THREE ESSAYS ON AGRICULTURAL LABOR AND RISK IN THE UNITED STATES

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Farm operations in the United States have been exposed to an increased amount of labor-related risk over the past two decades, both in terms of the labor they demand and the labor they supply. Farms increasingly face the risk of having their demand for immigrant labor go unmet, as increased anti-immigrant sentiment in the United States and improving conditions in their home countries have reduced the incentives for immigrants from Mexico and Central America to work in the US. On the other hand, off-farm work by at least one member of the household has become the norm for all but the largest farm operations. This increased integration with the off-farm or non-farm labor market, driven in part by growing female labor force participation, has, on the whole, improved the financial situation of the average farm household, relative to the average non-farm household. Off-farm income has also been found to be an important determinant of a farm's ability to pay off debt.

However, these boons are not without risk: farm finances become more directly intertwined with the performance of the economy in general and, crucially, increasingly reliant on job opportunities being available locally. As rural economies around the country continue to decline, there are likely to be impacts on the future viability of farm operations, especially for farms that support their operations with income earned off-farm. Because those farms tend to be medium-sized operations (either in acres operated or net farm income), they are the farms that will be most affected by increased volatility in the labor market. Therefore, understanding the impacts of that volatility on farm financial viability

may also give insight into the growing trend of farmland concentration, which may have its own part to play in the economic decline of rural areas.

Over the same period characterized by increasing rural decline, increasing off-farm labor market participation, and increasing reliance on an increasingly unreliable immigrant labor force, government programs aimed at stabilizing and bolstering farm incomes have changed dramatically. Rather than cash transfer and direct payment programs, crop insurance has become the centerpiece of farm support policies. Although crop insurance protects farms from production risk, anecdotal and theoretical evidence suggests that this may encourage farmers to take on more financial risk. These increased levels of financial risk might, in turn, have implications for the amount or kind of labor used on the farm, or implications for the the extent of the farm household's participation in the labor market. Changing farm support policies may cause farmers, or the members of their households, to substitute time spent on off-farm employment with an increased presence on-farm, or vice versa.

Given this situation, it is important to understand the impacts that these areas of increased risk have on farms' more short-term, day-to-day operating decisions as well as on their financial decisions that affect their longer term prospects. Although farm operations today are more reliant on the off-farm labor market than ever before, academic or policy-oriented research on the nature of this link has not kept pace with advances in empirical estimation techniques from the general labor economics literature. These estimation strategies can be applied to farm-level data, which include detailed records of labor demanded by the farm and the hours supplied by different members of the farm household to the non-farm economy. Together, these causal results yield valuable insights on the farm level impact of changes in the labor market.

The three essays in this dissertation each address a different facet of the implications of increased on-farm risk. Chapter I, "Behind Every Farmer: Off-farm labor and farm viability," speaks to how changes in the off-farm work opportunities for the farm operator and his spouse differentially affect the amount and kind of debt taken on by the farm business or farm household. The estimation strategy relies on the spatial dispersion of growing and shrinking job opportunities for men and women, driven by increased import competition from China over the past two decades. These results are important for understanding the extent to which farms need robust, thriving rural economies; they have implications for both farm and rural policy, which may be more and more interconnected in the future. Next, Chapter II addresses how the increased use of Federal crop insurance (FCI) has increased farms' use of short-term debt. This work is well-positioned to be extended to analyze how that increased short-term debt is being used on farm: for example, whether it encourages a increase in the capital-to-labor ratio or reduces the need for off-farm income. The third and final chapter examines the implications of an increasingly volatile supply of labor to the farm by looking at how local immigration enforcement causing labor supply shocks impacts farms' operating decisions. Counties with programs that allowed for increased enforcement of immigration laws operated fewer acres and had fewer workers. Additionally, the results suggest that the ability to substitute for this class of worker, either with machinery or native workers, is limited. American farm operations require access to a stable immigrant labor force in order to ensure expanded operations in the face of global population and income growth.

BIOGRAPHICAL SKETCH

Margaret Jodlowski began the Ph.D. program in the Dyson School of Applied Economics and Management in 2014. Prior to coming to Cornell University, she received her Masters of Science in Agricultural and Applied Economics from the University of Illinois at Urbana-Champaign. She also attended Illinois as an undergraduate, receiving a Bachelor of Science in Agricultural and Consumer Economcics with Highest Honors and a Bachelor of Arts in History, *cum laude* and with Highest Distinction. Her research focuses on agricultural issues both domestically and internationally, with a specific emphasis on farm labor and farm household dynamics.

To Brad and all our long ways,

and to Grandpa, the original road warrior

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CHAPTER 1

BEHIND EVERY FARMER: OFF-FARM LABOR AND FARM VIABILITY

1.1 Introduction

Agricultural production alone is no longer a viable occupation for all but the largest farms. Operators are reliant, to varying degrees, on income earned off the farm, in order to sustain both the farm business and the farm household, linking farm operations to the broader non-farm economy. These jobs "in town" are necessary for farm survival, and consequently, off-farm work has become a mainstay of US farm households. Indeed, without off-farm work, most farms would not be able to service their household (i.e., farm and non-farm) debt, as the farm debt repayment capacity utilization ratio (DRCU) for all but the largest farms has consistently averaged above 100% since 1998 (Briggeman, 2011).[1] In addition, off-farm work is credited with closing the income gap between the median farm and non-farm household; although both have trended upward, median farm household income has been higher than median non-farm household income since 1998 (Brown et al., 2013). Further, the necessity of off-farm income and the structure of most farm households lends a gendered component to the decision to work off-farm. Since 1996, the percent of farms with a male primary operator has remained constant at 95%, flouting trends of increasing female representation in traditionally male-dominated sectors over the same time period (Goldin, 2014). Therefore, it is often the farm spouse, who is female whenever the primary operator is male, who takes on an off-farm job, working "in town" to earn the income that is necessary for the continued operation of the farm business and for the maintenance of the farm household. As discussed below in section 3, these farm

[1] DRCU is calculated as the ratio of a farm household's *total outstanding debt* over the *maximum amount of debt it can afford to repay, given current levels of assets and income*.

spouses conform to many of the trends in female labor force participation documented by Goldin (2014) and others for the last two decades, including increased participation over time and increased earnings over time, although a gender wage gap between farm operators and farm spouses does persist.

Despite the existing body of exploratory work into the importance of off-farm income, its causal impact on farms' operational or financial characteristics remains unknown. To date, no study has been able to link off-farm income over time to measures of farm financial viability. In addition to estimating the causal impact of off-farm income on these measures, which include the DRCU, the debt-to-asset ratio, and total volume of debt, this paper also explores whether there are differential effects of off-farm income when it is earned by the farm spouse rather than the operator. Shocks to male employment in an area, either through changing job opportunities or income earning potential, affect the operator but not his spouse, and vice versa.

By examining the income earned by the farm spouse separately from the off-farm income earned by the operator, this question engages with a literature based on research from less developed countries which identifies that women and men spend income differently and documents those differences (Blumberg, 1988). Although the context differs greatly, it is reasonable to assume that differences may exist in an advanced agricultural economy like the United States. The "primary operator" designation itself may help motivate these differences, if, for example, it causes the household to think of the income he earns as supplementing the farm business and the income the spouse earns as supporting the household. In this case, the farm business may experience more financial strain when male work opportunities decline.

In addition to allocating their earned income differently, there is a well-established literature that acknowledges the different ways women and men participate in and are compensated by the labor market in the US. Female labor force participation is more variable, both across women and within individual women over time (Fogli and Veldkamp, 2011), more likely to be impacted by the earnings of her spouse (Mincer, 1962), and more likely to be responsive to fertility decisions (Bloom et al., 2009). In addition, while female labor force participation has been increasing dramatically since the start of the last century (Heath and Jayachandran, 2016), the gender wage gap has remained pervasive (Blau and Kahn, 2017).

The implications of the gender wage gap has never been explored for farm households in the US, despite the widespread labor force participation by farm spouses, which is often coupled with non-participation by farm operators. The financial penalties farm households assume as a result of the gender wage gap speak to both the important, non-monetary benefits to identifying as a farmer as well as to the social construct of the farm family. Estimates of these penalties, and their implications for the rural economy, are provided and discussed in section 5.2.

This paper is the first to causally estimate the role that off-farm income plays in the continued financial viability and production capacity of US farms. In doing so, I connect the agricultural risk literature with the extensive literature on labor force participation, especially female labor force participation. Finally, this work identifies off-farm income as one very important channel connecting farm households to the state of the rural economy, and quantifies that importance. Although future work is needed to estimate how off-farm income interacts with other aspects of a farm's risk management strategy, I highlight how important the state of the non-farm economy is for agricultural households in the US. This importance will only grow as "rural decline" continues to receive attention in the popular press.

The remainder of the paper proceeds as follows. Section 2 provides background on off-farm labor in the US, including early conceptual models of the decision to work off-farm and empirical work on the correlates of off-farm labor participation. Section 3 describes the data, and describes the nature of off-farm work decisions for farm households in the ARMS sample, including trends over time. Section 4 describes the empirical methods used to elicit causal estimates of the impact of off-farm income. Results appear and are discussed in section 5, and section 6 concludes.

1.2 Background

The earliest conceptual models of off-farm work in the United States have either failed to consider, or been unable to consider, the differences between the labor supplied off-farm by the primary operator and by his spouse. Early theoretical models of household off-farm labor allocation considered each household member to be identical, with identical preferences over farm work, off-farm work, and leisure (Huffman, 1980). In addition to this simplification, these models also relied on some unrealistic and untenable assumptions, including perfect flexibility in the off-farm labor market and no non-monetary benefits to work.

The work of Huffman and Lange (1989) provides a theoretical model of the intra-household bargaining system that farm households undertake when making off-farm work decisions. Their work advances earlier models by including non-negativity constraints on off-farm hours, which previous work, including Huffman (1980), failed to include. Despite this advancement, their work fails to consider differential levels of access men and women have to labor markets, especially in rural areas. Further, their theoretical model of off-farm labor supply fails to consider that some women (and men) might derive utility from off-farm work, as well as other developments from the "work as empowerment" literature that has been developed over the last few decades. Their findings on the determinants of off-farm work are not surprising, and highlight the role of human capital accumulation in influencing these decisions.

In cases where the operator and his spouse's off-farm work are considered separately, authors tend to treat the spouse's decision as identical to the operators', ignoring a wide body of literature on the mechanics of intra-household bargaining, as well as studies that model differences between how men and women access, participate in, and are compensated in the labor market. In fact, El-Osta et al. (2008) even says, "increasing access to off-farm job opportunities...is likely to be more important for husbands than for wives, in terms of both participation and earnings potential." However, data these authors use show that wives work off-farm at a rate almost double that of husbands, when only one partner works off-farm.

Farm income, and its volatility, is therefore intrinsically linked to household structure: farm households where the operator does not have a spouse have more variable income, and the mediating channel for this relationship is likely off-farm income. Off-farm work has long been understood to be a component in a farm's risk management strategy, as the variability of farm income has been found to have a significant relationship with off-farm labor supply in early empirical work (Mishra and Goodwin, 1998), (Serra et al., 2005). Farm household income variability has been the target of government programs since the beginning of the 20th century, with the most recent example being the Agricultural Act of 2014. Farm household income is so volatile because the income earned from agriculture is highly variable: one study of large-scale commercial farms found that the between-year change in farm income was about 180% of the median farm income (Key et al., 2017).

Studies that have looked at the relationship between off-farm labor and government programs in particular often struggle with identification in light of the endogeneity between the decision to work off farm and the amount of government payments received. El-Osta et al. (2008) addressed this endogeneity using the Smith-Blundell two-step estimator, and determined it was only "fairly successful."[2] They find a negative and significant relationship of government payments on the likelihood of just the husband working off-farm and of both spouses working off farm. These results provide some preliminary evidence that government payments are a substitute for the off-farm income of the primary operator.

However, the same relationship does not exist for households where the only off-farm wage earner is the wife: an increase in expected payments actually increases the likelihood of just the wife working off-farm. The authors attribute this to the higher dependence on farming among households with this off-farm work strategy. The wife's education level and household size have the expected relationship (positive and negative, respectively) on the likelihood of the wife working off farm. Using a sample from Canada, Poon and Weersink (2011) find that government payments have a significant and positive relationship with the variability of off-farm income, showing that government payments could be a substitute risk management option for off-farm work. The increase in variability indicates a possible decrease in full-time off-farm work associated with government payments for these farms (Poon and Weersink, 2011).

Serra et al. (2005) find similar results on the relationship between government support and off-farm labor decisions, using the 1996 farm policy reform that introduced fixed, decoupled payments into the suite of US government farm support as a plausibly exoge-

[2]See Smith and Blundell (1986), Rivers and Vuong (1988), and Wooldridge (2010), pp. 472-77 for more information on this estimation strategy.

nous policy change. However, the decreased likelihood of off-farm labor was potentially counterbalanced by an increased revealed aversion to risk, which in turn was related to increased off-farm labor participation. Overall, Serra et al. (2005) conclude that the reforms' net effects were small. Because of data limitations, these authors are only able to observe the extensive margin of off-farm work by *any* family member: a dummy variable equal to 1 if any household member reports off-farm work.

One possible means of addressing this endogeneity would be to use changes in levels of access to health insurance, as one of the many reasons an operator or his spouse chooses to work off-farm is to receive health insurance for the farm family. Farm household members receive insurance from different sources than the general population, although the percentage receiving health insurance from their employer is roughly the same in each group (57.0% of farm households and 59.7% of non-farm households). However, in 2006, 9.1% of non-farm individuals purchased insurance directly, while 20.8% of farm household members did. On the flip side, farm household members are less likely to receive health insurance from government programs (18.5%) than are members of the general non-farm population (27.0%) (Mishra et al., 2012). Mishra and Chang (2012) used the theoretical framework of Huffman and Lange (1989) to estimate the relationship between off-farm work and health-related expenditures and found that off-farm work is associated with significantly lower health care expenditures.

Over time, changes in tax policy also changed the incentives around self-employment, including farming. For example, changes in the deductability of health insurance premiums for the self-employed, brought about by legislation, increased the probability of self-employment both extensively and intensively (Heim and Lurie, 2010). More recently, changes to the country's health care system enacted by the Affordable Care Act (ACA) changed the possible sources of health insurance for some farm households. Expansions to Medicaid, in the states that elected to do so, made some households eligible, while the individual marketplace changed the incentives to work off-farm primarily for health insurance. Additionally, provisions in the ACA requiring employers of a certain size to provide health insurance for their employees may have impacted the structure and labor use decisions of the country's largest farms (Ahearn et al., 2014). In their paper, Ahearn et al. (2014) identify the farms that would be implicated by the ACA expansion, using data from 2011 prior to the bill's passage and relying on the assumption that farm characteristics would be somewhat constant. They find that 17% of farm families in 2011 would be eligible for Medicaid under the expansion, with roughly half of these farms located in states that elected to expand.

The research by Mishra, Ahearn and others indicates the non-monetary benefits of off-farm work, including access to group health insurance coverage, play a significant role in determining whether a household member works off-farm and the extent to which s/he does so. These authors also find significantly higher retirement savings for farm households who participate in more off-farm work, likely through the mechanism of company supported retirement savings systems, such as pensions and 401(k)s. Another possible way to exogenously identify these effects would be to use changes in the characteristics of the local labor market. Work by Alasia et al. (2009) has shown a significant association between local economic conditions and the likelihood of off-farm labor provision. Because trends in agricultural production tend to be decoupled from general economic trends, under some circumstances it is possible that changes in the local economy increased the likelihood of off-farm employment without affecting other on-farm production or financial decisions.

Using data from the 2010 Agricultural Resource Management Survey (ARMS), Brown et al. (2013) provide a descriptive analysis of the nature of off-farm work by both farm operators and, uncommon for this literature, their spouses as well. Because 2010 was the first year ARMS asked respondents about the specific occupation, rather than the industry, in which farm operators and their spouses worked, the authors use this information to highlight important complementarities between the current nature of farm management and the kinds of work operators and their spouses perform off-farm. Specifically, given the high levels of human capital required to manage a large farm, Brown and Weber predict that farm operators' off-farm jobs will be high skill, which would help explain the increase in median farm household income relative to the general population since 1998. These predictions are born out in the data: farm operators and spouses worked in management and professional positions more frequently than the general non-metro, non-farm population. Across all farm sizes, the most common position for a farm operator or his spouse was in a management or professional occupation. In another study using ARMS data, El-Osta (2011) shows that human capital attainment, specifically education, is an important contributor to a farm's capacity to generate income, both on and off the farm. However, El-Osta (2011) use only one year of ARMS data (2006) and are unable to control for the endogeneity of education in the economic outcome models. This result supports the findings of earlier work using county-level averages to show that increases in farmers' human capital, through education or extension, "directly increases the odds of their off-farm work", both at the extensive and intensive margin (Huffman, 1980), a finding also supported by the work of Alasia et al. (2009).

The importance of off-farm income does decline as farm size increases; off-farm income declines as a share of total farm household income as farm size increases (Brown et al., 2013). Separate empirical work on a sample of Canadian farmers shows a different, but related result: 50% of farms classified as "large" or "very large" in their sample were in the highest off-farm income volatility quintile. These authors suggest that operators and their families of large farms use off-farm income to self-insure when farm income is low (Poon and Weersink, 2011). The negative correlation between farm size and propensity to work off-farm is also found in other empirical work ((Mishra and Goodwin, 1998), (Serra et al., 2005)). While there is substantial variation in the occupation of the farm operator across farm sizes, the jobs held by farm spouses do not tend to vary: work in the education and health care sectors dominates all others, regardless of farm size. The opposite is true for hours worked, however: while the hours worked by the farm operator are relatively constant regardless of the occupation, across all occupations farm spouses work significantly less as farm size increases. In fact, spouses on farms with $250,000 or more in sales work an average of only 3.6 hours per week, compared to an average of 26.4 hours per week by spouses on farms with $50,000 or less in sales.

There are significant, systematic differences in work preferences by spouses across farm sizes, different opportunity costs of time, or some combination of the two (Brown et al., 2013). In a study examining the household income variability of large-scale commercial operations, Key et al. (2017) find that the off-farm income variability of these farms is significantly higher than the non-farm income of non-farm households. Off-farm income is clearly used very differently by farms of different sizes; for the largest operations, it is used only when necessary to compensate for the variation in farm income. In years where it is possible for these households to forgo off-farm work, they do so; in other years, when off-farm work is necessary due to farm income downturns, it is sought out.

The relationship between farm size and off-farm work is especially important given the well-documented trend of farmland concentration: the farm size distribution is hollowing out in the middle, increasing the proportion of farms that are either small or very large (MacDonald et al., 2018). This trend has worrying implications for the future viability of rural communities and the sustainability of US agriculture. It is crucial, therefore, to quantify the importance of off-farm work for US farm households over time, to identify how these households use off-farm income to manage risk, and to document its interaction with government programs.

1.3 Data

Previous explorations of the relationship between farm-level outcomes and off-farm work has been limited by two primary empirical challenges: the endogeneity of the off-farm work decision and poor data availability. Detailed data are needed to capture the granularity of off-farm work, measuring labor market participation on the intensive margin through either hours worked or income earned, rather than only capturing the extensive margin of participating or not. The data requirements on the outcome side are similarly stringent. For example, data on participation in government programs designed to reduce farm income variability can be used to estimate whether off-farm work is used to smooth farm income across time. Data on receipt of lump-sum sources of farm funding, on the other hand, can be used to estimate whether off-farm income is instead used to increase total income.

The USDA's Agricultural Resource Management Survey (ARMS) is the best available source for these data. ARMS is the only annual and nationally representative farm survey. It contains comprehensive information on all aspects of the farm business and household, including measures of participation in government programs, such as crop insurance, and an extensive section on the farm businesses' outstanding loans. Each year, ARMS surveys roughly 20,000 farms; it has been conducted each year since 1996. Although the data are cross-sectional, distributional requirements mean that there is a positive relationship between farm size and likelihood of appearing in the ARMS sample more frequently over time. As a consequence, there is a smaller, non-representative sub-sample of farms that are larger than the average ARMS farm that can be formed into an unbalanced panel. Weber et al. (2016) provides more detail on the ARMS panel, and how observations in the panel differ from those in the cross section.

The farms that respond to ARMS are not representative of all farms in the US, using the USDA's definition of a farm. They tend to be larger, both in terms of acres operated and in sales volume. This is reflected in the off-farm work strategy employed by farms in the sample. Across all years, nearly half (43.4%) of the more than 230,000 farm households recorded in ARMS with a spouse have neither partner working off-farm.[3] That rate of non-participation in the off-farm labor market is consistent with larger farm operations that are more likely to be self-sustaining. In contrast, not even 20% of farm operations have both partners working off-farm in the ARMS sample (19.2%). When just one partner works off farm, it is much more likely that it is the wife, rather than the primary operator: among all households, 20.9% have just the wife working off farm, compared to only 9.3% where it is just the primary operator working off the farm. All together, the farm spouse works off farm in 45% of households, regardless of what her husband does; the primary operator works off farm in only 30% of operation-year observations. The strategy where the wife works off-farm and the primary operator does not is therefore the second most common off-farm work decision in the ARMS sample, after both spouses not working.

In addition to providing the dependent variables of interest, ARMS also includes important information on the off-farm labor participation and time use separately for the

[3]The majority of operations in the sample explicitly state that the primary operator has a spouse: 84.2% across all years. The highest spouse rate was 90.7% of operations in 1996; the lowest was 77.1% in 2002. Only the farms that record a spouse explicitly by responding affirmatively to the question "Does the farm operator have a spouse?" are included in the sample. Note that the question was not asked in 1997.

farm operator and, crucially, his spouse. These variables form the set of "first stage" mechanisms; summary statistics for these variables are reported in table 1.1 for the primary operator and table 1.2 for the farm spouse. Specifically, ARMS reports the off-farm income earned, broken down into off-farm wage income and income generated from operating an off-farm business; both of these measures are reported annually. I also observe the on-farm wage paid to the operator and to the spouse for work they do on-farm. This is distinct from income generated by the farm business, and is quite noisy; its high variability may reflect income smoothing to comply with tax regulations rather than accurately reflecting the value-added to the farm business. On-farm wage, however, is correlated to some extent with the number of hours worked on farm. Although the number of paid on-farm hours worked is very low for both operators and their spouses, the unconditional mean of on-farm wage and paid farm hours a week is higher than the mean conditional on off-farm labor market participation.

ARMS also records the hours the operator and his spouse spent working on- and off-farm, which are depicted over time in figures 1.3 and 1.4, respectively. Time spent off-farm follows a similar pattern for both spouses, reflecting not only market conditions but also the changing sample of farms in ARMS. Because different commodities are targeted each year, it is likely that at least some of these averages are driven by sample compositional changes, as previous work has shown off-farm work to be more important or more necessary for some operation types than for others. Farm hours are much more stable, relative to off-farm hours, for both the operator and the spouse. Starting in 2004, the survey included a weekly time use section that recorded how each partner spent their time during an average week. To capture the influence of the farm production cycle, which is highly seasonal, these questions were asked for each quarter of the year. Within year variation in off-farm work is an important aspect of understanding the role off-farm income plays in the risk management strategy of the farm. This section also split on-farm work into paid and unpaid time. The final two categories in this section are personal care, which includes home production activities, and leisure hours. Summary statistics for these measures are also found in table 1.1. The average farm operator who works off-farm spends almost 30 hours a week at his job, while putting in similar hours (35.1 hours per week) doing on-farm work. Farm spouses, on the other hand, spent approximately the same time doing on-farm work, regardless of whether they held a job off-farm or not. Increasing their off-farm work hours is pulling time away from home production, personal care, or leisure time, rather than from on-farm work.

Table 1.1: Farm operator wages and hours worked, on- and off-farm

	all operations			operations with operator working off-farm		
	mean	sd	n	mean	sd	n
Off farm income	$21,455.17	$174,031.70	267,478	$37,402.16	$200,123.30	124,947
Off farm wage income	$20,863.05	$120,387.80	192,607	$61,133.02	$200,024.90	65,725
Off farm business income	$10,105.96	$162,476.90	170,238	$10,909.46	$155,846.30	60,069
On-farm wage	$3,284.43	$12,721.60	292,209	$2,337.51	$10,693.70	145,392
Farm hours per week	43.3	24.7	219,742	35.1	23.1	96,170
Unpaid farm hours per week	36.6	26.1	90,577	26.6	19.9	28,430
Paid farm hours per week	5.0	16.4	90,577	1.7	9.3	28,430
Off-farm hours per year	530.5	926.3	169,794	1,338.31	1,041.95	63,991
Off-farm hours per week	11.1	18.8	169,848	28.2	20.3	64,045

Table 1.2: Farm spouse wages and hours worked, on- and off-farm

	all operations			operations with spouse working off-farm		
	mean	sd	n	mean	sd	n
Off farm income	$12,283.46	$66,803.09	267,478	$20,021.78	$73,474.93	155,473
Off farm wage income	$16,767.87	$56,322.48	179,328	$35,162.41	$77,494.65	85,516
Off farm business income	$1,910.22	$58,919.06	145,850	$1,536.77	$50,295.63	68,909
On-farm wage	$712.52	$3,393.88	292,209	$533.13	$2,836.81	176,942
Farm hours per week	10.9	16.4	209,333	7.9	13.1	110,963
Unpaid farm hours per week	10.0	15.3	89,794	7.0	11.2	39,253
Paid farm hours per week	1.6	7.5	89,794	0.7	4.4	39,253
Off-farm hours per year	763.1	958.8	158,300	1572.4	786.3	74,367
Off-farm hours per week	15.5	19.0	158,350	31.9	14.7	74,415

Another important takeaway from these summary tables is that although farm spouses work more hours on average than their primary operator counterparts, their levels of compensation are about half those of the primary operator. The average primary operator with an off-farm job reported more than $37,000 as their annual off-farm income; the annual earnings of the average farm spouse were slightly more than $20,000. It is clear from the distributions, however, that the right tail of the primary operators' earning distribution is much larger. Figure 1.1, which plots average off-farm income over time, shows how the average off-farm earnings of the farm operator are much more variable over time. Given the variability of farm earnings, it is likely that farm operators enter and leave the labor force frequently, seeking off-farm employment when the farm business needs shoring up. There does not appear to be a wage penalty for this flexibility: in each observed year, the average earnings for the primary operator are higher than those of the farm spouse. Time on the job is certainly not driving the higher earnings: the average primary operator with an off-farm job worked 28.2 hours per week at it, compared with 31.9 hours per week for the average farm spouse. The difference is even greater when intra-year variability is taken into account: in a year, the average farm spouse works more than 200 more hours than the average operator. This belies a nearly $20,000 difference in compensation levels.

Figure 1.1: Average annual off-farm income

Tables 1.3 and 1.4 include farm or household characteristics and operator or spouse individual characteristics, respectively, and highlight important differences between operations with and without a spouse that works off-farm.

Table 1.3: Farm and household characteristics, by spousal off-farm work status

	all operations			operations with spouse working off-farm			operations without spouse working off-farm		
	mean	sd	n	mean	sd	n	mean	sd	n
Acres operated	1,267.9	6,463.7	233,972	1046.12***	3,314.2	106,796	1,454.1	8,219.7	127,176
Percent of acres harvested	0.5	0.5	233,964	53.7%***	53.7%	106,792	51.8%	40.1%	127,172
Total cropland acres	672.7	1,367.2	233,972	633.4***	1,161.6	106,796	705.6	1,517.7	127,176
Value of home consumption	197.1	580.3	194,544	190.9**	556.1	87,170	202.2	599.2	107,374
Total number of operators	1.6	1.0	198,091	1.6***	0.8	89,517	1.7	1.1	108,574
Sales class:									
$500,000 or more	29.9%		94,813	27.2%		51,758	33.9%		43,055
$250,000 – $499,999	13.2%		41,852	13.2%		25,108	13.2%		16,744
$100,000 – $249,999	14.8%		47,067	15.4%		29,394	13.9%		17,673
$40,000 – $99,999	10.9%		34,630	11.5%		21,852	10.1%		12,778
$20,000 – $39,999	6.7%		21,320	7.0%		13,277	6.3%		8,043
$10,000 – $19,999	5.9%		18,782	6.2%		11,754	5.5%		7,028
$9,999 or less	18.6%		59,123	19.6%		37,268	17.2%		21,855
Specialization:									
Cash grain	7.4%		23,370	8.3%		15,850	5.9%		7,520
Wheat	3.2%		9,996	3.4%		6,483	2.8%		3,513
Corn	9.9%		31,561	11.1%		21,175	8.2%		10,386
Soybeans	5.0%		15,721	5.4%		10,272	4.3%		5,449
Sorghum	0.2%		766	0.3%		491	0.2%		275
Rice	1.1%		3,422	1.0%		1,865	1.2%		1,557
Tobacco	1.2%		3,757	1.3%		2,474	1.0%		1,283
Cotton	2.4%		7,515	2.3%		4,424	2.4%		3,091
Peanut	0.4%		1,342	0.4%		826	0.4%		516
Other crops	11.6%		36,891	11.7%		22,329	11.5%		14,562
Fruit	5.5%		17,487	5.0%		9,484	6.3%		8,003
Vegetables	2.1%		6,586	1.8%		3,461	2.5%		3,125
Nursery	3.5%		11,257	3.0%		5,791	4.3%		5,466
Cattle	22.3%		70,849	22.3%		42,524	22.3%		28,325
Hogs	3.2%		10,265	3.6%		6,849	2.7%		3,416
Poultry	6.4%		20,323	5.9%		11,194	7.2%		9,129
Dairy	9.2%		29,168	7.3%		13,905	12.0%		15,263
Other livestock	5.45%		17,311	5.8%		11,014	5.0%		6,297
Household size	2.9	1.5	214,034	3.0***	1.3	98,949	2.8	1.6	115,085

***,**,*Significantly different from operations without a spouse working off-farm at the 1%, 5%, and 10% confidence level, respectively

Table 1.4: Operator and spouse individual characteristics, by spousal off-farm work status

	all operations			operations with spouse working off-farm			operations without spouse working off-farm		
	mean	sd	n	mean	sd	n	mean	sd	n
Operator age	55.2	12.3	169,259	51.6***	10.4	77,867	58.2	12.9	91,392
Operator education:									
Less than high school	7.8%		24,007	5.3%		9,798	11.6%		14,209
High school	38.6%		118,490	37.7%		69,333	40.0%		49,157
Some college	28.1%		86,268	29.9%		55,046	25.4%		31,222
College graduate or more	25.5%		78,375	27.1%		49,962	23.1%		28,413
Operator major occupation:									
Farming/ranching	71.5%		224,300	68.6%		128,961	76.0%		95,339
Other work	21.7%		67,892	26.2%		49,333	14.8%		18,559
Retired or not in labor force	6.8%		21,344	5.2%		9,827	9.2%		11,517
Spouse age	54.1	11.8	90,028	50.9***	10.1	39,835	56.7	12.4	50,193
Spouse education:									
Less than high school	5.2%		10,206	2.8%		3,321	8.8%		6,885
High school	34.6%		67,763	30.5%		35,858	40.8%		31,905
Some college	29.3%		57,328	30.8%		36,170	27.1%		21,158
College graduate or more	30.8%		60,316	35.9%		42,109	23.3%		18,207
Spouse major occupation:									
Farming/ranching	28.3%		61,655	17.5%		22,694	44.4%		38,961
Other work	46.5%		101,229	69.1%		89,669	13.2%		11,560
Retired or not in labor force	25.2%		54,738	13.5%		17,479	42.5%		37,259
Operator retired from farming	12.3%	32.9%	208,123	8.3%***	27.7%	94,509	15.6%	36.3%	113,614
Spouse experience	25.1	14.2	43,534	21.9***	12.8	18,048	27.4	14.8	25,486
Operator non-white	4.5%	20.7%	221,373	4.0%***	19.6%	104,296	4.9%	21.7%	117,077
Spouse non-white	6.3%	24.3%	153,468	5.5%***	22.8%	72,149	7.0%	25.5%	81,319

***,**,*Significantly different from operations without a spouse working off-farm at the 1%, 5%, and 10% confidence level, respectively

First, operations in which the farm spouse does not work off-farm are larger, reinforcing the idea from the literature that larger farms tend to be more self-sustaining and do not require the income support from an off-farm job as much as smaller farms. However, this does not necessarily translate into a more productive operation, as the percent of acres harvested is significantly greater in operations where the spouse does work off farm. Unsurprisingly, the average operator in the ARMS sample is in his mid-50s, and both the operator and the spouse are older for operations without a spousal off-farm job. Age is certainly an important factor in the decision to work off-farm, especially given that the average age of a farmer in the United States has been trending up consistently over the last 50 years (Katchova and Ahearn, 2017). Retirement from farming, even at advanced ages, is much less common, however: in this sample only 12% of farmers consider themselves to be retired. Finally, highlighting their human capital achievements and value to the farm business, the average farm spouse in the sample has around 25 years of experience working on a farm. This experience may also drive the off-farm labor participation decision, as the average years of experience for farm spouses without jobs is six years higher, indicating important opportunity costs to off-farm employment. Similarly, 35% of spouses who work off-farm have the highest educational attainment recorded in the survey (a Bachelor's degree or more), compared to 23.3% of spouses who do not work off farm.

Figure 1.2: Labor force participation rate

Figure 1.3: Average farm work hours per week

Figure 1.4: Average off-farm work hours per week

1.3.1 Dependent variables

Farm financial sustainability is broad and nebulous concept, and no one measure captures a farm operation's potential future viability. This paper focuses on debt loading as one element of a farm's financial picture. While farms often use debt as a means to expand operations, debt loading can also indicate financial stress when a farm's ability to service the debt is inhibited. The total value of debt alone, therefore, does not necessarily indicate reduced viability. The terms of the loans, and an operation's ability to make payments, are an important element of that picture (Briggeman et al., 2010). In addition to its relevance for farm finances, debt is an interesting outcome because it is accumulated by both the farm operation and the farm household. The relationship between farm operational debt and farm household debt is unknown, as is whether operations consider these two different kinds of debt separately.

In this paper, I use the total value of farm debt and household debt as the outcomes of interest, looking at how shocks to operator and spouse off-farm income changes the debt structure of US farm operations and households. In addition, I analyze the impact of off-farm work shocks on the components of farm debt and household debt. On the farm side, this includes non-real estate long term debt, real estate debt, and short term debt. Short term debt is defined as loans that have terms of one year or less; often, these are operating loans that are extended annually to a farmer on the same line of credit year after year. The average farm in the ARMS sample has more than $250,000 of farm debt, of which more than half is real estate debt. Household debt is decomposed into the mortgage for the household's dwelling, provided it is not owned by the farm operation, loans for other (non-farm) real estate, loans for any non-farm business, and personal loans. I also include the amount of debt intended for household purposes that is secured by farm assets as an outcome of interest; this measure captures one element of a farm household's ability and willingness to leverage farm assets to support the household. For the average ARMS farm, a little more than 20% of a household's $75,000 of household debt is secured by farm assets.

Finally, as mentioned above, the volume of loans alone is not meaningful: high loan levels are not problematic if the operation is able to serve them with its current income. In order to capture the potential financial stress of an operation's current loan burden, the final set of outcomes includes measures of loan repayment capacity. The first is the farm's leverage, or debt-to-asset ratio, one measure of a farm's long run financial solvency. Although some positive amount of leverage may be optimal, concerns for solvency arise when leverage increases to the point of potential farm equity loss. The average debt-to-asset ratio of an ARMS farm is 0.20, indicating the high level of asset holdings of these operations, especially relative to the average USDA-defined farm. The other two measures considered in this section are the maximum feasible debt repayment capacities at

10% and 7.5% interest. These measures approximate the maximum amount of debt a farm operation could expect to repay given its current assets and revenue. It is calculated by the USDA-ERS and reported in the ARMS data.

