Seasonal Agricultural Activity and Crime

Diane Charlton* Alexander James† Brock Smith‡§

August 16, 2021

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

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

Abstract

Sudden shocks to labor demand have sometimes been shown to increase local crime rates. We build on this literature by estimating the causal effect of labor-intensive seasonal agricultural activity on crime. We analyze a unique data set that describes criminal activity and fruit, vegetable, and horticultural (FVH) employment by month and U.S. county from 1990-2016. We find the FVH labor share is associated with reduced property and violent crime rates, and possibly the number of property crimes committed within county-years. Examining heterogeneities based on ethnicity, labor-intensive FVH activity decreases the rate of non-Hispanic arrests and victimization and increases the number of Hispanic arrests and victims (consistent with rising local Hispanic populations). Taken together, results are broadly consistent with the idea that agricultural harvest of labor-intensive crops enhances local labor market opportunities that reduce incentives to commit crimes. Results are robust to a battery of alternative specifications that address the inherent challenges associated with measuring seasonal agricultural labor.

*Department of Agricultural Economics & Economics, Montana State University. Bozeman, MT., 59717 *Corresponding Author. Email: diane.charlton@montana.edu*

†Department of Economics, University of Alaska Anchorage. Anchorage, AK. 99508.

‡Department of Agricultural Economics & Economics, Montana State University. Bozeman, MT., 59717.

§This research were funded in part by the USDA National Institute of Food and Agriculture (NIFA), Hatch projects MONB00100 and 1026193.

Fruit, vegetable, and horticultural (FVH) production is characterized by large seasonal changes in labor demand, much of which is met by migrant laborers. An estimated 38% of seasonal FVH workers were classified as migratory from 1990-2016, and approximately 48% were unauthorized immigrants over the same span.[1] Many Americans believe that immigrants, and especially "illegal" immigrants are more likely to commit violent crimes than the rest of the US population.[2] Such sentiments—along with anecdotal evidence—can lead to fear that agricultural activity causes crime. 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 knives used to harvest head lettuce during the spring combined with high poverty and crime (Martin, Fix, and Taylor 2006). Further to the west, 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 subsequently reduce home values (Mohan 2017). Nevertheless, rigorous investigation is required to test whether these fears and beliefs can be validated by a causal relationship between seasonal farm activities and crime. In this paper, we identify the short-term effects of seasonal, labor-intensive FVH activity on crime using monthly sector-specific employment data and crime reporting data at the county level from 1990-2016.

[1]Based on authors' analysis of the National Agricultural Workers Survey (NAWS). The NAWS defines a worker as migratory if they reported jobs that were at least 75 miles apart or who reported moving more than 75 miles to obtain a farm job during a 12-month period.

[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).

The relationship between crime and agricultural activity is theoretically ambiguous. On the one hand, migrant farm workers are predominantly male, and tend to be poor and relatively uneducated—all of which is associated with increased criminal activity (Campaniello and Gavrilova 2018; Kelly 2000; Lochner and Moretti 2004).[3] On the other hand, there is empirical evidence that immigrants are equally (Reid et al. 2005; Bell, Fasani, and Machin 2013; Bianchi, Buonanno, and Pinotti 2012; Chalfin 2014) or less (Baker 2015; Butcher and Piehl 2007; Stowell et al. 2009; Wadsworth 2010) likely to commit crimes than natural-born citizens, and an estimated 78% of FVH workers in the United States were foreign born between 1990-2016.[4,5] To the extent that agricultural activity creates economic spillovers that enhance local labor market opportunities (which we do find evidence of), this could also reduce incentives to commit crimes (Blakeslee and Fishman 2018; Carr and Packham 2019; Freedman and Owens 2016, 2018; Foley 2011; Gould, Weinberg, and Mustard 2002; Lin 2008; Watson, Guettabi, and Reimer 2019).[6] However, in certain instances significant and sudden increases in economic activity have also been shown to increase crime, as with the U.S. shale energy boom (Gourley and Madonia 2018; James and Smith 2017; Komarek 2018; Street 2018), a result James and Smith (2017) hypothesize could be explained by migration and subsequent

[3] 71.1 percent of FVH workers in 2016 who reported working the previous year had an annual income below $25,000, and 33.4 percent had income below $15,000. In the online supplementary appendix, we summarize statistics on income distribution, labor migration, weeks not working, and other variables of potential interest for FVH workers in the National Agricultural Workers Survey (NAWS).

[4] Based on authors' analysis of the Department of Labor, Employment & Training Administration (2017) for fiscal year 2016.

[5] 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 financially-motivated crimes.

[6] Whether agricultural activity creates meaningful short run economic spillovers remains an open question. However, the literature finds little to no evidence of long-run economic spillovers from the agricultural sector to other local non-farm sectors (Foster and Rosenzweig 2004; Hornbeck and Keskin 2015; Weber et al. 2015)

"social disorganization" in which the sudden inflow and outflow of people disrupts social cohesion, making it easier to successfully commit crimes without being caught (Freudenburg 1986; Sampson and Groves 1989).[7] It is also possible that migration increases crime rates among non-immigrant populations. For example, immigration has been shown to increase crime rates among non-migrant residents who face increased employment competition (Borjas, Grogger, and Hanson 2010). Furthermore, migrant farm workers often lack (perceived or actual) legal protections, thus potentially contributing to their frequent victimization (Moyce and Schenker 2018; Wallis 2019).

Despite these theoretical ambiguities and the attention this topic has received in popular media outlets, to our knowledge we are the first to examine how seasonal labor-intensive agricultural activity impacts local crime rates.[8] We fill this gap by combining Uniform Crime Reporting (UCR) data on crime counts and seasonal agricultural employment at the county-by-month level over the period 1990-2016. We analyze how seasonal variation in FVH employment is associated with seasonal patterns of crime. Causal inference is facilitated by the granularity of our data. While there may be unobserved factors that are correlated with agricultural activity and crime (such as population density or income), such factors tend to be specific to particular counties or years. But by exploiting county-by-month data, all identifying variation comes from within the county-year level. Our key identifying assumption is that within county-year variation in unobserved determinants of criminal activity is uncor-

[7] Whereas well acquainted neighbors with established friendships are more likely to keep a watchful eye on their neighbor's house, they may be less likely to recognize an ongoing burglary at a stranger's house. Relatedly, committing a crime in an environment of strong social organization is especially risky as witnesses are more likely to recognize the perpetrator's face and identify them to police.

[8] To our knowledge, the most closely related paper to ours is Blakeslee and Fishman (2018), which estimates effects of weather-driven agricultural income shocks on crime in India. Unlike the present study, Blakeslee and Fishman (2018) analyzes this relationship in a developing country, and does not study seasonal crime effects.

related with variation in our measure of labor-intensive agricultural activity after controlling for month-by-year fixed effects. As long as this assumption holds, we are able to estimate the short run (e.g., within year) causal effect of agricultural activity in FVH sectors on crime, but this comes at the cost of foregoing analysis of longer run, more permanent effects that might also be important.

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. To address this, we supplement our baseline specification with a semi-parametric one that describes how crime rates change each month relative to the month when FVH-intensive counties have their peak seasonal farm workforce.

We find that a one percentage point increase in the seasonal FVH employment share is associated with roughly five fewer property crimes per 100,000 members of the labor force[9] (compared to a sample average of 388 property crimes per 100,000). Consonant results are found for the violent crime rate, though with inconsistent statistical significance. Our semi-parametric specification reinforces these findings. Relative to five months before peak seasonal employment, property and violent crime rates are roughly 12% lower during the peak seasonal employment month in treated counties (defined as those with significant seasonal FVH employment shares) relative to control counties.

We also investigate potential mechanisms driving these negative crime effects. As dis-

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

cussed above, two possibilities are that migrant workers are less likely to commit crimes than the non-migrant population, and that the non-migrant population may commit fewer crimes during seasonal FVH activity due to economic spillovers. To explore these possibilities we conduct an analysis of National Incident-Based Reporting System (NIBRS) data, which identify (in some cases) the ethnicity of both the victims and arrestees associated with a crime (though the NIBRS program has much lower participation rates from police precincts than the UCR program). A number of important insights emerge. First, during labor-intensive FVH seasons both the victimization and arrest rate fall among non-Hispanics. Since the majority of seasonal agricultural workers are Hispanic, this suggests that the crime rate falls for the non-migrant population during labor-intensive seasons, consistent with the hypothesis that seasonal agricultural activity broadly improves local economic conditions and reduces the incentive to commit crimes. Second, while we do not find effects on Hispanic arrest rates, we find that the Hispanic victimization rate rises in response to seasonal labor-intensive FVH activity. In this case, the corresponding offenders are largely Hispanic, though there is evidence that some of these offenders are also non-Hispanic or of unknown ethnicity.

