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EVIDENCE FROM THE MEXICAN BRACERO EXCLUSION

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ABSTRACT

An important class of active labor market policy has received little rigorous impact evaluation: immigration barriers intended to improve the terms of employment for domestic workers by deliberately shrinking the workforce. Recent advances in the theory of endogenous technical change suggest that such policies could have limited or even perverse labor-market effects, but empirical tests are scarce. We study a natural experiment that excluded almost half a million Mexican ‘bracero’ seasonal agricultural workers from the United States, with the stated goal of raising wages and employment for domestic farm workers. We build a simple model to clarify how the labor-market effects of bracero exclusion depend on assumptions about production technology, and test it by collecting novel archival data on the bracero program that allow us to measure state-level exposure to exclusion for the first time. We cannot reject the hypothesis that bracero exclusion had no effect on U.S. agricultural wages or employment, and find that important mechanisms for this result include both adoption of less labor-intensive technologies and shifts in crop mix.

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A large literature evaluates how active labor market policy affects workers' wages and employment (LaLonde 2016; Crépon and van den Berg 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 endogenous technological advance that alters labor's marginal product. This can result in a flat or even upward-sloping labor demand curve—under models of directed technical change (Acemoğlu 2002, 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 et al. 2010). But the applicability of these models in practice remains an open empirical question (Acemoğlu 2010, 1071), and the limited rigorous evidence to date does not directly evaluate changes in workforce policy (Lewis 2011; Hornbeck and Naidu 2014; Hanlon 2015).

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 excluding 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. *Bracero* exclusion was conceived and executed as a federal policy measure 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 goals as active labor market policy. 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. This allows us to test the model's prediction that the economy adjusts to *bracero* exclusion by technological advance in subsectors where that is possible, and by Rybczynski adjustment otherwise—in either case without substantial labor-market effects.

The contribution of this work is to test a model of endogenous technical change stimulated by a very large reduction in labor supply with a natural quasi-experiment, and to evaluate the impact of a type of active labor market policy that is rarely directly evaluated. The literature testing models of endogenous technical change, going back to Hayami and Ruttan (1970), lacks a policy

experiment of deliberate labor supply reduction. And 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 But ‘refugee push’ and ‘policy pull’ effects could differ, given that refugees enter the labor market differently from other migrants (e.g. Cortes 2004), and the large majority of migrants are not refugees. Very recently the immigration literature has turned to evaluating labor-market effects of real-world changes to immigration policy restrictions (e.g. Beerli and Peri 2015; Kennan 2017), but evidence remains scant, and the *bracero* program in particular remains little-studied (Kosack 2016).² This is in part because immigration policies—like licensing restrictions in trade policy—are inherently difficult to quantify (Beine et al. 2015). Historical quasi-experiments are a largely untapped resource, both to establish short-run causal identification and to explore the speed of adjustment over time (Lewis 2013, 177, 182).

We find that *bracero* exclusion had little measurable effect on the labor market for domestic farm workers. Pre- and post-exclusion farm wages and farm employment were similar in states highly exposed to exclusion—which lost roughly one third of hired seasonal labor—and in states with no exposure. *Bracero* workers were not substantially replaced in the years immediately following exclusion with domestic workers, unauthorized Mexican workers, or authorized non-Mexican foreign workers. We find instead that employers adjusted to exclusion, as predicted by the theory of endogenous technical change, with large changes in technology adoption and crop production. We reject the semielasticity of wages to labor scarcity implied by the model in the absence of endogenous technical change, and offer direct evidence of induced technical advance. These findings suggest that new theories of technological change can inform the design and evaluation of active labor market policy.

Below in Section 1 we describe the exclusion of *bracero* workers and prior research on its eco-

¹Studies exploiting overseas refugee ‘push’ for identification include most influential studies of the labor-market effects of immigration based on natural experiments: Card (1990); Hunt (1992); Carrington and de Lima (1996); Friedberg (2001); Angrist and Kugler (2003); Gandal et al. (2004); Glitz (2012); Braun and Omar Mahmoud (2014); Borjas and Doran (2015); Foged and Peri (2016).

²Studies 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. González and Ortega 2011; Lafortune et al. 2015). A small strand of literature evaluates the effect of changes in immigration policy on migration, finding that visa requirements tend to markedly reduce bilateral mobility, but does not test the effects of these changes on labor-market conditions or technological change in the migrant-destination country (Ortega and Peri 2013; Freier 2014; Bertoli and Fernández-Huertas 2015; Lawson and Roychoudhury 2016).

nomic effects. [Section 2](#) then presents a stylized model of the labor-market effects of active labor supply reduction policy under endogenous technical advance. [Section 3](#) describes our novel archival data. [Sections 4](#) and [5](#) present empirical tests of the model, and [Section 6](#) concludes.

1 The exclusion of *bracero* workers

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. Here we review the history of the intervention and the literature that has sought to evaluate its effects.

1.1 The *bracero* agreements

The United States and Mexico made a set of three bilateral agreements to regulate Mexico-U.S. flows of temporary low-skill labor, spanning almost the entire period from 1942 through 1964.³ At first this *bracero* program supplied Mexican labor to a mix of manual jobs in U.S. agriculture, railroads, mining, and construction, raising remittance receipts and human capital investment in Mexico ([Kosack 2016](#)). After World War 2 the program evolved to focus almost entirely on agriculture, and grew to supply almost half a million seasonal workers each year to U.S. farms, with contracts typically lasting between six weeks and six months ([Salandini 1973](#), 158). The Kennedy administration began the process of *bracero* exclusion in March 1962, by making *braceros* “far less attractive” to farmers by greatly raising the required wage rate ([Craig 1971](#), 180). The Johnson administration completed *bracero* exclusion at the end of 1964 by fully terminating the final iteration of these agreements. To date, the program “has been widely criticized and not thoroughly studied” ([Durand 2007](#), 25).

The *bracero* agreements are sometimes portrayed as a government policy to ‘import’ labor to offset shortages originating in the absence of U.S. workers during World War Two (e.g. [Scruggs 1963](#)) and the Korean War (e.g. [Morgan 2004](#), 127). They are more correctly described as a relaxation of restrictions on private employers’ contracting with Mexican workers, given that the

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

principal lever of government policy in regulating migration—as in regulating trade—is the degree of restriction on private firms.⁴ Long before the *bracero* agreements, there was large-scale private-sector Mexican labor supply to the U.S. Southwest and Great Plains states, from the late 19th century through the 1920s (Clark 1908; Gamio 1930; Smith 1981; Durand et al. 2000). In Arizona, Kansas, and New Mexico, the Mexican-born fraction of the labor force was much higher in 1930—in peacetime—than it would be again until after 1990 (Borjas and Katz 2007, 19). In 1929 began 13 years of tight restrictions including mass deportations that reduced the Mexican-born fraction of the U.S. by a third.⁵ The *bracero* agreements temporarily eased these restrictions, from the 1940s until *bracero* exclusion after 1964. The beginning of the *bracero* program did not create U.S. demand for Mexican labor or Mexican supply of that labor, but the termination of the program did tightly restrict supply, as the 1929–1942 restrictions had previously done.

1.2 *Bracero* exclusion as active labor market policy

The principal policy goal of excluding *braceros* starting in 1965 was to improve labor-market conditions for U.S. farm workers by reducing the size of the workforce. “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 Congress voted to exclude *braceros*, 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 and cumulative in its impact” (Violet 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).

⁴The *bracero* agreements did not subsidize U.S. employers’ hiring of Mexican workers, as employers paid for the wages, housing, and transportation of those workers.

⁵Most Mexican departures during the 1930s arose from an organized deportation campaign begun by President Herbert Hoover—the first important *de facto* restriction of labor mobility across the border—though roughly 40 percent of emigrants departed voluntarily as jobs dried up (Taylor 1934, pp. 23–25; Gratton and Merchant 2013, 957–959). Before 1929 there was very little restriction on labor mobility: “As late as 1928, [the Border Patrol] had only 747 men to police the vast international boundaries (Canadian and Mexican) under its jurisdiction. Nor did members of the organization ever consider the apprehension of would-be illegal Mexican immigrants to be their primary function until the very end of the decade: most of their efforts went towards the enforcement of customs regulations and prohibition laws” (Cardoso 1980, 84).

The main evidence base for these claims at the time was the finding of a commission created by U.S. Secretary of Labor James P. Mitchell in 1959.⁶ President [Kennedy](#) (1962, 639) was referring to this study when he declared in October 1961, while reluctantly signing the law extending the program for two more years, “Studies of the operation of the Mexican labor program have clearly established that it is adversely affecting the wages, working conditions, and employment opportunities of our own agricultural workers.” The purpose of the commission was to independently and definitively assess whether or not the presence of *braceros* had adversely affected labor market conditions for U.S. workers. The commission concluded that it had, but not through an investigation that would be considered credible and transparent by economists or other social scientists today. Its analysis focused on anecdotes regarding the difficulty of measuring the ‘prevailing wage’ that *braceros* were required to receive, and the failure of some employers to pay *braceros* the prevailing wage ([Garrett et al. 1959](#)). But this was not direct evidence of changes to U.S. workers’ terms of employment. The commission did not use the systematic farm wage data that had been available from the Department of Agriculture for many years, nor the systematic data on unemployment available from the Bureau of the Census since 1957.⁷

In retrospect, the 1959 commission was not qualified to provide a disinterested, quantitative evaluation of the economic impact of this policy. The senior commissioner and only member with research training was Rufus von KleinSmid, who had co-founded an influential society of eugenicists that advocated blocking Mexican immigration due to their view that Mexicans constituted a genetically inferior race.⁸ This fact has gone unremarked in discussions of the 1959

⁶The 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 report also spurred an influential House Subcommittee report on the condition of farm workers in 1960, but that report did not assess the effect of *bracero* workers on domestic workers.

⁷Direct evidence of the effects on U.S. workers was considered only in a brief passage: “Between 1953 and 1958, the hourly farm wage rate in the U.S. increased 14 percent, according to the Department of Agriculture. An examination of wage surveys made by state agencies in areas using Mexican nationals showed that the average rate paid to domestic workers in these areas either remained unchanged or decreased in three-fifths of the cases” ([Garrett et al. 1959](#), 294). But it is difficult to assess this claim, as the authors did not display the data underlying the quoted sentence. They did not specify which area(s) of which state(s) they referred to, how many were examined, how they defined “using Mexican nationals”, or the degree to which the areas they examined were representative of areas with *braceros*. The Current Population Survey was expanded in 1956 to 330 primary sampling units with 35,000 households, allowing monthly state-level unemployment rates to be published starting in 1957.

⁸Von 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](#),

commission's findings, which typically describe the commission members as "impartial consultants" (Salandini 1973, 165).⁹ The commission's work has nevertheless been highly influential: it 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 by government analysts (Violet and McClure 1980, 58).

A successor of Mitchell's as Secretary of Labor, Willard Wirtz, spent years personally advocating for *bracero* exclusion (Hawley 1966). After the termination of the program, he issued a report stating that the exclusion had caused an improvement in the "terms and conditions of employment" for U.S. workers (Secretary of Labor 1966, 3, 11). The evidence offered to support this claim was the fact that farm wages across the nation rose during 1965 (p. 19). Secretary Wirtz likewise stated that "[t]ens of thousands of jobs were created for American workers" (p. 3), offering as evidence the fact that domestic seasonal farm employment rose between 1964 and 1965 in California, Michigan, and Texas (p. 10). There was no attempt to discern whether these trends preceded *bracero* exclusion, or happened to a different extent in states unaffected by the exclusion. Similarly, a report of the U.S. Senate (1966, 17) concluded that *bracero* exclusion had raised wages because agricultural wages rose between 1964 and 1965 in California, Arizona, and Texas—without comparing those increases to similar wage increases documented by the same report in dozens of states unaffected by *bracero* exclusion. Similar claims have persisted for decades: in 2004 Congressional hearings considering the creation of new temporary employment visas, both Democrats and Republicans asserted that the *bracero* agreements had reduced wages and employment for U.S. workers (U.S. House of Representatives 2004, 125, 130).

57; Stern 2005; 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).

⁹Beyond his eugenicist activism, Von KleinSmid was not an economist but a psychologist, born in 1875 and trained at a time when modern quantitative methods did not exist. He thus could not have possessed the tools to empirically evaluate the impact of a labor market policy. The other three members of the commission had no training in research methods: George Higgins was a labor activist and monsignor known as "the labor priest's priest" (Sullivan 2001); Edward Thye was a graduate of a vocational community college and a former dairy farmer and bureaucrat at state-level farm agencies before becoming a career politician; and Glenn Garrett ran the Good Neighbor Commission in Texas. All four commission members were handpicked by a Secretary of Labor who had sought for years to restrict and terminate the *bracero* program (Violet and McClure 1980, 45, 51).

1.3 Prior research findings on *bracero* exclusion

The policy debate on *bracero* exclusion proceeded and ended without any rigorous or disinterested evidence. “Anti-braceroists produced statistics . . . that increased employment of Mexicans had adversely affected the wages and working conditions of natives,” wrote [Craig \(1971, 144\)](#). “Those supporting [the law] presented equally elaborate data indicating clearly that the *bracero* had, if anything, proved beneficial to the wages and working conditions of domestics.” Years later, two studies in the economics literature sought to measure objectively and empirically the labor-market impacts of terminating the *bracero* agreements. But due to data and methods limitations, neither offers clear evidence for or against such impacts.

In the first of these studies, [Jones and Rice \(1980\)](#) measure farm wage and farm employment trends in four U.S. states 1954–1977, before and after the end of the *bracero* program, and do not detect any effects of the program’s termination. But the authors do not have access to data on the number of Mexican workers in each state. They are thus unable to measure the intensity of exposure to the program, or establish a control group of states with little or no exposure to the program.

In the second, [Morgan and Gardner \(1982\)](#) likewise seek to measure the effects of *bracero* exclusion on farm wages. They attempt this using a state-year panel for seven *bracero*-intensive states during 1953–1978. In regressions with state-average farm wages as the dependent variable, they find that a dummy for the years of the *bracero* program (1953–1964) has a negative and statistically significant coefficient. But it is difficult to give this coefficient any interpretation, given that 1) average farm wages across the country rose greatly during the period of time they analyze, and 2) the regressions do not include any time dummies or time trends. Thus a dummy variable for the early years of that period would have a negative coefficient even in states with no exposure to the program. But they do not analyze states with few or no *braceros*, so they, like [Jones and Rice](#), are unable to establish a credible counterfactual.

Despite this limited and problematic evidence on the labor-market effects of *bracero* exclusion, a long strand of literature has interpreted those effects as well-established. During the program, economists like [Hadley \(1956, 355\)](#) used anecdotes to claim that the presence of *braceros* reduced

domestic wages. “The availability of [*bracero*] workers,” wrote [Briggs \(1980\)](#) long after the program ended, “significantly depressed existing wage levels in some regions,” citing a calibrated simulation of what the wage effect of *bracero* exclusion in two crops might have been during the program, but not an empirical measurement of the wage effect of actual *bracero* exclusion ([Wise 1974](#)). In arguing against a renewed US-Mexico temporary worker agreement in the 1990s, [Martin \(1998, 892\)](#) claimed that *bracero* exclusion caused farm wages to rise by 30 to 50 percent, without a source for this estimate. [Grove \(1996, 320\)](#), citing the problematic results of [Morgan and Gardner](#), claimed that “employment of braceros reduced seasonal farm wages and domestic employment.” [Martin and Teitelbaum \(2001, 194\)](#) claim, “The rise in farm workers’ wages following the *bracero* program’s termination also showed how much it had depressed wages”—but do not compare this rise in wages to wage trends prior to *bracero* exclusion, or to wage trends in places unaffected by *bracero* exclusion. A literature review by [Levine \(2006, 5\)](#) concludes that the *bracero* agreements caused “a decrease in domestic farm worker employment, and a decrease in farm wages,” but without critical analysis of the evidence base.¹⁰ Social scientists outside of economics have made numerous similar claims.¹¹

But a minor literature at the time of exclusion dissented from this view. Just beforehand, [Mamer](#)

¹⁰In reviewing the evidence to draw her conclusion for the purposes of informing Congress in 2006 about the labor market effects of temporary work visas, [Levine](#) cites only the simulation of [Wise \(1974\)](#) and the regressions of [Morgan and Gardner \(1982\)](#) that did not account for the upward time-trend in wages nationwide, but omits the contradictory findings of [Jones and Rice \(1980\)](#).

¹¹Some historians and legal scholars have made the same claim, but without systematic quantitative evidence. First, in an influential book that helped secure *bracero* exclusion, historian Ernesto [Galarza \(1964, 199-218\)](#) claimed that the effect of *braceros* on domestic workers’ wages and employment was “severe”. To support this claim he offered several pages of anecdotes about *braceros* being paid relatively low wages for certain crops in certain counties of California for work that in other years had been performed by domestic workers, but does not offer evidence on the wages earned by domestic workers for the work they performed instead. Second, law professor Austin [Morris \(1966, 1940\)](#) wrote, “It was charged that ... Bracero-users never seriously tried to attract domestic labor, and that wages were kept down, and even lowered. ... That little effort was made, either by administrative insistence or grower cooperation, to recruit local workers is evidenced by the fact that as the number of entrants was cut down toward the end of the program (from a high of 459,850 in 1956 to about 200,000 in 1964, and only 20,000 in 1965), their places were taken by domestic workers. If domestic workers could be recruited in 1965, as they were, then clearly they could have been recruited in 1956.” He thus described it as “false, exaggerated, and baseless” to suggest that *braceros* had *not* displaced large numbers of domestic workers (p. 1943). But [Morris](#)—though he was writing two years after *bracero* exclusion and could have measured its observed effects—offers no evidence that hundreds of thousands of domestic workers did in fact enter seasonal farmwork to replace *braceros* as claimed. Third, [Bickerton \(2000, 910\)](#) decries the “low wages that resulted from the importation of foreign workers” under the agreements. To support this claim [Bickerton](#) cites a passage where [Calavita \(1992, 65 passim\)](#) discusses some *bracero* employers’ violations of their wage contracts; but this does not constitute direct evidence of adverse effects on the wages of other workers. Fourth, [Mitchell \(2012, 396\)](#) suggests that due to the exclusion of *bracero* workers from California, “[w]ages to domestic workers went up 23 percent”, but does not offer evidence that this trend was any different in places unaffected by *bracero* exclusion, or that a similar trend was not occurring in California prior to *bracero* exclusion.

