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Immigration Restrictions as Active Labor Market Policy: Evidence from the Mexican *Bracero* Exclusion

Michael A. Clemens,
Center for Global Development and IZA, 2055 L Street NW, 5th floor, Washington, DC 20036 USA

Ethan G. Lewis, and
Dartmouth College and NBER, Dartmouth College Department of Economics, 304 Rockefeller Hall, Hanover, NH 03755 USA

Hannah M. Postel
Princeton University, Office of Population Research, Wallace Hall 2nd floor, Princeton, NJ 08540 USA

Abstract

An important class of active labor market policy has received little impact evaluation: immigration barriers intended to raise wages and employment by shrinking labor supply. Theories of endogenous technical advance raise the possibility of limited or even perverse impact. We study a natural policy experiment: the exclusion of almost half a million Mexican '*bracero*' farm workers from the United States to improve farm labor market conditions. With novel archival data we measure state-level exposure to exclusion, and model the labor-market effect in the absence of technical change. We reject such an effect and fail to reject a null effect.

JEL

J08; J38; F22; J61

Keywords

bracero; farm; labor; immigrant; immigration; foreign; labor; worker; visa; barrier; restriction; wages; employment; displace; native; mexico

A large literature evaluates how active labor market policy affects workers' wages and employment (LaLonde 2016). One class of these interventions is little-studied relative to its policy importance: specific immigration policy interventions designed to raise domestic wages and employment by reducing the total size of the workforce. Recent theoretical contributions suggest that labor scarcity can have *ex ante* ambiguous effects on wages under

Correspondence to: Hannah M. Postel.

[11]In annual regressions these are year fixed effects. In quarterly (monthly) regressions they are, in different specifications, either year and quarter (year and month) fixed effects or quarter-by-year (month-by-year) fixed effects.

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endogenous technological advance that alters the marginal product of labor. This can result in a flat or even upward-sloping labor demand curve—under models of directed technical change (Acemoğlu 2007; Acemoğlu and Autor 2011) or models where production technologies of differing input intensities co-exist in equilibrium (Beaudry and Green 2003, 2005; Caselli and Coleman 2006; Beaudry, Doms and Lewis 2010; Manuelli and Seshadri 2014).

In this paper we evaluate the labor market effects of a large active labor market policy experiment in the United States, a change in immigration barriers that caused the exclusion of roughly half a million seasonally-employed Mexican farm workers from the labor force. This was the December 31, 1964 abrogation of the manual laborer ('*bracero*') agreements between the United States and Mexico. The primary goal of *bracero* exclusion was to improve wages and employment for domestic farm workers. We build a simple model to clarify assumptions about the production function that would or would not lead this policy change to meet its goal. We gather novel, primary archival data on the geographic locations of *bracero* workers, allowing us to construct the first complete database of state-level exposure to nationwide *bracero* exclusion.

We test for and reject the response in wages or employment predicted by the model in the absence of induced technical advance or Rybczynski adjustment. We find that *bracero* exclusion had little effect on the labor market for domestic farm workers, and offer suggestive evidence that endogenous technical advance was an important mechanism. These findings imply that new theories of technical advance can inform the design and evaluation of active labor market policy.

The contribution of this work is to quasi-experimentally evaluate the impact of an important form of active labor market policy that is rarely directly evaluated, as well as to inform the literature on endogenous technical change. Much of the literature using natural quasi-experiments to evaluate the labor-market effects of immigration evaluates the effects of changes in the 'push' of political refugees overseas, rather than changes in the 'pull' of immigration regulation.[1] Very recently the immigration literature has turned to the labor-market effects of real-world changes to immigration restrictions (e.g. Kennan 2017; Lee, Peri and Yasenov 2017; Mayda et al. 2017). Hornbeck and Naidu (2014) show that labor scarcity arising from interstate migration after flooding can induce, beyond a simple labor-market response, technical advance in agriculture. But evidence on the effects of workforce reduction through international migration barriers remains scant.[2]

[1] Studies exploiting overseas refugee 'push' for identification include most influential studies of the labor-market effects of immigration based on natural experiments, since Card (1990) and recently Braun and Omar Mahmoud (2014); Borjas and Doran (2015); Foged and Peri (2016).

[2] Studies that have evaluated the effect of immigration on firms' technological choices do not evaluate the impact of a specific policy decision to admit or exclude migrants (e.g. Lewis 2011; Lafortune, Tessada and González-Velosa 2015); an exception in the history literature is Lew and Cater (2017). Hanlon (2015) tests a model of induced technical advance in a setting without policy-generated labor scarcity.

I. The exclusion of *bracero* workers

The *bracero* agreements were a set of three bilateral agreements between the United States and Mexico to regulate bilateral flows of temporary low-skill labor, spanning 1942–1964.[3] After World War 2 the program focused almost entirely on agriculture. It grew to supply almost half a million seasonal workers each year to U.S. farms under typical contracts between six weeks and six months. The Kennedy administration began the process of *bracero* exclusion in March 1962, making *braceros* "far less attractive" to farmers by greatly raising the required wage rate (Craig 1971, 180). The Johnson administration eliminated the program on December 31, 1964.

The exclusion of *bracero* workers from the United States was one of the largest-ever active labor-market policies of workforce reduction designed to improve domestic terms of employment within the targeted sector. "The main reason given for the discontinuation of the program at the time was the assertion that the Bracero Program depressed the wages of native-born Americans in the agricultural industry" (Borjas and Katz 2007, 16). The year before exclusion, President John F. Kennedy stated, "The adverse effect of the Mexican farm labor program as it has operated in recent years on the wage and employment conditions of domestic workers is clear" (Vialet and McClure 1980, 52). This was seen as a straightforward consequence of economic principles: a University of California at Berkeley sociologist testified to Congress that, in voting to extend the program, it had "passed a law which repeals the law of supply and demand", tantamount to "repealing the law of gravity" (Anderson 1961, 361).