Table 1.5: Farm and household debt, by spousal off-farm work status

	all operations			operations with spouse working off-farm			operations without spouse working off-farm		
	mean	sd	n	mean	sd	n	mean	sd	n
Farm debt									
Total farm debt	$ 262,507.70	$ 864,150.90	194,307	$224,242.90***	$ 590,657.70	89,939	$ 295,482.40	$ 1,042,731.00	104,368
Non-real estate debt	$ 68,546.44	$ 538,636.50	201,658	$53,481.91***	$ 289,705.20	91,200	$ 80,984.51	$ 678,262.00	110,458
Real estate debt	$ 183,793.00	$ 866,639.90	201,658	$146,018.10***	$ 496,846.20	91,200	$ 214,981.90	$ 1,079,454.00	110,458
Short term debt	$ 71,810.28	$ 488,401.10	201,658	$55,847.14***	$ 288,910.20	91,200	$ 84,990.30	$ 605,131.90	110,458
Non-farm debt									
Total non-farm debt	$ 75,427.25	$ 318,963.30	194,307	$75,174.07	$ 276,362.30	89,939	$ 75,645.43	$ 351,558.80	104,368
Total non-farm debt secured by									
farm assets	$ 16,710.71	$ 232,997.60	90,688	$15,669.88	$ 210,749.50	43,863	$ 17,685.70	$ 252,061.60	46,825
Mortgage	$ 27,371.36	$ 154,517.60	73,917	$32,163.39***	$ 144,123.40	35,190	$ 23,016.99	$ 163,270.00	38,727
Other real estate debt	$ 31,105.74	$ 294,220.00	93,039	$28,073.20***	$ 251,812.80	44,907	$ 33,935.10	$ 328,869.00	48,132
Loans for non-farm businesses	$ 22,582.09	$ 314,591.50	92,789	$19,121.80***	$ 267,176.90	44,750	$ 25,805.47	$ 353,049.60	48,039
Personal loans	$ 6,651.73	$ 89,101.48	92,410	$7,662.03***	$ 61,073.70	44,689	$ 5,705.62	$ 108,991.00	47,721
Farm financial security									
Leverage	0.2	1.0	201,549	0.2***	0.8	91,158	0.2	1.1	110,391
DRCU 10.0%	$ 987,896.40	$ 4,554,064.00	201,658	$679,093.50***	$ 2,254,478.00	91,200	$ 1,242,861.00	$ 5,789,908.00	110,458
DRCU 7.5%	$ 1,071,417.00	$ 4,953,617.00	201,658	$736,520.90***	$ 2,452,766.00	91,200	$ 1,347,925.00	$ 6,297,810.00	110,458

***,**,*Significantly different from operations without a spouse working off-farm at the 1%, 5%, and 10% confidence level, respectively

1.4 Empirical strategy

As discussed above, the second barrier to estimating the direct impact of off-farm work on farm fiancial decisions is the endogeneity that comes from the simultaneity of the decision to work off-farm and to participate in government programs, take on debt, and other production considerations. Omitted variables are also a concern, especially given the inherent unobservability of factors that drive people to pursue off-farm work, such as innate motivation or aptitude. In the following section, I outline my empirical strategy, which is designed to overcome these obstacles.

The question of who works off farm, and how much they do so, is the outcome of a household bargaining decision. For the majority of farm households in the data, the outcome of that decision is that the primary operator remains male and the spouse works off-farm. This has remained true over time, with the share of female primary operators holding constant at roughly 5% of farm operations, even as other occupations in the non-farm economy showing signs of increasing gender parity. Further, given this designation, we expect that income earned off-farm by each member of the household to be used differently. In order to capture the extent to which this is true, therefore, our empirical strategy must be able to identify changes to off-farm work opportunities for men separately from those of women.

The empirical model for this paper is motivated first by a need for plausibly exogenous shifts in off-farm work opportunities that are unrelated to agricultural performance. Given the gender-based dichotomy in the off-farm work decision, these shifts should also be separately identifiable as affecting women's work opportunities more than men's, and vice versa. One such example would be the growth, or decline, in traditionally female employment opportunities, such as education or nursing. The second motivation for the empirical strategy comes from the kinds of off-farm work farm operators and their spouses perform. Research by Brown et al. (2013) shows that farm operators and their spouses are over-represented in off-farm management and professional occupations, occupying a higher share of these jobs than either metropolitan or non-metropolitan workers. Further, the share of operators or spouses in these positions increases with farm sales volume, indicating important human capital spillovers between operation of large farm businesses and non-farm professional work. Using one year of the ARMS survey, they show that almost a third of farm operators work in management and a quarter work in sales; for spouses, those shares are 40% and 37%, respectively.

This work highlights the difficulty in limiting shocks to one specific occupation or even sector. An economy-wide shock, differential by both gender and industry, is more conducive for estimating the impact of off-farm work for the farm household. To do so, we rely on a well-developed literature that looks at the labor impacts of the explosion of Chinese manufacturing imports, beginning with Autor et al. (2013). In this paper, the authors use spatial variation in exposure to import competition from China, based on the initial distribution of industries across the country. They find a host of labor market impacts, including increased unemployment and reduced wages and labor market participation between the years 1990 and 2004.

Following their framework, I construct a measure of import penetration of the following form:

$$IP^{cu}_{zt} = \sum_j \frac{L_{zj95}}{L_{z95}} \left(\frac{M^{cu}_{jt}}{Y_{j96} + M_{j96} - X_{j96}} \right) \tag{1.1}$$

where:

$$L_{zj95} \text{ is the employment in } z \text{ of industry } j \text{ in 1995}$$

$$L_{z95} \text{ is total employment in } z \text{ in 1995}$$

$$M^{cu}_{jt} \text{ are imports from China to the US of industry } j \text{ in } t$$

$$Y_{j96} + M_{j96} - X_{j96} \text{ is the initial absorption of } j \text{ in 1996}$$

In this measure, z indexes commuting zones, a geographical aggregation measure based on where people work and used to define local labor markets,[4]; j indexes industries defined by the SIC industry code, and t indexes years.

A gender component to this shock was added to their framework by including the fraction of female workers to the measure of import competition (Autor et al., 2018). With this component, the measure of import penetration separately identifies shocks by gender and by industry, using lagged data for employment counts and share of female workers by industry. This measure appears as follows:

$$IP^{cu}_{zt} = \sum_j f_{zj90} \frac{L_{zj95}}{L_{z95}} \left(\frac{M^{cu}_{jt}}{Y_{j96} + M_{j96} - X_{j96}} \right) \tag{1.2}$$

where:

$$f_{zj90} \text{ is share of women in } z \text{ working in industry } j \text{ in 1990}$$

Everything else as before

The measure I constructed differs from Autor et al. (2018) in a few ways. First, I observe my dependent variables annually, and so instead of using a differenced measure of

[4]There are 722 commuting zones, with complete coverage for the continental United States. They were developed by Tolbert and Sizer (1996).

import penetration, I construct one for each commuting zone in each year between 1996 and 2016. I also change the base years to more accurately reflect the start date of my data (that is, 1996 instead of 1990). Table A.1 in the appendix records the sources used in the creation of this import penetration measure.

Finally, to address the simultaneity between imports from China and local labor market conditions in commuting zones in the United States, I also follow Autor et al. (2013) and Autor et al. (2018) in constructing an instrument for the measure of Chinese import penetration in the US. To do so, I replace the measure of China's exports to the US for each industry with a measure of China's exports to a small set of comparably developed countries for the same industry and year.[5] The instrument is calculated as follows:

$$IP_{it}^{co} = \sum_j f_{ij80} \frac{L_{ij85}}{L_{i85}} \left(\frac{M_{jt}^{co}}{Y_{j93} + M_{j93} - X_{j93}}\right) \tag{1.3}$$

where:

f_{ij90} is share of women in i working in industry j in 1980

M_{jt}^{co} are imports from China to other countries of industry j in t

Everything else as before

The validity of this instrument, through the satisfaction of the exclusion restriction, rests on the assumption that the instrument captures the common component of import growth in the US and in other countries. This common component is due to factors specific to China's changing internal policies and promotion of manufacturing, rather than anything specific to the countries importing its products. Without the instrument, there would be concerns that factors specific to the commuting zones, such as localized de-

[5]These countries are: Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, Switzerland.

mand shocks, driving both the imports in a year as well as the employment-related dependent variables. Autor et al. (2013) also replicate their findings using a gravity trade model, which accounts for domestic demand, and find that correlated import demand shocks across countries are not important for their results. Finally, the instrument is calculated using ten year lags on the employment variables and three year lags on the trade variables to mitigate concerns about anticipatory behavior regarding trade with China affecting contemporaneous employment.

My estimation strategy leverages the instrumented import penetration from China in each commuting zone and in each year to first estimate the impact of changing work opportunities for men and women on the direct indicators of off-farm work decisions. This estimating equation takes the following form:

$$W_{sit} = \alpha_t + \beta_1 \underbrace{IP^{cu}_{szt}}_{=IP^{co}_{szt}} + \beta_2 F_{it} + \epsilon_{sit} \tag{1.4}$$

In this specification, W_{sit} is the on- or off-farm hours or income for gender group s in farm household i in year t; IP^{cu}_{szt} is the Chinese import pressure in commmuting zone z, as defined above in equation 1.2; IP^{co}_{szt} is the Chinese import pressure in other countries o, as defined in equation 1.3; F_{it} is a vector of farm characteristics; and α_t are year fixed effects.

In addition, I estimate a similar equation, with measures of farm debt and household debt, measured at the farm level, as the dependent variables of interest, rather than the intermediate outcomes of measure of work. This is defined as follows:

$$Y_{it} = \alpha_t + \beta_1 \underbrace{IP^{cu}_{szt}}_{=IP^{co}_{szt}} + \beta_2 F_{it} + \epsilon_{sit} \tag{1.5}$$

Here, Y_{it} is the debt in farm operation i in year t, and all other variables are defined as in equation 1.4.

1.5 Results

1.5.1 Estimation results

Table 1.6 presents the results from estimating equation 1.4.[6] These results confirm that the measure of import penetration does have an effect on the intermediate measures of on- and off-farm work for both farm operators and their spouses, albeit in different ways. A one unit increase in female import pressure leads farm spouses to increase their off-farm work hours; however, they do not seem to be earning more for this extra time. The same increase in female import pressure reduces spousal off-farm weekly income by approximately $135. Farm spouses substitute their time away from on-farm work in order to increase their off-farm labor market participation. This increase in non-farm hours in a week for less money indicates that farm spouses are possibly taking more hours at a lower wage or lower skill job. This could be to retain health insurance or other non-wage benefits from working.

Farm operators have the opposite response in terms of off-farm hours. Although they, much like their spouses, are earning less off-farm income, at least some of this can be explained by their reduced hours. There is no significant effect on their on-farm time; the average primary operator in the sample spends more than 40 hours a week doing on-farm work, although this masks significant seasonal variation. These results are robust to the inclusion of controls for operator and spouse characteristics including age, years of

[6] NB: Full tables available on request, pending ERS-ARMS disclosure review.

experience, and education levels. Controls also include a fixed effect for the farm typology and sales class.

Table 1.6: Off-farm import pressure and measures of off-farm labor participation

	Spouse:		
	(1) Off-farm income per week	(2) Non-farm hours per week	(3) Farm hours per week
Female import pressure	-135.3***	0.157**	-0.300***
	(44.14)	(0.0621)	(0.0680)
Mean dependent variable	$228.05	15.5	10.8
	Operator:		
	(1) Off-farm income per week	(2) Non-farm hours per week	(3) Farm hours per week
Male import pressure	-742.8***	-0.201*	-0.0696
	(259.1)	(0.104)	(0.161)
Mean dependent variable	$416.00	11.1	42.8
Observations	143,601	129,824	182,573

Standard errors robust to correlation at the CZ-level in parenthesis
Farm level controls and year FE surpressed
*** p<0.01, ** p<0.05, * p<0.10

Tables 1.7 and 1.8 show the results for estimating equation 1.4 where Y_{it+1} equals the total volume and the components of farm debt and the total volume and components of household debt, respectively. We see opposite impacts on farm debt than on household debt, depending on the household member whose employment opportunities are shocked. A one unit increase in import pressure for the operator's employment opportunities leads to an increase in farm debt with no effect on household debt. The opposite is true for the farm spouse: when her off-farm work opportunities are shocked, household debt increases without farm debt being affected. This suggests that the farm operator and his spouse allocate the money they earn differently, with the operator's off-farm income bolstering the farm business and the spouse's income going towards the household.

The increase in farm debt following a shock to male income-earning opportunities in the off-farm economy could reflect one of two adjustments by the farm operator, which will require further exploration, potentially by leveraging more of the loan-level information contained in the ARMS survey instrument, as the current analysis using the rougher decomposition of farm debt is not elucidating. On one hand, the increase in farm debt could reflect the operator expanding the operation in light of what he perceives as declining opportunities for income earning outside of the farm sector. In this case, the impact of the off-farm labor market on the question of future farm viability is not necessarily negative. However, the other case is that the operator had been using off-farm income as a way of paying down parts of the farm debt, and his capacity to do so has been hampered by declining work opportunities and off-farm income.

Table 1.7: Off-farm income shocks and farm debt

	(1) Farm debt	(2) Non-real estate debt	(3) Real estate debt	(4) Short term debt
Female import pressure	-2,696.3	-792.3	-1,369.3	-927.1
	(2,889.1)	(600.5)	(1,321.2)	(724.3)
Male import pressure	3,312.3***	-304.2	-67.2	-1,226.6
	(1,961.3)	(978.8)	(2,065.8)	(1,463.3)
Mean dependent variable	$255,562.30	$66,143.33	$179,883	$70,687.18
Observations	127,889	127,901	127,901	127,901

Standard errors robust to correlation at the CZ-level in parenthesis
Farm level controls surpressed
*** p<0.01, ** p<0.05, * p<0.10

Increases in female-dominated import pressure have no impact on the level of farm debt. Instead, the effect shocks to off-farm work opportunities for the farm spouse are concentrated in increased debt for the household. The categorical breakdown shows that this impact is driven by increases in other real estate (i.e., other non-farm real estate other than the household's primary dwelling) debt and in loans for non-farm businesses. One possible explanation is that farm spouses faced with declining work opportunities in their local area seek to diversify their earnings, either through real estate or other non-farm businesses.

Interestingly, the amount of non-farm debt secured by farm assets declines when either spouse's off-farm work opportunities is shocked. This is the only outcome where the effect of both import pressure measures move in the same direction, although the coefficient for the male shock is almost twice as large. Although it is unclear what exactly drives this result, it is suggestive of a move by both spouses to separate the farm's finances from the household's as import pressure causes either spouse's off-farm work opportunities to decline.

Table 1.8: Off-farm income shocks and household debt

	(1) Total household (non-farm) debt	(2) Non-farm debt secured by farm	(3) Mortgage	(4) Other real estate debt	(5) Loans for non-farm business	(6) Personal loans
Female import pressure	2,026.7***	-907.8**	269.2	1,564.3***	1,032.0**	-24.1
	(755.7)	(449.3)	(524.8)	(588.2)	(501.7)	(235.5)
Male import pressure	-1,382.7	-1,757.3**	1,411.0	-1,517.8	-1,600.0*	27.6
	(1,561.7)	(776.7)	(1,419.1)	(1,112.6)	(970.4)	(606.5)
Mean dependent variable	$77,572.77	$16,689.36	$27,280.29	$30,986.40	$22,567.52	$6,650.16
Observations	57,889	58,614	58,684	57,749	57,708	57,585

Standard errors robust to correlation at the CZ-level in parenthesis
Farm level controls surpressed
*** p<0.01, ** p<0.05, * p<0.10

The final set of results looks at two possible measures of a farm operation's financial health. Leverage and maximum debt repayment capacity both attempt to quantify the extent to which a farm's level of debt is manageable given their level of assets and revenue. These sort of measures serve as a place to start analyzing whether or not the observed expansion of farm debt represents a financially healthy farm sector. I find that shocks to male off-farm opportunities increase a farm's leverage slightly, with the opposite effect for shocks to female opportunities. Further, shocks to the primary operator's off-farm income earning possibilities causes both maximum debt repayments to decline. Although the coefficients are small, this does provide some preliminary evidence that there may be long-term financial implications for the farm sector as a result of declining off-farm work opportunities, especially for the male primary operator. Further work exploring this possibility is certainly required.

Table 1.9: Off-farm income shocks and financial indicators

	(1) Leverage		(2) DRCU 10.0		(3) DRCU 7.5	
Female import pressure	-0.002*		-1,808.4		-1,968.8	
	(0.0009)		(5,240.6)		(5,704.1)	
Male import pressure		0.004**		-13,980.7*		-15,149.6*
		(0.002)		(8,746.1)		(9,510.4)
Mean dependent variable	0.16		$987,987.30		$1,071,408	
Observations	127,894		127,901		127,894	

Standard errors robust to correlation at the CZ-level in parenthesis
Farm level controls surpressed
*** p<0.01, ** p<0.05, * p<0.10

1.5.2 Discussion

One of the primary takeaways from this work is that men, especially those with the demographic and human capital attributes typical of a farm operation's primary operator, out-earn farm spouses in the ARMS sample in their off-farm occupations. This is despite female farm spouses working more hours, on average, at these off-farm jobs. In some cases, this can be attributed to the role off health insurance availability in determining off-farm labor market participation, in that women adopting this strategy care more about the non-wage benefits of the job than any other aspect, including salary. However, it remains the case farm operators earn more than farm spouses in the same commuting zones, even when their educational attainment is the same. On average, the off-farm earnings of farm operators are more than $10,000 per year higher than those of farm spouses in the same commuting zones with the same levels of educational attainment. These differences are most pronounced for those with the lowest levels of education and those with the highest. Operators without a high school degree earn more than $13,500 more off-farm income per year than farm spouses without a high school degree; the difference is close to $12,000 per year for operators and spouses who have a Bachelor's degree or more.

Given the higher income earning potential for farm operators, especially in rural areas, these simple differences call into question the tendency for women to work off-farm for more hours than male operators. The opportunity cost for male operators, for each hour they spend on on-farm work, is likely to be higher than that of his spouse's. The nature of modern farm management is much less physically demanding than even twenty years ago; there are few factors, other than intangibles like tradition and gender norms, that explain why the rate of female primary operatorship has not grown over time. Especially for farm households in areas with expanding opportunities for employment in traditionally male jobs, the cost to maintaining these roles could be as high as $10,000 of "lost" income per year.

1.6 Conclusion

As more and more of the population of the United States moves to urban areas, the role of farm operations in sustaining rural communities and rural economies becomes all the more prescient. The two are crucially linked through the off-farm work that is needed to sustain farm operations, but that is increasingly difficult for rural communities to provide. This work provides evidence for the ways in which declining opportunities in the rural economy contribute to the financial outcomes of farms across the distribution of size and sales class. At its most extreme, this work shows how the well-noted trends in farm consolidation and farm closure, exemplified by the "hollowing out of the middle" of the farm size distribution, and of rural decline, can be either mitigated or expedited based on the conditions in the local labor market.

For policymakers, it is crucial to know how linkages between the farm and non-farm sectors function. Off-farm work may be an important mediator preventing further consolidation of rural agricultural land and the loss of "family farms," as they are typically conceived. It is also natural that off-farm income would have an impact on use of other types of farm income support, especially those designed to target the variability of farm income. From a policy perspective, it is increasingly important to know how changing economic conditions change the participation rates of these programs, as their value for the agricultural sector and the future of economic support for farm operations is under almost constant debate and discussion at the federal level. Off-farm employment is clearly one of the primary ways a farm operation stabilizes and boosts its own income. Declining job opportunities in rural areas may mean increased reliance on government support for farm operations, an unwelcome outcome for both policymakers looking to tighten budgets and for farm operators looking to be self-reliant.

The previously unexplored gender component of this question also sheds light on how farm households operate internally, and how continued support for female labor force participation and a narrowing of the gender wage gap can have important implications for the survival of farm businesses. Future work on this topic will repeat the estimation, exploiting changes in access to health insurance as the source of exogeneity driving the decision of whether or not to work off-farm. These changes include state and time variation in the deductability of health insurance premiums for the self-employed, and Medicaid expansions and other forms of increased access brought about by the Affordable Care Act. As that particular legislation and the changes it brought continue to be scrutinized, it is important for policymakers to understand how the expansion of health insurance networks impact the agricultural sector. If the expansion of access or change in price of non-employer-provided health care allows more operations to benefit from increased on-farm hours from the operator and his spouse, that would be a key unintended consequence of the ACA with implications for farm finances.

I show results that indicate that the farm operator and his spouse allocate the money they earn off-farm differently, and that there are differential effects by gender in response to changing off-farm work opportunities. The "primary operator" designation is more than just nomenclature; shocks to his income or work opportunities off the farm impacts the farm financially in ways that shocks to his spouse's opportunities do not. When the spouse's income is shocked, the household, rather than the farm operation, takes on more debt. These results indicate that, contrary to previous theoretical models of the US farm household, these are not two identical agents, but instead each may serve a more specialized role in the operation of the farm business. Finally, given that the operator and spouse tend to interact with the labor market very differently, programs and legislation implementing policies designed to revitalize rural economies should be attentive to the type of labor market participation they are encouraging. Thriving rural spaces, farm operations, and farm households are interlaced, and all three must be supported to have a healthy and sustainable future for any of them.

CHAPTER 2

CROP INSURANCE AND AGRICULTURAL CREDIT USE

with Jennifer Ifft and Todd Kuethe

2.1 Introduction

The United States Department of Agriculture (USDA) has pursued a variety of programs to address "the low earnings of most farm people and the great instability of income from farming" since the 1930s (Schultz, 1945, pp.x). Over the past two decades, the focus of U.S. farm policy has shifted from bolstering low earnings through direct income support to mitigating income instability through risk management. Through increased federal subsidies to crop insurance premiums, crop insurance has become the foundation of the federal farm safety net (Glauber, 2013; Bulut et al., 2012). The stated intent of the Federal crop insurance program is to support the "economic stability of agriculture"[1]. While "economic stability" is not formally defined in the legislation, it is commonly interpreted as supporting income levels and limiting income variability. Previous research, however, has argued that subsidizing crop insurance encourages farmers to assume more risk (Goodwin and Smith, 2013) or redistribute some of the reduced production or revenue risk to other parts of their enterprise, such as increased borrowing or financial risk (Gabriel and Baker, 1980). In extreme cases, the re-balancing of risk towards financial leverage could lead to reductions in farm equity (Featherstone et al., 1988). Consistent access to credit, however, is foundational for the creation of farm wealth and financial stability. Further, if all or even some farms are credit rationed, increased credit use may be welfare-improving. The impact of crop insurance on credit use thus has important

[1] Agricultural Adjustment Act Of 1938 Federal Crop Insurance Act

policy implications–as increased borrowing could either support or undermine the "economic stability of agriculture."

This study therefore seeks to provide novel, robust evidence on the relationship between crop insurance and credit. Although previous empirical research suggests that crop insurance and credit use are correlated, there are competing theories about its nature and direction. From a lender's perspective, crop insurance could serve, either explicitly or implicitly, as a form of collateral or provide a partial loan guarantee by lowering the probability of default. Through expected indemnity payments, operating loans are more likely to be repaid in low yield or revenue states. Crop insurance as a form of collateral has been previously noted (Collins et al., 2011; Liang, 2014), as it may increase lenders' willingness to extend operating credit. In addition, the decline in production or price risk provided by crop insurance could lead producers to increase financial risk (leverage), thus increasing their demand for credit (Gabriel and Baker, 1980). In our conceptual framework, we consider how both lender and producers' response to crop insurance would influence observed debt levels and draw implications for our empirical analysis of farm-level debt use.

This study builds on a large body of work on the drivers of credit access, farm debt structure, crop insurance entry and exit, and the potential relationship between production and financial risk. Credit access and use varies over different economic conditions and farm types. Hubbs and Kuethe (2017) find that, since the late 1970s, both excess supply and demand have existed in agricultural credit markets. Farm-level analysis suggests that farm operations often cannot access as much debt financing as they would like, though the degree of excess demand fluctuates by period and farm type (Briggeman et al., 2009; Hart and Lence, 2004; Bierlen et al., 1998; Bierlen and Featherstone, 1998). Katchova (2005) find that farm size, operator age, and various indicators of risk management are related to total farm debt levels. Ifft et al. (2018) used more recent farm survey data to show that similar factors predict demand for new credit, as well as off-farm income. Similar to debt, crop insurance participation is associated with larger farm size across studies in several countries (Sherrick et al., 2004; Enjolras and Sentis, 2011; Santeramo et al., 2016). Farms facing higher production risk are similarly more likely to use crop insurance (Sherrick et al., 2004; Enjolras and Sentis, 2011), while the relationship with debt structure is not consistent across studies (Sherrick et al., 2004; Enjolras and Sentis, 2011). Factors that influence participation are different than those that ensure duration of participation or exit (Cabas et al., 2008; Santeramo et al., 2016). Previous research has explored the correlation between farms' production risk and financial risk, including farm income stabilization policies and debt structure. Uzea et al. (2014) found that farms benefiting from Canada's risk-reducing government support policies may take on more financial risk, but the strength of the relationship varies across farm types. de Mey et al. (2014) similarly find farm-level evidence of risk balancing in the European Union, but the strength of the relationship also varies by farm type and country. Previous empirical work found a correlation between U.S. crop insurance enrollment and farm debt levels (Ifft et al., 2015), which suggests that crop insurance may influence credit use.

While previous studies demonstrate a correlation between crop insurance participation and farm debt use, the existing literature fails to provide a clearly identified mechanism or a theoretical justification for the observed relationship. In addition, the empirical techniques employed do not meet the requirements for casual identification. To address the shortcomings of the existing literature, we develop a conceptual framework that examines the relationship between crop insurance and farm credit use through changes to both lender and farmer behavior. We first discuss how lenders might increase the supply of operating credit through crop insurance's "collateral effect." We then examine different approaches lenders pursue to evaluate small business credit decisions, following Berger and Udell (2002), and draw implications on how crop insurance's collateral effect may influence these decisions. Next, we use a risk balancing framework to discuss under which conditions we might observe an increase in farm financial risk and associated credit demand, as crop insurance use or coverage levels increase. The implications of credit rationing for changes in credit supply or demand are taken into account. Based on consideration of these factors, we develop testable hypotheses on the observed relationship between crop insurance and credit use. We develop an empirical strategy (1) to test these predictions and to improve our understanding of how crop insurance influences debt use and (2) to determine whether crop insurance has a causal relationship with farm debt use. We use recent advances in empirical techniques to better understand how crop insurance affects farm operators' financial management decisions.

Our empirical analysis exploits a number of advantages of the USDA's Agricultural Resource and Management Survey (ARMS) to both address the simultaneity of crop insurance and credit use and to shed light on the underlying mechanism driving the observed relationship between crop insurance and credit use. Previous empirical studies of the (secondary) impacts of crop insurance are complicated by the simultaneity between crop insurance take-up and farm-level management decisions. Because some farms appear in the survey in multiple years due to intentional over-sampling, we are able to control for unobserved farm-level characteristics, such as managerial ability, that likely influence both crop insurance adoption and farm debt use. However, the farm fixed effects approach with panel data cannot fully address the simultaneity of crop insurance and debt use, as the direction of the relationship is uncertain. That is, higher debt use may influence crop insurance participation decisions. The panel of repeat observations also allows us to exploit variation in crop insurance policy design to identify the *causal* relationship between crop insurance and farm debt use, following Weber et al. (2016). Finally, the richness of the ARMS data allows us to examine whether borrowers that are more likely to be credit constrained under certain lending practices are able overcome credit constraints through crop insurance use. Given the central role of crop insurance in the farm safety net, such findings are important to policymakers, program administrators, agricultural lenders, insurance providers, and researchers.

The remainder of the paper is organized as follows. Section 2.2 outlines the conceptual framework for our empirical analysis, which is based on lending behavior and risk balancing. Section 2.3 summarizes our data (2.3.1) and empirical approach (2.3.2). Section 2.4 summarizes our empirical results, and section 2.5 discusses the implications of our key findings for policy and future research.

2.2 Conceptual framework

Crop insurance is the cornerstone of the federal farm safety net, designed to mitigate the effects of farm income instability by limiting downside yield or revenue risk. By limiting downside yield or revenue risk, crop insurance potentially influences lender and producer decisions related to farm debt. An increase in credit levels due to crop insurance use implies that (1) lenders were willing to supply more credit and (2) producers were willing to take on more credit. However, we do not know *a proiri* if there was excess supply or demand for credit in the sector and other factors that would allow us to fully characterize the credit market equilibrium. Given the difficulties of establishing credit market equilibrium prior to and after the introduction of crop insurance, we focus on lender and farm-level decisions.

To disentangle these relationships, we first show how crop insurance – from a first order perspective– may influence lender decisions. We then use the different approaches to small business lending decisions defined by Berger and Udell (2002) to develop testable hypothesis related to lenders' decisions to extend operating loans, as they relate to crop insurance. Producers' decisions are evaluated by examining internal adjustments to changes in risk levels following the risk balancing framework.

Our conceptual framework is subject to the following assumptions. We assume that most farm loans are self-collateralized or self-liquidating, when collateral is required (Office of the Comptroller of the Currency). Most farm loans are secured by "hard assets," and the type of collateral varies by the purpose of the loan. Long-term debt is typically secured by farm real estate or machinery and equipment. Short-term or operating debt is typically secured by crops or livestock. It is also worth nothing that we focus on the direct effect of crop insurance on credit, not second or third order effects. For example, crop insurance premium subsides imply a positive expected value for participation, with a long-term wealth effect. Our model does not consider this or other tertiary effects.

2.2.1 Crop insurance and lending decisions

It is straightforward to show how crop insurance, under a variety of assumptions about lenders' risk preferences, can increase credit supply through its "collateral effect." Consider the following simple illustration. A farmer wishes to receive a short-term operating line of credit, and the lender requires the current crop to serve as collateral. That is, if the farmer defaults on the loan, the lender gains full ownership of the current crop. In a typical year, the full revenue potential (H) is realized, but in a poor year, the low revenue

(L) is realized.[2] To ensure repayment, lenders are willing to extend a loan of up to 100 percent[3] of crop revenue in the bad state. With crop insurance, farmers pay premium p and receive an indemnity payment I in the bad state when a loss occurs. This increases the loan value from L to $L + I$ -p and increases loan value as long as $p < I$.

The example above assumes risk aversion on the part of lenders. However, under subsidized crop insurance, this example generalizes to a risk neutral lender that will lend up to the expected crop value. In this case, the probability of a bad state is γ and expected value of the crop is $(1 - \gamma)H + \gamma L$ without crop insurance and $(1 - \gamma)H + \gamma(L + I) - p$ with crop insurance. The amount of available credit increases by $\gamma I - p$. γI will be greater than p if the underlying total premium (subsidy plus farmer premium) is actuarially fair. Thus, even if lenders are not risk averse and will loan more than the lowest likely "bad" outcome, the premium subsidy will raise the expected value of collateral. Liang (2014) provides a more formal treatment of how crop insurance can address credit constraints, that similarly demonstrates how crop insurance can increase the amount of loans lenders are willing to extend.

The different approaches lenders use to make credit decisions provides useful intuition for how this "collateral effect" might be reflected in observed lending levels. Berger and Udell (2002) characterizes lending to small firms, who typically rely on private debt instead of raising equity, as either relationship-based or transactional. Transactional lending relies on "hard information" through three approaches: asset values, financial statements, and credit scores[4] (Berger and Udell, 2002). For asset-based lending, crop insurance provides an increase in collateral value for operating loans. Given the most operat-

[2]Note: While this simple illustration could be generalized to yield or revenue protection (YP or RP) insurance, we refer to revenue to maintain clarity.

[3]This number may be lower. We use 100% to make our example clearer, without loss of generality.

[4]For the purpose of this paper, we focus on asset values and financial statements and assume that crop insurance is not accounted for in formal credit scoring.

ing loans are self-liquidating (Office of the Comptroller of the Currency), crop insurance could have a general or universal impact on operating loan levels. Before crop insurance was widely available, yield risk was likely uninsurable due to systemic risk (Miranda and Glauber, 1997; Skees and Barnett, 1999). The correction of this market failure in insurance markets also increased the likelihood of operating loan repayment, potentially leading to investment at or closer to socially optimal levels. If operating loan decisions are primarily based on crop-collateral (asset) value, we would expect to see a general increase in operating loans due to crop insurance. However, lenders who use a financial statement-based approach to lending may primarily consider various indicators of financial health or cash flow.

Relationship lending, by contrast, relies on "soft information" based on a loan officer's knowledge of a firm or community and a more subjective assessment of the firm's repayment ability. If relationship lending approaches are used, crop-collateral may be less important, although not necessarily independent of lender decisions.[5] In either financial statement- or relationship-based lending, weaker borrowers may need to establish guaranteed collateral to receive the full or maximum operating loan level. In the former case, the value of crop-collateral becomes important for farms with a weaker financial situation. For relationship-based lending, there may be situations in which lenders are unfamiliar with a farm operation and thus unable to make a decision on creditworthiness. In this case, lenders who otherwise rely on their experience or knowledge of operations may require "hard proof" of repayment ability for unfamiliar or new farm operations. In either case, crop insurance is most relevant for credit availability for less financially healthy farms, or for new or beginning farms.

[5]For example, crop insurance could signal managerial ability.

2.2.2 Crop insurance and producer credit decisions

Farmers' demand for credit may also increase in response to crop insurance. As formalized by Gabriel and Baker (1980), economic theory suggests that if farmers seek to keep aggregate risk levels constant, crop insurance participants may offset the reductions in yield or revenue risk by increasing their exposure to risk in other portions of their enterprise.

Collins (1985) provides a theoretical model of optimal leverage under risk balancing with the following equilibrium condition:

$$\delta^{\star} = 1 - \left[\frac{\rho \sigma_A^2}{\tilde{R}_A - K} \right]. \tag{2.1}$$

Equation (2.1) suggests that optimal leverage $\delta^{\star}$ is a function of the expected rate of return on assets $\tilde{R}_A$, the variance of asset returns σ_A^2, the opportunity cost of capital K, and the farmer's relative risk aversion ρ. Collins (1985) demonstrates that subsidized crop insurance could both increase the expected rate of return on assets ($\tilde{R_A}$) and decrease the variance of asset returns (σ_A^2), which, in turn, increases the farms' optimal leverage. Featherstone et al. (1988) show that, in extreme cases, policies like crop insurance that decrease production risk could even increase leverage to the point of equity loss. That is, the increases in asset values lead to higher optimal debt levels in a manner that reduces the relative share of equity to (leveraged) assets. If the underlying assumptions of risk balancing hold, we would expect to see an increase in leverage for farms using crop insurance.

Risk balancing theory implies that crop insurance would lead to an increase in financial risk, as reflected in farm leverage. Farms could achieve this by increasing all types of lending, thus increasing demand for credit, or decreasing asset holdings. There are, however, some underlying assumptions of risk balancing that are relevant to the problem we consider. Following Featherstone et al. (1988), risk balancing requires that farmers do not face credit rationing or asset illiquidity. Most long term farm assets, also the collateral from long-term debt, are often considered illiquid due to the significant transaction costs to foreclose on machinery or land and the long term implications for farm business profitability (Barry et al., 1981). Crops and livestock, conversely, are relatively more liquid forms of capital. Hence, farmers may be more likely to be observed increasing operating loan levels as a risk balancing response to crop insurance. It is also worth noting that given the illiquidity of farm real estate and its large share in farm wealth, we would be much more likely to see an increase in credit demand as a risk balancing response to crop insurance, which is the focus of this study.

Credit rationing could limit risk balancing behavior; however in the previous section we discuss how crop insurance may address certain types of credit rationing and lead to increased operating loan use. Petrick (2005) defines credit rationing as "persistent" unmet demand for credit, while under-investment implies a level of investment that is less than socially optimal. While it may be difficult to distinguish between risk balancing vs. relaxed credit rationing as a cause of increased operating loan use, an increase in leverage would provide strong evidence for a risk balancing response, as it would suggest consistently higher unpaid operating loan balance or higher long-term debt.

2.2.3 Potential impacts on observed debt levels

Based on the response of both lenders and farm operators to crop insurance, we may observe different outcomes. Below we develop three hypothesis that can be tested with farm-level financial information.

First, if asset-based lending is a common practice and crops serve as required collateral for operating loans, there will be a general increase in operating loans in the presence of crop insurance. This hypothesis is drawn from the notion that crop insurance serves as "repayment" insurance or partial guarantee under the credit supply channel. The collateral effect would lead to observed increases in operating loan volume, and this relationship may vary depending on how lenders account for collateral value in lending decisions. The collateral effect may be captured through either participation (no crop insurance vs. crop insurance) or coverage level (as coverage level increase, the value of collateral is higher). We can test this hypothesis by considering different measures of operating loan volume. From a farm operator perspective, an increase in operating loans would be consistent with either alleviating credit rationing or risk balancing behavior.