(1961, 1205–1206) noted the absence of any systematic quantitative evidence for substantial labor-market effects of the program. And after exclusion was carried out, some economists predicted that rapid technological adjustment would negate its intended effects. 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 for Mexican workers, “excluding foreign labor will not have any lasting beneficial effects on the domestic farm labor force.” However, these predictions—that *bracero* exclusion would fail as active labor market policy, and why it would fail—have never been systematically tested.

2 A model of workforce reduction and technical advance

Here we seek to clarify testable implications of the above, skeptical view of *bracero* exclusion. To do this, 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 Acemoglu (2010) we refer to a technical advance as either the creation of a new production technology or the adoption of an existing technology.

2.1 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 endowments of land and labor are fixed at \bar{L} and \bar{T} respectively, and capital and materials are supplied elastically. 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 production technology using what will be called “traditional” capital K_0 , can be used to produce

the output crop Y . Production Y_0 with traditional technology is given by:

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

where for simplicity $T \equiv \min\{\tau, M\}$. We also assume, consistent with the evidence in [Herrendorf et al. \(2015\)](#) that $\mu, \sigma > 1$. K_0 is elastically supplied at rental rate r_0 , so in equilibrium,¹² $r_0 = \left\{ K_0^{\frac{\mu-1}{\mu}} + \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}} \right\}^{\frac{1}{\mu-1}} K_0^{\frac{-1}{\mu}}$, and thus $Y_0 = \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1}-1} \right)^{\frac{\mu}{\mu-1}} \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}$. Farmers are willing to rent land as long as $r_T + m$ is less than the marginal product of land. This implies that there is a cutoff $\bar{\phi}$ such that if $\bar{L}/\bar{T} < \bar{\phi}$, farmers will not rent all of the land available. This cutoff is defined by

$$(1-a) \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1}-1} \right)^{\frac{\mu}{\mu-1}} \left[a\bar{\phi}^{\frac{\sigma-1}{\sigma}} + (1-a) \right]^{\frac{\sigma}{\sigma-1}-1} = \underline{r}_T + m. \quad (2)$$

A potentially binding cutoff exists only if $\left(\frac{r_0^{\mu-1}}{r_0^{\mu-1}-1} \right)^{\frac{\mu}{\mu-1}} (1-a)^{\frac{\sigma}{\sigma-1}} < \underline{r}_T + m$ (which, notice, can occur even if $\underline{r}_T = 0$). The existence of this cutoff implies that the equilibrium wage never exceeds $\bar{w} = a \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1}-1} \right)^{\frac{\mu}{\mu-1}} \left[a + (1-a)\bar{\phi}^{-\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}-1} = \left[\left(\frac{r_0^{\mu-1}}{r_0^{\mu-1}-1} \right)^{-\frac{\mu(\sigma-1)}{\mu-1}} - (1-a)^{\sigma} (\underline{r}_T + m)^{1-\sigma} \right]^{-\frac{1}{\sigma-1}}$. If the cutoff binds, only $\bar{L}/\bar{\phi}$ acres of land will be used.

For some crop locations, the crop can alternatively be produced using an “advanced” or automated technology, where crop production Y_A is given by

$$Y_A = \left\{ K_A^{\frac{\mu-1}{\mu}} + \left[bL^{\frac{\sigma-1}{\sigma}} + (1-b)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}} \right\}^{\frac{\mu}{\mu-1}}, \quad (3)$$

where K_A is elastically supplied advanced-production capital. 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. In equilibrium, advanced production satisfies $Y_A = \left(\frac{r_A^{\mu-1}}{r_A^{\mu-1}-1} \right)^{\frac{\mu}{\mu-1}} \left[bL^{\frac{\sigma-1}{\sigma}} + (1-b)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}$. It is also only meaningful to talk about there being an “alternative technology” if each technology would be employed at least *some* values of labor/land ratios, which requires that $\left(\frac{1-a}{1-b} \right)^{\sigma} < \left(\frac{r_A^{\mu-1}}{r_A^{\mu-1}-1} / \frac{r_0^{\mu-1}}{r_0^{\mu-1}-1} \right)^{\frac{\mu}{\mu-1}(\sigma-1)} < \left(\frac{a}{b} \right)^{\sigma}$. Thus, without loss of generality, we assume that

¹²Derived in the Online Appendix.

this condition holds whenever an alternative technology exists.¹³

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. The optimal choice of technology depends on highly localized conditions of land price and capital price.¹⁴ Indeed, 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. 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—the cone of diversification. At the upper end, only the older technology is used, and at the lower end only the advanced technology is used. The cone of diversification is defined by the solution $[\phi_\ell, \phi_u]$ to

$$\begin{aligned} a \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[a + (1-a) \phi_u^{-\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}-1} &= b \left(\frac{r_A^{\mu-1}}{r_A^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[b + (1-b) \phi_\ell^{-\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}-1} \\ (1-a) \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[a \phi_u^{\frac{\sigma-1}{\sigma}} + (1-a) \right]^{\frac{\sigma}{\sigma-1}-1} &= (1-b) \left(\frac{r_A^{\mu-1}}{r_A^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[b \phi_\ell^{\frac{\sigma-1}{\sigma}} + (1-b) \right]^{\frac{\sigma}{\sigma-1}-1}. \end{aligned} \quad (4)$$

Dividing the first by the second equation reveals that $\phi_u = \left(\frac{1-b}{b} \frac{a}{1-a} \right)^\sigma \phi_\ell$, and hence that $\phi_u > \phi_\ell$, as is required for the existence of a meaningful cone of diversification. Furthermore,

$$\phi_\ell = \left(\frac{1-b}{b} \right)^{\frac{\sigma}{\sigma-1}} \left[\frac{\left(\frac{r_A^{\mu-1}}{r_A^{\mu-1} - 1} / \frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}(\sigma-1)} - \left(\frac{1-a}{1-b} \right)^\sigma}{\left(\frac{a}{b} \right)^\sigma - \left(\frac{r_A^{\mu-1}}{r_A^{\mu-1} - 1} / \frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}(\sigma-1)}} \right]^{\frac{\sigma}{\sigma-1}} \quad (5)$$

¹³Specifically, this is a necessary condition for the cone of diversification to exist. In contrast, if $\left(\frac{1-a}{1-b} \right)^\sigma > \left(\frac{r_A^{\mu-1}}{r_A^{\mu-1} - 1} / \frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}(\sigma-1)}$, then the alternative technology would produce less output than the traditional technology at all combinations of inputs (and so would never be used). The alternative technology will also never be used if $\bar{\phi}$ exceeds the labor/land ratio at the upper end of the cone of diversification, ϕ_u , defined below.

¹⁴Even if capital is frictionlessly mobile, the price of hiring capital could vary from place to place. For example, the efficient use of a particular machine could depend on planting an improved seed variety, the suitability of which could depend on local soil conditions.

Inside the cone, the wage is fixed at $\hat{w} = b^{\frac{\sigma}{\sigma-1}} \left(\frac{r_A^{\mu-1}}{r_A^{\mu-1}-1} \right)^{\frac{\mu}{\mu-1}} \left[\frac{\left(\frac{a}{b} \right)^\sigma - \left(\frac{1-a}{1-b} \right)^\sigma}{\left(\frac{r_A^{\mu-1}}{r_A^{\mu-1}-1} \right)^{\frac{\mu}{\mu-1}(\sigma-1)} - \left(\frac{1-a}{1-b} \right)^\sigma} \right]^{\frac{1}{\sigma-1}}$.

The cone of diversification is 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 both U.S. and Japanese agriculture for around 30 years (Binswanger 1986).

2.2 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 $\bar{L} \equiv B + N$. (Later we will revisit the assumption that these workers are perfect substitutes.) 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 $\% \Delta (\bar{L}/\bar{T}) = \frac{N/T - (B+N)/T}{(B+N)/T} = -\frac{B}{L}$. We explore the wage response to *bracero* exclusion (1) without the adjustment of capital, technology, or output (2) with the adjustment of capital, but not technology or output, and (3) with the adjustment of capital, technology, and output.

Without adjustment of capital, technology, or output. Using the traditional technology, the wage is given by

$$w = a \left\{ \left(\frac{K}{\bar{T}} \right)^{\frac{\mu-1}{\mu}} + \tilde{L}^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}} \right\}^{\frac{\mu}{\mu-1}-1} \tilde{L}^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}-1} \left(\frac{L}{\bar{T}} \right)^{\frac{-1}{\sigma}}, \quad (6)$$

where $\tilde{L} \equiv a \left(\frac{L}{\bar{T}} \right)^{\frac{\sigma-1}{\sigma}} + (1-a)$. Thus in the absence of adjustment in capital, technology, or output, exclusion causes the wage to rise:

$$\frac{\partial \ln w}{\partial (B/L)} \approx -\frac{\partial \ln w}{\partial \ln(L/\bar{T})} \approx s_K \frac{s_L}{s_L + s_T} \frac{1}{\mu} + \frac{s_T}{s_L + s_T} \frac{1}{\sigma} > 0, \quad (7)$$

where s_T, s_L, s_K are the income shares of land (plus materials), labor, and capital, respectively.¹⁵

¹⁵Expression (7) is derived in the Online Appendix.

Using the parameter estimates of [Herrendorf et al. \(2015\)](#) for postwar U.S. agriculture, the semielasticity (7) would be large, approximately 0.4.¹⁶ In a typical high-*bracero* state with $B/L = 0.3$, farm wages rise by approximately 12 percent after exclusion.

With adjustment of capital, but not technology or output. Most of the above response arises from the assumption that farms cannot purchase capital. If we allow capital to adjust, but not technology, equation (7) reduces to

$$\frac{\partial \ln w}{\partial(B/L)} \approx \frac{s_T}{s_L + s_T} \frac{1}{\sigma} > 0. \quad (8)$$

Under the same parameter assumptions, the magnitude of the semielasticity (8) is approximately 0.1, or one quarter as large as without capital adjustment. In a typical high-*bracero* state farm wages rise by approximately 3 percent after exclusion.

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 ($(B + N)/\bar{T} \leq \bar{\phi}$). The model then predicts three effects of exclusion. First, wages do not rise:

$$\frac{\partial \ln w}{\partial(B/L)} = 0, \quad (9)$$

thus the wage remains fixed at $w \equiv \hat{w}$ before and after exclusion. Second, the output share of the automatic harvest technology (or of less labor-intensive crops) rises in pre-exclusion *Bracero* share:

$$\frac{\partial \ln(Y_A/Y)}{\partial(B/L)} > 0. \quad (10)$$

Third, the output of the traditional technology, or of labor-intensive crops without an alternative technology, falls:

$$\frac{\partial Y_0}{\partial(B/L)} < 0. \quad (11)$$

¹⁶That is, with $\mu, \sigma \equiv 1.6$, $s_K \equiv 0.54$, $s_T \equiv 0.07$, and $s_L \equiv 0.39$. [Herrendorf et al. \(2015\)](#) specify the production function differently, imposing that capital and land are perfect substitutes. They estimate the elasticity of substitution between labor and this land-capital aggregate to be 1.6. They also estimate this aggregate's share of output at 0.61; separately, they report that land's share of output is around 0.07. 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 would imply even larger wage impacts, though more recent elasticity estimates tend to be larger in magnitude and be more in line with the magnitude of wage impacts predicted by [Herrendorf et al. \(2015\)](#)'s estimates.

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 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 [Acemoglu \(1998\)](#).¹⁷ 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. Notice that 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. In practice, both types of adjustments can occur, and both reduce potential wage impacts.¹⁸

[Figure 1](#) summarizes the model, juxtaposing the traditional production function (1) and the advanced production function (3). In the figure, *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 $\bar{\phi}$.

2.3 Extensions

Domestic labor supply. Suppose non-*bracero* workers are drawn from a population P and supply labor to farms as $\ln \frac{N}{P} = \varepsilon \ln w$, with $\varepsilon > 0$. If $\frac{\partial \ln w}{\partial (B/L)} = 0$, then excluded *bracero* workers will not be replaced with domestic (non-*bracero*) workers. Alternatively if $\frac{\partial \ln w}{\partial (B/L)} > 0$, then domestic workers will flow into the farm sector, further reducing the magnitude of the wage response.¹⁹ However, estimates of the domestic labor supply elasticity to the farm sector tend to be small ([Devadoss and Luckstead 2008](#); [Clemens 2017](#)). [Devadoss and Luckstead \(2008\)](#) find that the simulated wage impacts of adding workers are nearly identical over a reasonable range

¹⁷Indeed, a broad set of directed technical change (e.g., [Acemoglu 2007](#)) and choice-of-technique (e.g., [Beaudry and Green 2003, 2005](#); [Caselli and Coleman 2006](#)) models allow for flat 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.

¹⁸[Lafortune et al. \(2015\)](#) find that in the early twentieth century, farms adjusted to immigration-driven labor supply shocks both by adjusting technology and crop mix. They provide suggestive evidence that the latter was important only on land that was well-suited to multiple kinds of crops.

¹⁹In this case, ε could be estimated from the relative response of domestic employment, $\frac{\partial \ln N}{\partial (B/L)} / \frac{\partial \ln w}{\partial (B/L)}$.

of estimated supply elasticities.

Worker specialization. Now suppose there are two kinds of farm jobs, skilled (S) and unskilled (U), and redefine $L \equiv \left[\alpha_L U^{\frac{\rho-1}{\rho}} + (1 - \alpha_L) S^{\frac{\rho-1}{\rho}} \right]^{\frac{\rho}{\rho-1}}$. The expression for how average wages respond to a differential change in L/T remains as above, so to determine the impact of the *bracero* exclusion on average wages in this setup, we need only to update our expression for how it affects L/T . Log-differentiating, $d \ln(L/T) = \frac{s_U}{s_L} d \ln(U/T) + \frac{s_S}{s_L} d \ln(S/T)$, where s_U is the unskilled share of output and $s_S \equiv s_L - s_U$. So regardless of whether the *braceros* are skilled or unskilled, we can continue to expect $d \ln(L/T) < 0$, and thus weakly positive average wage impacts, as before.

The magnitudes may change, however. Consider briefly the specific case in which all *braceros* (and some non-*braceros*) are unskilled, so that $d \ln(L/T) = \frac{s_U}{s_L} d \ln(U/T) = \frac{s_U}{s_L} B/(B + N_U)$, where N_U is the non-*bracero* unskilled workforce. Also define N_S to be the skilled non-*braceros* workforce, so $N_U + N_S = N$. In this case, if $\frac{s_U}{s_L} < \frac{B+N_U}{B+N}$, then the average wage impact will be systematically smaller than was described above (and if $\frac{s_U}{s_L} > \frac{B+N_U}{B+N}$, it will be larger).

Furthermore, regardless of skill mix, the change in average wages is not necessarily the same as the change in wages for non-*bracero* workers. That is instead given by

$$\frac{N_U}{N} d \ln w_U + \frac{N_S}{N} d \ln w_S = d \ln w + \left[\frac{N_S}{N} \frac{s_U}{s_L} - \frac{N_U}{N} \frac{s_S}{s_L} \right] \frac{1}{\rho} d \ln \left(\frac{U}{S} \right), \quad (12)$$

where w_S, w_U are skilled and unskilled wages.²⁰ The term in brackets can be positive, which is especially likely when *braceros* are disproportionately unskilled.²¹ In this case $d \ln \left(\frac{U}{S} \right) < 0$, so the average wages of non-*bracero* workers may even fall – rather than rise – after exclusion, an outcome that is more likely if skilled and unskilled labor are highly complementary (ρ small) or if we are in one of the cases where the average wage impact is near zero. Furthermore, this would lead to smaller magnitude increase (or even a decrease) in employment responses to the exclusion of *braceros* (using the model of labor supply above).

²⁰Expression (12) uses expressions for the change in the average wages of skilled and unskilled workers derived in the Online Appendix.

²¹ $\frac{N_S}{N} \frac{s_U}{s_L} - \frac{N_U}{N} \frac{s_S}{s_L} > 0 \Leftrightarrow \frac{N_S}{N_U} > \frac{s_S}{s_U} = \frac{w_S(N_S+B_S)}{w_U(N_U+B_U)} = \frac{w_S N_S/N_U + w_S B_S/N_U}{w_U(1+B_U/N_U)} = \frac{w_S}{w_U(1+B_U/N_U)} \frac{N_S}{N_U} + \frac{w_S B_S/N_U}{w_U(1+B_U/N_U)}$, where B_U are unskilled *braceros* and B_S are skilled *braceros*. This holds for sufficiently high B_U/N_U , especially if B_S is small. Suppose, for example, that there are no skilled *braceros*, i.e., $B_S \equiv 0$. In that case, a sufficient condition is that $\frac{w_S}{w_U} < 1 + \frac{B_U}{N_U}$.

3 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. State coverage is complete after 1953, the period relevant to the empirics below, but there are gaps before that. The information published in government sources over this time period was originally gathered through a monthly farm survey conducted by the Department of Labor's state-level employment service offices using the ES-223 form. The survey was conducted in areas estimated to employ over 500 migrant and/or seasonal farm workers during the enumeration period, typically completed by 20,000–25,000 farmers each month by mail (USDA 1958, 4). The form defined seasonal hired workers as those hired to work on a farm for fewer than 150 consecutive days. Domestic workers encompass three separate, mutually exclusive groups: local (those living within commuting distance); intrastate (those whose permanent residence is elsewhere in the state but who are temporarily residing in the area of employment for the purpose of farm work); and interstate (those whose permanent residence is in a state different from the area of farm employment). It is important to note that the form defined foreign workers as those "who have legally entered the continental United States but who normally reside in a foreign country," specifically indicating that "illegal entrants are not to be included" (DOL 1955). Data therefore exclude unauthorized farm workers.

