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

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Selected Paper prepared for presentation at the 2016 Agricultural & Applied Economics Association Annual Meeting, Boston, Massachusetts, July 31-August 2

Abstract

This study provides the first causal estimate of the impact on an 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 through the Delegation of Immigration Authority 287(g) program on various indicators of labor supply shocks and profitability. The potential endogeneity of participation in the 287(g) program is addressed by using county jail occupancy as an instrumental variable. Our results are consistent with labor supply shocks and suggest a long-run 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 technology and native workers are at best partial substitutes for immigrant farm workers.

1 Introduction

Studies on the economy-wide effects of immigration have largely focused on the impact of immigration on the wage and earnings impacts for both immigrant and native workers ((Chiswick, 1978), (Borjas, 1987), (Ottaviano and Peri, 2012)). However, impacts on firms have received less attention, although the popular media has widely covered the negative impacts of rising anti-immigrant sentiment on business owners, including farmers, in the last two decades. Popular sentiment has also become increasingly anti-immigrant, and in some areas sentiments have spawned action, with localities passing ordinances requiring proof of legal residency to obtain housing or making English 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, permitting local officials to, for example, check a person's immigration status during a routine traffic stop. It therefore provides guidelines for how a local law enforcement agency can enforce national immigration policy in the area under its jurisdiction. Although 287(g) has been on the books since 1996, it has only been in the last decade, prompted at least in part by this growing anti-immigrant rhetoric, that law enforcement agencies have signed MOU agreements with US Immigration and Custom Enforcement (ICE) and thus have been enrolled in the program.

The introduction of 287(g) programs provides a unique opportunity to explore a negative labor supply shock. While the program's stated intent was to identify and remove only dangerous undocumented people, the extent to which this is true has been heavily debated, with those opposed to its widespread implementation saying that it provides a path for local law enforcement to racially target and harass residents of their jurisdictions (Shahani and Greene, 2009). More important even than the direct removal of immigrant labor that this program authorizes, which is non-trivial in many places, is the signal having such a policy sends to potential immigrants considering where to locate for the growing season. Agricultural workers are largely made up of immigrants (Zahniser et al., 2012), and the seasonality of their work means that they may have some level of geographical flexibility. 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. Through both direct action and indirect signalling, therefore, implementation of 287(g) has already been shown to decrease the local immigrant population and labor supply (Watson, 2013), (Kostandini et al., 2014).

In addition to shifting the wage rate, firms make other changes in response to labor supply shocks. Industries where it may be difficult to find native workers who are willing and able to perform the jobs, such as the farm sector, may undergo fundamental changes in response to immigration policy shifts. Further, changes seem to occur within the sectors of an economy, rather than between the sectors: research indicates that the broad sectoral composition of an area's economy, or the percent of an area's GDP coming from agriculture, manufacturing, and services, does not change based on levels of immigration. In examining sector level response, Lewis (2003) finds that labor supply shocks do not affect the local sector mix; rather, increases in the supply of a type of labor, such as low-skill immigrant workers, tend to increase relative factor intensity with no effect on wages. This result is consistent with theoretical models of endogenous technical change, and for the agricultural sector, suggests that a decrease in the supply of farm workers may cause substitution away from labor inputs.

However, the ability of agricultural producers to substitute completely for low-skill immigrant workers is limited. Certain tasks are not mechanized, and native workers may be unwilling to work in agriculture, even for wages higher than the minimum wage. The limits of technology as perfect substitutes for labor, especially in the agricultural sector, may actually drive some firms into reducing production levels. Additionally, the assumptions that Lewis (2003)'s model rely on, including no trade barriers and identical technology, are unlikely to hold for the agricultural sector. For one, the movement of agricultural labor is not free, in the sense that heterogeneous levels of immigration enforcement serve as a trader barrier. Different types of labor (i.e. immigrant and native) might not be considered perfect substitutes, and so the "identical technology" assumption is also unlikely to hold. Further, mechanization of farming, or substitution of capital, may not be feasible for all types of farm labor.

This study moves beyond estimating effects on local immigrant population levels to estimating impacts on firm production decisions by taking advantage of the natural experiment provided by spatial and temporal variation in 287(g) 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 use a unique and robust instrumental variables strategy to provide one the first estimates of the causal economic impact of immigration policy on U.S. firms. Unauthorized workers make up a large share of the labor force for several domestic industries, and the debate on U.S. immigration policy covers the economic impacts for workers and businesses as well as fiscal implications. Several studies have considered the impact of various aspects of immigration policy on domestic wages, employment and government spending, but few studies have rigorously estimated costs for U.S. businesses. Such costs 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.

1.1 Immigration Literature

Studies on the impact of immigration on wages have mixed results. Dustmann et al. (2013) explores the impact of immigration along the distribution of wages, finding that immigration does depress native wages below the 20th percentile, but increases wages for wages at the higher end of the distribution, with an overall slightly positive effect of immigration on wages. Based on the magnitude of these effects, the authors hypothesize that immigrant workers are paid less than the value of their contribution to production, which generates a surplus in production that they are unable to capture, lacking the bargaining power of a native workforce. In contrast, Borjas (2003) finds a small, but significant, negative impact of immigration on wages. A key component of this result, however, is that immigration harms the employment and wage outcomes of *competing* workers: an increase in the labor supply of immigrant workers reduces wages for native competing workers by 3-4%. As such, this work does not directly address what would happen in an industry, like agriculture, that does not necessarily have "competing" workers, where instead other factors of production, like mechanization are partially competing with immigrant workers.

Regardless of the outcome considered, overcoming endogeneity and providing causal identification of the impact of immigration remains the foremost challenge to understanding the impact of immigration. A key feature of previous work on the effects of immigration is Borjas's critique that "practically all empirical studies in the literature" ((Grossman, 1982)) 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) points out that this method ignores the endogeneity inherent in the relationship between local conditions and the supply of immigrants there. In other words, it is challenging to determine if local economic conditions, such as a rising wage rate, pulled more immigrants to a certain locale or if the arrival of immigrants caused the increasing wage rate. Papers have overcome this endoegeneity in two ways: either by exploiting an unexpected exogenous shock to the immigrant population, like the Mariel boatlift (Card, 1990), or by instrumenting for the current immigrant population level using past populations of immigrants from the same place, relying on the enclaving tendency of immigrants to settle in the same place over time (Card, 2009). The former occurs infrequently, and even then typically only in one place, affecting external validity, while the latter is really only effective for urban areas where enclaves are allowed to form. It remains a challenge, therefore, to find a valid instrumental variable for a rural area's immigrant labor supply, which tends to be more transient and informal than that in urban areas. Given the seasonality of farm work, rural immigrant workforces are less likely to stay in one place long enough for enclaves to form.

1.2 Farm Labor and Immigration

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). Increased enforcement of existing immigration policy may lead to labor supply shortages for agriculture if alternative labor pools are not available at feasible wage levels. Likewise, if mechanization or substitution of capital is not feasible, enhanced immigration enforcement may threaten farms producing livestock or labor-intensive crops across the U.S. While significant mechanization of U.S. agriculture has occurred over 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 are localized shortages of farm labor across the U.S., and Hertz and Zahniser (2013) identified several counties with wage growth of over 40 percent where agricultural employment had fallen. A simulation analysis of an improved temporary guest worker program or a large decline in the number of undocumented workers found a 1-2 percent increase in the output of labor-intensive agricultural sectors and 2-5 percent decline, respectively (Zahniser et al., 2012). These hypothetical estimates indicate that immigration policies can have a economically meaningful impact on the farm sector.

Similar to the general literature on labor and immigration policy, studies on farm labor issues have focused on wages, labor supply, and migration decisions. Tran and Perloff (2002) analyzed the decision of workers to stay in agriculture after the 1986 Immigration Reform and Control Act (IRCA). Kandilov and Kandilov (2010) and Sampaio et al. (2013) considered the impact of legal status on farm wages. Fan et al. (2015) confirmed the importance of demographics in driving agricultural labor migration, in addition to structural factors such as economic conditions and government policies. Taylor et al. (2012) predicted that that supply of U.S. agricultural labor from Mexico will decline in the future. Buccola et al. (2012) estimate the effect of immigration enforcement on the minimum wage paid to workers in Oregon's horticultural sector, using attempted border crossings as a proxy for immigrant-sourced farm labor in Oregon. 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. These studies jointly indicate many factors that drive farm labor markets and provide insight into these markets. However, they do not attempt to make causal claims about how immigration enforcement impacts the agricultural sector at the firm level.