Second, if financial statement or relational lending is more common practice than asset-based lending, crop-collateral value may be an important factor in lending decisions for farms that might be credit rationed due to weaker financial position (financial statement lending) or unfamiliarity to local lenders (relational lending). Similar to our first hypothesis, this implies that crop insurance participation or coverage levels will lead to higher operating loan levels. However, if crop insurance helps borrowers obtain credit in this context, we would expect the relationship to be stronger on average for borrowers with a weaker financial position, or for young or beginning farmers. It is important to note that these first and second hypotheses are not mutually exclusive. In reality, there is likely heterogeneity across lenders, with some relying on asset values and others first considering financial strength or soft information on repayment ability. If we see larger increases in operating loans for these "weaker" borrowers, it would suggest that crop insurance is an important consideration for lenders. Given these noisy or broad measures of weak borrowers, in our robustness check section we also directly test for whether crop insurance coverage and self-reported credit constraints are related, using data from the single year when this information was available (2014). A negative relationship would suggest that crop insurance use allows otherwise weaker borrowers to receive credit, consistent with this hypothesis.

Third, in the presence of risk balancing, crop insurance induces farms to increase financial risk through both crop insurance participation and increasing coverage levels. If the underlying assumptions of risk balancing hold, then crop insurance participation or higher coverage would lead to an increase in leverage. To test this hypothesis, we will consider the relationship between farm leverage and both measures of crop insurance use. We also note that changes to operating loan use, which may be observed if the first two hypotheses hold, do not automatically imply different leverage. To increase their leverage, farms could increase any type of debt.[6] Further, operating loans are typically taken out and repaid within a year and, therefore, may not be reflected in year-end debt and asset balances which are typically used to calculate leverage. We thus consider operating loan volume and year-end balance in our empirical model, as well as discussing results with different measures of long-term debt as a robustness check.

2.3 Data and empirical methods

To test how crop insurance influences farm debt use, we require detailed, farm-level data on crop insurance and financing decisions. We must also address empirical challenges that have complicated previous research on the (secondary) impacts of crop insurance. First, as previously stated, it is difficult to identify the causal outcomes of crop insurance

[6]Farms may also hold lower levels of assets, but we assume this to be unlikely given the high transaction costs and other issues with farmland sales, as well as beyond the scope of this study.

participation plausibly due to (1) unobserved farm-level characteristics such as management acumen that might be correlated and with both insurance and financial decisions and (2) the simultaneity between crop insurance take-up and farm-level financing decisions. There are number of empirical approaches to address the endogeneity of crop insurance adoption, but many of these approaches are difficult to implement given the limited credibility related to the exclusion restriction. In other words, it is difficult to find a measure (an instrumental variable) that is truly unrelated to any particular farm-level production decision but still related to crop insurance participation. Second, most sources of farm-level financial information are cross sectional, which limits researchers' ability to control for unobserved farm-level characteristics that drive both financial and production decisions. For example, the Agricultural Resource Management Survey (ARMS), the most detailed nation-wide farm level survey that is commonly used in agricultural economics research, is cross sectional.

Our study employs two recent advances in modeling farm-level decisions to establish the causal relationship between crop insurance coverage and farm debt use. Our analysis begins with a cross sectional analysis that takes advantage of the full richness of the ARMS data. We then impose increasingly restrictive limits on our sample of farms that allow us to more robustly identify the relationship between crop insurance and credit decisions. In order to estimate the causal linkage between crop insurance and credit decisions, we exploit recent advances for ARMS data developed by Weber et al. (2016). ARMS uses a complex, stratified non-random sampling procedure that intentionally over-samples larger farms. As a result, large farms are more likely to appear in the ARMS sample multiple times, and it is possible to link ARMS observations across multiple years. We discuss potential sample selection bias issues after providing further information on our data.

2.3.1 Data

We begin with ARMS data from 2000 to 2014, which had a total of 286,370 observations. To ensure a sample of farms for which crop insurance was likely to be a viable option throughout our study period, we limit the responses to operations with at least $10,000 value of production of crops with widespread crop insurance availability.[7] This restriction decreases the number of respondent farms in our analysis to 123,122. We refer to this as the "restricted" ARMS cross section.[8]

We compare farms with and without crop insurance in our restricted cross section, along a variety of characteristics, including these measures of debt use, in table 2.1. We find that farms that participate in crop insurance are systematically different from farms that do not. There are only two variables for which participants and non-participants are not statistically different: rice production and ratio of operating loan volume to variable and rental expenses [financed]. Farms that use crop insurance have higher levels of all types of debt, operate more acres, rent a higher share of acres operated, have operators with higher education levels, have higher sales volume, are more likely to produce field crops, are less likely to produce specialty crops and livestock, and are more likely to have farming as a the primary occupation of the principal operator.

Weber et al. (2016) demonstrate there are some differences between "repeat" farms that are surveyed more than once and "non-repeat" farms that appear only once. Farms sampled more than once are larger and have higher values of production than the typical respondent farm. Despite this potential selection bias, these differences are expected to

[7]These crops include: barley, canola, corn (for grain or silage), cotton, oats, sorghum, soybeans, and wheat. According to the RMA, in 2014 these crops together made up about 80% of all federal crop insurance program liabilities.

[8]For comparison purposes, summary statistics for the entire ARMS cross section and the "restricted" cross section are provided in the Appendix (table B.3).

have limited impact on our analysis, as the larger farms and farms with higher value of production are more emblematic of farms that participate in crop insurance. For example, the USDA defines a farm as any firm that typically produces at least $1,000 of agricultural products in a given year. Most smaller farm operations in the U.S. are not operated as businesses; the median U.S. farm typically has negative returns to farming activities (Economic Research Service, 2016). These small farm enterprises are extremely unlikely to participate in Federal crop insurance. The repeat farms are much more likely, therefore, to be representative of operations that use crop insurance. To explore the the implications for our regression analysis, we will compare regression results from the restricted cross section to the pooled restricted panel.

Dependent variables

ARMS provides many measures of farm debt use. For short-term debt, these measures include seasonal production loans obtained and repaid within the reference year [repaid], short-term debt that remains at the end of the year [dshort], and the total amount of short-term debt outstanding and repaid within the year [dshort + repaid = totalshort]. All three measures provide important information on operating loan levels. We also examine the impacts of crop insurance on long-run farm solvency using the debt-to-asset ratio [leverage]. In our robustness check section, we briefly discuss results using additional measures of debt use, which are reported in our supplemental appendix.

Measuring crop insurance

We use two complementary measures of crop insurance participation (P_{it}), both of which can directly affect credit supply and demand. The first measure is a binary indicator that equals one if the operation had acres enrolled in federal crop insurance in the reference year t. The binary indicator, therefore, allows us to compare participant versus non-participant farms. The variable has been widely used in the existing literature to measure the impact of the crop insurance program (see Ifft et al., 2015, for example). This measure also has the advantage of being asked on more versions of the ARMS survey, yielding a pooled cross section of 91,171 farm-level observations.[9]

The second measure of crop insurance participation (P_{it}) is the natural log of insurance premiums paid per acre operated. This continuous measure reflects the variation in coverage levels, as well as the value of crop being insured. A higher level of crop insurance premiums will generally correspond to higher coverage levels, or a larger share of production that is protected from yield or revenue loss. This measure is available from 2000-2014, but was not on all versions of ARMS surveys for all years, yielding a total of 88,867 observations in the restricted cross section. Actual coverage level were reported in the 2014 ARMS-equivalent survey, which we analyze in our robustness check section.

[9]Due to changes in the ARMS survey instrument, the "acres enrolled" dummy is only available for 2002-2013, excluding 2012.

Table 2.1: Summary statistics: Cross section by insurance status

	Any Insurance			No Insurance			Difference
	Obs.	**Mean**	**Std. Dev.**	**Obs.**	**Mean**	**Std. Dev.**	**significant at:**
FCI participation[b]	64,991	0.954	0.210	26,180	-	-	
Premium paid per acre ($)	64,145	10.03	17.56	24,722	-	-	
Outcomes							
Loans repaid within a year	86,989	$ 197,897.10	$ 673,003.30	36,133	$ 106,731.80	$ 782,117.70	***
Debt remaining at end of year	86,989	$ 103,916.10	$ 480,045.50	36,133	$ 74,375.34	$ 544,592.50	***
Total short term debt	86,989	$ 301,813.20	$ 936,834.50	36,133	$ 181,107.20	$ 1,035,899.00	***
Short term debt/variable expenses	86,976	0.562	0.837	35,884	0.541	28.98	
Real estate long-term debt	86,989	$ 211,898.70	$ 841,170.00	36,133	$ 194,386.40	$ 1,013,182.00	***
Non-real estate long-term debt	86,989	$ 87,799.53	$ 376,065.30	36,133	$ 77,372.48	$ 622,253.00	***
Debt-to-asset ratio	87,575	0.22	2.52	35,516	0.19	4.83	
Debt repayment capacity utilization	87,589	$ 24,306.30	$ 477,690.90	35,533	$ 32,308.99	$ 675,207.50	**

[a] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

[b] Measured as a binary variable equal to 1 if the operation has any acres enrolled in Federal crop insurance

Creation of an ARMS panel

The ARMS panel is a sub-sample of the ARMS cross section and an artifact of the over-sampling of larger farms. The differences between these repeat farms and those that appear only once are covered in detail by Weber et al. (2016). There are 92,272 farm-year observations in the full panel for the years we consider; most of these are farms that appear only twice, although there are farms that appear between three and eight times. Similar to Weber et al. (2016), for our panel analysis, we further restrict the sample of repeat farms to those that have at least one observed year of crop insurance participation and meet the same value of production requirement as the restricted cross section ($10,000 value of production of crops with widespread crop insurance availability). For simplicity, we will refer to our restricted data set of repeated ARMS observations as the "restricted panel."

The restricted panel farms differ markedly from the nationally-representative farm using the USDA definition and from typical non-repeat sampled farms. These sample restrictions, however limiting, make it less likely that our results are driven by comparing operations for which crop insurance is available to those for which it is not. The restricted panel farms are large operations with high levels of production and large loans. The average farm in the restricted panel operates more than 2,500 acres, and the average annual operating loan volume is nearly $500,000. In addition, 65% of the restricted panel operations had positive crop insurance premium expenditures, with an average premium payment of $7.35 per acre. The operations in the ARMS panel are not marginal participants in crop insurance programs. The average number of acres enrolled, per operation and across all years, is about 1,390.

Farms with and without crop insurance (based on acreage and expenses) from the restricted panel are compared in table 2.2. A similar share of farms in the restricted panel had some crop insurance as in the restricted cross section. In addition, farms with and without crop insurance are statistically different for nearly every measure considered in our analysis. Farms with crop insurance have higher debt levels for almost every measure, are slightly younger, operate more acres, and have higher levels of corn, soybean, and wheat production.

Table 2.2: Summary statistics: Panel by insurance status

	FCI Panel: Insurance			FCI Panel: No Insurance			Difference
	Obs.	**Mean**	**Std. Dev.**	**Obs.**	**Mean**	**Std. Dev.**	**significant at:**
FCI participation[b]	16,780	0.936	0.245	5,591	-	-	
Premium paid per acre ($)	21,177	$ 9.69	$ 15.69	6,744	-	-	
Outcomes							
Loans repaid within a year	23,137	$ 326,511.90	$ 1,063,437.00	7,820	$ 225,803.00	$ 1,331,468.00	***
Debt remaining at end of year	23,137	$ 157,104.40	$ 763,784.40	7,820	$ 157,115.70	$ 789,984.00	
Total short term debt	23,137	$ 483,616.30	$ 1,462,872.00	7,820	$ 382,918.70	$ 1,673,101.00	***
Short term debt/variable expenses	23,135	0.570	0.791	7,795	0.346	1.040	***
Real estate long-term debt	23,137	$ 313,314.50	$ 1,152,952.00	7,820	$ 378,051.10	$ 1,762,606.00	***
Non-real estate long-term debt	23,137	$ 133,104.60	$ 506,455.10	7,820	$ 158,151.10	$ 892,930.30	***
Debt-to-asset ratio	23,136	0.23	1.20	7,819	0.22	2.28	
Debt repayment capacity utilization	23,137	$ 42,052.86	$ 785,653.40	7,820	$ 73,220.44	$ 1,059,810.00	***
Operator characteristics							
Operator age	23,137	54.27	10.92	7,820	55.60	11.53	***
Acres operated	23,137	2772.20	5311.63	7,820	1744.10	8277.43	***
Soybeans share	23,137	23.61%	23.92%	7,820	11.95%	20.28%	***
Corn share	23,137	20.31%	22.11%	7,820	10.75%	18.22%	***
Wheat share	23,137	11.11%	17.81%	7,820	5.06%	12.60%	***

[a] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

[b] Measured as a binary variable equal to 1 if the operation has any acres enrolled in Federal crop insurance

To confirm that the differences between farms with and without crop insurance participation are systematic and persist over time, we do the same comparison between participants and non-participants in 2004 and in 2013. These comparisons are also reported in the Supplemental Appendix (tables B.14 and B.15 for the restricted cross section and in tables B.16 and B.17 for the restricted panel). The differences between crop insurance participants and non-participants that we observe in 2004 remain almost a decade later in 2013. Thus, the two groups appear to not become more similar over time. In the restricted panel, we do see a larger operated acre differential in 2013 relative to 2004, but this is likely due to year-to-year intentional changes in the ARMS sample. That is, the ARMS periodically over-samples specific subsets of farms to support sub-sector level analysis of critical issues. For example, the 2004 ARMS over-sampled hog farms, and the 2013 ARMS over-sampled rice farms. We control for these changes to the ARMS sample in our regression analysis. The differences between crop insurance participants and non-participants are also observed in our restricted cross-section, with some exceptions. For example, the difference between long-term real estate and non-real estate debt did increase over time. However, there is no evidence that the two groups were more similar in 2003 and then diverged, or that they have grown more similar over time.

2.3.2 Empirical model

Cross section model

We first estimate a cross sectional model, which allows us better understand the potential limitations of the restricted cross section and panel datasets. Given the predictions of increased short term debt and leverage outlined under different hypothesis in Section

2.2, we use a variety of measures of short term debt and leverage as our key dependent variables, which we collectively refer to as 'measures of debt use'. Let y_{it} represent the measure of debt use by farm i in year t. We posit that farmers' debt use is driven by a set of farm and operator characteristics ($\boldsymbol{F_{it}}$), as well as crop insurance participation (P_{it}). For each farm i in year t, this relationship can be expressed using the regression equation:

$$y_{it} = \beta_0 + \eta_1 P_{it} + \boldsymbol{\beta F_{it}} + \tau_t + \gamma_s + \varepsilon_{it} \tag{2.2}$$

where $\boldsymbol{F_{it}}$ is a vector of farm and operator characteristics, P_{it} is a variable that represents farmers' crop insurance participation, τ_t and γ_s are year and state fixed effects, and ε_{it} is a white noise error term. Year effects allow us to capture price movements, which would have been common given our restriction to field crop farms. Given the general increase in commodity prices across our study period, the year effect is an important control for these trends. For this and all other cross sectional models we estimate standard errors to be robust to correlation at at the strata level, to take into account ARMS survey design (Weber and Clay, 2013). The unknown coefficients $\boldsymbol{\beta}$ and η_1, therefore, capture the relationship between farm debt use and farm and operator characteristics and crop insurance participation, respectively. The ARMS data provide a rich set of farm and operator characteristics $\boldsymbol{F_{it}}$, including operator's education, age, total off-farm income, occupation, retirement status, gender, race, acres operated, farm sales class, share of acres owned, share of cropland operated, and farm specialization. Farm specialization covers multiple crop and livestock specializations based on USDA definitions, as well as categories for "other crops" and "other livestock."

The USDA makes periodic adjustments to ARMS to capture important changes in the agricultural sector. The ARMS-equivalent 2014 Tenure, Ownership, and Transition of Agricultural Land (TOTAL) survey included a number of additional questions related to crop insurance coverage levels, policy form (yield vs. revenue guarantees, and unit structure for several major commodities). While premium paid per acre (employed in the preceding analysis) captures the variation in coverage levels, it also reflects production history and various features of the insurance product, projected prices, and other factors. The 2014 ARMS questions, therefore, provide a more direct measure of insurance coverage levels and program features. In addition, the 2014 ARMS also includes self-reported risk tolerance. Risk preferences are unobserved in almost all other years and may have a strong relationship with both credit and insurance decisions and is one of the key factors of the risk balancing equilibrium conditions (Equation 2.1, ρ). As a result, we also estimate equation (2.2) substituting coverage level as our measure of insurance coverage (P_{it}) and control for self-reported risk tolerance (ρ). We are able to include actual acre-weighted average coverage level of the six crops included in the survey, the share of acres covered under a revenue policy, and a fixed effect for the insured commodity. We estimate this model with several of our key dependent variables: totalshort, financed, dshort, repaid, leverage, and operating loan interest rate.

The 2014 ARMS survey also provides three additional dependent variables that explicitly measure credit rationing, which could be influencing lender and producer response to crop insurance availability. The first variable [denied] captures whether or not the respondent reported being denied credit or not receiving all of the credit requested. The second variable [deterfromcredit] measures whether or not the respondent did not apply for credit due to fear of rejection, high cost of application, or risk associated with debt. The third variable [creditprob] is an indicator variable that takes the value of 1 if the respondent was either denied or deterred.

ARMS Panel Models

In addition to the cross section model, we estimate a similar set of regressions using our restricted panel of repeat ARMS observations. While we start with an estimating equation similar to (2.2) using state fixed effects and standard errors clustered at the farm level, the structure of our panel data allow us to run a more restrictive and robust specification with farm-level fixed effects (within estimation) to control for farm-specific characteristics that are difficult or impossible to observe. For example, farms that use crop insurance may have better financial management skills and may also be more likely to use credit. This fixed effect also addresses fixed local market and price conditions, which may vary over space. Using the panel data, the relationship between debt use and crop insurance (2.2), can be expressed:

$$y_{it} = \beta_0 + \beta_1 P_{it} + \boldsymbol{\beta G_{it}} + \tau_t + \gamma_i + \varepsilon_{it} \tag{2.3}$$

where γ_i are farm-fixed effects and $\boldsymbol{\beta G_{it}}$ are time-varying farm characteristics, including the number of acres operated, the operator's age, operator age squared, and the share of soybean, corn and wheat acres out of the total acres operated. While operator age may represent a time trend for farms with the same operator over our study period, several farms likely had a change in operator (Katchova and Ahearn, 2017). The continuous measure of crop insurance coverage yields a total of 30,957 farm-level observations in our restricted ARMS panel. For all panel models, we estimate standard errors to be robust to correlation at the farm level.

Instrumental variable identification strategy

As previously noted, farmers make decisions related to crop insurance coverage and debt use simultaneously. While our farm fixed effects model (2.3) addresses the issue of unobserved farm characteristics that influence both debt and crop insurance decisions, it is possible that unobserved, time varying farm- or market-level factors could also influence these decisions. For example, increasing demand for credit could lead lenders to encourage higher crop insurance use. To address this simultaneity, we use an instrumental variable developed by Weber et al. (2016), that was demonstrated to be robust to different ways of accounting for temporal shocks, survival bias, and alternative approaches to variable construction. This instrumental variable strategy exploits the limits to coverage levels that are codified in the Federal crop insurance program regulations, and are publicly available from the Risk Management Agency (RMA). These limits can vary by crop and by county.

Crucially for our analysis, incentives to expand coverage increased over our study period. However, the existence of coverage limits means that an individual farmer i's ability to increase coverage over time is constrained. This constraint is based on the initial coverage level ($P_{i,s}$) chosen by a farmer i in the first observed year s. Farmers who are initially closer to the maximum coverage rate (denoted $\text{Max}P_{i,s}$) cannot increase their coverage rates as much as farmers who are initially farther from this maximum. Therefore, the relationship between $\frac{P_{i,s}}{\text{Max}P_{i,s}}$ and the ratio of a coverage rate in a later year t to $P_{i,s}$ is negative and non-linear: it decreases at a decreasing rate as $P_{i,s}$ approaches $\text{Max}P_{i,s}$.

Weber et al. (2016) define this non-linearity using an exponential function:

$$\frac{P_{i,t}}{P_{i,s}} = \left(\frac{P_{i,s}}{\text{Max}P_{i,s}}\right)^{\theta} \tag{2.4}$$

After taking the log of both sides, this is equivalent to:

$$ln(P_{i,t}) - ln(P_{i,s}) = \theta ln\left(\frac{P_{i,s}}{\text{Max}P_{i,s}}\right). \tag{2.5}$$

Based on this relationship, we can use the log of the ratio of the initial premium per acre ($P_{i,s}$) and the maximum possible premium per acre (Max$P_{i,s}$) as an instrumental variable, denoted Z_{it} for the difference in premiums per acre between any later year t and the initial year s. We follow Weber et al. (2016) in calculating the maximum premium for a farm, using the Risk Management Agency's Summary of Business data to find, for each crop, the plan and coverage level with the highest premiums per acre, by year and county. Multiplying this premium per acre by the number of harvested acres for each crop on each farm gives the total, farm-level maximum premium. Using this ratio as an instrument to non-linearly predict changes in premium levels is plausibly exogenous to current farm conditions or decisions that would affect both the premiums paid per acre and the credit demands of a farm operation. The non-linearity is the result of program coverage limits that are determined nationally and are beyond the influence of any individual farmer.

Using this instrument and the 2SLS first differences model with the restricted ARMS panel, as recommended by Weber et al. (2016), we estimate:

$$y_{i,t} - y_{i,s} = \beta_0 + \theta_1 \underbrace{ln(P_{i,t}) - ln(P_{i,s})}_{=Z_{it}} + \boldsymbol{\beta G_{it}} + \boldsymbol{T_{i,s}}\delta_1 + \boldsymbol{T_{i,t}}\delta_2 + v_{c(i)} + \varepsilon_{it} \tag{2.6}$$

where $Z_{it} = ln(\frac{P_{i,s}}{MaxP_{i,s}})$. The additional terms $\boldsymbol{T_{i,s}}$ and $\boldsymbol{T_{i,t}}$ represent the year the farm was observed and the subsequent year, respectively, and $v_{c(i)}$ represents additional county fixed effects. County fixed effects are recommend by (Weber et al., 2016) to address any local changes in cropping intensity. We also estimate the model with state fixed effects, given the relatively low number of ARMS participants from any given county: this effectively removes most variation from our data. The remaining terms are consistent with equation (2.3). Standard econometric tests from the first stage confirm that this is a strong instrument, with a F-stat well above the accepted level of 10.[10] Any violations of the exclusion restrict would have to exist beyond the controls in our model, which include first differences at the farm level, temporal effects, and crop mix. Local basis shifts are a typical concern, but are somewhat removed from financial decisions. While we cannot disprove every hypothetical violation, as we have discussed, our model accounts for a wide range of common violations of the exclusion restriction.

Stratified sample analysis

As discussed in section 2.2, under hypothesis 2, lenders that rely on financial statements or relational borrowing to make decisions may favor crop insurance for weaker borrowers. If this is a typical practice, crop insurance may lead to larger increases in short term debt use for these groups. In other words, for borrowers who are credit constrained under financial statement or relational lending, β_1 would be larger. Given the reduced variation that the ARMS panel and IV identification strategy leverages, we avoid estimating equation (2.6) with stratified samples. Instead, we rely on evidence provided by the stratified samples using estimates of equation (2.3).

There are several ways to identify farms that may have weaker financial statements and thus be credit constrained. We use standard measures of repayment ability based on financial statements that reflect lower solvency or liquidity. The first stratification divides observations above and below the median level of debt-to-asset ratio. The second stratification divides observations above and below the median debt repayment capacity utilization (DRCU). DRCU measures a farm's ability to make principal and interest

[10]The first stage results are available from the corresponding author by request. To show the correlation between the instrument with the dependent variables, coefficients from an OLS model of the IV on the dependent variable are provided in the Supplemental Appendix, table B.18.

payments on outstanding debt using current income. Low DRCU suggests low levels of debt relative to income. Thus, the two measures capture issues related to farm solvency and farm liquidity, respectively. The distinction is particularly important in the current agricultural economy, as the current farm sector downturn has been characterized by low liquidity (or working capital) as opposed to low solvency (Zhang, 2017). Further, liquidity may be a missing variable from our model, as it increases demand for operating credit. However, liquidity and debt use measures are simultaneously determined, so we do not use it in our estimating equations. This analysis allows us to consider its relationship with crop insurance use and debt.

There are two main ways to stratify our sample to identify groups that may not have established relationships with lenders, taking advantage of detailed demographic characteristics in ARMS. We first split the sample based on operator age. We define the "young" sample as farms with a principal operator younger than 45, and the "old" sample as farms with a principal operator age 45 or older. Studies have shown that younger operators are more likely to be credit constrained, and hence crop insurance may play a role in their access to credit (Briggeman et al., 2009). Further, older operators may have stronger personal or professional connections with insurance agents or bankers, or may be more familiar with the crop insurance programs. On the other hand, younger operators may be more financially literate, having completed their education more recently. Similarly, we also conduct this analysis for farmers with ten or fewer years experience, regardless of age.

2.4 Results

This section discuss the key empirical findings of the relationship between crop insurance and farm debt use. We briefly discuss findings from the cross sectional and pooled models, then focus on estimation of our 'panel models' (equations 2.3 and 2.6), which are more effective at addressing the simultaneity of crop insurance and debt use decisions. The full, detailed estimation results for each specification, with a larger set of debt-related dependent variables, are reported in the Supplemental Appendix (tables B.4 – B.12).[11]

The results for the model as specified in equation (2.2) and estimated via ordinary least squares (OLS) for the restricted cross section and restricted panel are reported in tables B.1 and B.2, respectively. In the cross section and pooled models, all measures of short term debt are positively correlated with crop insurance participation and premiums. Leverage is positively correlated with participation in the cross section model, but does not have a statistically significant relationship with premium paid in either model. In the pooled specification using the restricted panel, we find no statistically significant relationship between crop insurance use and leverage. It is also worth nothing that results are generally similar in terms of sign and magnitude between the restricted cross section and panel. Farms in the panel do appear to have a larger magnitude of 'repaid' operating loans, which is consistent with the over-representation of larger farms in the restricted panel. While farms in the restricted panel are larger and more likely to use both crop insurance and debt financing, we do not observe evidence of another type of systematic selection bias in our basic regression analysis.

While ARMS allows us to control for a rich set of farm characteristics, these estimates

[11]We envision this as a Supplemental Appendix to this article. We have included it for peer review to assist the reviewer in assessing the robustness of our results and for transparency.

still may biased due to unobservable farm management strategies and skills, as well as simultaneity of debt and insurance decisions. As such, we focus on results from our panel models. The model as specified in equation (2.3) with farm-level fixed effects are reported in table 2.3. Table 2.4 reports the changes debt use in response to premium paid increases, using an instrumental variables specification, the most restrictive and informative model.

Table 2.3: Farm panel results: Pooled OLS with farm FE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Short term debt				Short term debt		
	Repaid	Outstanding	Total	Leverage	Repaid	Outstanding	Total	Leverage
Δ ln Premium paid	3,183***	1,171	4,354**	0.00197				
	(1,119)	(1,104)	(1,991)	(0.00137)				
FCI participation					52,193**	39,934*	92,126***	0.0248
					(21,831)	(22,626)	(35,385)	(0.0698)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Farm FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	27,921	27,921	27,921	27,921	22,371	22,371	22,371	22,369
R^2	0.227	0.049	0.099	0.215	0.037	0.088	0.037	0.078

[a] Standard errors robust to correlation at the farm level in parentheses
[b] Includes time variant controls for farm and operator characteristics
[c] *** p<0.01, ** p<0.05, * p<0.1

Table 2.4: Instrumental variable model

	(1) Δ Repaid	(2) Δ Outstanding	(3) Δ Total	(4) Δ Leverage	(5) Δ Repaid	(6) Δ Outstanding	(7) Δ Total	(8) Δ Leverage
Δ ln Premium paid	16,550	24,334***	40,884***	-0.0186	13,598	14,941*	28,539*	0.00987
	(10,377)	(7,310)	(14,156)	(0.0181)	(9,858)	(8,740)	(14,713)	(0.0572)
State FE	YES	YES	YES	YES				
County FE					YES	YES	YES	YES
Observations	16,504	16,504	16,504	16,504	16,504	16,504	16,504	16,504
R^2	0.227	0.049	0.099	0.215	0.037	0.088	0.037	0.078

[a] Standard errors robust to correlation at the farm level in parentheses
[b] Includes year fixed effects, state or county fixed effects as indicated, and time variant controls for farm and operator characteristics
[c] Dependent variables are calculated to be the difference between the current year and the next most recent year.
[d] *** p<0.01, ** p<0.05, * p<0.1

2.4.1 Panel models

The panel-farm fixed effect model (table 2.3), which explicitly controls for farm-level deviations in debt and crop insurance, suggests that crop insurance leads to an additional $92,000 in annual operating loan use [totalshort], about 19% of the restricted panel average. Likewise, a $1 increase in premium paid per acre is associated with nearly $4,400 in additional total short term debt. These estimates are comparable in magnitude, given that the mean premium paid for the restricted panel was nearly $10 per acre. The effect appears to be stronger in terms of repaid short term debt, as the impact of crop insurance premium level on outstanding short term debt is not statistically significant for the farm panel specification with farm-level fixed effects (table 2.3), and the impact of crop insurance participation from the same specification is weakly significant. For both premium paid and participation, there is not a statistically significant relationship with farm leverage.

The instrumental variable specification (table 2.4) considers the impact of the natural log of premium paid per acre on farm debt use. The results employ two different spatial fixed effects: county and state. Even in the most restrictive specification with county fixed effects, which effectively eliminates much of the variation in our data, the model suggests that increases in crop insurance coverage leads to higher short term debt use, albeit at a more marginal level of statistical significance. The impact appears to be concentrated in the increase in short term debt that is outstanding at the end of the calendar year, suggesting a $1,500-2,500 increase in response to a 10% increase in crop insurance premium per acre. Similar to our panel fixed effects model, crop insurance coverage does not have a statistically significant relationship with leverage.

2.4.2 Stratified sample analysis

If the collateral guarantee provided by crop insurance is more important for weaker borrowers, we would expect a stronger relationship between operating loan levels and crop insurance use for such borrowers. Using ARMS, we can identify borrowers who may be credit rationed by lenders that use financial statements or relational borrowing. Tables 2.5 reports the relationship between crop insurance use and total short term debt using our panel-farm fixed effects specification, across several different farm types.

Table 2.5: Stratified sample analysis: Pooled OLS with farm FE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Debt-to-asset ratio		Debt repayment capacity utilization		Operator age		Operator experience	
	below median	above median	below median	above median	45 years or younger	older than 45 years	ten years or less	more than ten years
	total short term debt							
FCI participation	66,106***	129,162	29,972**	121,501	-11,204	110,001***	72,406	99,369***
	(22,856)	(100,267)	(14,291)	(80,941)	(78,014)	(41,787)	(172,781)	(38,341)
Observations	11,010	11,361	11,088	11,283	4,031	18,340	1,574	20,797
R^2	0.02	0.06	0.03	0.01	0.04	0.03	0.01	0.03

[a] Standard errors robust to correlation at the farm level in parentheses
[b] Includes year fixed effects, farm fixed effects, and time variant controls for farm and operator characteristics
[c] Dependent variables are calculated to be the difference between the current year and the next most recent year.
[d] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

First, we consider farms' leverage and debt repayment capacity utilization, key measures of farm financial status. We find no compelling evidence that there is any relationship between crop insurance and credit use for the farms who have more financial risk in our sample. Instead, it is the less-indebted farms driving the relationship we observe. Debt capacity utilization measures a farm's financial stress and quantifies an operation's debt obligations relative to its current income. When the samples are split at the median of debt capacity utilization, it is the operations with lower debt capacity utilization, and thus higher debt repayment capacity, driving the effect on both absolute and relative debt use. The results from the sample split at the median of leverage also show that it is farms which are less leveraged that are increasing their short-term debt use in response to crop insurance. This result suggests that either (1) financial statement lending is not common, or (2) that the collateral guarantee provided by crop insurance is insufficient to overcome lender restrictions on borrowers with a weaker financial status. It further suggests that while liquidity measures may be missing from our main estimating equation, it is likely not driving our main result.

We also divide our data by two operator characteristics that may affect the relationship of a producer with their lender: age and years of experience as a farm operator. For older operators, crop insurance participation is related to higher operating credit use and intensity, but there is not a statistically significant relationship for younger operators. This provides some suggestive evidence that, consistent with other studies, younger operators may be credit rationed, and that crop insurance does not relax these constraints. The results for operator experience mirror those of age. Similar to our analysis for factors important to financial statement lending, this results suggests either limited relational lending or that the collateral guarantee of crop insurance is not a substitute for factors considered under this type of lending.

The results of stratified sample analysis are consistent with the presence of credit rationing or other restrictions put into place by lenders who making decisions based on financial statements or relationships. Despite anecdotes of lenders requiring crop insurance fir weaker borrowers, this is not borne out in our analysis. Rather, our analysis suggests that credit rationing may exist, but the collateral guarantee provided by crop insurance is insufficient to overcome it. This analysis raises the concern that credit rationing may limit the degree to which risk balancing is observed. While this analysis does not provide absolute proof that credit rationing limits risk balancing behavior, it suggests that this is one possible explanation for this and other research (i.e., de Mey et al. (2014)) that shows that risk balancing is not uniform across various farm types. In our robustness check section, we explore the direct relationship between crop insurance coverage and credit constraints using unique data from 2014.

2.4.3 Robustness checks

Cross sectional analysis of coverage levels and credit constraints (2014)

In addition to providing a robustness check for the equivalence of results with an actual measure of coverage levels relative to premium paid, analysis using 2014 data gives us additional insight into the relationship between crop insurance and credit constraints. The results, shown in table 2.6, are consistent with the main results.

Table 2.6: Coverage rate results: 2014 cross section

	(1) Total short	(2) Debt outstanding	(3) Debt repaid	(4) Leverage	(5) Denied credit	(6) Deterred from credit	(7) Credit problems
Coverage level	4,255***	2,037**	2,218**	0.002	0.002	3.54e-06	0.001
	(1,439)	(883.1)	(1,003)	(0.001)	(0.001)	(0.000)	(0.001)
Attitude toward risk	20,343***	7,631***	12,712***	-0.000	0.022***	-0.005**	0.018***
	(3,979)	(2,803)	(2,432)	(0.004)	(0.003)	(0.002)	(0.003)
Share of acres covered: revenue	2,056	-3,927	5,984	0.043*	0.015	-0.003	0.012
	(23,835)	(16,983)	(16,153)	(0.024)	(0.017)	(0.011)	(0.018)
Observations	5,063	5,063	5,063	5,061	5,063	4,608	5,063
R^2	0.227	0.099	0.215	0.037	0.088	0.037	0.078

[a] Robust standard errors in parentheses
[b] Includes insured commodity fixed effects, state fixed effects, and controls for farm and operator characteristics
[c] Coverage level is calculated as an acres-weighted average across all of an operation's covered crops.
[d] *** p<0.01, ** p<0.05, * p<0.1

Conditional on participating in crop insurance, the amount of short term debt used by farms increases with coverage level. Total short term debt is over $4,000 higher for each additional percentage point increase in crop insurance coverage rate. This relationship holds across both repaid and outstanding short term debt. Coverage levels however do not have a statistically significant relationship with leverage, also consistent with our main results.

Coverage levels have no statistically significant impact on any of our self-reported measures of credit constraints. This finding suggests that the primary driver of the relationship between crop insurance and short term credit use is likely not the alleviation of credit constraints facing weak borrowers. This is consistent with our stratified sample analysis and confirms that, on average, lenders do not allow crop insurance to compensate for other barriers to credit access.

Given that this analysis is only possible with 2014 data, we cannot implement our preferred panel methods and do not claim a a causal interpretation. However, we are able to control for a broad set of factors likely to impact financial decisions and crop insurance participation, including the operator's risk preference, an often omitted or unobserved driver of both decisions. Coefficients on this self-reported measure conform to *a priori* expectations and are statistically significant. Specifically, farmers who are more risk-seeking take out more loans, finance a greater share of variable expenses with loans, are more likely to have their credit applications denied, and are less likely to be deterred from credit.

Additional measures of debt use

Our primary analysis focuses on short term debt and leverage, following our conceptual framework, hypotheses, and previous findings. However, there are additional forms of credit that may be influenced by crop insurance. In our supplemental appendix, we report full results of our preferred panel models (tables B.8-B.11), with additional measures of debt use. While farm debt structure likely involves simultaneous decisions, our empirical strategy necessitates estimation of individual equations. Instead, we consider both disaggregate and aggregate measures of short and long term debt use.

