REPLICATION STUDY OF: IMMIGRATION RESTRICTIONS AS ACTIVE LABOR MARKET POLICY: EVIDENCE FROM THE MEXICAN BRACERO EXCLUSION

[Clemens et al., 2018]

NOVEMBER 5, 2023
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This study reproduces and replicates the results of the paper "Immigration Restrictions as Active Labor Market Policy: Evidence from the Mexican Bracero Exclusion" by Clemens, Lewis & Postel published in American Economic Review in 2018. The paper's main contribution is collection and aggregation of the employment, wages, and agricultural production data from 1950s and 1960s and showing ineffectiveness of the active labor reform cutting seasonal Mexican workers (braceros) from the US market on domestic employment and wages, using difference-indifference approach. The paper focuses on agriculture as a predominant workplace of the braceros.

The replication of the paper focuses on the 3 most important aspects. First, I reconstruct the treatment variable to remove the seasonal effect of weather and other exogenous factors potentially affecting the results. Second, I explore variation in employment to explain high values of standard errors and no correlation between the treatment and dependent variables. Third, I argue that technology adaptation is poorly proved in the paper and I show evidence that the accelerated adoption of technology in more exposed states did not take place in every sector, and that the examples shown are quite selective. Although the results hold after data manipulation performed, other assumptions raise doubts of the casuality imposed by the authors of the paper. Low value of the estimates and high standard errors point us to misspecification, endogeneity and mismesurment errors.

1. Introduction

The topic of immigration's effect on wages is increasingly studied for numerous reasons. The world's migration flow has been constantly increasing, at the same time it became one of the main political topics and numbers or correlations often serve as arguments in political debates. Economists attempt to summarize quantitative results of the change in immigration flow, its effect on local markets and long-run consequences. Peri and Sparber [Peri and Sparber, 2009] argue that local workers adjust and specialize in different tasks than the immigrants using their comparative advantage in communication skills. This results in modest effect of increased immigration on wages in a longer term. The fact is supported by studies of David Card [Card, 2001] [Card, 2007], Card and Ethan G. Lewis [Card and Lewis, 2005], and Lewis [Lewis, 2005] using city- and state-level data,

and find almost zero effect of immigration on the wages of less educated native workers. There has been numerous studies on Mariel boatlift labor supply shock in Florida following Card [Card, 1990], Bojras [George J. Borjas, 2015] and others implying that an increase of labor supply decreases the wage. Here, the authors state that the cut of the labor supply did not result in the analogous results.

The authors found that ending the bracero program didn't significantly impact wages and employment for domestic farm workers. This contradicts the belief that reducing labor supply would raise wages and create more jobs for local workers. According to the authors, employers adapted to the decreased labor supply by an increase in technology utilization in the production process, which explains the unexpected result.

This study attempts to analyze the results of Clemens, Lewis and Postel by manipulating the sample, studying regional effects, finding the source of variation in employment and wages, finally exploring the technology acceleration argument proposed as the main results driver. The paper proceeds as follows: Section 2 summarizes the paper's reasoning and results. Section 3 is a replication of the paper using reasonable modifications. Section 4 adds on several robustness checks. Section 5 concludes highlighting the importance of the analysis conducted in this study.

2. Summary of the paper

The authors first present a model in which the absence of adjustment in technology results in the wage increase when the labor supply decreases. When the authors use parameters estimated in different papers, the absence of any adjustment results in elasticity of wage with respect to braceros share of 0.4, with capital adjustment around 0.1 (an increase of braceros share by 1% decreases the wage by 0.1%. However, it is shown when the technology adjustment prevails, there can be a possibility of a constant wage when the share of output produced by advanced technologies increases. If the less labor-intensive technology is not available, an increase in wages may appear to a certain point. Thus, the authors argue that the technologies replacing labor were available before the studied reform.