These conclusions did not have a clear basis in evidence at the time. Kennedy's claims rested primarily on the findings of a commission created by the Department of Labor (Garrett et al. 1959).[4] The commission's report focused on anecdotes of employers not paying *braceros* the required 'prevailing wage', without direct evidence that this depressed U.S. workers' wages. It did not use the farm wage data then available from the Department of Agriculture, nor the unemployment available then available from the Bureau of the Census. The senior commissioner and only academic was Rufus von KleinSmid, who had co-founded a society of eugenicists that advocated blocking Mexican immigration due to their view that Mexicans were genetically inferior.[5]

Claims about the labor-market effects of *bracero* exclusion have likewise not been systematically evaluated since the event. The following year, the Secretary of Labor (1966,

[3]The two countries created much smaller bilateral labor agreements, also sometimes called *bracero* agreements, in 1910 and during 1919–1921.

[4]The centrality of this commission's findings in the anti-*bracero* arguments of the Administration and Congress is apparent in e.g. Williams (1962, 29). The commission's work caused the formation of a Senate subcommittee to investigate migrant farm workers (Norris 2009, 148), was frequently cited by influential advocates of *bracero* exclusion (e.g. Galarza 1964, 199), and was considered authoritative long afterward (Vialet and McClure 1980, 58)

[5]Von KleinSmid was a lifelong advocate of eugenics (von KleinSmid 1913). He was 84 when the commission's work was published. At age 53, as a senior professor at the University of Southern California, he had become a charter member of the eugenicist Human Betterment Foundation, with his name printed on its letterhead. He continued as a leading participant for at least a decade (Gosney 1937). The officers and trustees of the Human Betterment Foundation advocated restricting Mexican immigration, as well as the sterilization of some Mexican-Americans, on the grounds that they believed science had proven Mexicans to constitute a genetically inferior race (Holmes 1929; Kuhl 2002, 57; Stern 2015, 164). The work of von KleinSmid and his colleagues on 'racial' purification in California was specifically cited by the National Socialist regime of Adolf Hitler as providing an "essential basis" for German compulsory sterilization laws in the 1930s (Kuhl 2002, 43).

3, 10, 11, 19) claimed that due to *bracero* exclusion "[t]ens of thousands of jobs were created for American workers." He rested this claim on a rise in domestic seasonal farm employment in three states, without comparing it to similar trends in unaffected states. Similarly, a report of the U.S. Senate (1966, 17) claimed wage effects from *bracero* exclusion due to trends in affected states, without noting similar trends in unaffected states. Decades later, political debates simply asserted that *bracero* exclusion benefited U.S. workers (U.S. House of Representatives 2004, 125, 130).[6]

A few economists at the time of exclusion doubted the political consensus, but were largely ignored. Jones and Christian (1965, 528) predicted that any wage effects of *bracero* exclusion would be "almost completely nullified by an accompanying intensification of mechanization." William E. Martin (1966, 1137), later president of the Western Agricultural Economics Association, wrote that due to sudden substitution of capital, "excluding foreign labor will not have any lasting beneficial effects on the domestic farm labor force." We formalize and test these concepts.

II. Testing a model of workforce reduction and technical advance

We construct a simple model to explore how technological and production adjustment could shape the labor-market effects of an active policy of workforce reduction. Following Acemoğlu (2010) we refer to a technical advance as either the creation of a new production technology or the adoption of an existing technology.

A. Crop production in an open economy with alternative technologies

Let there be several locations, indexed by i (hereafter suppressed), each of which can produce a single crop (relaxed below). The crop can be sold to the world market (at price $p \equiv 1$) and is produced using capital (K), labor (L), land (τ) and materials (M). The endowment of land is fixed at $\bar{T}$; capital and materials are supplied elastically. The endowment of labor is initially $\bar{L}$, but we will consider below what happens when it is reduced. Land and labor markets are competitive. Farmers rent land (from landowners) at rate r_T, hire workers at wage w, and purchase materials at price m. Landowners receive payment $\underline{r_T} \geq 0$ per acre if they do not rent to farmers.

A nested constant elasticity of substitution (CES) production technology using "traditional" or "old" capital, K_0 elastically supplied at rental rate r_0, can be used to produce the output crop Y:

[6]Two academic studies have attempted to evaluate the effects of exclusion, have been hampered by data or specification problems. Jones and Rice (1980) fail to detect differences in farm wage and employment trends in four U.S. states 1954–1977, before and after exclusion, but cannot measure exposure intensity due to the unavailability of state-level *bracero* counts. Morgan and Gardner (1982) conduct a similar exercise with a state-year panel 1953–1978, but do not control for the nationwide rise in farm wages across this period, which would tend to generate a spurious 'effect' during the program years 1953–1964.

$$Y_0 = \left\{ K_0^{\frac{\mu - 1}{\mu}} + \left[aL^{\frac{\sigma - 1}{\sigma}} + (1 - a)T^{\frac{\sigma - 1}{\sigma}} \right]^{\frac{\sigma}{\sigma - 1}\frac{\mu - 1}{\mu}} \right\}^{\frac{\mu}{\mu - 1}}. \quad (1)$$

Land and materials are in another nest, for simplicity, $T \equiv \min\{\tau, M\}$. For some crop locations, the crop can alternatively be produced using an "advanced" or automated technology, where

$$Y_A = \left\{ K_A^{\frac{\mu - 1}{\mu}} + \left[bL^{\frac{\sigma - 1}{\sigma}} + (1 - b)T^{\frac{\sigma - 1}{\sigma}} \right]^{\frac{\sigma}{\sigma - 1}\frac{\mu - 1}{\mu}} \right\}^{\frac{\mu}{\mu - 1}}. \quad (2)$$

