 287(g) created labor shortages and decreased farm profitability in the counties where it was implemented, with positive spillovers for bordering counties. In addition to evidence of labor shortages in agriculture, our results show that changes in firm-level responses are an important component of the ongoing immigration policy debate.

1.1 The 287(g) program

In the last decade, anti-immigrant sentiment has become increasingly codified at the local level, motivated by discontent with state- and national-level approaches. Some localities have passed ordinances requiring proof of legal residency to obtain housing or have designated English as the "national language" of their municipality [Guzman, 2010]. Some counties have taken even more explicit action through an immigration enforcement program now widely known as 287(g). This program, outlined in Section 287(g) of the 1996 Immigration and Nationality Act, allows local law enforcement at the state, county, or county-equivalent level to be deputized as national immigration agents or enforce national immigration policy within its jurisdiction. Under this program, local officials can, for example, check a person's immigration status during a routine traffic stop. If the person does not have legal status, the 287(g) deputy can then begin the procedure to remove him/her from the United States. Although 287(g) has been on the books since 1996, it has only been in the last decade that law enforcement agencies have signed MOU agreements with US Immigration and Custom Enforcement (ICE) and been enrolled in the program.

While the program's stated intent was to identify and remove only dangerous undocumented individuals, the extent to which implementation aligned with intention is debated. Those opposed to its widespread implementation have argued that it provides a path for local law enforcement to racially target and harass residents of their jurisdictions [Shahani and Greene, 2009]. The direct removal of immigrant labor that this program authorized was non-trivial in many places, and the policy's signal to immigrants considering where to locate may have also have had a large impact. Through both direct action and indirect signalling, implementation of 287(g) has been shown to decrease the local immigrant population and labor supply, regardless their of legal status [Watson, 2013], [Kostandini et al., 2014]. This singalling effect is by no means limited to 287(g): a program that mandated E-verify in Arizona in 2007 was shown to reduce the population share of of 'Hispanic noncitizens' relative to other states [Bohn et al., 2014].

Because there is both temporal and spatial variation in the implementation of this program, it provides a unique opportunity to study the impacts of local (here, county-level) shocks to the population of immigrants. Bohn and Santillano [2017] used a contiguous-county pairs identification strategy for 287(g) participation and find diverse county-level employment impacts. Here, we look at the farm sector's response to a negative labor supply shock.

1.2 Immigrant labor and the farm economy

With the availability of detailed data, the farm sector provides a unique setting for causal identification of the firm-level impacts of a specific immigration policy. Outcomes in other industries that utilize undocumented labor, such as hospitality and construction, are difficult to untangle from general economic trends. However, the farm sector is not strongly tied to general economic conditions. For example, during the recession that began in 2008, U.S. farms were much less affected than the rest of the economy [Shane et al., 2009]. Hornbeck and Keskin [2015] show that productivity, revenue, and land value gains in agriculture largely do not influence the non-agricultural sector. The economic spillovers of agricultural gains are limited to only the immediate short run and are not persistent.

Immigration policy and enforcement has serious implications for U.S. farms. Approximately half of U.S. farm workers are estimated to be "undocumented", or lack legal status to work in the U.S. [Zahniser et al., 2012]. While significant mechanization of U.S. agriculture has occurred over the past several decades, many types of fruit, vegetable, livestock, greenhouse, and nursery specializations still rely on hired labor to perform complicated or delicate tasks. While labor expenses are only 17 percent of cash expenses for the U.S. farm sector, this share approaches 40 percent for more labor-intensive specializations [Zahniser et al., 2012]. There may be localized shortages of farm labor across the U.S.: Hertz and Zahniser [2013] identified several counties with wage growth of over 40 percent where agricultural employment had fallen.

Similar to the general literature on labor and immigration policy, studies on farm labor issues have focused on wages, labor supply, and migration decisions, e.g. Buccola et al. [2012], Taylor et al. [2012], and Fan et al. [2015]. Kostandini et al. [2014] find that authorization of local (county-level) immigration enforcement through 287(g) leads to a decline in non-citizen population levels, based on population estimates from the American Community Survey. However, they do not make causal claims about how immigration enforcement impacts the agricultural sector at the firm level.

This study takes advantage of unique farm level data and an innovative identification strategy for 287(g) participation to estimate these causal outcomes. Our findings will provide evidence on whether the assumptions, including no trade barriers and identical technology, that adaptive technological change models rely on hold for the agricultural sector. The movement of agricultural labor may not be free, if heterogeneous levels of immigration enforcement serve as a trade barrier. Different types of labor (i.e., immigrant and native) might not be perfect substitutes, and so the 'identical technology' assumption may not hold. Further, mechanization of farming, or substitution of capital, may not be feasible for all types of agricultural production or for all agricultural tasks. In order to evaluate this, our focus will be on firm level responses (production decisions) as well as indicators of farm structure and profitability.

2 Data

2.1 Farm survey data

Data on production decisions, farm structure, and profitability come from two national farm surveys, both conducted by the United States Department of Agriculture (USDA). The Agricultural Resource Management Survey (ARMS) is the only nationally representative annual farm level survey. This cross-sectional survey collects detailed data on production activities, finances, and household characteristics. In most years, approximately 20,000 farms complete the ARMS. Data collected from ARMS inform official agricultural statistics and has supported a wide body of research [Kuethe and Morehart, 2012].

Due to its large sample size relative to the U.S. farm population, over 60,000 farms have been included in ARMS at least twice since its inception in 1996. Weber et al. [2016] took advantage of these repeated observations to create an unbalanced panel to analyze the impact of crop insurance participation on fertilizer expenditure. We use a similar approach to analyze the impact of 287(g) authorization on various farm production decisions. ARMS uses a stratified random sampling procedure. While farms are randomly selected, larger farms are oversampled due to their low numbers relative to the rest of the population and lower response rates. While this may be an issue for studies that want to draw implications for the entire farm sector on average, our study is concerned with farms that are labor intensive or rely on hired labor. These farm types tend to be relatively large, as detailed by Weber et al. [2016], and hence this sampling design provides us with a larger number of farms potentially impacted by immigration policies.

While survey weights are provided for ARMS to calculate population-level statistics, due to survey design they are only applicable to single-year analyses. Hence, our estimates will be representative of farms that were randomly sampled more than once over our study period (1996-2012). Farm attrition is a potential concern, as is the representativeness of our sample. We address these issues through analysis of publicly available county-level Census of Agriculture data, which are collected from all U.S. farms every five years. Additionally, Census of Agriculture data provide an opportunity to use aggregated indicators of farm sector impacts, such as income levels.

These data sets allow us to consider a variety of potential impacts of 287(g) participation and enforcement, but generally do not include information on per-unit prices paid or received, wages, or yields. We use the available variables that measure how production decisions were impacted, as well as overall impacts on farm structure and profitability. For individual farm survey data from ARMS, we focus on key production decisions that might be affected by a labor supply shock: total labor expenses, total fuel expenses, acres fruit harvested, acres vegetables harvested, and acres of “entirely mechanized” crops harvested. While some fruits and vegetables can be mechanically harvested, others cannot. However, harvest of many other crops is almost entirely mechanized. To evaluate the degree to which farmers are able to switch to less labor-intensive crops, we aggregate acres for all crops individually reported in ARMS that are typically mechanically harvested.[1]

These measures all reflect production decisions made by farm operators that could be impacted by labor availability and costs, and have long term implications for farm profitability. In response to a labor supply shock, farmers might use less labor or switch to a more expensive source of labor; fuel expenses would increase if mechanical equipment was used to substitute for the loss of labor. Likewise, if farms decrease production or input levels, production expenses could decline. Given this ambiguity, results must be interpreted in the context of production levels. Vegetable and fruit production is generally more labor-intensive, so a decline in the number of vegetable or fruit acres harvested would reflect less labor use. Similarly, an increase in 'mechanized crops' would suggest substitution between different types of production. Table 1 reports average values of our key variables of interest throughout the study period and indicates that counties with 287(g) had larger labor and fuel expenses and less acres of mechanized crops. Fruit and vegetables acres did not have a statistically significant difference.

2.2 County-level farm survey data

Data from the US Census of Agriculture, which is undertaken every 5 years, is publicly available and reported at the county level. We use outcomes from the Census to establish first that a negative labor supply shock occurred in 287(g) counties, and then to look at aggregate measures of both production decisions and more medium-term financial outcomes. The first outcome, number of workers, directly addresses the first question: it is a county-level measure of the total agricultural labor force. Similar to our analysis with ARMS data, the next three outcomes are aggregated production decisions that are likely to be impacted by a labor supply shock: labor expenses, fuel expenses, and vegetable acres (fruit acres harvested are not reported). The next set of outcomes look at some of the less immediate potential responses to a labor supply shock: machinery value per operation, acres operated (per county), and

[1]These crops include barley, canola, corn, cotton, hay, oats, peanuts, potatoes, rice, sorghum, soybeans, sugar, tobacco, and wheat.

number of farms (per county). The final two outcomes, net farm income per operation and asset value per operation are used to assess whether the changing production environment is reflected in farms' financial status and asset holdings. Farm asset values reflect market expectations for long-term profitability of the farm business, with labor shortages potentially being capitalized into asset values.

Summary statistics for these outcomes measured before the onset of the 287(g) program are provided in table 2. Counties with 287(g) have more farm operations, and the operations in these counties are larger, in terms of net farm income and machinery values. There is also some limited evidence that counties with more immigrants selected into 287(g): prior to enrollment, treated counties had more agricultural workers and farms there spent more on labor.

2.3 Immigration enforcement data

In 2007, twenty-four (24) counties signed MOUs with ICE and enrolled in the 287(g) program. By 2010, the number had reached its peak with fifty-three (53) counties enrolled; these counties are pictured in 1. As the map shows, these counties are concentrated in the south, although a wide range of agricultural production systems are represented. A county signing an MOU measures participation in 287(g) on the extensive margin. In data accessible via a Freedom of Information Act (FOIA) request, ICE provides two yearly (and, in some cases, monthly) measures of the intensity of 287(g) enforcement for each participating jurisdiction in the program. 'Aliens identified' measures the number of undocumented immigrants identified by local officials, and 'aliens departed' measures the number of those that were successfully removed from the jurisdiction. As such, aliens departed is the stronger measure of enforcement, although both measures quantify the extent to which a county acts on their 287(g) mandate. Table 3 has summary statistics for enforcement data, showing the high degree of variability of 287(g) enforcement measures.

There are 134 jurisdiction-year observations where no aliens were identified, and 149 during which none were departed. A 287(g) program therefore both directly reduces the supply of immigrant labor in a county, but also discourages potential immigrant laborers. Watson [2013] shows that the 287(g) task force model significantly discourages immigrant inflows to a 287(g) location, in addition to pushing immigrants from 287(g) areas. This out-migration's impact is equivalent to a 15% decline in predicted labor demand, by Watson's calculations. Importantly, Watson's work also shows that these local immigration enforcement policies do not drive workers out of the United States entirely, but rather cause within-country migration between local areas that have the program and those that do not.