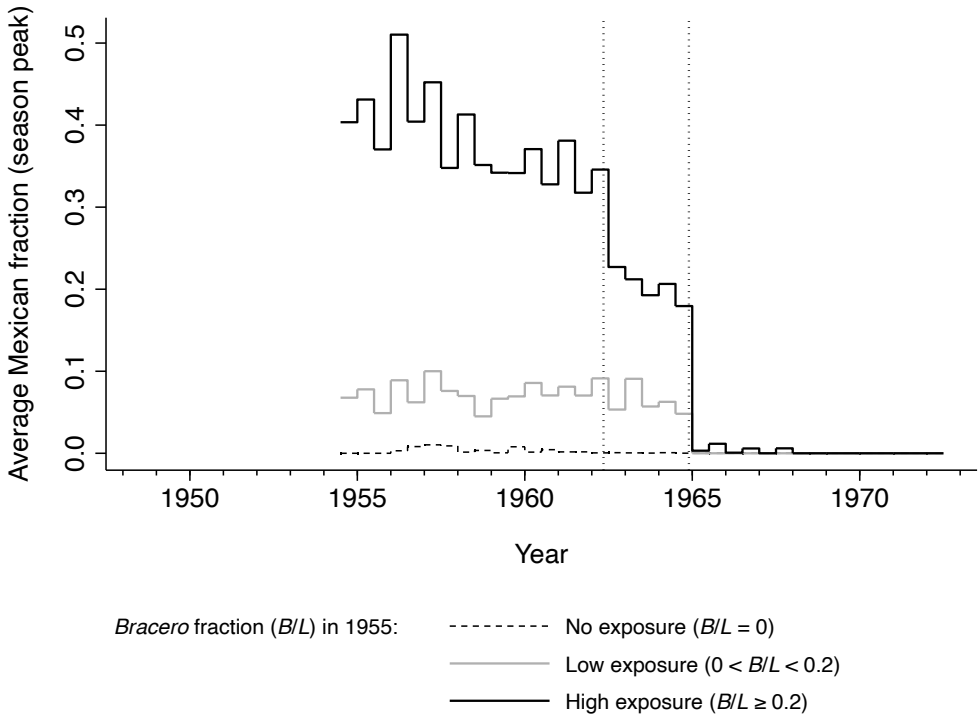
The paper studies the employment and wage consequences of the exclusion of Mexican seasonal farm workers (braceros) using difference-indifference analysis. The data is self-collected using governmental sources. It provides a full coverage of employment (by state, yearly) and wages (quarterly) from 1954 to 1971.

Clemens, Lewis and Postel proceed with a formulation of the treatment variable. They choose the ratio of bracero workers to all the workers in the peak of the immigration in 1955 as the treatment variable showing the effect of the reform on the states, experiencing different labor supply cut. States that are the exposed the most are with the share of braceros ≥ 0.2, certain exposure happens when the share $\in (0, 0.2)$, no exposure means the share is equal to 0. They define 1965 (the reform year) as the cut. As the share of bracero workers fell to 0 after the reform, the coefficient by the fraction of braceros γ measures the treatment. The specification follows:

$$y_{s,t} = \alpha' I_s + \beta' I_t + \gamma(I_{t\geq 1965} * \bar{l}_s^{1955}) + \epsilon_{s,t} \tag{2.1}$$

Where $y_{s,t}$ is wage (quarterly) and $\bar{l}_s^{1955}$ is the braceros share of labor force in 1955. Fixed effect of state and time are captured respectively by α and β. The authors test the result for both cuts, the announcement of the reform in 1962 as well as its implementation in 1965 motivated by the braceros share in 1955, as shown in Figure 1.

FIGURE 1. Average Mexican fraction in 1955 by exposure



The regressions results are presented in Table 1. Columns (3) and (4) cover only 1960-1970.

TABLE 1. Differences-in-differences with continuous treatment, quarterly

	(1)	(2)	(3)	(4)
	realwage_hourly	realwage_daily	realwage_hourly	realwage_daily
treatment_frac	-0.0356	-0.385	-0.0401	-0.0247
	(0.0426)	(0.495)	(0.0315)	(0.309)
N	4324	5813	2024	1901
adj. R^2	0.773	0.835	0.733	0.758
N_clust	46	46	46	46

Standard errors in parentheses

The results are inconclusive - a null hypothesis of the treatment coefficient being equal to 0 cannot be rejected. The estimates are very small, and in fact show that that if any effect exists, it's negative - more exposed states experience lower

wage growth. Semi-elasticities indicate a negative relation between exposure and wage, however a chi-squared test shows they're different from the predicted value of 0.1. The results are compatible with the theoretical case of capital, technology and output adjustment.

Employment evolution is presented in the figure 2, illustrating no visible change after stopping braceros program. The disparity between regions with high and low levels of exposure remains relatively consistent both before and after exclusion. Interestingly, the gap between areas with high exposure and those with no exposure decreases during the program period, contrary to the anticipated scenario where bracero exclusion would have led to an influx of domestic labor into farm work, potentially crowding the workforce.

FIGURE 2. Employment evolution of agriculture Mexican and domestic workers from 1954 to 1973

Similar results appear from the regression on employment presented in Table 2. The coefficient estimates are statistically indistinguishable from zero, mainly due to their high standard errors.

TABLE 2. Differences-in-differences with continuous treatment, monthly, Jan 1954–Jul 1973 only

	(1)	(2)	(3)	(4)
	domestic_seasonal	ln_domestic_seasonal	domestic_seasonal	ln_domestic_seasonal
treatment_frac	-6949.2	-0.311	1843.0	-0.113
	(9093.5)	(0.509)	(6859.3)	(0.375)
N	10329	6386	6072	3707
adj. R^2	0.055	0.085	0.079	0.076
N_clust	46	46	46	46

Standard errors in parentheses

The authors also examine three distinct worker groups: local, intrastate, and interstate. Their analysis reveals no substantial evidence of migration within or between states that could mitigate state-specific impacts on the bracero labor supply.