Fisher and Knutson (2013) note that these and other studies have had varying results, likely because farm labor markets, and hence shortages, are largely local instead of national, are based on local weather and biological conditions, and often have limited or differential labor mobility. Empirical analysis of immigration policies hence needs to control for this heterogeneity. As with other industries, analysis of the impacts of immigration policy requires both detailed data as well as effective identification of local enforcement. Both of these issues present a major challenge for research on firm-level impacts. Kostandini et al. (2014) find an association between authorization of counties to enforce immigration policy and and county-level net farm income in the short term, but do not address the potential endogeneity of local enforcement: local enforcement drives immigrant populations, but at the same time, immigrant population drives enforcement. However, with sufficient firm-level data, the farm sector does provide a unique opportunity for causal identification of firm-level impacts. While other sectors that utilize undocumented labor are closely tied to general economic conditions, such as hospitality and construction, the farm sector is only weakly tied to general economic conditions. During the recession that began in 2008, U.S. farms were much less effected than the rest of the economy (Shane et al., 2009). In the next section we discuss how we take advantage of the unique characteristics of the farm sector and national firm-level unbalanced panel data to analyze the firm-level impacts of local immigration enforcement.

2 Data and Empirical Model

2.1 Farm Survey Data

Data on farm structure and profitability comes from the two national farm surveys, both conducted by the United States Department of Agriculture (USDA). The Agricultural Resource Management Survey (ARMS) is the only annual nationally representative annual farm survey. This cross sectional survey collects detailed data on production activities, farm income and finances, and household characteristics. In most years approximately 20,000 farms complete the ARMS. Data collected from ARMS informs official agricultural statistics and has supported a wide body of research. Approximately 12 journal articles are published annually using ARMS data, in both agricultural economics journals as well as general audience economic journals (Kuethe and Morehart, 2012).

Due to its large sample size relative to the U.S. farm population, since its inception in 1996 over 60,000 farms have been included in ARMS at least twice. Weber et al. (2015) 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 and enforcement on various measures of farm structure and profitability. 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 and rely on hired labor. These farm types tend to be relatively large, and hence this sampling design provide us with a larger number of farms potentially impacted by immigration policy.

While survey weights are provided for ARMS to calculate population-level statistics, due to survey design they are only applicable to single-year analysis. Hence our estimates will representative of farms that were randomly sampled more than once over our study period (1996-2012). Farm attrition is a potential concern, as well as the overall representativeness of our sample. We address these and other issues by replicating our analysis with publicly available county-level tabulations of Census of Agriculture data, which is collected from all U.S. farms every five years. While not as detailed as ARMS, the Census of Agriculture county tabulations provide both an opportunity to validate our results as well as confidence that our findings are not driven by idiosyncratic features of the farms represented in ARMS, such as oversampling of large farms. See Tables 1 and 2 for summary statistics for the outcome variables used in our main specifications. Table 1 indicates counties with 287(g) had larger expenses, income and asset values, but lower equipment value. Fruit and vegetables acres did not have a statistically significant difference.

These data sets allows us to consider a variety of potential impacts of 287(g) participation and enforcement, but generally do not include information on prices paid or received, wages, or yields. However several variables do allow us to consider various impacts: (1) short term financial impacts (2) production and investment decisions and (3) capitalization into asset values. Under (1), we consider farm expenses that would be most directly affected by a labor supply shock. This includes expenditure for labor and fuel; as prices, quantities, and wages are not collected in the surveys. We also consider net farm income. While we would expect labor costs to increase in the short term, the impacts on labor and fuel expenditure may be uncertain because equilibrium prices and quantities may move in different directions. Further, immigration restrictions could affect local markets for agricultural products to the degree that short-term shortages lead to temporary prices increases. Due to the unbalanced panel structure we may be observing a farm a few years after the policy was first implemented, which means our estimates will reflect average medium term impacts. For these reasons, we are also interested in production decisions (which the operator has relatively more control over than price levels) and asset values. For (2), we consider production and investment decisions such as acres of fruit and vegetables harvested, debt levels, and machinery and equipment asset values. These measures all reflect production decisions made by farm operators that could be impacted by labor costs and availability and have long term implications for farm profitability. Acres harvested of fruit, vegetables and other crops are available in the ARMS and Census data as well as equipment asset value measures, but only ARMS collects information on farm debt. For (3), we consider different measures of farm asset values, including farm real estate value per acre operated and net worth. Farm asset values reflect market expectations for long-term profitability of the farm business, with labor shortages potentially being capitalized into asset values.

2.2 Immigration Enforcement Data

In 2007, twenty-five (25) counties signed MOUs with ICE and enrolled in the 287(g) program. By 2010, the number had reached its peak with fifty-one (51) 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 th 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. 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, as well 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.

2.3 Empirical Model

We treat all farm-level variables as a function of our treatment (immigration discouragement or enforcement), because in equilibrium a major shift in labor supply or costs could affect almost all production and financial decisions, as well as operator characteristics. We also control for whether or not a non-participating county bordered a 287(g) county, because there could be various spillovers. While it is uncertain *a priori* whether 287(g) would increase or decrease labor supply and cost in bordering countries, this is an important control due to the potential impact. We use a farm-level fixed effects model, as basic farm-level characteristics such as production activities and size have a strong relationship with labor use. Our panel is unbalanced, but we expect no bias, as farms are observed based on random selection into ARMS. 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 participation and 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:

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

This basic farm or county-level fixed effects regression may not provide causal estimates of the effect of enforcement because enforcement might be correlated with economic trends that also influence farm decisions and profitability. The extent of enforcement and the presence of immigrants in a jurisdiction itself could be subject to reverse causality: a high immigrant population could make stricter enforcement 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 fixed effects controls for farm characteristics and static local political conditions, we cannot disprove that broader social or economic trends might be 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).

2.4 Instrumental Variables Strategy

To resolve the potential endogeneity of 287(g) participation, we use an instrumental variable (IV) approach, with the county's aggregated occupancy of jails and prisons in 2006 as the instrument.[1] In addition to offering local law enforcement a "political trophy in local law enforcement campaigns", 287(g) offers counties or other jurisdictions the potential for financial gains as well. There are real 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 the case, 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. Included in the data set is information on the rated capacity of

[1]For cities or townships with 287(g) policies, we use jail occupancy levels of the county in which the city is located.

the jail, or the total number of inmates the facility can hold, and the total population of inmates. These are recorded at the facility level, and so are aggregated up to the county level, giving the total population and rated capacity of prison facilities in each county. 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 a relatively emptier facility. Although the data is yearly, we use the occupancy of prisons and jails in a county in 2006 as the instrument. In 2006, no rural county had begun acting on their 287(g) mandate, and the majority of 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 not significant 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 in any significant way.

Our estimating equation uses 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}:

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

2.4.1 Exclusion Restriction

The validity of jail occupancy as an instrumental variable rests on the plausibility of farm-level management decisions being unrelated to this measure of jail capacity. 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. There is nothing 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. Indeed, such an effect would have to impact these counties differently from their neighbors but affect the 287(g) program group, which represents a variety of agricultural systems, the same. Our results, below, indicate that bordering a 287(g) county had the opposite effect of participation, indicating substantial spillovers from a county with a stricter enforcement regime to one that is relatively less strict.

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 3, which shows that many differences exist between 287(g) counties and counties without the program. However, these differences serve to 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 (2015) for more information on this data set.) 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 have negative impacts on agriculture and 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 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. 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 nonfarm economy.

It is also possible that changes in housing values, or in other non-agricultural property values, are driving these results. 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 any 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 **??** shows the trend across the study period for properties with a mortgage and Figure **??** 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.

There is evidence to 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. Therefore, it could be argued that increasing temperature would affect jail occupancy and violate our exclusion restriction. This, however, is likely not the case: increasing temperatures, especially in the southern part of the US where 287(g) programs are concentrated, inhibit rather than enhance agricultural production. Greater precipitation, which

[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.

dampens levels of crime according to some studies, increase yields for agricultural commodities. Yields, on the other hand, decline with temperature (Rosenzweig et al., 2001). 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. 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. Complications that make comparison of, say, growing season temperature between these counties actually themselves strength the exclusion restriction: because the counties that enacted 287(g) are spread across the country, the growing seasons and in each county differ (see 1). In order for weather to have an impact, therefore, it would have to affect different counties with different weather patterns and agricultural systems in the same way.