Survey results were reported in the unpublished U.S. Department of Labor series 'In-Season Farm Labor Reports' for collation at the national level (USDA 1986, 17). Due to changes in bureaucratic organization and responsibilities over this time period, the final publication outlet for these data varies. Worker numbers for 1943–1947 and 1954–1973 were compiled from six

different Department of Agriculture and Department of Labor publications held in four archival locations. Data was not compiled on a national level for 1948–1953; state-level information for this time period for 15 states was sourced from twelve archival locations and library systems. The Online Appendix reports further detail.

The same survey collected information on farm wage rates on a quarterly basis. Each survey respondent was asked to report average wage rates for hired farm labor in his/her locality on the date of enumeration. Farm wages were reported in the Department of Agriculture publication *Farm Labor*, available online through Cornell University’s Mann Library. We utilize two different wage measures with varying coverage. The first, the hourly composite wage rate, is a weighted average of reported per-hour rates calculated by the Department of Agriculture. These data are available from 1948–1970 with full geographic coverage. As a robustness check, we also use the daily wage without board, which covers more years but omits three states in most years (California, Oregon, and Washington). We report our findings using both wage measures.²²

We are unable to directly observe the degree of sampling error or respondent bias in these farm surveys. But there is little evidence that these were large. First, the Departments of Agriculture and Labor constructed the samples to be representative of farms in each state. Contemporary criticism of the surveys centered on inconsistent definitions of hired ‘workers’ and treatment of family labor (Johnson and Nottenburg 1951)—neither of which are an important concern with *bracero* workers—and did not mention concerns with sampling. Second, the same farms reported both the numbers of hired workers on their own farms and wages “in your locality”. If there were discord between their survey reports of hired Mexican workers and reports from other sources, this would suggest that the sampled farms inadequately covered areas with Mexican workers and could miss any effects on wages those workers might have. But national totals of Mexican workers as reported in the farm surveys accord well with independent reports of *bracero* volumes departing Mexico, suggesting that the farm surveys exhibit good coverage of the establishments and geographic areas that employed Mexican seasonal labor.²³ Third, some of the most obvi-

²²Farmers were asked about the going rate for hired farm labor in their locality. Thus if *bracero* workers were paid less than domestic workers, we would predict a purely compositional effect on the average wage in a locality due to removing *braceros*, separate from the equilibrium-price effect. Because no contemporary observers claimed that *braceros* were paid more than domestic workers, and many claimed the opposite, we thus interpret the wage effects we measure—comprising both compositional and price-equilibrium effects—as upper bounds on the pure price-equilibrium effect.

²³The data sources are compared in the Online Appendix.

ous forms of possible respondent bias would tend to make the subsequent analysis overstate the effects of *braceros* on labor market conditions. For example, if employers systematically understated the number of Mexican workers they hired, this would tend to bias upward any estimated wage impact of those workers.²⁴ Alternatively, if employers systematically reported agricultural wages as those paid only to non-Mexican workers, this would only make the wage reports more useful for the purpose of estimating the effect of Mexican workers on domestic workers.

4 Results: Labor market adjustment

The model in [Section 2](#) predicts that even a large negative shock to the foreign labor supply in agriculture could have minimal effects on the domestic labor market, provided that capital, technology, and/or output can adjust. We begin by testing the predicted effects of *bracero* exclusion on labor market adjustment in this section, and proceed to test other adjustment margins in the following section. We use two regression specifications to estimate the effects of *bracero* exclusion on various economic outcomes. The first, quasi-experimental specification uses the natural experiment of *bracero* exclusion to estimate the effects of exclusion. As a robustness check, the second specification describes the observational relationship between *bracero* stocks and outcomes during the program.

4.1 Quasi-experimental tests

For each labor-market outcome, the first specification evaluates the effect of *bracero* exclusion as a quasi-experiment. Treatment is the degree of exposure to exclusion, defined as the fraction of seasonal agricultural labor in the state constituted by *braceros* at the program’s height in the mid-1950s. We use the differences-in-differences with continuous treatment specification, following [Card \(1992\)](#) and [Acemoglu et al. \(2004\)](#), of

$$y_{st} = \alpha' \mathbf{I}_s + \beta' \mathbf{I}_t + \gamma \left(I_{t \geq 1965} \cdot \bar{\ell}_s^{1955} \right) + \varepsilon_{st}, \quad (13)$$

²⁴For instance, if 10,000 workers caused wages to fall by one percent, but the presence of only 5,000 was reported on the surveys, the wage decline per Mexican worker would be estimated as double its true value. In the Online Appendix we present independent corroborating data from the Mexican government suggesting that U.S. farmers did not greatly understate their employment of *braceros* in the U.S. Department of Agriculture surveys.

where y_{st} is the outcome in state s in year, quarter, or month t , \mathbf{I}_s is a vector of state fixed effects, \mathbf{I}_t is a vector of time fixed effects,²⁵ $I_{t \geq 1965}$ 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 $\bar{\ell}_{1955}$ 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 variable ε_{st} is an error term, α and β 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. We compute [Liang-Zeger \(1986\)](#) standard errors, clustered by state.²⁶

Wages: [Figure 2](#) illustrates the core result of the paper, informally testing the zero wage-effect condition (9). The black line shows the average real wage for hired farm labor across states with highest exposure to *bracero* exclusion.²⁷ These are six states where *braceros* made up more than 20 percent of all seasonal agricultural labor in 1955: Arkansas, Arizona, California, New Mexico, South Dakota, and Texas. For the average state in this group, *braceros* made up approximately one third of the seasonal farm workforce in the late 1950s. The gray line shows the average wage in states with lower exposure to exclusion: states that had some *braceros* in 1955, but less than 20 percent of seasonal agricultural labor. For the average state in this group, *braceros* were approximately 3–4 percent of the seasonal farm workforce in the late 1950s. The dashed line shows the average wage in states with no exposure to exclusion: states that had zero *braceros* in 1955 and negligible numbers thereafter.²⁸

The figure shows that pre- and post-exclusion trends in real farm wages are similar in high-exposure states and low-exposure states. It also shows that wages in both of those groups rose more slowly after *bracero* exclusion than wages in states with no exposure to exclusion. This pattern confirms systematically what was remarked on anecdotally by economists—albeit in contrast to the prevailing narrative—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 conse-

²⁵In 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.

²⁶The Online Appendix presents a reanalysis of the results to follow with the ‘treatment’ year assumed to be 1962, when the first major restrictions on hiring *braceros* began, rather than 1965. The results are substantively unchanged.

²⁷Hourly wage, constant 1965 US\$ deflated with Consumer Price Index.

²⁸The exact fractions of *braceros* in the seasonal farm labor stock for each group of states over time are given in the Online Appendix.

quence 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 shows a linear parametric version of the same result, without classification of states into arbitrary groups, using the differences-in-differences with continuous treatment specification in equation (13). The first two columns use the hourly wage by state-quarter as the outcome; the second two columns use the daily wage without board.²⁹ Within each pair, the second regression narrows the window of analysis to five years before and after the termination of the program. All regressions include state and full quarter-by-year fixed effects, and standard errors are clustered by state. The state fixed effects absorb the influence of time-invariant differences between states, such as differences in arable land T in equation (6) or differences in the size of the overall state farm workforce at the time of exclusion. The difference-in-difference is negative—as predicted by equation (12) if *braceros* are relatively less-skilled than domestic workers—but statistically indistinguishable from zero in all cases.

The last three rows of Table 1 test the wage semielasticity prediction of the model. In all columns we reject at the 1 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 (7). We likewise reject at the 5 percent level the predicted wage semielasticity of 0.1 with adjustment of capital but without adjustment of technology or output, in equation (8). The results are therefore compatible with rapid adjustment of capital, technology, and output in equation (9).

All differences-in-differences analysis rests on the Stable Unit Treatment Value Assumption (SUTVA, Rubin 1977). This could be violated in the present case if wages in states without *braceros* were affected by *bracero* exclusion from other states. Suppose for example that the presence of *braceros* kept wages low in Arkansas, but the mere threat of bringing in *braceros* allowed employers to likewise keep wages low in Vermont, without any *braceros* present. Exclusion from Arkansas and other states could then in principle affect wages in Vermont. The evidence does not support this possibility 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*: no matter

²⁹The 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).

which of these two types of states are chosen as the control group, the difference-in-difference estimate is materially unchanged (Figure 2). SUTVA could also be violated if *bracero* exclusion caused large numbers of domestic workers to move between states; we will see below that this did not occur.

Employment: We now repeat the above analysis with employment of domestic seasonal farm workers as the outcome. Figure 3 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 zero-exposure states. In short, there is no clear sign that the exclusion of Mexican workers in the left panel caused employment of domestic workers in the right panel.

Figure 4 decomposes the domestic workforce in Figure 3 into local, intrastate migrant, and interstate migrant farm workers. In all three groups, post-exclusion trends are similar to pre-exclusion trends. There was a modest, transitory upsurge in the employment of local workers in high-exposure states during 1964–1966, but this was accompanied by a similar surge in zero-exposure states.

This null result is confirmed by the corresponding parametric differences-in-differences analysis with continuous treatment, from equation (13), in Table 2. The first two columns use all available data, with the outcome as either total domestic seasonal farm employment or its natural logarithm. The second two columns again restrict the window of analysis to just five years before and after the termination of the program. The coefficient estimates are negative but statistically indistinguishable from zero. Table 3 repeats the analysis for each type of domestic worker separately; the difference-in-difference estimate is negative and statistically indistinguishable from

zero in all cases.³⁰

This finding corroborates contemporary statements of the U.S. Department of Agriculture’s Economic Research Service, which wrote two years after exclusion, “Neither the growers nor the State Employment Service were able to recruit a labor force which would take over the jobs formerly performed by the braceros” (Metzler et al. 1967, 5). This result came despite many years of state and federal efforts to actively recruit domestic farm workers, both immediately after *bracero* exclusion and many years beforehand (U.S. Senate 1966; Metzler et al. 1967).

4.2 Observational robustness checks

To probe the robustness of these results we present alternative, standard specifications that require fewer assumptions but do not utilize the natural quasi-experiment. For example, we might be concerned that the above tests are misspecified because they do not capture important relationships between *bracero* stocks and domestic labor-market outcomes prior to exclusion, both linear and nonlinear. The second type of regression specification evaluates the relationship between the outcome and increases or decreases in the stock of *braceros* during the program, with state and time fixed-effects:

$$y_{st} = \alpha' I_s + \beta' I_t + \delta \ln L_{st}^{\text{mex}} + \zeta X_{st} + \varepsilon_{st}, \quad (14)$$

where X_{st} is a time-variant state characteristic that in some regressions is the stock of non-Mexican hired seasonal workers, $\ln(L_{st} - L_{st}^{\text{mex}})$. State fixed-effects capture any time-invariant state characteristics including initial *bracero* stocks, initial size of the domestic labor force, and initial crop specialization. The time fixed-effects capture any nationwide changes in the outcome during the period. We run both the linear specification in equation (14) and the corresponding semiparametric fixed-effects specification to allow for nonlinearities in the conditional relationship between *bracero* stocks and the outcome (Baltagi and Li 2002; Libois and Verardi 2013). In

³⁰In months where the source documents were published but no domestic worker stock is reported for a state-month, the analysis in Tables 2 and 3 assumes domestic worker stocks to be zero. In subsection A5.3 we present the results under alternative assumptions: truncating missing values from the dataset in the linear specification, or assuming missing values to be zero and including them in the dataset in the nonlinear specification (using the inverse hyperbolic sine transformation rather than the logarithm). The results are qualitatively invariant to these alternative assumptions.

the fixed-effects regressions, too, we generally compute [Liang-Zeger](#) standard errors clustered by state to account for serially-correlated errors, but in some robustness checks we assume a first-order autoregressive structure of the error term.

Wages: [Table 4](#) runs the fixed-effects specification (14) to test the observational relationship between real farm wages and *bracero* stocks under program exposure (that is, in state-quarters with nonzero *bracero* stocks). The outcome in the first two columns is the real hourly composite wage. While the state fixed effect captures the effect all time-invariant omitted confounders on the wage, including the initial size of the farm sector and farm workforce, we could be concerned that the size of the farm workforce does vary substantially over time; for example, state-month specific demand shocks (such as weather shocks) could generate correlation between Mexican worker shocks and wages, but we would expect such demand shocks to be at least partially reflected by demand for non-Mexican labor. Thus the second column controls for the size of the non-Mexican seasonal labor force in each state-month. The third and fourth columns repeat these tests with the real daily wage without board as the outcome. Rather than cluster standard errors by state, the fifth and sixth columns impose the assumption of first-order autoregressive errors within states.

Bracero stocks are positively correlated with farm wages throughout, again as predicted by equation (12) if *braceros* are typically less-skilled than domestic workers. But this correlation is statistically indistinguishable from zero in the most flexible specifications—columns 2 and 4—which allow for a time-varying overall farm workforce and an arbitrary structure of autocorrelated errors. The evidence thus fails to reject prediction (9) of no relationship between wages and declining *bracero* stocks.

We might be concerned that this linear fixed-effects specification conceals nonlinearities, such as if negative wage effects only set in at high concentrations of *braceros*.³¹ [Figure 5](#) shows the [Baltagi-Li](#) semiparametric fixed-effects version of the parametric regression in [Table 4](#), column 1. There is no evidence of marked nonlinearities in the relationship.

Employment: The fixed-effects regression of equation (14) takes log domestic seasonal farm em-

³¹[Braun and Omar Mahmoud \(2014\)](#) find that postwar German expellees' displacement effects on incumbent workers are highly nonlinear, only setting in at the highest inflow rates.

ployment as the outcome. [Table 5](#) shows this regression in column 1. The remaining columns break down the three types of domestic seasonal employment as outcomes. None of the coefficient estimates are negative, and all are statistically distinguishable from zero. Controlling for state and month-by-year fixed effects, there is no evidence of a rise in domestic employment concurrent with declines in *bracero* employment in state-months with nonzero *bracero* employment. If anything, these results suggest a fall in domestic farm employment concurrent with falls in *bracero* stocks during the program, which is compatible with some degree of complementarity between Mexican and domestic labor as in equation (12). [Figure 6](#) shows the semiparametric fixed-effects version of the regression in column 1 of [Table 5](#), which shows no significant tendency for domestic employment to rise with falling *bracero* stocks even at very high *bracero* stocks.

4.3 Illegal migration and other labor-market explanations

Before proceeding, we consider whether the above results are likely to arise from various adjustments within the labor market. The first is the possibility that when the program was terminated, *braceros* simply became unauthorized workers, or were replaced by unauthorized workers. *Prima facie* this appears unlikely to explain the lack of measurable increases in domestic wages or employment caused by exclusion, given that during the program—when employers could access Mexican labor without hiring on the black market—there is no negative relationship between *bracero* stocks and lower domestic wages or employment ([Tables 4 and 5](#)). We nevertheless consider more direct evidence below.

Very few Mexican workers overstayed their *bracero* visas when the program was terminated. This is evident in the statistics of the Mexican government, which conducted all recruitment under the agreements and tracked each *bracero*'s exit from Mexico and reentry into Mexico ([González 1974](#), 141, Cuadro 39). In 1963, for example, 189,528 *braceros* left Mexico and 188,512 returned the same year (a discrepancy of 1,106, or 0.54 percent). In 1964, the last year of the program, 179,298 *braceros* left Mexico and 179,535 returned (a discrepancy of 237, or 0.13 percent). If it were the case that Mexican workers on the black market substituted for *braceros* in 1965, this would require a very large and immediate jump in *new* unlawful entries.

There is no evidence of such a jump. [Figure 7](#) compares the total number of *bracero* visas given each year to the number of Mexican nationals apprehended by the U.S. Border Patrol. Apprehensions barely rose in the several years that followed *bracero* exclusion. There is no evidence of any stand-down in enforcement effort in the years just before or after 1965 that would allow minimal apprehensions to coincide with massive new illegal flows; Border Patrol staffing did not fall but in fact rose modestly during this period ([North and Houstoun 1976](#), 53). Bibliometric analysis of contemporary newspaper content independently confirms that illegal migration remained very low for several years after *bracero* exclusion ([Massey and Pren 2012](#), 10).³²

Other alternative explanations are likewise unsupported by the data: 1) Excluded *braceros* were not replaced by lawful but non-Mexican foreign workers: stocks of Jamaican, Bahamian, and other non-Mexican foreign seasonal agricultural workers barely rose after *bracero* exclusion.³³ 2) It is unlikely that the labor-market effects of *bracero* exclusion were offset by reduced labor demand due to the loss of local expenditures from *bracero* earnings, the effect posited by [Altonji and Card \(1991\)](#) and tested by [Hong and McLaren \(2015\)](#), since *braceros* tended to live in isolated work-camps and spend only a small fraction of their earnings in the United States. 3) There is no evidence that substantial numbers of domestic seasonal farm workers moved between states to offset the loss of *bracero* labor supply, the effect considered by [Card and DiNardo \(2000\)](#) and [Hatton and Tani \(2005\)](#), given the lack of response by domestic interstate migrants in [Tables 3 and 5](#). This accords with the recent finding of [Cadena and Kovak \(2016\)](#) that domestic workers are far less mobile across U.S. states than Mexican workers. 4) Finally, it is unlikely that policy-generated wage rigidities are responsible for the lack of wage effects in [Tables 1 and 4](#). Hired farm workers were exempt from the federal minimum wage until years after *bracero* exclusion ([Gardner 1972](#)).³⁴ And even if there had been a binding minimum wage before and after exclusion, this would leave unexplained the lack of effects on employment.

³²The analysis of [Massey and Pren \(2012, 10\)](#) counts in major newspaper content “the number of times undocumented, illegal, or unauthorized migrants or aliens were paired with Mexico or Mexicans and with the words crisis, flood, or invasion each year from 1965 through 2009”. They find that these mentions closely track trends in Border Patrol apprehensions after 1965, remaining very low until sharply rising in the early 1970s.

³³Data presented in the Online Appendix.

