Nothing to Fear but Fear Itself: Seasonal Farm Labor and Criminal Activity in the United States*

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Abstract

We combine county-by-month data on labor shares and crime to estimate how seasonal spikes in agricultural labor demand impact local criminal activity. We find that increased seasonal agricultural activity reduces property and violent crime rates, and possibly even the number of property crimes committed. These results are robust to several alternative specifications that address the inherent challenges associated with measuring seasonal agricultural labor. Taken together, we conclude that fears of criminal activity associated with migrant farm workers are misguided.

Keywords: Crime; Immigration; Seasonal Agriculture; Farm Workers; Seasonal Employment

JEL Classification: Q10; K14; R23; F22; E24

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1 Introduction

John Steinbeck brought to life the hardships and poverty associated with migrant farm work in the classic American novel *The Grapes of Wrath*. However, unlike Steinbeck's Joade family of the Dust Bowl era, few farm workers today are U.S. citizens. Raids to break up socialist uprisings in Steinbeck's time have been replaced by Immigration Customs and Enforcement (ICE) raids. Farm workers, about 50 percent of whom are unauthorized immigrants,[1] are often portrayed as a threat to public safety. Following the murder of Iowa resident Mollie Tibbetts by an illegal immigrant, a joint statement by Senators Chuck Grassley and Joni Ernst said that "Too many Iowans have been lost at the hands of criminals who broke our immigration laws. We cannot allow these tragedies to continue." More famously, during his speech announcing his candidacy for president, Donald Trump said "When Mexico sends its people, they're not sending their best...They're sending people that have a lots of problems, and they're bringing those problems with us. They're bringing drugs. They're bringing crime. They're rapists. And some, I assume, are good people."

Public perception appears to be that seasonal farm workers cause crime and threaten community safety. For example, Huron, California, a quintessential Central Valley town populated by farm workers has been called "knife-fight city" in reference to the ubiquity of lettuce knives during the spring harvest combined with high poverty and crime. When agricultural guest worker housing units were constructed in Spreckels, California, local residents raised concerns that the presence of seasonal farm workers would increase crime and reduce home values (Mohan, 2017). More broadly, many Americans believe that immigrants, and especially "illegal" immigrants, are more like to commit violent crimes than the rest of the U.S. population.[2]

There are practical reasons to believe that the increased presence of migrant farm workers

[1]Based on authors' analysis of the Department of Labor, Employment & Training Administration (2017).

[2]Data collected from a 2018 Grinnell College National Poll that asked 1,000 U.S. adults, "Compared to the U.S. population overall, do you think the rate of violent crime committed by illegal immigrants in the United States is higher, lower, or about the same?". While 30% of respondents answered "higher" just 20% answered "lower". Detailed results are available at: https://www.pollingreport.com/immigration.htm. This idea is reinforced by earlier survey data from 2000 in which 73.4% of respondents thought that it was "very likely" or "somewhat likely" that crime rates would increase as a result of increased immigration into the United States (Spenkuch, 2014).

during the labor-intensive harvesting season could be associated with increased crime. Migrant workers are disproportionately male, young, single and poor, which are all associated with increased crime rates. On the other hand, increased economic activity during harvest season, which we show extends to increased employment outside of the agricultural sector, could suppress criminal activity. However, to our knowledge, no studies have investigated the effects of seasonal farm labor—and seasonal agricultural activity more generally—on local crime rates.

We fill this gap in the literature by estimating the effects of labor-intensive seasonal agricultural activity on the frequency of criminal activity. We combine data on crime counts and seasonal agricultural employment at the county-by-month level over the period 1990-2016, allowing us to identify crime effects based on within-county seasonal variation in labor-intensive agricultural activity. Our analysis is made up of two parts. First, we estimate the marginal effect of an increase in the seasonal agricultural employment share of the labor force on measures of property and violent crime. Understanding marginal effects is important from a policy perspective, but this baseline analysis potentially masks important non-linearities in the relationship between seasonal employment and criminal activity. We therefore supplement our baseline specification with a less parametric one that describes how crime rates change in response to variation in the temporal distance from the month of peak seasonal employment.

We do not find any evidence that higher seasonal farm labor shares increase local crime rates. We find that a one percentage point increase in the seasonal employment share is associated with roughly five fewer property crimes per 100,000 members of the labor force.[3] Consonant results are found for the violent crime rate, though with inconsistent statistical significance when using our marginal effects model. Our more flexible and less parametric specification reinforces these findings. Relative to five months before peak seasonal employment, property and violent crime rates are roughly 12% lower during peak seasonal employment in treated counties with significant seasonal agricultural activity (relative to control counties).

We also estimate the effect of agricultural activity on the number of property and violent crimes committed. Even if seasonal farm workers commit crimes at a lower rate than

[3]Because population data is not available at the county-by-month level, we proxy crime rates with the number of crimes divided by the labor force. See Section 4.1 for further discussion.

non-seasonal workers, one would expect the crime count to increase in response to rising populations during harvest season. For violent crime counts, we again find somewhat mixed evidence, with some specifications estimating an increase. Interestingly, we find tentative evidence of decreased property crime counts during harvest seasons. One speculative interpretation of these results is that financially motivated crimes may be reduced by enhanced economic activity in secondary agricultural and service sectors.

Our analysis should be viewed in the context of two related literatures. First, we contribute to a sizable literature that estimates the impacts on crime of labor shocks, including large infrastructure projects (Freedman and Owens, 2016), access to legal employment for immigrants (Baker, 2015; Freedman and Owens, 2018), and macroeconomic recessions (Lin, 2008). There is also a tangential literature that estimates the effect of income shocks created by large cash transfers (Carr and Packham, 2019; Watson et al., 2019). Consistent with the seminal work of Becker (1968) and Ehrlich (1973) who model criminals as rational economic agents that weigh the benefit of criminal activity against the expected cost, this literature tends to find that improved economic conditions (more income and employment) decreases criminal activity.[4] Though, not all positive labor demand shocks are associated with falling criminal activity. For example, energy booms tend to increase income and employment but also crime rates (James and Smith, 2017; Gourley and Madonia, 2018; Komarek, 2018). While the mechanism is not well understood, James and Smith (2017) hypothesize that heterogeneous labor migration might play a key role. However, the present study analyzes an influx of migrant laborers and finds opposite effects. This may reflect the differing demographic composition of the agricultural crop workforce, about half of which were unauthorized immigrants as of 2016,[5] and the routine six-day work weeks that many agricultural workers keep during peak labor seasons (University of California, Davis, 2016).

We also contribute to the literature on the relationship between immigration and crime

[4]This is intuitive as a higher level of income decreases the marginal benefit of committing a crime, and improved labor market conditions increase the opportunity cost of spending time in prison. More generally the inverse relationship between criminal activity and improved economic conditions is well documented in the literature. See for example Gould et al. (2002); Lin (2008); Foley (2011); Carr and Packham (2019); Watson et al. (2019); Blakeslee and Fishman (2018).

[5]Authors' analysis of the Department of Labor, Employment & Training Administration (2017).

rates more generally. The key challenge in this literature is identifying exogenous variation in immigrant populations. If, for example, immigrants tend to concentrate in economically thriving communities (and economic prosperity is associated with less criminal activity), one might incorrectly estimate that increasing the migrant population share causes crime rates to decrease. To address this, some researchers have instrumented for variation in regional migrant populations using "supply-push" metrics, such as the aggregate number of people moving out of a country. Within-country regional variation in "supply-push" immigration is then estimated using the spatial distribution of migrant populations at the beginning of the sample period (or a pre-sample period).[6][7] This approach is utilized by Bianchi et al. (2012) in their analysis of migration into Italian provinces, and Bell et al. (2013) in an analysis of two distinct waves of immigration into the United Kingdom.[8]

Turning to the U.S. experience, Chalfin (2014) studied the effect of Mexican immigration on crime rates in the United States. He instruments for Mexican immigration using variation in Mexican rainfall and information on long run Mexican state–U.S. city migration networks, finding that Mexican immigration does not have any appreciable effect on either violent or property crimes in the United States. Similarly, in their analysis of immigration and crime across U.S. metropolitan areas, Reid et al. (2005) find no evidence that increasing the migrant population share increases violent or property crime rates. These results are complemented by Butcher and Piehl (2007) who find that incarceration rates for immigrants in the United States are far less than that for native-born citizens. They offer evidence that this reflects that

[6]This approach has received some criticism. As discussed by Chalfin (2014), there are several ways that prior migrant location decisions can influence future crime rates (other than the indirect effect of attracting future migrants). For example, suppose that, due to some unobserved serially-correlated factor, crime rates grow more quickly in "gateway" cities than in "non-gateway" cities. In this case, the relationship between future immigration and crime rates would be biased upward. As an example, consider that drought in the southwestern US is more common today than in the 1980s, and suppose that drought causes crime. Because many Hispanic immigrants settle in the southwestern United States, this induces a positive correlation between immigrant populations and crime rates.