There is little evidence to suggest that farm workers, whether they are native or immigrant workers, commit crime 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. Comparison of the means across these two groups, for a selection of reported crimes, can be found in Table 4. Extensive research on the relationship between immigration and crime has been conducted (Bell et al., 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 et al. (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 work force. Models of the impact of immigration on crime, therefore, penalize behavior, like criminal acts, that makes a person 'stick out:' because of the penalties an undocumented worker would face, these models predict lower rates of crime from undocumented workers. 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.

3 Results

Our main results using farm-level data are reported in Table 5. We report coefficients for selected variables representing short and medium term impacts as well as production response. Of the 25,000 farms in our sample, approximately 500 are located in treated or 287(g) counties. We first observe that not only did 287(g) authorization have a statistically significant and large impact for several measures in counties where it was implemented, but that in many cases counties that were adjacent to a 287(g) county experienced a statistically significant but opposite impact. This suggests substantial labor supply spillovers, with undocumented workers moving to nearby counties without 287(g) authorization, where production practices are likely to be similar. Authorization is our preferred indicator of participation in 287(g), as other studies have indicated that authorization alone had a substantial impact on immigration levels, likely through deterrence. Therefore, we report the primary results using an indicator variable, where 1 indicates that the county has authorized 287(g) in that year.

After 287(g) authorization, farms experienced both a statistically significant and large increase in labor and fuel expenses, with a decrease in these expenses in adjacent counties. While the impact on fruit acres harvested was not statistically significant, a statistically significant decline of almost 59 vegetable acres was experienced in 287(g) counties, and an increase of more than 23 acres in adjacent counties. The remaining variables (income, debt, net worth, equipment values and real estate value per acre operated) are less direct measures of impact, but aggregate several survey responses. Nevertheless, we find weak evidence for increasing debt levels and lower real estate values in 287(g) counties.

To explore county-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 6 reports these results, with the first stage results in Table 7. Although it is not a perfect replication, 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 farm asset values. Together, farm- and county-level results show evidence of a negative labor shock in the short term, and indicate that farms are making longer-term decisions that will ultimately affect their profitability and operations.

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 does not extend to all tasks. Increases in fuel expenses indicate that farm operators had to replace farm labor with equipment. No statistically significant change in fuel expenses is detected in bordering counties when using aggregate data, while a statistically significant decline 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 long-term investments in assets (mainly real estate) and machinery have significantly declined in 287(g) counties as well. Farm operators in these counties may be divesting their operations of durable assets or farmland values may be declining, both which signal low confidence about the viability of farm operations given the current climate. Without the security of being able to rely on a stable, experienced workforce, farm operators in counties with strong anti-immigration policies are disinclined to seriously invest in their operations, relative to non-adjacent counties without these policies. Their losses are further highlighted by the significant decline in net income experienced by operations in counties with 287(g); at the same time, 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 and so may experience a positive demand shock. These measures rely on the combination several survey responses in both ARMS and Census, so county-level aggregations for these "aggregate" variables may be more reliably measured than for individual surveys (ARMS).

The Census also provides county-level aggregate outcomes that contribute to our understanding of what is occurring in these counties. The results using these outcomes, in Tables 8 and 9, show statistically significant declines in the total number of workers and the number of acres operated in a county. Together, these results support our conclusion that farmers in these counties faced a negative labor supply shock that was not experienced in neighboring counties. Further, the area under cultivation declined: 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 temporarily. There is no significant impact on the number of farms in a county, so it is not possible to say whether the 287(g) program drove some operators out of agriculture entirely. Taken together, however, these results indicate that 287(g) changed the nature of agriculture in these counties, while leaving neighboring counties untouched or even better off.

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. The overall results are consistent with clustering the standard errors at either the state level, shown in Tables 5 and 10 or the agricultural region level, in Appendix Tables A1 and A2. However, neither of these levels of clustering are entirely appropriate. Agronomic conditions, weather, and cropping patterns are not constrained or contained by state 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 region boundaries often cross states and there may be some within-state correlation of standard errors, for example related to law enforcement practices. The small number of agricultural regions is also an issue for estimating clustered standard errors. The wild cluster bootstrap standard error developed by Cameron and Miller (2015) allows consistent estimation when there is a limited number of clusters. Future work will report t-statistics for our main regression coefficients using a wild cluster bootstrap for agricultural region-clustered standard errors. We will also explore estimation of spatial standard errors, which assume that counties within a certain distance of each other will be affected similarly and uniformly within that distance.

3.1 Robustness Checks

Alternative specifications test whether these results derive from a particular set of measures or control farms. 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. We consider the magnitude of 287(g) participation by estimating our main specification using measures of local enforcement levels for each county-year observation in place of of 287(g) authorization. These results appear in Table 11 and Table 12. "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 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, relative measures of enforcement 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 level "identified". Further, other work has provided evidence that 287(g) reduced the population of all immigrants in a county, regardless of their documentation status.

While this analysis does not indicate the same level of spillovers and coefficients are difficult to interpret, the direction of coefficients and levels of statistical significance for 287(g) counties reported in Table 11 are consistent with those reported in Table 5. For all of the outcomes considered using Census data (Table 12), the effect of an additional departure is larger than the effect of an additional identification. This is likely due to deportation being a stronger anti-immigration measure than identification. At the county level, we find that every alien deported in a county with 287(g) increases fuel expenses by $3,500. Machinery value per operation declines by $102 for each alien departed. Asset values per operation decline by $821 per alien identified and by more than $1,200 per alien departed. While the coefficients are difficult to interpret given the lack of data on total undocumented population or farm workers, these results highlight the scope of damages to farm operations in counties with more robust anti-immigrant action. Farms are not only suffering short term losses through increased daily operations expenses like fuel costs, they are experiencing an environment that is negatively affecting their long-term investments and profitability.

The implied magnitude of farm-level impacts is large: for example, an increase in labor expenses of over $250,000, as reported in Table 5. To better understand the underlying farm-level heterogeneity driving these results, we run our analysis (1) using natural log of the dependent variables; (2) excluding farms with total net worth greater than $5,000,000; (3) excluding farms with net worth per acre operated over $100,000. We report these results in Tables 13, 14, and 15, respectively. We find some evidence that 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 net worth greater than $5 million are excluded, the only measure that has a statistically significant effect from 287(g) is vegetable acres. These results suggest that large farms, which use more hired labor, are disproportionately affected by 287(g). However, when we exclude farms with high net worth per acre from our analysis (Table 15), the main results persist in terms of both magnitude and statistical significance, except for a counter-intuitive result for real estate value per acre operated. This may be due to changes in both reported acres operated and real estate value. The results are generally relatively smaller but still statistically significant and large in an absolute sense. Overall, we do find consistent results across these specifications. Results using the natural log of the dependent variables are largely not statistically significant in this specification, although almost always in the same direction as the main specification. These weak results may be due to lower levels of variation in an already-small treatment group.

Along with the question of which level is most appropriate to cluster the standard errors, the choice of which untreated counties should serve as the control is not set in stone. The main results designate every untreated, non-adjacent county in the contiguous United States as a control for the 50 counties or county-equivalents that have implemented a 287(g) policy. It is possible that doing so reduces the precision of our estimates by wrongly 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 so does not uniformly represent all of the country's agricultural systems. In Tables 16 and 17, we report our main, farm-level specification with non-participating states and regions excluded, respectively. The corresponding county-level results are found in Tables 18 and 19. Although we see statistically significant results when using the most inclusive control group, these 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 largely results are largely replicated with both of these specification, for both ARMS and Census data.

Most results have the expected sign and indicate substantial short and medium term negative welfare impacts for 287(g) counties. Likewise, neighboring counties appear to be experiencing some benefits due to labor supply spillovers, which would support observations that non-native workers simply moved to more supportive (or less restrictive) jurisdictions. 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 A3 addresses concerns that the instrument merely picks up a linear trend in which larger counties have both greater jail capacity 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 its jails and its total inmate population, on our main results. Further, these results are also robust to the inclusion of state-by-year fixed effects, presented in Table A4.