To further investigate mechanisms, we examine whether crime is affected by corn harvesting season in corn-intensive counties rather than FVH-intensive counties. We choose corn because it is widely grown but is not nearly as labor-intensive to harvest. We do not find evidence of crime effects in this case, suggesting that the crime reductions we find for FVH are driven specifically by labor market impacts rather than overall income effects (assuming that corn harvests provide a contemporaneous increase in income in corn-producing counties).

This study contributes most directly to the literature examining the social and economic effects of agricultural activity. To our knowledge we are the first to analyze how seasonal patterns in labor-intensive agricultural activity are associated with seasonal patterns in crime. We also contribute more generally to the literature examining the effects of labor demand shocks and immigration on local crime rates. As mentioned above, this literature includes analysis of the American shale energy boom, which is associated with increased crime (James and Smith 2017), and also studies of various international immigration shocks, which predominately find a negative association with crime, though results vary.

Data

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.[10] 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 the online supplementary appendix we discuss this issue further and we provide a robustness check in which we drop observations

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

with suppressed seasonal agricultural sectors.

To the extent that employers do not report unauthorized workers for unemployment insurance, we may also under-count seasonal farm workers in the QCEW. However, since the Immigration Reform and Control Act (IRCA) was passed in 1986, employers are held legally responsible for knowingly hiring unauthorized workers (Taylor and Charlton 2018). Thus, farm employers have incentive to require workers to provide some form of legal documentation. Many unauthorized workers provide a false social security number to their employers and would therefore be counted in the QCEW.[11]

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.[12] According to Rural Migration News (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).[13] This may cause us to under-count the seasonal

[11]It is estimated that undocumented immigrants contributed $13 billion to Social Security funds and $3 billion to Medicare in 2016 by submitting false social security numbers to employers for payroll (Roberts 2019).

[12]H-2A is a non-immigrant 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).

[13]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

farm work force in key states, particularly if crew leaders for farm labor contractors (FLCs) are considered individual employers. However, we drop Florida from our analysis due to irregular crime data (see Section), 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. We address the concern that QCEW under-counts seasonal farm workers by repeating our analysis using only counties in California and Washington where we know that all employees must be reported in the QCEW.

Additionally, because H-2A agricultural guest workers could differ from other seasonal farm workers given that they have legal temporary guest visas and are subject to the corresponding regulations, we repeat our analysis using H-2A guest worker shares as the explanatory variable.[14] The results from these robustness checks are reported in the online supplementary appendix.

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). If 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 the number of crimes per total labor force, which is distinct from total employment in that it includes people who

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.

[14]Data on the number of H-2A workers per county-month come from the Office of Foreign Labor Certification (OFLC) Disclosure data. Most observations include the worksite county and state for each H-2A application. However, county names were sometimes misspelled, or employers reported the city in place of the county. Marcelo Castillo (USDA, Economic Research Service (ERS)) generously shared with us the data that he matched to work site county using data matching methods across employers in multiple years.

are not employed but are looking for work, and so is less sensitive to economic swings and more representative of the working-age population, though the caveat remains that seasonal employment spikes could draw in workers who are otherwise out of the labor force altogether. 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 data from the National Agricultural Workers Survey (NAWS) 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 the online supplementary appendix. 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.

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 Clearances by Arrest", which contains counts of reported crimes at the month-by-agency level for several types of offenses. It is important to note that the UCR data is restricted to serious crimes. This is not to say, however, that an abundance of less serious criminal activity is not also important. 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.[15]

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

[15]Missing counties are fairly evenly spread throughout the country geographically (aside from dropping all counties in Florida and Alabama, as discussed below). Since rural counties are more likely to be missing, the most rural states like Mississippi, Montana, and South Dakota have the highest share of missing counties.

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. Our methods of dropping counties with misreported crimes data should only introduce sample selection bias if selection is correlated with both seasonal farm labor share and seasonal patterns of crime, which seems unlikely.

Table A1 provides basic summary statistics for the crime and employment data used in our analyses. 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 discussed in the next section.

Methodology

Production of fruit, vegetable, and horticultural (FVH) crops is characterized by high seasonal variation in labor demand.[16] Low-skilled Mexican immigrants, who make up a large share of seasonal farm workers, respond strongly to geographic variation in labor demand Cadena and Kovak (2016), so locations that experience an especially strong harvest are likely to draw a large number of immigrant workers. We exploit this feature of the farm-worker labor market and estimate the relationship between the seasonal employment share and criminal activity

[16] According to the 2012 Agricultural Census, there were 2.7 million workers hired on farms. 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.

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.

using two different specifications. The first specification (which we call our baseline parametric specification) measures the marginal effect of variation in the seasonal farm labor share 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.

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 the 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.[17] 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.[18]

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

[17]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.

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

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 the total labor force to yield the seasonal share in Equation 1.

Semi-Parametric Specification

Observing that employment in seasonal agricultural sectors typically displays a distinct peak period corresponding to harvest season, we alternatively perform a semi-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.[19] This yields 47 treatment counties, which are shown in red in figure A1. 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.[20] This is not surprising since seasonal farm labor demands are particularly high in these regions.[21] We drop counties that are below the 4% threshold but are above 1%, as these counties are potentially impacted by seasonal labor, though this does not meaningfully change the results.

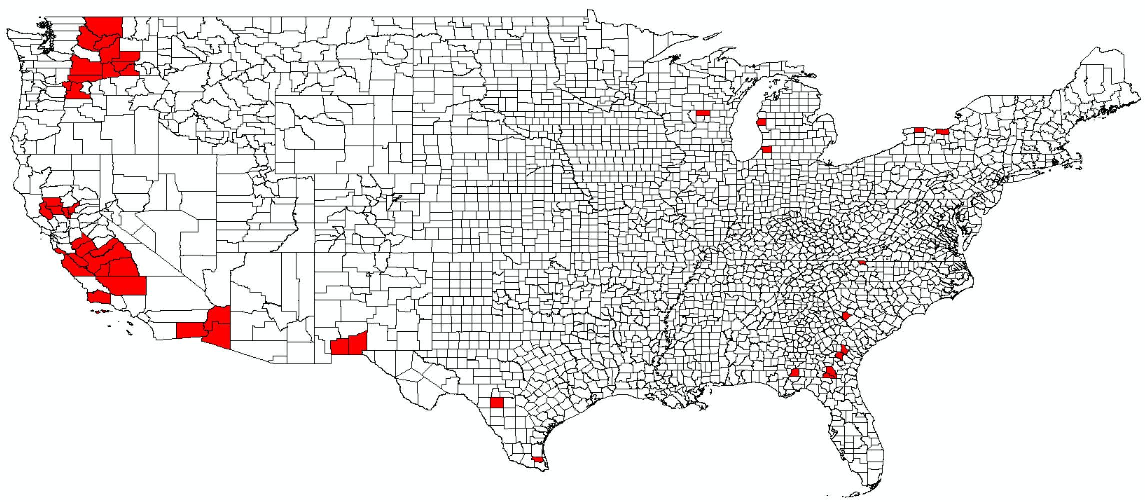


Figure 1: Treatment counties

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.[22][23]

[19]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.

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

[21]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.

[22]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.

[23]Peak seasonal farm employment months generally run from early summer through the fall. For 44 of our

With the treatment group and peak month for each treated county defined, we estimate the following equation:

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

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. Assuming agricultural activity reduces incentives to commit crimes (by enhancing legal economic opportunities) and that migrants are less likely to commit crimes than other groups, then our estimated effects on crime counts and crime rates will be negative. If agricultural activity does not alter local incentives to commit crimes, and migrants commit crimes at similar rates as non-migrants, then our estimated effects on crime counts will be positive but effects on crime rates will be zero. If on the other hand, seasonal workers commit crimes at lower but non-zero rates, effects on crime counts will be positive but effects on crime rates negative. And finally, if seasonal workers commit crimes at

47 treated counties, the peak month is between May and October, with the most common being July.

higher rates or permanent residents commit more crimes in response to increased agricultural activity, then effects on both crime counts and rates will be positive. Importantly, we should not expect crime count effects to be negative unless non-migrant residents commit fewer crimes in response to increased agricultural activity.