3. Replication results

First, the treatment variable seems slightly unclear. The measurement of exposure of the states to the reform is not necessarily captured by the peak in braceros workers. Also, the peak can differ across the states. What is more, taking the data from 1 year can be flawed by unknown conditions, e.g. weather, fertility, plant diseases etc. Therefore, to smooth those effects I construct a new treatment variable - an average fraction of Mexican workers between 1954 and 1962 in a state. Second, an interesting fact is that there's no state with exposure between 0.1 and 0.2. I modify that assumption defining high exposure as an average exposure over 1954-1962 ≥ 0.1, as I argue that a 10% drop in labor supply is a significant share. Our sample of highest exposure consists of 8 states comparing to 6 states in the paper.

3.1. **Wages.** Results of the quarterly wages regression with the new treatment variable is shown in Tables 3, 4 with p-value test for elasticities being equal to predicted 0.1 in Table 5. Columns (3) and (4) cover only 1960-1970, as 5 years before and after the reform.

TABLE 3. Differences-in-differences with continuous treatment, quarterly

	(1)	(2)	(3)	(4)
	realwage_hourly	realwage_daily	realwage_hourly	realwage_daily
treatment_frac	-0.0309	-0.278	-0.0341	0.106
	(0.0446)	(0.527)	(0.0320)	(0.316)
N	4418	5945	2068	1945
adj. R^2	0.776	0.837	0.735	0.758
N_clust	47	47	47	47

Standard errors in parentheses

TABLE 4. Semielasticities, DD with continuous treatment, quarterly

	(1)	(2)	(3)	(4)
	ln_realwage_hourly	ln_realwage_daily	ln_realwage_hourly	ln_realwage_daily
treatment_frac	-0.0701	-0.0864	-0.0644	-0.0216
	(0.0652)	(0.0884)	(0.0503)	(0.0496)
N	4418	5945	2068	1945
adj. R^2	0.711	0.806	0.627	0.689
N_clust	47	47	47	47

Standard errors in parentheses

TABLE 5. P-values of semielasticities, test elasticity=0.1

r1	r1	r1	r1
0.0122	0.0405	0.0020	0.0180

The results remain consistent, however we can observe a significant change in estimates' values. Comparing table 3 to table 1, the estimate value by daily wage is 28% higher. This shows notable difference when using the average exposure over years and only the peak year of 1955. It implies that the variance of bracero workers might be an issue in construction of a proper treatment variable. Again, the results are insignificant because of high values of standard errors. This can potentially lead us to misspecification of the model. The treatment variable measures indirectly the labor supply - after the reform we can observe a decline in number of farm workers 2. Therefore, our labor supply affects wages, but wages affect labor supply as well. Simultaneity issue is worth highlighting, even using 1955 treatment variable.

3.2. **Employment.** I conduct parallel analysis on employment rates, the results are presented in Table 6. Columns (3) and (4) cover only 1960-1970.

TABLE 6. Differences-in-differences with continuous treatment, monthly, Jan 1954-Jul 1973 only

	(1)	(2)	(3)	(4)
	domestic_seasonal	ln_domestic_seasonal	domestic_seasonal	ln_domestic_seasonal
treatment_frac	-7445.0	-0.390	2294.4	-0.145
	(9419.5)	(0.442)	(7208.1)	(0.349)
N	10553	6470	6204	3762
adj. R^2	0.054	0.087	0.078	0.078
N_clust	47	47	47	47

Standard errors in parentheses

Again, the estimates follow the same direction and do not differ much from the original ones. However, despite treatment variable smoothing, the standard

errors of the estimates remain critically high which does not allow us to draw any conclusion from the regression.

TABLE 7. Differences-in-differences with continuous treatment, monthly

	(1)	(2)	(3)	(4)	(5)	(6)
	Local_final	Intrastate_final	Interstate_final	ln_local	ln_intrastate	ln_interstate
treatment_frac	-3460.7	-8561.5	458.4	-0.596	-0.819	-0.596
	(4975.4)	(9494.6)	(1168.2)	(0.632)	(0.645)	(0.457)
N	10553	6454	6455	6824	4721	5839
adj. R^2	0.054	0.050	0.016	0.067	0.079	0.052
N_clust	47	47	47	47	47	47

Standard errors in parentheses

Results of Table 7 are also consistent with the authors' findings, that there was no additional movement within and across the states in the more exposed states.

3.3. **Region analysis.** Another reasoning that can be used to justify the variance in the results is simply geographical difference. States in different locations are characterized by completely different types of climate, thus crops, which makes them incomparable. Therefore, I conduct the analysis using West South Central region containing 4 states: Texas, Oklahoma, Arkansas and Louisiana. Fortunately, Texas belongs to the highest exposure group with average rate of braceros at 23.8%, Arkansas with 10.2% while OK and LA have no braceros which allows us to examine the impact of braceros share on wages and employment.