First, we examine the relationship between crop insurance and the intensity of short term debt use as captured by the share of variable expenses supported by short term debt [financed]. A positive relationship would suggest a short-term risk balancing response as financing a higher share of variable expenses implies higher financial risk, as opposed to an absolute increase because an absolute increase in short term debt may also reflect lenders increasing operating credit lines. Crop insurance does not have a statistically significant relationship with this variable in any of the models, with the exception of the panel fixed effects model with crop insurance participation (table B.9), where it is marginally significant. Second, we examine the relationship between crop insurance and variable expenses [evtot][12]. We find strong evidence of a positive relationship between crop insurance and variable expenses across all four models, which may be due to additional credit or other effects of crop insurance.

Finally, we examine the relationship between crop insurance and long-term debt, including both real estate and non-real estate debt ([dreale] and [dnreale], respectively).

[12]This variable is calculated using both variable expenses and rental expenses. Crop insurance premiums are not included.

A positive relationship between crop insurance and long-term debt would be consistent with risk balancing, yet it would suggest that annual guarantees of crop insurance would extend to noncurrent portion of the balance sheet. In all four models, with one exception (non-real estate long term debt in table B.11), no measure of long-term debt has a statistically significant relationship with crop insurance use. This finding suggests a limit to risk balancing behavior, as a result of limited increases in the demand for long-term debt or limitations imposed by financial intermediation.

2.4.4 Discussion

We find evidence that crop insurance participation leads to higher short term debt use, consistent with lender's response to the collateral guarantee provided crop insurance. In our farm fixed effects model which addresses farm-specific factors but not simultaneity of debt use and crop insurance, we see a strong effect on on operating loan volume (repaid and total), but not end-of-year outstanding short term debt. This result is consistent whether using the discrete change from no crop insurance to insurance or an increase in premium paid. However, when we use the more robust specification reported in table 2.4, we see a more pronounced effect on outstanding short term debt.

We interpret this change through the underlying assumptions of each model. Farms with higher expenses over time may demand more debt financing, which could lead to higher crop insurance use. Our farm fixed effects model cannot rule out this explanation. However, if our underlying assumptions for our IV model are true, we can rule out this explanation. Higher outstanding and total short term debt implies that lenders have indeed increased credit lines, consistent with the higher value of crop-collateral under crop insurance.

Our results support our first hypothesis: lenders increase operating credit limits in response to coverage insurance use and coverage. From a demand perspective, we cannot rule out credit rationing or risk balancing behavior as the driver of this result. However, based on our stratified sample analysis, we find evidence that crop insurance does not help weaker borrowers access credit. This finding contradicts our second hypothesis but is consistent with relaxation of broad credit rationing under asset based lending, due to insurance addressing systemic risk of crop yields. Likewise, the null result for a relationship between leverage and crop insurance suggests that while risk balancing may lead to an increase in demand for operating credit, it does not lead to higher leverage as predicted by our third hypothesis.

These findings are generally consistent with previous research (Ifft et al., 2015; de Mey et al., 2014) and indicate that, although there is a correlation between crop insurance use and some measures of long term debt use, there is no causal relationship. In sum, the results provide no evidence for risk balancing for long term debt use or leverage. Economic theory suggests that the finding may be driven by the relative illiquidity of farmland and equipment, the main form of collateral for these loans. Further, crop insurance does not cover the risks associated with long term price declines that affect farm operators and lenders. Theories of farm risk balancing predict that policies that decrease production risk could encourage farmers to increase leverage to the point of equity loss (Featherstone et al., 1988). Given our finding of no relationship between crop insurance and leverage, crop insurance is not associated with potential equity loss in the farm sector. This finding is also consistent with the stated purpose of crop insurance programs – to mitigate annual production and market risk.

The results suggest that farmers risk balancing is, at most, temporary. Crop insurance increases short term debt use, yet the additional debt does not affect farm solvency. One explanation is that debt is repaid quickly enough to not influence leverage. Alternatively, potential leverage changes may be muted by asset values. Changes in debt levels are small relative to asset values, as the average U.S. farm business in 2014 had asset values nearing $2 million, according to the USDA Economic Research Service. In addition, previous research suggests that the benefits of crop insurance may be capitalized in farm asset values (Ifft et al., 2014).

2.5 Conclusion

The Federal crop insurance program is designed to support the economic stability of the farm sector. Through its influence on lender and producer decisions, crop insurance may improve credit access and reduce credit rationing. However, increasing credit use may have a destabilizing effect if farms take on riskier financial positions. Drawing from a comprehensive, national survey of farm-level financial decisions (ARMS), we provide strong evidence of a causal link between crop insurance and short term farm debt use. Our results are most consistent with lender's responding to crop insurance's usefulness as collateral for operating loans. This result also suggests that in addition to addressing systemic yield risk in agriculture, the Federal crop insurance program also addressed systemic risk facing farm lenders. While crop insurance may have addressed credit rationing related to this systemic risk, it does not appear to increase credit use by weaker borrowers. In addition, the resulting increases in short term debt do not appear to yield increases in farm leverage.

While our results points towards an important role of crop insurance in loan collateral, our study cannot fully account for credit and asset market equilibria. While crop insurance leads to higher short term debt use, we do not find evidence that crop insurance generates additional financial risk, at least from a farm solvency or other long-term perspectives. The relationship between crop insurance and asset values may also influence this finding. This is an important topic for future research, but is beyond the scope of the current study. Weaker borrowers in our sample, as measured by indicators of financial statement or relational lending, generally do not have higher levels of operating credit use in response to crop insurance participation or coverage levels. This suggests that although lenders likely raised operating credit limits for most operators through crop insurance use, lending standards remain tighter for riskier borrowers.

Risk balancing is a useful framework for understanding the relationship between farm policy and financial risk, but financial intermediation is likely limiting the observation of risk balancing behavior in this study. While our empirical result of higher operating credit use is consistent with risk balancing, the underlying assumptions of asset liquidity and credit rationing are commonly violated in farm lending and may underlie our null result for farm leverage. Experimental studies on risk balancing may be more conclusive than analysis of farm survey or lender data.

While our study provides plausibly causal estimates of the relationship between crop insurance and credit, thus substantially improving on previous studies, we do make standard identification assumptions. By using the instrumental variable developed by Weber et al. (2016), we are able to examine the relationship between crop insurance and credit use under a very restrictive specification. This instrumental variable relies on how close a producer initially was to a program-specified maximum coverage level being uncorrelated with current financial and insurance decisions, after controlling for farm-level factors. This approach allows us to conduct an exhaustive analysis using ARMS data, and could be validated with analysis using farm management data or future crop insurance or farm policy changes.

This study finds that crop insurance plays a key part in the provision of credit to meet production expenses of the farm operation. This can help farms stay in operation during periods of sustained low returns or periods of increasing expenses, which typifies our study period. Generally, higher operating loan use may have a number of long-term benefits for U.S. farms operations. These include farm household income support, productivity enhancing investments, and farm expansion. While this is beyond the scope of this study, it is an interesting direction for further study. Given the important role of lenders in the causal relationship between crop insurance and short term credit use implied by our study, future research using lender or loan-level data would advance knowledge of the importance of crop insurance to farm lending and lead to more precise quantification of impacts. Understanding the factors affecting lending institutions, especially liquidity of collateral and credit rationing, may be an important part of future empirical research on farm policy and financial risk. Our empirical strategy necessitates a focus on relatively larger farms for which crop insurance was readily available during our study period. Relatively smaller or midsize farms may favor a wider variety of risk management alternatives, including off-farm income, which is an important topic for future study.[13] Understanding lender response to crop insurance may help as the program strives to increase use by diversified farms which have traditionally not participated (Olen and Wu, 2017).

Crop insurance is the foundation of the current farm safety net. The program is designed to provide economic stability for agriculture by addressing the variability of the returns to farming. Our study suggests that the impact of crop insurance on credit use is largely in line with policy objectives. Specifically, crop insurance appears to be serving

[13] We acknowledge an anonymous reviewer for this insightful point.

as repayment insurance for operating loans, while not leading to higher farm leverage. As such, there appears to be minimal evidence that crop insurance leads to destabilizing financial risk in agriculture.

CHAPTER 3

IS ICE FREEZING US AGRICULTURE? FARM-LEVEL ADJUSTMENT TO INCREASED LOCAL IMMIGRATION ENFORCEMENT

with Jennifer Ifft

3.1 Introduction

The wide-ranging debate on U.S. immigration policy has economic implications for many domestic industries that rely on immigrant labor, both authorized and unauthorized, for their labor force. Depending on the feasibility of mechanization and the substitutability of immigrant labor, a variety of labor market adjustments and firm-level adaptations are feasible. However, the impact of current immigration policies and labor force uncertainty on U.S. businesses is difficult to measure due to the need for firm-level data, as well as the challenge of disentangling the effects of immigration policy from broader economic trends. While there is a strong literature that studies the impacts of immigration policies on both immigrant and domestic workers (e.g., Chiswick, 1978; Borjas, 1987; Ottaviano and Peri, 2012)), less is known about the impacts of locally enforced immigration policies on firms. More broadly, the impact of labor scarcity on employment is ambiguous and the associated endogenous technical change is not fully understood (Clemens et al., 2018).

This paper explores how U.S. farms were affected by a change in local immigration enforcement intensity due to authorization of the 287(g) program, using an unbalanced farm-level panel data set and county-level Census of Agriculture aggregates. Similar to Clemens et al. (2018), we test for evidence of endogenous technological change in the agricultural sector after an immigration policy-induced labor supply shock. In brief, the

287(g) program authorized law-enforcement entities that applied and were approved to function as U.S. Immigration and Custom Enforcement (ICE) agents in order to identify and detain unauthorized immigrants within their jurisdiction as part of their routine duties. These entities include Sheriff's Offices and Police Departments in 13 cities and 49 counties across 20 states. Our study provides a firm- and county-level analysis of adaptation to these local changes in immigration enforcement. We contribute to an ongoing national policy debate that is supported by thin empirical evidence on how firms' non-wage decisions are affected by a fluctuating labor supply. Further, our approach allows us to provide evidence on the substitutabilty of native workers and/or capital for immigrant labor in a sector heavily dependent on an immigrant labor force. Foreign-born workers tend to specialize in manual labor, which can limit their substitutability (Peri and Sparber, 2009). The extent to which this substitutability exists, if it does at all, is a crucial point of difference in many arguments about the direction U.S. immigration policy should take, and plays a key role in theoretically determining firm-level impacts of immigration policy (Lewis, 2003).

Causal identification of labor supply shocks remains the foremost challenge to understanding the impact of immigration. Borjas (2003) posits that nearly all studies rely on the spatial relationship between native wages and immigrant numbers, thus ignoring the endogeneity between local conditions and the supply of immigrants. Generally, 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 (Card, 2009). The former occurs infrequently and typically only in one place, affecting external validity, while the latter is only really effective for urban areas, where such enclaves are able to form.

Studies of the impact of immigration policy or immigration enforcement on workers have mixed results. Clemens et al. (2018) finds evidence that at the state level, the 1960 bracero exclusion did not increase wages or employment for domestic farm workers. Instead, firms changed production techniques or reduced production intensity. Other papers have looked at more recent immigration policies or programs, including 287(g). Local immigration enforcement through 287(g) had varied employment impacts between industries (Bohn and Santillano, 2017) and led to a decline in the population of non-citizens in program jurisdictions (Kostandini et al., 2014). 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 how U.S. farm businesses adapted to increased local immigration enforcement. One advantage of analysis of the farm sector is that its trends are only weakly linked to those in the general economy. For most other industries, labor supply and firm revenue are strongly linked to broader economic conditions. Further, there is strong evidence the local farm economy is not integrated with the local nonfarm economy, especially in ways relevant to our study: it is highly unlikely that crop prices or farm profitability are influencing crime levels or law enforcement or vice versa. We use two different sets of agricultural survey data, one firm- and one county-level. We consider evidence for endogenous technical change, defined by the impact of 287(g) authorization on farm- and county-level fuel expenditure, labor expenditure, number of workers (county-level only) and acres harvested–the available variables that provide the best evidence on how employers adjusted labor use, capital mix, and production intensity. Due to data limitations, we are not able to consider wage impacts or directly estimate labor supply or other elasticities.

We implement a novel and robust instrumental variables strategy for local immigration enforcement decisions: we use measures of local jail occupancy before 287(g) authorization, which are arguably unrelated to agricultural production decisions, as instrumental variables for the decision to participate in 287(g). Aggregated up to the county level, both 1999 and 2006 local jail occupancy and pre-2006 local jail renovations are strong predictors of 287(g) participation, with little evidence of any relationship to farm sector activity. There was a widespread belief that 287(g) could increase revenue from jail detentions(Shahani and Greene, 2009), which underpins our first stage and is not influenced by anecdotes that this revenue was not realized. Further, we find no evidence of farm or non-farm sector pretrends that could be influencing our results. This approach allows us to provide one the first plausibly causal estimates of the economic impact of local immigration policy on U.S. firms. Similar to Clemens et al. (2018), we find evidence that, to some degree, firms used more intensive production methods in response to 287(g). We do not find evidence of changes to labor expenditure, but a decline in workers, which suggests some combination of an increase in wages and use of more skilled labor. Overall, we find evidence of partial or incomplete substitution of capital, as well as decline in agricultural land use, after a labor supply shock caused by increasing local immigration enforcement. This suggests that areas with more stringent immigration enforcement may disadvantage sectors that use manual or immigrant labor and, perhaps unsurprisingly, that endogenous technical change may not occur in response to local labor supply shocks.

3.1.1 Labor supply shocks and endogenous technical change

Under theoretical models of endogenous technical change, a decrease in the supply of one type of labor causes substitution away from that type. 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. The underlying assumption of adaptive technological change may be violated in industries, like agriculture, that rely on manual labor. In examining the sector-level response to labor supply shocks, Lewis (2003) finds that such shocks 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 the relative factor intensity of that type, with no effect on wages. The labor supplied by and demanded of immigrant workers in agriculture may also differ from that of native workers, and anecdotal evidence indicates it is difficult to procure native workers to do many manual agricultural tasks, even for relatively high wages (Peri and Sparber, 2009; Clemens, 2013). In addition to potentially not having 'competing' workers, other factors of production, like machinery, may only partially substitute for labor.

Clemens et al. (2018) develop a theoretical framework to analyze the impact of a labor supply shock on the agricultural sector. In it, they describes three potential outcomes for agriculture after a labor supply shock. If capital, technology, or output cannot adjust, farm wages will increase. If adjustment is possible within a 'cone of diversification', factor intensity but not wages will increase in response to changes in factor supply. The 'cone' is essentially an area where both automated and traditional technologies are feasible and substitution between them is possible. In response to the 'bracero exclusion' that Clemens et al. (2018) analyze, or more broadly to a decline in immigrant labor supply, their model of endogenous technical change predicts no changes to wages, an increase in output that uses automatic harvest technology, and a decline in output that uses traditional harvest technology. In the absence of fully substitutable automatic technologies, or when substitution is incomplete, wages will rise and output could fall to the point where alternative land uses become more profitable.

3.1.2 Immigrant labor and the farm economy

The farm sector provides a unique setting for causal identification of the firm-level impacts of a specific immigration policy. Changes in 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. Weber et al. (2014) find a similar result using recent data on crop price shifts, confirming only weak links between farm revenue and non-farm income or employment. This may reflect, to some degree, a small and stable farm population: the number of U.S. farms has been stable at slightly over 2 million since 1970[1].

Immigration policy and enforcement have 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). Richards (2018) estimated that in Cali-

[1] See https://www.ers.usda.gov/data-products/ag-and-food-statistics-charting-the-essentials/farming-and-farm-income/

fornia, a 50 percent decline in undocumented workers would lead to 22 percent increase in farm wages. 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 farm worker wage growth of over 40 percent where agricultural employment has 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; Fan et al., 2015). Kostandini et al. (2014) find that authorization of local (county) immigration enforcement through 287(g) leads to a decline in non-citizen population levels, based on population estimates from the American Community Survey. They also consider the association between sector-level outcomes and 2007 287(g) authorization. However, they do not make causal claims about how immigration enforcement impacts the agricultural sector at the firm level.

3.1.3 The 287(g) program

Over the last decade, anti-immigrant sentiment has become increasingly codified at the local level, motivated by discontent with state- and national-level approaches. For example, some localities have passed ordinances requiring proof of legal residency to obtain housing or have designated English as the "official language" of their municipality (Guzman, 2010). Other jurisdictions 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 law enforcement officials at the state, county, or city level to be deputized as national immigration agents and enforce national immigration policy within their jurisdiction. Jurisdictions elect into and are approved for one of two 287(g) "models", the jail model or the task force model, or can choose to have both. The jail model limits enforcement to is limited to checking immigration status as part of the intake process for someone already being jailed. With the task force model, enforcement reaches farther: for example, local officials can 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 and a half that law enforcement agencies have sought enrollment in the program. Because counties select into program participation, and then are either approved or declined by ICE administration, there is a group of counties that wished to participate but could not. In section 4.2.2 we extend our analysis to include these counties, picking up the impact of 287(g) as a proxy for local anti-immigrant sentiment, which presumably exists in all the counties that applied for 287(g) authorization, regardless of official approval.

While the 287(g) program's stated intent was to identify and remove only dangerous undocumented individuals, analysts debate the extent to which implementation aligned with this intention. Those opposed to its widespread implementation have argued that it provides the means for local law enforcement to racially target and harass residents of their jurisdictions (Shahani and Greene, 2009). Although the direct removal of immigrant labor that this program authorized was non-trivial in many places, the policy's true impact may have been as a signal to immigrants considering where to locate. This signalling 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 non-citizens" relative to other states (Bohn et al., 2014). Through both direct action and indirect signalling, implementation of 287(g) is related to a decrease in the local immigrant population and immigrant labor, regardless their of legal status (Watson, 2013; Kostandini 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, while Dee and Murphy (2018) use difference-in-difference and triple differences to find that 287(g) reduces the population of Hispanic students in a jurisdiction by 10% over two years. Many contributions to this area have focused on aggregated measures of employment or industry-level outcomes following 287(g) program implementation, such as (Kostandini et al., 2014), who estimated the immediate (2007 only) association between 287(g) participation and 2007 non-citizen population and some county-level indicators of farm labor supply shocks. Here, we look at the farm sector's response to this negative labor supply shock. Our outcomes include measures of the shock's magnitude, shorter term production decisions made as a response to this shock, and longer time estimates of the impact on farm profitability and financial stability.

3.2 Data

3.2.1 Farm survey data

Data on production decisions and farm structure 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 ARMS. Data collected from ARMS inform official agricultural statistics and have supported a wide body of research (Kuethe and Morehart, 2012).

ARMS uses a stratified random sampling procedure. Because of 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 unbalanced panel to analyze the impact of 287(g) authorization on various farm production decisions. While farms are randomly selected, larger farms are over-sampled 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, our study is concerned with farms that are labor intensive or use hired labor. Farms in the ARMS panel tend to be relatively large, as detailed by Weber et al. (2016), and so this sampling design provides us farms that are more likely to impacted by immigration policies. Of the approximately 23,000 farms in our sample, nearly 500 are located in 287(g) counties.

3.2.2 County-level farm survey data

While survey weights are provided for ARMS to calculate population-level statistics, they are only applicable to single-year analyses due to survey design. 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 external validity of our sample. Additionally, ARMS data are subject to survivor bias, in that our sample only considers farms in operation before and after 287(g) authorization. We address these issues and complement our ARMS analysis with analysis of publicly available county-level Census of Agriculture data, which are collected from all U.S. farms every five years. Response to the Census of Agriculture is mandatory and so represents a much broader population than ARMS; these data allow us to understand how labor supply shocks affected the entire farm economy. Census of Agriculture data provide an opportunity to understand the local farm sector and individual farm impacts.

3.2.3 Immigration enforcement data

In 2007, twenty-four (24) jurisdictions signed memoranda of understanding (MOU) with ICE and enrolled in the 287(g) program. By 2010, the number had reached its peak with fifty-four (54) jurisdictions enrolled; these are pictured in figure 3.1. As the map shows, participants are concentrated in the south, although a wide range of agricultural production systems are represented. For the purposes of our analysis, the level of treatment is at the county level, meaning that a county is considered in the program if its own law enforcement body, generally Sheriff's Offices, has 287(g) authorization, or if city in the county has signed an approved 287(g) agreement between its police department and

ICE.[2]

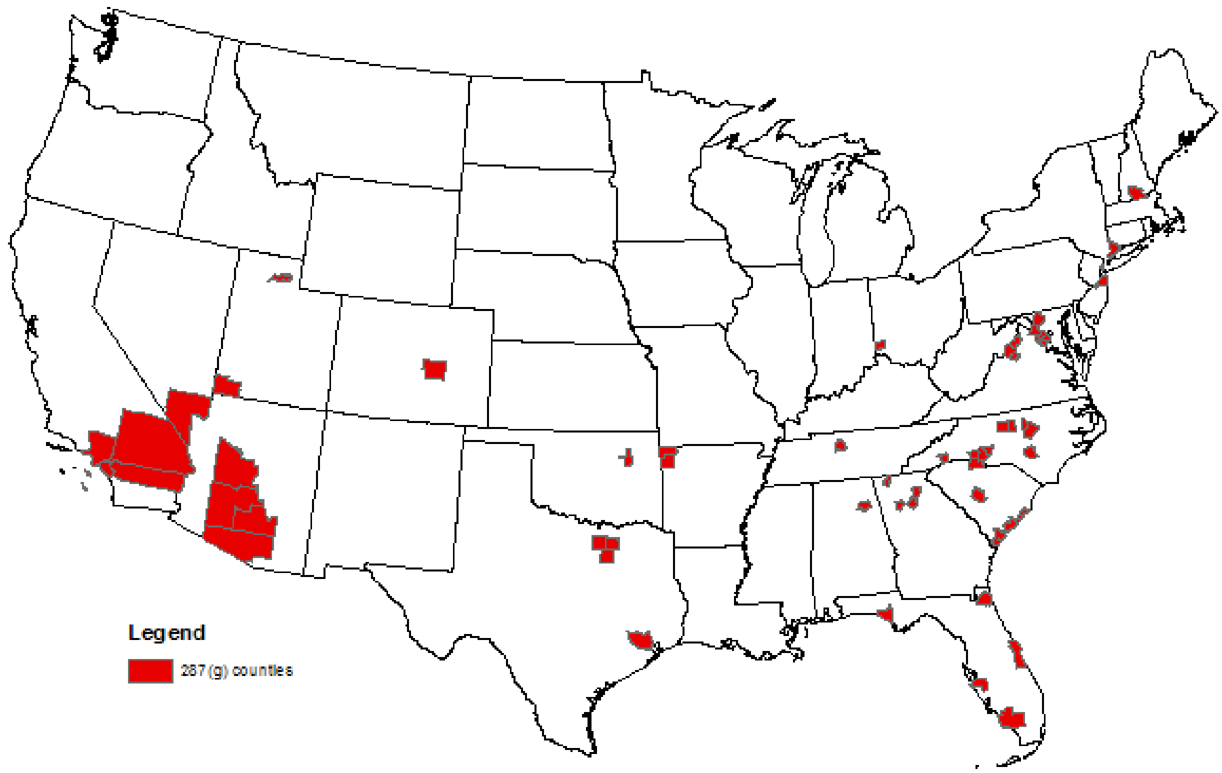

Figure 3.1: Counties with a 287(g) program

A county signing an MOU with ICE 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 deputized officials, and 'aliens departed' measures the number of those that were successfully removed from the juris-

[2]In addition to the city and county agreements described here, 13 states also had 287(g) agreements, typically between ICE and the state's Department of Corrections. Because these agreements were only likely to impact already incarcerated immigrants, their effect on farm workers is likely to be low.

diction.[3] 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. Appendix table C.1 has summary statistics for enforcement data, showing the high degree of variability of 287(g) enforcement measures across counties.

There are 134 jurisdiction-year observations where no aliens were identified, and 149 during which none were departed. A 287(g) program 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. The impact of this out-migration 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, or discourage undocumented immigrants from entering the U.S., but rather cause within-country or within-state migration between local areas that have the program and those that do not.

3.3 Empirical model

Our empirical model tests for evidence of endogenous technical change in response to a local labor supply shock. We use the theoretical model of Clemens et al. (2018) to inform our choice of outcome variables from available data reported in ARMS and the Census. Under this framework, we consider the impact of 287(g) authorization on labor expenditure, fuel expenditure, number of workers, and acres operated. When possible, we complement the farm-level outcome from ARMS with its corresponding county-level measure

[3] We follow the official language used by ICE here, hence the use of "departed" to describe this action rather than the more commonly seen "deported".

from the Census and vice versa. Table 3.1 reports average values of these outcomes prior to program onset.

Table 3.1: Summary statistics: Farm production variables, pre-287(g)

	All counties			**287(g) counties**			**Non-287(g) counties**		
	mean	SD	n	mean	SD	n	mean	SD	n
Acres operated/farm[a]	1670.619	8327.676	11,730	1146.934	7850.943	439	1,691.0	8,345.3	11,291
Acres operated / county[b]	106,550.3	176,354.9	1,288	98,424.8	163,268.0	52	106,892.2	176,937.5	1,236
Fuel expenses ($)[a]	$ 33,879.37	$ 82,954.97	11,730	36589.83	$ 106,976.00	439	$ 33,773.99	$ 81,883.10	11,291
Fuel expenses ($)[b]	$ 2,077,636.36	$ 3,937,702.92	1,320	$ 2,899,264.15	$ 3,758,092.82	53	$ 2,043,266.77	$ 3,942,725.33	1,267
Labor expenses ($)[a]	$ 169,791.30	$ 885,674.10	11,730	234,717.9	$ 749,999.60	439	$ 167,266.90	$ 890,464.70	11,291
Labor expenses ($)[b]	$ 39,466,877.65	$ 92,217,534.93	1,242	$71,449,801.40*	$ 130,710,505.80	50	$ 38,125,312.05	$ 90,074,547.96	1,192
# of workers / county[b]	1,089.8	3,310.8	1,324	2,256.4**	3,982.4	53	1,041.2	3,272.6	1,271

[a] Outcome from ARMS
[b] Outcome from Census
[c] ***,**,* Significantly different from non-287(g) counties at 1%, 5%, and 10% respectively
[d] Source: US Census of Agriculture (NASS QuickStats); USDA Agricultural and Resource Management Survey (ARMS)

By analyzing the impact on number of workers, we are able to test the finding of Kostandini et al. (2014) and others of a 287(g)-induced labor supply shock. No change to wages is predicted with endogenous technical change within in the 'cone of diversification'. Although hourly wage data is not available, labor expenditure is reported in ARMS (per operation) and the Census (per county), and the number of workers per county is reported in the Census. Although under endogenous technical change no increase in wages is possible, labor expenditure could decrease, if, for example, the number of hours worked declines with higher use of automatic technology. On the other hand, if there are more dynamic changes due to operators' inability to substitute for workers or technology, the impact on labor expenditure is indeterminate. More skilled workers may require higher wages even if hours decrease.

To test whether the use of 'automatic technologies' increases, we consider fuel expenditure, which is available in both surveys. Given that changes to fuel prices are largely driven by global oil prices, an increase in fuel expenditure would suggest an increase in use of automatic technologies that require more fuel. While we cannot precisely measure acres operated with automatic technologies, we can measure total acres operated by operation (ARMS) and by county (Census). A decline in acres would suggest that full substitution from one production type to another was not possible, and that alternative land uses became more profitable.

3.3.1 OLS model

These outcome variables are modeled as a function of 287(g) program authorization (effectively, codified immigration discouragement). We also control for whether or not a non-participating county bordered a 287(g) county, because there could be spillovers to neighboring counties, as suggested by Watson (2013). It is uncertain *a priori* whether 287(g) would increase or decrease immigrant labor supply and cost in nearby counties: that is, whether a 287(g) program deterred immigrants from an entire area or whether they simply relocated from a program county to neighboring counties. A similar result for 287(g) and border counties could suggest general economic or property value trends as a potential confounder, or at least motivate further investigation of these trends.

We use a farm fixed effects model with the ARMS data, 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 other than that imposed by stratification (previously discussed), as farms are observed based on random selection into ARMS. However, farms had to exist after the policy went into effect, so our results are interpreted as representative of surviving farms in operation before and after the policy. Thus, our basic estimating equation is as follows:

$$Y_{ict} = \alpha_0 + \alpha_1 G_{ct} + \alpha_2 B_{ct} + \tau_t + \gamma_i + \varepsilon_{ict} \tag{3.1}$$

where Y_{ict} is the outcome of interest for farm i in county c at time t; G_{ct} is either an indicator for 287(g) participation or a measure of 287(g) enforcement; B_{ct} is an indicator for whether a non-287(g) county c borders a 287(g) participating-county in year t, τ_t are year fixed-effects, γ_i are farm fixed-effects, and ε_{ict} represents variation in the dependent variable that cannot be explained by the model. For our analysis of ARMS data, we cluster standard errors to be robust to correlation at the strata[4] level to take into account survey

[4]Strata are based on value of farm sales, but the strata variable is not available before 2005 in our dataset. We group estimated value of farm sales into 30 clusters, based on pre-287(g) gross cash farm income and value of contract production.

design, based on the recommendation of Weber et al. (2016). We also estimate standard errors that account for various levels of spatial correlation in section 4.3.

Respectively, our estimating equation for Census of Agriculture data is as follows, with the unit of observation being a county c and year t, with $t \in [1997, 2002, 2007, 2012]$:

$$Y_{ct} = \alpha_0 + \alpha_1 G_{ct} + \alpha_2 B_{ct} + \tau_t + \gamma_c + \varepsilon_{ct} \tag{3.2}$$

Results from the estimation of these naive models are reported in table 3.2.

3.3.2 Instrumental variable strategy

The farm or county fixed effects regressions described by equations 3.1 and 3.2 will not provide causal estimates of the effect of 287(g) participation if participation is correlated with other economic factors that also influence farm sector outcomes. Farms located in 287(g) counties are different in many dimensions than farms in other counties (see table C.2), and these counties do have a higher number of pre-program farm workers (table 3.1). Broad farm sector trends appear be similar between 287(g), border and other counties, and key population trends are also similar. However, participation in 287(g) itself is not random, as law enforcement agencies select into the program. Participation and the presence of immigrants in a jurisdiction could therefore 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 (county) fixed effects controls for farm (county) characteristics and static local political and geographic conditions, we cannot prove that broader social or economic factors are not driving both farm decisions and 287(g) participation or enforcement levels. We thus address the potential endogeneity of 287(g) participation by using measures of local law enforcement intensity before 287(g) program authorization as instrumental variables (IV).

As part of preliminary research on the 287(g) program, we spoke to representatives from about one-third of enforcement units that had ever been enrolled in 287(g) about their interest in the program. The most common reasons shared for motivation for 287(g) participation were "politics" and the belief that there was the potential to earn revenue through the program by housing suspected unauthorized immigrants. While we found insufficient variation in voting patterns for a standard 'election outcomes' instrumental variables approach, various measures of local capacity for housing detainees turned out to be highly correlated with 287(g) authorization. We discuss potential confounding effects for this relationship in section 3.2.3, and see only limited viability for any alternative explanations.

County jail activity and the 287(g) program

In addition to offering local law enforcement a "political trophy in local law enforcement campaigns", law enforcement agencies may have seen in 287(g) the perceived potential for financial gains (Shahani and Greene, 2009). Despite statutes disallowing such reimbursements, there have been concerns that 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.

Therefore, we use changes in a county's aggregated jail occupancy[5] over time, which measures the number of "empty beds" in county jails to instrument for program participation.

Data on jail occupancy come from the Census of Jails, which is conducted by the Bureau of Justice Statistics every five to six years and surveys every locally operated jail facility in the United States; measures are taken to ensure that complete coverage is achieved for a small subset of variables.[6] These include information on the rated capacity of a jail, or the total number of inmates the facility can legally hold, and the total population of inmates. To construct our instrument, both measures, which are recorded at the facility level, are aggregated up to the county level; values from the 2005/2006 Census are used to measure pre-program capacity for 2007 and later, and values from the 1999 Census are used before 2007.[7] 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.

Appendix figure C.1 shows the distribution of jail occupancy across the United States. Although there are certainly areas where jail over-crowding occurs, there is no evidence that there is meaningful spatial autocorrelation. The Moran's I, a standard measure of spatial correlation, is not significantly different than zero using the minimum threshold distance spatial weights matrix for US counties (see appendix figure C.2). 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 populations move

[5]Vacancy may be a more accurate term in this case, but we use 'occupancy', as it is less emotive and a more general term that can encompass either overcrowding or unused space.

[6]States with combined jail-prison systems (Connecticut, Delaware, Hawaii, Rhode Island, Vermont, and Alaska) are not included in the Census. Only one 287(g) jurisdiction (City of Danbury, CT) is in one of these states, and none are states with a particularly large agricultural sector.

[7]The Census of Jails was scheduled to occur in 2005; however, it was split into the the 2005 Census of Jail Inmates and the 2006 Census of Jail Facilities in order to ease administrative burden. Each iteration reported total population and rated capacity with almost universal coverage.

together.

To demonstrate the mechanism of increased law enforcement activity and the robustness of our approach, we also estimate our results using: (1) annual pre-program jail renovations and (2) renovations and occupancy together. These alternatives address the potential concern that jail occupancy as an IV may reflect population loss. Analysis of jail construction and renovation data indicates that 287(g) counties were making larger investments in local jail capacity well before the program was authorized (appendix figure C.3). Appendix table C.3 contains statistics on jail renovation and construction by 287(g) status: in almost every pre-287(g) year, future 287(g) counties were constructing or renovating their jails. This IV uses a smaller sample than the occupancy IV, due to non-response for the renovation and construction questions.[8] Appendix figure C.4 shows total renovations between 1997 and 2006 by county across the U.S.; no spatial correlations are calculated due to the preponderance of zeros in this value. High levels of law enforcement activity or a more 'active' local law enforcement culture are likely key drivers of both jail renovations, jail occupancy, and interest in 287(g) participation. Even if local population growth or crime was a factor behind each of these, the lower levels of occupancy in 287(g) counties suggest disproportionate investment in jail capacity, and the differences in jail renovation behavior between 287(g) and non-287(g) counties point to a potential mechanism that explains why the jail capacity in a county is predictive of its interest in the program.

[8]Although the 2006 Census of Jail Facilities was mandatory, approximately 33% of counties' jail(s) did not answer the questions on jail renovations and construction: non-response was 18% for 287(g) counties and 35% for border counties. Appendix table C.4 compares the pre-program outcome variables across counties that did and did not report jail renovation data. Counties with jail data tend to have a more robust agricultural sector: they have more farms, and larger farms, on average, as well as more farm workers. This means that counties with jail renovation data are more likely to be responsive to any potential labor shocks caused by 287(g). Further analysis shows that these differences are driven by the control counties, rather than the program or border counties. That is, our sample of control counties for this specification is more likely to be sensitive to a fluctuating agricultural labor supply.

2SLS model

Because we use an instrumental variable, all of the coefficients from our main results are estimates of the local average treatment effect (LATE). We estimate the effect of program participation only for compliers, or the sub-population of counties whose 287(g) status was manipulated by jail occupancy. The counties who would have participated in 287(g) regardless of their jail occupancy are not part of this group: our instrumental variables strategy identifies off of counties with a more active local law enforcement culture. As such, we report reduced form estimates with jail occupancy and renovations as the independent variable of interest. These results are presented in appendix table C.8. Ultimately, we are interested in how farms adapt to labor shortages driven by changes in immigration policy enforcement. To the degree that our estimates capture local enforcement relative to actual 287(g) participation, our estimates should still reflect the impact of enhanced enforcement on firm behavior.

Our estimating equation using jail occupancy levels as an instrumental variable ($Z_{ct)}$) for 287(g) participation is as follows, with instrumented 287(g) participation represented by G^*_{ct} and the other variables defined as in equation 3.1:

$$Y_{ict} = \alpha_0 + \alpha_1 \underbrace{G^*_{ct}}_{=Z_{ct}} + \alpha_2 B_{ct} + \tau_t + \gamma_i + \varepsilon_{ict} \tag{3.3}$$

We also estimate an equivalent specification, with county-year observations and county fixed effects for use with Census of Agriculture data.

$$Y_{ct} = \alpha_0 + \alpha_1 \underbrace{G^*_{ct}}_{=Z_{ct}} + \alpha_2 B_{ct} + \tau_t + \gamma_c + \varepsilon_{ct} \tag{3.4}$$

Exclusion restriction

The validity of our jail occupancy instrumental variable rests on the plausibility of farm decisions and outcomes being unrelated to local jail investment decisions and law enforcement intensity. We consider a variety of factors and trends that may be related to both farm outcomes and local law enforcement and find no evidence to suggest that there is any consistent relationship between them. Further, we find no explanation for how program counties, which are spread across the country, differ structurally from their neighboring counties in a way that would influence our analysis.