Results

We present the findings from our primary specifications in the sections that follow. First, we present the results from the parametric specification and then the results from the semi-parametric specification.

Baseline Parametric Specification Results

Panel A of Table A2 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 as the size of the labor force increases.

Panel B of Table A2 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.[24] Unlike for property crimes, here we do find a positive and significant increase in the count of violent crimes. The estimate of 0.004 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; 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.

Semi-parametric Results

Before presenting the results of the semi-parametric specification (Equation (3)), we first demonstrate that our definition of seasonal labor described in Section indeed produces a distinct spike in observed seasonal farm labor in our treatment counties. Figure A2 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

[24]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 2: Property & Violent Crime Results

	A. Property Crime Results		
	(1)	(2)	(3)
	Property Crime Rate	Ln(Property Crime Rate)	Ln(Property Crime Count)
Seasonal emp. share	-4.894***	-0.012***	-0.004
	(1.277)	(0.003)	(0.003)
N	838,332	814,987	814,987

	B. Violent Crime Results		
	(1)	(2)	(3)
	Violent Crime Rate	Ln(Violent Crime Rate)	Ln(Violent Crime Count)
Seasonal emp. share	-0.207	-0.004*	0.004***
	(0.407)	(0.002)	(0.001)
N	814,020	765,667	765,667

Notes: Based on county-by-month data from from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. 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.

(or zero "months since peak") than this same difference five months before (the reference category).

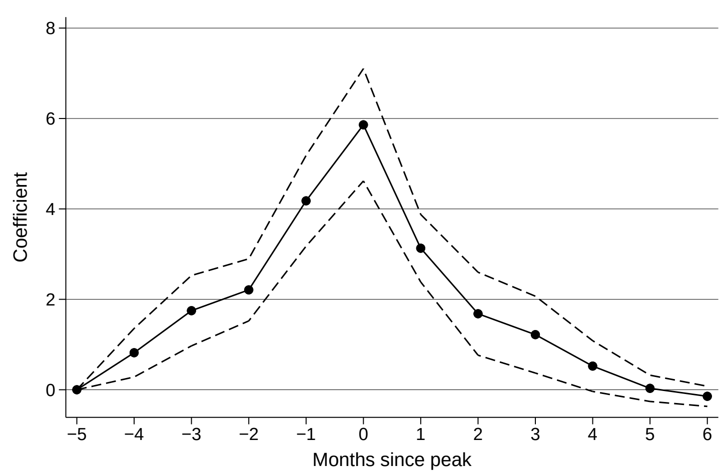


Figure 2: Seasonal agricultural employment share, semi-parametric results

Notes: Based on county-by-month data from from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. The graph plots coefficients and 95% confidence intervals from estimating Equation (2) with seasonal employment share as the dependent variable.

Figure A3 shows our semi-parametric results for property and violent crimes. They are largely consistent with the parametric results shown in Table A2. 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. We explore this idea further in section below.

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

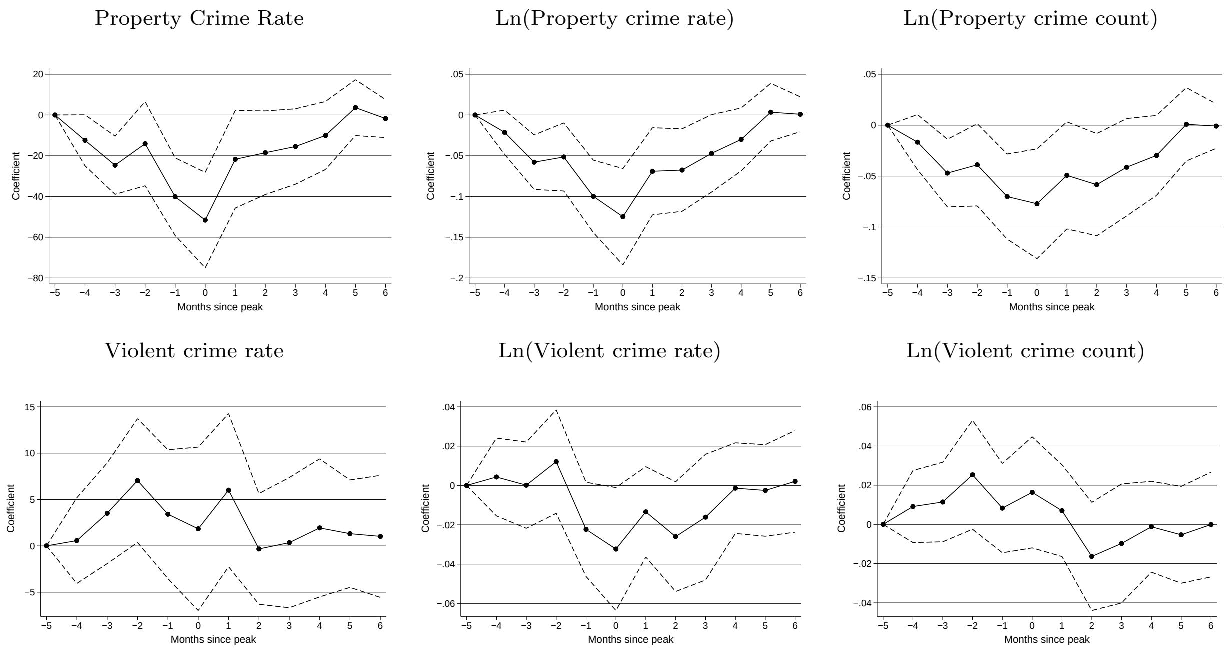


Figure 3: Property & violent crime, semi-parametric results

Notes: Based on county-by-month data from from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. The graph plots coefficients and 95% confidence intervals from estimating Equation (2). The dependent variables are indicated in the figure headers. Standard errors are clustered by county.

in Table A2, with the exception of finding no statistically significant effects on violent crime counts.

Extension: Year-by-Year Estimates

Because the identifying variation in our baseline specification shown in Equation (1) is within-year variation, we can identify marginal effects separately for each year, and test whether effects change from 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 A4. Estimates are generally consistent with the overall effects shown in Table A2, 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.

Mechanisms

As discussed in the Introduction, there are at least two reasons to think that seasonal agricultural activity reduces crime. First, if increased agricultural activity creates non-agricultural sectoral spillovers and improves labor market opportunities, this possibly raises the opportunity cost of engaging in illegal activity and reduces crime. Second, according to some estimates, immigrants are *less* likely to commit crimes than non-immigrants (Butcher and Piehl 2007; Stowell et al. 2009; Wadsworth 2010; Baker 2015), and the large majority of migrant farm workers are immigrants. But these effects may be partially offset by the effects of social disorganization and the inward migration of people possibly prone to committing crimes (as discussed earlier). In addition, non-migrant residents may commit crimes against immigrants either because they are perceived to lack any legal power or means of retribution, or because they view migrants as threats to their security or well-being and react preemptively. While explicitly testing the viability of each of these theories is beyond the scope of this paper, here we explore whether these proposed mechanisms are consistent with available data.