K_A is advanced-production capital, elastically supplied at rental rate r_A. This technology is less labor-intensive, that is, $b < a$. Its output is a perfect substitute for Y_0, so let $Y \equiv Y_0 + Y_A$ represent total output of the crop. Because it is more land intensive, the advanced technology is more productive only at low levels of labor per unit of land. The advanced technology does not dominate the traditional technology in the sense of producing more from given inputs. Farmers may use a combination of technologies in a competitive equilibrium. Let $[\phi_\ell, \phi_u]$ be the range of $\bar{L}/\bar{T}$ over which this occurs—defining the cone of diversification. That is, there exists an allocation of land (T_0, T_A with $T_0 + T_A = \bar{T}$) and labor (L_0, L_A with $L_0 + L_A = \bar{L}$) to each technology such that the marginal products of land and labor are the same in each technology. In the Appendix we show that, inside the cone, the wage is fixed at

$$\widehat{w} = b^{\frac{\sigma}{\sigma - 1}} \left(\frac{r_A^{\mu - 1}}{r_A^{\mu - 1} - 1} \right)^{\frac{\mu}{\mu - 1}} \left[\frac{\left(\frac{a}{b}\right)^{\sigma} - \left(\frac{1 - a}{1 - b}\right)^{\sigma}}{\left(\frac{r_A^{\mu - 1}}{r_A^{\mu - 1} - 1} \middle/ \frac{r_0^{\mu - 1}}{r_0^{\mu - 1} - 1} \right)^{\frac{\mu}{\mu - 1}(\sigma - 1)} - \left(\frac{1 - a}{1 - b}\right)^{\sigma}} \right]^{\frac{1}{\sigma - 1}}. \quad (3)$$

Wages are invariant to factor supply inside the cone because factor proportions are fixed within each technology. Figure 1 summarizes the model, juxtaposing the traditional production function (1) and the advanced production function (2). The cone of diversification is also not a rare state. Across a range of industries from brewing to railroads, major innovations were used alongside traditional technologies for several decades (Mansfield 1961). It is also typical of major innovations in agriculture; for example, horses existed alongside tractors and power-tillers in U.S. agriculture for half a century (Manuelli and Seshadri 2014).

B. Workforce reduction policy

We can now explore the impact of a policy change that excludes a portion of the labor force. Let labor consist of *bracero* workers B and non-*bracero* workers N, such that $\bar{L} \equiv B + N$. Without loss of generality, if the alternative technology exists, assume $(B+N)/\bar{T} > \phi_\ell$; that is, at least one farm uses the traditional technology. When *bracero* workers are excluded, the relative change in labor supply is $\%\Delta(\bar{L}/\bar{T}) = \frac{N/T - (B+N)/T}{(B+N)/T} = -\frac{B}{L}$. Using the traditional technology, the wage is given by $w = a\left\{\left(\frac{K}{T}\right)^{\frac{\mu-1}{\mu}} + \widetilde{L}^{\frac{\sigma}{\sigma-1}\frac{\mu-1}{\mu}}\right\}^{\frac{\mu}{\mu-1}-1} \widetilde{L}^{\frac{\sigma}{\sigma-1}\frac{\mu-1}{\mu}-1}\left(\frac{L}{T}\right)^{\frac{-1}{\sigma}}$, where $\widetilde{L} \equiv a\left(\frac{L}{T}\right)^{\frac{\sigma-1}{\sigma}} + (1-a)$. Thus *in the absence of adjustment in capital, technology, or output*, exclusion raises wages:

$$\frac{\partial \ln w}{\partial (B/L)} \approx -\frac{\partial \ln w}{\partial \ln (L/T)} \approx s_K \frac{s_L}{s_L + s_T}\frac{1}{\mu} + \frac{s_T}{s_L + s_T}\frac{1}{\sigma} > 0, \quad (4)$$

where s_T, s_L, s_K are the income shares of land (plus materials), labor, and capital, respectively.[7] Using the parameter estimates of Herrendorf, Herrington and Valentinyi (2015) for postwar U.S. agriculture, the semielasticity (4) would be large, approximately 0.4.[8] In a typical high-*bracero* state with $B/L = 0.3$, farm wages rise by about 12 percent after exclusion. *If we allow capital (only) to adjust*, (4) reduces to

$$\frac{\partial \ln w}{\partial (B/L)} \approx \frac{s_T}{s_L + s_T}\frac{1}{\sigma} > 0. \quad (5)$$

Under the same parameter assumptions, the magnitude of the semielasticity (5) is approximately 0.1, or one quarter as large as without capital adjustment. In a typical high-*bracero* state, exclusion raises farm wages by approximately 3 percent.

With adjustment of capital, technology, and output—Now suppose that technology can adjust to *bracero* exclusion. Assume that both the traditional and advanced technologies are in use ($(B+N)/\bar{T} \in [\phi_\ell, \phi_u]$) and remain in use after exclusion ($N/\bar{T} \in [\phi_\ell, \phi_u]$), *or* the crop is already at or below the shutdown margin ($(B+N)/\bar{T} \le \phi$). The model then predicts three effects of exclusion. First, the wage remains fixed at $w \equiv \hat{w}$ (or $\bar{w}$, defined below) so wages do not rise:

[7] This and other expressions derived in the Appendix.

[8] That is, with $\mu, \sigma \equiv 1.6$, $s_K \equiv 0.54$, $s_T \equiv 0.07$, and $s_L \equiv 0.39$, from Herrendorf, Herrington and Valentinyi (2015). They specify the production function differently, imposing that capital and land are in the same nest. Given cost shares, their estimate of the elasticity of substitution between labor and capital+land is likely dominated by capital. Other estimates suggest labor's substitutability with land is much lower (e.g., Binswanger 1974), so σ may actually be smaller and wage responses larger. Indeed, a meta-analysis by Espey and Thilmany (2000) finds that the median wage elasticity of labor demand for hired farm workers across all published studies is −0.5, which also implies larger wage impacts.

$$\frac{\partial \ln w}{\partial(B/L)} = 0. \quad (6)$$

Second and third:

$$\frac{\partial \ln (Y_A/Y)}{\partial(B/L)} > 0, \quad \frac{\partial Y_0}{\partial(B/L)} < 0, \quad (7)$$

That is, the output share of the automatic harvest technology rises in pre-exclusion *bracero* share, while the output of the traditional technology, or of crops that lack a less- labor-intensive alternative technology, falls. Intuitively, *bracero* exclusion does not affect wages within the diversification cone because any fall in the labor-land ratio only raises the fraction of farmers using the advanced technology, without changing the land/labor ratio that is employed in each technology and therefore without changing the marginal product of labor. Firms adopt the technology that emphasizes the factor whose relative supply has risen, without any necessary change in the price of the factor whose relative supply has fallen, as in Acemoğlu (1998).[9] For crops that lack a feasible advanced technology ($L/T \gg \phi_u$), output falls and wages rise, but only up to a point where the higher wages make it profitable to decrease production of that crop by switching land use—to an alternative crop, fallow land, or non-farm use.[10]

In Figure 1, *bracero* exclusion represents a leftward movement, which leaves the wage fixed at $\hat{w}$ within the diversification cone $[\phi_\ell, \phi_u]$. For a crop that lacks advanced technology for production, the wage can rise, but stops at $\bar{w} > \hat{w}$ where L/T reaches the shutdown margin $\bar{\phi}$.