Jail occupancy varies spatially and temporally. Because we examine agricultural outcomes 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, the effects would not be localized in just the 287(g) counties. Indeed, any such 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 economic, agricultural, and even legal systems, to the extent that laws and regulations differ by state and county.

County economic specialization

Using the 2004 County Typology data from the USDA Economic Research Service (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 appendix table C.2, which shows that while many differences exist between 287(g) counties and counties without the program, these differences generally do not compromise the exogeneity of the instrument. Most importantly, these data indicate that none of the 287(g) counties are agriculture-dependent (see appendix figure C.5).[9] Although weak to begin with, the connection between economic trends at large and trends in the farm economy are likely to be strongest in counties considered agriculture dependent. As no counties that participated in 287(g) could meet even this low threshold for dependence on agriculture, it is even less likely that general economic conditions affecting jail occupancy or renovations in a county would also be impacting agriculture or vice versa.

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. In addition, none of these counties lost significant population between 2000 and 2010, using the 2015 edition of this data set. 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 15% 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, in that most 287(g) counties did not experience widespread unemployment. Other factors, rather than job loss or other economic decline, 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

[9] 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 https://www.ers.usda.gov/data-products/county-typology-codes.aspx for more information on these data.

counties where poor outcomes in the agricultural sector are likely to affect outcomes for the county's non-farm economy.

Local population shifts

Concerns that growing, rather than shrinking, populations drive both jail occupancy and agricultural outcomes are mediated first by the calculation of the occupancy instrument itself. While both county jail population and county jail rated capacity are strongly and positively correlated with total county population ($r = 0.88$ and $r = 0.84$, respectively), the difference between the two, or the county jail occupancy in our specifications, is only weakly negatively correlated with total county population in a given year ($r = -0.09$). Although not a perfect control, because county population estimates may be endogenous due to inclusion of non-citizens, we also estimated our primary model with annual county population in the estimating equation.

The counties that participated in 287(g) include many major urban areas: for example, Davidson County, TN (Nashville); Fulton County, GA (Atlanta); and Los Angeles County, CA (Los Angeles). Although these counties are atypical in terms of both population (incarcerated or otherwise) and farm outcomes, we test whether our main results are consistent when the largest urban areas are excluded from our sample of both treated and control counties. We use two approaches to exclude more urban areas from our analysis. First, using rural-urban continuum codes (RUCC), we are able to exclude the largest urban areas. Second, both county and city governments had the opportunity to enroll in 287(g). We expect the county-wide programs to have more force, as they cover the rural and agricultural parts of a county where law enforcement officials are more likely to encounter farm workers. However, if our results were driven by growth in these cities competing with agriculture, we would see the opposite–limited impact of the county-level programs.

A related potential confounding factor is that 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 that influence law enforcement. When data on housing values in 287(g) counties is compared to data on housing values in neighboring 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. Appendix figure C.6 shows the trend across the study period for properties with and 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 difference in any year, which provides some evidence that housing value trends are not confounding our estimation strategy.

Weather

Weather patterns, including trends of increasing temperatures, may affect jail occupancy as well as farm outcomes. The lumpiness of jail capacity or renovations data makes analyzing this relationship statistically challenging: while we have access to monthly or even daily weather for each county, jail capacity and renovations are 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 differ in each county. 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 border counties differently than their neighboring program counties.

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 U.S. 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, even if weather had some impact in each county on jail occupancy, the weather events driving an area's jailed population 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, would inhibit rather than enhance agricultural production.

Farm workers and crime

There is little evidence to suggest that farm workers, whether native or immigrant, commit crimes with any more frequency than other members of the population. In fact, agriculturally intensive counties 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. Nonetheless, comparison of the means across these two groups, for a selection of reported crimes, can be found in appendix table C.5.

Extensive research on the relationship between immigration and crime has been conducted (Bell and 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 and et al. (2013) point out, immigrant workers 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.

Recent studies provide no evidence that immigrant farmworkers drive crime-related trends. Hopkins (2010) finds that counties with higher crime rates are actually less likely to consider anti-immigrant ordinances. Likewise, the Secure Communities program, which allowed for the Federal government to check the immigration status of anyone locally arrested, had no impact on local crime levels (Miles and Cox, 2014). Bianchi et al. (2012) found, using data from Italy from 1990-2003, that a higher immigration population did not cause an increase in the crime rate. Robberies did increase, but were a small share of all criminal activity. Hines and Peri (2019) find that increase in deportations under the Secure Communities program did not affect violent or property offenses. Thus, based on available evidence, we believe that it is unlikely that an increasingly criminal immigrant farm labor force was driving both agricultural outcomes and jail occupancy or renovations during our study.

3.4 Results

In this section we report results on the impact of county 287(g) participation on farm and county-level indicators of endogenous technical change. For the purposes of estimating the impact of local immigration enforcement on agriculture using a comparable control group, we restrict our analysis to states that contain at least one local 287(g) program in our main results. This limits the extent to which our results could be driven by comparing outcomes across very different agricultural production areas, although we do explore the implications of this assumption in our robustness check section. We first estimate equations 3.1 and 3.2: the impact of 287(g) at the farm (ARMS) and county (Census) levels without an instrumental variable (see table 3.2)[10].

[10]The reduced form estimates of the impact of the instrumental variable on the outcomes are available in appendix table C.8.

Table 3.2: Impact of 287(g) authorization on production decisions: OLS

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
287(g) authorization	-473.3	-16,581***	3,110	428,258	9,325	1,896,668	-446**
	(347.5)	(4,381)	(9,088)	(618,786)	(18,438)	(7,792,045)	(213)
287(g) border	-100.2	-6,031**	2,061	422,630	-15,576	1,482,159	11
	(81.63)	(3,032)	(3,978)	(589,675)	(26,006)	(5,235,648)	(49.3)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,839	5,125	21,839	5,282	21,839	3,633	3,940
Number of counties	-	1,321	-	1,328	-	1,310	1,326
Number of farms	11,753	-	11,753	-	11,753	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

As discussed, these estimates may be influenced by county- or farm-specific trends correlated with 287(g) participation and farm outcomes. Our main results account for this endogeneity by estimating equation 3.3 using farm-level data and equation 3.4 using county-level data. The first stage results for both specifications are reported in appendix table C.9. The first stage confirms that both jail occupancy and jail renovations are strongly correlated with 287(g) participation using the standard benchmarks for strong instrumental variables. The county-level Census variables are not perfect replications of information reported from ARMS, because variable availability differs, and, more importantly, the data sets each represent a different population of farms. However, both data sets provide information on endogenous technical change in agriculture after implementation of 287(g).

Following these results, we present a variety of robustness checks designed to test whether the main results are sensitive to various changes in the model specification. These changes include the alternative instrumental variables described above (section 4.2.1), alternative measures of enforcement (section 4.2.2), and different control groups (section 4.2.3). In 4.2.3, we also describe results from placebo tests designed to eliminate concern that either time trends or idiosyncrasies about these particular counties drive these results. Section 4.3 describes the sensitivity of our findings when our preferred specification is estimated under different assumptions regarding standard errors. In our final results section, we extend our analysis of the 287(g) acreage response by presenting results for specific acreage allocations. We also report analysis of a group of outcomes that represent the possible impacts of a labor supply shock on the financial health of the local farm economy, or the financial viability of individual farms in program counties.[11]

[11] As with our main results, we report the OLS and reduced form specifications for these results in appendix tables C.10-C.13.

3.4.1 Main results

In table 3.3, we report coefficients for key dependent variables. Fuel expenses per farm and the county level both increase as a result of 287(g) program authorization, while the number of workers and number of acres operated in a county both decline. The decline in workers combined with the increase in fuel suggests farms' attempts at substitution, while the decline in acres is evidence that these efforts fell short of maintaining previous levels of production.

Table 3.3: Impact of 287(g) authorization on production decisions: 2SLS

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
287(g) authorization	-446.7	-747,784**	80,960**	-6,665,275	515,971	-101,677,716	-2,177**
	(488.7)	(305,888)	(36,350)	(8,829,000)	(476,880)	(144,124,166)	(998)
287(g) border	-83.11	-32,684***	7,011	167,180	16,336	-3,151,814	-60.9
	(56.79)	(11,809)	(5,373)	(672,827)	(13,015)	(8,478,773)	(62.7)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,403	5,090	21,403	5,246	21,403	3,607	3,914
Number of counties	-	1,317	-	1,324	-	1,305	1,322
Number of farms	11,501	-	11,501	-	11,501	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

We find that the primary change at the farm level was in fuel expenses, which increased substantially for farms in program counties. Increases in fuel expenses suggest that farm operators had to replace farm labor with equipment, or at least increased their use of equipment. Fuel-powered machinery is one of the only available substitutes for farm labor, although its effectiveness may not extend to all tasks. Our coefficient suggests an average increase of nearly $81,000 per operation in 287(g) counties. Fuel is a major farm expense, averaging nearly $37,000 per farm in a 287(g) county. This large coefficient suggests relatively larger farms increased fuel use in response to 287(g).

Program counties are operating significantly fewer acres, although the farms in the ARMS sample are not decreasing the size of their operations. The decline of 16,000 acres in program counties is roughly 16 percent of the average acreage in 287(g) counties. The most direct evidence of a negative labor supply shock, however, comes from column (7) of table 3.3: the number of agricultural workers in 287(g) counties declined by more than 2,000. This is slightly more than the mean number of workers in 287(g) counties; counties with more labor intensive agriculture were likely more effected. 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.

We do not find a statistically significant change in labor expenses at the farm or county level. This suggests that wages may have increased, given the decline in number of farm workers. However, it is unclear the degree to which this reflects use of more skilled or managerial labor, versus a relative increase in wages for manual labor. Given the increase in fuel expenses and a decline in acreage, we would expect some substitution to more expensive types of workers. Overall, our results suggest that some farms were able to adapt to 287(g), but the decline in acreage suggests limited endogenous technical change in response to the program.

3.4.2 Robustness checks

We conduct multiple robustness checks for our main specification. In addition to being generally robust to different approaches for standard error estimation, our results are largely consistent under a variety of control groups, noisier measures of 287(g) implementation, and changing the composition of the non-treated, counties. We also conduct a series of placebo tests to test the validity of our instrumental variable strategy.

Alternative IVs

As discussed above, earlier construction of new jails and renovation of existing ones could be one mechanism through which expanding jail capacity influenced program participation. To test this, we use the running total of jail renovations in a county as an alternative instrument and report the results in table 3.4. Although weaker, the results are consistent with the evidence of a labor supply shock found above: increasing fuel expenses at both the farm and county level, as well as a direct decline on the number of workers in a county. We also use both the occupancy and renovations instrumental variables together, again with consistent results. These results appear in appendix table C.14.

Table 3.4: Impact of 287(g) authorization: 2SLS with jail renovations IV

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
287(g) authorization	203.3	-51,332	103,600**	28,482,309*	-302,444	319,888,737	-7,362*
	(308.8)	(59,423)	(51,432)	(16,135,580)	(216,437)	(280,417,391)	(4,047)
287(g) border	-102.2	-7,188**	8,893	1,464,935*	-52,543	15,548,739	-280
	(119.8)	(3,532)	(8,135)	(782,050)	(56,920)	(13,140,350)	(180) [b]
County FE	NO	YES	NO	YES	NO	YES	YES [t]
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	14,591	5,063	14,591	5,218	14,591	3,589	3,894
Number of counties	-	1,305	-	1,312	-	1,294	1,310
Number of farms	7,858	-	7,858	-	7,858	-	- [b]

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Alternative measures of enforcement

Although we see an impact on farm production decisions using authorization alone, which suggests strong signalling effects, we also have access to information on the extensive margin of program participation by each program jurisdiction. Statistics on the number of "aliens identified" or "departed" in a given jurisdiction in a year are also reported by ICE. "Aliens identified" 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.[12]

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 3.5 and appendix table C.15 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 farm workers than counties with minimal enforcement.

[12]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).

Table 3.5: Impact of 287(g) enforcement levels: Aliens identified, 2SLS

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
Aliens identified	-0.0714	-983**	12.94**	-8,692	82.49	-103,824	-2.55**
	(0.0729)	(441)	(5.125)	(11,615)	(64.09)	(148,353)	(1.1)
287(g) border	-61.10	-27,264***	3,021	230,364	-9,092	-1,206,080	-24.9
	(72.73)	(9,409)	(3,793)	(637,723)	(21,069)	(6,570,462)	(54)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,403	5,090	21,403	5,246	21,403	3,607	3,914
Number of counties	-	1,317	-	1,324	-	1,305	1,322
Number of farms	11,501	-	11,501	-	11,501	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

In this noisier specification, the direction of coefficients and levels of statistical significance for 287(g) counties reported in these results are consistent with those reported in table 3.3. The magnitude of 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.

In addition, anti-immigrant hostility or increased policing of immigrant communities is not necessarily restricted to communities with 287(g) programs. There are, for example, counties or cities that applied for 287(g) program authorization but were denied. These counties may have had similarly hostile environments to authorized counties, but without any official mandate. By restricting our treated sample to these counties alone (excluding counties with 287(g) authorization), we can pick up the effect of being in a relatively more immigrant-hostile community on farm production decisions and the farm economy.

Our county-level results are largely robust to this specification, although with lower levels of statistical significance and lower magnitudes. Counties that wanted but were denied 287(g) programs have fewer agricultural workers in the period after their application was denied. They also have fewer agricultural acres operated, as in the main specification. Unlike in program counties, we also find a lower level of labor expenses, as well as lower fuel expenses (see table 3.6). Taken together, these outcomes could indicate declining levels of production, which suggests law enforcement attitudes as a deterrent for farm workers and low substitutabilty between native and immigrant farm workers. With farm-level data (ARMS), we find no statistically significant relationship of 287(g) with fuel expenditure. There were only slightly over 200 farms in denied counties, which is less than half the number of farms in counties with 287(g) authorization, which means that our estimates may be less precise than when using counties with authorization.

Table 3.6: Impact of 287(g) program application denial on production decisions: 2SLS

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
Applied for & denied 287(g)	-2,235	-123,943*	-190,243	-6,650,983*	993,784	-164,294,993**	-1,952***
	(6,280)	(72,124)	(285,275)	(3,977,318)	(1.808e+06)	(74,202,027)	(586)
Denied 287(g) border	-68.17	-10,158***	-2,523	-203,522	23,984	-974,168	-117**
	(152.9)	(3,768)	(6,289)	(433,448)	(49,548)	(6,388,808)	(47.9)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	20,904	8,478	20,904	8,695	20,904	6,048	6,484
Number of counties	-	2,181	-	2,189	-	2,160	2,188
Number of farms	11,470	-	11,470	-	11,470	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses
[b] *** p<0.01, ** p<0.05, * p<0.1
[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

In a separate specification, we include all the jurisdictions that wanted 287(g) authorization, grouping both treated and denied counties together, and report these results in table 3.7. These results are very similar to those in our main specification: fewer acres operated, increased fuel expenses, and a decreasing population of agricultural workers. Like the results for the denied-alone group, we also see lower labor expenses for counties that wanted a 287(g) program, irrespective of whether they received it. Results for ARMS variables are very similar to the results for farms in 287(g)-authorized counties only, but this may reflect the larger population of farms in authorized counties.

Table 3.7: Impact of 287(g) program application (denied and accepted) on production decisions: 2SLS

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
Wanted 287(g) authorization	-427.2	-234,084**	71,214**	-4,408,513	475,860	-117,101,409**	-1,728***
	(463.6)	(103,157)	(32,773)	(3,281,700)	(442,676)	(57,738,016)	(431)
Wanted 287(g) border	-25.82	9,473	10,555*	373,506	31,346	12,146,331***	83.8**
	(50.67)	(6,844)	(6,161)	(302,427)	(29,027)	(4,293,885)	(42.3)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,403	8,816	21,403	9,064	21,403	6,300	6,757
Number of counties	-	2,274	-	2,283	-	2,252	2,281
Number of farms	11,501	-	11,501	-	11,501	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Alternative control groups

As discussed, demographic change, through forces such as population loss or urbanization, is one potential confounder of these effects. To address the former, we show that none of these counties were classified as experiencing significant population loss (see appendix table C.2). For the latter, we run our main specifications on a sample of counties that excludes (1) all of the largest urban areas from both the treated 287(g) group and from the control and (2) counties with city-level 287(g) programs. These results are presented in table 3.8 and appendix table C.16. Our findings from section 3.4.1 on per-county acres operated and on the number of workers per county from the Census specifications are robust to these restricted specifications. The results on fuel expenses do not hold; increased fuel costs may have been correlated with production in more urbanized counties.

Table 3.8: Impact of 287(g) authorization: 2SLS without very urban counties

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
287(g) authorization	-1,095	-1,241,543*	-45,723	-39,247,146*	225,490	-351,341,514	-8,305***
	(2,248)	(682,748)	(35,257)	(22,806,081)	(528,499)	(337,010,501)	(3,179)
287(g) border	-136.6**	-7,504	241.3	157,182	2,512	2,358,064	-88.6
	(66.26)	(8,659)	(3,769)	(937,699)	(17,233)	(9,011,994)	(94.5)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	18,604	9,128	18,604	9,374	18,604	6,536	6,987
Number of counties	-	2,356	-	2,365	-	2,339	2,363
Number of farms	10,004	-	10,004	-	10,004	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

[d] Rural-Urban continuum code (RUCC) developed by the Economic Research Service (ERS) of USDA; very urban counties (RUCC = 1) excluded

We expect the county-wide 287(g) programs to be more salient for farm workers, who are likely to spend most of their time in rural areas. If our results were driven by urbanization, we would expect to see impacts from city-level programs as well. Results that exclude city-level programs show that our findings are indeed driven by the county level programs (see appendix table C.16, although there is a small signaling effect of 287(g) from city programs. These results, along with a specification controlling for county population (see appendix table C.17, address concerns that major demographic shifts drive both jail occupancy and farm outcomes.

When we drop urban areas or city-program counties from ARMS, we lose nearly half of our treated farms. Given that treated ARMS farms may or may not use hired labor and represent a smaller number of farm specializations, generally it is not surprising that our result for fuel expenditure becomes inconclusive. However, pressure from metropolitan areas does not explain the decline in acres operated or in the number of agricultural workers, which suggests our empirical strategy isolates the 287(g) labor supply shock. Generally, this analysis suggests some interesting heterogeneity in 287(g) response which is difficult to isolate with our data sets and empirical strategy.

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[13]. These results do not provide definitive evidence of the validity of our instrument, but instead address concerns about pre-trends or other confounding factors driving our results in the treated counties. In the first placebo test, we naively treat the year 2002 as the year 287(g) authorizations began for the treated counties. In doing so, we effectively shift our analysis five years into the past: for example, a county that signed

[13]Data structure, as well as slow processing times through secure data access for farm-level ARMS data, preclude us from implementing similar approaches with farm-level data

an MOU in 2008 is modeled as having signed it in 2003. In this test, significant coefficients would suggest that endemic characteristics of these treated counties, other than the 287(g) program, are driving our results. We find only one weakly significant coefficient (on fuel expenses) in this analysis, which is reported in table 3.9.

Table 3.9: Census placebo test: Authorization moved 5 years earlier, 1997-2007

	(1) Acres operated per county (Census)	(2) Fuel expenses ($) (Census)	(3) Labor expenses ($) (Census)
287(g) authorization	-129	-19,919	924,895,544
	(285)	(24,948)	(816,792,052)
287(g) border	-5.05	-7,888**	28,947,413
	(25)	(3,270)	(58,750,081)
County FE	YES	YES	YES
Farm FE	NO	NO	NO
Year FE	YES	YES	YES
Observations	3,944	3,814	3,950
Number of counties	1,335	1,325	1,335

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

[d] 287(g) authorization manually set to five years earlier than actual authorization; outcome variables limited to those available at the county level in 1997

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 widespread trends in agriculture, or unobserved characteristics of the time in which these programs were enacted. To address, though not fully 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. We randomly selected 53 counties, without replacement, and randomly assigned them the start date of an actual 287(g) county. Although this method falsely 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 500 times, with nothing to indicate more than spurious results.[14] We are also able to explore whether there are certain 287(g) counties that are driving our results by randomly dropping 10% of the treated sample and re-running our main specification. Our findings from table 3.3 are robust to this procedure, and there is no indication a particular county is driving these results.[15]

Finally, our results are robust to a specification that uses all counties in the contiguous United States in the control, rather than limiting the control group untreated, non-adjacent counties in states with a 287(g) jurisdiction, as in our main specification. These results are available in appendix table C.20. 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. Our restricted control group takes into account that program participation may not have been as viable, economically or politically, in different parts of the country.

[14]Results reported in table C.18.
[15]Results available in table C.19.

3.4.3 Standard errors

We report standard errors that are robust to correlation at the strata level for the ARMS results (following Weber and Clay (2013)) and heteroskedasticity-robust standard errors for Census results, and acknowledge that any single approach to estimating standard errors for this study requires assumptions that may not always hold. Our results are largely consistent across a number of approaches to specifying standard errors. In the Appendix, we report our main results with standard errors robust to correlation at the ERS production region and state level (appendix tables C.21 and C.22, respectively), as well as county-level clusters for our Census outcomes (appendix table C.23).

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. Further, at the region level, an insufficiently large number of clusters becomes a concern.

While more numerically intensive approaches are not possible for our farm-level data due to slow processing times through secure ARMS access, we are able to use estimate wild cluster bootstrap standard errors following Cameron and Miller (2015) with Census data, which are noted in appendix table C.21. We also report Conley spatial standard errors for Census data, which are robust to correlation within 200km from the county centroid. The Conley spatial standard errors are not designed to be used with an IV, so the results are reported from manual 2SLS in appendix table C.24. Our main results are generally consistent across these varied assumptions regarding standard error calculation.

3.4.4 Additional measures of 287(g) impact

Together, farm- and county-level results show evidence of a negative labor supply shock with limited endogenous technical change. As such, farms may be making longer-term adjustments that affect their profitability and operational structure. Our data also allow us, although with imprecise measures, to consider 287(g) impacts on substitution between crop type and impacts on farm viability. We first estimate the impact of the 287(g) program on a set of variables related to specific acreage allocations. Although not a direct test of the theory of endogenous technical change due to data limitations, we examine whether there is evidence of operations shifting acreage in or out of different production specializations, which may to some degree reflect increased use of automatic technology. This effectively disaggregates the "acres operated" outcome into possible component parts: vegetable acres harvested, fruit acres harvested, and mechanized acres harvested.[16] While many fruits and vegetables can be mechanically harvested, others cannot; harvest of many other crops, like field or row 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.[17] Vegetable and fruit production is generally more labor-intensive, so a decline in the number of vegetable or fruit acres harvested may reflect less labor use. Similarly,

[16] All of these are calculated from the ARMS data and thus farm-level; of these, only vegetable acres harvested is reported in the Census. All are only reported in terms of acres harvested.

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

an increase in 'mechanized crops' would suggest substitution between different types of production. Our data does not allow us to capture a shift from manual to automatic technologies within the fruit and vegetable category. Table C.6 presents summary statistics for this set of supplementary outcomes.

We find no statistically significant change in acres of fruit, vegetables, or mechanized crops harvested, shown in table C.25. While unexpected, this may reflect our inability to distinguish between processing and fresh market speciality crop production, or the degree to which fruit and vegetable production is mechanized. Our null result for 'mechanized acres' may reflect the high cost associated with changing production system, or agronomic suitability playing a primary role relative to labor costs or shortages. Further, we also observe that counties adjacent to a 287(g) county experienced a statistically significant increase of 17 vegetable acres harvested, which is about half of the average non-287(g) county production. Hypothetically, 287(g) authorization could have deterred immigrant labor in adjacent counties or encouraged workers to move to these bordering counties. This result provides some suggestive evidence that there may have been positive labor supply spillovers associated with 287(g). In other words, undocumented workers may have moved to nearby counties, where production practices are similar, but do not have the punitive 287(g) authorization programs in place. This may have allowed farms in bordering counties to expand production levels without increasing expenditure.

The endogenous technological change framework suggests a decline in production in terms of output and land use if labor and technology are not substitutable or if substitution is incomplete. This could lead to a decline in firm profitability or other, broader local economic impacts. We augment our ability to predict the long-term effects of labor supply shocks on the farm economy using a set of variables that capture the financial health of the farm business. These outcomes, summarized in table C.7, include machinery value and real estate asset value, two measures of the agricultural asset base; net cash farm income, a measure of farm profitability; and two measures that approximate the long term viability of the farm business (value of debt held) and of the county-level farm economy (number of farms). Changes in these variables point to larger structural changes in the local farm sector. In particular, number of farms captures the most extreme outcome, farm exit or ceased operations, that can occur as a result of a challenging farm business environment. Both net farm income and asset value 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. Changing machinery values could reflect a shift to automatic technologies, as well as changes to the quantity of machinery and equipment owned by farm operations.

Although no truly long-term impacts can be considered, we have data from up to five years following 287(g) implementation. Results of our analysis of the relationship between 287(g) and various indicators of farm financial status are reported in table C.26. For the farm-level data, we do not find strong evidence that the relationship between 287(g) and farm machinery value, net income, real estate asset value or debt is statistically different than zero. For these ARMS farms, sampled before and after 287(g) implementation, this provides no support for broader impacts for farms that survived post-287(g). Our negative, weakly significant coefficient on real estate values does provide suggestive evidence that our results of 287(g) impact are not confounded by urban development, which typically drives up land values (Borchers et al., 2014).

However, results using the Census data do suggest a decline in the farm economy of treated counties: most importantly, a significantly smaller number of farms in these counties. This joint decline in both the number of farms and the number of acres is also accompanied by a decline in net income and machinery value. Although machinery value, one important farm asset, declines in value, we do not observe any change in real estate asset values at the county level. This finding suggests that our findings in general are not driven by urbanization pressures or other land use changes that drive up land values. It is also worth noting that our farm-level sample is more representative of large, commercial-scale operations. While detailed analysis of structural change in the farm sector is beyond the scope of this study, the difference in results using farm and county level data suggest that larger farms may have been more likely to survive in a higher-labor cost environment.

3.5 Conclusion

This study provides a unique and robust analysis of how enhanced local immigration enforcement affected firms in a sector with a high level of undocumented immigrant labor. We use an unbalanced panel of farm survey data and a balanced panel of county-level agricultural Census data to test the degree to which endogenous technical change occurs in response to local labor supply shocks in agriculture. We consider the impact of 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". Based on reported perceptions of possible revenue generation through 287(g) participation, we developed a novel approach to address the potential endogeneity of participation in the 287(g) program. We use pre-program county jail occupancy as an instrumental variable, as pre-program jail occupancy is strongly correlated with 287(g) participation and arguably exogenous from farm sector trends. This instrumental variables approach may be useful in future studies on law enforcement and immigration policy; further, the first stage may be relevant to scholars of criminal justice and local government.

We find that county participation in 287(g), across a number of specifications, leads to increased fuel expenditure at the farm and county level, as well as a decline in the number of workers and land in farms at the county level. Likewise, we do not find evidence of a change in labor expenditure at the farm or county level. Jointly, these results imply (1) consistent with other studies, 287(g) caused a a labor supply shock; (2) farms that remained in operation likely used machinery more intensively; (3) suggestive evidence that wage levels increased or that there was increased use of skilled/managerial labor; and (4) limited substitutability of immigrant farmworkers, as indicated by the decline in land in farms in 287(g) counties. 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. Overall, we see limited evidence for endogenous technical change in the case of enhanced local immigration enforcement.

Our data and empirical strategy preclude estimation of labor supply or demand elasticities. Available data unfortunately provide insufficient information on agricultural wages, worker types or meaningful output quantities. We also do not observe specific production technologies and our measures of mechanization are noisy, i.e., we cannot distinguish between fruit and vegetable production that uses different levels of automation. As such, we rely on measures of production expenditures, acres operated, and number of workers, which are the best available direct measures of responses to a labor supply shock and may reflect endogenous technical change. Another concern is how results of this study are interpreted. We estimate the local average treatment effect, which is driven by counties whose participation in 287(g) was driven by lower jail occupancy and likely reflects 'active' local law enforcement. As our broad research interest is local immigration enforcement as opposed to any particular program, we believe that our results provide important information about about how a sector that is dependent on manual labor responds to a local labor supply shock. Further, 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 narrow geographic scope of 287(g) participation.

Evaluation of programs that are more widely implemented would improve external validity of future research, especially policies across different jurisdictions. State and national policies may provide opportunities for further analysis of how the farm sector is responding to labor supply shocks. Analysis of firms in other sectors that use manual labor would provide broader information on the impacts of immigration policy on the U.S. economy. Future work would be improved by accurate, precise data on actual wages by worker type, production quantities, specific crop type and automation technologies. This would allow for a more structural interpretation of our findings and more provide more detail on farm and sector-level adaption to labor shortages. The growth of guestworker programs may provide insight into the substitutability of immigrant labor.

Immigration policy will likely continue to be politically contentious and it is uncertain whether a political agreement will ever emerge to provide more certainty for sectors with high levels of undocumented labor. Our results imply that the agricultural sector did, to some degree, adapt to increased local immigration enforcement through the 287(g) program, but they also provide strong evidence that neither technology nor native workers are complete substitutes for undocumented farm workers when local immigration enforcement increases. This implies that locations with relatively stringent immigration enforcement, or an otherwise restricted labor supply, may be less competitive for production of labor-intensive crops. Given that 287(g) was not widely implemented, it is not surprising that mechanization or innovation or substitution with native workers in response to the policy was limited. Broader, national policy changes such as the bracero exclusion considered by Clemens et al. (2018) may be more likely to induce mechanization, with impacts consistent with endogenous technical change. Current investments in robotics (Seabrook, 2019) suggest that the tight domestic labor market and current immigration policies may be driving innovation for the sector as a whole.

Our research suggests that firm-level impacts are an important part of the broader literature that considers the many impacts of immigration policy: wages, the well-being of individuals 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, and economic, debate on immigration policy.

APPENDIX A

CHAPTER 1 OF APPENDIX

Table A.1: Data sources for import penetration measure

Variable	Source
f_{ij95}	IPUMS 1990*
L_{ij95}	County Business Patterns 1995
L_{i95}	County Business Patterns 1995
M_{jt}^{cu}	Comtrade 1996
Y_{j96}	Comtrade 1996
M_{j96}	Comtrade 1996
X_{j96}	Comtrade 1996

*Census 5% sample

APPENDIX B

CHAPTER 2 OF APPENDIX

B.1 Additional results

Table B.1: Farm cross section results: OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Short term debt				Short term debt		
	Repaid	Outstanding	Total	Leverage	Repaid	Outstanding	Total	Leverage
ln Premium paid	1,774***	1,246***	3,020***	-0.000124				
	(599.2)	(418.3)	(930.7)	(0.000622)				
FCI Participation					35,724***	22,088**	57,811***	0.0439***
					(8,752)	(8,556)	(16,460)	(0.0138)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	60,168	60,168	60,168	60,158	85,998	85,998	85,998	85,983
R-squared	0.227	0.049	0.099	0.215	0.037	0.088	0.037	0.078

[a] Standard errors robust to correlation at the ARMS strata level in parentheses
[b] Includes controls for farm and operator characteristics
[c] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.2: Farm panel results: Pooled OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Short term debt				Short term debt		
	Repaid	Outstanding	Total	Leverage	Repaid	Outstanding	Total	Leverage
Premium paid	3,061***	1,762**	4,603***	0.00987				
	(1,008)	(756.4)	(1,626)	(0.0572)				
FCI Participation					83,193***	30,841**	115,785***	-0.000808
					(20,059)	(14,345)	(26,962)	(0.0361)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	27,921	27,921	27,921	27,921	22,371	22,371	22,371	22,369
R-squared	0.227	0.049	0.099	0.215	0.037	0.088	0.037	0.078

[a] Standard errors robust to correlation at the farm level in parentheses
[b] Includes controls for farm and operator characteristics
[c] *** p<0.01, ** p<0.05, * p<0.1

B.2 Full results and summary statistics

Table B.3: Full ARMS cross section and restricted cross section

	Full ARMS cross section			**Restricted cross section**		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
FCI participation*	216,100	0.364	0.481	91,171	0.680	0.467
Premium paid per acre ($)	197,452	5.424	77.71	88,867	$ 7.24	$ 15.58
Outcomes						
Loans repaid within a year	286,370	$ 95,983.94	$ 578,071.10	123,122	$ 171,142.50	707988.9
Debt remaining at end of year	286,370	$ 61,394.06	$ 507,707.80	123,122	$ 95,246.66	500031.9
Total short term debt	286,370	$ 157,378.00	$ 852,557.30	123,122	$ 266,389.20	$ 968,516.80
Short term debt/variable expenses	281,672	0.482	17.87	122,860	0.556	15.68
Real estate long-term debt	286,370	$ 158,077.00	$ 829,087.90	123,122	$ 206,759.30	$ 895,115.90
Non-real estate long-term debt	286,370	$ 59,108.42	$ 716,008.30	123,122	$ 84,739.47	$ 462,139.70
Debt-to-asset ratio	286,232	0.17	3.15	123,091	0.21	3.36
Debt repayment capacity utilization	286,370	$ 23,710.82	$ 874,709.70	123,122	$ 26,615.87	$ 542,141.70
Operator characteristics						
Operator age	286,370	56.32	12.49	123,122	55.05	12.08
Retired from farming	277,802	12.90%	33.72%	119,508	7.75%	27.33%
Female	282,228	5.74%	23.26%	120,685	2.11%	14.36%
Hispanic or non-white	275,372	4.06%	19.73%	118,575	2.43%	15.41%
Total off-farm income	272,331	$ 64,831.96	$ 185,222.10	117,149	$ 53,171.37	$ 142,985.80
Operation characteristics						
Acres operated	286,370	1224.16	6525.16	123,122	1681.34	4262.38
Share of acres owned	286,370	0.868	17.97	123,122	0.551	1.738
Share of cropland operated	286,032	0.663	2.301	123,071	0.835	2.050
Operators' education is:						
Some high school	286,370	7.28%	25.98%	123,122	5.16%	22.12%
High school diploma	286,370	41.54%	49.28%	123,122	41.53%	49.28%
Some college	286,370	26.99%	44.39%	123,122	29.50%	45.61%
4-year college graduate+	286,370	22.82%	41.97%	123,122	23.05%	42.12%
Other	286,370	1.37%	11.62%	123,122	0.76%	8.67%
Sales class						
$500,000+	286,370	29.03%	45.39%	123,122	42.18%	49.39%
$250,000-$499,000	286,370	12.53%	33.11%	123,122	18.56%	38.88%
$100,000-$249,000	286,370	14.13%	34.84%	123,122	18.47%	38.81%
$40,000-$99,999	286,370	10.74%	30.97%	123,122	11.08%	31.39%
$20,000-$39,000	286,370	6.82%	25.21%	123,122	4.54%	20.81%
$10,000-$19,000	286,370	6.24%	24.19%	123,122	2.40%	15.32%

$9,999 or less	286,370	20.51%	40.38%	123,122	2.76%	16.39%
Specalization						
General cash grain	286,370	6.75%	25.09%	123,122	15.26%	35.96%
Wheat	286,370	3.02%	17.12%	123,122	6.57%	24.77%
Corn	286,370	9.82%	29.76%	123,122	22.07%	41.47%
Soybeans	286,370	4.87%	21.53%	123,122	10.19%	30.25%
Sorghum	286,370	0.22%	4.73%	123,122	0.46%	6.76%
Rice	286,370	1.09%	10.39%	123,122	1.73%	13.03%
Tobacco	286,370	1.01%	9.98%	123,122	1.22%	10.98%
Cotton	286,370	2.07%	14.25%	123,122	4.66%	21.07%
Peanut	286,370	0.38%	6.19%	123,122	0.63%	7.89%
Other crops	286,370	12.62%	33.21%	123,122	7.41%	26.19%
Fruit	286,370	5.91%	23.59%	123,122	2.31%	15.02%
Vegetable	286,370	2.02%	14.08%	123,122	1.85%	13.49%
Nursery	286,370	3.86%	19.27%	123,122	1.03%	10.09%
Cattle	286,370	22.65%	41.86%	123,122	9.05%	28.69%
Hogs	286,370	2.69%	16.19%	123,122	3.59%	18.60%
Poultry	286,370	6.57%	24.77%	123,122	3.07%	17.24%
Dairy	286,370	8.47%	27.85%	123,122	7.07%	25.64%
Other livestock	286,370	5.95%	23.66%	123,122	1.85%	13.48%
Operator occupation						
Work on farm	286,370	70.32%	45.69%	123,122	86.06%	34.64%
Off-farm employment	286,370	15.67%	36.35%	123,122	8.93%	28.52%
Not in workforce	286,370	11.32%	31.68%	123,122	4.24%	20.14%
Other occupation	286,370	2.70%	16.20%	123,122	0.78%	8.80%

Full ARMS cross section includes all farms in the ARMS sample.