TABLE 8. Differences-in-differences with continuous treatment - Region West South Central, quarterly

	(1)	(2)	(3)	(4)
	realwage_hourly	realwage_daily	realwage_hourly	realwage_daily
treatment_frac	0.0462	0.111	0.119	1.241
	(0.204)	(1.866)	(0.211)	(1.617)
N	376	532	176	176
adj. R^2	0.944	0.950	0.932	0.898
N_clust	4	4	4	4

Standard errors in parentheses

The standard errors of the estimates remain very high, the values low, which leads us to conclude that the estimates remain consistent, as they are indistinguishable from 0.

Employment regression follows same pattern as the original results.

4. ROBUSTNESS CHECK

4.1. **Different estimation.** Authors conduct a test of the relationship between wage, employment and bracero stocks during the program using a different estimation method - Baltagi-Li semiparametric fixed effects regression under nonzero

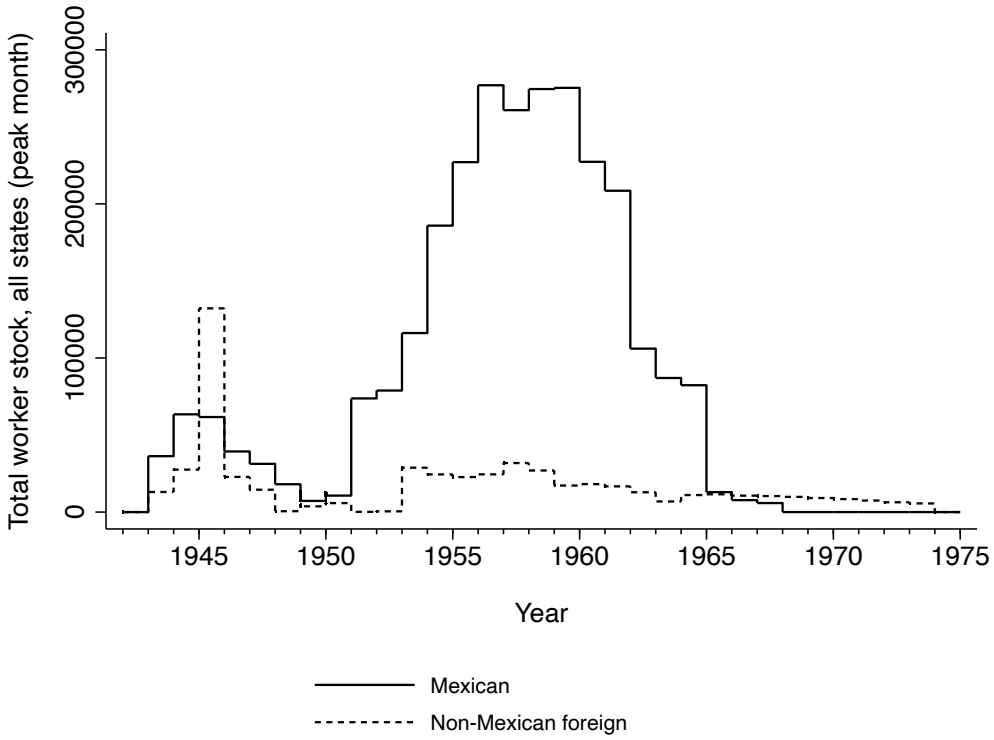
bracero stocks. These findings demonstrate that there is no notable economic or statistical inclination for wages or domestic employment to increase as the number of bracero workers decreases.

4.2. **Pre-existing trends.** Concerns about pre-existing trends potentially biasing the results of difference-in-differences regressions were addressed. When analyzing the data on a yearly basis, the study shows that there were no notable pre-existing trends in wages. Although there were pre-trends in employment, these were not significant when the analysis was limited to states with nonzero exposure to the program. The inclusion of state-specific linear time trends in the difference-in-differences regressions did not substantially impact the outcomes for wages or employment.

4.3. **1962 as the exposure year measure.** The authors address the doubts of using only the peak year as the intensity of exposure replacing it by 1962 (year of the program cut announcement). They reach the same conclusions of no influence of intensity of exposure on further wages and employment.

4.4. **Illegal immigration.** Next issue might be other sources of immigration. A simple replacement of Mexican workers by other foreigners does not seem to be the case as per Figure 3.