[7]This approach was first used by Altonji and Card (1991) in their analysis of immigration and labor market outcomes of less-skilled workers in the United States.

[8]The first wave in the 1990s was comprised of asylum seekers and the second wave in the mid 2000s was made up of workers from the European Union that faced better labor market opportunities than the asylum seekers that came before them. The first wave led a small but statistically significant increase in property crime, while the second wave had a small negative effect. For both waves, they find no effect on violent crime or arrest rates. Their results are consistent with the idea that improving labor market opportunities reduces the incentive to commit financially-motivated crimes.

the process of migration "selects individuals who either have lower criminal propensities or are more responsive to deterrent effects than the average native". Wadsworth (2010) credits immigration for the significant drop in crime rates in the 1990s noting that, from 1990 to 2000, the cities with the largest increase in immigration experienced the largest decreases in both homicide and robbery during that same period (see also Stowell et al. (2009)). However, not all research finds that immigration does not cause increased crime in the United States. One notable exception is Spenkuch (2014) who finds that increasing the U.S. county migrant share of the population is associated with more burglaries, larcenies, and grand theft-auto (but no effect on violent crimes). He further finds that these effects only hold for immigrants from Mexico, who he posits have relatively poor labor market opportunities and so might be prone to commit for financially-motivated crimes.

Our research is related to this literature on immigration and crime, but it is important to note that we do not estimate the crime rates of immigrants or how they compare to non-migrants. Our empirical design estimates how crime rates are affected by seasonal agricultural activity in places where non-permanent farm workers make up some portion of the labor force for part of the year. Compositional changes involving an influx of the migrant population may be an underlying mechanism, but we do not identify this effect. There are a variety of reasons to think that agricultural shocks might be associated with more (or less) crime including enhanced labor market opportunities, increased wages and income, inward and outward migration, social disorganization, and broader compositional changes. Therefore, our analysis should be thought of as an estimation of the "net" or "overall" effect of seasonal agricultural activity on criminal activity.

Further, the direction of victimization is not apparent in crime rate statistics. While we find some evidence for inreases in the count of violent crimes (though not the rate), we do not observe whether migrant workers are perpetrators or victims of such crimes. Contrary to popular belief, DeAngelo et al. (2018) found that Whites in Los Angeles county were more likely to assault Hispanics and Blacks than Hispanics and Blacks were to assault Whites. Prior beliefs about farm workers may spur violent behavior, as when in 2016 unfinished housing units intended for seasonal strawberry workers were torched in an act of arson in Nipomo, California

(Mohan, 2017).

This paper proceeds as follows: the following section provides background on seasonal agricultural activity and labor. Section Three details our empirical methodology, Section Four discusses our primary data sources, Section Five presents and discusses our main results, along with a series of robustness checks and extensions, and Section Six concludes.

2 Background

According to the 2012 Agricultural Census, there were 2.7 million workers hired on farms.[9] A little more than 60 percent of these workers (1.7 million) were employed for fewer than 150 days.[10] Many of the highest value crops produced in the United States, including most fruits and vegetables, require large crews of seasonal farm workers to cultivate and harvest the crops. However, few farm workers were born in the United States, and over 60 percent migrated to U.S. farms from Mexico.

Since fruits and vegetables are often delicate and difficult to harvest mechanically without bruising and destroying the product, most are still harvested by hand. Approximately half of all farm workers, including half of those who were employed more than 150 days and half of those who were employed for fewer than 150 days, were employed on Fruit, Vegetable, and Horticultural (FVH) farms. Three quarters of workers hired on fruit and nut farms, two thirds of workers hired on vegetable farms, and nearly one half of workers hired in greenhouses and nurseries were employed for fewer than 150 days in 2007 (Martin and Taylor, 2013). Other agricultural industries, such as dairies, may be highly labor-intensive, but demand a steady labor force year-round.[11]

[9]There were 3 million workers reported in the 2002 Agricultural Census, 2.6 million in 2007, and 2.4 million in 2017. Data come from the U.S. Department of Agriculture, National Agricultural Statistics Service (NASS) https://quickstats.nass.usda.gov. Retrieved on April 27, 2020.

[10]Farm operators can also hire workers through a Farm Labor Contractor (FLC). All labor hired through an FLC would be seasonal or temporary in nature.

[11]Although dairy operators reported that about one-half of their employees worked fewer than 150 days in the 2007 Agricultural Census, this reflects high worker turnover rather than seasonality of labor demand.

2.1 Characteristics of the Seasonal Farm Workforce

Poverty rates are high among seasonal farm workers, in part due to the seasonal nature of labor demand. The National Agricultural Workers Survey (NAWS) is a nationally representative random-sample survey of U.S. crop workers conducted at the place of work. We summarize characteristics of workers in the NAWS who reported that they were working for their employer seasonally (not year-round). According to the NAWS, 87.8 percent of seasonal farm workers in 2016 who reported working the previous year had annual incomes below $25,000 in 2015, and 49.5 percent reported incomes below $15,000. Figure 1 shows the income distribution.

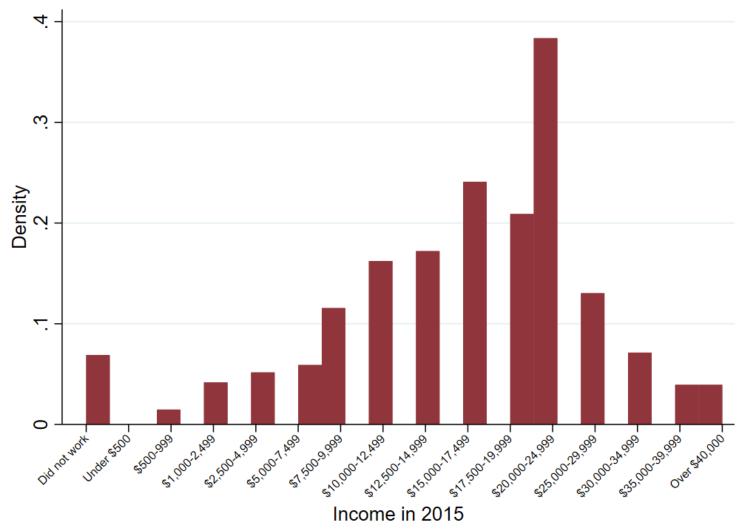


Figure 1: Income of Seasonal Farm Workers (2015)

Incomes are low in part because many workers do not work year-round. In 2015, seasonal farm workers did not work for an average of 17.4 weeks of the year. Figure 2 shows mean weeks per year that seasonal farm workers report that they did not work between 1990-2016. The mean weeks not worked is 13.2 for the entire sample period.

Seasonal farm workers are significantly more migratory than the general population. Some workers migrate from farm to farm (follow-the-crop workers), and some migrate back and forth between their place of work in the United States and their homes, often in Mexico (shuttle migrants). Panel (a) of Figure 3 shows the migratory (i.e. either follow-the-crop or shuttle

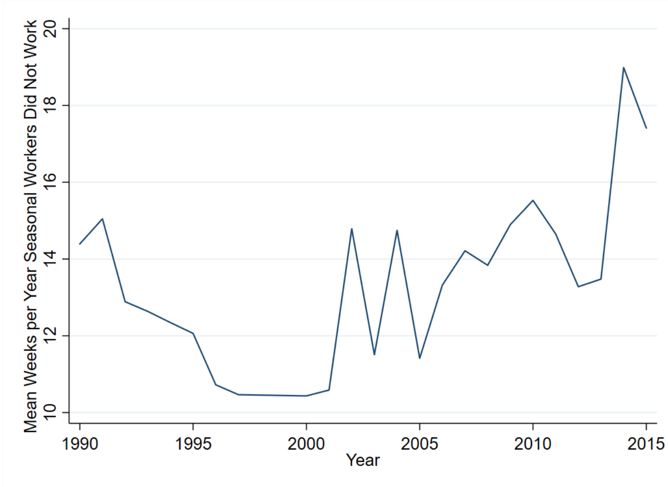


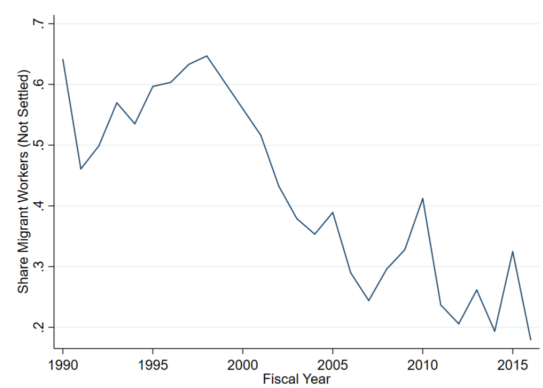
Figure 2: Mean Weeks per Year that Seasonal Farm Workers Did Not Work

migration) share of seasonal farm workforce each year from 1990-2016. There is a clear downward trend in the migratory share of seasonal farm workers. Fan et al. (2015) conclude that the statistically significant decline in migration after 1999 can be attributed to both demographic changes in the workforce and unobservable factors, including structural changes in the U.S. and Mexican economies.