4 Conclusion

We use an unbalanced panel of confidential nationally representative farm survey data and county-level agricultural census data to estimate how firms 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 farm expenditure and income, we consider various indicators of farm production decisions and profitability. The potential endogeneity of participation in the 287(g) program is addressed by using county jail occupancy as an instrumental variable. We find that jail occupancy is strongly correlated with 287(g) participation and any differences that could lead to violation of the exclusion restriction point to bias in the opposite direction of our results.

County participation in 287(g) leads to local farms experiencing increased labor and fuel expenses and lower vegetable production, with some results suggestive of a decline in short and long-term profitability. At the county level, we observe a decline in the number of farm workers and the total area of land in farms. These results are consistent with a permanent labor supply shock, and are likely driven by impacts on larger farms more likely to use hired labor. Likewise, farms in counties adjacent to those participating in 287(g) appear to have benefited from a positive labor supply shock, with a large 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 do reflect the different populations and survey methodologies.

These results suggest that that neither technology nor native workers are complete substitutes for undocumented farm workers. Further, national policies that increase immigration enforcement or otherwise impose restrictions on immigrant farm labor may lead to declining production levels and lower profitability for U.S. farms that use hired labor.

5 Tables

Table 1: Summary Statistics: ARMS

Variable	287(g) mean	All others mean
Labor expenses ($)	311,709*	146,194
Fuel expenses ($)	54,021*	42,691
Fruit acres harvested	18	32
Vegetable acres harvested	33	29
Net cash farm income ($)	427,856*	258,564
Debt ($)	508,529	451,283
Net worth ($)	3,364,715*	2,699,482
Equipment ($)	258,733*	360,269
Real estate assets ($)	2,829,372*	2,143,243
Number Observations	1,156	50,550

Statistically different at 5% test level indicated by *

Table 2: Summary statistics: Census outcome variables, 2002

	n	mean
Asset value per acre ($/acre)	3,067	2,351 (4,149)
Asset value, total ($)	3,067	373,248,973 (403,756,873)
Contract labor expenses ($)	2,900	1,179,412 (8,466,924)
Hired labor expenses ($)	2,970	34,849,409 (67,292,965)
Fruit sales ($)	1,473	8,944,459 (60,930,059)
Total farm area (acres)	3,078	735,530 (3,943,763)
Net farm income ($)	3,041	13,297,565 (31,812,755)
Livestock sales ($)	2,970	34,849,409 (67,292,965)
Share of fuel expenses (%)	3,055	.0494 (.0199)
Value of machinery per acre ($/acre)	3,067	105 (85)

Standard deviation in parenthesis

Table 3: 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 4: 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

Table 5: Impact of 287(g) Authorization on Selected ARMS Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Labor	Fuel	Fruit	Vege.	Income	Debt	Net Worth	Equip.	Real estate
287(g)	275,220***	127,788**	-2,019	-58.77***	-491,267	1.080e+06*	-7.859e+06	39,976	-66,767*
	(101,339)	(52,878)	(1,454)	(19.17)	(392,094)	(571,084)	(5.847e+06)	(202,169)	(40,041)
Border 287(g)	-63,656*	-24,989**	418.4	23.50*	75,317	-195,883*	970,831	-50,255	10,930
	(34,248)	(12,292)	(319.5)	(13.54)	(75,136)	(100,813)	(1.365e+06)	(32,030)	(8,270)
Constant	128,009***	23,093***	34.76***	37.28***	190,807***	387,475***	1.414e+06***	235,026***	6,728***
	(20,046)	(4,010)	(12.36)	(6.432)	(42,752)	(70,341)	(164,496)	(27,715)	(547.0)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	47,646	47,646	47,646	47,646	47,646	47,646	47,646	47,646	47,645
Number of farms	25,845	25,845	25,845	25,845	25,845	25,845	25,845	25,845	25,844

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

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

Table 6: Main Census Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Vegetable acres	Fuel expenses	Labor expenditure	Net income per operation	Machinery value per operation	Asset value per operation
287(g) authorization	1,968	5,445,916*	-31,885,083	-211,875***	-158,389***	-2,011,241***
	(1,761)	(3,019,628)	(41,654,507)	(36,417)	(34,915)	(464,530)
287(g) border county	-114	-602,623	7,099,182	12,090**	7,641	128,933*
	(297)	(453,978)	(6,525,568)	(5,303)	(5,297)	(70,579)
County FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES
Observations	6,731	9,168	8,423	9,105	9,192	9,208

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7: IV first stage: Census variables

	(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 in parentheses;
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Alternative Census outcomes, using jail occupancy as the instrument

	(1)	(2)	(3)	(4)
	Fruit sales	Number of workers	Total expenditure	Asset value per acre
287(g) authorization	28,700,583	-2,789**	-8,666,157	24,353***
	(36,196,082)	(1,305)	(80,356,599)	(8,473)
287(g) border county	298,127	333	4,652,723	-2,467**
	(8,201,434)	(203)	(8,509,336)	(1,097)
County FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES
State authorization	YES	YES	YES	YES
Observations	4,513	9,097	9,196	9,208

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

Table 9: County-level Census outcomes, using jail occupancy as the instrument

	(1) Number of farms	(2) Acres operated	(3) Total asset value
287(g) authorization	28,700,583	-2,789**	-8,666,157
	(36,196,082)	(1,305)	(80,356,599)
287(g) border county	298,127	333	4,652,723
	(8,201,434)	(203)	(8,509,336)
County FE	YES	YES	YES
Year FE	YES	YES	YES
State enforcement	YES	YES	YES
State authorization	YES	YES	YES
Observations	9,209	9,133	9,208

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

Table 10: Main Census Results: State clustered SE

	(1)	(2)	(3)	(4)	(5)	(6)
	Vegetable acres	Fuel expenses	Labor expenditure	Net income per operation	Machinery value per operation	Asset value per operation
287(g) authorization	1,968	5,445,916	-31,885,083	-211,875**	-158,389*	-2,011,241
	(2,784)	(4,140,946)	(70,942,238)	(92,663)	(88,381)	(1,524,552)
287(g) border county	-114	-602,623	7,099,182	12,090	7,641	128,933
	(316)	(574,690)	(9,516,529)	(9,912)	(9,334)	(171,785)
County FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES
Observations	6,731	9,168	8,423	9,105	9,192	9,208

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

Table 11: Impact of 287(g) Enforcement Levels on Selected ARMS Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Labor	Fuel	Fruit	Vege.	Income	Debt	Net Worth	Equip.	Real estate
alieniden	23.81*	13.32**	-0.175	-0.00882*	-146.4	136.1***	-1,172	8.708	-11.48
	(12.32)	(5.179)	(0.170)	(0.00454)	(92.47)	(36.10)	(722.9)	(22.09)	(8.779)
g287_border	-18,242	-4,451	85.35	14.70	19,385	-27,908	-206,754	-44,929***	1,294
	(23,990)	(4,108)	(79.80)	(11.55)	(40,672)	(39,054)	(485,684)	(11,901)	(1,838)
Constant	128,787***	23,455***	29.05***	37.11***	189,408***	390,534***	1.392e+06***	235,139***	6,539***
	(20,137)	(4,011)	(6.480)	(6.419)	(42,324)	(70,123)	(154,643)	(27,573)	(507.4)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	47,646	47,646	47,646	47,646	47,646	47,646	47,646	47,646	47,645
Number of panelvar	25,845	25,845	25,845	25,845	25,845	25,845	25,845	25,845	25,844

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

Table 12: Census results: Alternative measures of enforcement

	(1)	(2)	(3)	(4)	(5)	(6)
	Vegetable acres	Fuel expenses	Labor expenditure	Net income per operation	Machinery value per operation	Asset value per operation
Aliens identified	.759	2,214	-12,475	-83.7*	-64.6	-821
	(.947)	(1,626)	(29,770)	(47.9)	(46.5)	(764)
287(g) border county	1.19	3,519	-19,489	-131*	-102	-1,295
	(1.43)	(2,565)	(47,401)	(79.1)	(77.6)	(1,270)
Aliens departed	1.19	3,519*	-19,489	-131***	-102***	-1,295***
	(1.06)	(1,937)	(25,427)	(21.2)	(21.8)	(291)
287(g) border county	110	-43,714	3,850,009	-9,143***	-8,455***	-75,807
	(120)	(385,218)	(4,595,001)	(2,836)	(2,904)	(50,275)
County FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES
Observations	6,731	9,168	8,423	9,105	9,192	9,208