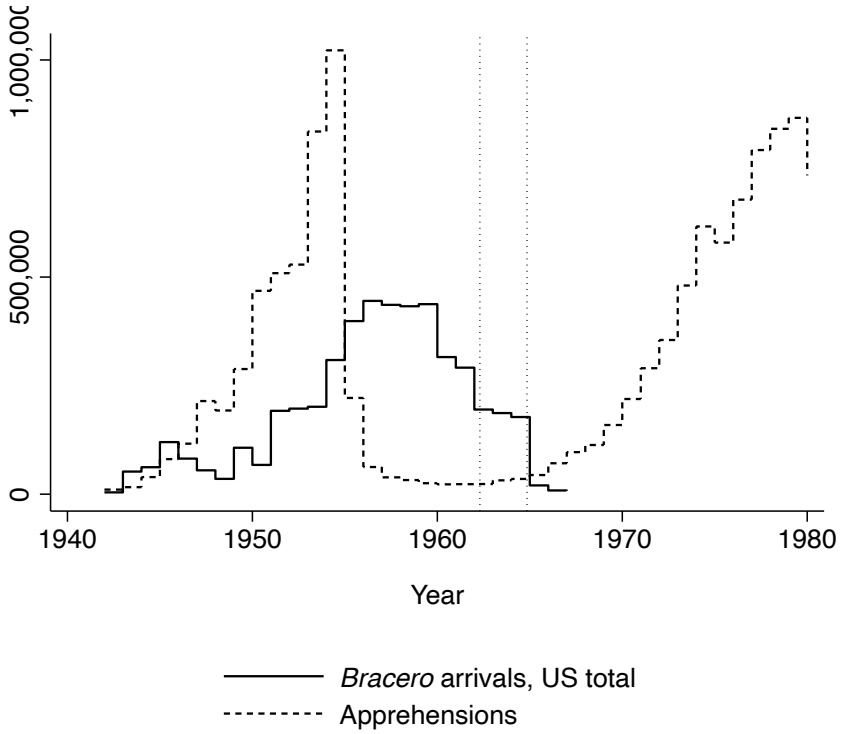
FIGURE 3. Mexican and non-Mexican foreigners worker stock



Moreover, the authors argue there was no illegal immigration issue as 99.5% of braceros were registered back at their return. However, I display their graph

showing braceros stock and Mexicans apprehensions using 1 scale while in paper they use 2 different scales, 2 times smaller for the bracero arrivals.

FIGURE 4. Annual bracero fows compared to apprehensions of Mexicans



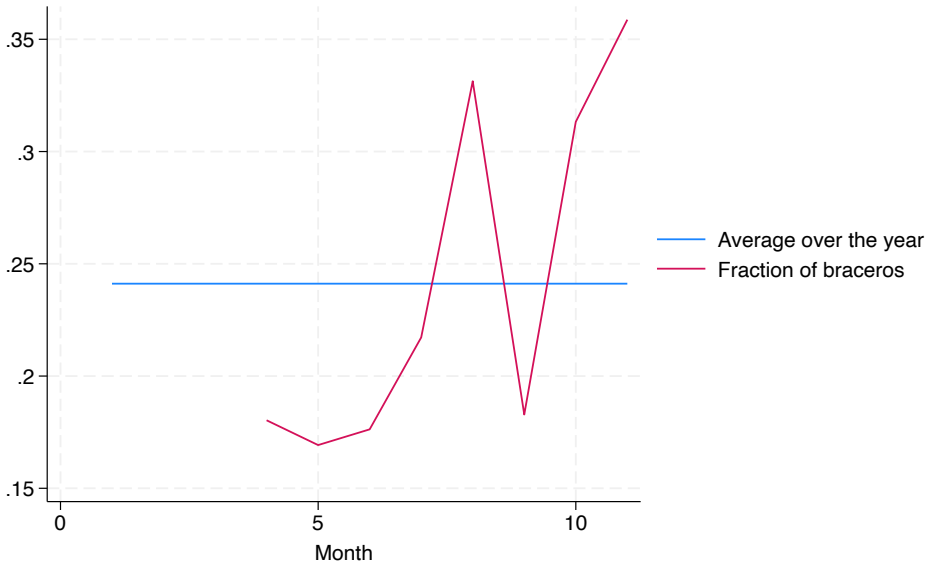
We see a huge increase in apprehensions reaching the peak level of arrivals in 1973 and almost doubling them before 1980. By the year 1970, apprehensions had reached a level comparable to the bracero stock of 1960, indicating a complete substitution by legal laborers. This observation strongly implies a significant interchange between legal and illegal Mexican labor throughout the entire duration examined in the paper. Consequently, the patterns of legal and illegal Mexican farm labor flows indicate that the commencement and termination of the bracero program had minimal impact on wages due to the negligible alteration in the actual labor supply.

4.5. **Variation in wages and workers.** As the estimates of exposure seem to be uncorrelated with neither the wage nor employment, I study their time-variation to potentially discover the real reason behind. Surprisingly, the utilization of bracero labor within a state exhibited significant fluctuations between peak and non-peak seasons. During non-peak periods, the presence of bracero labor was considerably diminished, often amounting to a fraction (See figure 5) of its representation in peak seasons. As a result, there exists a limited correlation between the 1955 proportion of bracero labor in a state (considered as the treatment measure) and the quantity of bracero labor available in a specific year-quarter when

wages were assessed. Even during the year 1955, there's a significant variation of braceros' fraction in one of their main' recipient - Texas.