3 Empirical model

We model farm production decisions, structure and profitability as a function of 287(g) participation (immigration discouragement), because a major shift in labor supply or costs could affect almost all farm decisions. We also control for whether or not a non-participating county bordered a 287(g) county, because there could be various spillovers, as suggested by Watson [2013]. It is uncertain *a priori* whether 287(g) would increase or decrease labor supply and cost in bordering countries, depending on whether immigrants were deterred from or moved to neighboring counties. We use a farm-level (or county-level) fixed effects model, as time-invariant farm-level characteristics, such as specialization, have a strong relationship with labor use. While our ARMS panel is unbalanced, we expect no bias, as farms are observed based on random selection into ARMS. However, farms had to exist after the policy went into effect, so our results need to interpreted as representative of farms operating before and after the policy. Our basic estimating equation is as follows:

$$Y_{icst} = \alpha_0 + \alpha_1 G_{cst} + \alpha_2 G_{st} + \alpha_3 B_{cst} + \tau_t + \gamma_i + \varepsilon_{icst} \tag{1}$$

where Y_{icst} is the outcome of interest for farm i in county c and state s at time time t; G_{cst} is either an indicator for 287(g) participation or a measure of 287(g) enforcement; G_{st} is a vector of indicators for state-level 287(g) participation or enforcement; B_{cst} is an indicator for whether a non-287(g) county borders a 287(g) participating-county, τ_t are year fixed-effects, γ_i are farm fixed-effects, and ε_{icst} represents variation in the dependent variable that cannot be explained by the model.

Respectively, our estimating equation for Census of Agriculture data is as follows:

$$Y_{cst} = \alpha_0 + \alpha_1 G_{cst} + \alpha_2 G_{st} + \alpha_3 B_{cst} + \tau_t + \gamma_c + \varepsilon_{cst} \tag{2}$$

Results from estimation of these naive models are reported in tables 4 (ARMS) and 5 (Census). This basic farm or county fixed effects regression may not provide causal estimates of the effect of 287(g) participation because participation might be correlated with other economic factors that also influence farm decisions and profitability. The extent of enforcement and the presence of immigrants in a jurisdiction could be subject to reverse causality: a high immigrant population could make 287(g) participation more popular, while at the same time enforcement decreases the immigrant population directly through removals and indirectly by signalling hostility towards immigrants. While our use of farm-level (county-level) fixed effects controls for farm (county) characteristics and static local political conditions, we cannot disprove that broader social or economic factors are not driving both farm decisions and 287(g) participation or enforcement levels. Further, the decision to participate in 287(g) is not random; county and state law enforcement agencies select in to the program. Farms located in 287(g) counties are different in many dimensions than from farms in other counties (see table 1), and these counties do have a higher number of pre-program farm workers and labor expenses (table 2).

3.1 Instrumental variable strategy

To address the potential endogeneity of 287(g) participation, we use an instrumental variable (IV) approach, with a county's aggregated occupancy of jails and prisons in 2006 as the instrument. In addition to offering local law enforcement a "political trophy in local law enforcement campaigns", 287(g) has been reported to have offered counties or other jurisdictions the perceived potential for financial gains. There have been concerns that, despite statutes disallowing such reimbursements, ICE has “misrepresented” the extent to which this is actually the case. Shahani and Greene [2009] cite evidence that, even if this was not actually true, local law enforcement were under the impression that they would be reimbursed for the cost of housing incarcerated non-citizens under the 287(g) program.

The Bureau of Justice Statistics (BJS) publishes a publicly available Annual Census of Jails, which provides information on a nationally representative set of jails and prisons from across the United States [Bureau of Justice Statistics]. This data set includes information on the rated capacity of the jail, or the total number of inmates the facility can hold, and the total population of inmates. Both measures, which are recorded at the facility level, are aggregated up to the county level. We define occupancy as the total population subtracted from the rated capacity, such that a negative value for occupancy indicates prison overcrowding and a positive value indicates an emptier facility.

Although the data is yearly, we use the occupancy of prisons and jails in a county in 2006 as the instrument. This is the first year data from nearly all counties was available, while still avoiding inclusion of 287(g)-related detentions. In 2006, no rural county had begun acting on their 287(g) mandate, and nearly all MOUs between jurisdictions and ICE had yet to be signed. Figure 2 shows the distribution of jail occupancy across the United States. Although there are certainly areas where jail over-crowding (or under-crowding) seem to be an issue, there is no evidence to suggest that there is meaningful spatial autocorrelation. The Moran’s I, a standard measure of spatial correlation, (see Figure 3) is not significantly different than 0 using the minimum threshold distance spatial weights matrix for US counties. Although this does not definitively prove that jail capacity in one county does not impact capacity in its neighboring counties, it alleviates concerns that county-level jail capacities move together.

All of the coefficients from our main results are estimates of the local average treatment effect, or LATE, as we are using an instrumental variable with heterogeneous treatment effects. Therefore, we estimate the effect of program participation for compliers only, or the sub-population of counties whose 287(g) status was manipulated by jail occupancy. The counties who would have participated regardless of their jail occupancy are not part of this group.

Our estimating equation using jail occupancy levels (Z_{cs2006}) as an instrumental variable for 287(g) participation or enforcement levels is as follows, with instrumented 287(g) participation represented by G^*_{cst}:

$$Y_{icst} = \alpha_0 + \alpha_1 \underbrace{G^*_{cst}}_{=Z_{cs2006} \times \tau_t} + \alpha_2 G_{st} + \alpha_3 B_{cst} + \tau_t + \gamma_i + \varepsilon_{icst} \tag{3}$$

3.2 Exclusion Restriction

The validity of jail occupancy as an instrumental variable rests on the plausibility of farm decisions and structure being unrelated to this measure of jail capacity. Because of the measure of jail occupancy used to calculate our instrument is time-invariant, its validity also rests on there being no confounding trends, in addition to the standard exclusion restriction. We consider a variety of factors and trends that may be related to farm outcomes and jail capacity and find no evidence to suggest that these counties, which are spread across the country, differ idiosyncratically from their neighboring counties in a way that would drive the results.

As shown in figure 2, jail occupancy varies widely across the country. Because we use only the impacts in the relatively few counties with 287(g) programs, the remaining counties in the United States, including counties that border 287(g) counties, form the control. If there were any economy-wide (or even state-wide) trends driving our findings, these effects would not be localized in just the 287(g) counties. Indeed, such an effect would have to impact these counties differently from their neighbors but have the same effect across the 287(g) program group, which represents a variety of agricultural and even legal systems, to the extent that laws and regulations differ by state and county. Our results below indicate that 287(g) border counties often had the opposite effect of participation, indicating substantial spillovers from a county with a stricter enforcement regime to one that is relatively less strict.

3.2.1 County economic specialization

Using county typology codes from the USDA ERS, which characterize a county's sectoral economic dependence and other policy-relevant features, we can compare 287(g) counties to the rest of the country in 2004, before 287(g) adoption in any county. This comparison is summarized in Table A1, which shows that many differences exist between 287(g) counties and counties without the program, but generally in ways that support the exogeneity of the instrument rather than hinder it. Most importantly, these data indicate that none of the counties that implemented 287(g) are agriculture-dependent countries (see Figure 4). According to the ERS, a county is agriculture-dependent if it meets one of two criteria: either 1) farm earnings account for an annual average of 15 percent or more of total county earnings during 1998-2000 or 2) farm occupations account for 15 percent or more of all occupations of employed county residents in 2000. (See Parker for more information on these data.) As no counties that enacted 287(g) could meet even this low threshold for dependence on agriculture, it makes it even less likely that general economic conditions affecting jail occupancy would impact agriculture or vice versa. Overall, while the importance of agriculture to the country's GDP has certainly declined markedly over the last century, in the last two decades it has stayed roughly constant and very low, with about 2% of the GDP coming from agriculture.

Another important difference between 287(g) counties and non-287(g) counties is that no 287(g) county experienced population loss, defined as a significant decline in the county population between the 1990 and 2000 census.[2] Further, only 2% of 287(g) counties are classified as being low-employment counties by the ERS, where a low-employment county is one where less than 65% of the working age population in the county is employed. This is significantly different than the 14.8% of non-287(g) counties, and this difference is maintained in the 2015 ERS data as well. Hopkins [2010] found that counties with increasing unemployment were more likely to enact anti-immigrant ordinances; what we see here is actually the opposite,

[2]Further, none of these counties experienced population loss between 2000 and 2010, using the 2015 edition of this data set. Because the 287(g) program began in earnest in 2007, we use the 2004 ERS County Topology data, taken prior to the program's onset, as our main data set.

in that most 287(g) counties did not experience widespread unemployment. Other factors, rather than job loss, are likely driving 287(g) participation.

These classifications, therefore, provide some evidence that these are not a subset of counties doing significantly worse economically than other counties, and neither are they counties where poor outcomes in the agricultural sector are likely to affect outcomes for the county's non-farm economy.

3.2.2 Housing values

Changes in housing values, or in other non-agricultural property values, could potentially affect farm asset values and operational costs as well as being related to broader economic trends. When data on housing values in 287(g) counties is compared to data on housing values in counties that border 287(g) counties, however, there is little evidence that the trend in housing values is different between the two. Tests of the difference in the means of median housing values, for properties with and without a mortgage, between 287(g) counties and border counties reveal no significant differences. Figure 5 shows the trend across the study period for properties with a mortgage and Figure 6 shows the trend for the same years for properties without a mortgage. If property values were driving the results we find, there is no reason to expect one county to be affected by changing property values while leaving neighboring counties untouched. Instead, we see property values in both groups moving together, with no statistically different means in any year.

3.2.3 Weather

Weather patterns, including trends of increasing temperatures, may affect jail occupancy as well as farm outcomes. A lack of data on jail capacity makes analyzing this relationship statistically challenging: while we have access to monthly or even daily weather for each county, jail capacity is measured only once a year. This complication actually strengthens the exclusion restriction: because the counties that enacted 287(g) are spread across the country, the growing seasons in each county differ (see figure 1). In order for weather to have an impact it would have to affect different counties with different weather patterns and agricultural systems in the same way, while affecting counties that border 287(g) areas differently.

Recent studies suggest that there is a strong correlation, if not a causal relationship, between an area's temperature and crime levels. Ranson [2014], for example, finds a strong positive effect of temperature on criminal behavior over the last 30 years, using US county-level data. However, yields typically decline with temperature [Rosenzweig et al., 2001]. Likewise, greater precipitation, which dampens levels of crime according to some studies, increase yields for agricultural commodities. As such, it remains unlikely that even if weather had some impact, the weather events driving an area's jail capacity differ from those driving its agricultural outcomes. This potential effect would bias downwards any impact of 287(g) on agriculture in the context of our empirical strategy: increasing temperatures, especially in the southern part of the US where 287(g) programs are concentrated, inhibit rather than enhance agricultural production.