The restricted cross section is limited to farms that have at least $10,000 value of production for crops with widespread crop insurance availability.

*Measured as a binary variable equal to 1 if the operation has any acres enrolled in Federal crop insurance

Table B.4: Full results: OLS with cross-section and premium paid

	(1) totalshort	(2) financed	(3) dshort	(4) repaid	(5) evtot	(6) dreale	(7) dnreale	(8) leverage
Premium paid per acre	3,020***	0.002***	1,246***	1,774***	4,411***	1,491***	2,028**	-0.000
	(930.7)	(0.001)	(418.3)	(599.2)	(1,361)	(501.9)	(884.9)	(0.001)
High school grad	29,097	-0.007	16,876	12,221	100,545**	13,530	8,658	0.077
	(32,746)	(0.022)	(13,447)	(20,130)	(40,786)	(14,665)	(6,361)	(0.057)
Some college	23,611	0.004	16,068**	7,543	92,691***	24,873**	10,911	0.010
	(14,873)	(0.019)	(7,383)	(9,537)	(25,147)	(12,059)	(6,796)	(0.024)
College grad	69,237	-0.016	33,406*	35,831	197,305***	56,087**	24,384***	0.025
	(44,078)	(0.021)	(20,169)	(24,764)	(71,716)	(22,795)	(3,775)	(0.030)
Sales $250,000-$499,999	-308,155***	-0.037***	-108,121***	-200,035***	-598,550***	-218,816***	-91,002***	0.069
	(69,892)	(0.007)	(28,514)	(42,129)	(138,666)	(57,409)	(21,754)	(0.110)
Sales $100,000-$249,999	-332,663***	-0.092***	-120,395***	-212,267***	-617,827***	-242,162***	-103,166***	-0.046
	(67,565)	(0.018)	(28,419)	(39,868)	(125,718)	(53,285)	(20,965)	(0.033)
Sales $40,000-$99,999	-329,108***	-0.129**	-120,460***	-208,648***	-590,461***	-248,219***	-102,028***	-0.035
	(64,476)	(0.058)	(27,508)	(38,100)	(115,424)	(46,487)	(17,856)	(0.045)
Sales $20,000-$39,999	-313,811***	-0.261***	-116,031***	-197,780***	-573,314***	-252,165***	-96,355***	-0.056
	(63,625)	(0.038)	(27,628)	(37,291)	(114,669)	(44,660)	(16,272)	(0.048)
Sales $10,000-$19,999	-313,101***	-0.297***	-116,409***	-196,692***	-589,198***	-251,211***	-96,592***	-0.0872*
	(64,294)	(0.052)	(27,495)	(38,307)	(119,601)	(46,199)	(17,901)	(0.050)
Sales $9,999 or less	-319,151***	-0.273***	-117,675***	-201,476***	-669,795***	-269,942***	-97,265***	-0.059
	(66,042)	(0.054)	(28,191)	(39,303)	(135,390)	(52,654)	(21,668)	(0.049)
Acres operated	56.29***	8.02e-06**	21.38***	34.91***	108.1***	30.47***	14.29***	-3.90e-06
	(10.36)	(3.35e-06)	(4.486)	(6.232)	(40.17)	(6.412)	(3.131)	(3.26e-06)
Share acres owned	-5,139	-0.026***	-474.2	-4,664	8,321	36,907*	-0.241	-0.037*
	(4,806)	(0.009)	(2,451)	(2,974)	(12,229)	(20,462)	(1,416)	(0.019)
Total off-farm income	0.053	-1.15e-08	0.019	0.034	0.132**	0.012	0.008*	1.04e-07
	(0.068)	(2.47e-08)	(0.018)	(0.051)	(0.051)	(0.028)	(0.004)	(8.68e-08)
Percent cropland	-1,725***	-8.08e-05	-680.5***	-1,045**	-3,612**	-865.6**	-407.9*	1.23e-05
	(664.2)	(0.000)	(259.1)	(415.6)	(1,697)	(347.9)	(215.3)	(8.76e-05)
Operator age	-2,314***	-0.007***	-690.2***	-1,623***	190.3	-2,073***	-688.7***	-0.002
	(613.2)	(0.001)	(254.8)	(374.0)	(649.2)	(502.4)	(243.9)	(0.003)
Wheat	-152,056**	-0.072***	-55,022***	-97,034**	-275,065**	-84,405**	-30,791	0.026
	(65,112)	(0.025)	(20,305)	(44,993)	(132,991)	(40,604)	(19,841)	(0.020)
Corn	33,807	0.081***	19,755**	14,051	-21,208	29,597*	591.7	0.061

	(21,697)	(0.010)	(9,364)	(12,581)	(19,647)	(17,632)	(7,795)	(0.059)
Soybean	-8,502	0.055***	3,561	-12,063	-17,736	14,647	-259.2	-0.033
	(10,113)	(0.013)	(6,995)	(8,585)	(27,879)	(12,182)	(5,097)	(0.021)
Sorghum	-101,989**	0.023	-35,823**	-66,166**	-157,461	-33,525	-5,980	0.039
	(43,096)	(0.074)	(15,296)	(27,978)	(104,132)	(24,332)	(21,909)	(0.054)
Rice	25,007	0.198***	8,935	16,072	-163,767	-78,405**	-24,663	-0.297
	(54,462)	(0.054)	(24,434)	(36,311)	(122,154)	(34,667)	(15,826)	(0.231)
Tobacco	8,925	-0.054*	-20,825	29,750*	168,093***	-2,706	-11,370	-0.007
	(28,989)	(0.030)	(13,489)	(16,692)	(50,191)	(17,658)	(16,767)	(0.062)
Cotton	18,895	0.167***	-20,439	39,334***	-95,825	-66,539***	-19,116	0.557
	(21,949)	(0.020)	(14,412)	(9,257)	(66,143)	(13,912)	(12,281)	(0.445)
Peanut	37,183**	0.214***	-7,199	44,382**	-22,062	-36,969	-16,282	0.145
	(17,507)	(0.045)	(13,723)	(19,264)	(45,131)	(32,714)	(11,157)	(0.112)
Other crops	21,205	0.038**	2,964	18,241	20,799	-22,143	1,812	0.029
	(18,491)	(0.019)	(6,191)	(12,921)	(55,867)	(19,451)	(10,135)	(0.020)
Fruit	-77,443	0.164	-14,955	-62,488**	-22,885	113,167	-72,058***	0.011
	(58,806)	(0.251)	(32,310)	(27,930)	(126,120)	(97,056)	(24,526)	(0.042)
Vegetable	524,350*	-0.059**	222,293	302,058*	1.262e+06**	58,232	98,469	0.063**
	(293,903)	(0.024)	(143,085)	(153,588)	(638,100)	(52,958)	(67,440)	(0.027)
Nursery	57,390	-0.238***	34,265	23,125	1.196e+06**	115,466	46,113	0.116
	(86,601)	(0.037)	(32,025)	(56,099)	(584,725)	(81,759)	(39,635)	(0.080)
Cattle	85,553*	0.090***	64,369**	21,185	145,016*	13,544	10,128	0.032
	(46,758)	(0.026)	(30,248)	(22,643)	(85,698)	(19,822)	(8,886)	(0.021)
Hogs	-81,535***	-0.037	6,695	-88,231***	92,232	78,115***	9,337	0.061
	(28,428)	(0.029)	(8,065)	(23,114)	(67,441)	(17,582)	(9,370)	(0.039)
Poultry	-162,860***	-0.021	-23,255	-139,604***	-143,067	99,622***	-12,155	0.104
	(57,570)	(0.050)	(25,849)	(37,419)	(140,970)	(26,388)	(19,301)	(0.071)
Dairy	36,351	-0.337***	63,875	-27,524	950,548*	408,136*	171,538**	0.065*
	(126,983)	(0.027)	(61,582)	(66,948)	(511,400)	(208,963)	(79,372)	(0.039)
Other livestock	-9,080	-0.157***	8,515	-17,595	170,343**	53,951	16,809	-0.006
	(34,833)	(0.023)	(10,906)	(25,747)	(83,727)	(35,359)	(11,501)	(0.014)
Nonfarm employment	-40,960**	0.014	-7,856	-33,104***	-10,338	-12,321	-5,928	0.011
	(16,364)	(0.021)	(5,571)	(12,761)	(34,711)	(9,943)	(5,649)	(0.015)
Not in workforce	-686.7	0.013	5,670	-6,356	26,485	-12,340	2,411	0.033
	(7,443)	(0.032)	(8,406)	(5,677)	(40,210)	(15,499)	(6,368)	(0.039)
Occupation other	151,982	-0.048	77,245	74,738	48,088	48,535	76,709	0.022
	(149,248)	(0.085)	(66,447)	(83,260)	(132,042)	(74,566)	(70,972)	(0.059)
Retired from farming	-20,183	-0.084**	-14,843**	-5,340	-11,408	-31,401**	1,864	-0.050
	(15,115)	(0.037)	(7,412)	(9,906)	(15,545)	(13,409)	(8,465)	(0.044)

Female	-55,700**	-0.098***	-11,378	-44,322**	-62,550***	-41,535	-6,186	0.026
	(22,486)	(0.028)	(13,660)	(18,510)	(15,820)	(27,835)	(9,160)	(0.027)
Nonwhite or Hispanic	1,784	0.012	19,846	-18,061*	7,609	26,600*	-6,772	-0.0031
	(27,654)	(0.024)	(18,564)	(10,394)	(27,728)	(15,456)	(8,953)	(0.048)
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	366,740***	0.972***	95,924**	270,816***	250,854	266,040***	90,814***	0.130
	(80,921)	(0.0520)	(39,027)	(48,958)	(202,391)	(24,360)	(15,625)	(0.360)
Observations	60,168	60,048	60,168	60,168	60,168	60,168	60,168	60,158
R^2	0.120	0.022	0.054	0.112	0.207	0.068	0.057	0.002

Standard errors robust to correlation at the ARMS strata level in parentheses

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

Table B.5: Full results: OLS with cross-section and acres enrolled dummy

	(1) totalshort	(2) financed	(3) dshort	(4) repaid	(5) evtot	(6) dreale	(7) dnreale	(8) leverage
Acres enrolled dummy	57,811***	0.057	22,088**	35,724***	12,986	21,061*	14,757***	0.044***
	(16,460)	(0.071)	(8,556)	(8,752)	(18,487)	(10,793)	(4,997)	(0.014)
High school grad	9,463	0.011	9,846	-382.3	77,348***	7,323	6,114	0.042
	(20,566)	(0.0315)	(8,644)	(12,661)	(29,241)	(10,279)	(5,291)	(0.037)
Some college	9,504	0.108	9,831**	-326.6	64,962***	15,937**	6,941	0.001
	(8,394)	(0.105)	(4,681)	(5,341)	(18,186)	(6,550)	(6,019)	(0.020)
College grad	52,206*	0.006	26,482*	25,724	150,210***	43,579***	19,079***	0.005
	(30,268)	(0.029)	(14,104)	(16,612)	(51,651)	(14,689)	(3,490)	(0.022)
Sales $250,000-$499,999	-308,650***	-0.056***	-108,370***	-200,279***	-575,029***	-205,767***	-90,141***	0.033
	(62,531)	(0.018)	(24,931)	(38,131)	(116,130)	(48,451)	(17,115)	(0.075)
Sales $100,000-$249,999	-340,870***	-0.144***	-122,553***	-218,317***	-609,451***	-230,619***	-103,490***	-0.046*
	(60,305)	(0.045)	(24,607)	(36,181)	(106,856)	(44,840)	(16,132)	(0.027)
Sales $40,000-$99,999	-339,562***	-0.237**	-122,780***	-216,782***	-591,512***	-235,508***	-102,973***	-0.043
	(56,876)	(0.094)	(23,377)	(34,185)	(97,149)	(38,677)	(12,820)	(0.032)
Sales $20,000-$39,999	-321,988***	-0.338**	-116,440***	-205,548***	-574,379***	-236,929***	-99,331***	-0.051
	(55,883)	(0.131)	(23,029)	(33,566)	(95,608)	(36,498)	(11,147)	(0.034)
Sales $10,000-$19,999	-320,252***	0.407	-115,235***	-205,017***	-591,418***	-239,019***	-100,226***	-0.086**
	(56,670)	(0.688)	(22,981)	(34,494)	(98,335)	(37,674)	(12,094)	(0.033)
Sales $9,999 or less	-314,981***	-0.425**	-111,436***	-203,545***	-662,029***	-257,864***	-100,186***	-0.063*
	(54,562)	(0.177)	(21,199)	(33,835)	(104,933)	(43,140)	(13,162)	(0.036)
Acres operated	51.56***	6.71e-06***	19.27***	32.29***	94.27***	28.39***	12.36***	-2.00e-06
	(9.259)	(1.98e-06)	(3.605)	(5.940)	(36.29)	(5.639)	(3.304)	(1.85e-06)
Share acres owned	-5,786	-0.021**	-999.4	-4,786*	5,437	30,666*	-444.5	-0.028*
	(4,089)	(0.011)	(1,672)	(2,879)	(8,695)	(16,336)	(1,203)	(0.015)
Total off-farm income	0.047	-8.44e-08	0.022	0.025	0.158***	0.020	0.004	8.74e-08
	(0.060)	(8.60e-08)	(0.018)	(0.043)	(0.039)	(0.024)	(0.003)	(7.22e-08)
Percent cropland	-1,863**	-4.32e-05	-702.3**	-1,161**	-3,682**	-853.7**	-445.4*	-1.26e-05
	(810.2)	(0.000)	(295.2)	(521.9)	(1,827)	(345.1)	(251.0)	(7.41e-05)
Operator age	-2,201***	-0.007***	-619.0**	-1,582***	-44.65	-1,771***	-714.6***	-0.003
	(635.0)	(0.001)	(241.7)	(406.2)	(573.0)	(474.2)	(192.0)	(0.002)
Wheat	-145,242***	-0.099*	-52,748***	-92,494**	-253,509**	-80,845**	-30,942*	0.018
	(55,075)	(0.057)	(18,590)	(36,615)	(113,054)	(32,649)	(15,974)	(0.020)
Corn	37,448**	0.126***	19,718***	17,730*	-14,224	28,591*	4,126	0.0445

	(17,979)	(0.020)	(7,527)	(10,674)	(19,787)	(15,046)	(5,213)	(0.039)
Soybean	-6,984	0.016	2,791	-9,775	-15,913	9,216	-416.5	-0.026*
	(8,290)	(0.032)	(4,375)	(8,079)	(19,995)	(8,413)	(3,622)	(0.016)
Sorghum	-77,178**	-0.112	-28,179**	-48,999**	-137,826*	-44,249**	-8,687	0.035
	(34,593)	(0.110)	(12,709)	(22,008)	(82,137)	(21,404)	(16,484)	(0.048)
Rice	57,907	0.240***	12,993	44,914	-146,020	-72,297**	-19,598	-0.224
	(49,868)	(0.062)	(23,409)	(32,715)	(106,642)	(31,807)	(13,740)	(0.172)
Tobacco	47,483*	0.066	-5,178	52,661***	210,111***	18,409	12,428	0.002
	(24,367)	(0.126)	(11,521)	(15,450)	(61,975)	(16,717)	(8,649)	(0.039)
Cotton	32,418	0.217***	-16,340	48,758***	-68,603	-68,904***	-12,496	0.398
	(22,648)	(0.070)	(12,410)	(11,363)	(61,637)	(18,707)	(12,052)	(0.318)
Peanut	54,618***	0.310***	-3,545	58,162***	14,692	-25,904	-4,338	0.121
	(17,288)	(0.118)	(9,244)	(19,518)	(47,165)	(34,115)	(11,777)	(0.082)
Other crops	27,976*	0.387	3,372	24,604**	7,036	-21,844	-1,458	0.0352*
	(16,828)	(0.356)	(6,719)	(10,789)	(56,818)	(18,452)	(8,577)	(0.018)
Fruit	-42,606	0.180	-2,995	-39,611	5,754	114,901	-38,777	-0.003
	(61,684)	(0.192)	(32,996)	(29,859)	(140,295)	(76,909)	(26,808)	(0.0276)
Vegetable	433,262**	0.0173	171,200	262,062**	1.060e+06**	33,784	71,042	0.084***
	(215,915)	(0.080)	(104,448)	(114,752)	(505,332)	(31,310)	(46,322)	(0.027)
Nursery	31,006	-0.209***	22,390	8,616	955,232**	90,619	57,779***	0.167
	(70,861)	(0.030)	(23,756)	(49,468)	(409,011)	(57,164)	(21,296)	(0.116)
Cattle	84,689**	0.083**	58,957**	25,731	132,273*	6,197	6,625	0.037*
	(39,296)	(0.037)	(23,952)	(18,525)	(69,615)	(12,917)	(6,345)	(0.020)
Hogs	-80,110***	-0.011	5,028	-85,138***	45,403	70,797***	4,340	0.056*
	(28,965)	(0.039)	(9,839)	(21,931)	(54,757)	(18,406)	(7,535)	(0.030)
Poultry	-169,495***	0.010	-31,482	-138,013***	-186,948*	91,297***	-20,293	0.0992*
	(55,699)	(0.055)	(23,398)	(35,816)	(112,102)	(23,396)	(16,004)	(0.054)
Dairy	16,814	-0.263***	51,817	-35,003	789,758*	355,875**	148,884**	0.062**
	(109,075)	(0.071)	(52,289)	(58,686)	(402,973)	(168,365)	(66,071)	(0.031)
Other livestock	-24,289	-0.070	4,406	-28,695	124,578**	41,826*	11,286	-0.002
	(26,177)	(0.051)	(7,478)	(20,321)	(56,234)	(25,300)	(8,744)	(0.012)
Nonfarm employment	-40,381**	0.187	-10,257**	-30,124**	-15,752	-7,700	-5,568	0.022
	(16,905)	(0.172)	(4,960)	(12,474)	(27,397)	(8,403)	(4,802)	(0.015)
Not in workforce	-9,400	-0.184	484.5	-9,885*	20,407	-2,462	3,933	0.020
	(7,719)	(0.174)	(5,125)	(5,293)	(32,088)	(12,017)	(5,212)	(0.021)
Occupation other	70,157	-0.511	36,859	33,298	81,340	41,319	34,190	-0.018
	(62,886)	(0.391)	(28,125)	(35,501)	(90,993)	(44,291)	(27,303)	(0.032)
Retired from farming	-9,116	0.346	-9,464**	348.3	-2,937	-22,782**	3,007	-0.035
	(10,227)	(0.430)	(4,576)	(7,248)	(13,731)	(9,831)	(6,666)	(0.032)

Female	-52,619***	-0.145*	-12,138	-40,481**	-61,960***	-39,406*	-8,507*	0.020
	(19,689)	(0.074)	(10,784)	(16,000)	(12,713)	(21,732)	(4,863)	(0.019)
Nonwhite or Hispanic	-5,039	0.047	15,153	-20,192***	-8,939	19,057	453.1	-0.003
	(13,832)	(0.069)	(10,556)	(5,774)	(21,662)	(13,093)	(7,498)	(0.035)
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	383,098***	0.863***	107,132***	275,966***	370,799**	271,415***	109,059***	0.204
	(69,223)	(0.069)	(29,988)	(44,765)	(151,019)	(21,178)	(10,026)	(0.259)
Observations	85,998	85,825	85,998	85,998	85,998	85,998	85,998	85,983
R^2	0.124	0.003	0.057	0.117	0.201	0.069	0.055	0.002

Standard errors robust to correlation at the ARMS strata level in parentheses

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

Table B.6: Full results: Pooled OLS with panel and premium paid

	(1) totalshort	(2) financed	(3) dshort	(4) repaid	(5) evtot	(6) dreale	(7) dnreale	(8) leverage
Premium paid per acre	4,603***	0.002***	1,762**	3,061***	3,123**	2,829	605.4**	0.000
	(1,626)	(0.001)	(756.4)	(1,008)	(1,233)	(2,178)	(281.0)	(0.000)
Acres operated	36.31***	3.91e-06**	14.31***	22.33***	62.41***	10.97	6.618**	1.53e-06
	(11.35)	(1.73e-06)	(3.643)	(8.366)	(18.02)	(7.975)	(2.843)	(1.03e-06)
Operator age	23,972***	0.006*	10,641***	13,563***	33,390***	6,749	4,296*	-0.005
	(5,037)	(0.004)	(2,400)	(4,362)	(11,583)	(8,566)	(2,591)	(0.006)
(Operator age)^2	-250.1***	-0.000***	-107.5***	-146.1***	-349.0***	-91.09	-53.74**	7.11e-06
	(44.68)	(3.29e-05)	(21.46)	(38.10)	(103.3)	(73.87)	(22.88)	(4.43e-05)
Soybean share	-133,875***	0.166***	-92,709***	-46,242**	-399,941***	-187,594***	-47,229***	-0.010**
	(30,970)	(0.034)	(18,066)	(21,643)	(47,227)	(28,994)	(12,366)	(0.048)
Corn share	371,026***	0.130***	135,431***	237,972***	61,765	30,323	45,875**	0.088**
	(52,323)	(0.037)	(29,562)	(38,575)	(132,191)	(49,286)	(19,241)	(0.042)
Wheat share	-120,252	-0.021	-69,741	-74,158*	-355,944***	-284,477***	-51,764*	-0.022
	(76,686)	(0.045)	(53,617)	(42,077)	(85,666)	(55,344)	(26,414)	(0.035)
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	-541,973***	0.506***	-248,863***	-297,922**	-745,386**	-16,284	-92,738	0.378**
	(142,728)	(0.113)	(67,544)	(120,671)	(313,953)	(233,686)	(71,469)	(0.164)
Observations	27,921	27,888	27,921	27,921	27,921	27,921	27,921	27,290
Number of farms	12,668	12,664	12,668	12,668	12,668	12,668	12,668	12,668

[a] Standard errors robust to correlation at the farm level in parentheses
[b] *** p<0.01, ** p<0.05, * p<0.1

Table B.7: Full results: Pooled OLS with panel and acres enrolled dummy

	(1) totalshort	(2) financed	(3) dshort	(4) repaid	(5) evtot	(6) dreale	(7) dnreale	(8) leverage
Acres enrolled dummy	115,785***	0.180***	30,841**	83,193***	-26,722	14,729	-7,075	-0.001
	(26,962)	(0.015)	(14,345)	(20,059)	(28,071)	(25,886)	(10,965)	(0.036)
Acres operated	28.11***	2.80e-06*	11.45***	16.27**	50.44***	13.02***	3.431**	1.41e-06
	(9.392)	(1.58e-06)	(3.619)	(6.421)	(15.90)	(3.986)	(1.625)	(1.02e-06)
Operator age	15,639***	0.00671*	9,155***	8,386**	20,226	3,870	7,496***	-0.006
	(5,222)	(0.004)	(3,036)	(3,951)	(15,617)	(7,162)	(2,112)	(0.007)
(Operator age)^2	-175.2***	-0.000***	-94.80***	-99.13***	-216.7	-55.22	-82.25***	1.69e-05
	(46.40)	(3.23e-05)	(27.29)	(34.42)	(150.1)	(63.74)	(19.09)	(5.54e-05)
Soybean share	-168,943***	0.165***	-110,882***	-60,971**	-366,436***	-215,445***	-54,136***	-0.121*
	(36,735)	(0.037)	(22,298)	(24,212)	(51,023)	(36,202)	(13,887)	(0.063)
Corn share	387,818***	0.160***	136,567***	255,263***	-11,805	83,695	32,996	0.082
	(58,407)	(0.037)	(32,461)	(42,156)	(86,901)	(55,264)	(20,758)	(0.055)
Wheat share	-173,992*	-0.046	-121,943*	-94,973*	-470,437***	-371,760***	-92,774***	-0.031
	(98,374)	(0.053)	(70,253)	(54,446)	(91,981)	(71,245)	(29,491)	(0.055)
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	-290,699*	0.475***	-180,921**	-147,505	-358,754	134,654	-150,344***	0.702***
	(150,287)	(0.114)	(86,650)	(110,932)	(392,927)	(193,564)	(57,868)	(0.223)
Observations	22,371	22,345	22,371	22,371	22,371	22,371	22,371	22,369
Number of farms	11,888	11,881	11,888	11,888	11,888	11,888	11,888	11,888

[a] Standard errors robust to correlation at the farm level in parentheses
[b] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.8: Full results: Pooled OLS and farm FE with panel and premium paid

	(1) totalshort	(2) financed	(3) dshort	(4) repaid	(5) evtot	(6) dreale	(7) dnreale	(8) leverage
Premium paid per acre	4,354**	-0.001	1,171	3,183***	6,533***	3,983	821.2	-3.31e-05
	(1,991)	(0.001)	(1,104)	(1,119)	(1,582)	(4,129)	(609.8)	(0.001)
Acres operated	46.59*	2.57e-06	16.40	30.19*	113.3***	-139.8	18.52***	9.30e-06
	(26.41)	(5.49e-06)	(14.05)	(15.51)	(19.72)	(143.2)	(6.210)	(6.19e-06)
Operator age	37,504***	-0.008	14,807**	22,697**	53,378***	19,389	2,227	-0.010
	(11,813)	(0.007)	(5,846)	(9,914)	(16,354)	(21,891)	(5,844)	(0.014)
(Operator age)^2	-265.0***	2.17e-05	-100.3**	-164.7**	-311.6**	-121.3	9.979	8.25e-05
	(98.07)	(5.77e-05)	(51.10)	(80.33)	(145.1)	(180.9)	(50.98)	(0.000)
Soybean share	-34,936	0.023	-37,512	2,576	-78,959	-33,039	-5,406	-0.122
	(45,976)	(0.056)	(30,788)	(35,402)	(62,497)	(39,920)	(23,184)	(0.082)
Corn share	189,921**	-0.224***	83,217**	106,704	286,663*	4,496	47,820	0.054
	(81,594)	(0.064)	(41,333)	(74,105)	(173,385)	(84,647)	(40,471)	(0.067)
Wheat share	80,722	-0.063	60,317	20,405	81,987	-140,124	47,498	0.053
	(139,534)	(0.069)	(95,419)	(70,822)	(98,868)	(115,161)	(35,059)	(0.264)
Farm FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	-954,774***	1.000***	-405,173***	-549,602*	-1.385e+06***	-9,171	-80,014	0.48
	(342,418)	(0.185)	(151,277)	(298,012)	(451,990)	(486,513)	(160,505)	(0.366)
Observations	27,921	27,888	27,921	27,921	27,921	27,921	27,921	27,920
R^2	0.009	0.003	0.004	0.006	0.050	0.043	0.005	0.004
Number of farms	12,668	12,664	12,668	12,668	12,668	12,668	12,668	12,668

[a] Standard errors robust to correlation at the farm level in parentheses
[b] *** p<0.01, ** p<0.05, * p<0.1

Table B.9: Full results: Pooled OLS and farm FE with panel and acres enrolled dummy

	(1) totalshort	(2) financed	(3) dshort	(4) repaid	(5) evtot	(6) dreale	(7) dnreale	(8) leverage
Acres enrolled dummy	92,126***	0.040*	39,934*	52,193**	73,153***	17,290	-399.5	0.025
	(35,385)	(0.022)	(22,626)	(21,831)	(27,620)	(37,343)	(17,353)	(0.070)
Acres operated	29.17	-6.61e-06	14.31	14.86	88.90***	-109.4	11.94***	1.22e-05
	(28.98)	(1.19e-05)	(16.77)	(14.72)	(18.22)	(99.07)	(3.688)	(8.98e-06)
Operator age	-1,680	-0.00381	-333.3	-1,346	5,240	-10,448	5,979	-0.0212
	(14,163)	(0.00770)	(9,313)	(11,829)	(26,965)	(25,109)	(6,839)	(0.0287)
(Operator age)^2	4.704	2.67e-05	12.12	-7.412	-51.52	86.35	-55.07	0.000
	(121.4)	(6.66e-05)	(79.22)	(98.16)	(251.5)	(212.8)	(60.39)	(0.000)
Soybean share	-118,294*	0.0745	-108,186**	-10,108	-156,899**	-26,888	-20,973	-0.199
	(69,830)	(0.0689)	(51,116)	(44,099)	(66,557)	(55,367)	(33,157)	(0.124)
Corn share	183,182	-0.149*	111,081*	72,101	30,207	-50,504	19,409	-0.0289
	(140,565)	(0.0828)	(59,165)	(125,405)	(116,486)	(111,470)	(48,737)	(0.0987)
Wheat share	186,901	-0.0852	137,943	48,958	117,105	-136,364	1,889	0.346
	(219,834)	(0.0908)	(159,445)	(107,806)	(120,280)	(125,690)	(84,840)	(0.481)
Farm FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	188,160	0.821***	32,513	155,647	217,547	915,046	-121,107	1.151
	(394,548)	(0.221)	(249,290)	(337,925)	(700,988)	(658,401)	(189,457)	(0.774)
Observations	22,371	22,345	22,371	22,371	22,371	22,371	22,371	22,369
R^2	0.010	0.007	0.003	0.010	0.051	0.030	0.007	0.004
Number of farms	11,888	11,881	11,888	11,888	11,888	11,888	11,888	11,888

[a] Standard errors robust to correlation at the farm level in parentheses
[b] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.10: Full results: 2SLS with panel and county FE

	(1) Δ totalshort	(2) Δ financed	(3) Δ dshort	(4) Δ repaid	(5) Δ evtot	(6) Δ dreale	(7) Δ dnreale	(8) Δ leverage
Δ ln Premium paid per acre	28,539*	-0.013	14,941*	13,598	26,912***	-8,592	-6,384	-0.023
	(14,713)	(0.010)	(8,740)	(9,858)	(8,752)	(8,745)	(9,684)	(0.014)
Acres operated	8.751	2.37e-07	1.601	7.150*	38.39***	11.64***	9.229**	-5.27e-06**
	(7.047)	(1.95e-06)	(3.837)	(3.871)	(4.261)	(3.405)	(3.852)	(2.46e-06)
Operator age	6,918**	0.001	6,506***	412.3	3,501	938.0	-1,658	0.001
	(2,987)	(0.004)	(2,082)	(3,214)	(2,541)	(2,487)	(1,605)	(0.003)
(Operator age)^2	-76.27***	-1.28e-05	-61.53***	-14.74	-42.40*	-14.53	11.23	-1.01e-05
	(27.55)	(3.60e-05)	(18.75)	(28.30)	(23.74)	(22.62)	(14.40)	(2.55e-05)
Soybean share	-64,160*	-0.027	-57,785***	-6,376	-145,367***	7,837	-10,070	-0.114*
	(34,033)	(0.036)	(19,773)	(28,036)	(29,767)	(25,916)	(15,002)	(0.062)
Corn share	82,486*	-0.041	50,859*	31,627	55,770	30,509	36,409*	0.0307
	(43,846)	(0.0414)	(28,027)	(32,710)	(41,567)	(35,630)	(20,142)	(0.041)
Wheat share	-88,076	-0.092	-54,283	-33,793	52,247*	-12,097	62,253*	0.062
	(58,384)	(0.057)	(37,226)	(32,295)	(31,396)	(28,552)	(32,180)	(0.041)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	-86,145	0.279	-58,405	-27,739	17,622	275,654*	98,772**	0.096
	(97,750)	(0.297)	(63,750)	(84,483)	(82,575)	(154,913)	(49,730)	(0.091)
Observations	16,504	16,503	16,504	16,504	16,504	16,504	16,504	16,504
R^2	0.082	0.089	0.119	0.044	0.135	0.088	0.070	0.022

[a] Standard errors robust to correlation at the farm level in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Dependent variables calculated as the difference between the current year and the next most recent year reported, regardless of whether that year was immediately preceding the current year.

Table B.11: Full results: 2SLS with panel and state FE

	(1) Δ totalshort	(2) Δ financed	(3) Δ dshort	(4) Δ repaid	(5) Δ evtot	(6) Δ dreale	(7) Δ dnreale	(8) Δ leverage
Δ ln Premium paid per acre	40,884***	-0.014	24,334***	16,550	28,695***	-20,429**	-3,008	-0.019
	(14,156)	(0.012)	(7,310)	(10,377)	(9,990)	(10,337)	(8,278)	(0.018)
Acres operated	8.167	6.70e-07	1.945	6.222**	32.38***	10.50***	8.577***	-4.28e-06**
	(5.827)	(1.75e-06)	(3.332)	(3.135)	(3.760)	(3.146)	(3.128)	(2.12e-06)
Operator age	6,541**	-0.001	6,700***	-159.8	4,954*	980.2	-1,145	-0.000
	(2,894)	(0.004)	(2,373)	(2,743)	(2,643)	(2,273)	(1,452)	(0.002)
(Operator age)^2	-75.23***	5.57e-06	-64.85***	-10.38	-57.22**	-18.08	5.835	-2.16e-06
	(26.53)	(3.33e-05)	(21.58)	(24.19)	(25.07)	(21.22)	(13.18)	(1.93e-05)
Soybean share	-56,258*	-0.029	-46,012**	-10,247	-79,589***	10,128	6,374	-0.0578
	(28,890)	(0.032)	(18,489)	(21,992)	(27,848)	(20,347)	(13,038)	(0.043)
Corn share	100,143**	-0.063*	74,489**	25,654	136,006***	43,456	39,596**	0.021
	(44,190)	(0.033)	(30,734)	(29,087)	(37,240)	(27,322)	(18,125)	(0.031)
Wheat share	1,127	-0.035	44,503	-43,375	8,124	-21,159	40,562	0.061
	(71,895)	(0.047)	(64,491)	(27,162)	(28,837)	(24,000)	(26,511)	(0.043)
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	-138,134	0.0508	-72,017	-66,117	-33,392	111,591	134,669***	0.145**
	(85,737)	(0.115)	(67,087)	(70,287)	(73,725)	(69,215)	(50,204)	(0.070)
Observations	16,504	16,503	16,504	16,504	16,504	16,504	16,504	16,504
R^2	0.008	0.005	0.012	0.006	0.067	0.017	0.011	0.005

[a] Standard errors robust to correlation at the farm level in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Dependent variables calculated as the difference between the current year and the next most recent year reported, regardless of whether that year was immediately preceding the current year.