C. Archival data on braceros, farm employment, and farm wages

A dataset to test the above model did not exist when we began this investigation. No secondary source reported *bracero* employment by U.S. state for a substantial number of states, even though this information was collected and disseminated at the time in widely available government publications. Collection of these data required in-person visits to study primary print sources at government archives around Washington, DC and at presidential archives in Abilene, Kansas and Independence, Missouri. We also assembled primary data for a novel database of hired agricultural worker wages by state-quarter. Here we describe these new data sources and the regression models we use. Data on seasonal hired farm workers (foreign and domestic) are monthly stocks of hired workers on farms by state from 1943 to 1973, with complete state coverage after 1953. Farm wage data are quarterly, with complete geographic coverage for the principal wage index 1948–1971. The Appendix details the sources and discusses the potential for measurement error.

[9]Indeed, a broad set of directed technical change (e.g., Acemoğlu 2007) and choice-of-technique (e.g., Beaudry and Green 2003; Caselli and Coleman 2006) models allow for at or (in the former case) even upward sloping factor demand curves (post-technology adjustment). The present model attempts to capture key features of farm production.

[10]This model is isomorphic to a 2 × 2 small, open economy model in which adjustments to exclusion occurs through shifting production towards less labor-intensive crops, likewise blunting the wage impact. Note that below we consider the outcomes for non-Mexicans, and any Mexican-non-Mexican specialization in employment would further mitigate the wage impacts on that group.

III. Results

Here we test the model's prediction that even a large negative shock to the foreign labor supply could have minimal labor-market effects, provided that capital, technology, or output can adjust.

A. Quasi-experimental tests: Wages and employment

For each labor-market outcome, the first regression specification evaluates the effect of *bracero* exclusion as a quasi-experiment. Treatment is the degree of exposure to exclusion, defined as *braceros*' fraction of seasonal agricultural labor in the state at the program's height in the mid-1950s. We use differences-in-differences with continuous treatment, following Card (1992):

$$y_{st} = \boldsymbol{\alpha}'\boldsymbol{I_s} + \boldsymbol{\beta}'\boldsymbol{I_t} + \gamma\left(I_{t\geq 1965} \cdot \overline{\ell}_s^{1955}\right) + \varepsilon_{st}, \quad (8)$$

where y_{st} is the outcome in state s in year, quarter, or month t, $\boldsymbol{I_s}$ is a vector of state fixed effects, $\boldsymbol{I_t}$ is a vector of time fixed effects,11 $I_{t\geq 1965}$ is an indicator for an observation after *bracero* exclusion, L_{st}^{mex} is the stock of Mexican hired seasonal workers, L_{st} is the stock of hired seasonal workers of any nationality (including domestic), and $\overline{\ell}_{1955}$ is the mean fraction of Mexican workers $\frac{L_{st}^{\text{mex}}}{L_{st}}$ in state s across all months of 1955, years before exclusion. The error term is ε_{st}, $\boldsymbol{\alpha}$ and $\boldsymbol{\beta}$ are vectors of coefficients to be estimated, and γ is the coefficient of interest. Assuming that trends in the outcome would have been similar in the states most affected by exclusion to trends in unaffected states had exclusion not occurred, the estimate $\hat{\gamma}$ captures the effect of exclusion.

Wages—Figure 2 illustrates the core result, informally testing the zero wage-effect condition (6). Figure 2a shows the natural experiment of *bracero* exclusion. It shows the Mexican fraction of hired seasonal farm labor, averaged across states, within three groups of states. The group with high exposure to exclusion (black line) is the six states where *braceros* made up more than 20 percent of hired seasonal farm labor in 1955: Arkansas, Arizona, California, New Mexico, South Dakota, and Texas. The group with low exposure to exclusion (gray line) is the states that had some *braceros* in 1955, but less than 20 percent of seasonal agricultural labor. The group with no exposure to exclusion (dashed line) is the states that had zero *braceros* in 1955. Figure 2b shows farm wage trends in the same three groups.[12] Pre- and post-exclusion trends in real farm wages are similar in high-exposure states and low-exposure states. Wages in both of those groups rose more slowly after *bracero* exclusion than wages in states with no exposure to exclusion.[13]

[12]Hourly wage, constant 1965 US$ deflated with Consumer Price Index.

[13]This pattern confirms systematically what was remarked on anecdotally at the time of exclusion: Varden Fuller (1967, 288) wrote of California two years afterward, "Higher wage rates are believed to have been both a consequence of the departure of the Braceros and the means by which a greater supply of domestic workers was obtained. Surprisingly, however, in 1965 and 1966 California farm wages rose at virtually the same rate as in the nation at large."

Table 1 conducts this test more formally, using regression (8). The first two columns use the hourly wage by state-quarter as the outcome; the second two columns use the daily wage without board.[14] Within each pair, the second regression narrows the window of analysis to five years before and after the termination of the program. State fixed effects absorb the influence of time-invariant differences between states, such as differences in arable land or the initial farm work-force, and quarter-by-year fixed effects absorb national and seasonal trends. The difference-in-difference is negative but statistically indistinguishable from zero. The last three rows of Table 1 show tests of the wage semielasticity prediction of the model. In all columns we reject at the one percent level the predicted wage semielasticity $\frac{\partial ln w}{\partial (B/L)}$ of 0.4 without adjustment of capital, technology, or output, in equation (4). We likewise reject the predicted wage semielasticity of 0.1 with adjustment of capital but without adjustment of technology or output, in equation (5), at the one percent level for the hourly wage and at the five percent level for the daily wage. The results are compatible with rapid adjustment of capital, technology, and output in equation (6).