3.2.4 Farmworkers and crime

There is little evidence to suggest that farm workers, whether they are native or immigrant workers, commit crimes with any more frequency than other members of the population. In fact, counties that are most agriculturally intensive have significantly lower rates of virtually all crimes for which statistics are reported, although this may be due to the rural nature of these areas as opposed to farm sector intensity. Comparison of the means across these two groups, for a selection of reported crimes, can be found in Table A2.

Extensive research on the relationship between immigration and crime has been conducted [Bell and Machin, 2013], with most empirical studies finding no relationship between increased immigration to an area and the crime rate. Because many of these papers use an IV approach where the population of immigrants from a particular country is the instrument, and these data are generally only available for urban areas, their focus is on the impact in cities. However, as Bell and Machin [2013] point out, immigrants may experience an extra disutility from crime compared to native workers, as they face the additional penalty of deportation. This is especially true for undocumented workers, who make up a large share of the agricultural workforce. Additionally, the use of past, rather than contemporaneous, jail data makes it more unlikely that some unobserved relationship between jail capacity and farm decisions is influencing the results. Further, in his study of what factors motivate counties to enact anti-immigrant ordinances, Hopkins [2010] finds that counties with higher crime rates are actually less likely to consider anti-immigrant ordinances.

4 Results

Our main results using farm-level data are reported in table 8, with the first stage results in table 6. The first stage confirms that jail occupancy is is strongly related to 287(g) participation. In table 8 we report coefficients for selected variables representing production responses. We find that both labor and fuel expenses went up substantially for these farms, despite a decline of more than 50 acres in vegetable acres harvested. These changes were statistically significant at the 1 or 5 percent test level. However, we see no statistically significant change in acres of fruit harvested or acres of mechanized crops harvested. Together, these results suggest higher expenses combined with a decline in production, and are consistent with higher wage levels and more use of machinery after 287(g) authorization. Higher wage levels may have been due to use of more expensive labor, including guestworker programs or machinery operators. Combined with the decline in vegetable production, this suggests that even with more equipment and fuel use, farms were not able to maintain production levels. Further, we also observe counties adjacent to a 287(g) county experienced a weakly statistically significant, but opposite, impact for expenses and vegetable acres. This suggests that we are observing a labor supply shock with spillovers. In other words, undocumented workers appear to have moved to nearby counties, where production practices are similar, but do not have the punitive 287(g) authorization. This may have allowed farms in bordering counties to expand production levels without increasing expenditure.

To explore broader farm-level impacts and ensure that the results we find using the farm-level data are not being driven by idiosyncratic ARMS sampling, we run the same specification using county-level aggregated data from the Census of Agriculture. Table 9 reports these results, with the first stage results in table 7. Although it is not a replication of ARMS, because some variables that are provided by ARMS do not appear in the Census and, more importantly, the population of farms represented by the two data sets differ, it is possible to verify the major impacts of the 287(g) policy on the decisions made by agricultural producers. The results are largely consistent with those in the main specification: 287(g) programs in a county are associated with statistically significant higher fuel expenses, lower net farm income, and reduced per-operation machinery values. Together, farm- and county-level results show evidence of a negative labor shock and indicate that farms are making longer-term adjustments that may affect their profitability and scale of operation.

The number of agricultural workers in a county with 287(g) authorization declined by almost 6,000 (table 9, column 1), confirming that these counties did experience a negative labor supply shock as a result of the 287(g) authorization. It is unlikely that this decline is the result of direct 287(g) action, but rather it is driven by the signalling impact of 287(g) for immigrants, both with and without legal authorization to work, in a county. Columns 2-4 report short term operational or production responses. Fuel expenses in counties with a 287(g) policy experience a statistically significant increase using both data sets: fuel-powered machinery is one of the only available substitutes for farm labor, although its effectiveness may not extend to all tasks. Increases in fuel expenses indicate that farm operators had to replace farm labor with equipment. A statistically significant decline in bordering counties' fuel expenses is observed when using farm-level data. Hence it is unlikely that this result is driven by increased farm activity caused by weather or positive demand shocks.

Our county-level analysis indicates that machinery value per operation significantly declined in 287(g) counties (Table 9, column 5), which is consistent with the overall decline in production levels. While \$23,000 worth of machinery is very low relative to the value of modern farm equipment, the more relevant implication is that machinery investments did not increase in response to 287(g). Further, farm operators in these counties could be divesting their operations of durable assets, which would signal low confidence about the viability of farm operations due to a potentially long-term labor shortage.

Losses for farms in 287(g) counties are further highlighted by the statistically significant decline in net income. Over the same period, farm operations in counties bordering 287(g) counties experience an increase in net income per operation. Border counties are most likely to benefit from these policies, as climate and other growing conditions are unlikely to differ sharply across county lines but labor availability may have increased. This decline in income was likely driven by higher expenses and lower production, as indicated by the previous results.

Although we do not observe a statistically significant decline in vegetable acres, as in analysis of ARMS data, total acres operated in 287(g) counties experienced a statistically significant decline of over 57,000 acres. This difference may be due to heterogeneity in fruit and vegetable production across counties that may not reflected in individual farm data but is captured by an overall decline in acres operated in aggregate data. Combined with lower net income per operation, no change in labor expenses, higher fuel expenses, lower machinery values, and fewer workers, these results support that 287(g) caused a labor supply shock that led to a decline in agricultural production.

We look at the impact of the 287(g) program no more than five to six years after it was implemented in the majority of counties, but there are some indicators of long-term outcomes in the Census. While we see no significant impact of 287(g) on farm entrance or exit in either authorized counties or border counties, we do find that, while the number of farms does not change, the number of acres under agricultural operations in these counties does decline. The loss of a stable labor force may have pushed farmers in these counties to reduce the size of their operations overall or take some fields out of production. Taken together, these results indicate that 287(g) changed the structure of the farm sector in these counties, while leaving neighboring counties untouched or better off.

4.1 Standard errors

While we have reservations with any single approach to estimating standard errors in the context of this study, we generally report standard errors that are robust to correlation at the state level. In Appendix B, we report these main results with standard errors robust to correlation at the county and the ERS production region level, as well as heteroskedasticity-robust standard errors without any clustering. We also report Conley spatial standard errors, robust to correlation within 200km from the county centroid.[3]

However, none of these levels of clustering are entirely appropriate. Agronomic conditions, weather, and cropping patterns are not constrained or contained by state (or county) boundaries, and so clustering at the state level ignores these important relationships in the dependent variable that cross state lines. The agricultural region clusters were implemented in part to address this: the regions disregard state boundaries and look only at counties that are agronomically and agriculturally similar. They are limited in their suitability, however, as resource regions contain many states: there is likely within-state correlation of standard errors related to law enforcement practices, agricultural or legal policies, and immigrant populations.

4.2 Robustness Checks

In addition to being generally robust to different approaches to estimation of standard errors, our results are robust to a variety of control groups, noisier measures of 287(g) implementation, and exclusion of larger farmers from our ARMS sample. Further, we conduct a series of placebo tests for the validity of our instrumental variables strategy. Many of our robustness checks also address the issue of using a time-invariant instrumental variable.

[3]Due to the small number of clusters for the ERS production regions, the significance of these results should be interpreted with caution. Additionally, the Conley spatial standard errors are not designed to be used with an IV, and so the results reported are from manual 2SLS and so a similar caution applies.

Alternate measures of enforcement

Our main specification uses a county-by-year dummy variable that indicates when, if ever, a county signed an MOU agreement authorizing the 287(g) program. Statistics on the number of "aliens identified" or "departed" in a given jurisdiction in a year are also reported by ICE. "Aliens identified" (alieniden) is the number of potentially unauthorized individuals taken into custody by local authorities, and "aliens departed" is the number of undocumented individuals who left the county, willingly or otherwise, as a direct result of identification via 287(g) action. These measures are imperfect because they do not account for county size or the local undocumented population; however, measures of local undocumented populations are also problematic. For example, "share of noncitizens" is the best available indicator of the total population of undocumented immigrants in a county, but does not provide an accurate count of undocumented immigrants for comparison to the population "identified" under 287(g). Further, previous work has provided evidence that 287(g) reduced the population of all immigrants in a county, regardless of their documentation status [Kostandini et al., 2014].

While authorization of 287(g) is our preferred measure, we also consider the magnitude of 287(g) implementation by estimating our main specification using measures of local enforcement levels for each county-year observation in place of 287(g) authorization. These results appear in table 10 and tables 11 and 12 for aliens identified and departed, respectively. While the coefficients are difficult to interpret in light of the issues discussed above, this approach allows us to further validate whether we are observing a 287(g)-induced labor supply shock. Counties with higher levels of implementation or enforcement would likely have been less attractive to farmworkers than counties with minimal enforcement.

While this noisier specification does not indicate the same level of spillovers to neighboring counties, the direction of coefficients and levels of statistical significance for 287(g) counties reported in table 10 are consistent with those reported in table 8. For the outcomes considered using Census data (Tables 11 and 12), the effect of an additional departure is larger than the effect of an additional identification, which is consistent with deportation being a stronger anti-immigration measure than identification. While the coefficients should be interpreted with caution given the lack of data on the total undocumented population, these results highlight that the impact on farm operations of higher enforcement was consistent with that of authorization.

Alternate control groups

We initially designated every untreated, non-adjacent county in the contiguous United States as a control for the 53 counties or county-equivalents that had implemented a 287(g) program. It is possible that doing so reduced the precision of our estimates by including counties in the control group that are inappropriate. Because program participation was based on self-selection, every county in the United States had the opportunity to participate: nonetheless, program participation is concentrated in counties in the southern part of the United States and does not uniformly represent all agricultural systems. To account for this, our main specifications report results without counties from the two agricultural production (ERS resource) regions that have no 287(g) programs included.[4]

To check that our results are robust to inclusion of these regions, we report results using all counties in the contiguous U.S. in table C1 for the ARMS results and table C2 for the Census. We also check the precision of our results in tables C3 (ARMS) and C4 (Census), which exclude any state without at least one 287(g) county. Although we see statistically significant results when using the most inclusive control group, the treated-state only regressions take into account that program participation may not have been as viable, economically or politically, in different parts of the country. Although *de jure* each county had an equal chance to participate, in reality such an option may have been less likely to even be considered in some states or agricultural regions. Our results are largely replicated with both of these specifications, for both ARMS and Census data.

The implied magnitude of some farm-level impacts could be considered large: for example, an increase in labor expenses of over $250,000, as reported in table 8. To better understand the underlying farm-level heterogeneity driving these results, we run our analy-

[4]These are: Region 3: Northern Great Plains and Region 9: Mississippi Portal

sis (1) excluding farms with total net worth greater than \$5,000,000 and (2) excluding farms with net worth per acre operated over \$100,000. We report these results in tables C5 and C6, respectively. We find some evidence that our results are driven by changes occurring on large farms, which is expected given that such farms are more likely to use hired labor. When farms with a net worth greater than \$5 million are excluded, the only measure that has a statistically significant effect from 287(g) is vegetable acres. This suggests that farms had decline in vegetable acres but no change in production costs. These results also suggest that large farms, which would use more hired labor, are disproportionately affected by immigration enforcement. When we exclude farms with high net worth per acre from our analysis (table C6), the main results persist in terms of both magnitude and statistical significance. Overall, we do find consistent results when we exclude the largest farms.