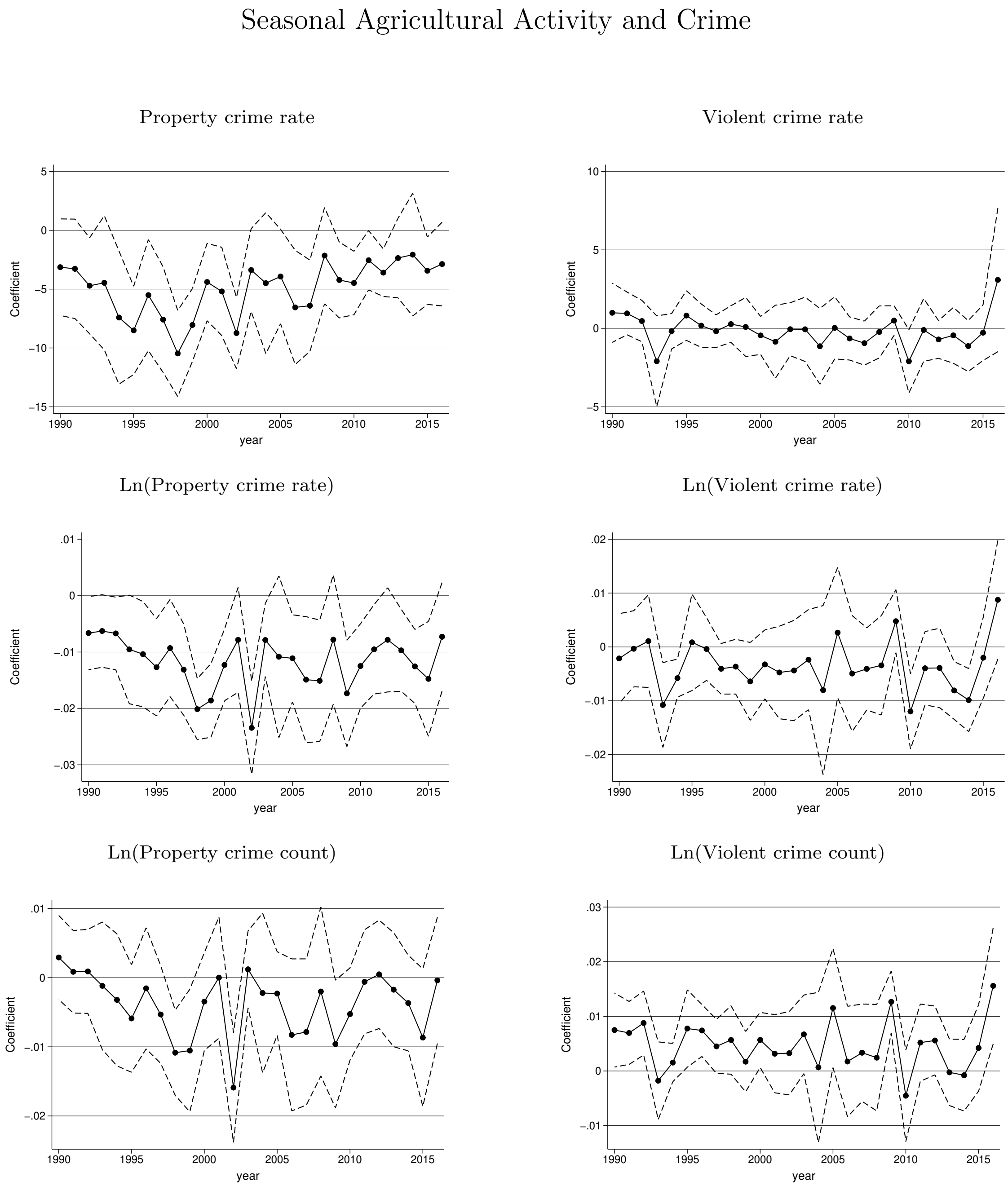


Figure 4: Crime effects by year

Notes: Based on county-by-month data from from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW). 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 below each panel. Standard errors are clustered by county.

Economic Spillovers

First, we investigate whether seasonal agricultural activity creates short-term economic spillovers into non-farm sectors that could reduce the propensity of permanent residents to commit crimes. There are two necessary conditions that must hold for this "economic" channel to explain our core set of results. The first is that broad labor market conditions improve as the seasonal farm-labor share increases. The second is that the non-migrant crime rate falls in response to seasonal agricultural activity.[25] To test the first condition, we re-estimate equation (2) with total non-agricultural employment as the dependent variable. The results are consistent with the idea that seasonal agricultural activity generates local economic spillovers. Specifically, figure A5 shows that non-agricultural employment peaks in tandem with peak seasonal agricultural 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.

Ethnicity of Arrestees and Victims

The second necessary condition for the economic channel to hold is that seasonal agricultural activity reduces the non-migrant crime rate. While we do not observe a non-migrant crime rate, we can use crime data that includes information on ethnicity to shed light on this mechanism. Since 95.3% of migrant FVH workers are Hispanic according to NAWS, the non-

[25]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, Guettabi, and Reimer 2019), but it has little to say about the local economic effects of seasonal agricultural activity.

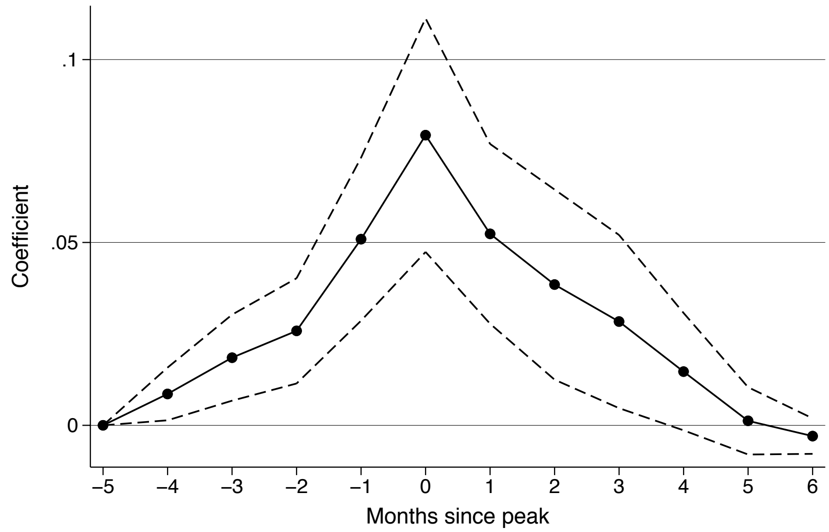


Figure 5: Non-agricultural employment

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. The graph plots coefficients and 95% confidence intervals from estimating Equation (2) with the natural log of non-agricultural employment as the dependent variable. Standard errors are clustered by county.

Hispanic crime rate can serve as a rough proxy for the crime rate of a subset of the population that is non-migrant (only a subset because of course there are non-migrant Hispanics as well). To do this, we use data from the National Incident-Based Reporting System (NIBRS), which provides far more detail on individual crimes than UCR, including the ethnicity (if known) of arrestees and victims. However, since precinct participation is voluntary and imposes a heavier reporting burden, it has much lower coverage than UCR. As of 2016, the last year of our data, precincts representing roughly 31% of the US population participated in NIBRS. We use data going back to 2000, as coverage gets especially sparse going further back (about 15% of the population is covered in 2000). These results should therefore be viewed with caution but do provide interesting suggestive evidence regarding the mechanisms of our negative crime effects.

We identify the count of crimes with at least one Hispanic arrestee and the count with at least one non-Hispanic arrestee at the county-month level. We also identify the count of crimes with at least one Hispanic victim, and the count with at least one non-Hispanic victim. We then run our main specification with these counts or corresponding crime rates as the dependent variable.[26,27]

For completeness, we measure crimes in four ways: 1) crime rate (crimes per labor force), 2) the crime count, 3) the natural log of the crime count, and 4) the inverse hyperbolic sine of the crime count to account for observations with zero crimes.[28] We find that, while increasing the seasonal employment share has no effect on aggregate crimes (see the first row of Table A3), it causes a significant reduction in both the number and rate of crimes committed by non-Hispanics. We also find that the seasonal employment share is negatively associated with the rate of non-Hispanic victimization. Given that such a small fraction of migrant farm workers are non-Hispanic, this is consistent with the idea that the positive economic spillovers documented above reduce the incentive to engage in illegal activity.

In contrast to the results for non-Hispanics, increasing the seasonal employment share increases the number of Hispanic victims, but not the rate of Hispanic victimization. While this may simply reflect an increase in the local Hispanic population, it is worth noting that we do not document a corresponding increase in the aggregate number of Hispanic arrests.

[26] In this context, a crime rate is measured as the number of crimes committed by people of Hispanic (or non-Hispanic) ethnicity, relative to the labor force.

[27] As with the UCR data, we drop all precincts in a given calendar year that do not have full reporting for the year. Any county-year combination with no fully reporting precincts are not included in this analysis.

[28] Zero-crime observations are much more common in this analysis than when using UCR data for a few reasons. First, we only have data for ethnicity of an arrestee, and roughly half of crimes in NIBRS do not result in an arrest. Second, the ethnicity of the arrestee/victim is often not known or otherwise not given. Third, many parts of the country have low Hispanic population.

This is important given that a 1 percentage point increase in the seasonal employment share amounts to roughly 437 additional workers. Even if migrant farm workers committed crimes at a relatively low rate, one might expect to find the number of crimes committed by this group increases during picking season. These results are consistent with the idea that migrant farm workers commit very few crimes such that, even when their population swells, no new crimes can be detected.