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

Table 13: Impact of 287(g) Authorization on Selected ARMS Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Ln(Labor)	Ln(Fuel)	Ln(Fruit)	Ln(Vege)	Ln(Income)	Ln(Debt)	Ln(Net Worth)	Ln(Equip.)	Ln(Real estate)
g287_authorize	-1.025	1.134*	-0.0833	-0.147	-0.593	0.342	-1.047	0.119	-1.642*
	(1.095)	(0.595)	(0.226)	(0.131)	(0.459)	(0.524)	(0.771)	(0.635)	(0.995)
g287_border	0.161	-0.325	-0.0440	0.0886***	-0.106	-0.0794	0.0267	-0.156	0.0984
	(0.216)	(0.213)	(0.0368)	(0.0229)	(0.116)	(0.120)	(0.130)	(0.125)	(0.181)
Constant	8.175***	8.877***	0.360***	0.437***	11.13***	11.09***	13.22***	11.35***	6.745***
	(0.196)	(0.0643)	(0.0267)	(0.0711)	(0.0992)	(0.105)	(0.0609)	(0.0953)	(0.0443)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	47,646	47,646	47,646	47,646	35,170	47,646	47,000	47,646	43,039
Number of panelvar	25,845	25,845	25,845	25,845	22,805	25,845	25,765	25,845	24,272

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

Table 14: Impact of 287(g) Authorization on Selected ARMS Variables, farms with net worth less than $5,000,000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Labor	Fuel	Fruit	Vege.	Income	Debt	Net Worth	Equip.	Real estate
g287_authorize	211,777	65,698	-3,644	-79.60**	-15,625	231,716	-1.553e+06	-268,648	-49,297
	(139,563)	(52,152)	(2,223)	(39.75)	(264,616)	(252,441)	(1.072e+06)	(178,430)	(50,597)
g287_border	-41,627	-16,510	718.3	26.66**	-14,805	-80,521	80,535	19,534	9,551
	(39,171)	(10,923)	(500.6)	(10.68)	(44,565)	(55,543)	(198,755)	(27,707)	(9,209)
Constant	92,645***	20,536***	19.68	23.79***	113,885***	326,298***	628,018***	214,771***	6,013***
	(15,610)	(1,647)	(23.72)	(5.706)	(20,519)	(34,470)	(49,164)	(16,527)	(497.1)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	42,386	42,386	42,386	42,386	42,386	42,386	42,386	42,386	42,385
Number of panelvar	24,390	24,390	24,390	24,390	24,390	24,390	24,390	24,390	24,389

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

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

Table 15: Impact of 287(g) Authorization on Selected ARMS Variables, farms with net worth less than $100,000 per acre

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Labor	Fuel	Fruit	Vege.	Income	Debt	Net Worth	Equip.	Real estate
g287_authorize	295,935**	157,349**	-1,990	-73.46***	163,642	1.117e+06*	-3.871e+06	252,065	11,799**
	(115,160)	(74,088)	(1,577)	(28.39)	(771,939)	(665,697)	(4.801e+06)	(321,353)	(4,616)
g287_border	-59,478*	-28,340*	392.1	26.00*	-27,941	-187,016	498,737	-81,886	-2,213**
	(31,445)	(15,615)	(323.3)	(13.74)	(123,942)	(118,023)	(1.139e+06)	(57,369)	(1,105)
Constant	119,988***	22,491***	32.61***	37.84***	178,747***	389,219***	1.348e+06***	236,893***	3,546***
	(19,802)	(4,089)	(11.95)	(6.520)	(42,680)	(70,967)	(156,719)	(28,114)	(375.3)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	46,860	46,860	46,860	46,860	46,860	46,860	46,860	46,860	46,860
Number of panelvar	25,611	25,611	25,611	25,611	25,611	25,611	25,611	25,611	25,611

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

Table 16: Impact of 287(g) Authorization on Selected ARMS Variables, states with 287(g) counties only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Labor	Fuel	Fruit	Vege.	Income	Debt	Net Worth	Equip.	Real estate
g287_authorize	215,486***	106,065***	-2,212	-66.57***	-422,566	1.277e+06*	-6.681e+06	134,269	-65,898
	(65,074)	(38,453)	(1,596)	(23.49)	(400,087)	(688,224)	(5.693e+06)	(164,452)	(40,145)
g287_border	-57,159	-21,803*	498.1	25.66*	103,929	-233,680*	916,177	-58,777*	12,000
	(37,120)	(11,312)	(380.8)	(15.03)	(86,451)	(133,820)	(1.457e+06)	(31,439)	(9,120)
Constant	200,753***	25,257***	64.06*	44.54***	317,042***	538,120***	1.828e+06***	225,536***	9,737***
	(42,694)	(8,479)	(36.75)	(12.14)	(88,321)	(96,428)	(302,905)	(44,686)	(1,023)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	21,800	21,800	21,800	21,800	21,800	21,800	21,800	21,800	21,799
Number of panelvar	11,753	11,753	11,753	11,753	11,753	11,753	11,753	11,753	11,752

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

Table 17: Impact of 287(g) Authorization on Selected ARMS Variables, ag regions with 287(g) counties only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Labor	Fuel	Fruit	Vege.	Income	Debt	Net Worth	Equip.	Real estate
g287_authorize	271,350***	124,042**	-2,029	-54.78***	-436,210	1.121e+06*	-7.578e+06	34,936	-66,632*
	(101,053)	(50,516)	(1,458)	(18.08)	(388,152)	(605,052)	(5.760e+06)	(201,238)	(40,403)
g287_border	-64,321*	-22,555*	427.8	23.53*	66,106	-198,711*	902,332	-42,875	10,873
	(34,530)	(11,544)	(324.8)	(13.87)	(74,513)	(106,471)	(1.357e+06)	(30,354)	(8,345)
Constant	138,805***	20,568***	34.86**	38.24***	188,089***	392,777***	1.413e+06***	217,444***	7,600***
	(22,648)	(4,421)	(14.73)	(6.790)	(46,832)	(80,274)	(182,740)	(30,888)	(601.1)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	42,830	42,830	42,830	42,830	42,830	42,830	42,830	42,830	42,829
Number of panelvar	23,198	23,198	23,198	23,198	23,198	23,198	23,198	23,198	23,197

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

Table 18: Census results: Treated states only

	(1) Vegetable acres	(2) Fuel expenses	(3) Labor expenditure	(4) Net income per operation	(5) Machinery value per operation	(6) Asset value per operation
287(g) authorization	2,690	6,254,961	-4,037,672	-85,266***	-34,161*	-284,069
	(2,542)	(3,830,079)	(53,044,898)	(18,647)	(17,547)	(687,943)
287(g) border county	-119	-581,471	4,917,918	1,759	-5,160	-74,220
	(305)	(630,629)	(9,539,754)	(3,890)	(3,329)	(75,412)
County FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES
Observations	3,103	4,011	3,644	3,983	4,039	4,038

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

Table 19: Census results: Treated regions only

	(1) Vegetable acres	(2) Fuel expenses	(3) Labor expenditure	(4) Net income per operation	(5) Machinery value per operation	(6) Asset value per operation
287(g) authorization	2,039	6,125,587	-38,458,123	-177,978**	-111,267*	-1,595,552
	(2,765)	(4,147,085)	(72,522,428)	(74,066)	(63,671)	(1,324,427)
287(g) border county	-114	-569,383	7,366,702	10,230	3,669	94,831
	(316)	(577,671)	(9,836,000)	(8,098)	(6,712)	(149,394)
County FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES
Observations	6,202	8,099	7,430	8,055	8,138	8,139