The variation occurs also in the number of braceros admitted. The years 1956 to 1959 witnessed the peak influx of bracero workers, with more than 400 000 Mexicans admitted each year. However, a discernible decline occurred between 1959 and 1960, with a reduction of ca. 120 000 braceros. Likewise, between 1961 and 1962, there was a drop amounting to over 95 000 fewer braceros admitted. Subsequently, from 1962 to 1964, the number of bracero laborers admitted remained relatively stable at around 180,000. Then, the number of braceros dropped almost to 0, which constitutes a comparable to the shock of 1959-1960 which is left unconsidered in the paper. The substantial fluctuations in bracero labor over time suggest that the cessation of the program in 1965 did not have a decisive impact on altering the labor supply for farm workers. Thus, it is unreasonable to use 1965 as a "cut", as wages before that period might have been already affected by the decreasing foreign labor share.

FIGURE 5. Share of braceros on farms in Texas in 1955 by month



Furthermore, there is a problem of unaddressed variation of the farm size and type of crops picked by braceros. According to US Congress Agriculture Committee [House of Representatives and 2009, 1963], in 1959, the bracero program reached its zenith with 275,000 braceros employed, with Texas hosting 137,000, constituting half of the total workforce, predominantly engaged in cotton harvesting. However, by September 1962, the peak employment figures dwindled to 106,000, of which 68 percent, or 72,000 workers, were located in California. This dynamics might be a reason behind inconclusiveness of the authors' results. The authors use the average over all type of crops and farms which might lead to biased estimates.

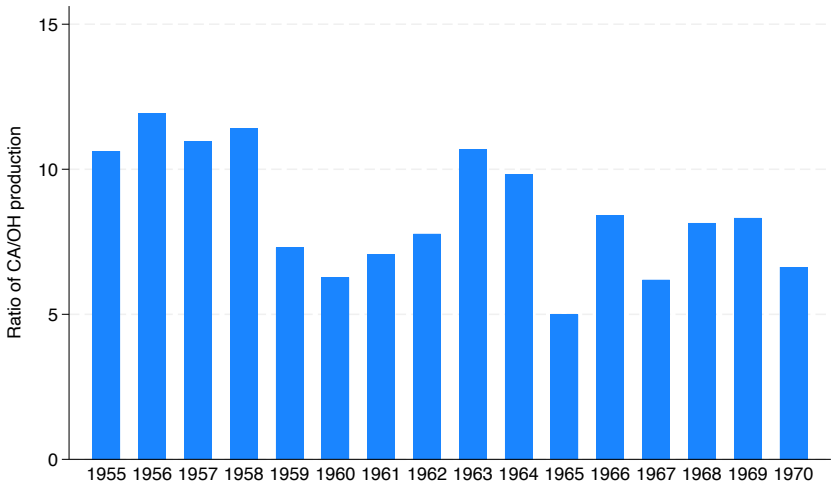
5. Mechanism analysis - technology

The theoretical model gives us a clear idea of the reason behind no visible wage growth - capital-labor substitution and adjustment of technology. In this section, I analyze the section of the paper explaining the technology advancement when stock of braceros is shrinking. I show that the tomatoes mechanization rate is inaccurate and it is impossible to draw conclusions on the basis of the event study conducted.

5.1. **California and Ohio comparison.**

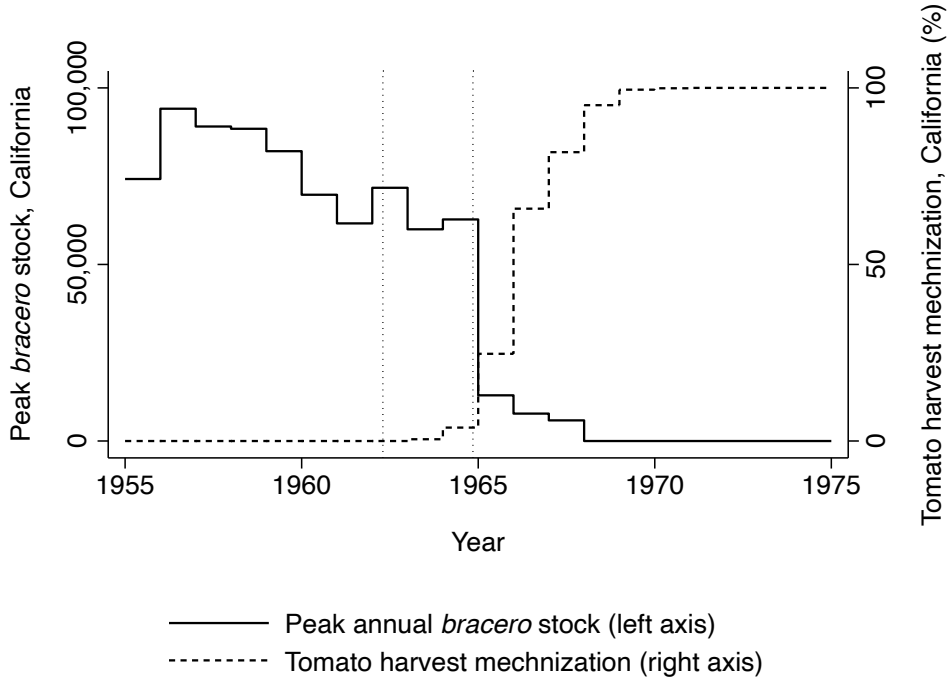
The authors illustrate that the technological adjustment proceeded quickly using tomatoes mechanization rate in California (high exposure) and Ohio (low exposure state). The graph indicates that after 1965 the mechanization rate accelerated from 3.8% in 1964 reaching 99.5% in 1969. In Ohio these rates are 0% in 1964 and a peak of 14% in 1975. However, comparing these two states cannot lead to any conclusion, as there's a high chance the type of farming tomatoes in California and Ohio was completely different. To demonstrate it I construct a variable measuring ratio of production in both states (See figure 6).