We do however find that the number of Hispanic victims increases in response to an increase in the seasonal employment share of the labor force. In the last three panels of Table A3, we analyze crimes that have at least one Hispanic victim and at least one arrestee that is Hispanic, non-Hispanic, or of unknown ethnicity. The effects on crimes against Hispanics appear to be primarily driven by Hispanic offenders. However, depending on how crime counts are measured, there is evidence that some of these crimes are committed by non-Hispanics and people of unknown ethnicity as well. Taken together, these data are consistent with the idea that agricultural activity both creates economic opportunities that reduce crime and attracts a migrant population that is less prone to criminal activity than other groups, at least during the labor-intensive season of agricultural production. The data is also consistent with the idea that migrant farm workers are victimized by both Hispanic and non-Hispanic people.

Effects of Corn Harvests

To further disentangle the economic and migration mechanisms we analyze crime effects of labor shares in corn farming. We choose corn because it is a crop that is grown extensively in the United States, has a common harvest season typically in the Fall, but is not nearly as

Table 3: Effects by Ethnicity of Reported Offender and Victim

	(1) Crime Rate	(2) Crime Count	(3) Ln(Crime Count)	(4) IHS(Crime Count)
All Crimes				
Seasonal Emp. Share	-1.962	0.677	0.002	0.002
	(3.114)	(1.156)	(0.003)	(0.003)
N	251018	251018	246646	251018
Non-Hispanic Arrests				
Seasonal Emp. Share	-1.523***	-0.227***	-0.005**	-0.005**
	(0.358)	(0.068)	(0.002)	(0.002)
N	251018	251018	208520	251018
Hispanic Arrests				
Seasonal Emp. Share	-0.061	0.023	0.006	0.007*
	(0.236)	(0.059)	(0.004)	(0.004)
N	251018	251018	101998	251018
Non-Hispanic Victim				
Seasonal Emp. Share	-4.967***	-0.921*	0.003	0.002
	(1.441)	(0.490)	(0.003)	(0.003)
N	251018	251018	215258	251018
Hispanic Victim				
Seasonal Emp. Share	0.121	0.161**	0.009*	0.013**
	(0.278)	(0.076)	(0.005)	(0.005)
N	251018	251018	109997	251018
Unknown Off|Hisp Victim				
Seasonal Emp. Share	0.034	0.090	0.005	0.010**
	(0.210)	(0.073)	(0.004)	(0.005)
N	246646	246646	99506	246646
Non Hisp Off | Hisp Victim				
Seasonal Emp. Share	-0.018	0.009	0.008***	0.003
	(0.036)	(0.014)	(0.002)	(0.003)
N	246646	246646	50623	246646
Hisp Off | Hisp Victim				
Seasonal Emp. Share	0.113	0.065*	0.009**	0.012***
	(0.087)	(0.035)	(0.004)	(0.004)
N	246646	246646	51257	246646

Note: based on data from the National Incident-Based Reporting System (NIBRS) from 2000-2016. The type of crime for the dependent variable is given in the panel title, and the specific transformation of the type of crime is given in the column header. The "All Crimes" includes crimes for which no arrest is made, while all other panels are based only on crimes that include an arrest. 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.

labor-intensive as the harvest of FVH crops and is not grown in large quantity in counties with high FVH employment. Thus, we should not expect corn production activities to attract many migrant workers or correlate with the timing and location of labor-intensive FVH activities.

Corn harvest therefore provides variation in farm revenue while holding (roughly) constant the migrant farm worker labor share (which does increase during harvest season but at about one tenth of the rate as for FVH sectors). If increased agricultural revenue translates to broad economic improvements and enhanced local labor market opportunities, we expect to find a negative crime effect of corn farming. However, it is also possible that economic spillovers originate from farm worker income, and in this case we should not expect to find significant crime effects of corn farming. The results are shown in Table A4. The estimates are all negative but statistically insignificantly different from zero. This suggests that the economic effects of seasonal agricultural activity on reduced crime are specifically associated with the enhanced employment effects of labor-intensive crops rather than seasonal increases in farm revenues alone.

Table 4: Crimes Results: Corn Employment Shares

	Crime Rate	Ln(Crime Rate)	Ln(Crime Count)
Property Crimes			
Seasonal Corn Emp. Share	-2.665	-0.006	-0.005
	(2.377)	(0.012)	(0.012)
N	838332	814987	814987
Violent Crimes			
Seasonal Corn Emp. Share	-1.564	-0.019	-0.019
	(1.027)	(0.015)	(0.015)
N	814020	765667	765667

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. The table reports estimates when using seasonal corn employment labor force shares as the explanatory variable of interest. 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.

Discussion

It is interesting to compare our estimates to those from analyses of energy booms. (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 related estimates? The answer may be that energy booms appear to attract, rather than create, criminal activity. In fact, Street (2018) finds that the North Dakota oil boom decreased the crime rate among residents that lived in the state prior to the boom (consistent with the idea that improved economic conditions raise the opportunity cost of committing crimes) whereas James and Smith (2017) find that the shale boom had especially large (positive) effects on aggregate criminal activity in North Dakota. Considered jointly, the observed rise in criminal activity in boom towns appears to reflect the inward migration of criminally-prone individuals. Our results are consistent with previous findings that show that economic activity can reduce criminal activity among non-migrant residents (Street 2018) and the literature that finds that foreign-born immigrants are no more likely than natural-born citizens to commit crimes on average (Reid et al. 2005; Butcher and Piehl 2007; Stowell et al. 2009; Wadsworth 2010; Bianchi, Buonanno, and Pinotti 2012; Bell, Fasani, and Machin 2013; Chalfin 2014; Baker 2015).

Robustness

We perform several robustness checks and present the results in the online supplementary appendix. The appendix includes complete explanations and discussions of each robustness check, but we briefly summarize them below.

First, we adjust the labor force denominator to account for seasonal farm workers who might remain in the local region but drop out of the labor force. We use data from the NAWS to estimate the percentage of seasonal farmworkers who are settled in a single location and the mean share of weeks each year that they do not work in any labor sector, and we adjust the labor force denominator accordingly. Results are qualitatively similar to the main results. Second, one might be concerned that employees of FLCs travel to distant farms and do not necessarily work in the county of their employer's address. Thus, we compute the number of FLC workers in each county using the county share of state expenses for contract labor in the Agricultural Census and multiply this share by the total number of FLC workers in the state each month. We use this alternative measure of FLC workers to compute total number of seasonal workers for each observation. Third, we examine the effects of H-2A agricultural guest worker employment shares on crime rates. Fourth, we repeat our analysis when dropping counties with any suppressed FVH employment data. Fifth, we repeat our analysis using the inverse hyperbolic sine transformation in place of the natural log. Sixth, we repeat our analysis dropping observations from the election year 2016. Seventh, we repeat our analysis weighting for the total labor force using weighted least squares. Eighth, we repeat our semi-parametric specification using only counties that rank among the top 5% for average employment in corn production as the control group. Our baseline conclusions prove to be quite robust.

Next, in the appendix we investigate whether Hispanics are more or less likely than non-Hispanics to report being victim to different types of crimes according to the National Crime Victimization Survey. Our findings show that Hispanics who were victim to violent crimes (or attempts) were 1.2 percentage points more likely to report the crime than non-Hispanic victims. However, Hispanics who were victims to personal thefts were 4.1 percentage points less likely to report the crime than non-Hispanic victims. Both of these differences are statistically significant, though qualitatively quite small, and we find no statistically significant difference in the probability of reporting burglary or motor vehicle theft. If seasonal farm workers are more or less likely to report crimes than other residents, this could be problematic for causal identification in our analysis. Nevertheless, the findings from the NIBRS analysis in table A3 show that seasonal farm labor is associated with a significant decrease in *non-Hispanic* crime victimization. Since few non-Hispanics are seasonal farmworkers, this result cannot be explained by possible differences in the probability that farmworkers report crimes, thus validating the credibility of our main findings even though there is a possibility of relatively small measurement error. Nevertheless, we note that crimes committed against farmworkers, who may have significant obstacles to press charges against offenders[29] are of utmost importance, even if these crimes mostly go undetected by the communities who host seasonal farmworkers and cannot be quantified in available data.

[29]see for example Soriano (2020).

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, in part, by the observation that many Americans think immigrants—and undocumented immigrants in particular—are more likely to commit crimes than natural-born citizens, and that many Americans associate seasonal farm labor with drudgery[30] and crime more generally.