Employment—We repeat the above analysis with employment of domestic seasonal farm workers as the outcome. Figure 3a illustrates the core result. The left panel shows the average *bracero* stock in the three groups of states over time. *Bracero* exclusion removed tens of thousands of farm workers from the average high-exposure state. The right panel shows the average number of domestic seasonal farm workers in the same groups of states. The gap between high- and low-exposure states is approximately constant before and after exclusion. The gap between high- and no-exposure states narrows during the program and remains approximately constant after exclusion—the opposite of what would be expected if *bracero* exclusion had crowded more domestic labor into farm work. There appears to be a slight upward deviation from trend in the high-exposure states during 1964–1966, but a similar bump occurs in no-exposure states.

This null result is confirmed by the corresponding regression (8), in Table 2. The outcome is either total domestic seasonal farm employment or its natural logarithm, and the unit of observation is state-month. The first two columns use all data, while the second two columns again restrict the window of analysis to ten years. The coefficient estimates are negative and statistically indistinguishable from zero. Similar results are obtained for local, intrastate, and interstate U.S. workers separately (Figure 3b and Table 3). This indicates that domestic seasonal workers did not move between states in substantial numbers to dissipate state-specific shocks to the *bracero* labor supply.

B. Robustness checks

We also conduct tests of the relationship between labor market outcomes and *bracero* stocks during the program, using simple fixed-effects regressions. Figure 4a shows a Baltagi-Li (2002) semiparametric regression of real wage on *bracero* stock by state-quarter, with state and quarter-by-year fixed effects. Figure 4b shows the analogous semiparametric regression of domestic seasonal farm employment on *bracero* stock by state-month, with state and

[14]The hourly wage has full state coverage but fewer years (1948–1971); the daily wage has more years (1942–1975) but is missing three states (CA, OR, WA) for most years (1951–1962 and 1965–1975).

month-by-year fixed-effects. These reveal no economically or statistically significant tendency for wages or domestic employment to rise with falling *bracero* stocks. The corresponding linear parametric fixed-effects regressions, shown in the Appendix, show a positive and statistically significant relationship between *bracero* stocks and both real wages and domestic seasonal farm employment.

Numerous additional robustness tests are reported in the Appendix. First, inspection of Figure 3a suggests that pre-trends could bias the results of differences-in-differences regressions. Recasting the regressions as a year-by-year event study reveals no significant pre-trends in wages. In employment there are are significant pre-trends, but not when the sample is restricted to states with nonzero exposure to the program. The results are not sensitive to this restriction, though even the restricted sample retains states with a wide range of exposure (Table 2, cols. 5–6). Neither the results for wages nor employment are substantially affected by including state-specific linear times trends in the differences-in-differences regressions.

Second, an important concern is the potential substitution of *braceros* by unobserved, unauthorized Mexican workers. The results here are robust to testing for effects on a different measure of farm employment that includes both unauthorized workers and nonseasonal domestic workers.[15] Counts of these 'total hired farm workers' in the original sources, unlike the counts of 'hired seasonal farm workers', are not disaggregated by nationality. Thus the hypothesis that *braceros* were not replaced by other farm workers (U.S. or not, authorized or not, seasonal or not) corresponds to a coefficient estimate of unity. All specifications fail to reject a coefficient of unity. These estimates should be considered only suggestive because the original sources omit 'total hired farm workers' counts for 11 of the 46 states. But further evidence shown in the Appendix is incompatible with substantial short-term replacement of *bracero* workers with unauthorized workers: Over 99.5 percent of the *braceros* that arrived in 1963 and 1964 were registered as returning to Mexico. And border apprehensions of Mexicans did not substantially rise in the years immediately after exclusion, while measured border enforcement effort did not fall.

Third, the Stable Unit Treatment Value Assumption that underlies differences-in-differences tests could be violated if wages in states without *braceros* were affected by *bracero* exclusion from other states—such as if the mere threat of bringing in *braceros* from Arkansas allowed employers in Vermont to keep wages low in Vermont. To the extent that such a threat would be more credible in states with small numbers of *braceros* than in states that never had any *braceros*, this concern is incompatible with the observed invariance of the result to dropping states with zero *braceros*. The Appendix also reports several additional tests of robustness to alternative assumptions regarding the treatment year, interpretation of missing values, and others.

[15]We follow the test recommended by Card (2009) and Peri and Sparber (2011): a fixed effects regression of total hired farm workers on Mexican seasonal workers, both normalized by a scale measure (predetermined worker stock, or predetermined state population, or arable land area), with state and month-by-year fixed effects.

C. Mechanisms

These results imply that *bracero* exclusion failed as active labor market policy. The model predicts a mechanism for this failure. Equations (6)–(7) suggest that the policy's effects could be nullified by capital-labor substitution and technological adjustment.

Such adjustment is clear in the state (California) and crop (tomatoes) that used *bracero* labor the most. Tomato harvesting sat within the technological cone of diversification at the time of exclusion. Harvesting machines had been available since the late 1950s, machines that roughly doubled harvest productivity per worker (Harper 1967, 12), but adoption was low for the first several years (Vandermeer 1986, 22). Figure 5a shows that *bracero* exclusion was followed immediately by a dramatic adoption of this existing technology, as predicted by equation (7). This corroborates qualitative studies claiming that exclusion caused sudden adoption of the harvester (Martin 1966, 1144; Martín 2001, 313). No such shift occurred in Ohio (Figure 5b), which was unaffected by exclusion. The Appendix presents analogous, suggestive evidence that *bracero* exclusion encouraged mechanization in cotton harvesting and sugar beet field preparation.