³⁴The federal minimum wage for farm workers was only effective after 1967, and only covered about one third of hired domestic farm labor as it exempted all but the largest one percent of U.S. farms. Prior to this only five continental U.S. states had a state-level minimum for farm workers: California, Michigan, New Jersey, North Dakota, and Wisconsin ([Koziara 1967](#)). An immaterial exception to the lack of a federal minimum was sugar-beet workers, whose wage was regulated by a crop-specific minimum during the early postwar years ([BLS 1946, 197](#)).

5 Results: Technology and production adjustment

The above results suggest that *bracero* exclusion signally failed as an active labor market policy intended to cause increases in domestic wages and employment in farm work. Why? At the time of exclusion, as noted above, some economists predicted that its labor-market effects would be nullified by capital-labor substitution and technological adjustment (Jones and Christian 1965, 528; Martin 1966, 1137). These predictions equate to the model's predictions in equations (9)–(11), and we can now test them.

Mechanization technology was available for some of the most important crops produced with *bracero* labor, but not for others. Table 6 shows the principal crops where *bracero* labor was employed. Advanced mechanization technology was available for adoption to produce tomatoes, cotton, and sugar beets. No comparable mechanization technology was then available for production of asparagus, strawberries, lettuce, celery, cucumbers, citrus, or melons (Sanders 1965; Harper 1967). Equation (10) therefore predicts that *bracero* exclusion would accelerate mechanization in the production of tomatoes, cotton, and sugar beets. In these crops, the decline in production will be offset commensurately with the easy of switching to the advanced technology. And for the other crops, in the absence of substantial wage effects, equation (11) predicts that there will be a fall in production at the shutdown margin $\bar{\phi}$ accompanied by capital-labor substitution under the traditional technology, perhaps accompanied by switching to other, non-mechanized production techniques. In order to be capable of offsetting effects in the seasonal market for labor, adjustment at these other margins must occur rapidly.

5.1 Tomato mechanization

At the time of *bracero* exclusion, no single activity employed more *braceros* than tomato harvesting. 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). This placed tomato production within the technological diversification cone at the time of exclusion.

Figure 8a shows that *bracero* exclusion was followed immediately by a dramatic adoption of this existing technology, as predicted by equation (10). It compares *bracero* stocks in California, where most tomatoes were produced, to the fraction of California tomato production using a mechanical harvester. This corroborates qualitative studies claiming that exclusion caused sudden adoption of the harvester (Martin 1966, 1144; Martín 2001, 313). California is an appropriate state of focus because this mechanization episode covers the most important *bracero* crop nationwide, in the most important *bracero* state nationwide, as well as being the most important state for tomato production. To our knowledge, tomato mechanization time series data exist for only one other state, Ohio, which was essentially unaffected by *bracero* exclusion. An important falsification exercise is thus to contrast mechanization in Ohio with California. Figure 8b does this. There was no leap in tomato harvest mechanization in Ohio that was comparable in timing or magnitude to the leap in California.

5.2 Cotton mechanization

Another crop with coexisting traditional and advanced technologies was cotton. Commercial-scale mechanical picking of cotton had been technically feasible since the 1920s. It took two decades to become practically feasible for a substantial number of farmers, after the development of technologies complementary to mechanical picking—including cotton plant varieties with taller and more uniformly opening bolls, chemical for weed control to reduce trash in machine-picked cotton, and gins apt for machine-picked cotton (Fite 1980, 191–5). These were in place by the late 1940s and harvesting of upland cotton entered the cone of diversification, with eight percent mechanical harvesting by 1950, rising to 78 percent by 1964 (USDA 1974, 218).

The literature broadly agrees that the presence of *braceros* slowed harvest mechanization (Grove 1996) and *bracero* exclusion accelerated mechanization (Violet and McClure 1980, 46; Morgan and Gardner 1982, 399; Heinicke and Grove 2008, 288). McBride (1963) details how Labor Secretary James Mitchell’s regulatory actions to restrict *bracero* usage caused cotton farmers to universally adopt mechanical harvesters in the Lower Rio Grande Valley of Texas.

We observe the effect of *bracero* exclusion on cotton mechanization in Table 7. The table shows difference-in-difference regressions (13) in the first column, with state-year cotton harvest mech-

anization as the outcome, and shows in columns 3 and 4 the corresponding fixed-effects regressions (14). The first panel of [Figure 9](#) shows the fixed-effects result graphically. The difference-in-difference effect of exclusion is positive, and *bracero* stocks are negatively correlated with adoption during the program. We note that mechanical harvesting was only part of the technological change induced by *bracero* exclusion. Other labor-saving changes included pre-harvest technologies such as greater use of herbicides, flame cultivators, and rotary hoes ([Martin 1966](#), 1144).

5.3 Sugar beet mechanization

At the time of *bracero* exclusion, the mechanization transition in sugar beet production was not in harvesting—where mechanization had been near universal since the early 1950s—but in field preparation and maintenance. Sugar beet cultivation requires blocking and thinning (the removal of some plants so that the eventually-harvested plants can thrive) as well as weeding, activities traditionally employing intensive field labor. Mechanization of these tasks had been technically feasible for two decades ([Mervine and Barmington 1943](#)), but its spread was slow until the concomitant spread of seed varieties that avoided irregular growth and clumping of plants that reduced the efficiency of such machines. At the time of exclusion about 40 percent of U.S. farms had adopted mechanized thinning and weeding ([Rogers and Cohen 1963](#), 11, 22, Table 3), placing the crop near the middle of the diversification cone.

The literature broadly agrees that *bracero* exclusion created a large new incentive for mechanization ([Rasmussen 1967](#), 35; [USDA 1971](#), 16). The president of the American Society of Sugar Beet Technologists made this plain just before exclusion:

“In agriculture, complete mechanization from planting through harvest has been demonstrated and is practiced in limited areas. The demand for faster progress is being thrust upon us, however, by the imminent loss of a great proportion of the available hand labor through expiration of Public Law 78, commonly referred to as the Bracero Program. . . . Those who have depended upon availability of Mexican Nationals for thinning and weeding operations must look elsewhere to get this work accomplished. . . . Work to bring about the desired full mechanization must be pushed with all speed and in an all-out cooperative effort” ([Rorabaugh 1964](#), 2–3).

We also observe the effect of *bracero* exclusion on sugar beet mechanization in [Table 7](#). Column

2 shows the difference-in-difference results for state-year adoption of mechanized thinning and weeding in sugar beet production. These are less reliable than the analogous figures for cotton, because data are available for a more limited window of time.³⁵ Columns 5 and 6 show the corresponding fixed-effects regressions, shown graphically in the second panel of Figure 9. Just as for cotton, the difference-in-difference is positive and *bracero* stocks are negatively correlated with adoption.

5.4 Production adjustment

Equations (10) and (11) predict that *bracero* exclusion will cause a decline in production for all crops, a decline that is smaller in magnitude as there are lower barriers to technical advance. The model thus predicts only a small adjustment of production in tomatoes and cotton. It predicts a more substantial adjustment of production in sugar beets, where the spread of mechanization was limited by the spread of complementary technologies. And it predicts a large adjustment of production in crops where no feasible mechanization technology existed, so that capital-labor substitution must proceed under the traditional technology. Here we test and find support for all of these.

Crop production data are much more complete than surviving state-year data on mechanization. (Above, tomato mechanization data are available for only two states, sugar beet mechanization data for only four timepoints.) We thus test the model's predictions on production adjustment with the more flexible event-study specification (Jacobson et al. 1993):

$$y_{st} = \alpha' \mathbf{I}_s + \beta' \mathbf{I}_{t \neq 1964} + \gamma' \cdot \mathbf{I}_{t \neq 1964} \cdot \bar{\ell}_s^{-1955} + \varepsilon_{st}, \quad (15)$$

similar to equation (13) but where $\mathbf{I}_{t \neq 1964}$ is a vector of year dummies that omits the base-group 1964, and γ is a vector of parameters to be estimated ($\gamma_{1964} \equiv 0$). The outcome y_{st} is a state- and crop-specific index of physical production (e.g. pounds), scaled so that the index for each crop in each state equals 100 in 1964. Graphing the coefficient estimates $\hat{\gamma}_t$ against time shows how time-trends in crop production differed with the degree of state-level exposure to *bracero* exclusion.

³⁵The only state-year data of which we are aware for sugar beet thinning/weeding mechanization cover the period 1960–65, thus including only one post-exclusion year.

Figure 10 shows the event-study coefficients from regression (15) for nine of the most important *bracero* crops. At the top of the figure, we see *bracero* exclusion followed by relative declines in tomato and cotton production that are modest and short-lived, as the model predicts. Here, frictions on technical advance existed—for example, the new machines were inapt for delicate, fresh-market tomatoes comprising about one fifth of production (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 only proceed under the traditional technology, we observe large and lasting relative declines in production in and after 1965 in five: asparagus, fresh strawberries, lettuce, celery, and pickling cucumbers. Contemporary experts stated qualitatively that exclusion had caused important declines in production of all of these crops (Martin 1966, 1141; Hirsch 1966, 2; Secretary of Labor 1966, 16–18). The exception is citrus, where relative production fell after 1962. Certainly *bracero* exclusion caused limited changes in production techniques for these six crops as well, even if cultivation processes could not be mechanized. For example, *bracero* exclusion caused lettuce growers to adopt less-selective cutting methods and more mechanized techniques for packing the harvest into shipping-boxes (DOL 1966, 17). Such adjustments appear insufficient to avoid production declines in exposed states relative to unexposed states. Collectively, these results provide empirical support for technical and production adjustment as important mechanisms for the lack of labor-market effects as predicted by equation (9), though they do not rule out other explanations.

6 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 after *bracero* exclusion, leading agricultural economist William E. Martin uncharitably assessed the advocates of exclusion in a little-read book chapter. Those who had believed exclusion would help domestic farm workers “were obviously...extremely naïve”, he wrote, and the hoped-for effects in the labor market never arrived because “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). Exclusion advocates had mocked Martin’s view as one that “repeals the law of supply and demand” (Anderson 1961, 361). The present study assembles the data to test Martin’s claim for the first time. All of the data we use were available to contemporaries but were never, to our knowledge, compiled or analyzed.

The theory and evidence we discuss here contradicts a long literature claiming, largely without quantitative evidence, that *bracero* exclusion succeeded as active labor market policy. We find that *bracero* exclusion failed to raise wages or substantially raise employment for domestic workers in the sector. The theory of endogenous technical change suggests a mechanism for this null result: employers adjusted to foreign-worker exclusion by changing production techniques where that was possible, and changing production levels where it was not, with little change to the terms on which they demanded domestic labor.

The theory is supported by the relationship between novel archival data on state-level *bracero* stocks and data on domestic wages and employment, technological change in agriculture, and crop-specific production adjustment. We can reject the wage semielasticity to workforce reduction that is implied by a model without endogenous technical change, and we corroborate this with direct evidence of technical change causally linked to exclusion. This is the first evidence on endogenous technical change by labor scarcity arising from a natural policy experiment to deliberately reduce the labor supply. Promising paths for future research are to seek other natural experimental settings in which labor scarcity might cause endogenous technical change, as urged by Acemoglu (2010, 1071), and to apply emerging theory and rigorous evaluation methods to other episodes of large changes in real-world immigration restrictions, as urged by Peri (2016, 25).

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Figure 1: The diversification cone $[\phi_\ell, \phi_u]$ and shutdown margin $\bar{\phi}$

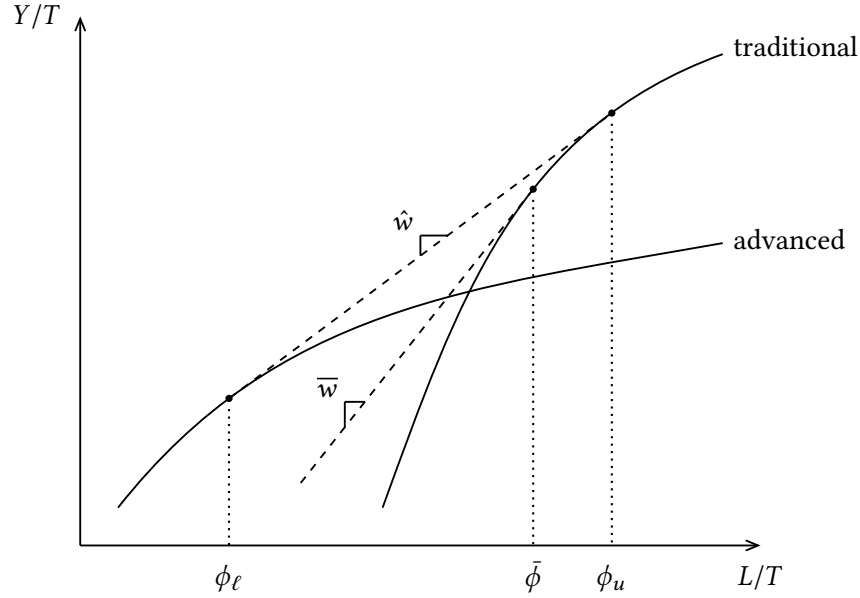
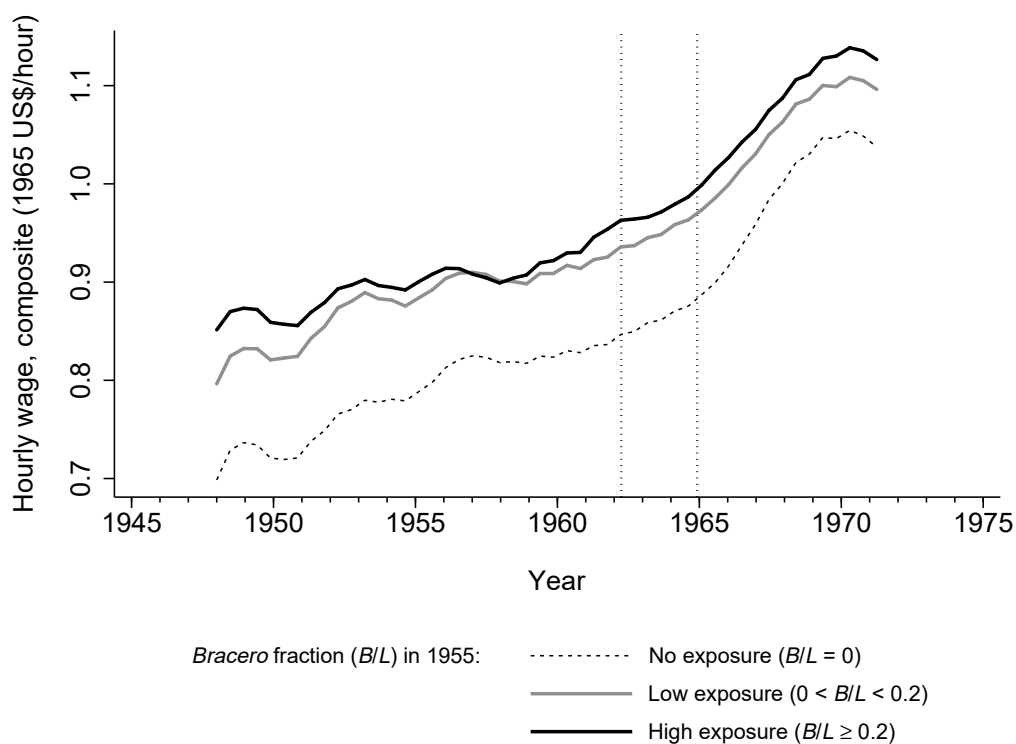


Table 1: Effects of *bracero* exclusion on real wages: Differences-in-differences with continuous treatment, quarterly

<i>Dep. var.</i>	Wage, all years		Wage, 1960–1970	
	Hourly composite	Daily w/o board	Hourly composite	Daily w/o board
$I_{t \geq 1965} \cdot \bar{\ell}_s^{-1955}$	-0.0356 (0.0426)	-0.385 (0.496)	-0.0401 (0.0315)	-0.0247 (0.309)
Fixed effects:				
State	Yes	Yes	Yes	Yes
Quarter-by-year	Yes	Yes	Yes	Yes
Std. err. clustered by	State	State	State	State
<i>N</i>	4324	5714	2024	1892
adj. R^2	0.773	0.840	0.733	0.758
Clusters	46	43	46	43
Semielasticity $\frac{\partial \ln w}{\partial (B/L)}$	-0.0403 (0.0481)	-0.0464 (0.0599)	-0.0419 (0.0329)	-0.00272 (0.0342)
p -val. of χ^2 test: $\frac{\partial \ln w}{\partial (B/L)} = 0.1$	[0.0036]	[0.0145]	[< 0.001]	[0.0026]

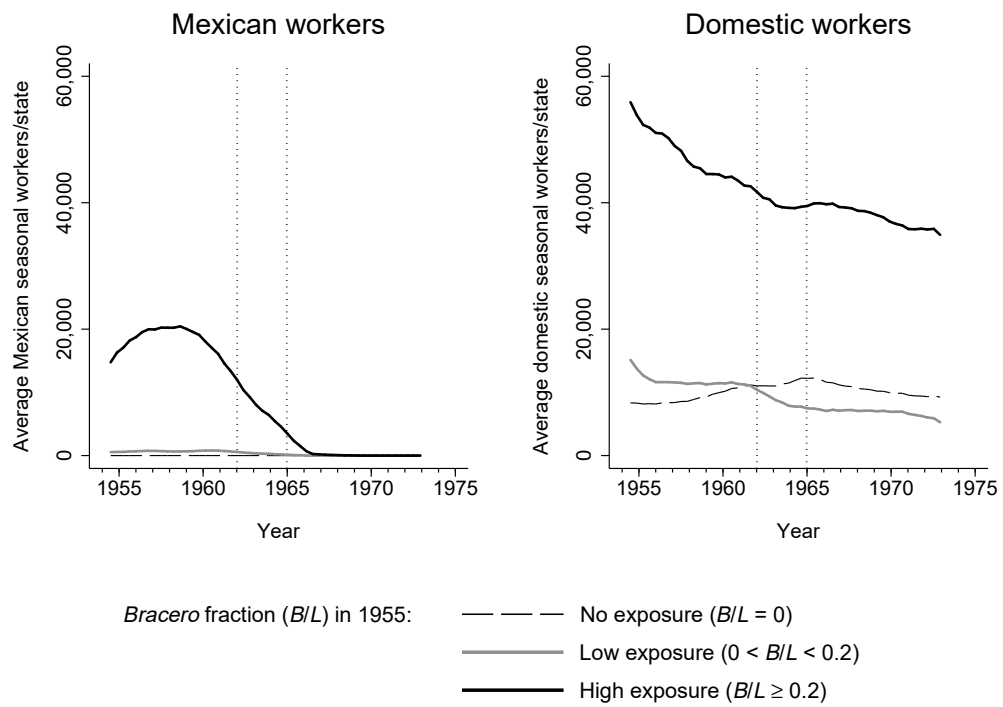
'Treatment' is the degree of exposure to exclusion. Observations are state-quarters. Standard errors clustered by state in parentheses. $\bar{\ell}_{1955}$ is average fraction of Mexicans among the state's total hired seasonal workers across the months of 1955. Wages in constant 1965 US\$ deflated by CPI. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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). Farm worker stocks missing in original sources for 1955 in Rhode Island and New Hampshire.