While it is not possible to replicate the above check with the Census, we are able to explore whether there are certain 287(g) counties that are driving our results. To provide some evidence that this is not the case, we randomly drop 10% of the treated sample and re-run our main specification. Our findings from table **??** are robust to this procedure, and there is no indication a particular county is driving these results.[5]

Alternative specifications

We perform additional robustness checks designed to address concerns that our results are driven by the size of an individual county, or by other potential drivers of agricultural production and investment decisions. Table C7 addresses concerns that the instrument merely picks up a linear trend in which larger counties have both greater jail occupancy and also greater numbers of immigrants. In this specification, occupancy is defined as a share of the total rated capacity:

$$occ = \frac{totpop}{ratcap} \tag{4}$$

With this specification, we mitigate concerns about the impact of county size, or county crime rates, both of which are reflected to some extent in both the total rated capacity of

[5]Results available upon request.

its jails and its total inmate population, on our main results. Further, our Census results are also robust to the inclusion of state-by-year fixed effects, presented in Table C8. Most results have the expected sign and indicate substantial short and medium term negative welfare impacts for 287(g) counties.

Placebo tests

Finally, to address concerns that our results reflect characteristics of these particular counties or nation-wide trends in agriculture, we perform a series of placebo or falsification tests for our Census results. These results do not provide definitive evidence of the validity of our instrument, as the instrument does not meet the criteria for a strong instrument in the first stage, in either case. In the first, we naively treat the year 2002 as the year 287(g) authorizations began in these particular counties, effectively shifting our analysis five years into the past. That is, a county that signed an MOU in 2008 would be treated as having signed it in 2003. In this test, significant coefficients would suggest that endemic characteristics of these treated counties, other than 287(g), which was not present in 2002, are driving our results. We find only one weakly significant coefficient (on fuel expenses), in this analysis, which is reported in Appendix A, table C9.

Another concern is that it is not a characteristic of the counties in question that are driving results but instead that the results are the product of a general agricultural trend, or a characteristic of the time. The previous supports our use of a time-invariant instrumental variable–a potentially confounding time trend of a declining farm sector may present the same results in other counties. To further address, though not refute, this claim, we perform a similar placebo test to the one described above, except instead of naively treating the year, the timing is kept constant and it is counties across the contiguous United States that are assigned to 287(g) treatment status. To do so, we randomly selected 53 counties, without replacement, and randomly assigned them the start date of an actual 287(g) county. Although this method is not perfect because it assumes an equal likelihood of 287(g) enrollment across all counties, it is a useful exercise to highlight a lack of general, country-wide economic trends behind our results. This process was replicated 200 times, with nothing to indicate more than spurious results.[6]

5 Conclusion

This study provides a unique and robust analysis of how enhanced immigration enforcement affected firms in a sector with a high level of undocumented immigrant labor. We use an unbalanced panel of confidential, nationally representative farm survey data and county-level agricultural census data to estimate how farms are affected by a decline in the undocumented labor force. We take advantage of local labor supply shocks caused by county-level authorization of the Delegation of Immigration Authority 287(g) program, which through authorization alone had a strong deterrent affect on undocumented workers and has been linked to a lower local population of "non-citizens". Because the survey data is largely in terms of expenditure or aggregate production decisions, we consider various indicators of production expenditure and acres harvested with the survey data and broader measures of farm-level impacts using Census data. The potential endogeneity of participation in the 287(g) program is addressed by using 2006 county jail occupancy as an instrumental variable. We find that jail occupancy is strongly correlated with 287(g) participation and that 2006 jail occupancy is arguably exogenous: any differences that could lead to violations of the exclusion restriction point to bias in the opposite direction of our results.

County participation in 287(g) leads to increased labor and fuel expenses and lower vegetable production for farms. At the county level, we observe a decline in the number of farm workers and the total area of land operated in farms, as well as a decline in net farm income. These results are consistent with a labor supply shock and are likely driven by impacts on larger farms that are more likely to use hired labor. Farm-level adaptation through substitution of machinery or alternative labor sources appears to have been attempted, but ultimately was insufficient to prevent a decline in production and profitability. Likewise, farms in counties adjacent to those participating in 287(g) appear to have benefited from a

[6]Results available on request.

positive labor supply shock, with a statistically significant impact in the opposite direction for several measures. The impacts are generally consistent using either farm-level or county-level data, although in some cases coefficients do reflect the different populations and survey methodologies.

These results do not imply that 287(g) decimated agriculture in counties where it was enacted, although they provide strong evidence that neither technology nor native workers are complete substitutes for undocumented farm workers and suggest significant firm-level production and profitability responses to immigration policy. Our study cannot predict what would happen in the event of a nationwide change in immigration policy that affected all farms–external validity is limited by the geographic scope of 287(g) participation. Further, the results of this study are an estimate of the local average treatment effect. However, in the absence of mechanical substitutes and/or a willing domestic labor force, it is difficult to imagine farms maintaining current levels of production and production costs.

Immigration policy will likely continue to be politically contentious and it is uncertain whether a political agreement will ever provide more certainty for sectors with high levels of undocumented labor. Our research suggests that firm-level impacts are an important part of the broader literature that considers the many impacts of immigration policy: wages, well-being of individual unauthorized to work in the U.S., fiscal impacts, and others. While mechanization and robotics increasingly characterize the global economy, labor shortages specific to particular industries or specializations are likely to endure through economic and political changes. The impact of these shortages on firms is an important part of the political debate on immigration policy.

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6 Tables

6.1 Summary statistics

Table 1: Summary statistics: Key ARMS farm production decision variables

	Full sample			**287(g) counties**			**Non-287(g) counties**		
	Mean	**SD**	**N**	**Mean**	**SD**	**N**	**Mean**	**SD**	**N**
Labor expenses ($)	133,687	25,806	712,127	267909***	639	838,571	130,279	25,167	708,311
Fuel expenses ($)	31,452	25,806	74,288	41005***	639	121,040	31,210	25,167	72,698
Fruit acres harvested	41	25,806	2,029	24	639	212	41	25,167	2,054
Vegetable acres harvested	30	25,806	272	38	639	297	30	25,167	272
Mechanized crops acres harvested	750	25,806	1,206	248***	639	737	763	25,167	1,213

***,**,* Significantly different from non-287(g) counties at 1%, 5%, and 10% respectively

Table 2: Summary statistics: Census outcome variables, 2002

	Full sample			**287(g) counties**			**Non-287(g) counties**		
	Mean	**SD**	**N**	**Mean**	**SD**	**N**	**Mean**	**SD**	**N**
Number of workers	994	2,757	3,053	2293**	3,966	53	972	2,726	3,000
Labor expenses	37,346,646	72,013,087	2,830	72,389,081*	130,302,823	50	36,716,386	70,408,264	2,780
Fuel expenses	2,180,165	2,978,700	3,058	2,979,490	3,725,708	53	2,166,067	2,962,675	3,005
Vegetable acres	1,564	7,850	2,142	2,637	6,541	50	1,538	7,878	2,092
Machinery value per operation	70,298	44,934	3,063	49,224***	25,440	53	70,669	45,116	3,010
Net farm income per operation	18,969	31,199	3,037	32,067**	47,335	53	18,736	30,798	2,984
Asset value per operation	586,124	397,036	3,067	660,980	408,843	53	584,808	396,768	3,014
Acres operated per county	110,824	154,195	2,984	101,131	162,655	52	110,995	154,065	2,932
Number of farms per county	465	402	3,072	703***	571	53	461	397	3,019

***,**,* Significantly different from non-287(g) counties at 1%, 5%, and 10% respectively

Table 3: Summary statistics: Enforcement and jail occupancy by county

	Full sample			**287(g) counties**			**Non-287(g) counties**		
	Mean	**SD**	**N**	**Mean**	**SD**	**N**	**Mean**	**SD**	**N**
287(g) authorization (1=yes)	0.007	0.082	3,238	0.400	0.494	55	-	-	3,183
Aliens identified*	843	1,348	24	843	1,348	24	-	-	-
Aliens departed*	618	1,036	24	618	1,036	24	-	-	-
Jail occupancy**	89	4,435	3,218	- 267	1,536	55	95	4,469	3,163

*Conditional on program participation in 2007; **Equal to rated capacity minus total population pre-287(g)
Source: Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

6.2 Results

Table 4: OLS estimates of the impact of 287(g) on select ARMS outcomes

	(1)	(2)	(3)	(4)	(5)
	Labor exp	evfuelo	hfruit	hveg	hmech
g287_border	-19,407	-112.2	50.73	13.71	-17.70
	(28,460)	(4,519)	(45.28)	(13.28)	(14.01)
287(g) authorization	30,319	3,602	-5.173	-2.068	-34.04**
	(21,984)	(7,070)	(21.16)	(12.27)	(13.27)
Constant	138,994***	20,874***	29.74***	38.06***	571.1***
	(22,737)	(4,437)	(9.176)	(6.777)	(42.21)
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES
Observations	42,830	42,830	42,830	42,830	42,830
Number of farms	23,159	23,159	23,159	23,159	23,159

Standard errors robust to correlation at the state level in parentheses

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

Source: USDA ARMS; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table 5: OLS estimates of the impact of 287(g) on select Census outcomes

	(1) Num. workers	(2) Labor exp.	(3) Fuel exp.	(4) Veg. acres	(5) Mach. val.†	(6) Net income†	(7) Asset val.†	(8) Acres op.	(9) Num. farms
287(g) authorization	-460**	4,738	323,438	-289	-16,975***	-23,018***	-170,937***	-10,319**	9.59
	(219)	(7,241,983)	(554,042)	(307)	(2,812)	(3,553)	(52,198)	(4,239)	(17.9)
287(g) border county	-67.4	-447,874	687,412**	-18.4	23,302***	14,645***	226,626***	1,164	-45.9*
	(92.3)	(4,914,648)	(331,069)	(94.2)	(2,751)	(3,537)	(66,559)	(2,086)	(26.9)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,072	7,456	8,140	6,233	8,167	8,084	8,180	7,879	8,182
Number of fips	2,723	2,660	2,730	2,406	2,727	2,720	2,731	2,703	2,729

Standard errors robust to correlation at the state level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
†Outcome measured per operation.
Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table 6: IV first stage: ARMS