Panel (b) of Figure 3 plots the share of seasonal farm labor who are foreign-born each year. From 1990-2016, 71.3 percent of seasonal farm workers were born in Mexico, 22.4 percent were born in the United States, and 3.3 percent were born in Central America. Most seasonal farm workers are not native English speakers. From 1990-2016, 75.1 percent of NAWS respondents responded that their most comfortable language was Spanish while 22.4 percent responded that English was their most comfortable language.

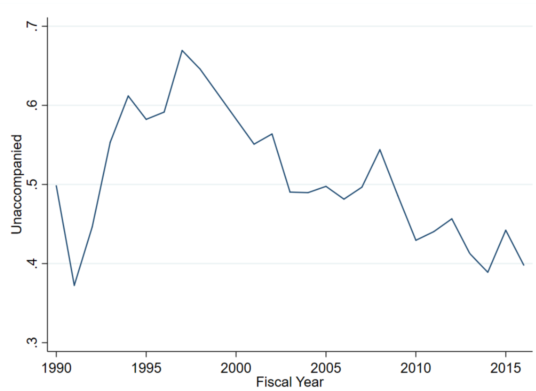
Individuals who migrate unaccompanied by family may have less social accountability than those who live with their nuclear family and may consequently have less severe expected consequences from committing a crime. Panel (c) of Figure 3 plots the share of seasonal farm workers who have no nuclear family members accompanying them. There is a clear downward trend in the share of unaccompanied farm workers from a peak over 65 percent in the late 1990s to 40 percent in 2016.

Figure 3: Characteristics of Seasonal Farm Workers



(a) Migratory

(b) Forign-Born



(c) Do Not Live with Nuclear Family

Note: Panel (a) gives the share of seasonal farm workers who are migratory (either follow-the-crop or shuttle migration). Panel (b) gives the share of seasonal workers born outside the United States. Panel (c) gives the share of seasonal workers who do not live with their nuclear family. Data taken from the National Agricultural Workers Survey (NAWS).

3 Methodology

We estimate the relationship between seasonal employment shares and criminal activity using two different specifications. The first specification (which we call our baseline parametric specification) measures the marginal effect of seasonal farm labor shares on county-month level crime rates. Our second specification (which we call our semi-parametric specification) estimates how crime varies each month relative to the month when agricultural-intensive counties have their peak seasonal farm workforce.

3.1 Baseline Parametric Specification

We estimate the marginal impact of seasonal agricultural labor on crime outcomes with the following equation:

$$Y_{imy} = \alpha + \beta * Seasonal\_Share_{imy} + \mu_{my} + \gamma_{iy} + \epsilon_{imy}, \tag{1}$$

where Y_{imy} is the outcome of interest for county i in month m of year y, $Seasonal\_Share_{imy}$ is the share of the labor force taken up by seasonal agricultural laborers (defined below), measured in percentage points. Month-by-year fixed effects are given by μ_{my}, and γ_{iy} is county-by-year fixed effects. Month-by-year fixed effects control for any nation-wide month-specific shocks in crime. County-by-year effects control for any factors constant over a calendar year within a county. Therefore, all identifying variation comes from monthly shifts in seasonal agricultural labor shares within a county-year, controlling for any monthly national shocks. β is the coefficient of interest and represents the average change in crimes associated with a one percentage point increase in seasonal agricultural labor share. Standard errors for all regressions are clustered at the county level.

To measure the seasonal agricultural labor share, we begin by identifying twelve Fruit Vegetable and Horticultural (FVH) sectors in the QCEW data. These sectors, by NAICS title, are apple orchards, grape vineyards, strawberry farming, berry (except strawberry) farming, orange groves, citrus (except orange) groves, other vegetable and melon farming (excluding potatoes), other non-citrus fruit farming, fruit and tree nut combination farming, food crops grown under cover (Greenhouse), and nursery and floriculture production.[12] These sectors consist of crops with high shares of seasonal labor demand.

The QCEW employee counts for the sectors listed above do not include labor hired through farm labor contractors (FLCs), who hire farm workers and contract them to work on individual farms for short-term jobs. FLCs provide a service to reduce labor market frictions when many workers are needed in various locations for short periods. We account for employees of FLCs using multiple methods. In our main specification, we include the employees hired under the NAICS title farm labor contractors and crew leaders in the counties where they are reported. However, given that FLCs may transport workers to different counties to work on multiple farms throughout the year, we perform a robustness check in which we estimate the number of FLC workers contracted in each county based on the share of labor expenditures per county attributed to contract labor in the Agricultural Censuses in 2002, 2007, and 2012.[13]

Of course, each of the twelve FVH sectors contain permanent laborers, in addition to seasonal ones, and workers who may work continuously throughout the year on multiple farms. We estimate the number of seasonal laborers in a given month by performing the following steps for each of the twelve FVH sectors and FLCs: first, for a given set of twelve monthly observations within a county-year, we identify the month with the lowest employment count, and assume this count is the number of "permanent" jobs for that county-year group of observations. Then for a given county-month observation, the difference between total employment in the specified sector that month and the permanent employment count is our estimate of the number of seasonal workers in the specified FVH sector. We then sum together seasonal employment from all twelve seasonal sectors and FLCs to yield a total seasonal employment count. Total seasonal employment is then divided by total labor force to yield the seasonal share in Equation 1.

[12] Employment on potato farms is reported separately from other vegetables, and potato harvests are generally highly mechanized. Therefore, we excluded potatoes from our analysis. See, for example, Patterson (2015) for a cost and return study for potato production in Idaho.

[13] We linearly interpolate shares of labor contracted through FLCs between 2002-2007 and 2007-2012 to impute FLC shares in years between censuses.

3.2 Semi-Parametric Specification

Observing that employment in seasonal agricultural sectors typically displays a distinct peak period corresponding to harvest season, we alternatively perform a less parametric empirical design that estimates how crime is affected over time relative to the peak. To do this we first define a "treatment group" of counties that typically have high shares of seasonal agricultural labor, and then for each of these counties identify a "peak" month where seasonal labor shares are highest.

To define a treatment group, for each set of twelve monthly observations within a county-year, we find the month with the highest share of seasonal agricultural employees, as defined above. We then find the average of this yearly maximum seasonal share over all years in the sample (1990-2016). We then include a county in the treatment group if this average maximum share exceeds 4%, which is roughly the 95th percentile among counties that have non-zero seasonal labor.[14] This yields forty-six treatment counties, which are shown in red in Figure 4. There is a high concentration of treatment counties in the Central, Salinas, and Imperial Valleys of California and in the major apple-growing regions of Washington state.[15] This is not surprising since seasonal farm labor demands are particularly high in these regions.[16] We drop counties that are below the 4% threshold but are above 1%, as these counties are still meaningfully impacted by seasonal labor, though this does not meaningfully change the results.

For each treated county, we find the peak calendar month for seasonal farm labor, defined as the month with the highest average seasonal labor share across all years in the sample.[17] With the treatment group and peak month for each treated county defined, we estimate the

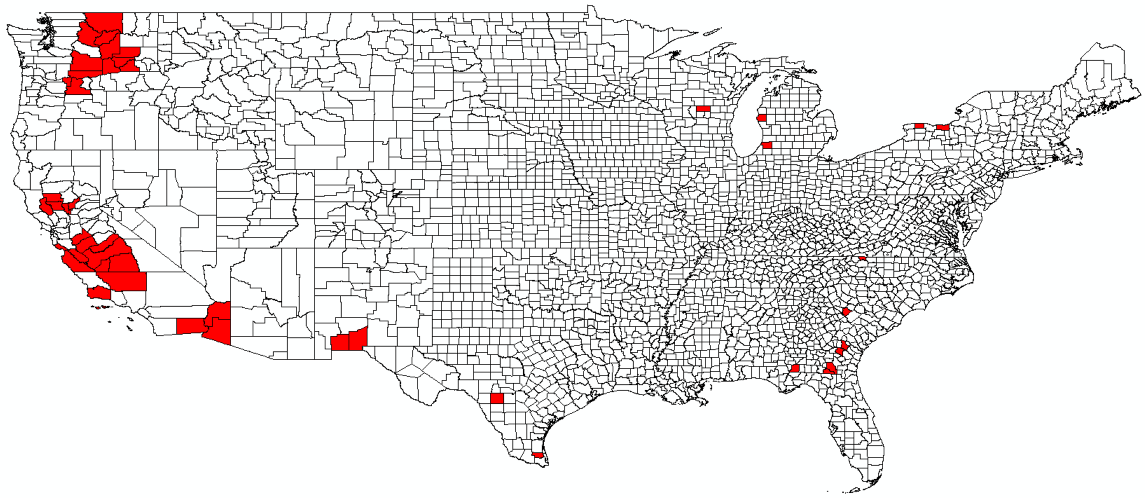
[14]While the choice of threshold is necessarily arbitrary, results are qualitatively similar when using a threshold of 2% or 6%, though somewhat weaker for the former and stronger for the latter, as expected. These results are available upon request.