Figure 2: Quarterly average real farm wages in states grouped by exposure to *bracero* exclusion



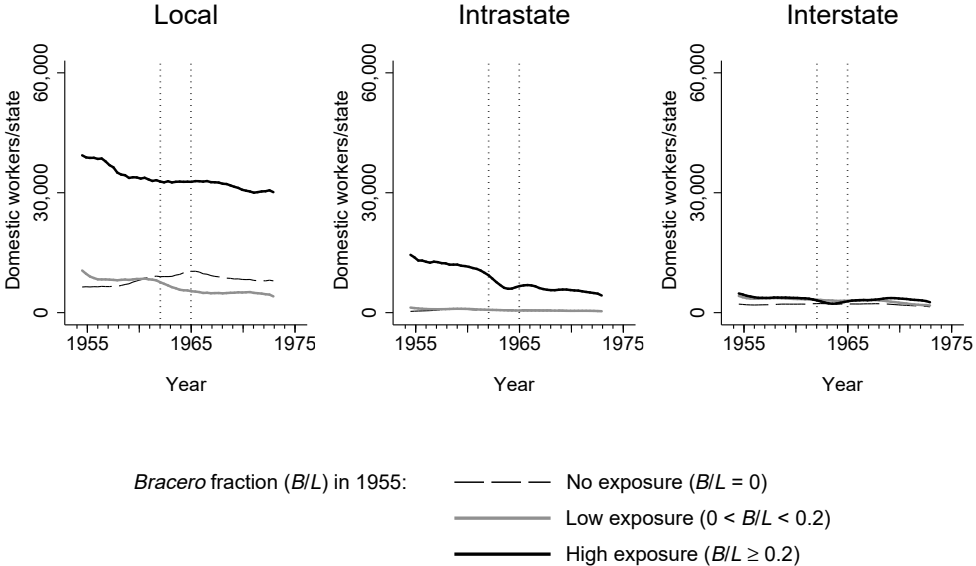
Fan-Gijbels (1992) local linear regressions of quarterly state-average hourly wage on quarter-by-year, Epanechnikov kernel, bandwidth 2 quarters. Real wage adjusted by national Consumer Price Index. Vertical dotted lines show the beginning of government efforts toward *bracero* exclusion (March 1962) 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



Fan-Gijbels (1992) local linear regressions of monthly state-average number of workers employed on month-by-year, Epanechnikov kernel, bandwidth 9 months. 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).

Figure 4: Number of U.S. seasonal farm workers of different types employed, state averages grouped by exposure



Fan-Gijbels (1992) local linear regressions of monthly state-average number of workers employed on month-by-year, Epanechnikov kernel, bandwidth 9 months. 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).

Table 2: Effects of *bracero* exclusion on domestic seasonal agricultural employment: Differences-in-differences with continuous treatment, monthly

<i>Dep. var.:</i>	Domestic seasonal workers, all years		Domestic seasonal workers, 1960–1970	
	<i>Specification:</i>		linear	ln
$I_{t \geq 1965} \cdot \bar{\ell}_s^{-1955}$	-12801.7 (13225.3)	-0.419 (0.553)	-1648.3 (8147.6)	-0.0614 (0.482)
Fixed effects:				
State	Yes	Yes	Yes	Yes
Month-by-year	Yes	Yes	Yes	Yes
Std. err. clustered by	State	State	State	State
<i>N</i>	9476	6369	5520	3707
adj. R^2	0.195	0.352	0.217	0.403
Clusters	46	46	46	46

‘Treatment’ is the degree of exposure to exclusion. Observations are state-months. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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.

Table 3: Effects of *bracero* exclusion on the three types of domestic seasonal agricultural employment: Differences-in-differences with continuous treatment, monthly

<i>Specification:</i>	linear			ln		
	Local domestic	Intrastate domestic	Interstate domestic	Local domestic	Intrastate domestic	Interstate domestic
$I_{t \geq 1965} \cdot \bar{\ell}_s^{-1955}$	-6929.2 (7327.0)	-9851.2 (9637.5)	-233.2 (1160.8)	-0.638 (0.780)	-1.140* (0.652)	-0.559 (0.524)
<i>N</i>	9476	6366	6367	6611	4675	5720
adj. R^2	0.166	0.066	0.117	0.297	0.198	0.242
Clusters	46	46	46	46	46	46

‘Treatment’ is the degree of exposure to exclusion. Observations are state-months. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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.

Table 4: Parametric fixed-effects regression of real wage on *bracero* stock, quarterly, under nonzero *bracero* stocks

<i>Specification:</i>	Fixed effects				Fixed effects, AR(1) err.		
	<i>Dep. var.</i>	Real wage (Hourly composite)		Real wage (Daily w/o board)		Real wage (Hourly composite)	Real wage (Daily w/o board)
$\ln(L^{\text{mex}})$		0.00855* (0.00452)	0.00482 (0.00512)	0.0681*** (0.0236)	0.0189 (0.0254)	0.0102** (0.00393)	0.0558*** (0.0184)
$\ln(L - L^{\text{mex}})$			0.0109 (0.0133)		0.0318 (0.0415)	0.0532*** (0.00517)	0.0810*** (0.0307)
Fixed effects:							
State		Yes	Yes	Yes	Yes	Yes	Yes
Quarter-by-year		Yes	Yes	Yes	Yes	Yes	Yes
Std. err. clustered by		State	State	State	State	—	—
Assumed error struc.		—	—	—	—	AR(1)	AR(1)
<i>N</i>		499	429	654	377	400	351
adj. R^2		0.571	0.498	0.492	0.619	0.686	0.903
Clusters		30	29	29	26	—	—

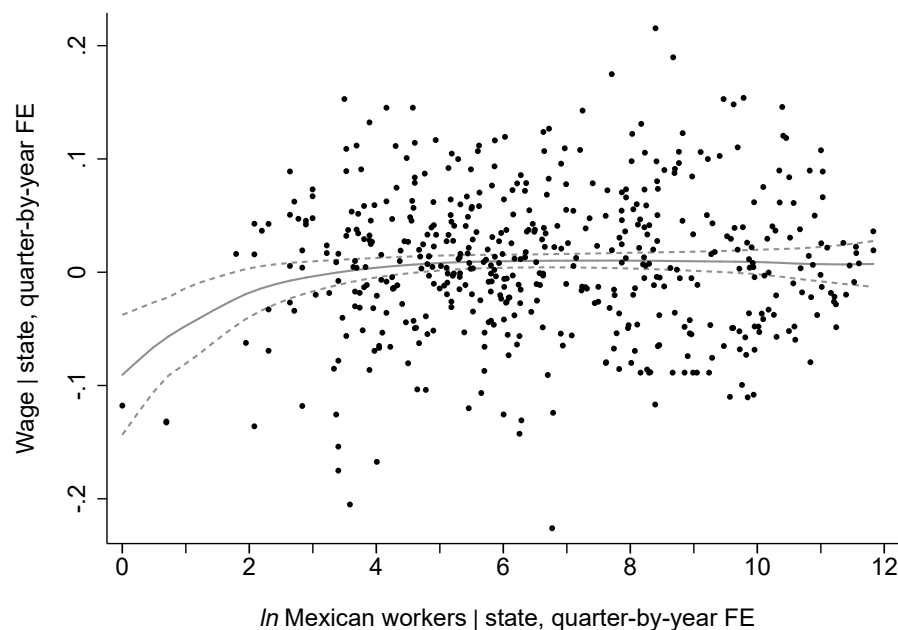
Observations are state-quarters. Standard errors clustered by state in parentheses. L^{mex} is stock of Mexican hired seasonal agricultural workers at the beginning of each quarter, by state; L is total stock of hired seasonal agricultural workers of any nationality (domestic and foreign), by state. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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).

Table 5: Parametric fixed-effects regression of domestic farm employment on *bracero* stock, by state-month

<i>Dep. var.</i>	In Employment of domestic seasonal farm workers			
	Total domestic	Local domestic	Intrastate domestic	Interstate domestic
$\ln(L^{\text{mex}})$	0.242*** (0.0380)	0.195*** (0.0398)	0.238*** (0.0521)	0.401*** (0.0512)
Fixed effects:				
State	Yes	Yes	Yes	Yes
Month-by-year	Yes	Yes	Yes	Yes
Std. err. clustered by	State	State	State	State
<i>N</i>	1377	1358	1185	1263
Clusters	31	31	29	31

Observations are state-months. Standard errors clustered by state in parentheses. L^{mex} is stock of Mexican hired seasonal agricultural workers in each month, by state. “Total domestic” is the sum of “local domestic”, “intrastate domestic”, and “interstate domestic”. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 5: Baltagi-Li semiparametric fixed-effects regression of real hourly wage on *bracero* stock, quarterly, under nonzero *bracero* stocks (1942–1966)



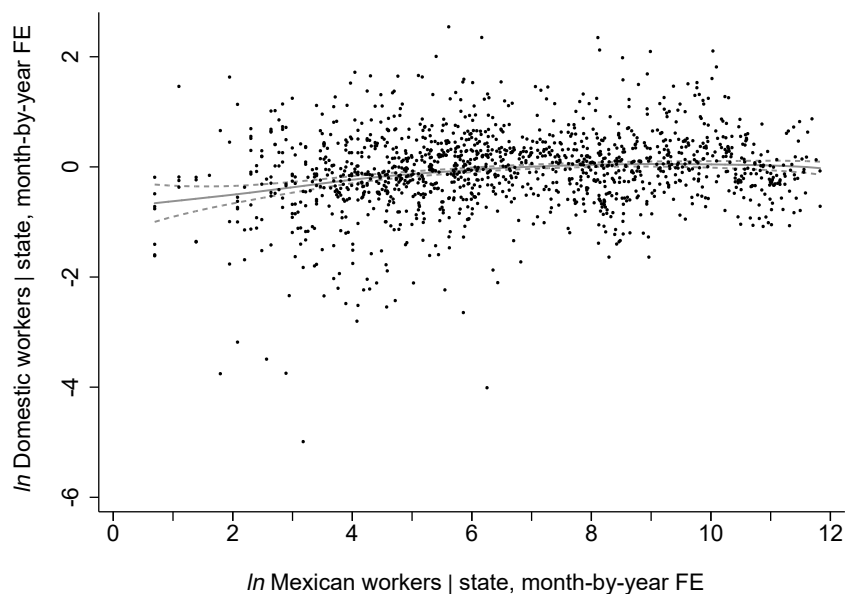
Baltagi and Li (2002) regression of quarterly state-average wage on \ln *bracero* stock, with state and quarter-by-year fixed effects, local linear with Epanechnikov kernel, bandwidth 2 log-points. Dashed lines show 95% confidence interval. Real wage is hourly wage deflated to 1965 US\$ by Consumer Price Index.

Table 6: U.S. employment of Mexican hired seasonal labor by crop, 1964

Crop	Thousand man-months			% of all foreign labor
	Total labor	Foreign labor	Foreign/total, %	
Tomatoes	345.1	90.5	26.2	14.3
Citrus	319.8	69.1	21.6	10.9
Lettuce	122.5	67.8	55.3	10.7
Cotton	1769.4	65.2	3.7	10.3
Strawberries	308.5	42.5	13.8	6.7
Sugar beets	160.6	31.9	19.9	5.0
Cucumbers	105.5	28.9	27.4	4.6
Melons	64.7	18.4	28.4	2.9
Celery	44.4	14.4	32.4	2.3
Asparagus	60.5	11.5	19.0	1.8

From Hirsch (1966, 6). For these crops the ‘foreign labor’ employed was almost entirely Mexican labor. Two other crops had a comparable intensity of foreign labor—tobacco and sugarcane harvesting—but these employed primarily non-Mexican hired seasonal workers.

Figure 6: Baltagi-Li semiparametric fixed-effects regression of domestic seasonal farm employment on *bracero* stock, by state-month



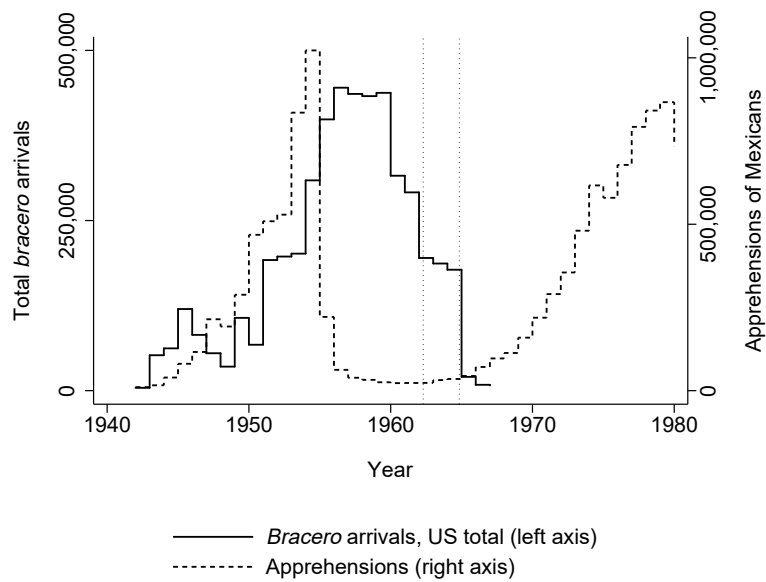
Baltagi and Li (2002) regression of monthly stock of ln employed domestic seasonal farm workers on ln *bracero* stock, with state and month-by-year fixed effects, local linear with Epanechnikov kernel, bandwidth 2 log-points. Dashed lines show 95% confidence interval.

Table 7: Cotton and sugar beet mechanization, annual

Specification:	Diffs-in-diffs		Fixed effects			
	Cotton mech.	Sugar beet mech.	Cotton mechanization		Sugar beet mechanization	
$I_{t \geq 1965} \cdot \bar{\ell}_s^{-1955}$	0.0120*** (0.00258)	0.0136*** (0.00384)				
$\ln(L^{\text{mex}})$			-0.00118*** (0.000300)	-0.000927*** (0.000226)	-0.000764*** (0.000176)	-0.000658*** (0.000141)
$\ln(L - L^{\text{mex}})$				-0.00176 (0.00114)		-0.00127** (0.000468)
Years	1951–1972	1960–1965	1951–1967	1951–1967	1960–1965	1960–1965
N	344	48	97	91	32	32
adj. R^2	0.105	0.129	0.232	0.288	0.253	0.322
Clusters	16	12	9	9	11	11

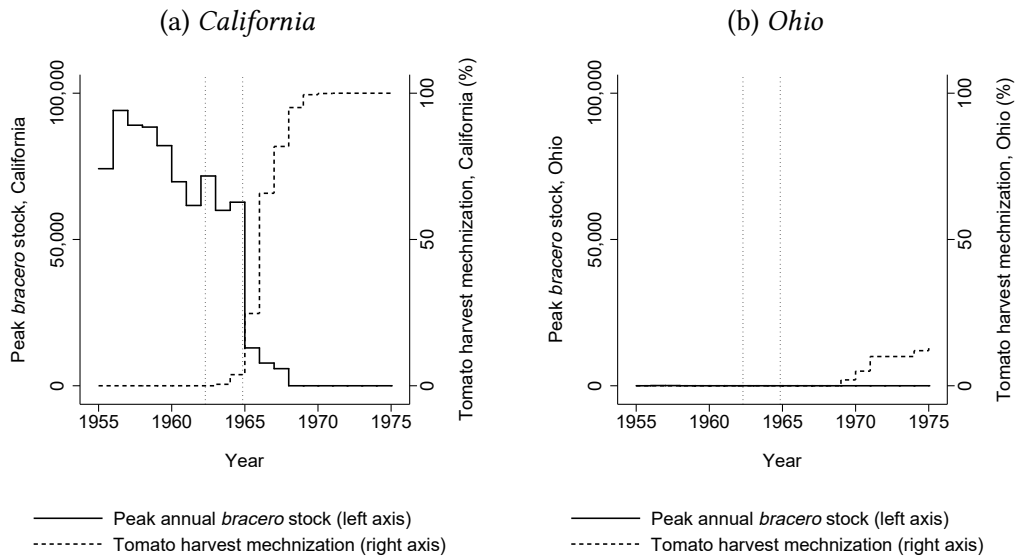
Observations are state-years. For cotton, mechanization fraction is the fraction of production harvested by machine (USDA 1974, 218). For sugar beets, mechanization fraction is fraction of production thinned and/or weeded by machine (Rogers and Cohen 1963, 22, Table 3). Diffs-in-diffs regressions include state-years with zero *braceros*; fixed-effects regressions use logged regressor and thus omit state-years with zero *braceros*. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 7: Annual *bracero* flows compared to apprehensions of Mexicans



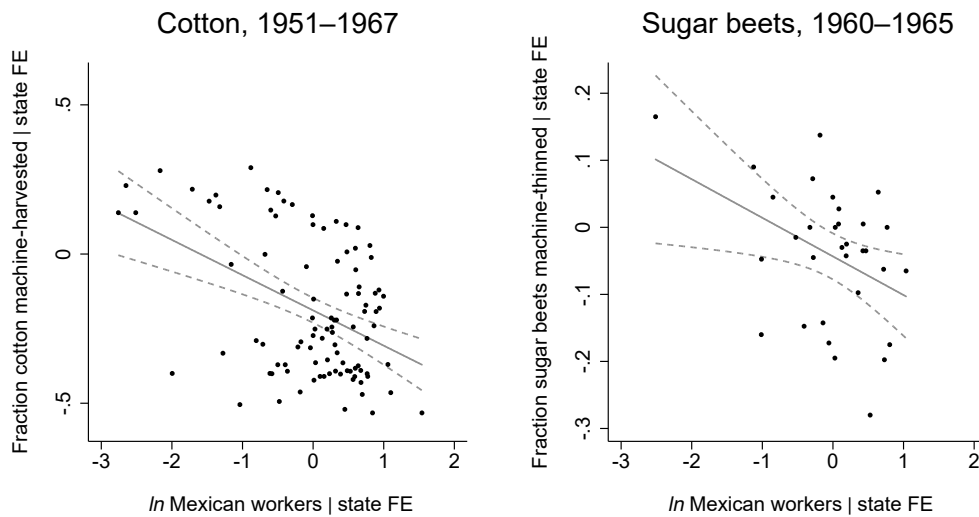
Mexican nationals only in both series. Total *bracero* arrivals per year from [Gastelúm \(1991, 54, Cuadro 3\)](#). Apprehensions of unauthorized Mexican nationals (also “Mexican deportable aliens located”) from *Statistical Yearbook of the Immigration and Naturalization Service* and *Yearbook of Immigration Statistics*, various editions 1960–2013. Vertical dotted lines show start of major efforts toward exclusion (March 1962) and near-complete exclusion (December 1964).