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

6 Figures

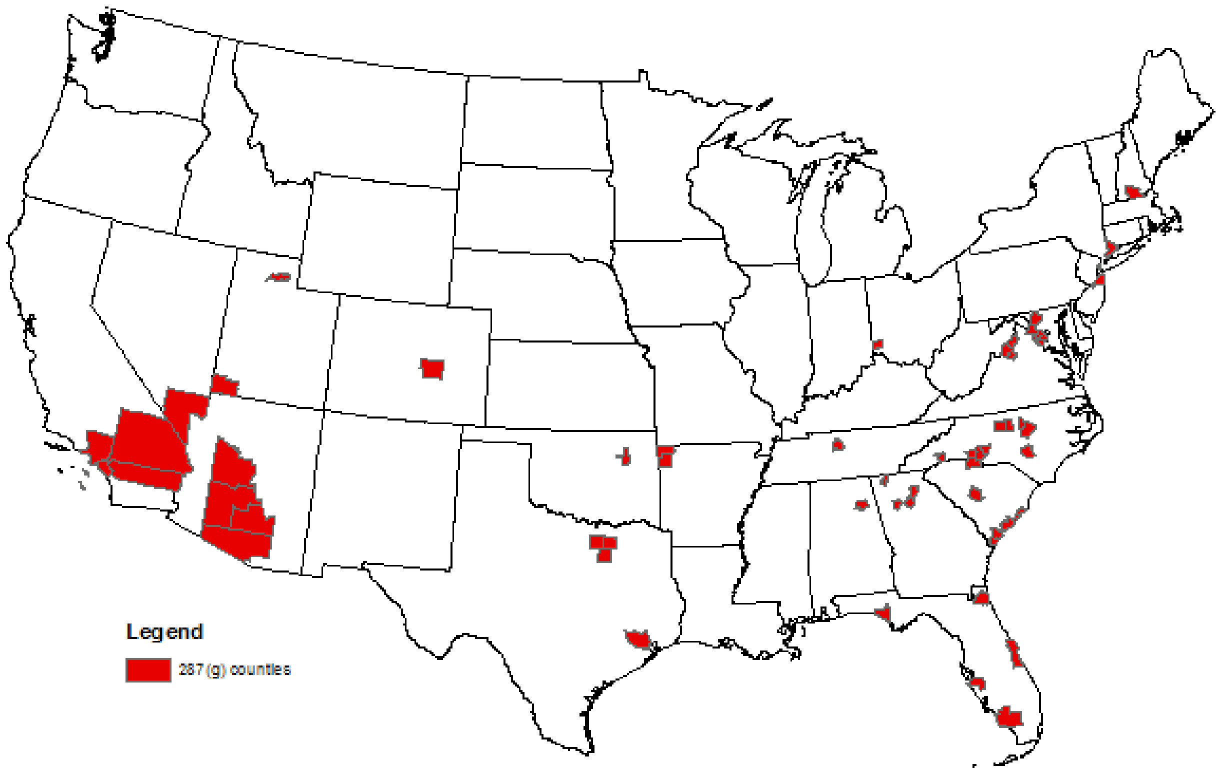
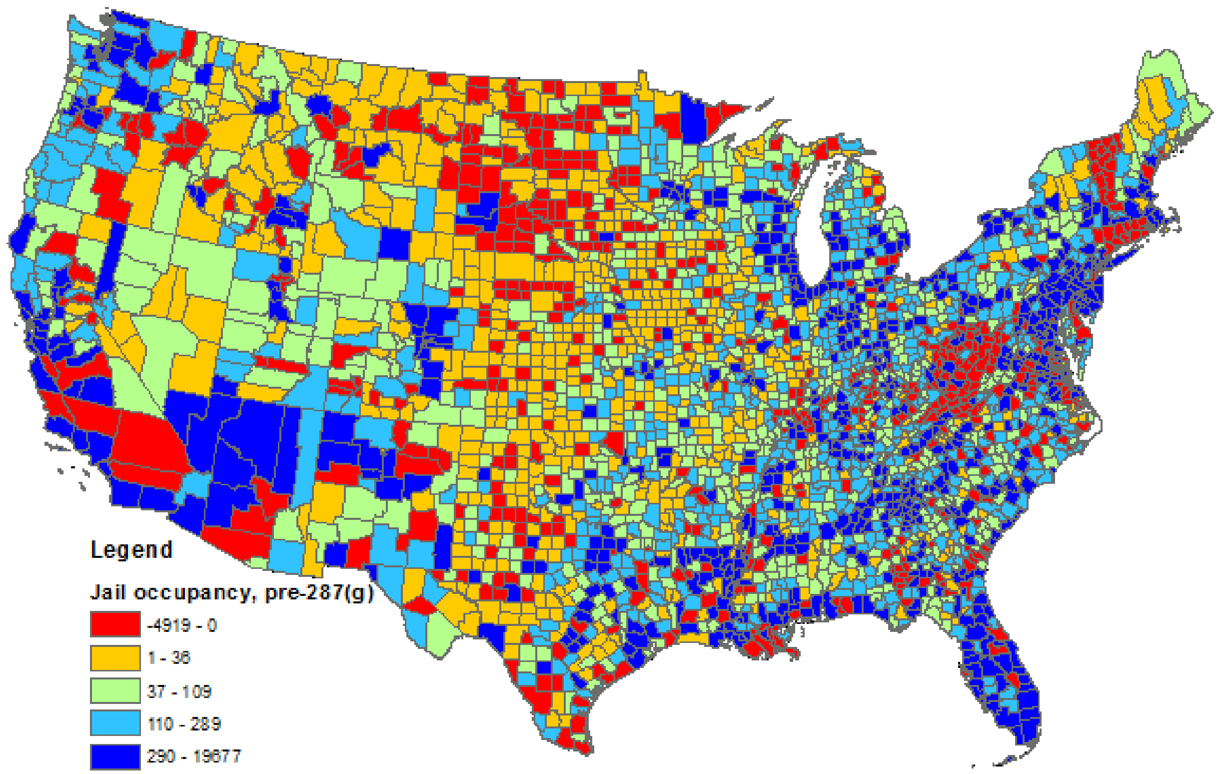