We observe both criminal and labor-intensive agricultural activity—measured as the seasonal FVH 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. We find that increased seasonal FVH labor employment reduces the property crime rate, and has little or no effect on the violent crime rate. We also find that it reduces both the rate of crime and of victimization among the non-Hispanic population (our proxy for the non-migrant farm worker population). Taken together, our results are consistent with idea that agricultural activity improves local economic conditions, which we provide evidence of, and that this reduces the incentive to commit property crimes (Lin 2008; Freedman and Owens 2016; Carr and Packham 2019; Watson, Guettabi, and Reimer 2019). Contrary to potential fears, we find that labor intensive agricultural activity is not associated with increased violent or property crimes and thus, concerns to the contrary are largely unwarranted.

One caveat to our analyses and conclusion is that we do not know the extent to which crimes committed against migrant farm workers go unreported. As such, it is possible that

[30] See for example, (Friendly, Murrow, and Lowe 1960; Steinbeck 1939)

seasonal agricultural activity is associated with crimes that are undetected in our data. For example, there is anecdotal evidence suggesting that female farm workers are often victims of sexual assault, and the problem may be worse for migrant female workers (Soriano 2020). We hope that our work will help motivate others to explore this issue further.

References

Baker, S. 2015. "Effects of immigrant legalization on crime." *American Economic Review* 105:210–213.

Bell, B., F. Fasani, and S. Machin. 2013. "Crime and Immigration: Evidence from Large Immigrant Waves." *The Review of Economics and Statistics* 95:1278–1290.

Bianchi, M., P. Buonanno, and P. Pinotti. 2012. "Do Immigrants Cause Crime?" *Journal of the European Economic Association* 10:1318–1347.

Blakeslee, D., and R. Fishman. 2018. "Weather shocks, agriculture, and crime evidence from India." *Journal of Human Resources* 53:750–782.

Borjas, G.J., J. Grogger, and G.H. Hanson. 2010. "Immigration and the economic status of African-American men." *Economica* 77:255–282.

Butcher, K.F., and A.M. Piehl. 2007. "Why are Immigrants' Incarceration Rates so Low? Evidence on Selective Immigration, Deterrence, and Deportation." Working Paper No. 13229, National Bureau of Economic Research, July.

Cadena, B.C., and B.K. Kovak. 2016. "Immigrants equilibrate local labor markets: Evidence from the Great Recession." *American Economic Journal: Applied Economics* 8:257–90.

Campaniello, N., and E. Gavrilova. 2018. "Uncovering the gender participation gap in crime." *European Economic Review* 109:289–304.

Carr, J.B., and A. Packham. 2019. "SNAP benefits and crime: Evidence from changing disbursement schedules." *Review of Economics and Statistics* 101:310–325.

Chalfin, A. 2014. "What is the contribution of Mexican immigration to US crime rates? Evidence from rainfall shocks in Mexico." *American Law and Economics Review* 16:220–268.

Department of Labor, Employment & Training Administration. 2017. "National Agricultural Workers Survey."

Fan, M., S. Gabbard, A. Alves Pena, and J.M. Perloff. 2015. "Why Do Fewer Agricultural Workers Migrate Now?" *American Journal of Agricultural Economics* 97:665–679.

Foley, C. 2011. "Welfare payments and crime." *The Review of Economics and Statistics* 93:97–112.

Foster, A.D., and M.R. Rosenzweig. 2004. "Agricultural Productivity Growth, Rural Economic Diversity, and Economic Reforms: India, 1970-2000." *Economic Devolopment and Cultural Change* 52:509–542.

Freedman, M., and E. Owens. 2018. "Immigration, employment opportunities, and criminal activity." *American Economic Journal: Economic Policy* 10:117–151.

—. 2016. "Your friends and neighbors: Localized economic development and criminal activity." *Review of Economics and Statistics* 98:233–253.

Freudenburg, W.R. 1986. "The density of acquaintanceship: an overlooked variable in community research?" *American Journal of Sociology* 92:27–63.

Friendly, F., E.R. Murrow, and D. Lowe. 1960. "Harvest of Shame." CBS Documentary.

Gould, E.D., B.A. Weinberg, and D.B. Mustard. 2002. "Crime rates and local labor market opportunities in the United States: 1979-1997." *Review of Economics and Statistics* 84:45–61.

Gourley, P., and G. Madonia. 2018. "Resource booms and crime: Evidence from oil and gas production in Colorado." *Resource and Energy Economics* 54:37–52.

Hornbeck, R., and P. Keskin. 2015. "Does Agriculture Generate Local Economic Spillovers? Short-Run and Long-Run Evidence from the Ogallala Aquifer." *American Economic Journal: Economic Policy* 7:192–213.

James, A., and B. Smith. 2017. "There will be blood: Crime rates in shale-rich U.S. counties." *Journal of Environmental Economics and Management* 84:125 – 152.

Kelly, M. 2000. "Inequality and crime." *Review of economics and Statistics* 82:530–539.

Komarek, T. 2018. "Crime and natural resource booms: Evidence from unconventional natural gas production." *The Annals of Regional Science* 61:113–137.

Lin, M.J. 2008. "Does unemployment increase crime? Evidence from US data 1974-2000." *Journal of Human Resources* 43:413–436.

Lochner, L., and E. Moretti. 2004. "The effect of education on crime: Evidence from prison inmates, arrests, and self-reports." *American economic review* 94:155–189.

Martin, P. 2017. "The H-2A Farm Guestworker Program Is Expanding Rapidly: Here Are the Numbers You Need to Know"." Working paper, Economic Policy Institute.

Martin, P., M. Fix, and J.E. Taylor. 2006. *The New Rural Poverty*. Washington, D.C.: The Urban Institute Press.

Mohan, G. 2017. "Desired for their Labor, Rejected as Neighbors. Farmworkers in California Face Hostile Communities." *Los Angeles Times*, pp. .

Moyce, S.C., and M. Schenker. 2018. "Migrant workers and their occupational health and safety." *Annual review of public health* 39:351–365.

Patterson, P.E. 2015. "Southcentral Idaho: Magic Valley: Russet Burbank Potatoes with Fumigation: Production and Storage Costs." 2015 Costs and Returns Estimate No. EBB3-Po3-15, University of Idaho, College of Agricultural and Life Sciences.

Reid, L.W., H.E. Weiss, R.M. Adelman, and C. Jaret. 2005. "The immigration–crime relationship: Evidence across US metropolitan areas." *Social science research* 34:757–780.

Roberts, N. 2019. "Undocumented Immigrants Quietly Pay Billions into Social Security and Receive No Benefits." *Marketplace*, January 28, pp. , https://www.marketplace.org/2019/01/28/undocumented-immigrants-quietly-pay-billions-social-security-and-receive-no/. Accessed March 18, 2021.

Roka, F.M., S. Simnitt, and D. Farnsworth. 2017. "Pre-employment costs associated with H-2A agricultural workers and the effects of the "60-minute rule"." *International Food and Agribusiness Management Review* 20:335–346.

Rural Migration News. 2020. "A Tale of Two States: Farm Labor in CA and FL."

Sampson, R.J., and W.B. Groves. 1989. "Community structure and crime: Testing social-disorganization theory." *American journal of sociology* 94:774–802.

Soriano, S. 2020. "The Rape Crisis among California's Farm Workers." *Capitol Weekly: Covering California Government and Politics* January 9.

Spenkuch, J. 2014. "Understanding the impact of immigration on crime." *American Law and Economics Review* 16:177–219.

Steinbeck, J. 1939. *The Grapes of Wrath*. New York: Penguin Books.

Stowell, J., S. Messner, K. McGeever, and L. Raffalovich. 2009. "Immigration and the recent violent crime drop in the United States: A pooled, cross-sectional time-series analysis of metropolitan areas." *Criminology* 47:889–928.

Street, B. 2018. "The impact of economic opportunity on criminal behavior: Evidence from the fracking boom." *Working Paper*, pp. .

Taylor, J.E., and D. Charlton. 2018. *The Farm Labor Problem: A Global Perspective*. Elsevier Academic Press.

Taylor, J.E., and D. Thilmany. 1993. "Worker Turnover, Farm Labor Contractors, and IRCA's Impact on the California Farm Labor Market." *American Journal of Agricultural Economics* 75:350–360.

Wadsworth, T. 2010. "Is immigration responsible for the crime drop? An assessment of the influence of immigration on changes in violent crime between 1990 and 2000." *Social Science Quarterly* 91:531–553.

Wallis, E. 2019. “Immigrant Female Farmworkers in California Need Greater Protection Against Sexual Harrasment.” *CSW Policy Briefs* 2019.