Figure 8: Peak annual *bracero* stock and mechanization of the tomato harvest, in the two states with mechanization time series



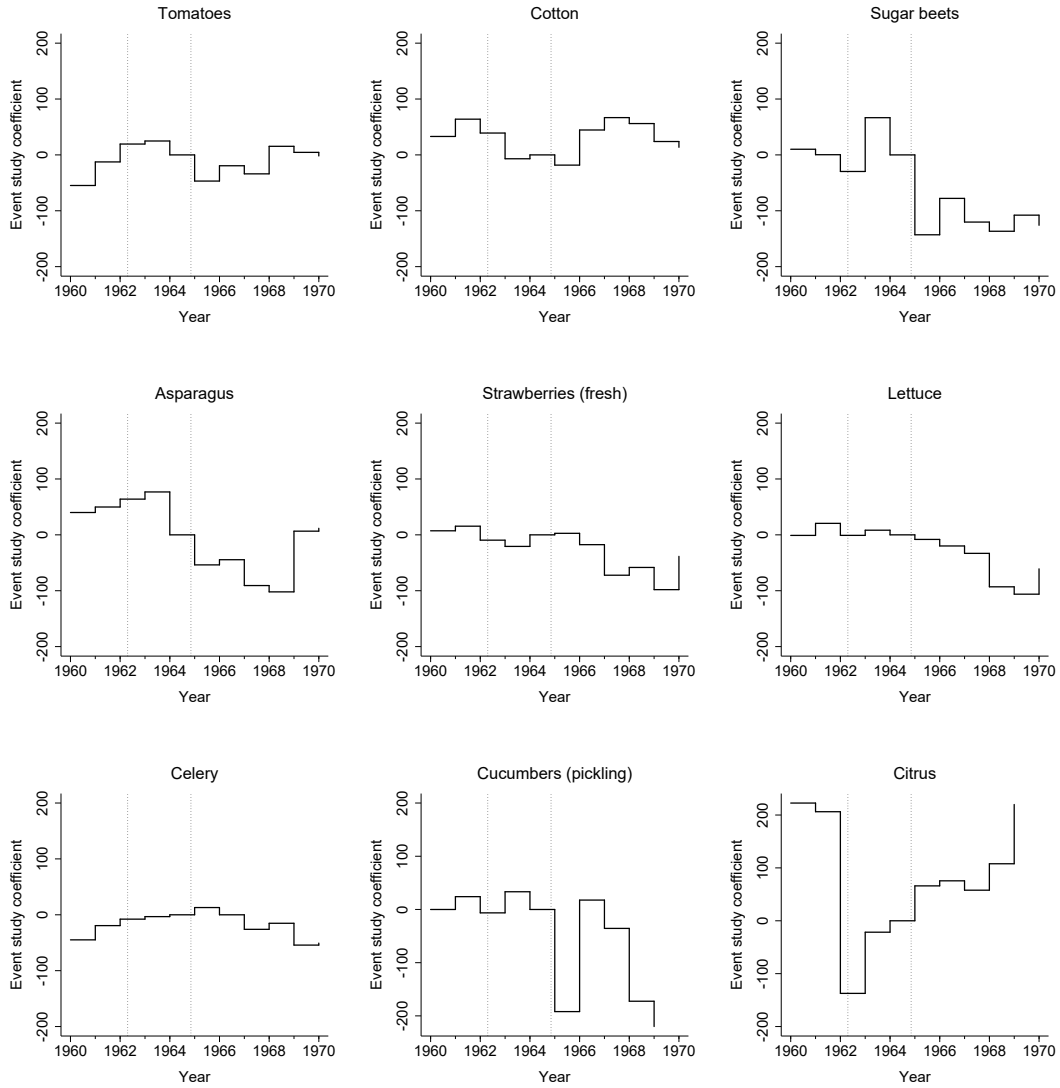
Left axis shows the total number of *braceros* working in state in the peak month of each year (almost always October). 'Mechanization' means that tomatoes were harvested with the Blackwelder tomato harvester, reported by Vandermeer (1986). Vertical dotted lines show the beginning of major government efforts toward *bracero* exclusion (March 1962) and near-complete exclusion at the termination of the program (December 1964). There were 74 and 64 *braceros* in Ohio in the peak month 1956 and 1957, respectively, zero in all other years.

Figure 9: Mechanization versus *bracero* stock, during program



Observations are state-years. For cotton, mechanization fraction is the fraction of production harvested by machine. For sugar beets, mechanization fraction is fraction of production thinned and/or weeded by machine.

Figure 10: Event study regression coefficients: crop physical production index



Observations are state-years, and vertical axis shows event-study regression coefficients from equation (15). For each crop, the dependent variable is a production index normalized so that each state's physical production of the crop in 1964 is 100. 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). The graphs for cucumbers and citrus are truncated at ± 200 in order to preserve the same vertical range across all graphs for comparability.

Online Appendix

“Labor Market Effects of Immigration Restrictions: Evidence from the Mexican *Bracero* Exclusion”

A1 Data sources

A1.1 State-level stocks of Mexican and non-Mexican farm workers

This section describes our construction of a novel database of monthly stocks of seasonal agricultural workers in the 48 continental United States, 1943–1973. It offers full monthly coverage of all 48 states from April 1953 to July 1973, with three minor gaps (July 1953, October 1953, and January–March 1970). This is the period that is relevant for the analysis in the present paper. The database also covers the earlier period of 1943–1953, but with some important gaps: state coverage is complete from June 1943 to December 1947 (except April 1944), but for the period January 1948 to March 1953, only four states have complete coverage, while another 11 states have partial coverage. Details follow below.

April 1953–June 1973, the relevant period for the present paper: Agricultural worker data from April 1953 to July 1973 was compiled as follows. Data for each month is typically reported in the month following (e.g. April 1953 worker statistics reported in May 1953 publication issue). The dates in this section refer to the month of employment, not the month of publication. Farm employment data are typically published in a clearly labeled appendix to each issue; the title of the relevant table varies slightly but is typically “Estimated employment and origin of seasonally hired workers in agriculture and food processing by State and selected agricultural reporting areas.” Though data are reported at a sub-state level for some locations, we collect aggregate state information only. These reports were published seasonally, typically excluding winter months when minimal farm work occurred.³⁶ Data for July 1953, October 1953, and January–March 1970 were missing from all locations.

- April to September 1953: *Farm Labor Market Developments* (Library of Congress).
- November 1953: *Farm Labor Market Developments Employment and Wage Supplement* (National Agricultural Library).
- May 1954 to May 1957: *Farm Labor Market Developments Employment and Wage Supplement* (Library of Congress).
- June 1957: *Farm Labor Market Developments Employment and Wage Supplement* (National Agricultural Library).
- July 1957: *Farm Labor Market Developments Employment and Wage Supplement* (Library of Congress).
- August 1957: *Farm Labor Market Developments Employment and Wage Supplement* (National Agricultural Library).
- September 1957 to November 1957: *Farm Labor Market Developments Employment and Wage Supplement* (Library of Congress).
- May 1958: *Farm Labor Market Developments Employment and Wage Supplement* (National Agricultural Library).
- June 1958: *Farm Labor Market Developments Employment and Wage Supplement* (Library of Congress).

³⁶Omitted months are December to April for 1954, 1958, 1961, and 1962; December to March for 1955, 1956, and 1957; January to May for 1959; January to April for 1960; November and December for 1967.

- July 1958: *Farm Labor Market Developments Employment and Wage Supplement* (National Agricultural Library).
- August 1958 to December 1959: *Farm Labor Market Developments Employment and Wage Supplement* (Library of Congress).
- May 1960 to May 1962: *Farm Labor Market Developments Employment and Wage Supplement* (Wirtz Labor Library).
- June 1962 to November 1962: *Farm Labor Market Developments Employment and Wage Supplement* (Library of Congress).
- December 1962 to November 1963: *Farm Labor Market Developments Employment and Wage Supplement* (Wirtz Labor Library).
- December 1963 to November 1964: *Farm Labor Market Developments Employment and Wage Supplement* (National Agricultural Library).
- December 1964 to October 1967: *Farm Labor Developments Employment and Wage Supplement* (National Agricultural Library).
- January 1968 to July 1970: *Farm Labor Developments* (National Agricultural Library).
- August 1970 to July 1973: *Rural Manpower Developments* (National Agricultural Library).

June 1943–December 1947: Hired seasonal farm worker statistics by state for June 1943 to December 1947 are reported in the United States Department of Agriculture National Agricultural Statistics Service’s monthly *Farm Labor* publication, made available [online](#) through Cornell University’s Mann Library. Each issue features a table of “foreign workers employed in or available for agricultural work by country of origin and state of employment.” The date of reference for these tables varies; for those dated at the beginning of a month, we assume the workers reported were present in the month prior. For example, figures for workers present in April 1944 were taken from the May 1944 issue of *Farm Labor*. The publication reports all major groups of foreign workers present: typically Mexican, Bahamian, and Jamaican. The April 1944 issue reports only Bahamian and total numbers; there is no separate category for Mexican workers. Data for April 1945 is missing.

January 1948–March 1953: To our knowledge there is no primary- or secondary-source national compilation of state-level hired seasonal farm worker data for January 1948 to March 1953. We initially sought such a compilation in six archival locations.³⁷ U.S. National Archives staff supported the hypothesis that due to bureaucratic reorganization during this time period—responsibility for this data collection shifted from the Department of Agriculture to the Department of Labor—no such collation either existed or was made public. Since the ES-223 form was implemented by local employment service offices, we found it plausible that if any such data existed, it might be on a state level. We were able to track down at least partial data for fifteen states for this time period from twelve university libraries and archival locations. We were able to fill the gap entirely for four states: Arizona, California, Michigan, and Virginia. Data for individual states was sourced as follows (we exclude other available publication volumes that did not provide sufficiently granular information for inclusion in the database):

- *Arizona (1948–1953)*: 1948: Post-Season Farm Labor Report for 1948, table on p. 7 (Arizona State University); 1949: Post-Season Farm Labor Report for 1949, domestic numbers from table on p. 14 and bracero numbers from text on p. 20 (Arizona State University); 1950–1953: *Agricultural Employment in Arizona 1950–1964*, table on p. 10 (Wirtz Labor Library).

³⁷U.S. National Archives (College Park, MD), U.S. Department of Agriculture National Agricultural Library (Beltsville, MD), U.S. Department of Labor Wirtz Labor Library (Washington, DC), Library of Congress (Washington, DC), Harry S. Truman Presidential Library (Independence, MO), Dwight D. Eisenhower Presidential Library (Abilene, KS).

- *Arkansas (1951–1953)*: 1951: Arkansas Agricultural Activities 1951, table on p. 49, data from first semi-monthly period; 1952: Arkansas Agricultural Activities 1952, table on p. 45, data from first semi-monthly period; 1953: Arkansas Annual Agricultural Report 1953, table on p. 13 qualified by text on pp. 5–6 (all from University of Arkansas).
- *California (1948–1953)*: 1948, 1949, 1950: Recruitment and Placement of Farm Laborers in California 1950 (Special and Partial Report of the Joint Legislative Committee on Agriculture and Livestock Problems, California Senate), table on p. 22 (Wirtz Labor Library); 1951 & 1952: California Annual Farm Labor Report 1952, table on p. 33 (University of Colorado Boulder); 1953: California Annual Farm Labor Report 1954, table on p. 7 (Wirtz Labor Library).
- *Colorado (1949, 1950, 1953)*: 1949: Colorado Post Season Farm Report 1949, text on p. 13; 1950: Colorado Post Season Farm Report 1950, text on p. 14; 1953: Colorado Post Season Farm Report 1953, text on p. 16 (all from Colorado State Library).
- *Indiana (1950, 1952, 1953)*: 1950: Indiana Post Season Farm Labor Report 1950, table on p. 47; 1952: Indiana Farm Labor Report 1952, table entitled “Out of State Workers Employed in Seasonal Agricultural and Food Processing Activities,” n.p. 1953: Indiana Farm Labor Report 1953, table entitled “Out of State Workers Employed in Seasonal Agricultural and Food Processing Activities,” n.p. (all from Wirtz Labor Library).
- *Louisiana (1952, 1953)*: 1952: Louisiana Post-Season Agricultural and Food Processing Report 1952, text on p. 10; 1953: Louisiana Annual Farm Labor Report 1953, table on p. 19 qualified by text on p. 20 (all from Wirtz Labor Library).
- *Maryland (1948, 1949, 1953)*: 1948 & 1949: The Maryland Farm Labor Program 1948–1949, text on page beginning “A careful appraisal of each crew leader ...”, n.p. (Wirtz Labor Library); 1953: Maryland’s Farm Labor Report 1953, text pp. 14–15 (University of Maryland College Park).
- *Michigan (1948–1953)*: 1948: Post Season Farm Labor Report State of Michigan 1948, table p. 18 (University of Michigan HathiTrust); 1949: Characteristics of Migratory Farm Labor in Michigan, table entitled “Agricultural and Food Processing Industries Seasonal Labor Force for 1949”, n.p. (Truman Presidential Library, President’s Committee on Migratory Labor (Record Group 220), Box 8, Folder “Hearing statements, Saginaw, Michigan, September 11–12, 1950”); 1950: Characteristics of Migratory Farm Labor in Michigan, table entitled “Agricultural and Food Processing Industries Seasonal Labor Force for 1950”, n.p. (Truman Presidential Library, President’s Committee on Migratory Labor (Record Group 220), Box 8, Folder “Hearing statements, Saginaw, Michigan, September 11–12, 1950”); 1951: Post Season Farm Labor Report State of Michigan 1951, table p. 13 (Wirtz Labor Library); 1952: Post Season Farm Labor Report State of Michigan 1952, table p. 12 (Wirtz Labor Library); 1953: Post Season Farm Labor Report 1953, table p. 33 (Wirtz Labor Library).
- *Minnesota (1950–1953)*: 1950: Post Season Agricultural and Food Processing Report 1950, summed by month from table entitled “Seasonal Agricultural Workers Hired for Principal Minnesota Farm Crops — 1950”, n.p. ; 1951: Post Season Agricultural and Food Processing Report 1951, summed by month from table p. 14; 1952: Post Season Agricultural and Food Processing Report 1952, summed by month from table p. 14; 1953: Post Season Agricultural and Food Processing Report 1953, summed by month from table entitled “Seasonal Labor Used for Principal Farm Crops — 1953”, n.p. (all from Minnesota Historical Society).
- *New Jersey (1949)*: Post-Season Agricultural and Food Processing Report for State of New Jersey 1949, text from p. 10 (Wirtz Labor Library).
- *New Mexico (1948, 1953)*: 1948: Annual Report Farm Placement in New Mexico 1949, text from p. 12; 1953: Annual Report Farm Placement in New Mexico 1955, text from p. 6 in comparison with “Exhibit C: State Summary — Employment,” n.p. (both from University of New Mexico).

- *New York (1950)*: 1950 Annual Report Farm and Food Processing Labor, text from p. 9 (Wirtz Labor Library).
- *Oregon (1950, 1952, 1953)*: 1950: Oregon's Farm Labor Market, table entitled "Number of Hired Seasonal Agricultural Workers by Type of Worker, 1950," n.p. (Truman Presidential Library, President's Committee on Migratory Labor (Record Group 220), Box 8, Folder "Hearing statements, Portland, Oregon, October 16–18, 1950"); 1952: Post-Season Farm Labor Report 1952, Table C ("Number of Hired Seasonal Workers in Agriculture by Local Office and Type of Worker — 1952"), qualified by text on page beginning "carried on through the Clearance Program ..." n.p. (Wirtz Labor Library); 1953: Post-Season Farm Labor Report 1953, Table 5 ("Number of Hired Seasonal Workers in Agriculture by Agricultural Area, Local Office, and Type of Worker — 1953"), qualified by text on page beginning "it was estimated early in the season ..." n.p. (Wirtz Labor Library).
- *Virginia (1948–1953)*: 1948: Post-Season Farm Labor Report for 1948 State of Virginia, table on p. 12 qualified by text on p. 4 (Truman Presidential Library, President's Committee on Migratory Labor (Record Group 220), Box 8, Folder "Hearing statements, Washington, D.C., October 2–3, 1950"); 1949: Farm and Processing Labor Virginia 1949, text on p. 24 (University of Virginia); 1950: Farm and Food Processing Worker Placement Virginia 1950, text on p. 22 (Wirtz Labor Library); 1951: Farm Employment in Virginia 1951, table on p. 51 qualified by text on p. 45 (Wirtz Labor Library); 1952: Farm Employment in Virginia 1952, table on p. 44 qualified by text on p. 39 (Wirtz Labor Library); 1953: Farm Employment in Virginia 1953, table on p. 32 qualified by text on pp. 23–24 (Wirtz Labor Library).
- *Washington (1951, 1953)*: 1951: Annual Farm Placement Report 1951, Attachment 4A ("State of Washington Estimated Employment of Hired Seasonal Workers in Agriculture in 1951"), data from first semi-monthly period, n.p. ; 1953: Annual Farm Placement Report 1953, text from p. 11 (both from Wirtz Labor Library).