[15]See for example, Washington Grown. 2020. "Crops by County." http://www.wagrown.com/crops-by-county/ Last visited March 31, 2020.

[16]Seasonal farm labor demand is also high in Florida, but we dropped Florida from the analysis for two primary reasons. The first is that Florida does not report all seasonal farm labor in the QCEW. The second is that Florida does not have consistent records in the UCR crime data.

[17]The month with the highest share of agricultural labor is not necessarily always the same month within a given county each year. We choose a single calendar month per county to simplify the analysis.

Figure 4: Treatment Counties



following equation:

$$Y_{imy} = \alpha + \sum_{s=-4}^{6} \beta_s(\lambda_s T_i) + \mu_{my} + \gamma_{iy} + \epsilon_{imy}, \quad (2)$$

where T_i is an indicator equal to one if county i is in the treatment group, and λ_s is an indicator equal to one if the observation is s months after the peak seasonal labor month. All other variables are defined similarly to Equation 1. β_s then represents the average effect of being s months after the peak month, where five months before the peak month is the omitted category.

We estimate both the parametric and semi-parametric specifications to estimate the change in violent and property crime rates, as well as the natural log of crime counts, associated with monthly changes in the seasonal farm workforce. These are complimentary outcomes in evaluating the overall impact on crime. If we assume that seasonal workers temporarily residing in a county commit crimes at the same rate as the permanent population, and the permanent population does not change its criminal activity with changes in the presence of seasonal workers, then our estimated effects on crime counts will be positive but effects on crime rates will be zero. If seasonal workers commit crimes at lower but non-zero rates, effects on crime counts will be positive but effects on crime rates negative. If seasonal workers commit crimes at higher rates, or if the permanent population commits more crimes when seasonal farm workers are present, then effects on both crime counts and rates will be positive. We should not expect crime count effects to be negative unless seasonal workers commit very few crimes and the permanent population commits fewer crimes when seasonal farm workers are present. If changes in seasonal farm labor shares are associated with other mechanisms, such as an increase in secondary agricultural employment, then we might see a decline in crimes as more permanent residents are employed or employed longer hours.

4 Data

4.1 Employment Data

Our employment data come from the Quarterly Census of Employment and Wages (QCEW), a census of all establishments that are covered by unemployment insurance compiled by the Bureau of Labor Statistics (BLS). The QCEW provides month-by-county-by-industry employment counts for all counties and years from 1975-present. Industries are classified by NAICS codes and employment counts are available at the six-digit level.[18] One pitfall of these data is that when there are a small number of employers in a given county-industry-year combination (or some other reason that employers could be identifiable), wage and employment data are suppressed. When employment data for any of our seasonal agricultural sectors are suppressed we will under-measure the seasonal employment share. In Section 5.3.3 we discuss this issue further and perform a robustness check in which we drop observations with suppressed seasonal agricultural sectors.

To the extent that employers may not report unauthorized workers for unemployment insurance, we may under-count seasonal farm workers in the QCEW. However, since employers are legally responsible for knowingly hiring unauthorized workers it is believed that most unauthorized workers at least provide a social security number when they are hired and are consequently counted in the QCEW.

[18] We drop any county-year observations that report zero total employment for any month within the year, though this is very rare.

Another concern with the QCEW is that farm employers in some states with few employees are not required to report workers for unemployment insurance, and consequently, farm employers with few employees in these states do not record their workers in the QCEW. Furthermore, employers in some states are required to report H-2A agricultural guest workers for unemployment insurance while employers in other states are not.[19] According to University of California, Davis (2020), farm employers of all sizes in Washington and California must report all employees for unemployment insurance, including H-2A workers, but farm employers in Florida do not. Florida, North Carolina, Georgia, Washington, and California employed half of all H-2A workers in 2016 (Martin, 2017).[20] This may cause us to under-count the seasonal farm work force in key states, particularly if crew leaders for FLCs are considered individual employers. However, we drop Florida from our analysis due to irregular crime data (see Section 4.2), and we know that two of the other leading states in H-2A employment, California and Washington, report H-2A workers in the QCEW along with other farm workers in the same sector. To the extent that the QCEW may under-count FLC employees or associate FLC employees with a county where workers are not actually working and residing in a given month given the mobility of FLC crews, we use multiple methods to estimate the number of farm workers hired through an FLC in each county, including imputation methods based on the Agricultural Census. To the extent that QCEW may under-count seasonal farm workers because some states do not require employers of few workers or H-2A workers to report employees in QCEW, we repeat our analysis using only counties in California and Washington where we know that all employees must be reported in the QCEW. Finally, to the extent that H-2A agricultural guest workers may differ from other seasonal farm workers since they have legal temporary guest visas and are subject to the corresponding regulations, we repeat our

[19] H-2A is a nonimmigrant guest worker visa for seasonal farm workers. Program take-up was extremely low between 1986-2010, but rose rapidly from 2011-2018. Nevertheless, in 2016 H-2A workers made up only 7 percent of the national farm workforce (Martin, 2017).

[20] According to conversations with several of the leading researchers in the field of farm labor economics, including administrators of the NAWS, there is no known database indicating which states have a threshold number of employees below which agricultural employers do not report to Unemployment Insurance. According to a phone call with the North Carolina Department of Commerce, farm employers in North Carolina do not report H-2A workers in the QCEW or any farm employees if total employees is fewer than ten employees in 20 weeks of a calendar year or payroll less than $20,000 per year. The Georgia Department of Labor was unable to disclose any information about what Georgia employers do or do not report.

analysis using H-2A guest worker shares as the explanatory variable.[21]

An important caveat for this study is that while we have county-by-month data on crime counts and employment, we do not have monthly estimates of population, which creates a challenge in estimating rates of crime (as opposed to counts). To the extent that the harvest season employment spike draws workers who work seasonally and remain in the same county even after their employment ends, the increase in employment will exceed the true proportional increase in population. For this reason we calculate monthly crime rates as number of crimes per total labor force, which is distinct from total employment in that it includes people who are not employed but are looking for work, and so is less sensitive to economic swings and more representative of the working-age population. We draw county-by-month labor force counts from the Bureau of Labor Statistics, which constructs labor force estimates based on several sources, including the Current Population Survey, American Community Survey, the Current Employment Statistics Survey, and state unemployment insurance data. In addition, we run a robustness check that attempts to account for seasonal farm workers who remain in the same county while they are not working. Using NAWS data we find the percentage of seasonal farm workers who report that they are settled in one location and the annual average share of the year that these workers report that they did not work, and we adjust our labor force denominator accordingly. This exercise is further discussed and presented in Section 5.3.2. Further, although measurement error in population is of consequence in the interpretation of our estimated effects of seasonal farm labor shares on crime rates, it is not of concern for our analysis of crime counts.

4.2 Crime Data

Crime data are drawn from Uniform Crime Reporting (UCR), which is a compilation of incident counts by over 16,000 law enforcement agencies. We use the "Offenses Known and

[21] We use the number of H-2A visas issued by county, as reported in the Department of Labor, Office of Foreign Labor Certification (OFLC) Disclosure data. The disclosure data include the worksite county that each employer is required to report in each visa application. However, county names were often misspelled, or employers reported the city rather than the county. Marcelo Castillo (USDA, Economic Research Service (ERS)) generously shared with us the data that he cleaned to correct for some of these reporting errors.

Clearances by Arrest", which contains counts of reported crimes at the month-by-agency level for several types of offenses. Our main outcomes of interest are rates of all crimes, violent crimes and property crimes. Violent crimes include homicide, rape, robbery, and aggravated assault. Property crimes include burglary, larceny and motor vehicle theft. We aggregate agency-level crime counts to the county level for our analysis.

One key issue with UCR data is that agencies are not required to report crimes. However the data do indicate the number of months reported for a given agency and year. We drop any agency-year combination with less than 12 months of reporting. Therefore we ensure that the jurisdictional populations are equivalent for each month within a county-year. The month-level design of this study makes the UCR reporting problem much less problematic than designs that aggregate to the county-year level, because agencies can be added or removed from a county or experience large changes in reporting on a year-to-year basis. Since all of our regressions include county-by-year fixed effects, all identifying variation is within the county-year level where these issues do not apply. Further, some counties are not included at all in the UCR, and this can vary by year. In our main sample, an average of 2,587 counties are included per year. Missing counties are typically low in population.