Watson, B., M. Guettabi, and M. Reimer. 2019. “Universal cash and crime.” *Review of Economics and Statistics*, pp. 1–45.

Weber, J.G., C. Wall, J. Brown, and T. Hertz. 2015. “Crop Prices, Agricultural Revenues, and the Rural Economy1.” *Applied Economic Perspectives and Policy* 37:459–476.

Zahniser, S., J.E. Taylor, T. Hertz, and D. Charlton. 2018. “Farm Labor Markets in the United States and Mexico Poze Challenges for U.S. Agriculture.” Working paper, U.S. Department of Agriculture, Economic Research Service, Nov.

Appendix

Descriptive Statistics from the National Agricultural Workers Survey

We summarize some of the notable characteristics of FVH workers interviewed in the National Agricultural Workers Survey (NAWS), which is designed to be nationally and regionally representative of crop workers in the United States. The NAWS a random-sample survey that interviews crop workers at their place of work and asks questions about income, hours of work, migration, job mobility, and immigration status, among other questions.

Poverty rates are high among FVH workers. According to the NAWS, 71.1 percent of FVH workers in 2016 who reported working the previous year had income \$25,000 in 2015, and 33.4 percent had income below \$15,000. Figure A6 shows the income distribution of workers interviewed in 2016 who reported working the previous year.

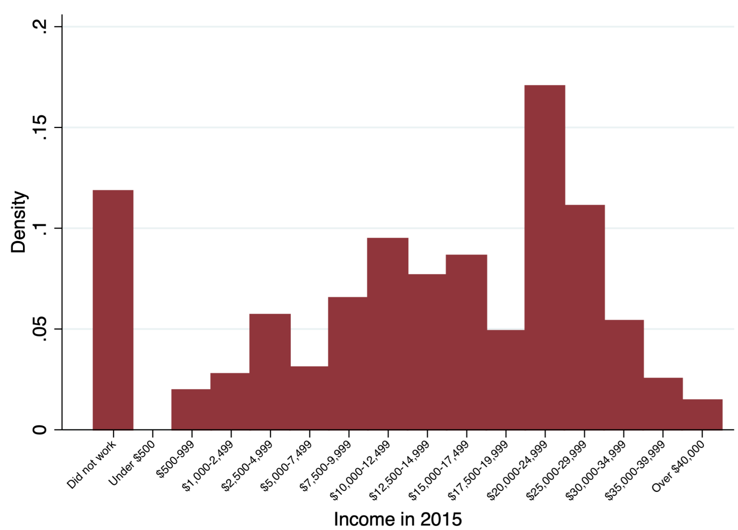


Figure A1: Income of fruit vegetable, and horticultural workers (2015)

Many FVH workers do not work year-round. FVH workers reported that they did not work (in any job, whether farm or non-farm) for an average of 10.6 weeks of the year in 2016. Figure A7 shows mean weeks per year that FVH workers report that they did not work between 1990-2016. From 1990-2016, the mean weeks not worked each year is 9.8.

Figure A2: Mean weeks per year that fruit vegetable, and horticultural workers did not work

Some FVH 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 another location that they call their home, often in Mexico (shuttle migrants). Panel (a) of figure A8 shows the migratory (i.e. either follow-the-crop or shuttle migration) share of FVH farm workforce each year from 1990-2016. There is a clear downward trend in the migratory share of FVH workers. Fan et al. (2015) conclude that the steep decline in the share of crop workers who migrate since 1998 can be attributed to both demographic changes in the workforce (which they find are responsible for about one-third of the decline) and structural changes in the U.S. and Mexican economies (which they find are responsible for about two-thirds of the decline).

Panel (b) of figure A8 plots the share of FVH workers who are foreign-born each year. From 1990-2016, 73 percent of FVH workers were born in Mexico, 20.1 percent were born in the United States, and 3.8 percent were born in Central America. Most FVH workers are not native English speakers. From 1990-2016, 79.9 percent of NAWS respondents said that their most comfortable language was Spanish while 17.3 percent responded that English was their most comfortable language.

Panel (c) of figure A8 plots the share of FVH 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 roughly 40 percent in 2016.

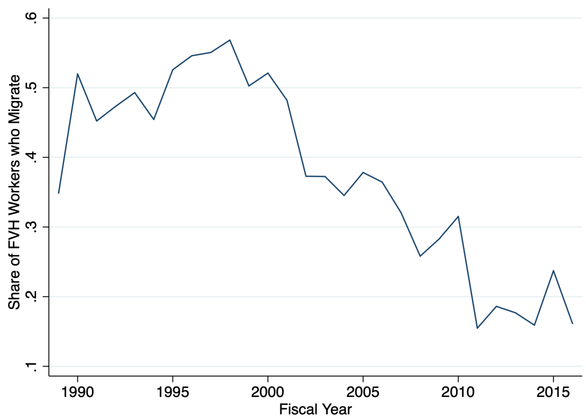
Robustness Checks

Labor Force Denominator Adjustment

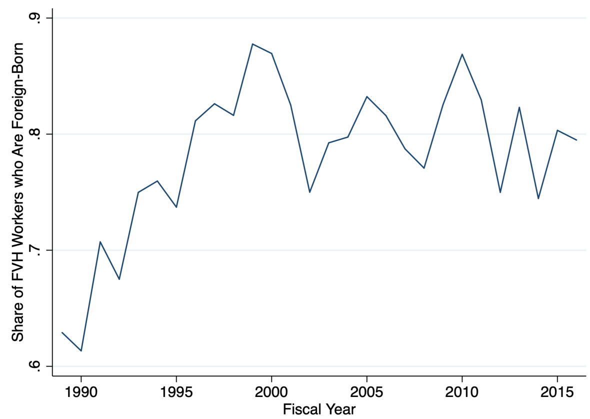
As discussed in the data section, a key component of our identification strategy is the use of changes in labor force as a proxy for changes in population. However, if seasonal agricultural workers who are settled in the same county where they perform seasonal work do not work at all for some share of 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[31] estimates of the percentage of the FVH workforce that is settled and average weeks per year not working.

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

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



(a) Migratory



(b) Foreign-born

(c) Do not live with nuclear family

Figure A3: Characteristics of fruit vegetable, and horticultural workers

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

equation:

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

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 who 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 FVH workers who are settled in the place of their seasonal employment and report not working at least one week of the year. $weeks\_nw_{ry}/52$ is the NAWS region-by-year average number of weeks spent not working at all among settled FVH workers divided 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 Table 2 with crime rates and seasonal worker share calculated with these adjusted labor force estimates.[32] These results are shown in Table A5. Similar to our main findings, we find a statistically significant negative association

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

between seasonal farm labor shares and property crime rates in panel A,[33] 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 A1: Property & Violent Crime Results, Adjusting for Seasonal Workers Settled but not Working

	A. Property Crime Results		
	(1)	(2)	(3)
	Property Crime Rate	Ln(Property Crime Rate)	Ln(Property Crime Count)
Adjusted Seasonal emp. share	-4.553***	-0.012***	-0.005
	(1.553)	(0.004)	(0.003)
N	837,084	813,747	813,747

	B. Violent Crime Results		
	(1)	(2)	(3)
	Violent Crime Rate	Ln(Violent Crime Rate)	Ln(Violent Crime Count)
Adjusted Seasonal emp. share	0.370	-0.001	0.006***
	(0.507)	(0.002)	(0.001)
N	812,772	764,427	764,427

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. 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.

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

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

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, Simnitt, and Farnsworth 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 A6 are very similar to our baseline results.[34]

Table A2: Property and Violent Crime Results, Alternative Measure of Contract Workers

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

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

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. 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.

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

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

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.[35]

To our knowledge, there is no database identifying which states require employers to report employment and earnings of H-2A workers in the QCEW. Nevertheless, H-2A workers represent a small share of total seasonal farm workers–only an estimated 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. Panel A of Table A7 shows a statistically significant negative association between farm labor share and logged property crime rates in the limited geographic sample. Panel B 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. There are several reasons H-2A workers may affect crime rates differently than other seasonal

[35] Florida is already omitted from our analysis due to irregular UCR data (See the data section of the paper appearing in the *AJAE*).

Table A3: Property & Violent Crime Results, California and Washington Only

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

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

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. 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.

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 Table A8. Note that these regressions only cover the years 2008-2016 since H-2A visa counts are only available starting in 2008, so these results are not directly comparable to our main results in Table 2. These estimates are generally smaller in magnitude than in Table 2, and are not statistically significant. Hence we find no evidence that H-2A employment shares are associated with increased crime, but do not find decreases in rates of crime as in our main results.