Measures of Mexican contract workers, local (domestic, nonmigratory) workers, intrastate migrants, and interstate migrants were consistent across the full time period. Information on other foreign workers was inconsistently collated in different publications. Jamaican and Bahamian workers were identified as such from 1943–1947; combined under the umbrella "British West Indies" from 1946 to 1954 and 1959 to 1973; and aggregated as "British West Indies and others" from 1953–1958. A similar trend holds for Canadians, identified as a single group from 1943 to 1954 and 1966 to 1972 and aggregated into "Canadians and others" from 1959–1964. "Other foreign" workers are reported from 1944–1954 and 1964–1966. Data on Puerto Rican workers are reported for the full time period.

A1.2 Wages

All farm wage data were sourced from the Department of Agriculture publication *Farm Labor* on a quarterly basis. The daily wage without board measure was consistently reported for the relevant quarter: for example, July 1945 rates reported in the July 1945 publication issue. Hourly composite wage rates are available beginning in January 1948. This composite index was computed by "converting the monthly, weekly, and daily rates to an hourly basis and weighting the rates by approximate distributions of workers hired by the different arrangement." (USDA 1969, 16). The measure was rebased with new weights from a 1948 agriculture survey in the January 1951 edition of *Farm Labor*; we source data for January 1948 to October 1950 from this issue. Beginning in January 1951, hourly composite rates are reported alongside the daily without board rate in the relevant quarterly issue. 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). In order to balance the panel, we set the latter measure for these three states to missing for all years.

A1.3 Data availability, entry, and reconciliation

We began the search for missing worker data from 1948 to 1953 at the National Archives in College Park, Maryland. Data prior to 1948 was collected by the Bureau of Agricultural Economics in the Department of Agriculture; these records fall under Archives Record Group 83. Upon the abolition of this office in 1953, the Office of Employment Security of the Department of Labor assumed responsibility for collating and publishing data on farm employment in 1954; these records fall under Record Group 183 ([U.S. National Archives n.d.](#)). Upon review of the relevant files for the years in question and consultation with archivists, it became clear that most of the National Archives content was correspondence and testimony. It was suggested that any historical statistical information would likely be stored within the Federal Depository Library system, charged with collecting and storing official government publications. Under the Superintendent of Documents Classification (SuDocs) system of the United States Government Publishing Office (GPO), the relevant files (Bureau of Employment Security) are held under SuDocs stem L7. Thanks to the help of Celina Nichols McDonald at the University of Maryland, we sent a request for this catalog entry to all US Federal Depository Libraries; none held the information we were looking for. With the help of Julie Day from the Department of Labor Wirtz Labor Library, we ascertained that Washington University in St. Louis, Missouri and the Dwight D. Eisenhower Presidential Library in Abilene, Kansas, possessed SuDocs L7 holdings. The former did not seem to include granular information on foreign workers; the latter not only held information under this call number, but also the full record of the President's Commission on Migratory Labor and supporting information, from 1938 to 1966.³⁸

We therefore visited the Eisenhower Library in December 2016, pulling all archival material potentially related to bracero employment from 1948–1953. From record group 220 (U.S. President's Committee on Migratory Labor) this included boxes 1–4, 11–14, 62–76, 87–97, and 99. From the broader Migratory Labor subject guide, this included boxes 102, 139, 141, and 178 of the Mitchell papers; box 4 of the Eisenhower papers (Ann Whitman file), box 17 of the Oveta Culp Hobby papers, box 13 of the Clyde Wheeler papers, and boxes 10 and 20 of the White House Cabinet Secretariat records. We also visited the Harry S. Truman Presidential Library in Independence, Missouri for their nine boxes of material on the U.S. President's Committee on Migratory Labor.³⁹ These searches yielded important yet sporadic worker data by state to fill the national level gap from 1948 to 1953, as well as qualitative background material and methodological information.

We also reviewed all potentially relevant congressional hearings for this period stored at the Library of Congress. Though some offered relevant information, most data reported was for hires/contracts or border crossings over a certain time period, not the monthly stock estimates of farm employment we sought. This extensive archival work in combination with a 1978 [records disposal request](#) authorizing the disposal of original ES-223 forms and related reports, gives us confidence that our dataset is complete to the fullest extent possible.

We outsourced the entry of both worker and wage data from scanned PDFs to Excel to two separate data entry professionals on [Upwork](#), an online network for freelancers. These two individuals were selected through a rigorous sample data entry process; any discrepancies between the final dual entry datasets were hand-checked and harmonized in Stata to yield the final database.

A1.4 U.S. state Mexican-born and total populations

Census-year estimates of Mexican-born population by U.S. state are calculated directly from census 1% sample microdata in 1920, 1930, 1940, 1950, and 1960 from IPUMS-USA (Steven Ruggles, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. [Integrated Public Use Microdata Series: Version 6.0](#) [Machine-readable database]. Minneapolis: University of Minnesota, 2015). Annual estimates of total U.S.

³⁸Full migratory labor subject guide available [online](#).

³⁹Box list available [online](#).

state populations 1920–1960 come from the U.S. Bureau of the Census, https://www.census.gov/popest/data/state/asrh/1980s/80s_st_totals.html, accessed August 24, 2016.

A2 Model derivation

Portions of the model in the main text are derived as follows.

A2.1 Equilibrium output under traditional technology

The expression for Y_0 at equilibrium is derived by noting that

$$\begin{aligned} r_0 &= \left\{ K_0^{\frac{\mu-1}{\mu}} + \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}} \right\}^{\frac{1}{\mu-1}} K_0^{-\frac{1}{\mu}} \\ \Leftrightarrow r_0^{\mu-1} &= 1 + K_0^{\frac{1-\mu}{\mu}} \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}} \\ \Leftrightarrow K_0 &= \left(r_0^{\mu-1} - 1 \right)^{\frac{\mu}{1-\mu}} \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}, \end{aligned}$$

and substituting into the production function we have that

$$Y_0 = \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}.$$

A2.2 Change in wage with and without capital adjustment

Expression (7) uses the fact that $\frac{\partial \ln w}{\partial \ln(L/T)} = \left(\frac{\mu}{\mu-1} - 1 \right) (1 - s_K) \frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu} \frac{\sigma-1}{\sigma} \frac{s_L}{1-s_K} + \frac{\mu-\sigma}{(\sigma-1)\mu} \frac{s_L}{1-s_K} \frac{\sigma-1}{\sigma} + \frac{-1}{\sigma}$ (by differentiating (6)) – and expressions for factor shares under the traditional technology, which are:

$$\begin{aligned} s_K &= \frac{r_0 K_0}{Y_0} = \left\{ K_0^{\frac{\mu-1}{\mu}} + \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}} \right\}^{-1} K_0^{\frac{\mu-1}{\mu}} \\ 1 - s_K &= \left\{ K_0^{\frac{\mu-1}{\mu}} + \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}} \right\}^{-1} \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}} \\ s_L &= a \left\{ K_0^{\frac{\mu-1}{\mu}} + \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu}} \right\}^{-1} \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1} \frac{\mu-1}{\mu} - 1} L^{\frac{\sigma-1}{\sigma}} \\ \frac{s_L}{1 - s_K} &= \left[aL^{\frac{\sigma-1}{\sigma}} + (1-a)T^{\frac{\sigma-1}{\sigma}} \right]^{-1} aL^{\frac{\sigma-1}{\sigma}}, \end{aligned}$$

with $s_L + s_T = 1 - s_K$. After capital adjusts,

$$\begin{aligned} w &= \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[a \left(\frac{L}{T} \right)^{\frac{\sigma-1}{\sigma}} + (1-a) \right]^{\frac{1}{\sigma-1}} a \left(\frac{L}{T} \right)^{-\frac{1}{\sigma}}. \\ \text{Thus } \frac{\partial \ln w}{\partial(B/L)} &\approx - \frac{\partial \ln w}{\partial \ln(L/T)} = - \left[\frac{1}{\sigma-1} \frac{\sigma-1}{\sigma} \frac{s_L}{1-s_K} + \left(-\frac{1}{\sigma} \right) \right] = \frac{s_T}{s_L + s_T} \frac{1}{\sigma}. \end{aligned}$$

A2.3 Change in average wages under differentiated skill

Expression (12) uses the change in the average wages of skilled and unskilled workers as follows. Consider the traditional technology modified by the two labor types, S and U . The corresponding wages are

$$w_U = \alpha_L \cdot a \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[a + (1-a) \left(\frac{L}{T} \right)^{-\frac{\sigma-1}{\sigma}} \right]^{\frac{1}{\sigma-1}} \left[\alpha_L U^{\frac{\rho-1}{\rho}} + (1-\alpha_L) S^{\frac{\rho-1}{\rho}} \right]^{\frac{1}{\rho-1}} U^{-\frac{1}{\rho}}$$

$$w_S = (1-\alpha_L) \cdot a \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[a + (1-a) \left(\frac{L}{T} \right)^{-\frac{\sigma-1}{\sigma}} \right]^{\frac{1}{\sigma-1}} \left[\alpha_L U^{\frac{\rho-1}{\rho}} + (1-\alpha_L) S^{\frac{\rho-1}{\rho}} \right]^{\frac{1}{\rho-1}} S^{-\frac{1}{\rho}}$$

$$w_U U + w_S S = a \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[a + (1-a) \left(\frac{L}{T} \right)^{-\frac{\sigma-1}{\sigma}} \right]^{\frac{1}{\sigma-1}} \left[\alpha_L U^{\frac{\rho-1}{\rho}} + (1-\alpha_L) S^{\frac{\rho-1}{\rho}} \right]^{\frac{\rho}{\rho-1}}.$$

And in this setup define $s_L \equiv \frac{w_U U + w_S S}{Y_0}$ and $s_U \equiv \frac{w_U U}{Y_0}$. Then

$$\frac{s_U}{s_L} = \left[\alpha_L U^{\frac{\rho-1}{\rho}} + (1-\alpha_L) S^{\frac{\rho-1}{\rho}} \right]^{-1} \alpha_L U^{\frac{\rho-1}{\rho}}.$$

Note that the expression for $w_U U + w_S S$ can be also be used to show that the expression for average wages remains as it was before,

$$w = \frac{w_U U + w_S S}{L} = a \left(\frac{r_0^{\mu-1}}{r_0^{\mu-1} - 1} \right)^{\frac{\mu}{\mu-1}} \left[a + (1-a) \left(\frac{L}{T} \right)^{-\frac{\sigma-1}{\sigma}} \right]^{\frac{1}{\sigma-1}},$$

since $L = \left[\alpha_L U^{\frac{\rho-1}{\rho}} + (1-\alpha_L) S^{\frac{\rho-1}{\rho}} \right]^{\frac{\rho}{\rho-1}}$. This expression for w is equivalent to the expression for w in the preceding subsection.

One can also use the above expressions to show

$$d \ln w_U = d \ln w - \frac{1}{\rho} \left(1 - \frac{s_U}{s_L} \right) d \ln \left(\frac{U}{S} \right),$$

since $w_U = w \cdot \alpha_L \left[\alpha_L U^{\frac{\rho-1}{\rho}} + (1-\alpha_L) S^{\frac{\rho-1}{\rho}} \right]^{\frac{1}{\rho-1}} U^{-\frac{1}{\rho}} = w \cdot \alpha_L \left[\alpha_L + (1-\alpha_L) \left(\frac{U}{S} \right)^{-\frac{\rho-1}{\rho}} \right]^{\frac{1}{\rho-1}}$. Similarly,

$$d \ln w_S = d \ln w + \frac{1}{\rho} \frac{s_U}{s_L} d \ln \left(\frac{U}{S} \right),$$

since $w_S = w \cdot (1-\alpha_L) \left[\alpha_L \left(\frac{U}{S} \right)^{\frac{\rho-1}{\rho}} + (1-\alpha_L) \right]^{\frac{1}{\rho-1}}$.

A3 Descriptive statistics

Table A1 presents descriptive statistics for the variables used in the regressions in the main text.

Appendix Table A1: Descriptive statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
<i>State-by-quarter wage regressions</i>					
Year	1958.128	9.6	1942	1975	6384
Quarter	2.489	1.121	1	4	6384
Wage, hourly (1965 US\$)	0.905	0.222	0.154	1.578	4512
Wage, daily (1965 US\$)	8.556	2.257	2.087	14.983	5978
$\ln(L^{\text{mex}})$	6.343	2.436	0	11.828	783
$\ln(L - L^{\text{mex}})$	9.118	1.44	2.89	12.431	1759
$I_{t \geq 1965} \cdot \bar{\ell}_s^{1955}$	0.016	0.068	0	0.609	6118
<i>State-by-month employment regressions</i>					
Year	1958.937	9.467	1942	1975	14544
Month	6.505	3.339	1	12	14544
Total domestic	9285.370	24626.204	0	260941	14544
Local domestic	15762.811	27123.697	0	248050	6778
Intrastate domestic	1661.222	5837.364	0	118315	6693
Interstate domestic	2550.377	4417.239	0	46643	6700
\ln Total domestic	8.968	1.456	1.386	12.472	6753
\ln Local domestic	8.661	1.521	1.386	12.421	6719
\ln Intrastate domestic	6.047	1.893	0.693	11.681	4676
\ln Interstate domestic	6.828	1.784	0.693	10.75	5789
$\ln(L^{\text{mex}})$	6.233	2.467	0	11.828	2303
$I_{t \geq 1965} \cdot \bar{\ell}_s^{1955}$	0.018	0.072	0	0.609	13938
<i>State-by-year mechanization regressions</i>					
Year	1963	7.214	1951	1975	1200
Cotton mechanization	0.526	0.375	0	1	344
Sugar beet mechanization	0.414	0.263	0	0.92	52
$\ln(L^{\text{mex}})$	6.048	2.056	2.204	11.218	298
$I_{t \geq 1965} \cdot \bar{\ell}_s^{1955}$	0.022	0.08	0	0.609	1150
<i>State-by-year crop production regressions</i>					
Year	1958.5	9.814	1942	1975	1632
Tomatoes	112.205	52.302	0	433.333	544
Cotton	83.162	32.698	1.471	222.54	301
Sugar beets	93.637	33.953	0	239.965	240
Asparagus	99.406	24.755	0	162.264	144
Strawberries (fresh)	107.856	64.745	10.087	520	434
Lettuce	111.163	61.193	31.586	681.592	276
Celery	131.24	113.755	41.176	986.667	162
Cucumbers (pickling)	131.184	61.445	41.698	425.139	106
Citrus	132.055	83.433	3.819	427.083	64
$\ln(L^{\text{mex}})$	5.955	1.937	1.712	11.218	406
$I_{t \geq 1965} \cdot \bar{\ell}_s^{1955}$	0.016	0.07	0	0.609	1564

A4 Further characterization of worker stocks

A4.1 Effect of exclusion on the Mexican fraction of seasonal farm labor

Figure A1 shows the fraction of seasonal farm labor that is Mexican, averaged across states, within three groups of states. The group with high exposure to exclusion is the six states where *braceros* made up more than 20 percent of all seasonal agricultural labor in 1955: Arkansas, Arizona, California, New Mexico, South Dakota, and Texas. The group with low exposure to exclusion 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 is the states that had zero *braceros* in 1955.

A4.2 Effect of *bracero* exclusion on non-Mexican foreign seasonal farm labor

Figure A2 shows that stocks of non-Mexican foreign seasonal agricultural workers remained very low after *bracero* exclusion.

A4.3 Mexican fraction of hired seasonal workers

Table A2 shows the fraction of total seasonal farm workers made up by Mexicans, averaged across the months of 1955, and the corresponding classification into groups of states with ‘high’, ‘low’, and ‘no’ exposure to *bracero* exclusion.

A4.4 Seasonal variation in *bracero* stocks

Figure A3 shows the distribution of *bracero* stocks on average over the course of a year, for the entire United States. In the 1950s and 1960s, the stock of *bracero* workers over the course of each year occurred in October. In the 1960s, almost all *braceros* present at any point in the year were present in or after the month of May.

A5 Robustness checks

The section checks the robustness of some assumptions underlying results in the main text.

A5.1 Alternative ‘treatment’ year: 1962

Here we present re-analysis of main-text Tables 1 and 2 with the new assumption that ‘treatment’ begins in 1962, when the first major restrictions were placed on farms’ hiring of *braceros*, rather than 1965 when the program was terminated. Table A3 shows the differences-in-differences analysis of wage effects under this new assumption, and Table A4 shows the analysis of employment effects. There are no substantial differences from the original results.

A5.2 Sensitivity of wage graph to smoothing and wage measure

Here we show the smoothed and unsmoothed versions of the wage graph in the main text (Figure 2), the hourly composite wage, in Figure A4a and Figure A4b. We also show the same analysis with a different measure of the wage—the average daily wage without board, which covers more years but omits three states (California, Oregon, and Washington)—with and without smoothing, in Figure A4c and Figure A4d.

A5.3 Alternative assumptions about missing values

In the differences-in-differences regressions in the main text with domestic employment as the dependent variable, we assume that when there were no domestic seasonal agricultural workers reported for a state-month in a month when the source document was issued, there were zero domestic workers of each type

Appendix Table A2: Mexican fraction of total seasonal farm workers, average across months of 1955

High exposure		Low exposure		No exposure	
NM	0.609	NV	0.090	ME	0.000
NE	0.323	AR	0.080	NJ	0.000
AZ	0.269	WY	0.050	SC	0.000
TX	0.241	CO	0.045	LA	0.000
CA	0.231	MI	0.034	KY	0.000
SD	0.212	UT	0.026	DE	0.000
		MT	0.025	KS	0.000
		IN	0.017	ND	0.000
		MO	0.015	OK	0.000
		ID	0.012	CT	0.000
		MN	0.010	NC	0.000
		WI	0.008	AL	0.000
		IL	0.007	MD	0.000
		WA	0.006	WV	0.000
		TN	0.006	PA	0.000
		OR	0.005	OH	0.000
		GA	0.003	VT	0.000
				IA	0.000
				NY	0.000
				VA	0.000
				FL	0.000
				MA	0.000
				MS	0.000
				NH	—
				RI	—

Rhode Island and New Hampshire are missing in all regressions using the 1955 *bracero* fraction. The original sources do not report domestic or foreign farm worker stocks for those two states in 1955 (and most other years).