An indirect, more easily observable consequence predicted by the model is that—for crops without an advanced mechanization technology that could be rapidly adopted—production should fall at the shutdown margin $\bar{\phi}$ accompanied by capital-labor substitution under the traditional technology. This could perhaps be accompanied by switching to other, non-mechanized production techniques (equations (7)). Advanced mechanization technology was available for adoption to produce tomatoes, cotton, and partially for sugar beets. No comparable technology was then available for production of most other *bracero*-intensive crops, including asparagus, strawberries, lettuce, celery, cucumbers, citrus, and melons (Sanders 1965; Harper 1967). We thus expect relatively greater declines in production after exclusion for this latter group of crops. We test this prediction with the event-study specification

$$y_{st} = \boldsymbol{\alpha}'\boldsymbol{I}_s + \boldsymbol{\beta}'\boldsymbol{I}_{t \neq 1964} + \boldsymbol{\gamma}' \cdot \boldsymbol{I}_{t \neq 1964} \cdot \overline{\ell}_s^{1955} + \varepsilon_{st}, \qquad (9)$$

similar to equation (8) but where $\boldsymbol{I}_{t \neq 1964}$ is a vector of year dummies that omits the base-group 1964, and $\boldsymbol{\gamma}$ is a vector of parameters to be estimated ($\gamma_{1964} = 0$). The outcome y_{st} is a state- and crop-specific index of physical production (e.g. pounds), scaled to 100 in 1964. Figure 6 shows the event-study coefficient estimtes $\hat{\gamma}_t$ from regression (9) for nine of the most important *bracero* crops. At the top of the figure, *bracero* exclusion is followed by modest, short-lived relative declines in tomato and cotton production. Here, frictions on technical advance existed—for example, the machines were inapt for delicate, fresh-market tomatoes (Harper 1967, 11)—but were minor. For sugar beets, where adoption of the advanced technology faced greater frictions, the relative decline after exclusion is larger and longer. Of the remaining six crops, where capital-labor substitution could largely proceed only under the traditional technology, we observe large and lasting relative declines in production in and after 1965 in five (declines corroborated by Martin 1966, 1141; Hirsch 1966, 2; Secretary of Labor 1966, 16–18).

An important concern is the potential for reverse causation of *bracero* exclusion by technical advance. In principle, agribusiness lobbyists in *bracero* states could have stopped supporting the program precisely when exogenous technical advance had reduced their need for labor. But Congressional voting data collected by Alston and Ferrie (2007, Table 5.3, pp. 110–111) show that no such shift occurred. The sharp 1963 decline in political support for the program occurred only among representatives of states that did *not* rely on the program. The Appendix shows that 'high exposure' states supported the program even as it ended.

IV. Conclusion

The exclusion of Mexican *bracero* workers was one of the largest-ever policy experiments to improve the labor market for domestic workers in a targeted sector by reducing the size of the workforce. Five years afterward, the agricultural economist William Martin called advocates of the policy "obviously…extremely naïve" since "capital was substituted for labor on the farm and increased effort was exerted by the agricultural engineers in providing the farmers these capital alternatives" (Wildermuth and Martin 1969, 203). We find that in broad terms this assessment, perhaps uncharitable, was accurate: *bracero* exclusion failed to raise wages or substantially raise employment for domestic workers in the sector. Employers appear to have instead adjusted to foreign-worker exclusion by changing production techniques where that was possible, and changing production levels where it was not. This mechanism requires further elucidation. Further research should explore other natural experiments to test causal links between labor scarcity and endogenous technical change, as urged by Acemoğlu (2010, 1071).

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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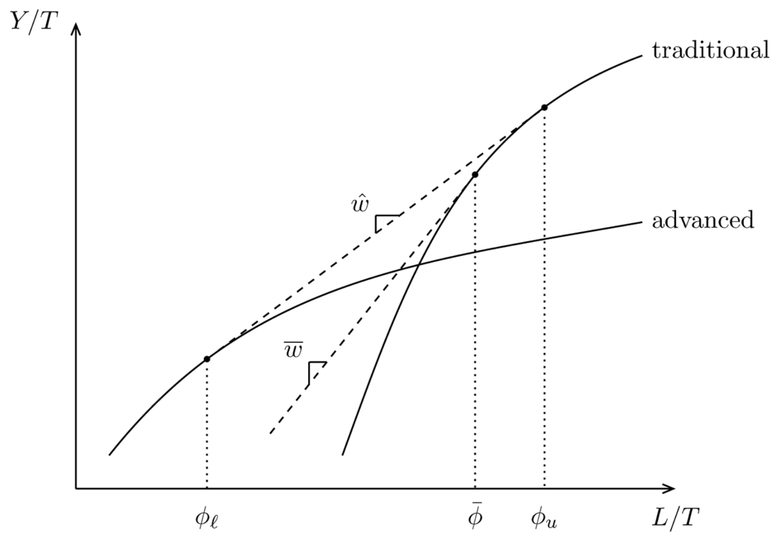


Figure 1.
The diversification cone [ϕ_ℓ, ϕ_u] and shutdown margin $\bar{\phi}$

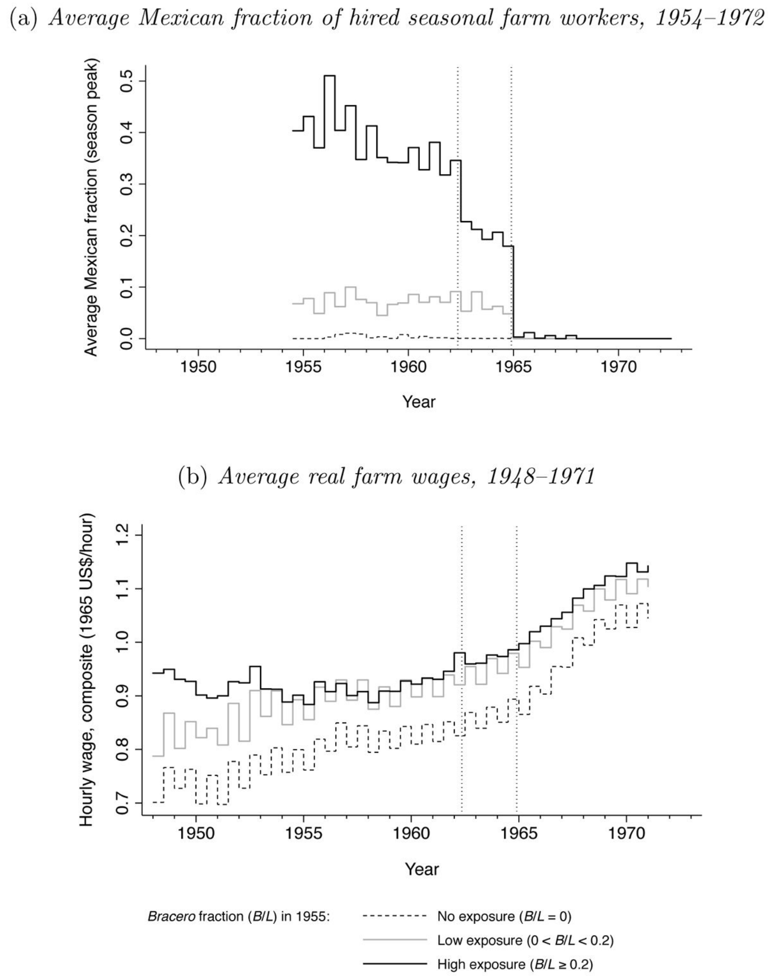


Figure 2.