	(1)
	287g Authorization
Border state	0.165***
	(0.00858)
State-level 287(g) policy	0.00988***
	(0.00222)
Jail occupancy x 2007	4.07e-05***
	(6.85e-06)
Jail occupancy x 2008	2.86e-05***
	(6.90e-06)
Jail occupancy x 2009	2.80e-05***
	(9.59e-06)
Jail occupancy x 2010	4.33e-05***
	(9.86e-06)
Jail occupancy x 2011	5.54e-05***
	(8.68e-06)
Jail occupancy x 2012	3.23e-05**
	(1.26e-05)
Constant	0.00180
	(0.00451)
F-stat	26.04
σ_u	0.0705
σ_e	0.0943
ρ	0.3588
Observations	42,869
Number of farms	23,198

Standard errors robust to correlation at the state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Source: USDA ARMS; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table 7: IV first stage: Census

	(1)
	287(g) authorization
2002	-.0058**
	(.0022)
2007	-.0113***
	(.0019)
State-level 287(g) policy	.0117***
	(.0030)
Border state	.1143***
	(.0055)
Jail occupancy x year	.0000293***
	(.000023)
Constant	-.000023
	(.0012)
F-stat	13.55
σ_u	.0575
σ_e	.0663
ρ	.4292

Standard errors robust to correlation at the state level in parentheses

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

Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table 8: Impact of 287(g) authorization on select ARMS variables

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Labor exp	Fuel exp	Fruit acres	Veg. acres.	Mech. acres
287(g) authorization	271,350***	124,042**	-2,029	-54.78***	-318.5
	(101,053)	(50,516)	(1,458)	(18.08)	(218.7)
287(g) border county	-64,321*	-22,555*	427.8	23.53*	35.31
	(34,530)	(11,544)	(324.8)	(13.87)	(37.65)
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES
Observations	42,830	42,830	42,830	42,830	42,830
Number of farms	23,198	23,198	23,198	23,198	23,198

Standard errors robust to correlation at the state level in parentheses

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

Source: USDA ARMS; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table 9: Impact of 287(g) authorization on select Census variables

	(1) Num. workers	(2) Labor exp.	(3) Fuel exp.	(4) Veg. acres	(5) Mach. val. †	(6) Net income †	(7) Asset val. †	(8) Acres op.	(9) Num. farms
287(g) authorization	-5,854**	-21,831,558**	9,282,862**	-4,430**	-23,099***	-28,747***	-3,809	-57,537**	55
	(2,393)	(8,695,267)	(4,617,856)	(1,738)	(5,255)	(6,085)	(126,169)	(23,082)	(68.4)
287(g) border county	-1,061*	-4,809,285	2,362,065***	-1,011**	22,103***	13,624**	258,591**	-8,451	-37.5
	(552)	(8,959,011)	(746,261)	(440)	(5,365)	(5,339)	(100,430)	(5,880)	(41.3)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,049	7,338	8,134	5,923	8,163	8,063	8,175	7,821	8,179
Number of fips	2,701	2,542	2,725	2,096	2,724	2,700	2,727	2,645	2,727

Standard errors robust to correlation at the state level in parentheses

*** p¡0.01, ** p¡0.05, * p¡0.1

†Outcome measured per operation.

Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

6.3 Robustness checks

6.3.1 Alternative specifications

Table 10: Impact of 287(g) enforcement levels on select ARMS variables: Aliens identified

	(1)	(2)	(3)	(4)	(5)
VARIABLES	evlabor	evfuelo	hfruit	hveg	hmech
alieniden	23.02*	12.93***	-0.175	-0.00768*	-0.0515
	(12.81)	(4.765)	(0.169)	(0.00393)	(0.0395)
g287_border	-19,359	-2,588	92.33	15.19	-11.52
	(24,593)	(4,179)	(83.94)	(11.66)	(14.50)
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES
Observations	42,830	42,830	42,830	42,830	42,830
Number of farms	23,198	23,198	23,198	23,198	23,198

Standard errors robust to correlation at the state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Source: USDA ARMS; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table 11: Impact of 287(g) enforcement levels on select Census variables: Aliens identified

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Num. workers	Labor exp.	Fuel exp.	Veg. acres	†Mach. val.	†Net income	†Asset val.	Acres op.	Num. farms
Aliens identified	-2.6	-9,290	4,118	-1.99	-10.2**	-11.9	-1.69	-25.1*	.0244
	(1.8)	(16,280)	(2,983)	(2.79)	(4.35)	(7.98)	(41.5)	(14.3)	(.0285)
287(g) border county	-168	-1,025,235	859,576**	-84.2	26,038***	18,045***	259,228***	1,814	-46.4*
	(149)	(4,955,949)	(417,514)	(224)	(2,661)	(3,549)	(64,997)	(2,543)	(26.9)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,070	7,456	8,138	6,233	8,165	8,082	8,178	7,879	8,180
Number of fips	2,722	2,660	2,729	2,406	2,726	2,719	2,730	2,703	2,728

Standard errors robust to correlation at the state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
†Outcome measured per operation.
Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table 12: Impact of 287(g) enforcement levels on select Census variables: Aliens departed

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Num. workers	Labor exp.	Fuel exp.	Veg. acres	†Mach. val.	†Net income	†Asset val.	Acres op.	Num. farms
Aliens departed	-3.91	-13,955	6,183	-3	-15.4**	-17.9	-2.53	-37.7*	.0366
	(2.66)	(24,342)	(4,440)	(4.22)	(6.88)	(11.8)	(62.4)	(21.8)	(.0416)
287(g) border county	-144	-911,149	842,603**	-57.2	26,154***	18,094***	259,247***	2,117	-46.7*
	(129)	(4,923,888)	(411,149)	(195)	(2,631)	(3,545)	(64,940)	(2,500)	(26.9)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,070	7,456	8,138	6,233	8,165	8,082	8,178	7,879	8,180
Number of fips	2,722	2,660	2,729	2,406	2,726	2,719	2,730	2,703	2,728

Standard errors robust to correlation at the state level in parentheses

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

†Outcome measured per operation.

Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

7 Figures

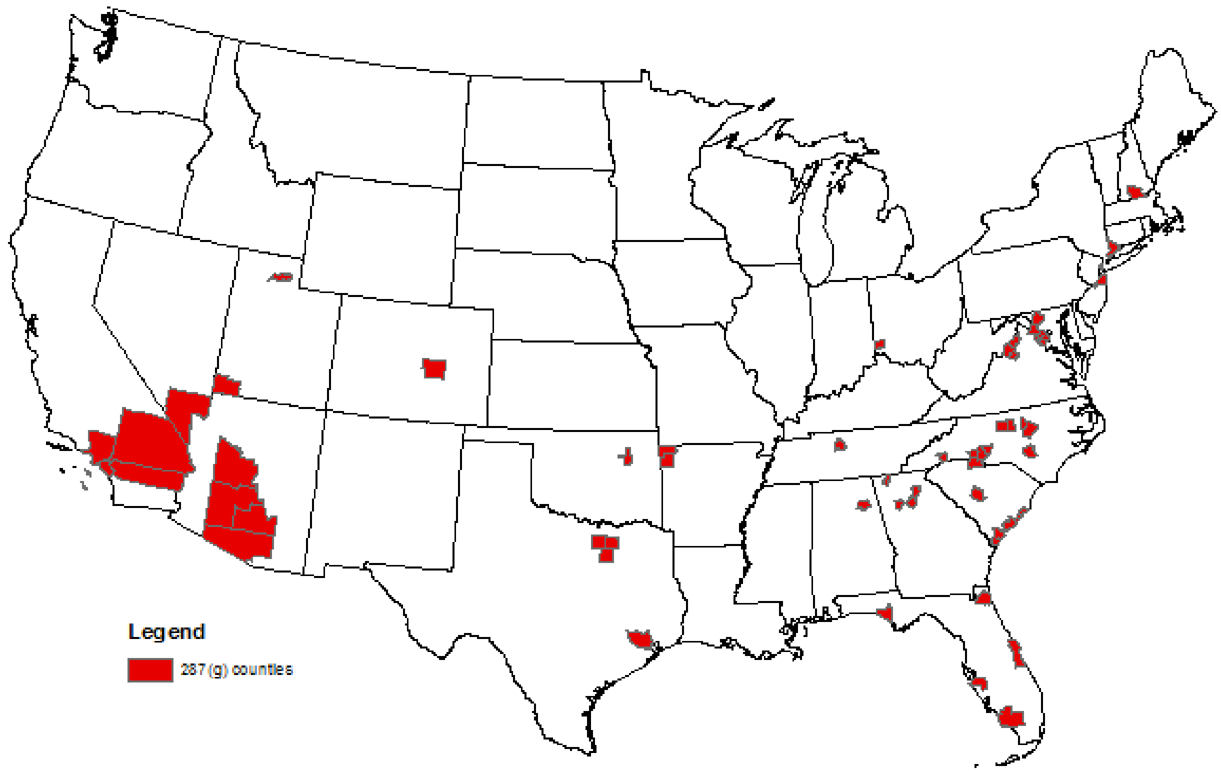

Figure 1: Counties with a 287(g) program

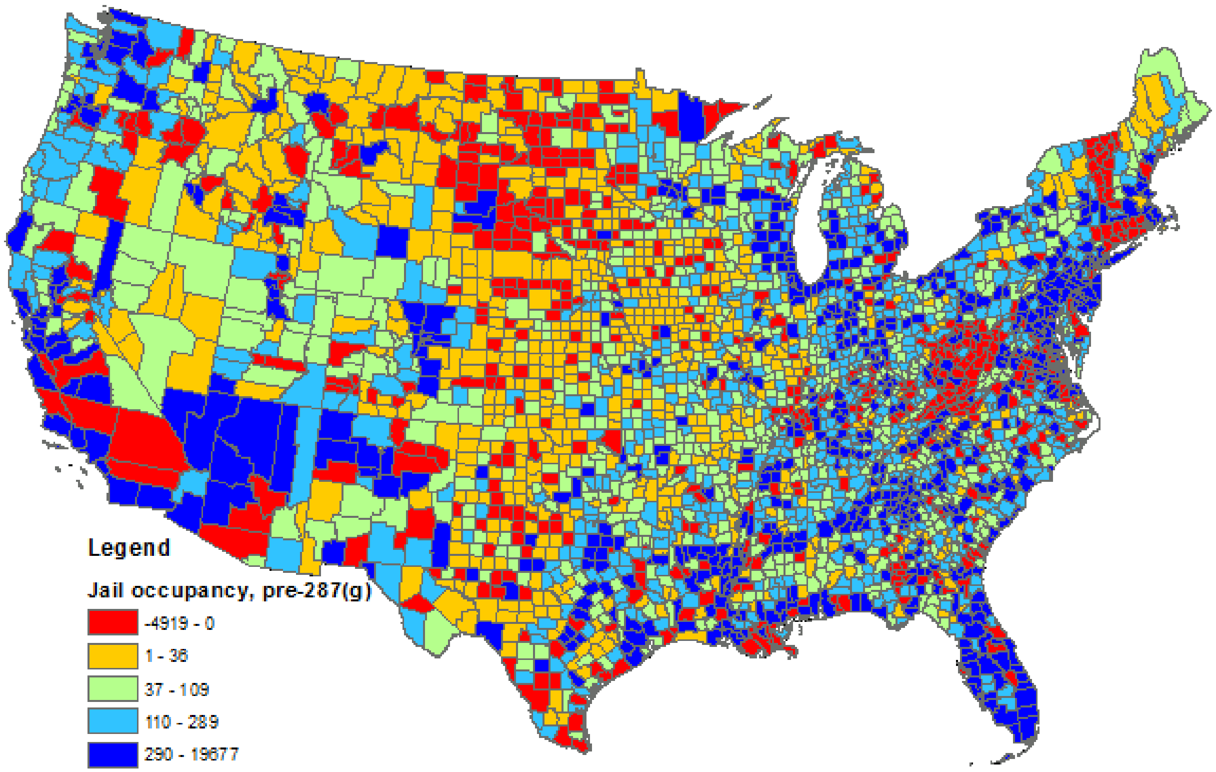

Figure 2: U.S. Jail Occupancy

Appendix A: 287(g) County Comparisons

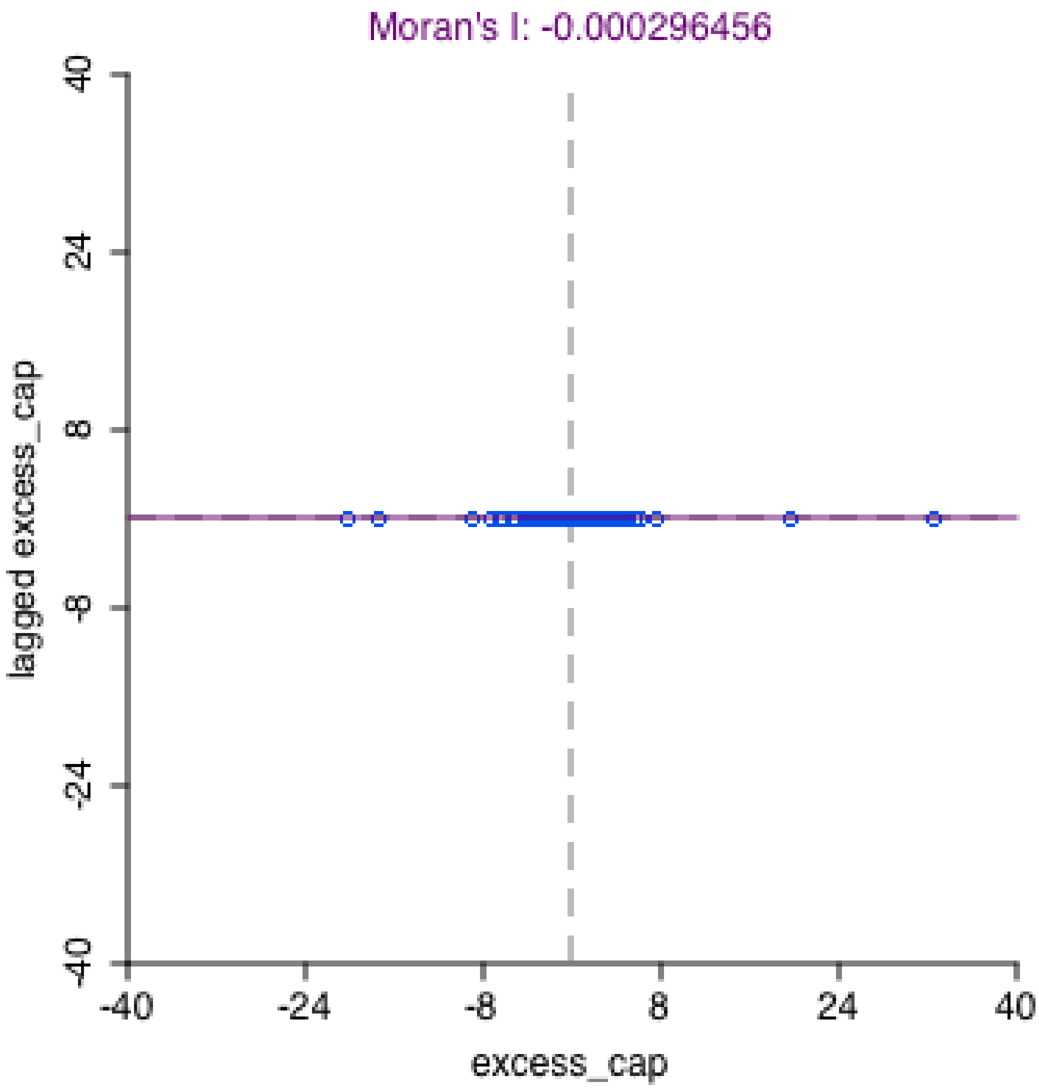

Figure 3: Moran's I rejects spatial correlation for jail occupancy

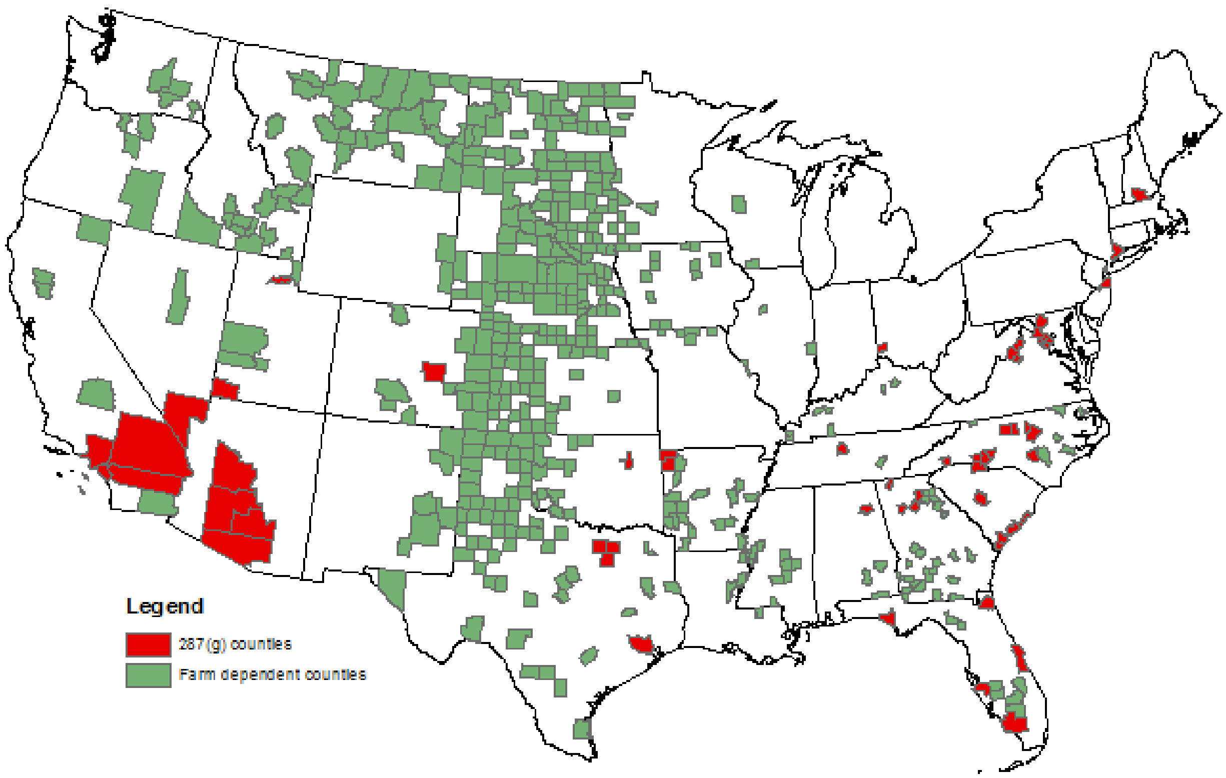

Figure 4: Counties with the 287(g) program and those classified as agriculture-dependent