One remaining issue with UCR data is that in some cases even an agency that indicates 12 months of reporting loads a disproportionate number of crimes on a single month. Most commonly in this case, agencies will have zero counts for all months except December, but it sometimes happens for other months as well. To address this, we first drop all counties in Florida and Alabama from our analysis since this issue is extremely common in those states. For remaining counties, within each year we find the month with the highest number of crimes. If the ratio of crimes in this month to the average of all other months within the year is greater than 10, we drop that county-year combination from the analysis (we perform this step separately for violent and property crimes). This step drops less than 1% of observations. The threshold ratio of 10 is meant to remove especially extreme outliers that could skew results.

Summary statistics for our main regression variables are shown in Table 1. This table also provides statistics for seasonal employment count and seasonal employment share for the peak seasonal employment month for the treatment group used in our semi-parametric specification.

5 Results

5.1 Baseline Parametric Specification Results

Table 2 presents the results from estimating Equation 1 for the property crime rate per 100,000 labor force participants, log of property crime rate, and log of property crime count. Seasonal agricultural labor share is associated with a statistically significant decrease in the property crime rate. The coefficient of -4.89 implies that increasing the seasonal agricultural employment share of labor force by one additional percentage point is associated with 4.89 fewer property crimes per 100,000 labor force participants. This reduction is roughly 1.5% of the sample median property crime rate of 335 per 100,000. We also find a statistically significant reduction on log property crime rates, implying a one percentage point increase in seasonal employment share is associated with a reduction in property crime rates of roughly 1%. Somewhat surprisingly given the influx of temporary laborers, we do not find evidence of effects on property crime counts, and the point estimate is in fact negative. In sum, we find that the increased seasonal labor force share is not associated with an increase in the number of property crimes, and therefore the property crime rate declines even as the size of the labor force increases.

Table 3 shows the estimated effects on violent crimes. For non-transformed violent crime rates we find a negative and insignificant effect, though for log violent crime rates the negative effect is significant at a 10% level.[22] Unlike for property crimes, here we do find a positive and significant increase in the count of violent crimes. The estimate of 0.005 implies that a one percentage point increase in seasonal agricultural labor share is associated with a 0.5% increase in violent crime counts. The results for violent crime counts and rates are not contradictory;

[22] Note that the sample sizes for the log-transformed rate regressions are smaller due to observations with zero crimes, but using this reduced sample for the non-transformed rate regressions does not meaningfully change the result.

Table 1: Summary Statistics

Variable	
Property Crimes per 100,000 Labor Force	388.3
	(285.7)
	[838332]
Violent Crimes per 100,000 Labor Force	171.6
	(152.1)
	[814020]
Labor Force	48969
	(158387)
	[838332]
Seasonal Employment	57.01
	(670.9)
	[838332]
Seasonal Employment Share (pp)	0.08
	(0.68)
	[838332]
Has Non-zero Seas. Employment Indicator	0.15
	(0.36)
	[838332]
Seasonal Employment in Peak Month (T Group Only)	2109
	(4306)
	[14964]
Seasonal Emp. Share in Peak Month (T Group Only)	0.07
	(0.07)
	[14964]

The table shows means of each variable for the baseline property and violent crime rate regression samples. Standard errors are shown in parenthesis and sample size used in the main regression specifications are in brackets.

they collectively imply that the number of violent crimes tends to increase with the influx of seasonal farm labor, but the increase in the labor force is sufficiently large that the measured crime rate falls.

Table 2: Property Crime Results

	(1) Property Crime Rate	(2) Ln(Property Crime Rate)	(3) Ln(Property Crime Count)
Seasonal emp. share	-4.894***	-0.012***	-0.004
	(1.277)	(0.003)	(0.003)
N	838332	814987	814987

Notes: The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

Table 3: Violent Crime Results

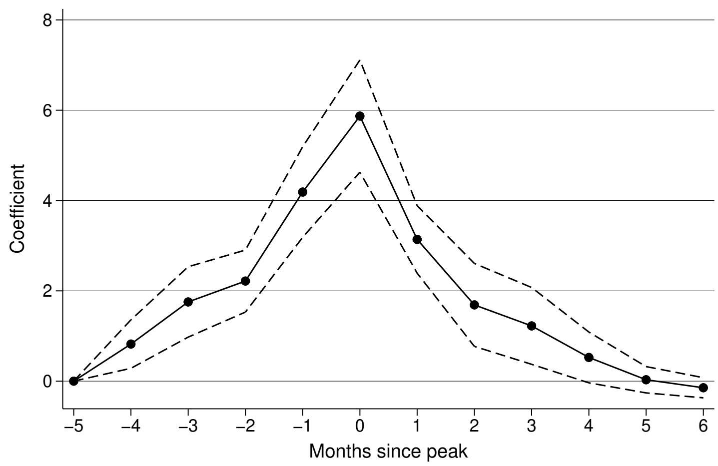
	(1) Violent Crime Rate	(2) Ln(Violent Crime Rate)	(3) Ln(Violent Crime Count)
Seasonal emp. share	-0.207	-0.004*	0.004***
	(0.407)	(0.002)	(0.001)
N	814020	765667	765667

Notes: The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

5.2 Semi-parametric Results

Before presenting our estimates for crime rate effects using the semi-parametric specification of Equation (3), we first demonstrate that our definition of seasonal labor described in Section 3.1 indeed produces a distinct spike in observed seasonal farm labor in our treatment counties. Figure 5 shows the estimated coefficients from Equation (3) using seasonal agricultural labor share as the dependent variable. The results imply that the seasonal agricultural employment share is on average six percentage points higher relative to control counties during the peak month (or zero "months since peak") than this same difference five months before (the reference category).

Figure 5: Seasonal Emp. Share, Semi-Parametric Results

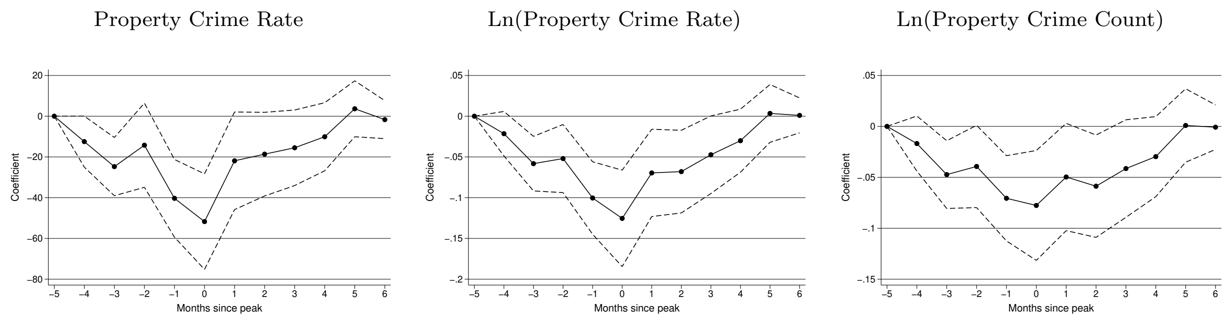


The graph plots coefficients and 95% confidence intervals from estimating Equation (2) with seasonal employment share as the dependent variable.

Figures 6 and 7 show our semi-parametric results for property and violent crimes. They are largely consistent with the parametric results shown in Tables 2 and 3. For property crimes, the effects on crime rate and log crime rate both experience a dip in the peak month. The negative coefficient estimates are statistically significant at the 5% confidence level for the peak month and one month before.

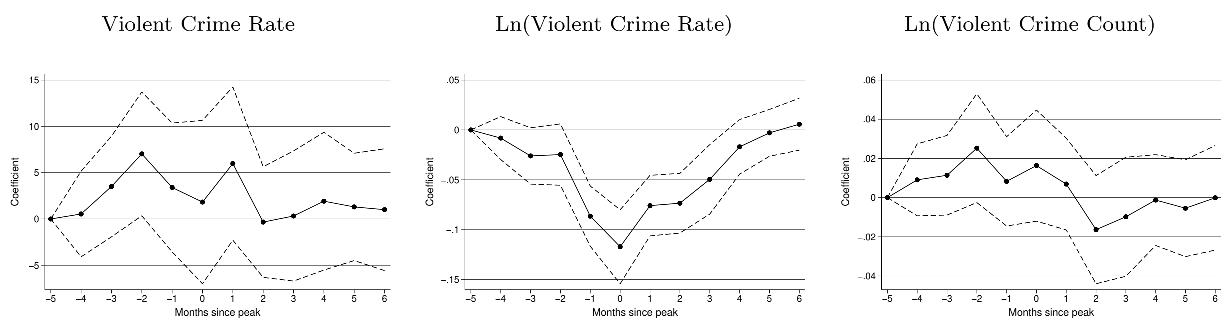
For log of property crime counts, effects are generally negative relative to five months before peak and intermittently statistically significant, though the dip is much less pronounced. While we interpret this as merely suggestive and inconclusive evidence for reductions in crime counts, it is an interesting and unexpected result. A speculative interpretation of this finding is that a seasonal rise in agricultural activity improves local economic conditions and reduces the incentives to commit financially-motivated crimes. The extant literature offers an abundance of evidence that economic improvement reduces the incentive to commit property crimes (Lin, 2008; Baker, 2015; Freedman and Owens, 2018; Carr and Packham, 2019; Watson et al., 2019), but has little to say about the local economic effects of seasonal agricultural activity. We offer some supporting evidence of broadly improved economic conditions by re-estimating equation (2) for total non-agricultural employment. Figure 8 shows that non-agricultural employment peaks in tandem with peak seasonal employment. The fact that non-agricultural employment follows the same inverted "V" pattern as the seasonal employment share could be indicative of increased population driving service sector employment, upstream or downstream linkages to the agricultural sector, or both.