Figure 1: Counties with a 287(g) program



capacity.png capacity.png

Figure 2: U.S. Jail Occupancy

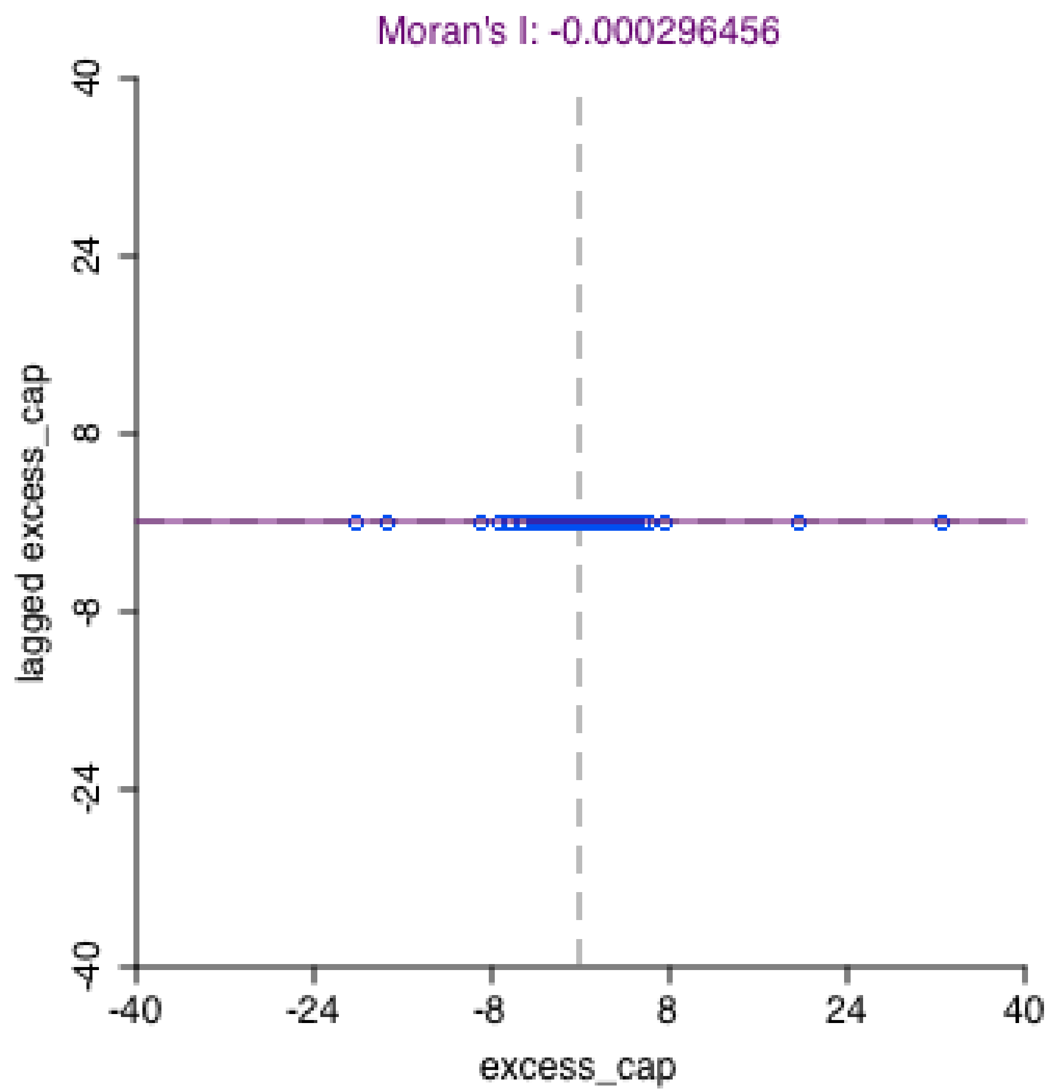


Figure 3: Moran's I

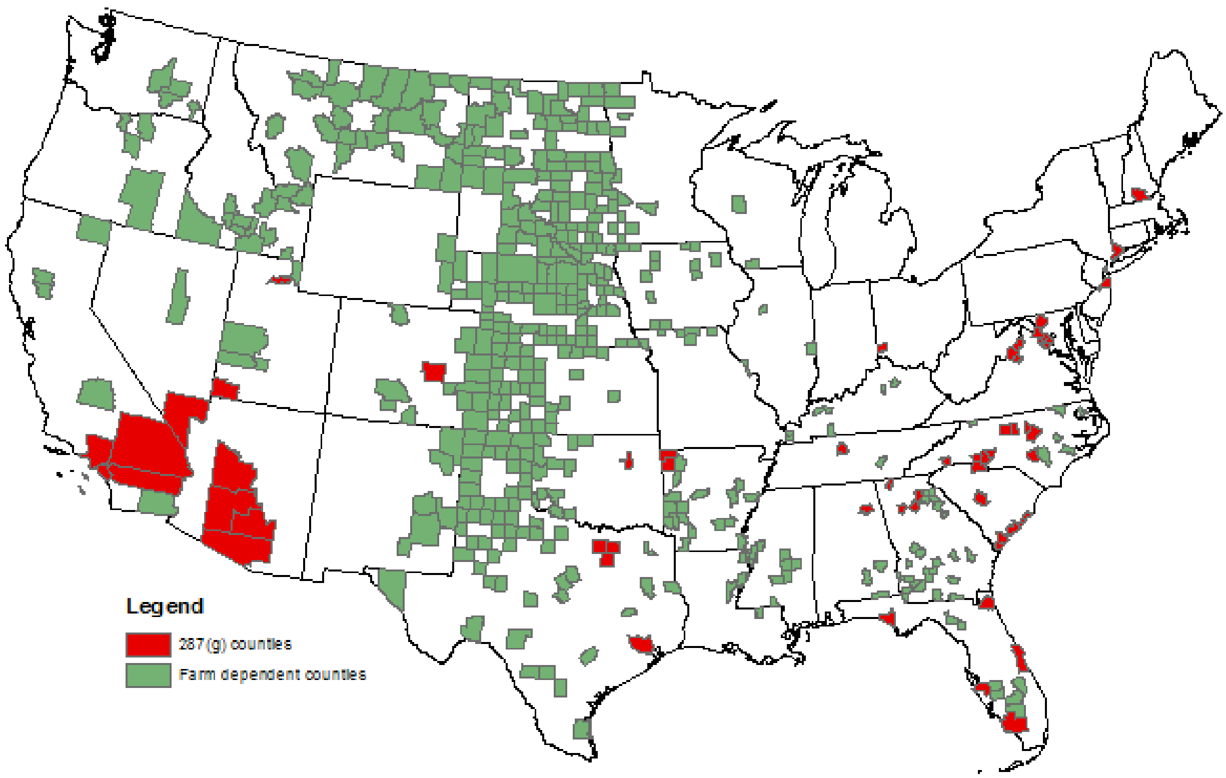


Figure 4: Counties with the 287(g) program and those classified as agriculture-dependent