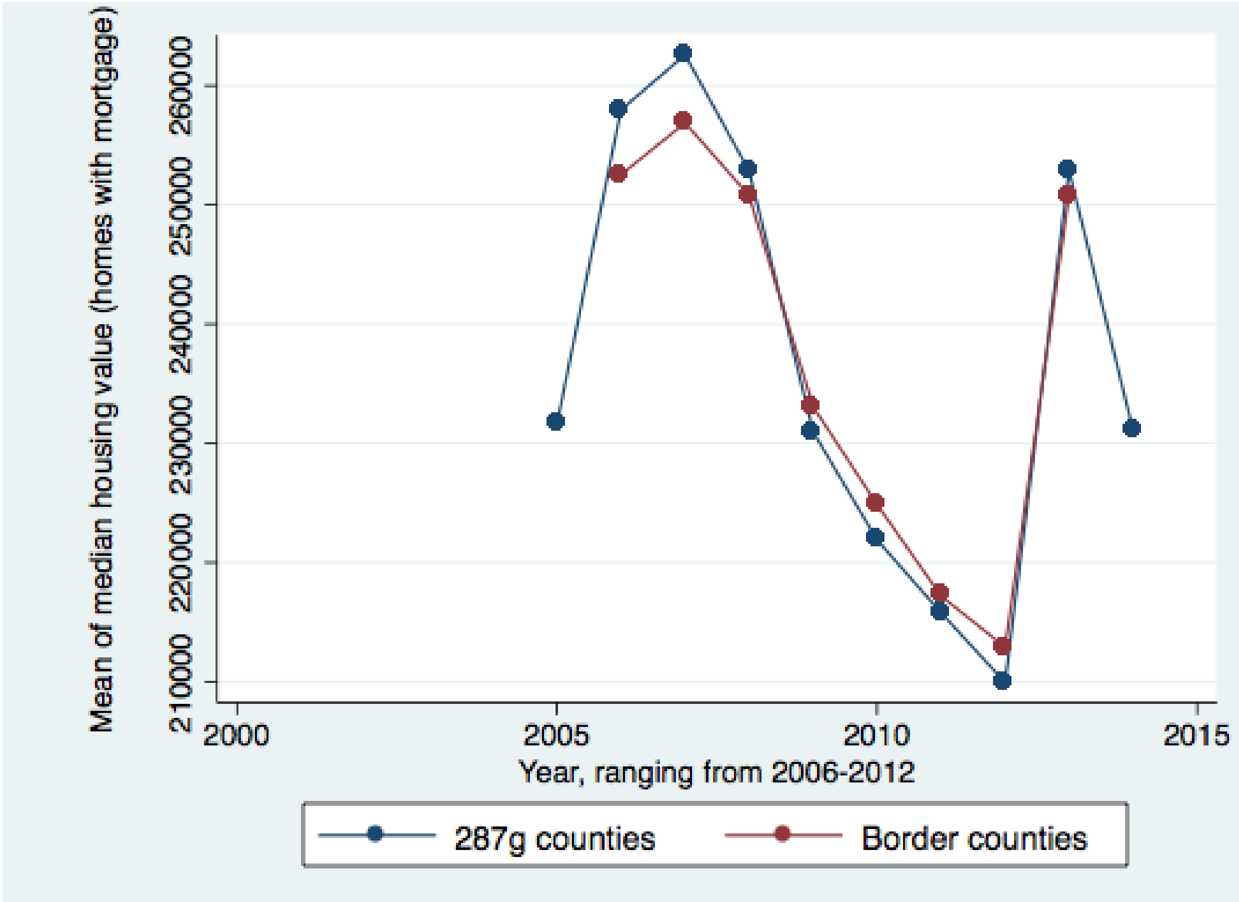

Figure 5: Mean of median housing value, properties with a mortgage

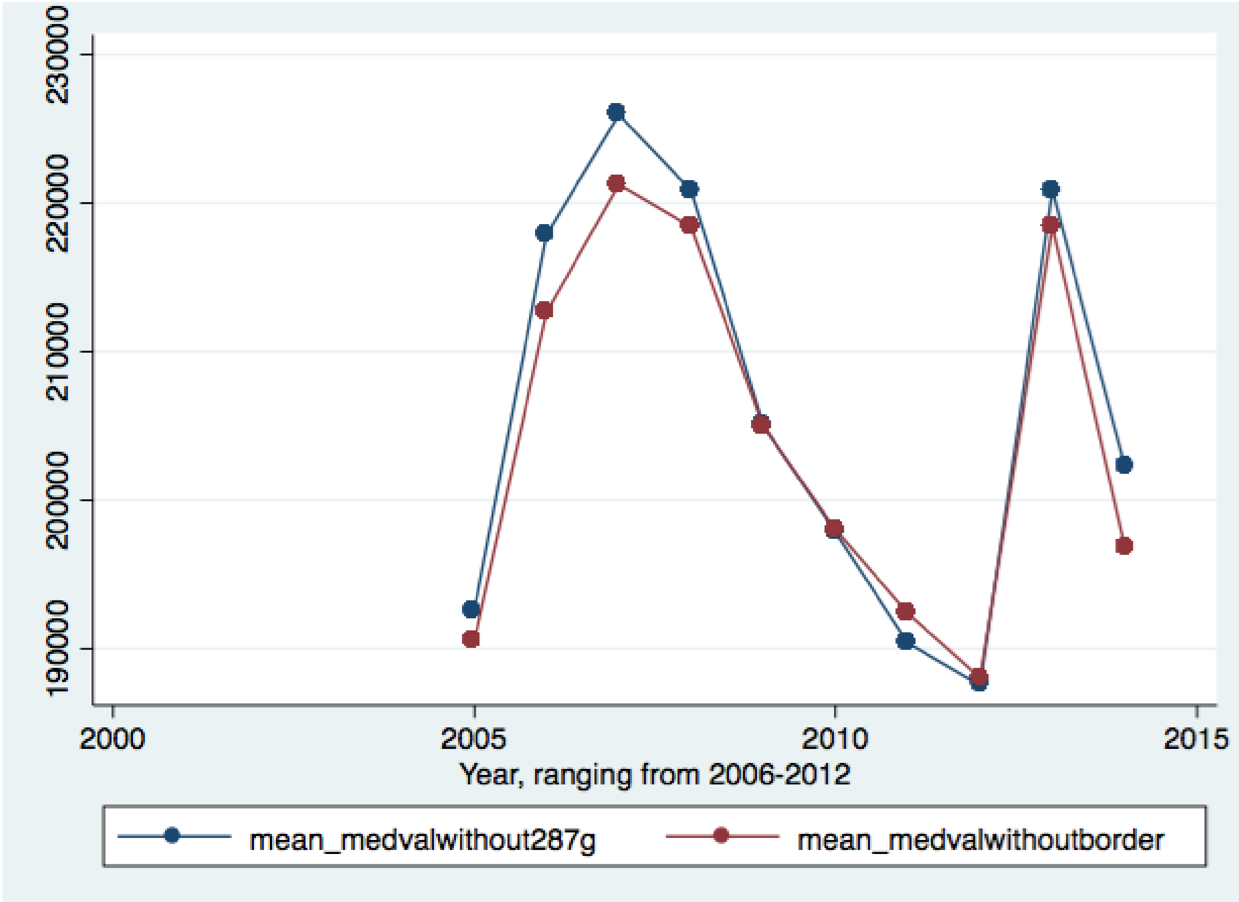

Figure 6: Mean of median housing value, properties without a mortgage

Table A1: County typology comparisons

	Non 287(g) counties mean	287(g) counties mean	287(g) border counties mean
County is:			
farm dependent	0.142***	0	0.050***
	(0.349)	(0)	(0.219)
mine dependent	0.0412***	0	0.010***
	(0.199)	(0)	(0.010)
manufacturing dependent	0.289***	0.211	0.352***
	(0.453)	(0.409)	(0.478)
federal/state government dependent	0.121	0.0916	0.176***
	(0.327)	(0.289)	(0.381)
services dependent	0.105***	0.426	0.191***
	(0.307)	(0.496)	(0.393)
nonspecialized dependent	0.302	0.271	0.211*
	(0.459)	(0.445)	(0.408)
a non-metro recreation destination	0.106***	0.0558	0.0905**
	(0.308)	(0.230)	(0.287)
a retirement destination	0.139***	0.287	0.281
	(0.346)	(0.453)	(0.450)
County has:			
housing stress	0.168***	0.398	0.186***
	(0.374)	(0.491)	(0.389)
low education	0.199***	0.0876	0.166***
	(0.400)	(0.283)	(0.372)
low employment	0.148***	0.0199	0.0704***
	(0.355)	(0.140)	(0.256)
persistent poverty	0.124***	0	0.0452***
	(0.330)	(0)	(0.208)
population loss	0.193***	0	0.0201***
	(0.395)	(0)	(0.140)
persistent child poverty	0.236***	0.0319	0.161***
	(0.425)	(0.176)	(0.367)
n	3093	50	199

Standard deviations in parenthesis

***,**,*Significantly different from 287(g) county at 1%, 5%, and 10% respectively

Table A2: Crime rate comparisons between agriculture-dependent and non-dependent counties

	Non-farm dependent	**Farm dependent**
	mean	mean
	(sd)	(sd)
Murder	4.566***	0.418
	(24.07)	(2.322)
Rape	8.995***	0.902
	(30.07)	(3.994)
Robbery	35.83***	1.327
	(195.4)	(6.975)
Assault	148.0***	13.24
	(760.1)	(94.54)
Burglary	91.31***	9.175
	(346.2)	(35.00)
Larceny	377.9***	20.36
	(1140)	(66.82)
Motor vehicle theft	46.91***	3.191
	(301.5)	(16.78)
Arson	5.296***	0.466
	(15.99)	(1.497)
Weapons charges	52.53***	4.041
	(232.5)	(23.74)
Drug violations	507.5***	45.05
	(2206)	(257.0)
Liquor-related violates	217.2***	25.98
	(678.2)	(61.37)
Disorderly conduct	229.9***	13.97
	(841.8)	(29.67)
Vagrancy	9.560***	0.268
	(93.39)	(1.315)
n	2704	340

Standard deviations in parenthesis;
***,**,*Significantly different from farm dependent county at 1%, 5%, and 10% respectively

Appendix B: Alternate standard errors

Table B1: Impact of 287(g) authorization on select ARMS variables: ERS region clustered S.E.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Labor exp	evfuelo	hfruit	hveg	hmech
g287_authorize	271,350	124,042**	-2,029***	-54.78***	-318.5***
	(171,758)	(52,432)	(230.5)	(11.09)	(70.03)
g287_border	-64,321***	-22,555*	427.8***	23.53*	35.31
	(23,776)	(13,388)	(85.35)	(12.44)	(21.87)
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES
Observations	42,869	42,869	42,869	42,869	42,869
Number of farms	23,198	23,198	23,198	23,198	23,198

Standard errors robust to correlation at the state level in parentheses

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

Source: USDA ARMS; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table B2: Impact of 287(g) authorization on select Census variables: ERS region clustered S.E.

	(1) Num. workers	(2) Labor exp.	(3) Fuel exp.	(4) Veg. acres	(5) Mach. val.†	(6) Net income†	(7) Asset val.†	(8) Acres op.	(9) Num. farms
287(g) authorization	-5,854**	-21,863,629***	9,281,722*	-4,430**	-23,142***	-28,796***	-4,116	-57,519**	55.6
	(2,574)	(5,486,591)	(5,303,006)	(1,756)	(6,100)	(7,599)	(146,011)	(25,522)	(74.7)
287(g) border county	-1,061	-4,800,459	2,361,997***	-1,012**	22,145***	13,659***	258,727**	-8,448	-37.6
	(678)	(12,676,644)	(883,582)	(427)	(7,728)	(4,796)	(128,736)	(7,606)	(45.5)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,025	7,326	8,110	5,903	8,151	8,051	8,151	7,815	8,167
Number of fips	2,693	2,538	2,717	2,089	2,720	2,696	2,719	2,643	2,723

Standard errors robust to correlation at the agricultural production region in parentheses
*** p<0.01, ** p<0.05, * p<0.1
†Outcome measured per operation.
Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table B3: Impact of 287(g) authorization on select ARMS variables: Hetereoskedasticity robust S.E.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Labor exp	evfuelo	hfruit	hveg	hmech
g287_authorize	271,350	124,042***	-2,029	-54.78	-318.5***
	(190,128)	(36,956)	(2,097)	(57.19)	(97.31)
g287_border	-64,321	-22,555***	427.8	23.53	35.31*
	(43,239)	(6,968)	(441.2)	(15.99)	(21.41)
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES
Observations	42,869	42,869	42,869	42,869	42,869
Number of farms	23,198	23,198	23,198	23,198	23,198

White-corrected standard errors in parentheses

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

Source: USDA ARMS; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table B4: Impact of 287(g) authorization on select Census variables: Hetereoskedasticity robust S.E.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Num. workers	Labor exp.	Fuel exp.	Veg. acres	Mach. val. †	Net income †	Asset val. †	Acres op.	Num. farms
287(g) authorization	-5,854**	-21,831,558	9,282,862*	-4,430	-23,099***	-28,747*	-3,809	-57,537**	55
	(2,866)	(32,644,592)	(4,832,072)	(5,043)	(3,884)	(15,770)	(92,656)	(22,432)	(58.9)
287(g) border county	-1,061*	-4,809,285	2,362,065**	-1,011	22,103***	13,624***	258,591***	-8,451	-37.5
	(587)	(8,061,640)	(1,050,245)	(1,236)	(3,083)	(4,557)	(68,677)	(5,642)	(27.9)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,070	7,456	8,138	6,233	8,165	8,082	8,178	7,879	8,180
Number of fips	2,722	2,660	2,729	2,406	2,726	2,719	2,730	2,703	2,728

Robust standard errors in parentheses
*** p¡0.01, ** p¡0.05, * p¡0.1
†Outcome measured per operation.
Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table B5: Impact of 287(g) authorization on select Census variables: County clustered S.E.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Num. workers	Labor exp.	Fuel exp.	Veg. acres	Mach. val.†	Net income†	Asset val.†	Acres op.	Num. farms
287(g) authorization	-5,854**	-21,831,558	9,282,862*	-4,430	-23,099***	-28,747*	-3,809	-57,537**	55
	(2,865)	(32,629,698)	(4,829,702)	(5,040)	(3,882)	(15,762)	(92,611)	(22,420)	(58.8)
287(g) border county	-1,061*	-4,809,285	2,362,065**	-1,011	22,103***	13,624***	258,591***	-8,451	-37.5
	(587)	(8,057,962)	(1,049,730)	(1,235)	(3,081)	(4,555)	(68,643)	(5,639)	(27.9)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,049	7,338	8,134	5,923	8,163	8,063	8,175	7,821	8,179
Number of fips	2,701	2,542	2,725	2,096	2,724	2,700	2,727	2,645	2,727

Standard errors robust to correlation at the county level in parentheses

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

†Outcome measured per operation.

Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table B6: Impact of 287(g) authorization on select Census variables: Spatial S.E.

	(1) Num. workers	(2) Labor exp.	(3) Fuel exp.	(4) Veg. acres	(5) Mach. val.†	(6) Net income†	(7) Asset val.†	(8) Acres op.	(9) Num. farms
287(g) authorization	-5,975***	-21,556,225	9,455,347***	-4,579	-23,477*	-27,592	-3,874	-57,648***	56
	(1,146)	(21,455,399)	(2,462,895)	(3,274)	(13,376)	(25,121)	(253,714)	(15,859)	(134)
287(g) border county	-1,111***	-4,479,338	2,413,184***	-817	22,177***	13,600***	258,589***	-7,629**	-37.1
	(212)	(8,532,611)	(531,547)	(624)	(4,035)	(5,188)	(89,969)	(3,863)	(39.8)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,070	7,456	8,138	6,233	8,165	8,082	8,178	7,879	8,180
Number of fips	2690	2485	2713	2078	2722	2694	2726	2626	2727

Spatial standard errors robust to correlation within 200 miles of the county centroid in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
†Outcome measured per operation.
Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Appendix C: Additional Robustness Checks

C.0.1 Alternative control groups

Table C1: Impact of 287(g) authorization on select ARMS variables: All regions

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Labor exp	evfuelo	hfruit	hveg	hmech
g287_authorize	275,220***	127,788**	-2,019	-58.77***	-331.9
	(101,339)	(52,878)	(1,454)	(19.17)	(243.5)
g287_border	-63,656*	-24,989**	418.4	23.50*	22.46
	(34,248)	(12,292)	(319.5)	(13.54)	(41.19)
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES
Observations	47,646	47,646	47,646	47,646	47,646
Number of farms	25,845	25,845	25,845	25,845	25,845

Standard errors robust to correlation at the state level in parentheses

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

Source: USDA ARMS; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table C2: Impact of 287(g) authorization on select Census variables: All agricultural production regions

	(1) Num. workers	(2) Labor exp.	(3) Fuel exp.	(4) Veg. acres	(5) †Mach. val.	(6) †Net income	(7) †Asset val.	(8) Acres op.	(9) Num. farms
287(g) authorization	-5,855**	-20,434,900	9,173,549*	-4,440	-26,026***	-31,868**	-30,709	-57,502**	55.4
	(2,863)	(32,592,211)	(4,827,832)	(5,039)	(4,332)	(15,769)	(94,671)	(22,389)	(58.8)
287(g) border county	-1,065*	-5,540,191	2,481,048**	-1,011	26,627***	17,759***	287,497***	-8,278	-37.3
	(590)	(8,072,141)	(1,051,944)	(1,239)	(3,204)	(4,583)	(69,099)	(5,651)	(27.9)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	9,097	8,423	9,168	6,731	9,192	9,105	9,208	8,898	9,209
Number of fips	3,066	3,001	3,073	2,624	3,069	3,062	3,074	3,046	3,071

Standard errors robust to correlation at the state level in parentheses
*** p¡0.01, ** p¡0.05, * p¡0.1
†Outcome measured per operation.
Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table C3: Impact of 287(g) authorization on select ARMS variables: States with 287(g) counties only

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Labor exp	evfuelo	hfruit	hveg	hmech
g287_authorize	215,486***	106,065***	-2,212	-66.57***	-326.7
	(65,074)	(38,453)	(1,596)	(23.49)	(241.5)
g287_border	-57,159	-21,803*	498.1	25.66*	39.36
	(37,120)	(11,312)	(380.8)	(15.03)	(44.36)
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES
Observations	21,800	21,800	21,800	21,800	21,800
Number of farms	11,753	11,753	11,753	11,753	11,753

Standard errors robust to correlation at the state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C4: Impact of 287(g) authorization on select Census variables: States with 287(g) counties only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Num. workers	Labor exp.	Fuel exp.	Veg. acres	†Mach. val.	†Net income	†Asset val.	Acres op.	Num. farms
287(g) authorization	-5,930**	-20,455,738	9,730,840**	-4,360	-18,257***	-20,824	22,680	-63,544***	47.1
	(2,913)	(33,176,217)	(4,913,852)	(5,140)	(3,077)	(15,940)	(80,458)	(22,970)	(59.5)
287(g) border county	-1,195*	-4,572,497	2,650,309**	-1,208	9,160***	6,626	69,213	-8,282	-43.8
	(682)	(8,731,691)	(1,176,098)	(1,418)	(2,630)	(5,079)	(72,034)	(6,534)	(33.9)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	3,849	3,511	3,875	2,996	3,904	3,848	3,903	3,741	3,907
Number of fips	1,297	1,270	1,302	1,143	1,302	1,297	1,302	1,288	1,303

Standard errors robust to correlation at the state level in parentheses

*** p¡0.01, ** p¡0.05, * p¡0.1

†Outcome measured per operation.

Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table C5: Impact of 287(g) authorization on select ARMS variables: Farms with net worth less than $5,000,000

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Labor exp	evfuelo	hfruit	hveg	hmech
g287_authorize	215,566	65,271	-3,667	-76.59**	-231.6
	(144,092)	(50,027)	(2,238)	(38.91)	(149.2)
g287_border	-42,005	-14,582	732.1	26.82**	34.65
	(40,223)	(10,231)	(508.9)	(10.78)	(26.48)
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES
Observations	38,077	38,077	38,077	38,077	38,077
Number of farms	21,863	21,863	21,863	21,863	21,863

Standard errors robust to correlation at the state level in parentheses

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

Source: USDA ARMS; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table C6: Impact of 287(g) authorization on select ARMS variables: Farms with net worth less than $100,000 per acre

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Labor exp	evfuelo	hfruit	hveg	hmech
g287_authorize	293,488**	150,681**	-2,002	-66.65***	-333.0
	(115,594)	(70,375)	(1,585)	(25.03)	(264.1)
g287_border	-60,386*	-25,452*	402.1	25.65*	36.91
	(31,829)	(14,700)	(329.4)	(14.00)	(44.02)
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES
Observations	38,077	38,077	38,077	38,077	38,077
Observations	42,050	42,050	42,050	42,050	42,050
Number of farms	22,965	22,965	22,965	22,965	22,965

Standard errors robust to correlation at the state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Source: USDA ARMS; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

C.0.2 Other robustness checks

Table C7: Impact of 287(g) authorization on select Census variables: Jail occupancy measured as a share

	(1) Num. workers	(2) Labor exp.	(3) Fuel exp.	(4) Veg. acres	(5) †Mach. val.	(6) †Net income	(7) †Asset val.	(8) Acres op.	(9) Num. farms
287(g) authorization	-795	-10,742,666	-414,701	-140	-19,953***	-32,975***	-16,680	-12,113	19.5
	(538)	(6,938,468)	(702,844)	(255)	(5,789)	(9,441)	(205,101)	(7,405)	(26.1)
287(g) border county	-94.5	-3,100,690	628,698*	7.35	22,651***	12,330***	230,958***	1,551	-37.5
	(76.2)	(5,403,053)	(363,687)	(83.4)	(3,050)	(3,547)	(67,159)	(2,426)	(29.4)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	7,400	6,874	7,445	5,804	7,469	7,408	7,480	7,238	7,478
Number of fips	2,490	2,443	2,496	2,231	2,493	2,489	2,497	2,475	2,493

Standard errors robust to correlation at the state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
†Outcome measured per operation.
Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Table C8: Impact of 287(g) authorization on select Census variables: Controlling for state-year FE

	(1) Num. workers	(2) Labor exp.	(3) Fuel exp.	(4) Veg. acres	(5) †Mach. val.	(6) †Net income	(7) †Asset val.	(8) Acres op.	(9) Num. farms
287(g) authorization	-4,412	-173,710,418**	-5,631,813	-3,370	-29,534***	-39,822*	-664,506***	-51,917**	44.5
	(2,851)	(78,034,823)	(6,310,661)	(5,278)	(10,382)	(20,935)	(226,404)	(26,146)	(65.3)
287(g) border county	-804	-36,986,448**	-592,822	-747	8,655**	4,856	-35,679	-4,584	-19.8
	(541)	(17,346,684)	(1,194,951)	(1,237)	(3,723)	(5,841)	(85,747)	(5,891)	(27.6)
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State*Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,070	7,456	8,138	6,233	8,165	8,082	8,178	7,879	8,180
Number of fips	2,722	2,660	2,729	2,406	2,726	2,719	2,730	2,703	2,728

Standard errors robust to correlation at the state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
†Outcome measured per operation.
Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures

Falsification tests

Table C9: Census results placebo test: 2002 as the year of treatment

	(1) Num. workers	(2) Labor exp.	(3) Fuel exp.	(4) Veg. acres	(5) Mach. val. †	(6) Net income †	(7) Asset val. †	(8) Acres op.	(9) Num. farms
287(g) authorization	-	262,036,423	26,416,519*	7,870	-21,547	-	-1,233,742	63,666	1,532
		(245,906,110)	(13,660,574)	(13,745)	(128,124)		(1,897,080)	(71,628)	(1,921)
287(g) border county	-	-48,690,065	-4,930,905*	-1,908	-1,884	-	224,112	-18,112	-301
		(48,129,853)	(2,618,361)	(3,177)	(23,773)		(352,494)	(14,885)	(370)
County FE		YES	YES	YES	YES		YES	YES	YES
Year FE		YES	YES	YES	YES		YES	YES	YES
State authorization		YES	YES	YES	YES		YES	YES	YES
Observations	-	6,779	7,206	5,481	7,226	-	7,225	7,006	7,232
Number of fips	-	2,381	2,410	2,092	2,411	-	2,412	2,396	2,411

Standard errors robust to correlation at the state level in parentheses.

*** p¡0.01, ** p¡0.05, * p¡0.1

†Outcome measured per operation. Number of workers and net farm income are not reported in 1997.

Source: US Census of Agriculture; Bureau of Justice Statistics; ICE FOIA Proactive Disclosures