Figure 6: Property Crime, Semi-Parametric Results



Notes: The graph plots coefficients and 95% confidence intervals from estimating Equation (2). The dependent variables are indicated in the figure headers.

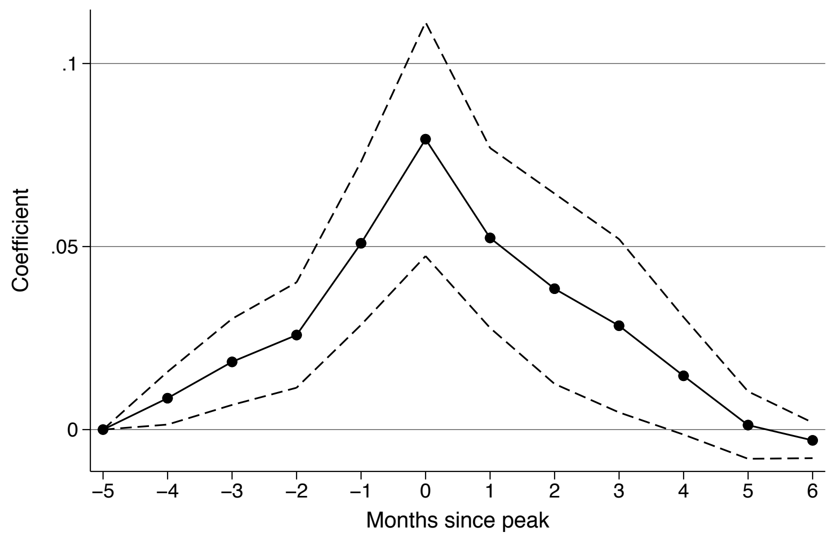
Figure 7: Violent Crime, Semi-Parametric Results



Notes: The graphs plot coefficients and 95% confidence intervals from estimating Equation (2). The dependent variables are indicated in the figure headers.

Effects on violent crime rates are largely insignificant, though there is a positive and statistically significant estimate for two months before peak. Similar to the results for property crimes, the log of the violent crime rate shows a significant dip corresponding to the peak month. The difference in results when using log of violent crime rate could indicate that counties with more seasonal agricultural labor tend to have lower violent crime rates overall and experience large percentage drops in violent crime rates during labor-intensive seasons. For log violent crime counts, there are no statistically significant effects. This is somewhat in contrast to the results using the parametric specification, which showed a small, statistically significant increase in the log violent crime count associated with increased seasonal farm employment. Overall, the semi-parametric results are consistent with the parametric results in Table 3, with the exception of finding no statistically significant effects on violent crime counts.

Figure 8: Non-Agricultural Employment



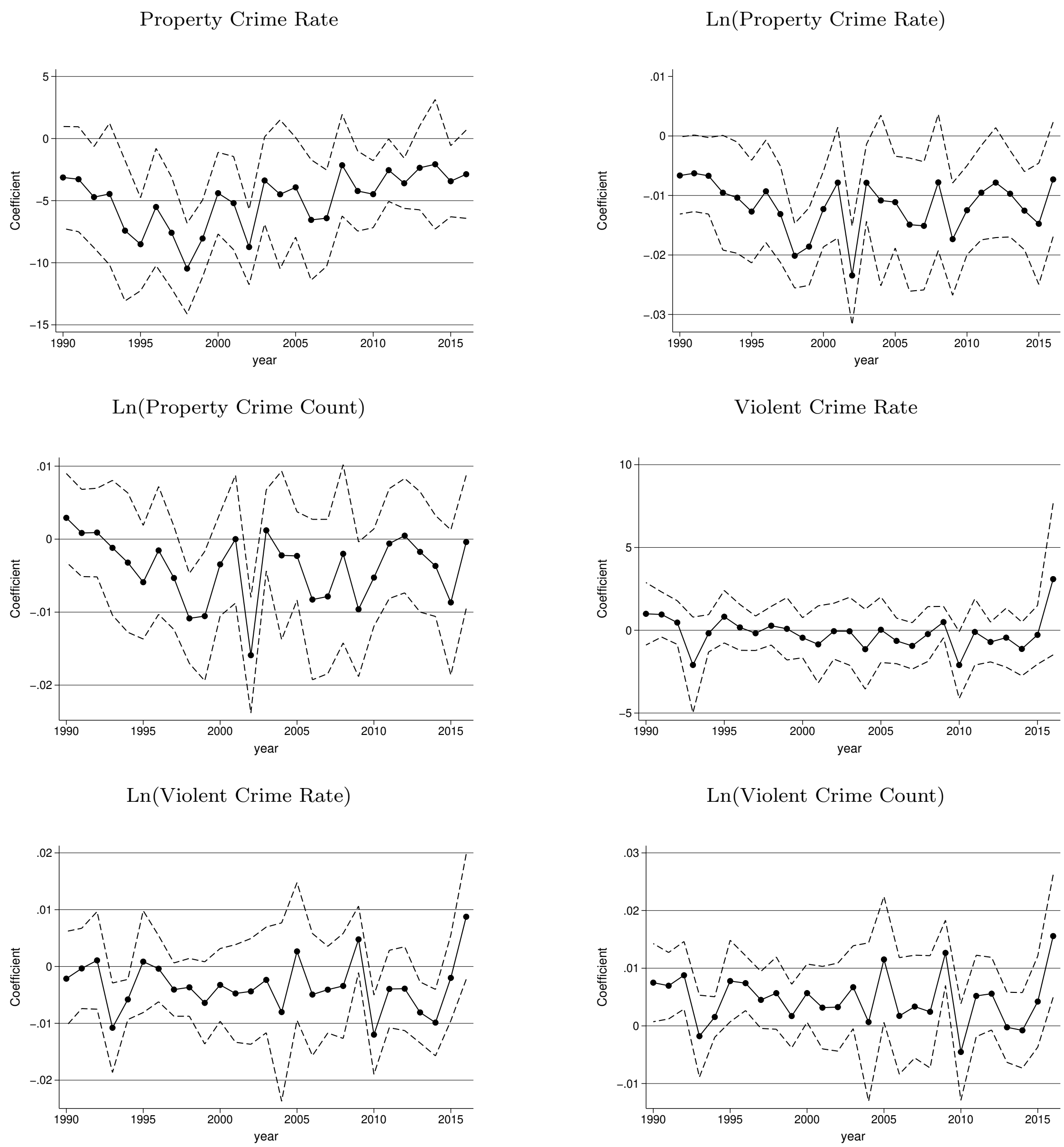
The graph plots coefficients and 95% confidence intervals from estimating Equation (2) with the natural log of non-agricultural employment as the dependent variable.

5.3 Extensions and Robustness

5.3.1 Year-by-Year Estimates

Because the identifying variation in our baseline specification shown in Equation (1) is within-year variation, we can identify effects separately for each year, and examine any patterns between 1990-2016. This may be consequential since migration of farm workers declined significantly over this time, due in part to demographic changes in the farm workforce (Fan et al., 2015). We do this for each of our six main outcomes in Figure 9. Estimates are generally consistent with the overall effects shown in Tables 2 and 3, and fairly trendless throughout the sample period. One exception is that property crime effects are trending down in the first six years of the sample (and non-transformed property crime rate effects gently trend up thereafter, though remain negative throughout). Also, violent crime effects experience a large positive spike in 2016, the last year of the sample.

Figure 9: Crime Effects by Year



Notes: The graphs plot coefficients and 95% confidence intervals from estimating Equation (1) separately for each year from 1990-2016. The dependent variables are indicated in the figure headers.

5.3.2 Labor Force Denominator Adjustment

As discussed in Section 4.1, a key component of our identification strategy is the use of changes in labor force as a proxy for changes in population. However, if seasonal agricultural workers who are settled in the same county where they perform seasonal work do not work at all for some share of the year, they may not be counted in the labor force in some months when they are still residing in the county. Crime rates for those months will be artificially inflated, since the denominator will be artificially small. To address this we obtain rough estimates of how much our denominator is spuriously reduced by this issue through the NAWS, which asks respondents whether they are settled in their place of employment and how many weeks they did not work in the previous year. We use these to create region-by-year[23] estimates of the percentage of the seasonal farm workforce that is settled and average weeks per year not working.