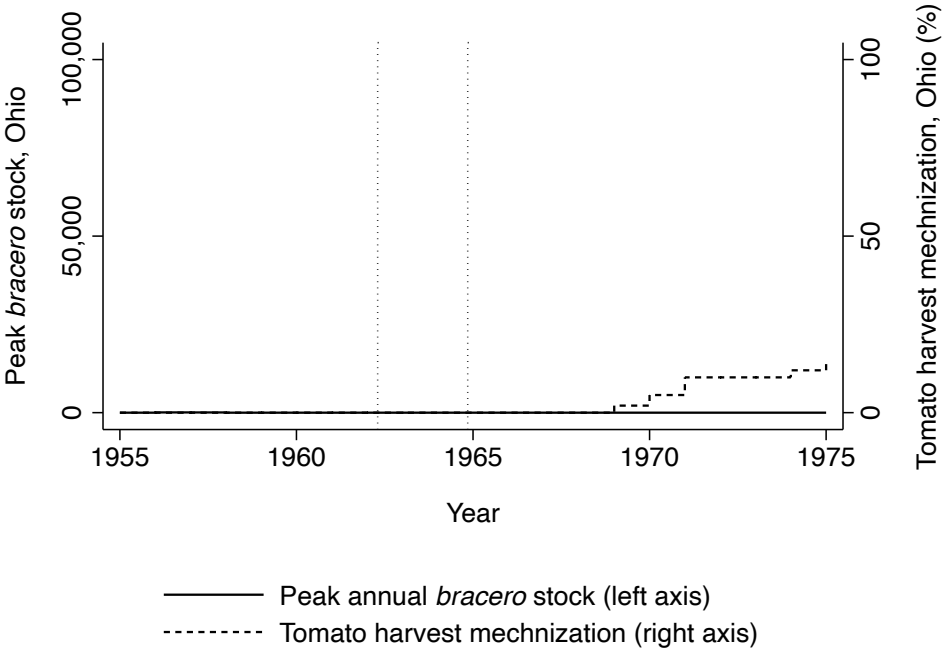
Figure 6. Tomatoes production ratio of California and Ohio



California produces almost 10 times more tomatoes in 1964 with the ratio declining but still remaining high level of over 5 in 1970. The potential heterogeneity in farm sizes, productivity and other unobservable factors make it infeasible to compare these states based on exposure. Moreover, as argued before, the braceros' labor share experience significant shocks before 1965. Following authors' reasoning, technology adjustment should follow earlier which is not visible on the graph (See figure 7).

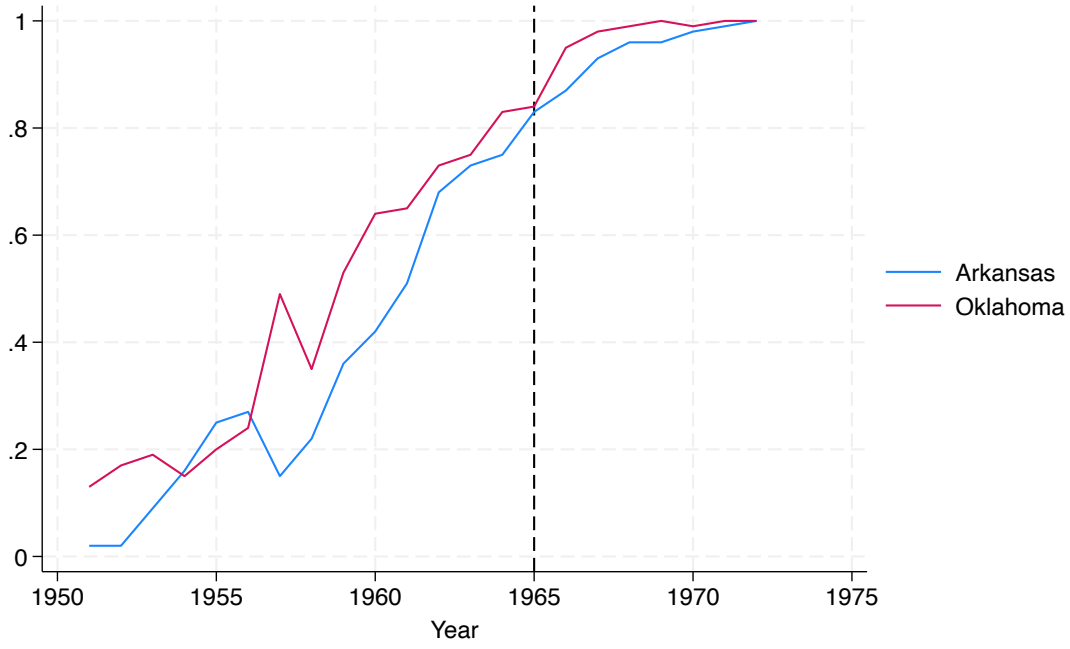
FIGURE 7. Peak Annual Bracero Stock and Mechanization of the Tomato Harvest, in the Two States with Mechanization Time Series