Table A4: Property & Violent Crime Results, H2A Workers

	A. Property Crime Results		
	(1)	(2)	(3)
	Property Crime Rate	Ln(Property Crime Rate)	Ln(Property Crime Count)
H2A emp. share	-0.113	-0.002	-0.002
	(1.237)	(0.002)	(0.002)
N	297528	288361	288361

	B. Violent Crime Results		
	(1)	(2)	(3)
	Violent Crime Rate	Ln(Violent Crime Rate)	Ln(Violent Crime Count)
H2A emp. share	1.180	0.001	0.001
	(1.130)	(0.002)	(0.002)
N	291540	275527	275527

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and H-2A data from the Department of Labor, Office of Foreign Labor Certification (OFLC) for the years 2008-2016. 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.

Employment Suppression Robustness Check

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 Table A9 are qualitatively similar to the full sample.

Table A5: Property & Violent Crime Results, Observations with Suppressed Agricultural Employment Data Excluded

	A. Property Crime Results		
	(1)	(2)	(3)
	Property Crime Rate	Ln(Property Crime Rate)	Ln(Property Crime Count)
Seasonal emp. share	-5.938***	-0.011***	-0.003
	(1.731)	(0.003)	(0.003)
N	521824	502648	502648
	B. Violent Crime Results		
	(1)	(2)	(3)
	Violent Crime Rate	Ln(Violent Crime Rate)	Ln(Violent Crime Count)
Seasonal emp. share	-0.064	-0.003	0.005***
	(0.469)	(0.002)	(0.001)
N	500182	460222	460222

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. 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.

Inverse Hyperbolic Sine

When analyzing crime counts, we use a natural log transformation to yield a percentage change interpretation and to prevent high-population counties from dominating the results, but this causes zero-crime observations to be dropped. This results in roughly 3% and 6% of observations being dropped for property and violent crimes, respectively. To address, this in Table A10 we report the results from using an Inverse Hyperbolic Sine transformation, which is defined at zero and converges to a natural log at larger values of the transformed variable. The results for both propeerty and violent crimes are similar to our baseline logged specification (see Table 2).

Table A6: Crime Results: Inverse Hyperbolic Sine Transformation

	IHS(Crime Rate)	IHS(Crime Count)
Property Crimes		
Seasonal Emp. Share	-0.013***	-0.004
	(0.003)	(0.003)
N	838332	838332
Violent Crimes		
Seasonal Emp. Share	-0.007***	0.004***
	(0.002)	(0.001)
N	814020	814020

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. The table reports estimates when using inverse hyperbolic sine transformations for property and violent crime counts. 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.

Excluding 2016

Figure 4 showed that the effect of seasonal labor share on violent crime jumped upwards in 2016, a year when immigration was a controversial issue due to the presidential election.

To check that results are not driven by this possible "election year" effect, we repeat our main specifications excluding 2016 observations. The results shown in Table A11 are virtually unchanged.

Table A7: Crime Results: Excluding 2016

	Crime Rate	Ln(Crime Rate)	Ln(Crime Count)
Property Crimes			
Seasonal Emp. Share	-4.949***	-0.012***	-0.004
	(1.295)	(0.003)	(0.003)
N	805212	783001	783001
Violent Crimes			
Seasonal Emp. Share	-0.298	-0.004**	0.004***
	(0.404)	(0.002)	(0.001)
N	781596	735011	735011

Notes: The table reports estimates when excluding 2016 observations. 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.

Weighting by Labor Force

Since many counties with high agricultural labor share have low populations, in Table A12 we check whether our main results are driven by these low-population counties by using Weighted Least Squares weighting by total labor force. In this case, the negative effect magnitudes for property crimes are actually larger, and the effect for natural log of property crime counts is significant at a 10% level (this estimate is insignificant in our main specification). For violent crimes, the effect on the log of violent crime rate is similar in magnitude but no longer significant, while the other results are qualitatively similar.

Table A8: Crime Results: Weighted by Labor Force

	Crime Rate	Ln(Crime Rate)	Ln(Crime Count)
Property Crimes			
Seasonal Emp. Share	-9.609***	-0.016***	-0.010**
	(2.783)	(0.004)	(0.004)
N	838332	814987	814987
Violent Crimes			
Seasonal Emp. Share	-0.372	-0.003	0.003**
	(0.471)	(0.002)	(0.001)
N	814020	765667	765667

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. The table reports Weighted Least Squares estimates weighting by labor force participants. 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.

Corn Producer Control Counties

We next use corn farming to construct an alternative control group in our semi-parametric specification. In our main specification we are including all counties in the United States (subject to the restrictions discussed in Data and Methods sections). To limit the comparison to other agriculture-intensive counties, we find the top 5% of counties by average corn employment during the sample period, and repeat our semi-parametric specification with only the original treatment group and corn-producing counties included in the sample. These results are shown in figure A9 and are qualitatively similar to the main results from Figure 3.

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

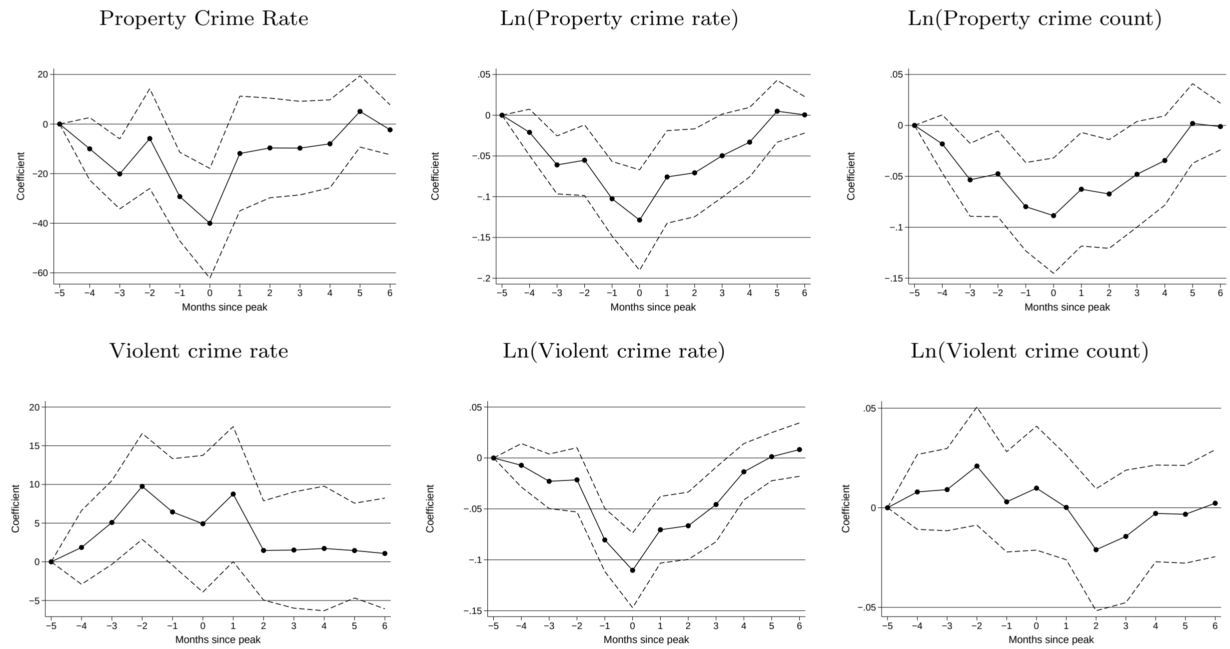


Figure A4: Property & violent crime, corn-producing counties control group

Notes: Based on county-by-month data from Uniform Crime Reports (UCR) and the Quarterly Census of Employment and Wages (QCEW) for the years 1990-2016. The graph plots coefficients and 95% confidence intervals from estimating Equation (2) when using the top 5% of corn-producing counties as controls. The dependent variables are indicated in the figure headers. Standard errors are clustered by county.

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,[36] personal theft,[37] burglary, and motor vehicle theft[38] to the police from 1992-2016 using the National Crime Victimization Survey (NCVS). The NCVS is an annual survey

[36]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.

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

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

conducted by the Bureau of Justice Statistics with a nationally representative sample of individuals. 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' ethnicity, 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 A13. 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 A9: 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: Based on the National Crime Victimization Survey (NCVS) for the years 1992-2016. 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.