For each observation we calculate an adjusted labor force estimate using the following equation:

$$Adjusted\_LF_{imy} = LF_{imy} + (max\_seas_{iy} - current\_seas_{imy}) * pct\_settled_{ry} * (weeks\_nw_{ry}/52) \quad (3)$$

LF_{imy} is the original labor force estimate provided by the BLS in county i, month m, and year y. $max\_seas_{iy} - current\_seas_{imy}$ is our estimate of seasonal workers that are not currently working their seasonal jobs. We make the assumption that the total number of individual seasonal farm workers within a county-year is the number of seasonal workers in the peak month for that year (this is max_seas_{iy}). We subtract from max_seas_{iy} the number of seasonal workers in month m year y, so the difference is seasonal workers not in their seasonal jobs. $pct\_settled_{ry}$ is the NAWS region-by-year estimate of the percentage of seasonal workers that are settled in the place of their seasonal employment. $weeks\_nw_{ry}/52$ is the NAWS region-by-year average number of weeks spent not working at all among settled seasonal workers divided

[23]NAWS reports data at the regional level. There are six NAWS regions, and the NAWS is intended to be regionally and nationally representative.

by 52. We thus add to the original BLS labor force an estimate of the number of seasonal workers who are currently in the same county but not working, as these are the workers who will be under-counted in the original labor force figure.

We rerun our baseline regressions from Tables 2 and 3 with crime rates and seasonal worker share calculated with these adjusted labor force estimates.[24] These results are shown in Tables 4 and 5. Similar to our main findings, we find a statistically significant negative association between seasonal farm labor shares and property crime rates in Table 4,[25] though the point estimate is slightly smaller in magnitude, as expected. We find a statistically significant positive association with logged violent crime counts, but no significant associations with violent crime rates, again similarly to our baseline results.

Table 4: Property Crime Results, Adjusting for Seasonal Workers Settled but not Working

	(1) Property Crime Rate	(2) Ln(Property Crime Rate)	(3) Ln(Property Crime Count)
Adjusted Seasonal emp. share	-4.540***	-0.012***	-0.005
	(1.553)	(0.004)	(0.003)
N	837084	813747	813747

Notes: The table reports estimates when adjusting the measure of labor force as described in the text. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

Table 5: Violent Crime Results, Adjusting for Seasonal Workers Settled but not Working

	(1) Violent Crime Rate	(2) Ln(Violent Crime Rate)	(3) Ln(Violent Crime Count)
Adjusted Seasonal emp. share	0.380	-0.001	0.006***
	(0.509)	(0.002)	(0.001)
N	812772	764427	764427

Notes: The table reports estimates when adjusting the measure of labor force as described in the text. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

[24]The crime count variable is unchanged by this adjustment, but we still rerun the count regressions on the adjusted seasonal farm labor share variable.

[25]Note that the sample sizes are slightly smaller in these tables compared to our baseline regressions, due to the exclusion of Alaska and Hawaii in the NAWS.

5.3.3 Employment Suppression Robustness Check

As mentioned in Section 4, one caveat to this study is that the QCEW suppresses employment data at the county-industry-year level in cases where firms could be identifiable, which typically means cases where there are a small number of firms. Our measure of seasonal agricultural labor share will be too low in cases where any of our twelve seasonal sectors have suppressed employment data. However, almost by definition, employment suppression overwhelmingly occurs in cases with a very small number of firms in the sector. Across observations in our main sample, the average number of firms in a seasonal agricultural sector that is suppressed is 2.6 (the number of firms is still provided for suppressed sectors), while the average number for non-suppressed (and non-zero) sectors is 25.9. Therefore this issue should typically only cause under-measurement of seasonal sectoral shares in cases where the sectoral employment is quite low (but non-zero). The exception is cases where a single firm employs a very large number of people and is suppressed, though for this phenomenon to cause bias in our estimates it would have to be somehow related with seasonal crime rates. For these reasons we do not see the suppression issue as a significant threat to the validity of our estimates.

Nevertheless, we perform a highly conservative robustness check in which we drop all observations where any one of our 12 seasonal sectors is suppressed. Because these are often small sectors, and are also fairly common even outside of major agricultural regions, this strict condition drops roughly 40% of county-year combinations in the sample. Even so, the results shown in Tables 6 and 7 are qualitatively similar to the full sample.

Table 6: Property Crime Results, Observations with Suppressed Agricultural Employment Data Excluded

	(1) Property Crime Rate	(2) Ln(Property Crime Rate)	(3) Ln(Property Crime Count)
Seasonal emp. share	-5.938***	-0.011***	-0.003
	(1.731)	(0.003)	(0.003)
N	521824	502648	502648

Notes: The table reports estimates when dropping observations with suppressed agricultural employment data. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

Table 7: Violent Crime Results, Observations with Suppressed Agricultural Employment Data Excluded

	(1) Violent Crime Rate	(2) Ln(Violent Crime Rate)	(3) Ln(Violent Crime Count)
Seasonal emp. share	-0.064	-0.003	0.005***
	(0.469)	(0.002)	(0.001)
N	500182	460222	460222

Notes: The table reports estimates when dropping observations with suppressed agricultural employment data. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

5.3.4 Contract Labor

In 2012, workers hired through Farm Labor Contractors (FLCs) accounted for 19 percent of total labor expenditures, 38 percent of all fruit and nut labor expenditures, and 31 percent of vegetable and melon labor expenditures (Zahniser et al., 2018). FLCs are intermediaries who hire farm workers directly and contract labor to farms. FLCs may reduce the costs of recruiting workers directly, especially if there are substantial frictions in the labor markets that prevent the matching of workers to farms during peak seasonal labor demands. Growers may also prefer to hire workers through FLCs to manage and mitigate risks associated with hiring unauthorized immigrants. Taylor and Thilmany (1993) find suggestive evidence that FLCs may be willing to take on more risk than farm employers since they can more easily hide from immigration enforcement. FLCs are constantly transporting workers from one location to another and it is relatively easy for them to close their business and reopen under a new name.

Although the QCEW records the number of FLC employees per county each month, FLC employees may not work or reside at the address of the FLC. Surveys conducted with FLCs in Florida indicate that USDA Department of Labor regulations implemented in 2012 to limit the transport of H-2A workers to within 60 miles of their housing severely restricted the movement and profitability of FLCs who hired H-2A workers (Roka et al., 2017). To account for potential measurement error in the number of seasonal farm employees located in each county, we construct an alternative measure in which we impute the number of contracted workers by county-year using data on contracted labor expenses from the Agricultural Censuses, which we have for every five years from 1987-2017.

For a given census year, we calculate each county's share of its state's total contract labor expense. Since the agricultural census is every five years, we impute expense shares for missing years by linear interpolation. We then find for each month the total number of QCEW FLC employees for a state, and assign each county a share of these employees according to its share of contract labor expense. We then include this alternative measure of contract laborers in our estimate of total seasonal agricultural laborers, rather than the county-level FLC counts from QCEW. This method assumes that the contract employees work in the same state as the address of the FLC, but this is subject to relatively small measurement error. The correlation between the QCEW figure and our alternative measure is .95.

Unsurprisingly, given this high correlation, the results using the alternative measure shown in Table 8 and Table9 are very similar to our baseline results.[26]

Table 8: Property Crime Results, Alternative Measure of Contract Workers

	(1) Property Crime Rate	(2) Ln(Property Crime Rate)	(3) Ln(Property Crime Count)
Seasonal emp. share	-5.018***	-0.013***	-0.004
	(1.347)	(0.003)	(0.003)
N	834552	811371	811371

Notes: The table reports estimates when using seasonal labor share constructed with the alternative measure of contract workers, as described in the text. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

Table 9: Violent Crime Results, Alternative Measure of Contract Workers

	(1) Violent Crime Rate	(2) Ln(Violent Crime Rate)	(3) Ln(Violent Crime Count)
Seasonal emp. share	-0.122	-0.004**	0.005***
	(0.408)	(0.002)	(0.001)
N	810636	762751	762751

Notes: The table reports estimates when using seasonal labor share constructed with the alternative measure of contract workers, as described in the text. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

[26]Note that sample sizes are slightly smaller in these regressions due to some missing counties in the Agricultural Census.

5.3.5 H-2A Workers

In recent years, agricultural employers have increasingly hired farm workers through the H-2A agricultural guest worker program. Employers can apply for H-2A visas prior to the season when workers are needed.

Some, but not all, states report H-2A workers in the QCEW. All H-2A workers are exempt from the federal FUTA tax, which supports unemployment insurance administration. However, states have differing policies regarding whether employers are required to report employment and earnings and pay Unemployment Insurance taxes for H-2A workers. For example, California and Washington require employers to report employment and earnings of H-2A workers in the QCEW while Florida does not.[27]

To our knowledge, there is no database identifying which states require employers to report employment and earnings of H-2A workers. Nevertheless, H-2A workers represent a small share of total seasonal farm workers–only 7 percent of the crop workforce in 2016 even though H-2A jobs had increased 160 percent from 2006-2016 (Martin, 2017). Consequently, we expect measurement error arising from omitted H-2A workers in some states to have little impact on our main findings. Nevertheless, we conduct two robustness checks related to H-2A employment.