Appendix Table A3: Alternative ‘treatment’ year 1962: Effects of *bracero* exclusion on real wages: Differences-in-differences with continuous treatment, quarterly

<i>Dep. var.</i>	Wage, all years		Wage, 1960–1970	
	Hourly composite	Daily w/o board	Hourly composite	Daily w/o board
$I_{t \geq 1962} \cdot \bar{\ell}_s^{-1955}$	-0.0167 (0.0367)	-0.309 (0.454)	-0.00626 (0.0248)	0.240 (0.263)
Fixed effects:				
State	Yes	Yes	Yes	Yes
Quarter-by-year	Yes	Yes	Yes	Yes
Std. err. clustered by	State	State	State	State
<i>N</i>	4324	5714	2024	1892
adj. R^2	0.773	0.840	0.732	0.758
Clusters	46	43	46	43
Semielasticity $\frac{\partial \ln w}{\partial (B/L)}$	-0.0189 (0.0414)	-0.0373 (0.0549)	-0.00655 (0.0260)	0.0264 (0.0290)
p -val. of χ^2 test: $\frac{\partial \ln w}{\partial (B/L)} = 0.1$	[0.0041]	[0.0123]	[< 0.001]	[0.0113]

‘Treatment’ is the degree of exposure to exclusion. Observations are state-quarters. 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. Wages in constant 1965 US\$ deflated by CPI. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A4: Alternative ‘treatment’ year 1962: Effects of *bracero* exclusion on domestic seasonal agricultural employment: Differences-in-differences with continuous treatment, monthly

<i>Dep. var.</i>	Domestic seasonal workers, all years		Domestic seasonal workers, 1960–1970	
	linear	ln	linear	ln
$I_{t \geq 1962} \cdot \bar{\ell}_s^{1955}$	-19008.2 (15984.6)	-0.592 (0.460)	-7575.2 (10054.3)	0.0474 (0.391)
Fixed effects:				
State	Yes	Yes	Yes	Yes
Month-by-year	Yes	Yes	Yes	Yes
Std. err. clustered by	State	State	State	State
<i>N</i>	9476	6369	5520	3707
adj. R^2	0.197	0.353	0.217	0.402
Clusters	46	46	46	46

‘Treatment’ is the degree of exposure to exclusion. Observations are state-months. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ 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 for a month when source report was issued, assume zero.

in that state-month. This accords with inspection of the source documents, where missing values for a state-month typically precede or follow very low values for that state in other months, which would be expected if the report authors used nonreporting to represent zero. Here we make the alternative assumption: for months in which the source report was issued, if there is no report of domestic workers stocks for a state-month, that stock is assumed to be missing, not zero. This is done in columns 1 and 3 of [Table A5](#) and the first three columns of [Table A6](#). The results in the main text are qualitatively invariant to this alternative assumption.

Of course, the logarithmic specification of these regressions in the main text takes zero values as missing by construction. To allow for such nonlinearity *without* truncating zeros from the dataset, we repeat the regressions of the main text using the inverse hyperbolic sine of domestic employment as the dependent variable (returning to the assumption that missing values in the original sources, in months were the source document was issued, represent zeros). These results are shown in columns 2 and 4 of [Table A5](#) and the last three columns of [Table A6](#). These results, too, are qualitatively the same as the results in the main text.

A5.4 Compare independent measurements of *bracero* (outward) flows and inward stocks

[Figure A5](#) compares the annual total outflow of *braceros* reported by the government of Mexico (a) with snapshots of the national-total stocks of *braceros* reported in state-by-state data by the U.S. Department of Agriculture in various months (b). The quantities are not strictly comparable: the flow measure captures how many people made the trip at any point in the year, which will exceed the stock present in the U.S. at any given moment to a greater degree the shorter the duration of stay for each worker. But given that

Appendix Table A5: Alternative handling of missing worker stocks: Effects of *bracero* exclusion on domestic seasonal agricultural employment: Differences-in-differences with continuous treatment, monthly

<i>Dep. var.</i>	Domestic seasonal workers, all years		Domestic seasonal workers, 1960–1970		
	<i>Specification:</i>	linear	inverse hyperbolic sine	linear	inverse hyperbolic sine
<i>Missing vals assumed:</i>	missing	zero	missing	zero	
$I_{t \geq 1965} \cdot \bar{\ell}_s^{1955}$	-27669.0 (25400.0)	-0.918 (1.370)	-10753.5 (14505.3)	-0.744 (1.143)	
Fixed effects:					
State	Yes	Yes	Yes	Yes	Yes
Month-by-year	Yes	Yes	Yes	Yes	Yes
Std. err. clustered by	State	State	State	State	State
<i>N</i>	6397	9476	3714	5520	
adj. R^2	0.170	0.603	0.194	0.620	
Clusters	46	46	46	46	

‘Treatment’ is the degree of exposure to exclusion. Observations are state-months. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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 missing in the regressions with linear dependent variable; assume zero in regressions with inverse hyperbolic sine dependent variable.

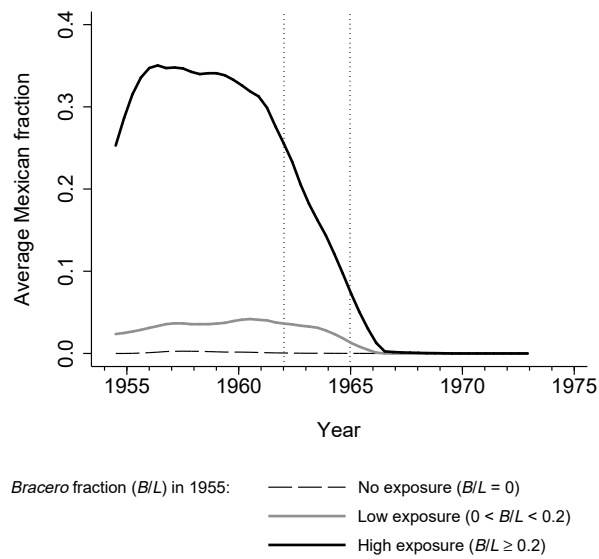
Appendix Table A6: Alternative handling of missing worker stocks: Effects of *bracero* exclusion on the three types of domestic seasonal agricultural employment: Differences-in-differences with continuous treatment, monthly

<i>Specification:</i>	linear			inverse hyperbolic sine		
	<i>Missing vals assumed:</i>					
	missing			zero		
<i>Dep. var.</i>	Local domestic	Intrastate domestic	Interstate domestic	Local domestic	Intrastate domestic	Interstate domestic
$I_{t \geq 1965} \cdot \bar{\ell}_s^{1955}$	-17535.9 (15553.5)	-9851.2 (9637.5)	-233.2 (1160.8)	-0.676 (1.409)	-0.995 (1.061)	-1.630 (1.035)
<i>N</i>	6396	6366	6367	9476	6366	6367
adj. <i>R</i> ²	0.137	0.066	0.117	0.593	0.129	0.180
Clusters	46	46	46	46	46	46

'Treatment' is the degree of exposure to exclusion. Observations are state-months. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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 missing in the regressions with linear dependent variable; assume zero in regressions with inverse hyperbolic sine dependent variable.

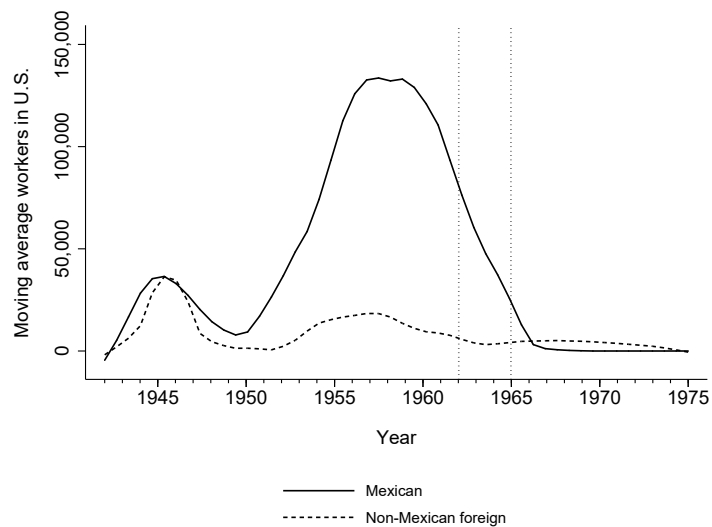
some *braceros* were contracted only for spring work rather than the fall harvest, and some fall workers' contracts for early-harvest crops ended before October, it is plausible that the stock of *braceros* present in October would be roughly 60 percent of the number that had departed for the U.S. at any point in the year—as is the case in the mid- to late-1950s. This comparison suggests that the stock measures collected from farmers by the U.S. Department of Agriculture were not severely undercounting the number of *braceros* present in each state.

Figure A1: Average Mexican fraction of hired seasonal farm workers reported, by exposure group



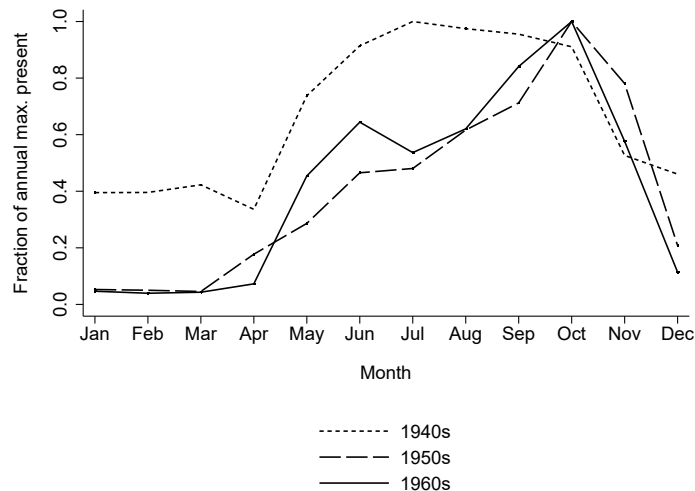
Fan-Gijbels (1992) local linear regressions of monthly state-average Mexican fraction on month-by-year, Epanechnikov kernel, bandwidth 9 months. Vertical dotted lines show the beginning of major government efforts toward *bracero* exclusion (March 1962) and near-complete exclusion at the termination of the program (December 1964).

Figure A2: Mexican versus other foreign seasonal farm workers in the U.S., total



Fan-Gijbels (1992) local linear regressions of monthly national-total workers on month-by-year, Epanechnikov kernel, bandwidth 9 months. 'Non-Mexican foreign' for the purposes of this comparison includes nationals of Jamaica, the Bahamas, other British West Indies, and Canada, as well as U.S. nationals from Puerto Rico. 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).

Figure A3: Fraction of annual maximum monthly stock of *braceros* present in each month, entire U.S.



“1940s” is 1943–1949, “1960s” is 1960–1965. For each decade in each month, we sum the total stock of *bracero* workers present in that month across all the years of the decade in question, then divide that summed stock in each month by the maximum monthly stock. For example, for the 1950s the graph shows for month m the quantity $\left(\sum_{y=1950}^{1959} \sum_{s \in S} b_{s,m,y}\right) / \left(\max_m \sum_{y=1950}^{1959} \sum_{s \in S} b_{s,m,y}\right)$, where $b_{s,m,y}$ is the stock of *braceros* in state s in month m in year y , and S is the set of all states. Thus a value of 1 on the vertical axis means that in that month the sum of all *bracero* stocks in that month across the years of that decade is equal to the maximum such sum for any month.

Figure A4: Quarterly average real farm wages in states grouped by exposure to *bracero* exclusion

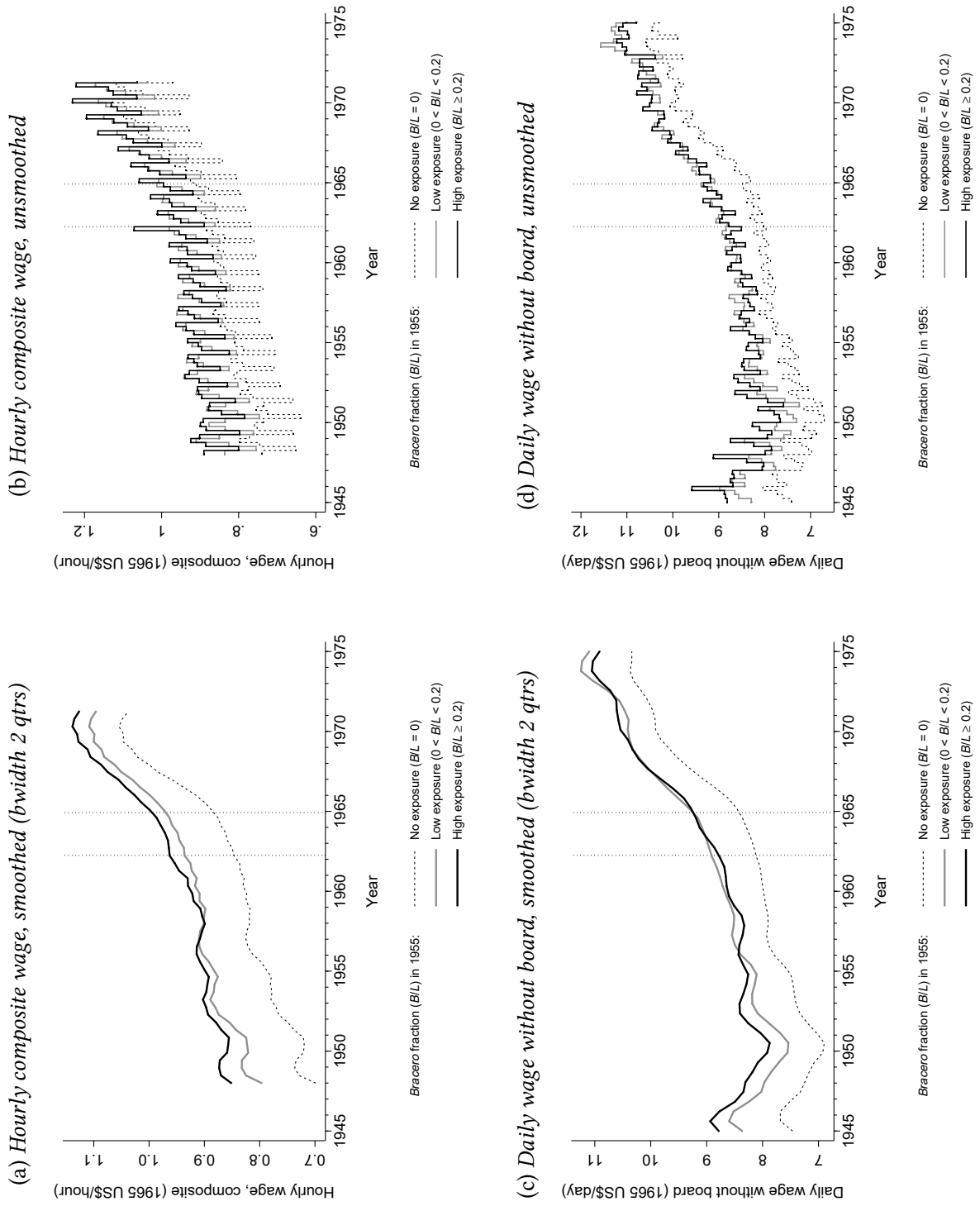
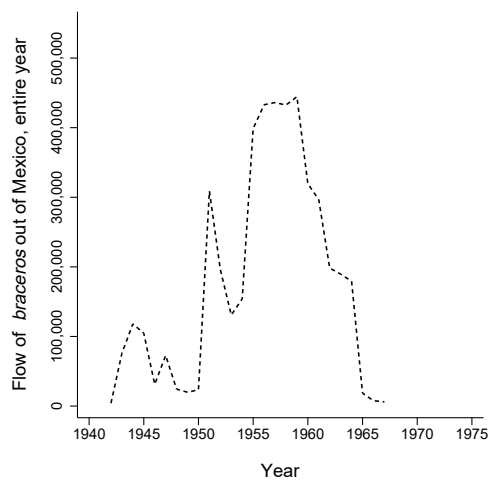
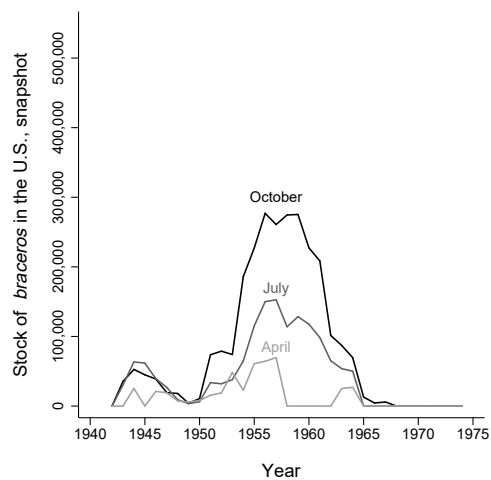


Figure A5: Comparing outward *bracero* flows reported by Mexico with inward stocks reported by U.S. Dept. of Agriculture

(a) *Outflows from Mexico reported by Mexican government, entire year*



(b) *Stocks reported in U.S. by U.S. farmers, snapshot in various months*



Sources: In [Figure A5a](#) the annual total outflow of *braceros* from Mexico to the U.S. is from [González Navarro \(1974, Vol. 2, Cuadro 39, p. 141\)](#), which sources principally the *Anuario Estadístico de los Estados Unidos de México* but contains data not reported in the *Anuario*, for years 1955–1957, gathered directly from the government by the author.