Further, I proceed with a counter-example of the mechanization rates shown. I take two states that are actually similar (in terms of agriculture conditions, based on geography), and that experience different exposure rates. Arkansas had an average share of braceros of 10.2% in 1954-1962 while Oklahoma had no braceros. As the tomato mechanization data is unavailable for these states, I use cotton mechanization as a measure of technology adaptation. Figure 8 proves there's no particular correlation between high share of braceros and faster technology adaptation. Both neighbouring states experienced similar rates reaching 100% in 1970s.

FIGURE 8. Mechanization rate of the Cotton Harvest in Arkansas and Oklahoma



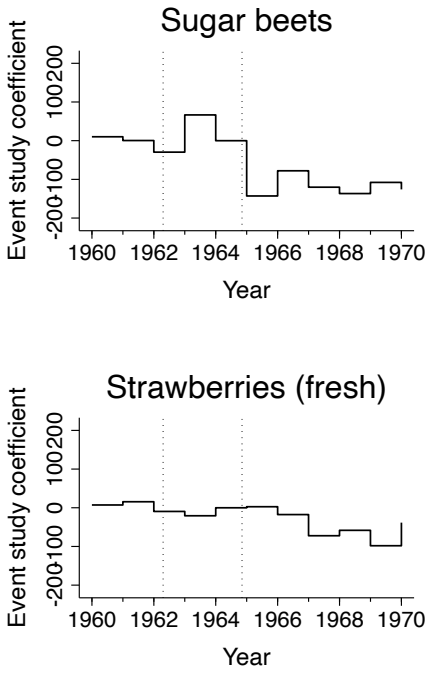
5.2. **Crops after the reform - inconclusiveness.** As per authors, advanced mechanization technology was accessible for cultivating tomatoes, cotton, and to some extent, sugar beets. However, there was no equivalent technology at that time for growing most other labor-intensive crops such as asparagus, strawberries, lettuce, celery, cucumbers, citrus, and melons. They conduct an event study to prove that crops with no technology available and labor-dependent experienced drop in production. An event study is specified as follows:

$$y_{s,t} = \alpha' I_s + \beta' I_{t \neq 1964} + \gamma' I_{t \neq 1964} * \bar{l}_s^{1955} + \epsilon_{s,t} \tag{5.1}$$

The variable $I_{t \neq 1964}$ represents a vector of year indicators, excluding the reference year 1964. The parameter vector γ is to be estimated, with the condition that $\gamma_{1964} \equiv 0$. The variable $y_{s,t}$ denotes a state- and crop-specific measure of physical production, which has been adjusted to a scale of 100 in the year 1964. Following

authors, this coefficient should stay at a similar level for crops with technology available and drop for others. However, for sugar beets (with technology partially available) we observe a decline after 1965 with a stable, lower level in the next years. The strawberry production (with no automatization available) experiences a small decline only after 1966 with a recovery in 1971 (See figure 9). This is largerly inaccurate evidence of technology adaptation and braceros' exclusion dependency.

FIGURE 9. Event Study Regression Coefficients: Crop Physical Production Index



6. CONCLUSIONS

The study of Clemens, Lewis and Postel significantly contributes to the field by an enormous effort of data collection. The robustness checks provided in the appendix are exhaustive, and the narration is convincing. No effect of the reform is shown in various ways, using different estimations and specifications of the model, adding time- and state-trends.

However, some of the checks are selective and overestimate the significance of the reform. In fact, I prove there were a lot of different factors affecting the results. While failing to come up with more accurate estimates, I show that mismeasurement of the treatment variable, as well as of wages and employment, pre-reform falling foreign labor supply, independence of the technological change and the reform dramatically reduce the causality of the results imposed in this paper. I also identify counter arguments for mechanization and exposure to the

reform correlation. All these effects lead us to the conclusion that the effect of the reform is hardly measurable.

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