In the first robustness check, we limit our sample only to counties located in California and Washington where we know that H-2A workers are included in the QCEW. Results are qualitatively similar to our main specifications. Table 10 shows a statistically significant negative association between farm labor share and logged property crime rates in the limited geographic sample. Table 11 shows a statistically significant negative association between farm labor share and log violent crime rate but a statistically significant positive association between farm labor share and log violent crime count. This suggests that the number of violent crimes rises with increased farm labor share in California and Washington, but by less than the proportional increase in population.

Second, we measure the effects of H-2A share of the labor force on county crime rates.

[27]Florida is already omitted from our analysis due to irregular UCR data (See Section 4).

Table 10: Property Crime Results, California and Washington Only

	(1) Property Crime Rate	(2) Ln(Property Crime Rate)	(3) Ln(Property Crime Count)
Seasonal emp. share	-2.136	-0.005**	0.004
	(1.944)	(0.002)	(0.003)
N	31404	31334	31334

Notes: The table reports estimates when limiting the sample to California and Washington. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

Table 11: Violent Crime Results, California and Washington Only

	(1) Violent Crime Rate	(2) Ln(Violent Crime Rate)	(3) Ln(Violent Crime Count)
Seasonal emp. share	-0.656	-0.003*	0.006***
	(0.459)	(0.002)	(0.001)
N	31344	31075	31075

Notes: The table reports estimates when limiting the sample to California and Washington. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

There are several reasons H-2A workers may affect crime rates differently than other seasonal farm workers. First, all H-2A workers have temporary legal work visas. Incentives to commit crime and potential consequences may differ across work status, particularly if H-2A workers want to have their work visas renewed the following year. Second, H-2A workers are not legally able to remain in the United States after their visa expires, so H-2A workers may have lower social accountability in the community compared to other seasonal farm workers. Third, employers are required by law to pay for the transport of H-2A workers from and to their country of origin and provide worker housing. Consequently, economic incentives to commit crimes may differ for H-2A workers.

The results from estimating Equation (1) using the monthly H-2A worker share of the labor force are shown in Tables 12 and 13. We find a statistically significant negative association between the H-2A employment share and the logged property crime rate and logged property crime count (Table), similar to our findings in our main results. Table shows no significant association between H-2A employment share and violent crime rates or counts.

Table 12: Property Crime Results, H2A Workers

	(1) Property Crime Rate	(2) Ln(Property Crime Rate)	(3) Ln(Property Crime Count)
H2A emp. share	0.113	-0.005**	-0.004**
	(1.506)	(0.002)	(0.002)
N	297528	288361	288361

Notes: The table reports estimates when using the share of H-2A workers in the labor force. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

Table 13: Violent Crime Results, H2A Workers

	(1) Violent Crime Rate	(2) Ln(Violent Crime Rate)	(3) Ln(Violent Crime Count)
H2A emp. share	1.728	0.003	0.003
	(1.447)	(0.002)	(0.002)
N	291540	275527	275527

Notes: The table reports estimates when using the share of H-2A workers in the labor force. The dependent variable is given in the column header. Each regression includes month-year and county-year fixed effects. Standard errors clustered by county are given in parentheses. *, **, *** represent significance at 10%, 5% and 1%, respectively.

5.3.6 Evidence on Share of Crimes Reported to Police

The UCR data record only crimes that are reported to the police. Increases in population through seasonal migration may affect crime rates through changes in the actual number of per capita crimes committed or through changes in rates of reporting crime given that a crime was committed. Non-citizens or unauthorized immigrants may be less likely to report crimes if they feel less trustful of the police. Since a greater share of seasonal farm workers are unauthorized immigrants and non-citizens than the U.S. population at large, this may be an important mechanism in the effects of migrant farm labor shares on local crime rates.

We compare differences in the probability that Hispanics and non-Hispanics report violent crimes,[28] personal theft,[29] burglary, and motor vehicle theft[30] to the police from 1992-2016 using the National Crime Victimization Survey (NCVS). The NCVS is an annual survey conducted by the Bureau of Justice Statistics with a nationally representative sample of in-

[28]Violent crimes include rape, attempted rape, sexual attack with serious or minor assault, completed robbery with injury from assault, aggravated assault, unwanted sexual contact, and verbal threat of rape or assault.

[29]Personal theft includes completed or attempted purse snatching, completed pocket picking, and completed or attempted personal larceny.

[30]We include only incidents of completed burglary and motor vehicle theft.

dividuals. The primary advantage of the NCVS is that it collects information on crimes that are not reported to the police, so we can evaluate reporting rates for different demographics. The NCVS collects information on respondents' race, but it does not contain any information on immigration status and geographic identifiers are available only at the regional level in the public-use data.

We present percentage of crimes reported by type of crime for Hispanics and non-Hispanics for the years 1992-2016 in Table 14. Hispanics are more likely to report violent crimes to the police and less likely to report incidents of personal theft, and these differences are statistically significant (reporting rates for burglary and vehicle theft are not significantly different). It is possible that due to this issue our estimates of effects of seasonal agricultural activity on violent crimes are biased upwards, while estimates of property crimes are biased downwards. However, the differences in reporting rates are qualitatively quite small.

Table 14: National Crime Victimization Survey Comparison of Means

Crime Type	Share of Incidents Reported if Hispanic	Share of Incidents Reported if Non-Hispanic	Difference (Hisp. − Non-Hisp.)	Observations
Violent Crime or Attempt	0.456	0.444	0.012*	51,366
	(0.007)	(0.003)	(0.007)	
Personal Theft	0.260	0.301	-0.041***	74,501
	(0.005)	(0.002)	(0.005)	
Burglary	0.533	0.551	-0.018	29,015
	(0.009)	(0.003)	(0.010)	
Motor Vehicle Theft	0.931	0.913	0.018	6,964
	(0.009)	(0.004)	(0.010)	

Notes: Standard errors are reported in parentheses. Differences and p-values are derived from the linear regression of the variable of interest as the dependent variable on a binary variable equal to 1 if the respondent was Hispanic and 0 otherwise.

6 Conclusion

We estimate the effect of labor-intensive seasonal agricultural activity on crime, and to the best of our knowledge, we are the first to do so. Our analysis is motivated by the observation that many Americans think immigrants—and undocumented immigrants in particular—are more likely to commit crimes than natural-born citizens. Roughly half of seasonal farm laborers in the United States currently lack legal immigration status.

We observe both criminal activity and agricultural activity—measured as the seasonal agricultural labor share—by month and U.S. county. The richness of our data allows us to leverage seasonal variation in agricultural activity while controlling for any unobserved factors that are fixed within a county in a given year. Consistent with previous findings that migrants commit crimes at a lower rate than natural born citizens (Reid et al., 2005; Butcher and Piehl, 2007; Wadsworth, 2010; Chalfin, 2014), we find that increased seasonal farm labor employment is associated with lower property crime rates. We document mixed evidence that the property crime count actually falls as the seasonal farm labor share increases. To the extent that seasonal agricultural production is associated with broad local economic improvement (which we do find evidence of), this result is consistent with the idea that economic success reduces the incentive to commit crimes (Lin, 2008; Freedman and Owens, 2016; Carr and Packham, 2019; Watson et al., 2019). Taken together, we find that labor intensive agricultural activity is not associated with increased violent or property crimes, and that concerns to the contrary are largley unwarranted.

It is interesting to compare our findings with those from the literature on crime effects of the shale boom, which also increased economic activity and involved an influx of outside labor. James and Smith (2017) find that the shale energy boom increased rates of property crimes as well as some violent crimes (aggravated assault) by roughly 10%-20%. Consonant results are documented by Gourley and Madonia (2018) and Komarek (2018) in their analyses of the Colorado and Pennsylvania shale booms, respectively. Why do our results differ from these other estimates? James and Smith (2017) hypothesize that labor migration might play a key role in explaining their results and it is certainly possible that migrants to energy boom towns are more prone to criminal activity than migrants to agricultural communities. But other possible explanations also exist. The two-week on, one week off structure of the work week is unique to the oil and gas industry and this may contribute criminal activity, especially when coupled with relatively large pay checks and various forms of social disorganization. Regional energy booms also occurr suddenly, are often unexpected, and are typically not reoccuring in any systematic way. These features make investing in risk management strategies more challenging as adapting to risk takes time. These same effects do not carry over to the agricultural sector as picking season is fully anticipated and re-occuring. Understanding the mechanisms behind these observed patterns of criminal activity is important, and something we leave to future research to explore.

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