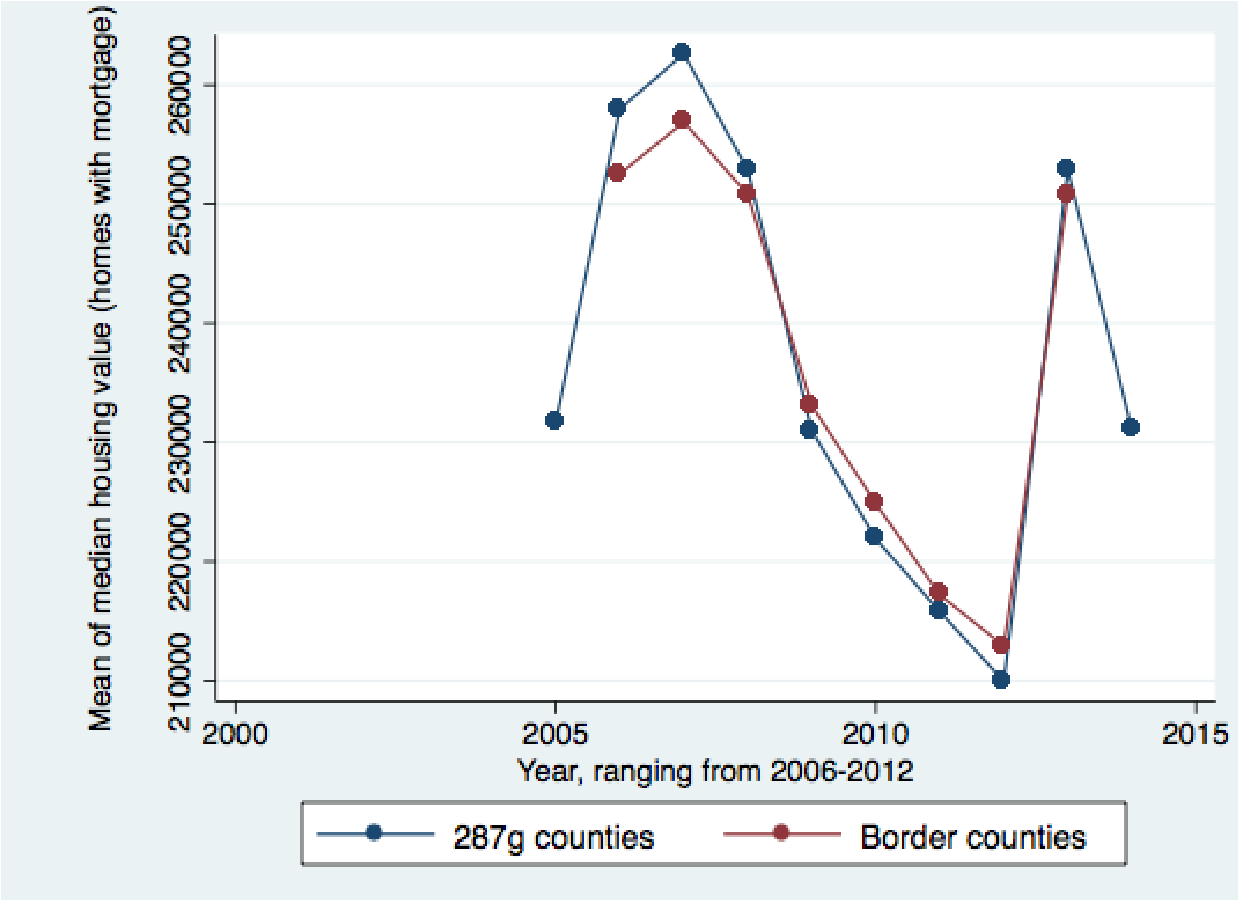


Figure 5: Mean of median housing value, properties with a mortgage

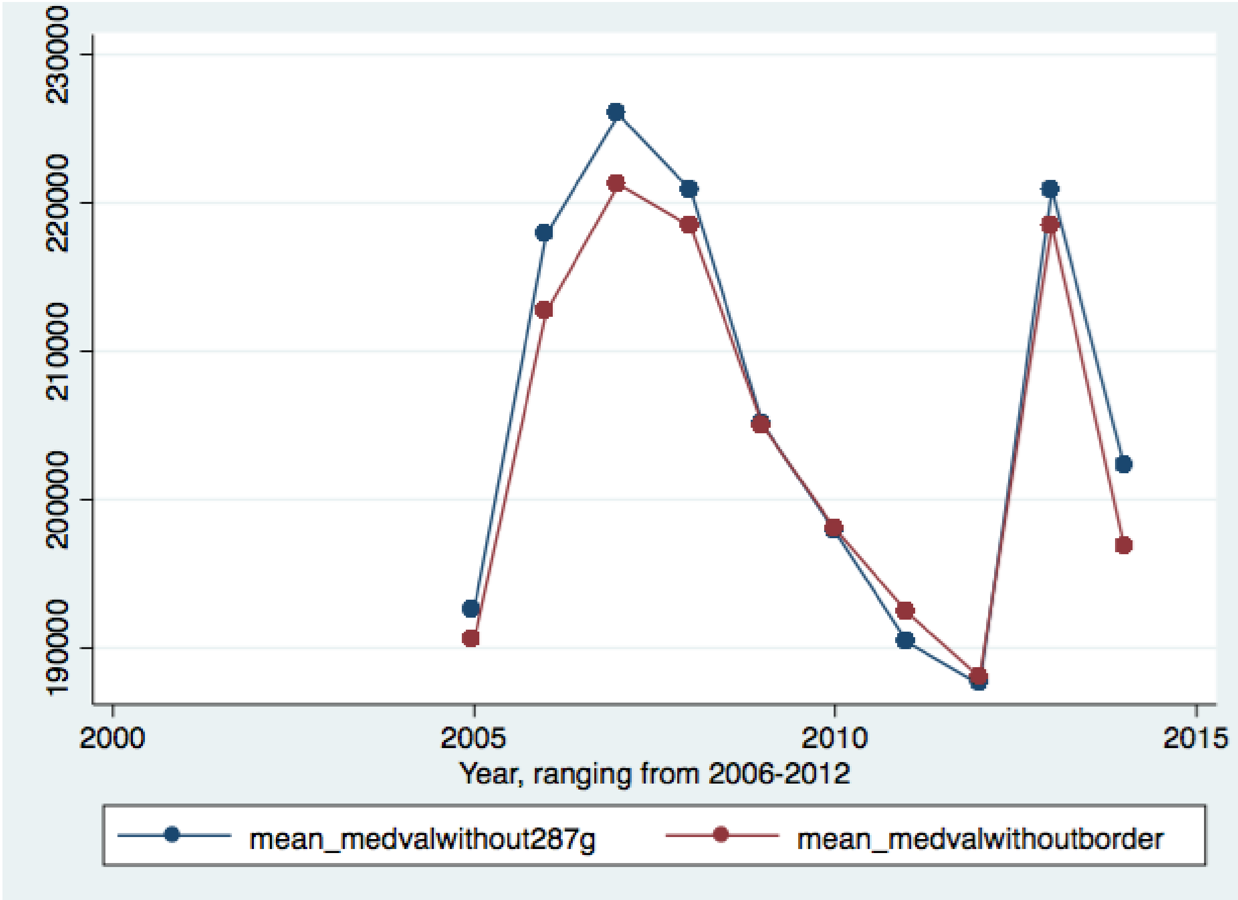


Figure 6: Mean of median housing value, properties without a mortgage

7 Appendix A

Table A1: Impact of 287(g) Authorization on Selected ARMS Variables

VARIABLES	(1) Labor	(2) Fuel	(3) Fruit	(4) Vege.	(5) Income	(6) Debt	(7) Net Worth	(8) Equip.	(9) Real estate
g287_authorize	275,220*	127,788**	-2,019***	-58.77***	-491,267***	1.080e+06***	-7.859e+06***	39,976	-66,767***
	(166,428)	(59,180)	(215.0)	(10.71)	(165,591)	(234,069)	(1.381e+06)	(90,035)	(10,758)
g287_border	-63,656***	-24,989*	418.4***	23.50**	75,317	-195,883**	970,831**	-50,255**	10,930***
	(23,608)	(15,003)	(81.85)	(11.79)	(62,844)	(91,895)	(483,568)	(24,746)	(3,104)
Constant	128,009***	23,093***	34.76***	37.28***	190,807***	387,475***	1.414e+06***	235,026***	6,728***
	(18,223)	(4,742)	(9.475)	(6.658)	(51,497)	(92,302)	(98,319)	(36,122)	(646.8)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	47,685	47,685	47,685	47,685	47,685	47,685	47,685	47,685	47,684
Number of farms	25,845	25,845	25,845	25,845	25,845	25,845	25,845	25,845	25,844

Standard errors in parentheses are robust to correlation at the agricultural production region level

To address the small number of clusters, wild cluster bootstrap standard errors will be reported in a future draft

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

Table A2: Main Census Results: ERS region clustered SE

	(1)	(2)	(3)	(4)	(5)	(6)
	Vegetable acres	Fuel expenses	Labor expenditure	Net income per operation	Machinery value per operation	Asset value per operation
287(g) authorization	1,957	5,442,565	-31,526,371	-211,273**	-158,669	-2,018,189
	(2,075)	(5,607,273)	(75,572,133)	(107,769)	(108,472)	(1,712,230)
287(g) border county	-112	-603,386	7,035,665	11,987	7,656	129,611
	(243)	(390,740)	(10,949,336)	(9,617)	(9,688)	(187,678)
County FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES
Observations	6,731	9,168	8,423	9,105	9,192	9,208

Standard errors in parentheses are robust to correlation at the agricultural production region level
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A3: Main census results: Jail occupancy measured as a share

	(1) Vegetable acres	(2) Fuel expenses	(3) Labor expenditure	(4) Net income per operation	(5) Machinery value per operation	(6) Asset value per operation
287(g) authorization	-1,841	3,353,955	-289,748,692**	-423,352*	-354,183*	-4,387,802**
	(2,757)	(7,963,893)	(146,129,840)	(252,830)	(194,825)	(2,141,480)
287(g) border county	433	-655,741	36,022,406*	36,471	32,248	429,895
	(306)	(1,023,099)	(21,089,889)	(31,419)	(25,576)	(304,180)
County FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES
Observations	5,963	7,826	7,252	7,797	7,854	7,858

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

Table A4: Census results: Controlling for state-year FE

	(1) Vegetable acres	(2) Fuel expenses	(3) Labor expenditure	(4) Net income per operation	(5) Machinery value per operation	(6) Asset value per operation
287(g) authorization	2,402	-3,146,049	-94,224,844***	-129,124***	-70,669**	-1,555,464**
	(3,537)	(7,479,985)	(21,221,490)	(33,189)	(28,242)	(764,103)
287(g) border county	-149	707,697	16,472,194**	5,676	3,748	161,544*
	(406)	(1,245,466)	(6,439,643)	(6,490)	(3,932)	(84,020)
County FE	YES	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES	YES
Observations	6,731	9,168	8,423	9,105	9,192	9,208

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

Table A5: Replication of Table 8 of Kostandini et al. (2014), using jail occupancy as the instrument

	(1) Hired labor share	(2) Expense per worker	(3) Workers per acre	(4) Share of fuel expenses	(5) Log of machinery value	(6) Share of vegetable acres	(7) Log of farm income per acre
287(g) authorization	.0782	-372,567	-77.1	-.0523	.0351	-.00216	-3.44
	(.162)	(246,338)	(2,829)	(.0361)	(.392)	(.00296)	(2.33)
287(g) border county	-.0078	24,139	-78	.00102	.00657	.000229	.0995
	(.0143)	(20,241)	(200)	(.00322)	(.0387)	(.000375)	(.205)
County FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
State enforcement	YES	YES	YES	YES	YES	YES	YES
State authorization	YES	YES	YES	YES	YES	YES	YES
Observations	5,602	6,061	6,066	6,102	6,134	4,412	5,208

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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