Illustration of natural quasi-experiment and core result, states grouped by exposure

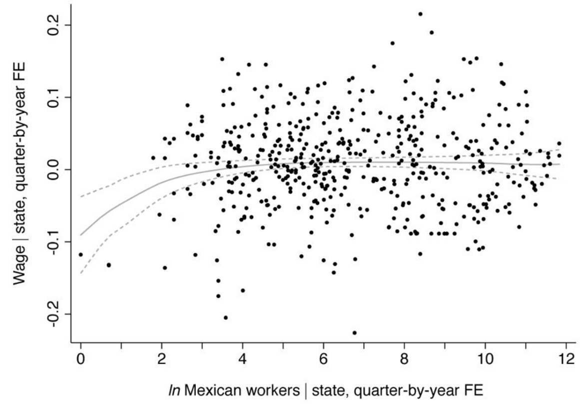
Note: Average across states, by season, of each outcome. Each year is split into two seasons: the first half of each year is the early season (February to July), the second half of each year is the late season (August to November). The outcomes are (a) peak Mexican fraction of hired seasonal farm workers during any month in the season, and (b) average real hourly wage in the two quarters that comprise that season. Real wage adjusted by national Consumer Price Index. Vertical dotted lines show the beginning of government efforts toward *bracero* exclusion (March 1962) and near-complete exclusion at the termination of the program (December 1964). High-exposure group is AZ, CA, NE, NM, SD, TX. Low-exposure group is AR, CO, GA, ID, IL, IN, MI, MN, MO, MT, NV, OR, TN, UT, WA, WI, WY. No-exposure group is AL, CT, DE, FL, IA, KS, KY, LA, MA, MD, ME, MS, NC, ND, NJ, NY, OH, OK, PA, SC, VA, VT, WV.

Figure 3.

Number of seasonal farm workers employed, state averages grouped by exposure

Note: Average across states, in each year, of peak-month worker stock of each type. Vertical dotted lines show the beginning of major government efforts toward *bracero* exclusion (March 1962) and near-complete exclusion at the termination of the program (December 1964).

(a) *Baltagi-Li semiparametric fixed-effects regression of real hourly wage on bracero stock, quarterly, under nonzero bracero stocks (1942–1966)*



(b) *Baltagi-Li semiparametric fixed-effects regression of domestic seasonal farm employment on bracero stock, by state-month*

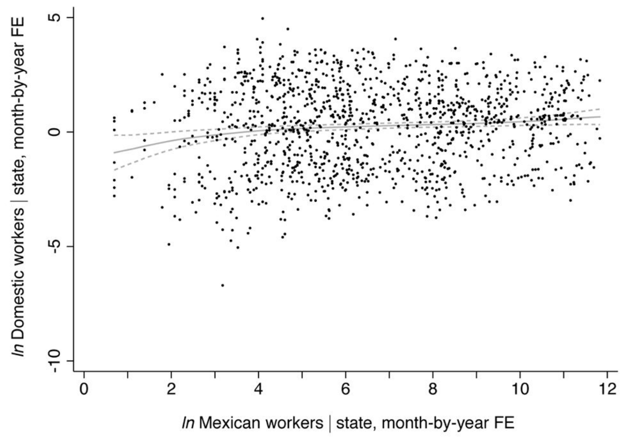


Figure 4.

Semiparametric fixed-effects regressions

Note: (a) Baltagi-Li (2002) regression of quarterly state-average wage on ln *bracero* stock, with state and quarter-by-year fixed effects. (b) Baltagi-Li (2002) regression of monthly stock of ln employed domestic seasonal farm workers on ln *bracero* stock, with state and month-by-year fixed effects. Both are local linear with Epanechnikov kernel, bandwidth 2 log-points. Dashed lines show 95% confidence interval, clustered by state. Real wage is hourly wage deflated to 1965 US$ by Consumer Price Index.

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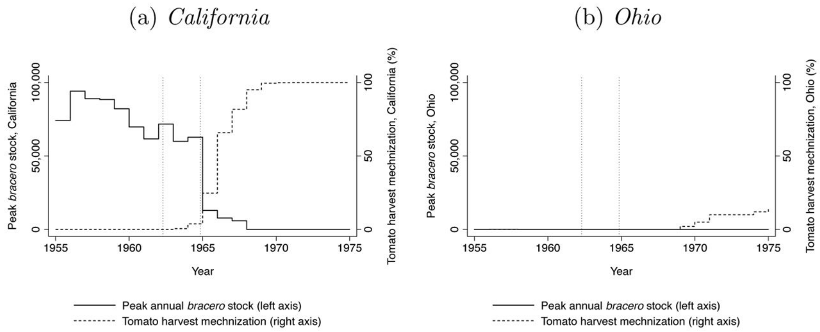

Figure 5.

Peak annual *bracero* stock and mechanization of the tomato harvest, in the two states with mechanization time series

Note: Left axis, total *braceros* working in state in the peak month of each year (almost always October). ‘Mechanization’ means that tomatoes were harvested with the Blackwelder tomato harvester, reported by Vandermeer (1986). Vertical dotted lines show the beginning (March 1962) and completion (December 1964) of exclusion. There were 74 and 64 *braceros* in Ohio in the peak month 1956 and 1957, respectively, zero in all other years.