Table B.12: Full results: OLS with coverage rate and 2014 cross section

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	totalshort	financed	dshort	repaid	leverage	denied	deterfromcredit	creditprob
Coverage level[a]	4,255***	0.00672***	2,037**	2,218**	0.00171	0.00108	3.54e-06	0.00107
	(1,439)	(0.00249)	(883.1)	(1,003)	(0.00131)	(0.000924)	(0.000497)	(0.000953)
Attitude toward risk	20,343***	0.0203***	7,631***	12,712***	-0.000417	0.0217***	-0.00459**	0.0176***
	(3,979)	(0.00775)	(2,803)	(2,432)	(0.00420)	(0.00280)	(0.00178)	(0.00299)
Share acres under RP	2,056	-0.0336	-3,927	5,984	0.0425*	0.0148	-0.00290	0.0124
	(23,835)	(0.0453)	(16,983)	(16,153)	(0.0244)	(0.0171)	(0.0109)	(0.0184)
Insured commodity:								
Soybeans	189,683*	-0.0359	137,889	51,795	-0.0435	0.0311	-0.0689***	-0.0320
	(110,943)	(0.0849)	(102,588)	(41,993)	(0.0270)	(0.0485)	(0.0171)	(0.0492)
Wheat	-1,924	-0.00386	870.0	-2,794	-0.0261	-0.0147	-0.0242	-0.0390
	(34,023)	(0.0663)	(23,882)	(17,888)	(0.0270)	(0.0250)	(0.0147)	(0.0264)
Cotton/Generic	-10,079	0.00393	5,099	-15,177	-0.0275	0.0163	-0.0194	-0.00170
	(45,991)	(0.0727)	(29,473)	(26,506)	(0.0318)	(0.0294)	(0.0163)	(0.0307)
Rice	21,171	-0.0537	-56,006	77,176	0.0681	0.145***	-0.0138	0.128**
	(94,772)	(0.115)	(42,243)	(79,912)	(0.0845)	(0.0526)	(0.0295)	(0.0538)
All other	34,197	0.0667	-153,332**	187,528*	0.364	0.0969	-0.0744***	0.0230
	(122,590)	(0.116)	(67,154)	(102,741)	(0.384)	(0.0817)	(0.0228)	(0.0833)
High school grad	-15,370	0.0511	35,582*	-50,952	-0.00257	-0.153***	-0.0154	-0.168***
	(62,991)	(0.0714)	(20,926)	(54,849)	(0.0276)	(0.0429)	(0.0255)	(0.0426)
Some college	-27,014	0.0201	24,657	-51,671	-0.0267	-0.136***	-0.00513	-0.140***
	(62,796)	(0.0718)	(20,432)	(54,781)	(0.0274)	(0.0432)	(0.0258)	(0.0429)
College grad	-23,707	0.0339	39,747*	-63,453	-0.00183	-0.164***	-0.0180	-0.180***
	(65,807)	(0.0752)	(23,855)	(56,460)	(0.0294)	(0.0436)	(0.0259)	(0.0433)
$250,000-$499,999	-243,417***	-0.00389	-116,381***	-127,035***	-0.00531	-0.0285	0.00418	-0.0237
	(36,870)	(0.0377)	(17,934)	(25,897)	(0.0211)	(0.0197)	(0.00935)	(0.0200)
$100,000-$249,999	-235,358***	0.0318	-127,693***	-107,665***	-0.0257	-0.0807***	0.0228**	-0.0595***
	(47,004)	(0.0485)	(21,305)	(34,218)	(0.0325)	(0.0212)	(0.0112)	(0.0217)
$40,000-$99,999	-236,041***	0.0212	-133,709***	-102,331**	-0.0307	-0.108***	0.0371**	-0.0755***
	(55,360)	(0.0728)	(24,762)	(40,299)	(0.0644)	(0.0250)	(0.0148)	(0.0262)
$20,000-$39,999	-228,372***	-0.0807	-135,005***	-93,367**	-0.0908**	-0.202***	0.0251	-0.181***
	(61,144)	(0.123)	(27,300)	(44,150)	(0.0365)	(0.0342)	(0.0219)	(0.0370)
$10,000-$19,999	-189,757***	-0.102	-126,797***	-62,960	-0.150***	-0.220***	0.0713*	-0.155***
	(63,386)	(0.146)	(27,787)	(47,928)	(0.0318)	(0.0410)	(0.0396)	(0.0489)
$9,999 or less	-236,878***	0.0670	-148,920***	-87,958*	-0.194***	-0.234***	0.103**	-0.146***
	(69,461)	(0.357)	(32,148)	(51,393)	(0.0331)	(0.0416)	(0.0458)	(0.0505)
Acres operated	90.15***	-6.46e-06	28.47***	61.68***	-3.27e-06	2.02e-06	7.86e-07	2.74e-06
	(26.62)	(5.43e-06)	(10.73)	(18.96)	(2.82e-06)	(3.51e-06)	(1.27e-06)	(3.40e-06)
Share acres owned	401.5	0.0192***	405.2	-3.695	-0.00347	-0.00188*	0.00360***	0.00172
	(830.7)	(0.00193)	(486.8)	(532.3)	(0.00299)	(0.00103)	(0.000388)	(0.00130)
Total off-farm income	0.233	2.18e-08	0.0942	0.139	-3.08e-08**	-7.06e-08***	5.51e-08**	-1.74e-08
	(0.173)	(5.83e-08)	(0.0712)	(0.133)	(1.31e-08)	(2.10e-08)	(2.73e-08)	(4.08e-08)
Percent cropland	348,698***	-0.203	146,074***	202,624***	0.133***	0.0268	-0.0172	0.0132
	(79,861)	(0.124)	(50,838)	(54,295)	(0.0414)	(0.0385)	(0.0221)	(0.0401)
Operator age	-2,509***	-0.00987***	-1,142**	-1,368**	-0.00583***	-0.00436***	-0.00119***	-0.00548***
	(799.1)	(0.00164)	(552.9)	(537.9)	(0.000758)	(0.000577)	(0.000291)	(0.000587)
Wheat	-163,434***	-0.0978	-59,211*	-104,223***	0.0129	-0.0719**	-0.0179	-0.0911**
	(49,669)	(0.0799)	(34,241)	(29,868)	(0.0293)	(0.0344)	(0.0184)	(0.0354)

Corn	29,374	0.0440	16,686	12,688	0.0372	-0.0152	0.0114	-0.00439
	(27,649)	(0.0445)	(17,190)	(19,263)	(0.0318)	(0.0222)	(0.0110)	(0.0227)
Soybeans	4,245	0.0788	-1,300	5,546	0.0204	-0.0191	0.00390	-0.0161
	(23,252)	(0.0550)	(15,285)	(16,255)	(0.0272)	(0.0226)	(0.0116)	(0.0233)
Sorghum	-142,574	0.191	-62,261	-80,313	-0.0561	0.127	0.0546	0.177**
	(97,165)	(0.220)	(55,215)	(71,993)	(0.0596)	(0.0885)	(0.0636)	(0.0860)
Rice	-402,735**	0.0133	14,430	-417,164***	-0.116	-0.260**	0.0750	-0.193*
	(189,123)	(0.143)	(109,017)	(148,531)	(0.331)	(0.106)	(0.0528)	(0.111)
Tobacco	284,559	-0.0352	59,901	224,658	0.0480	0.0167	0.0112	0.0287
	(226,017)	(0.130)	(80,972)	(200,154)	(0.0670)	(0.0941)	(0.0567)	(0.0992)
Cotton	-44,082	0.179	30,049	-74,132	-0.109	0.00371	0.0118	0.0180
	(100,585)	(0.119)	(44,211)	(95,154)	(0.0794)	(0.0568)	(0.0320)	(0.0571)
Peanut	-65,109	-0.113	-21,017	-44,093	-0.106	-0.0981	-0.0105	-0.113
	(83,609)	(0.146)	(43,111)	(72,817)	(0.0835)	(0.0965)	(0.0417)	(0.0999)
Other crops	23,241	0.0642	47,227	-23,986	-0.0368	0.0140	0.00410	0.0196
	(53,223)	(0.0774)	(41,173)	(32,655)	(0.0299)	(0.0351)	(0.0193)	(0.0363)
Fruit	-296,304	-0.311***	-124,375	-171,928*	-0.401**	-0.215*	-0.0513**	-0.259**
	(181,985)	(0.112)	(121,540)	(104,053)	(0.162)	(0.118)	(0.0222)	(0.119)
Vegetable	825,256***	0.638	227,744**	597,512**	-0.0113	0.111	-0.0199	0.0960
	(289,050)	(0.513)	(99,899)	(259,530)	(0.0439)	(0.0956)	(0.0517)	(0.0912)
Nursery	-103,246	-0.346***	-18,976	-84,270	0.0400	0.240	-0.0658***	0.181
	(204,272)	(0.133)	(131,507)	(96,742)	(0.0629)	(0.291)	(0.0240)	(0.294)
Cattle	186,391***	0.00964	109,009***	77,382**	0.0254	0.0206	-0.0234*	-0.000501
	(59,322)	(0.0628)	(39,319)	(34,383)	(0.0218)	(0.0298)	(0.0130)	(0.0305)
Hogs	-66,367	-0.0183	-56,902*	-9,465	0.0107	-0.0311	0.00414	-0.0258
	(57,258)	(0.152)	(33,108)	(40,222)	(0.0318)	(0.0515)	(0.0246)	(0.0525)
Poultry	-67,483	-0.0951	-49,255	-18,228	0.103*	-0.000337	0.0101	0.0101
	(71,614)	(0.120)	(46,133)	(45,172)	(0.0604)	(0.0731)	(0.0399)	(0.0762)
Dairy	4,619	-0.299***	36,378	-31,759	0.0357	0.0485	-0.00590	0.0440
	(66,863)	(0.0558)	(55,505)	(30,690)	(0.0394)	(0.0376)	(0.0190)	(0.0386)
Other livestock	123,866	0.0163	-11,376	135,242	0.0140	-0.0365	-0.0347	-0.0659
	(130,487)	(0.144)	(59,939)	(119,475)	(0.0610)	(0.0704)	(0.0275)	(0.0709)
Non-farm employment	-28,717	0.0400	148.8	-28,866**	0.0934*	-0.00118	-0.0182	-0.0151
	(22,060)	(0.0603)	(16,513)	(12,629)	(0.0493)	(0.0209)	(0.0119)	(0.0219)
Not in workforce	20,265	0.166	3,222	17,044	-0.0244	-0.0488	-0.0750***	-0.115***
	(37,496)	(0.228)	(18,392)	(30,556)	(0.0243)	(0.0424)	(0.0207)	(0.0444)
Number of operators	40,106*	-0.0266*	23,330**	16,776	-0.00234	0.00562	0.00501	0.0104
	(20,521)	(0.0154)	(11,450)	(15,945)	(0.0101)	(0.00795)	(0.00634)	(0.00797)
Retired from farming	-25,338	-0.117	-32,847**	7,509	-0.0245	-0.0148	0.0285	0.00946
	(31,627)	(0.0976)	(14,620)	(27,172)	(0.0246)	(0.0349)	(0.0255)	(0.0398)
Female	-90,731	-0.0377	-37,998	-52,732	0.00583	-0.0508	-0.0246	-0.0726*
	(64,197)	(0.115)	(35,033)	(39,177)	(0.0442)	(0.0414)	(0.0212)	(0.0432)
Nonwhite or Hispanic	-83,807	-0.152	-39,879	-43,928	-0.0514	-0.0369	0.0420	0.00819
	(56,825)	(0.0953)	(31,983)	(37,393)	(0.0493)	(0.0639)	(0.0422)	(0.0663)
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	-394,113**	0.757**	-146,318	-247,795**	0.408***	0.471***	0.168***	0.629***
	(167,170)	(0.311)	(96,582)	(123,559)	(0.111)	(0.113)	(0.0604)	(0.115)
Observations	5,063	5,057	5,063	5,063	5,061	5,063	4,608	5,063
R^2	0.227	0.049	0.099	0.215	0.037	0.088	0.037	0.078

Robust standard errors in parentheses

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

[a] Coverage level is calculated as an acres-weighted average across all of an operation's covered crops.

Table B.13: Full summary statistics: Cross section

	Any Insurance			No Insurance			Difference
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	significant at:
Insurance acres dummy	64,991	0.95	0.21	26,180	-	-	
FCI premium paid per acre ($)	64,145	10.03	17.56	24,722	-	-	
Outcomes							
totalshort	86,989	$ 301,813.20	$ 936,834.50	36,133	$ 181,107.20	$ 1,035,899.00	***
financed	86,976	0.56	0.84	35,884	0.54	0.29	
dshort	86,989	$ 103,916.10	$ 480,045.50	36,133	$ 74,375.34	$ 544,592.50	***
repaid	86,989	$ 197,897.10	$ 673,003.30	36,133	$ 106,731.80	$ 782,117.70	***
dreale	86,989	$ 211,898.70	$ 841,170.00	36,133	$ 194,386.40	$ 1,013,182.00	***
dnreale	86,989	$ 87,799.53	$ 376,065.30	36,133	$ 77,372.48	$ 622,253.00	***
leverage	87,575	0.22	2.52	35,516	0.19	4.83	
drcu	87,589	$ 24,306.30	$ 477,690.90	35,533	$ 32,308.99	$ 675,207.50	**
Operator characteristics							
Operator age	86,989	54.34	11.85	36,133	56.75	12.46	***
Operator retired from farming	84,211	6.76%	25.97%	35,297	10.09%	30.18%	***
Principle operator is female	85,023	1.51%	12.19%	35,662	3.53%	18.45%	***
Principal operator is Hispanic or non-white	84,323	2.08%	14.26%	34,252	3.31%	17.90%	***
Total off-farm income	83,094	$ 51,218.25	$ 144,638.60	34,055	$ 57,936.95	$ 138,757.00	***
Operation characteristics							
Acres operated	86,989	1906.01	3642.47	36,133	1140.45	5436.16	***
Share of acres owned	86,989	0.44	0.72	36,133	0.81	2.99	***
Share of cropland operated	86,961	0.89	2.13	36,111	0.72	1.902	***
Operators' education is:							
Some high school	86,989	3.72%	18.93%	36,133	8.62%	28.06%	***
High school diploma	86,989	39.88%	48.97%	36,133	45.49%	49.80%	***
Some college	86,989	31.52%	46.46%	36,133	24.64%	43.09%	***
4-year college graduate and beyond	86,989	24.05%	42.74%	36,133	20.66%	40.49%	***
Other	86,989	0.83%	9.06%	36,133	0.59%	7.64%	***
Sales class							
$500,000+	86,989	45.21%	49.77%	36,133	34.90%	47.67%	***
$250,000-$499,000	86,989	20.62%	40.45%	36,133	13.60%	34.28%	***
$100,000-$249,000	86,989	19.05%	39.27%	36,133	17.07%	37.63%	***
$40,000-$99,999	86,989	10.01%	30.01%	36,133	13.68%	34.36%	***
$20,000-$39,000	86,989	3.18%	17.54%	36,133	7.81%	26.84%	***
$10,000-$19,000	86,989	1.27%	11.21%	36,133	5.13%	22.05%	***
$9,999 or less	86,989	0.67%	8.15%	36,133	7.80%	26.82%	***
Specalization							

General cash grain	86,989	17.54%	38.03%	36,133	9.77%	29.69%	***
Wheat	86,989	8.10%	27.29%	36,133	2.87%	16.69%	***
Corn	86,989	25.25%	43.45%	36,133	14.41%	35.12%	***
Soybeans	86,989	10.39%	30.51%	36,133	9.69%	29.59%	***
Sorghum	86,989	0.56%	7.45%	36,133	0.22%	4.73%	***
Rice	86,989	1.75%	13.12%	36,133	1.67%	12.82%	
Tobacco	86,989	1.55%	12.35%	36,133	0.43%	6.58%	***
Cotton	86,989	5.86%	23.48%	36,133	1.77%	13.18%	***
Peanut	86,989	0.77%	8.75%	36,133	0.28%	5.28%	***
Other crops	86,989	6.73%	25.05%	36,133	9.04%	28.67%	***
Fruit	86,989	2.04%	14.14%	36,133	2.95%	16.93%	***
Vegetable	86,989	1.62%	12.62%	36,133	2.42%	15.36%	***
Nursery	86,989	0.29%	5.40%	36,133	2.80%	16.51%	***
Cattle	86,989	7.04%	25.58%	36,133	13.90%	34.60%	***
Hogs	86,989	3.31%	17.90%	36,133	4.25%	20.16%	***
Poultry	86,989	1.14%	10.63%	36,133	7.70%	26.66%	***
Dairy	86,989	4.77%	21.32%	36,133	12.61%	33.20%	***
Other livestock	86,989	1.29%	11.27%	36,133	3.22%	17.64%	***
Operator occupation							
Work on farm	86,989	89.12%	31.14%	36,133	78.68%	40.96%	***
Off-farm employment	86,989	7.08%	25.66%	36,133	13.37%	34.03%	***
Not in workforce	86,989	3.17%	17.51%	36,133	6.81%	25.19%	***
Other occupation	86,989	0.63%	7.90%	36,133	1.14%	10.63%	***

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

Table B.14: Cross section summary stats and insurance status: 2004

	Insurance: 2004			No Insurance: 2004			Difference
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	significant at:
Insurance acres dummy	5,265	0.94	0.24	1,930	-	-	***
FCI premium paid per acre ($)	3,445	$ 5.39	$ 14.67	1,054	-	-	***
Outcomes							
totalshort	5,265	$ 205,681.10	$ 439,885.80	1,930	$ 114,516.80	$ 446,543.50	***
financed	5,265	0.63	0.89	1,928	0.37	1.35	***
dshort	5,265	$ 69,443.68	$ 228,785.80	1,930	$ 44,400.13	$ 211,619.40	***
repaid	5,265	$ 136,237.40	$ 303,400.80	1,930	$ 70,116.70	$ 279,588.20	***
dreale	5,265	$ 156,439.50	$ 485,934.50	1,930	$ 136,634.00	$ 440,978.70	
dnreale	5,265	$ 68,680.93	$ 293,015.50	1,930	$ 55,395.97	$ 337,157.60	
leverage	5,264	0.23	0.96	1,929	0.70	19.94	*
drcu	5,265	$ 15,809.00	$ 286,228.40	1,930	$ 6,028.16	$ 108,067.00	
Operator characteristics							
Operator age	5,265	52.53	11.46	1,930	54.02	12.61	***
Operator retired from farming	4,925	17.95%	38.38%	1,737	20.78%	40.59%	***
Principle operator is female	5,265	1.39%	11.69%	1,930	2.49%	15.58%	***
Principal operator is Hispanic or non-white	5,265	1.67%	12.82%	1,930	1.81%	13.35%	
Total off-farm income	5,098	$ 42,382.42	$ 84,686.21	1,839	$ 50,866.14	$ 116,238.00	***
Operation characteristics							
Acres operated	5,265	1774.88	2429.66	1,930	1191.02	8039.86	***
Share of acres owned	5,265	39.70%	34.29%	1,930	56.00%	37.67%	***
Share of cropland operated	5,265	85.41%	19.88%	1,930	76.32%	29.22%	***
Operators' education is:							
Some high school	5,265	4.54%	20.82%	1,930	8.60%	28.05%	***
High school diploma	5,265	37.74%	48.48%	1,930	43.42%	49.58%	***
Some college	5,265	32.90%	46.99%	1,930	26.17%	43.97%	***
4-year college graduate and beyond	5,265	20.99%	40.73%	1,930	17.98%	38.41%	***
Other	5,265	3.84%	19.21%	1,930	3.83%	19.21%	
Sales class							
$500,000+	5,265	31.95%	46.63%	1,930	28.86%	45.32%	**
$250,000-$499,000	5,265	25.17%	43.40%	1,930	19.38%	39.54%	***
$100,000-$249,000	5,265	26.02%	43.88%	1,930	20.62%	40.47%	***
$40,000-$99,999	5,265	10.86%	31.12%	1,930	12.80%	33.42%	**
$20,000-$39,000	5,265	3.65%	18.75%	1,930	7.25%	25.94%	***
$10,000-$19,000	5,265	1.65%	12.75%	1,930	4.72%	21.20%	***

$9,999 or less	5,265	0.70%	8.35%	1,930	6.37%	24.43%	***
Specialization							
General cash grain	5,265	16.77%	37.36%	1,930	11.19%	31.53%	***
Wheat	5,265	11.60%	32.03%	1,930	3.99%	19.58%	***
Corn	5,265	23.23%	42.23%	1,930	19.95%	39.97%	***
Soybeans	5,265	8.83%	28.38%	1,930	9.69%	29.59%	
Sorghum	5,265	0.38%	6.15%	1,930	0.21%	4.55%	
Rice	5,265	1.52%	12.23%	1,930	2.80%	16.50%	***
Tobacco	5,265	1.46%	12.01%	1,930	0.98%	9.88%	
Cotton	5,265	6.23%	24.17%	1,930	2.12%	14.42%	***
Peanut	5,265	1.92%	13.72%	1,930	0.47%	6.81%	***
Other crops	5,265	6.69%	24.98%	1,930	8.29%	27.58%	**
Fruit	5,265	0.93%	9.60%	1,930	1.30%	11.31%	
Vegetable	5,265	1.60%	12.53%	1,930	2.28%	14.93%	*
Nursery	5,265	0.09%	3.08%	1,930	1.30%	11.31%	***
Cattle	5,265	6.86%	25.27%	1,930	10.62%	30.82%	***
Hogs	5,265	5.83%	23.44%	1,930	5.85%	23.48%	
Poultry	5,265	1.03%	10.08%	1,930	5.18%	22.17%	***
Dairy	5,265	3.19%	17.58%	1,930	10.62%	30.82%	***
Other livestock	5,265	1.84%	13.45%	1,930	3.16%	17.50%	***
Operator occupation							
Work on farm	5,265	87.86%	32.66%	1,930	76.11%	42.65%	***
Off-farm employment	5,265	0.84%	9.10%	1,930	1.40%	11.75%	**
Not in workforce	5,265	8.07%	27.24%	1,930	15.44%	36.14%	***
Other occupation	5,265	3.23%	17.68%	1,930	7.05%	25.60%	***

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

Table B.15: Cross section summary stats and insurance status: 2013

	Insurance: 2013			No Insurance: 2013			Difference
	Obs.	**Mean**	**Std. Dev.**	**Obs.**	**Mean**	**Std. Dev.**	**significant at:**
Acres enrolled dummy	5,522	0.93	0.26	2,262	-	-	***
FCI premium paid per acre ($)	6,208	$ 13.91	$ 26.89	2,360	-	-	***
Outcomes							
totalshort	6,208	$ 320,276.40	$ 813,685.80	2,360	$ 142,711.00	$ 772,391.80	***
financed2	6,206	0.47	0.75	2,319	0.32	1.84	***
dshort	6,208	$ 110,944.40	$ 483,156.40	2,360	$ 61,239.11	$ 404,751.40	***
repaid	6,208	$ 209,332.10	$ 575,542.50	2,360	$ 81,471.93	$ 636,310.40	***
dreale	6,208	$ 239,089.80	$ 938,392.00	2,360	$ 187,112.80	$ 891,878.20	**
dnreale	6,208	$ 113,497.20	$ 347,710.30	2,360	$ 79,763.30	$ 829,324.80	***
leverage	6,208	0.18	0.49	2,360	0.13	0.49	***
drcu	6,208	$ 25,459.10	$ 251,051.20	2,360	$ 32,636.11	$ 493,381.60	
Operator characteristics							
Operator age	6,208	56.39	11.95	2,360	59.05	12.30	***
Operator retired from farming	6,208	3.88%	19.32%	2,360	8.31%	27.60%	***
Principle operator is female	6,208	1.79%	13.25%	2,360	4.70%	21.18%	***
Principal operator is Hispanic or non-white	5,706	4.42%	20.55%	2,080	6.92%	25.39%	***
Total off-farm income	5,914	$ 66,920.54	$ 216,749.70	2,207	$ 74,888.93	$ 167,810.60	
Operation characteristics							
Acres operated	6,208	1601.04	2690.11	2,360	922.15	3886.29	***
Share of acres owned	6,208	0.51	1.57	2,360	0.95	3.16	***
Share of cropland operated	6,208	0.85	0.21	2,360	0.60	0.38	***
Operators' education is:							
Some high school	6,208	2.30%	15.00%	2,360	7.12%	25.72%	***
High school diploma	6,208	38.56%	48.68%	2,360	43.14%	49.54%	***
Some college	6,208	31.38%	46.41%	2,360	25.25%	43.46%	***
4-year college graduate and beyond	6,208	27.75%	44.78%	2,360	24.49%	43.01%	***
Other	6,208	0.00%	0.00%	2,360	0.00%	0.00%	
Sales class							
$500,000+	6,208	53.98%	49.85%	2,360	33.56%	47.23%	***
$250,000-$499,000	6,208	17.24%	37.77%	2,360	10.30%	30.40%	***
$100,000-$249,000	6,208	15.45%	36.14%	2,360	15.04%	35.76%	
$40,000-$99,999	6,208	8.83%	28.37%	2,360	12.42%	32.98%	***
$20,000-$39,000	6,208	2.56%	15.80%	2,360	9.11%	28.78%	***
$10,000-$19,000	6,208	1.16%	10.71%	2,360	5.97%	23.71%	***

$9,999 or less	6,208	0.79%	8.85%	2,360	13.60%	34.29%	***
Specalization							
General cash grain	6,208	14.11%	34.82%	2,360	6.06%	23.86%	***
Wheat	6,208	4.45%	20.61%	2,360	2.16%	14.54%	***
Corn	6,208	34.31%	47.48%	2,360	14.92%	35.63%	***
Soybeans	6,208	9.49%	29.31%	2,360	9.32%	29.08%	
Sorghum	6,208	0.79%	8.85%	2,360	0.55%	7.40%	
Rice	6,208	5.33%	22.47%	2,360	3.26%	17.77%	***
Tobacco	6,208	0.98%	9.86%	2,360	0.25%	5.04%	***
Cotton	6,208	4.14%	19.92%	2,360	0.64%	7.95%	***
Peanut	6,208	2.06%	14.21%	2,360	0.47%	6.81%	***
Other crops	6,208	6.91%	25.37%	2,360	10.89%	31.16%	***
Fruit	6,208	2.96%	16.96%	2,360	4.58%	20.90%	***
Vegetable	6,208	0.71%	8.39%	2,360	2.03%	14.12%	***
Nursery	6,208	0.19%	4.39%	2,360	3.14%	17.43%	***
Cattle	6,208	5.61%	23.00%	2,360	17.88%	38.33%	***
Hogs	6,208	1.88%	13.60%	2,360	2.25%	14.82%	
Poultry	6,208	0.89%	9.37%	2,360	8.14%	27.34%	***
Dairy	6,208	4.32%	20.33%	2,360	9.66%	29.55%	***
Other livestock	6,208	0.87%	9.29%	2,360	3.81%	19.16%	***
Operator occupation							
Work on farm	6,208	87.64%	32.91%	2,360	72.80%	44.51%	***
Off-farm employment	6,208	11.07%	31.37%	2,360	21.40%	41.02%	***
Not in workforce	6,208	1.29%	11.28%	2,360	5.81%	23.39%	***
Other occupation	6,208	0.00%	0.00%	2,360	0.00%	0.00%	-

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

Table B.16: Panel variables: 2004 insurance status

	Insurance: 2004			No Insurance: 2004			Difference
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	significant at:
Acres enrolled dummy	1,276	0.90	0.30	405	-	-	***
FCI premium paid per acre ($)	1,152	$ 5.50	$ 6.11	325	-	-	***
Outcomes							
totalshort	1,276	$ 301,092.80	$ 601,463.20	405	$ 216,770.10	$ 829,916.30	**
financed	1,276	0.64	0.79	403	0.35	0.60	***
dshort	1,276	$ 99,217.43	$ 369,072.00	405	$ 83,152.85	$ 385,275.20	
repaid	1,276	$ 201,875.40	$ 381,406.00	405	$ 133,617.20	$ 512,412.10	***
dreale	1,276	$ 205,298.40	$ 493,907.30	405	$ 231,057.90	$ 574,637.20	
dnreale	1,276	$ 112,247.50	$ 509,708.90	405	$ 84,549.69	$ 294,102.60	
leverage	1,276	0.25	1.06	405	0.55	7.56	
drcu	1,276	$ 19,067.79	$ 258,033.80	405	$ 15,679.63	$ 209,479.10	
Operator characteristics							
Operator age	1,276	51.80	10.32	405	52.24	11.37	
Acres operated	1,276	2524.46	3387.27	405	2427.90	17238.97	
Soybeans share	1,276	23.75%	23.32%	405	13.74%	20.55%	***
Corn share	1,276	19.37%	21.30%	405	13.66%	20.49%	***
Wheat share	1,276	12.49%	19.00%	405	7.54%	15.79%	***

[a] *** p<0.01, ** p<0.05, * p <0.1

Table B.17: Panel variables: 2013 insurance status

	Insurance: 2013			No Insurance: 2013			Difference
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	significant at:
Acres enrolled dummy	1,402	0.923	0.267	446	-	-	***
FCI premium paid per acre ($)	1,720	$ 13.22	$ 15.95	487	-	-	***
Outcomes							
totalshort	1,720	$ 527,323.40	$ 1,146,400.00	487	$ 260,543.60	$ 858,653.30	***
financed	1,720	0.49	0.74	481	0.34	1.44	***
dshort	1,720	$ 162,459.70	$ 737,960.50	487	$ 143,961.80	$ 733,415.50	
repaid	1,720	$ 364,863.70	$ 831,502.30	487	$ 116,581.80	$ 434,392.00	***
dreale	1,720	$ 321,416.70	$ 1,269,756.00	487	$ 396,462.30	$ 1,556,267.00	
dnreale	1,720	$ 159,495.60	$ 486,030.80	487	$ 125,085.80	$ 657,773.10	
leverage	1,720	0.22	0.70	487	0.16	0.31	*
drcu	1,720	$ 39,891.46	$ 372,123.40	487	$ 97,750.66	$1,038,175.00	*
Operator characteristics							
Operator age	1,720	57.64	10.70	487	58.46	11.27	
Acres operated	1,720	2500.51	3703.80	487	1616.11	7307.54	***
Soybeans share	1,720	26.52%	25.79%	487	10.66%	20.05%	***
Corn share	1,720	20.04%	21.93%	487	8.19%	16.68%	***
Wheat share	1,720	8.91%	15.59%	487	3.56%	11.26%	***

[a] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.18: OLS relationship between IV (weberiv_op) and dependent variables

Dependent variable		Coefficient	Std. Err.	Obs.	R^2
(1)	repaid	-232.7	(1,785)	65,333	0.116
(2)	dshort	886.0	(1,930)	65,333	0.051
(3)	totalshort	653.3	(2,829)	65,333	0.123
(4)	financed	0.0190***	(0.00307)	65,208	0.024
(5)	evtot	-858.2	(3,346)	65,333	0.204
(6)	dreale	12,691***	(1,800)	65,333	0.068
(7)	dnreale	4,776***	(997.1)	65,333	0.057
(8)	leverage	-0.308	(0.298)	65,321	0.002

[a] Robust standard errors in parentheses.
[b] Includes controls for farm and operator characteristics, state FE, and year FE
[c] *** p<0.01, ** p<0.05, * p <0.1

APPENDIX C

CHAPTER 3 OF APPENDIX

C.1 Figures

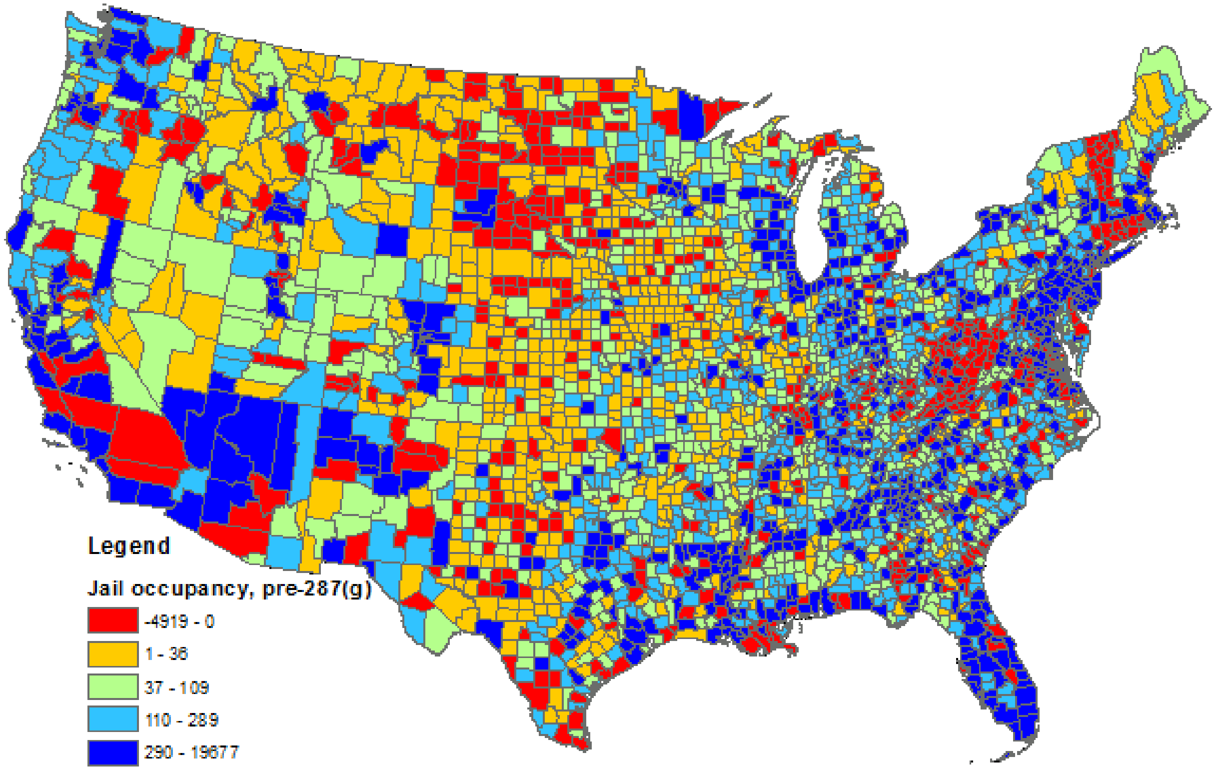

Figure C.1: U.S. jail occupancy, 2005-2006

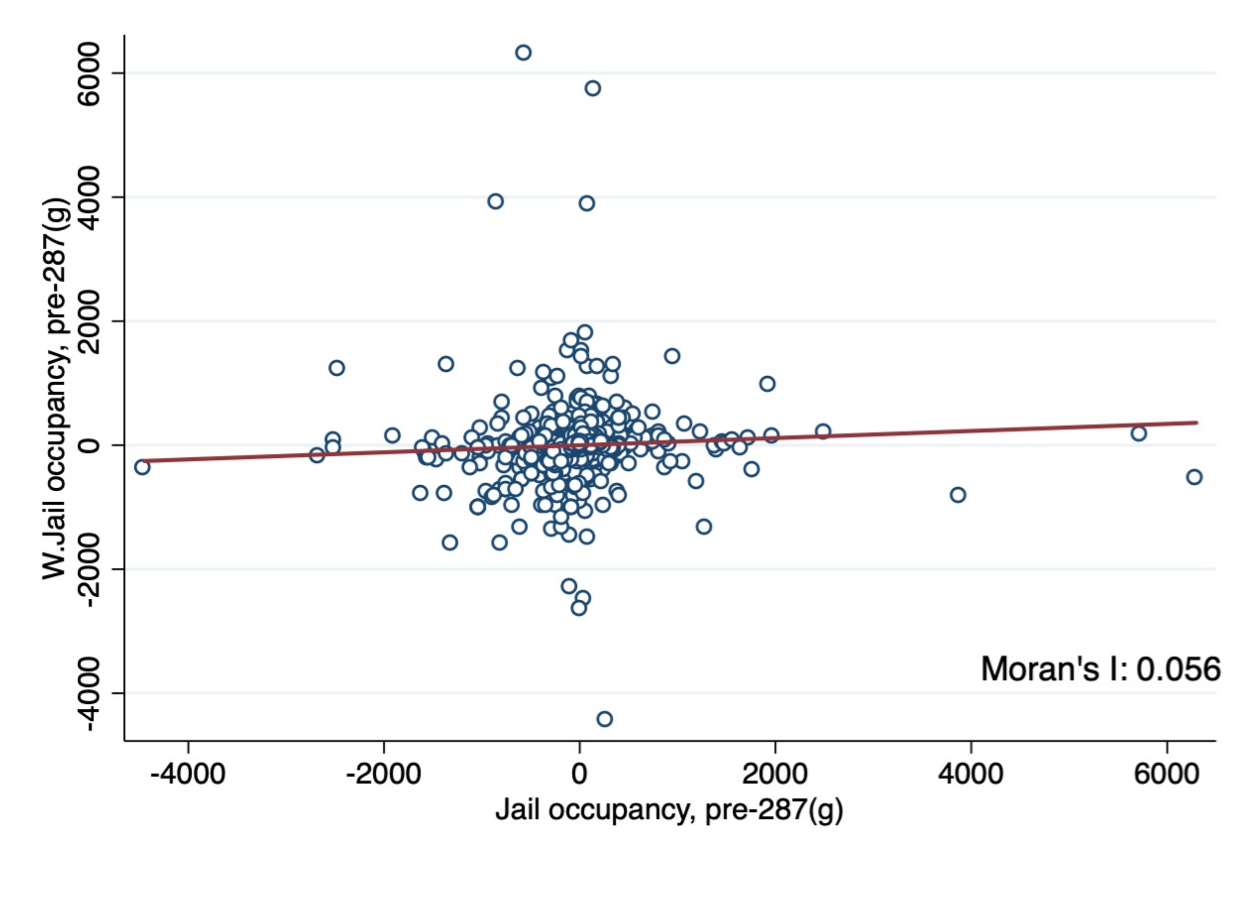

Figure C.2: Moran's I rejects spatial correlation for jail occupancy

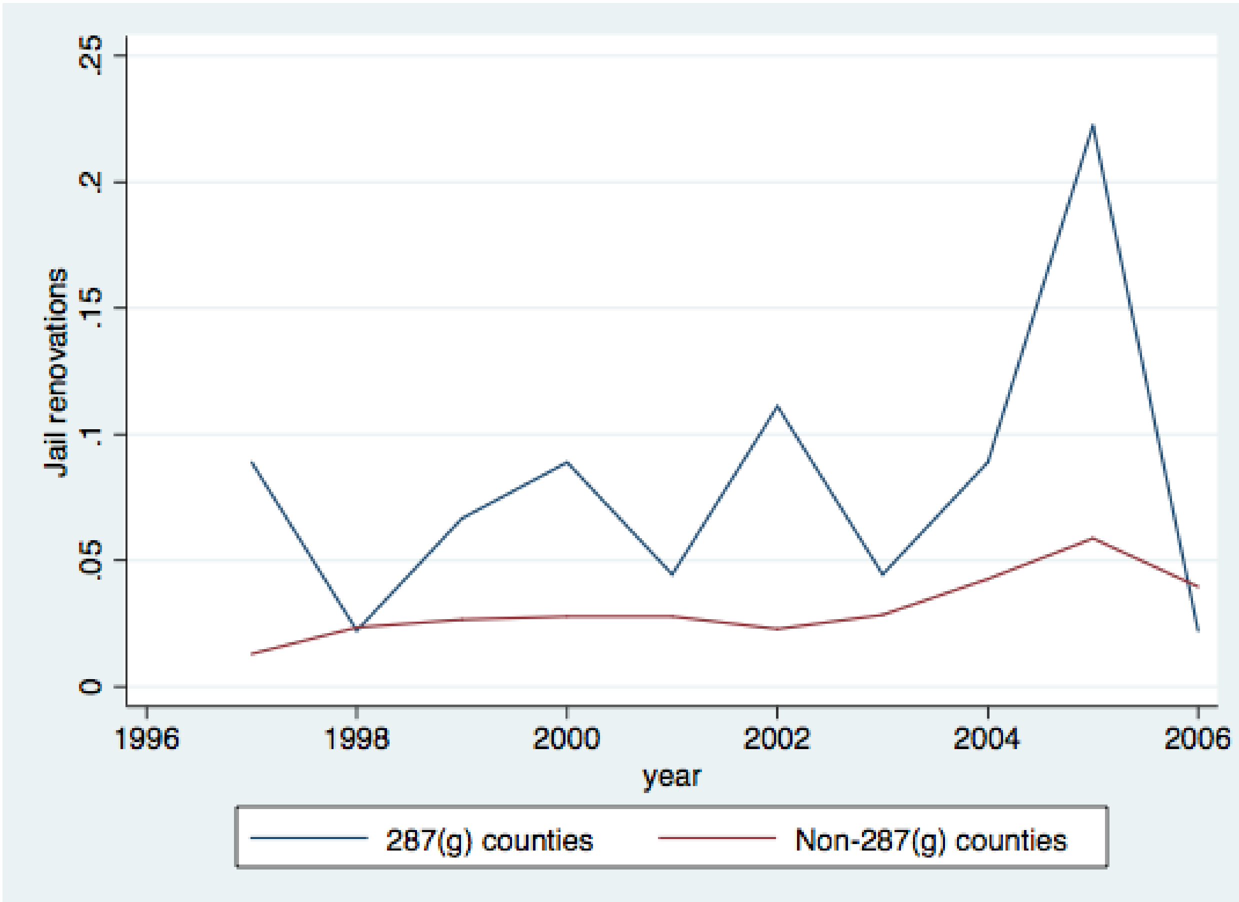

Figure C.3: 287(g) counties were investing more in local jails since 1996

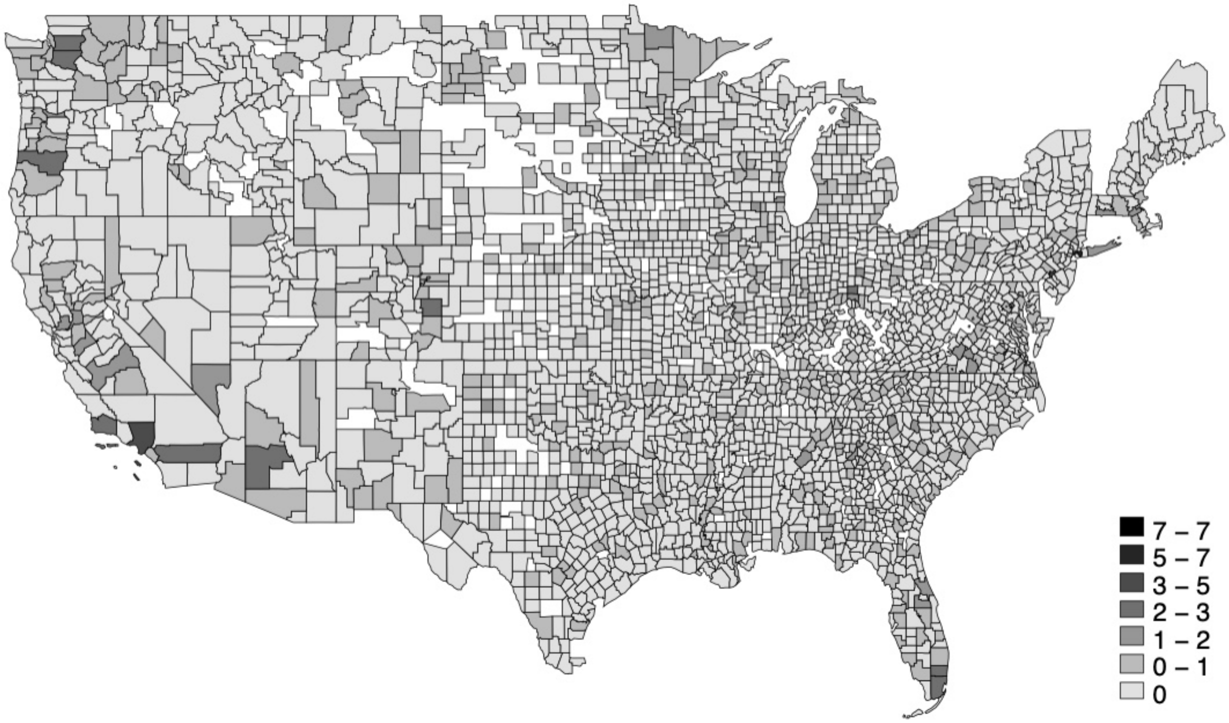

Figure C.4: U.S. jail renovations, 1997-2006

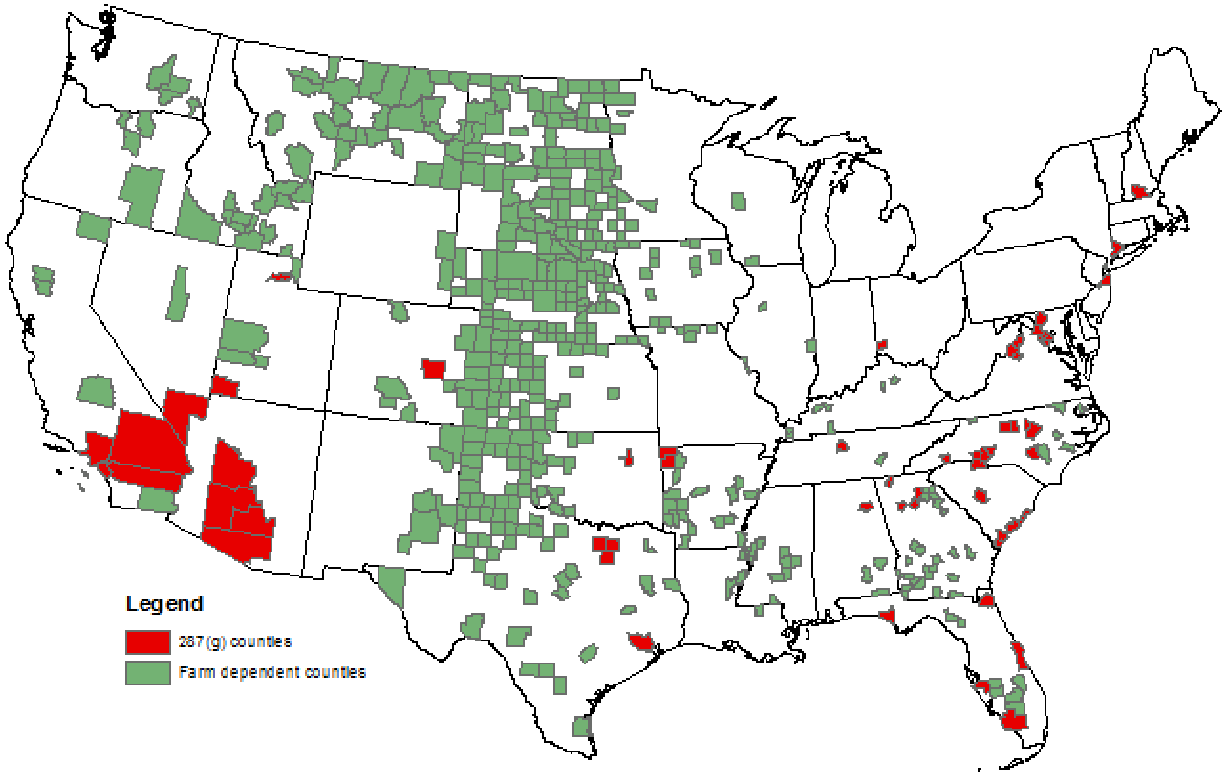

Figure C.5: Counties with the 287(g) program and those classified as agriculture-dependent

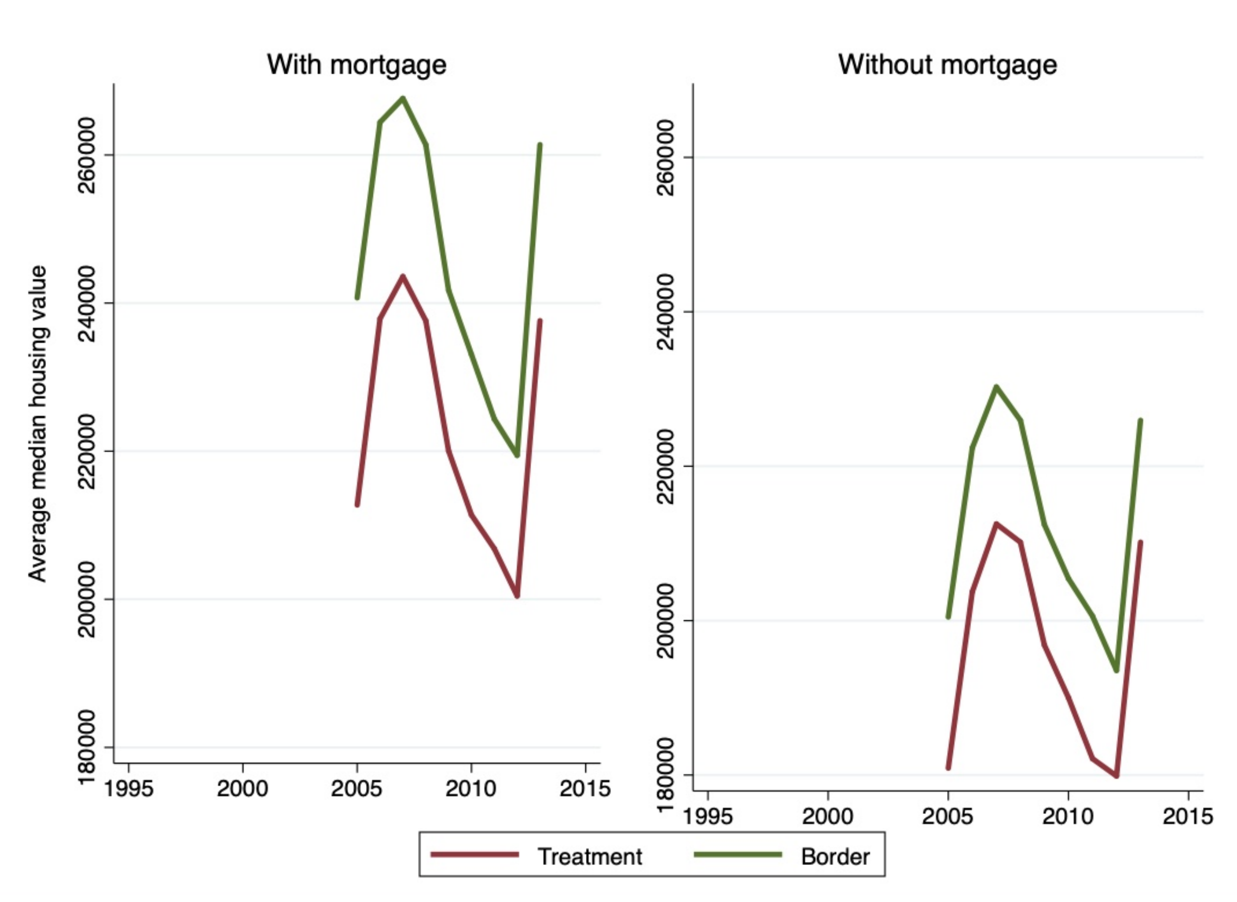

Figure C.6: Housing values followed a similar trend in program and border counties

C.2 Summary statistics

Table C.1: Summary statistics: 287(g) participation and jail occupancy

	287(g) counties			**Non-287(g) counties**		
	mean	SD	n	mean	SD	n
287(g) authorization: all years	33.5%	47.3%	212	0%	0%	5100
287(g) authorization: 1997	0%	0%	53	0%	0%	1275
287(g) authorization: 2002	0%	0%	53	0%	0%	1275
287(g) authorization: 2007	39.6%	49.4%	53	0%	0%	1275
287(g) authorization: 2012	94.3%	23.3%	53	0%	0%	1275
287(g) enforcement[a]	57.5%	49.7%	106	-	-	0
Year 287(g) program authorized[a]	2007.6	0.9	106	-	-	0
Aliens identified[a]	457.6	908.2	106	-	-	0
Aliens departed[a]	258.2	596.5	106	-	-	0
Jail occupancy pre 2007	-121.6	1006.9	212	-22.2	306.9	5100

[a] Conditional on having 287(g) program authorization
[b] Sample is restricted to counties in states that have a jurisdiction with a 287(g) program
[c] Source: ICE FOIA Proactive Disclosures; Bureau of Justice Statistics (BJS) National Jail Census

Table C.2: Summary statistics: County typology comparison between 287(g) and non-287(g) counties

	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 C.3: Summary statistics: Jail construction and renovation

	Neither 287(g) nor border counties			**287(g) counties**			**287(g) border counties**		
	mean	SD	n	mean	SD	n	mean	SD	n
Average year of jail construction	1980.2	23.3	2,812	1987.8***	10.4	140	1984.7***	15.8	312
Average year of jail renovation	1997.5	8.7	1,132	1999.0	5.9	94	1997.7	7.3	162
Number of jails constructed, 1997-2006	0.30	0.63	2,812	0.53***	0.72	140	0.34	0.51	312
Number of jails renovated, 1997-2006	0.28	0.50	1,132	0.81***	1.11	94	0.43***	0.71	162
Number of jails constructed, 2001-2006	0.14	0.45	2,812	0.25**	0.49	140	0.14	0.34	312
Number of jails renovated, 2001-2006	0.19	0.42	1,132	0.55***	0.87	94	0.25	0.53	162
Number of jails constructed, 2006	0.00	0.04	2,812	0***	0.00	140	0.01	0.10	312
Number of jails renovated, 2006	0.04	0.21	1,132	0.03***	0.16	94	0.05	0.25	162

[a] ***,**,*: Significantly different from "neither" counties at 1%, 5%, and 10%, respectively
[b] Sample is restricted to counties in states that have a jurisdiction with a 287(g) program
[c] Source: BJS National Jail Census, 2006

Table C.4: Summary statistics: Production outcomes by availability of jail renovation data

	No jail renovation data			**Jail renovation data**		
	mean	sd	n	mean	sd	n
Acres operated	103,917.28*	106,595.59	925	115,137.94	184,768.99	1,523
Number of farms	446.65***	342.59	945	487.65	419.73	1,560
Number of workers	786.58***	1,110.73	943	1,102.78	3,435.36	1,550
Net income	$11,969,042.83	$ 17,338,400.59	934	$ 13,445,655.04	$ 35,941,215.19	1,548
Asset value	$337,551,853.70***	$ 274,243,015.30	943	$ 401,138,188.10	$ 459,549,600.50	1,558

[a] ***,**,*Mean significantly different from mean of counties with jail renovation data at 1%, 5%, and 10%, respectively
[b] Sample is restricted to counties in states that have a jurisdiction with a 287(g) program
[c] Source: Bureau of Justice Statistics (BJS) National Jail Census, 2006

Table C.5: Summary statistics: Crime rate comparison between agriculture-dependent and non-dependent counties

	Non-farm dependent mean (sd)	**Farm dependent** mean (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 C.6: Summary statistics: Acres harvested, pre-287(g)

	All counties			287(g) counties			Non-287(g) counties		
	mean	SD	n	mean	SD	n	mean	SD	n
Vegetable acres [a]	28.8	334.8	11,730	24.5	217.3	439	29.0	338.6	11,291
Vegetable acres [b]	1,915.8	12,128.8	1,123	2,776.9	7,152.9	51	1,874.9	12,316.1	1,072
Fruit acres [a]	78.8	3,007.6	11,730	19.3	225.6	439	81.1	3,065.1	11,291
Mechanized acres [a]	545.6	1,091.5	11,730	286.8421***	847.2	439	555.6	1,098.7	11,291

[a] Outcome from ARMS
[b] Outcome from Census
[c] ***,**,* Significantly different from non-287(g) counties at 1%, 5%, and 10% respectively
[d] Source: US Census of Agriculture (NASS QuickStats); USDA Agricultural and Resource Management Survey (ARMS)

Table C.7: Summary statistics: Financial indicators, pre-287(g)

	All counties			**287(g) counties**			**Non-287(g) counties**		
	mean	SD	n	mean	SD	n	mean	SD	n
Machinery value (1000 \$) [a]	\$ 254	\$ 527	11,730	\$201**	\$ 363	439	\$ 256	\$ 533	11,291
Machinery val/op. (1000 \$) [b]	\$ 57	\$ 41	1,326	\$47**	\$ 27	53	\$ 58	\$ 42	1,273
Net income (1000 \$) [a]	\$ 210	\$ 1,200	11,730	\$298	\$ 1,023	439	\$ 207	\$ 1,206	11,291
Net income/county (1000 \$) [b]	\$ 13,969	\$ 44,599	1,314	\$29,264**	\$ 46,922	53	\$ 13,326	\$ 44,403	1,261
Real estate assets (1000 \$) [a]	\$ 2,126	\$ 10,500	11,730	\$2,683	\$ 7,455	439	\$ 2,105	\$ 10,600	11,291
Real estate assets/county (1000 \$) [b]	\$ 371,315	\$ 518,338	1,325	\$546,440**	\$ 570,682	53	\$ 364,019	\$ 514,996	1,272
Debt (ARMS) (1000 \$) [a]	\$ 361	\$ 1,463	11,730	\$329	\$ 1,099	439	\$ 362	\$ 1,476	11,291
Num. of farms/county [b]	494.4	474.2	1,328	664.5**	560.7	53	487.3	469.1	1,275

[a] Outcome from ARMS
[b] Outcome from Census
[c] ***,**,* Significantly different from non-287(g) counties at 1%, 5%, and 10% respectively
[d] Source: US Census of Agriculture (NASS QuickStats); USDA Agricultural and Resource Management Survey (ARMS)

C.3 Additional results

Table C.8: Reduced form estimates: impact of jail occupancy on production decisions

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
Jail occupancy, pre-2007	0.041	6,925***	-7.48***	55,911	-47.66	883,899	18.4**
	(0.042)	(2,076)	(3.422)	(72,720)	(41.16)	(1,244,284)	(8.07)
287(g) border	-54.03	-3,918	1,739	424,844	-17,259	1,535,385	33.8
	(78.56)	(3,013)	(3,424)	(595,012)	(27,805)	(5,222,764)	(50.1)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,403	5,090	21,403	5,246	21,403	3,607	3,914
Number of counties	-	1,317	-	1,324	-	1,305	1,322
Number of farms	11,501	-	11,501	-	11,501	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.9: 2SLS First stage: jail occupancy and renovations

	(1) ARMS	(2) ARMS	(3) Census	(4) Census
Jail occupancy, pre-2007	-9.24e-05***		-0.008***	
	(3.75e-06)		(0.002)	
Jail renovations, pre-2007		0.044***		0.030***
		(0.007)		(0.010)
Border county	-0.065***	-0.086***	-0.043***	-0.020***
	(0.006)	(0.008)	(0.009)	(0.005)
F-stat	43.02	43.42	12.58	10.00
σ_u	0.051	0.799	0.094	0.05
σ_e	0.142	0.157	0.109	0.071
ρ	0.362	0.354	0.428	0.327
Observations	21,403	14,591	3,914	10,659
Number of farms	11,753	7,858	-	-
Number of counties	-	-	1,322	2,737

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.10: Impact of 287(g) authorization on acreage decisions: OLS

	(1) Vegetable acres (ARMS)	(2) Vegetable acres (Census)	(3) Fruit acres (ARMS)	(4) Mechanized acres (ARMS)
287(g) authorization	7.706	-160	17.08	-58.37***
	(7.607)	(359)	(30.22)	(17.78)
287(g) border	14.81*	267	7.887	-14.89
	(7.385)	(215)	(19.09)	(10.52)
County FE	NO	YES	NO	NO
Farm FE	YES	NO	YES	YES
Year FE	YES	YES	YES	YES
Observations	21,839	4,345	21,839	21,839
Number of counties	-	1,299	-	-
Number of farms	11,753	-	11,753	11,753

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.11: Reduced form estimates: impact of jail occupancy on acreage decisions

	(1) Vegetable acres (ARMS)	(2) Vegetable acres (Census)	(3) Fruit acres (ARMS)	(4) Mechanized acres (ARMS)
Jail occupancy, pre-2007	-0.00319	49.5	-0.160	-0.00664
	(0.00361)	(33.2)	(0.184)	(0.00845)
287(g) border	14.32*	286	3.880	-13.67
	(7.350)	(219)	(14.40)	(10.17)
County FE	NO	YES	NO	NO
Farm FE	YES	NO	YES	YES
Year FE	YES	YES	YES	YES
Observations	21,403	4,319	21,403	21,403
Number of counties	-	1,295	-	-
Number of farms	11,501	-	11,501	11,501

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.12: Financial impact of 287(g) authorization: OLS

	(1) Machinery value (ARMS)	(2) Machinery value per operation (Census)	(3) Net income (ARMS)	(4) Net income per county (Census)	(5) Real estate asset values (ARMS)	(6) Real estate asset values per county (Census)	(7) Debt (ARMS)	(8) Number of farms per county (Census)
287(g) authorization	-10,540	-17,018***	65,947	-10,031,422**	-1.157e+06	7,568,635	288,640	-10.3
	(16,844)	(2,489)	(99,783)	(4,685,079)	(835,649)	(48,431,008)	(224,353)	(19.1)
287(g) border	-18,465	-8,018***	61,906	-1,398,168	-153,429	24,214,977	-60,393	.665
	(20,203)	(2,879)	(105,162)	(1,653,505)	(349,303)	(46,833,481)	(42,348)	(13)
County FE	NO	YES	NO	YES	NO	YES	NO	YES
Farm FE	YES	NO	YES	NO	YES	NO	YES	NO
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	21,839	5,308	21,839	3,765	21,839	5,306	21,839	5,312
Number of counties	-	1,328	-	1,325	-	1,328	-	1,328
Number of farms	11,753	-	11,753	-	11,753	-	11,753	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.13: Reduced form estimates: financial impact of jail occupancy

	(1) Machinery value (ARMS)	(2) Machinery value per operation (Census)	(3) Net income (ARMS)	(4) Net income per county (Census)	(5) Real estate asset values (ARMS)	(6) Real estate asset values per county (Census)	(7) Debt (ARMS)	(8) Number of farms per county (Census)
Jail occupancy, pre-2007	-18.83	4,623***	25.18	1,255,997***	406.0*	2,966,100	-78.27	5.84***
	(24.53)	(1,045)	(42.38)	(412,414)	(225.1)	(5,755,112)	(80.18)	(1.98)
287(g) border	-16,563	-6,414**	56,211	-644,260	-60,325	25,568,658	-79,002	2.33
	(21,478)	(2,886)	(97,143)	(1,685,653)	(382,817)	(47,062,064)	(57,600)	(13.2)
County FE	NO	YES	NO	YES	NO	YES	NO	YES
Farm FE	YES	NO	YES	NO	YES	NO	YES	NO
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	21,403	5,272	21,403	3,769	21,403	5,270	21,403	5,276
Number of counties	-	1,324	-	1,321	-	1,324	-	1,324
Number of farms	11,501	-	11,501	-	11,501	-	11,501	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.14: Impact of 287(g) authorization: 2SLS with jail occupancy and renovations IVs

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
287(g) authorization	289.4	-108,123**	152,368***	19,314,993*	121,573	117,739,539	-4,766**
	(365.0)	(50,123)	(50,034)	(10,276,604)	(189,905)	(108,872,010)	(1,958)
287(g) border		-9,316***		1,118,348*		6,394,987	-170*
		(3,402)		(652,334)		(6,382,467)	(93.5)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	14,571	5,042	14,571	5,196	14,571	3,575	3,880
Number of counties	-	1,303	-	1,310	-	1,291	1,308
Number of farms	7,846	-	7,846	-	7,846	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.15: Impact of 287(g) enforcement levels: Aliens departed, 2SLS

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
Aliens departed	-0.0917	-1,881**	16.62**	-16,588	105.9	-178,855	-4.47**
	(0.0948)	(874)	(6.993)	(22,294)	(84.87)	(258,551)	(1.95)
287(g) border	-60.61	-25,608***	2,931	243,155	-9,662	-645,894	-14.1
	(73.04)	(9,035)	(3,787)	(631,891)	(21,404)	(6,133,708)	(53.1)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,403	5,090	21,403	5,246	21,403	3,607	3,914
Number of counties	-	1,317	-	1,324	-	1,305	1,322
Number of farms	11,501	-	11,501	-	11,501	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.16: Impact of 287(g) authorization: counties with 287(g) programs in cities dropped, 2SLS

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
287(g) authorization	1,996	-856,174**	107,536	-9,642,789	-3.580e+06	-148,243,533	-2,177**
	(4,556)	(369,872)	(266,690)	(10,171,152)	(4.885e+06)	(165,041,273)	(1,110)
287(g) border	20.83	-28,041***	7,967	127,698	-203,659	-4,054,138	-37.4
	(290.8)	(10,693)	(15,680)	(661,757)	(262,809)	(7,702,578)	(59.2)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,208	5,074	21,208	5,230	21,208	3,592	3,896
Number of counties	-	1,317	-	1,324	-	1,305	1,322
Number of farms	11,398	-	11,398	-	11,398	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.17: Impact of 287(g) authorization: control for county population

	(1) Acres operated per county (Census)	(2) Fuel expenses ($) (Census)	(3) Labor expenses ($) (Census)	(4) Number of workers per county (Census)
287(g) authorization	-2,021,194	-6,185***	-50,097***	-12,509
	(1,371,159)	(1,696)	(15,302)	(9,700)
287(g) border	-71,388	-350,694	-3,797,425	-339
	(48,079)	(367,159)	(8,531,531)	(340)
County FE	YES	YES	YES	YES
Farm FE	NO	NO	NO	NO
Year FE	YES	YES	YES	YES
Observations	5,090	5,246	3,607	3,914
Number of counties	1,317	1,324	1,305	1,322
Number of farms	-	-	-	-

[a] Heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.18: Impact of 287(g) authorization: Treatment status randomly assigned to non-treated counties

	(1) Acres operated per county (Census)	(2) Fuel expenses ($) (Census)	(3) Labor expenses ($) (Census)	(4) Number of workers per county (Census)
(False) 287(g) authorization	-3,499,312	-118,944	297,393	20,454
	(649,446,096)	(1,041,580)	(2,535,457)	(175,233)
County FE	YES	YES	YES	YES
Farm FE	NO	NO	NO	NO
Year FE	YES	YES	YES	YES
Observations	5,090	5,246	3,607	3,914
Number of counties	1,317	1,324	1,305	1,322
Number of farms	-	-	-	-

[a] Heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] Results reflect boot-strapped estimates of coefficients and standard errors from 500 iterations of the main regression in which the start dates actual 287(g) programs were randomly assigned to non-treated counties.

[c] *** p<0.01, ** p<0.05, * p<0.1

[d] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.19: Impact of 287(g) authorization: Treated counties randomly dropped

	(1) Acres operated per county (Census)	(2) Fuel expenses ($) (Census)	(3) Labor expenses ($) (Census)	(4) Number of workers per county (Census)
287(g) authorization	-49,829**	21,500*	33.1	-7,670***
	(24,412)	(11,440)	(29)	(2,009)
287(g) border	512	-147***	4.59	465***
	(3,180)	(12.5)	(11.6)	(167)
County FE	YES	YES	YES	YES
Farm FE	NO	NO	NO	NO
Year FE	YES	YES	YES	YES
Observations	5,090	5,246	3,607	3,914
Number of counties	1,317	1,324	1,305	1,322
Number of farms	-	-	-	-

[a] Heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] Results reflect boot-strapped estimates of coefficients and standard errors from 100 iterations of the main regression in which 10% of the treated sample was randomly dropped in each iteration.

[c] *** p<0.01, ** p<0.05, * p<0.1

[d] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.20: Impact of 287(g) authorization: All counties in control, 2SLS

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
287(g) authorization	-585.0	-635,630**	70,043*	-27,637,626**	448,542	-289,375,153*	-4,782***
	(527.9)	(283,663)	(35,821)	(11,000,491)	(438,643)	(154,759,497)	(1,258)
287(g) border	-104.8***	-14,468**	1,305	-506,249	-6,279	-7,587,390	-92.1
	(34.17)	(6,313)	(4,152)	(588,571)	(15,626)	(6,254,038)	(57.9)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	44,875	10,665	44,875	10,954	44,875	7,624	8,166
Number of counties	-	2,756	-	2,767	-	2,731	2,764
Number of farms	24,299	-	24,299	-	24,299	-	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS Quick-Stats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.21: Impact of 287(g) authorization: 2SLS with ERS region clustered S.E.

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
287(g) authorization	-446.7	-747,784*	80,960***	-6,665,275	515,971***	-101,677,716	-2,177**
	(349.9)	(417,942)	(13,488)	(20,961,593)	(133,791)	(199,182,243)	(1,028)
287(g) border	-83.11	-32,684***	7,011	167,180	16,336**	-3,151,814	-60.9
	(92.66)	(7,785)	(4,987)	(1,245,256)	(7,549)	(10,526,073)	(40.8)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,403	5,090	21,403	5,246	21,403	3,607	3,914
Number of counties	-	1,317	-	1,324	-	1,305	1,322
Number of farms	11,501	-	11,501	-	11,501	-	-

[a] Standard errors robust to correlation at ERS resource region in parentheses
[b] *** $p<0.01$, ** $p<0.05$, * $p<0.1$
[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures
[d] Wild cluster bootstrap t-stat for 287(g) authorization for Census specifications: (2) -2.46, (4) -0.37, (6) -0.47, (7) -1.84

Table C.22: Impact of 287(g) authorization: 2SLS with State clustered S.E.

	(1) Acres operated per farm (ARMS)	(2) Acres operated per county (Census)	(3) Fuel expenses ($) (ARMS)	(4) Fuel expenses ($) (Census)	(5) Labor expenses ($) (ARMS)	(6) Labor expenses ($) (Census)	(7) Number of workers per county (Census)
287(g) authorization	-446.7	-747,784**	80,960*	-6,665,275	515,971	-101,677,716	-2,177*
	(458.3)	(312,645)	(47,667)	(18,304,657)	(661,954)	(220,948,273)	(1,217)
287(g) border	-83.11	-32,684***	7,011	167,180	16,336	-3,151,814	-60.9
	(89.84)	(11,339)	(4,416)	(978,288)	(25,436)	(11,343,150)	(57.6)
County FE	NO	YES	NO	YES	NO	YES	YES
Farm FE	YES	NO	YES	NO	YES	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,043	5,090	21,403	5,246	21,403	3,607	3,914
Number of counties	-	1,317	-	1,324	-	1,305	1,322
Number of farms	11,501	-	11,501	-	11,501	-	-

[a] Standard errors robust to correlation at the state level in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.23: Impact of 287(g) authorization: 2SLS with County cluster SE

	(1) Acres operated per county (Census)	(2) Fuel expenses ($) (Census)	(3) Labor expenses ($) (Census)	(4) Number of workers per county (Census)
287(g) authorization	-747,784**	-6,665,275	-101,677,716	-2,177**
	(305,888)	(8,829,000)	(144,124,166)	(998)
287(g) border	-32,684***	167,180	-3,151,814	-60.9
	(11,809)	(672,827)	(8,478,773)	(62.7)
County FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	5,090	5,246	3,607	3,914
Number of counties	1,317	1,324	1,305	1,322

[a] Standard errors robust to correlation at the county level in parentheses
[b] *** p<0.01, ** p<0.05, * p<0.1
[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.24: Impact of 287(g) authorization: 2SLS with Spatial SE

	(1) Acres operated per county (Census)	(2) Fuel expenses ($) (Census)	(3) Labor expenses ($) (Census)	(4) Number of workers per county (Census)
287(g) authorization	-839,268***	-6,775,750	-107,117,152	-2,224**
	(185,370)	(8,276,344)	(147,596,757)	(968)
287(g) border	-36,352***	162,994	-2,604,170	-52.2
	(7,609)	(482,511)	(7,290,862)	(56.7)
County FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	5,090	5,246	3,607	3,914
Number of counties	1,317	1,324	1,305	1,322

[a] Spatial standard errors robust to correlation within 300 miles of the country centroid in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.25: Impact of 287(g) authorization on farm acreage decisions: 2SLS

	(1) Vegetable acres (ARMS)	(2) Vegetable acres (Census)	(3) Fruit acres (ARMS)	(4) Mechanized acres (ARMS)
287(g) authorization	34.58	-5,461	1,735	71.91
	(41.91)	(3,795)	(1,975)	(95.03)
287(g) border	16.57**	14.6	116.9	-8.984
	(7.352)	(226)	(138.7)	(11.45)
County FE	NO	YES	NO	NO
Farm FE	YES	NO	YES	YES
Year FE	YES	YES	YES	YES
Observations	21,403	4,319	21,403	21,403
Number of counties	-	1,295	-	-
Number of farms	11,501	-	11,501	11,501

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** $p<0.01$, ** $p<0.05$, * $p<0.1$

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

Table C.26: Impact of 287(g) authorization on financial indicators: 2SLS

	(1) Machinery value (ARMS)	(2) Machinery value per operation (Census)	(3) Net income (ARMS)	(4) Net income per county (Census)	(5) Real estate asset values (ARMS)	(6) Real estate asset values per county (Census)	(7) Debt (ARMS)	(8) Number of farms per county (Census)
287(g) authorization	203,898	-559,179***	-272,655	-145,433,902**	-4.395e+06*	-357,671,609	847386	-707***
	(278,104)	(180,765)	(460,024)	(57,693,030)	(2.636e+06)	(699,858,835)	(929,858)	(262)
287(g) border	-3,287	-28,053***	38,458	-6,627,485**	-346,501	11,715,982	-23,829	-25
	(12,087)	(7,356)	(76,102)	(2,688,297)	(317,702)	(55,115,926)	(31,119)	(16)
County FE	NO	YES	NO	YES	NO	YES	NO	YES
Farm FE	YES	NO	YES	NO	YES	NO	YES	NO
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	21,403	5,272	21,403	3,769	21,403	5,270	21,403	5,276
Number of counties	-	1,324	-	1,321	-	1,324	-	1,324
Number of farms	11,501	-	11,501	-	11,501	-	11,501	-

[a] Standard errors robust to correlation at the strata level (ARMS) and heteroskedasticity-robust (Huber-White) standard errors (Census) in parentheses

[b] *** p<0.01, ** p<0.05, * p<0.1

[c] Source: USDA Agricultural and Resource Management Survey (ARMS); US Census of Agriculture (NASS QuickStats); BJS National Jail Census; ICE FOIA Proactive Disclosures

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