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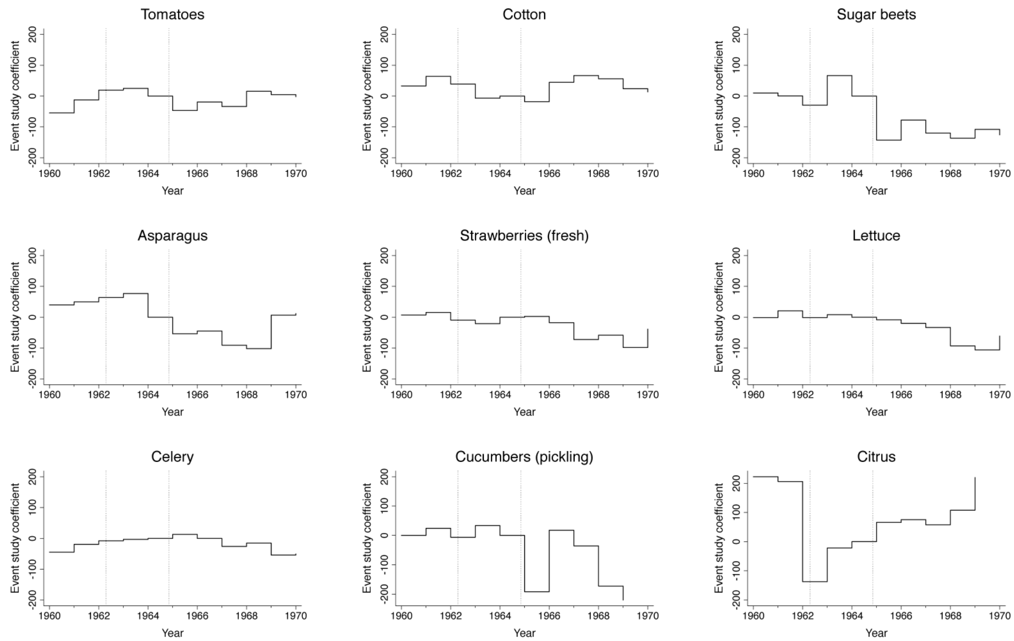

Figure 6.

Event study regression coefficients: crop physical production index

Note: Observations are state-years. Vertical axis shows event-study regression coefficients from equation (9). For each crop, dependent variable is a production index normalized so that each state's physical production of the crop in 1964 ≡ 100. Vertical dotted lines show the beginning (March 1962) and completion (December 1964) of exclusion. Cucumbers, citrus truncated at ± 200 identical vertical ranges.

Table 1

Effects of *bracero* exclusion on real wages: Differences-in-differences with continuous treatment, quarterly

	Wage, all years		Wage, 1960–1970	
Dep. var.	**Hourly composite**	**Daily w/o board**	**Hourly composite**	**Daily w/o board**
$I_{t \geq 1965} \cdot \overline{\ell}_s^{1955}$	−0.0356	−0.385	−0.0401	−0.0247
	(0.0426)	(0.495)	(0.0315)	(0.309)
N	4324	5813	2024	1901
adj. R^2	0.773	0.835	0.733	0.758
Clusters	46	46	46	46
Semielasticity $\frac{\partial \ln w}{\partial (B/L)}$	−0.0831	−0.110	−0.0750	−0.0410
	(0.0654)	(0.0916)	(0.0507)	(0.0541)
p-val. χ^2 test: $\frac{\partial \ln w}{\partial (B/L)} = 0.1$	[0.0075]	[0.0263]	[0.0012]	[0.0124]

‘Treatment’ is the degree of exposure to exclusion. Observations are state-quarters. All regressions include state and quarter-by-year fixed effects. Standard errors clustered by state in parentheses. $\overline{\ell}_{1955}$ is average fraction of Mexicans among the state’s total hired seasonal workers across the months of 1955. Wages in constant 1965 US$ deflated by CPI. Hourly wage has full state coverage but fewer years (1948–1971); daily wage has more years (1942–1975) but is missing for three states (CA, OR, WA) in most quarters after 1949Q1. Farm worker stocks missing in original sources for 1955 in Rhode Island and New Hampshire. Semielasticity is the coefficient on $I_{t \geq 1965} \cdot \overline{\ell}_s^{1955}$ in an otherwise identical regression with ln *wage* as the dependent variable.

Table 2

Effects of *bracero* exclusion on domestic seasonal agricultural employment: Differences-in-differences with continuous treatment, monthly

	All states, all years		All states, years 1960–1970		Exposed states only, all years	
Specification:	**linear**	**ln**	**linear**	**ln**	**linear**	**ln**
$I_{t \geq 1965} \cdot \bar{\ell}_s^{1955}$	−6949.2	−0.311	−1843.0	−0.113	−312.2	−0.142
	(9093.5)	(0.509)	(6859.3)	(0.375)	(7463.0)	(0.566)
N	10329	6386	6072	3707	5168	3189
adj. R^2	0.055	0.085	0.079	0.076	0.028	0.053
Clusters	46	46	46	46	23	23

Dep. var.: Domestic seasonal workers. 'Treatment' is the degree of exposure to exclusion. Observations are state-months. All regressions include state and month-by-year fixed effects. Standard errors clustered by state in parentheses. $\bar{\ell}_{1955}$ is average fraction of Mexicans among the state's total hired seasonal workers across the months of 1955. Covers only January 1954 to July 1973, as in original sources. Farm worker stocks missing in original sources for 1955 in Rhode Island and New Hampshire. If no workers reported for state-month in a month when source report was issued, assume zero. 'Exposed states' means states with nonzero *bracero* stocks in 1955 (i.e., only the 'high' and 'low' groups in the figures).

Table 3

Effects of *bracero* exclusion on the three types of domestic seasonal agricultural employment: Differences-in-differences with continuous treatment, monthly

Specification:	**linear**			**ln**		
Dep. var.:	**Local**	**Intra-state**	**Inter-state**	**Local**	**Intra-state**	**Inter-state**
$I_{t \geq 1965} \cdot \overline{\ell}_s^{1955}$	−2971.3	−9083.2	−578.7	−0.472	−0.997	−0.574
	(4677.9)	(9777.7)	(1127.7)	(0.738)	(0.639)	(0.458)
N	10329	6370	6371	6736	4720	5773
adj. R^2	0.055	0.052	0.016	0.064	0.080	0.052
Clusters	46	46	46	46	46	46

Notes: See Table 2. Here ‘local’, ‘intrastate’, and ‘interstate’ refer to three mutually exclusive, collectively exhaustive types of domestic hired seasonal farm workers.