



Comment on Kaestner, “Revisiting the Bracero Guest Worker Reforms”

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[LINK TO ABSTRACT](#)

In Clemens, Lewis, and Postel (2018), we evaluate one of the largest active labor market policy interventions of its kind: the federal government decision to bar almost half a million Mexican bracero workers from the U.S. labor market in the 1960s. We—henceforth CLP—found no evidence that this policy intervention raised wages or employment for U.S. workers. Robert Kaestner (2020) likewise presents no evidence that this policy raised wages or employment for U.S. workers. Apparently there is consensus that no such evidence exists.

What Kaestner does instead is to state three speculations.

Robustness

First, Kaestner speculates that the results in CLP (2018) might not be robust to different definitions of the policy treatment. He conjectures that the empirical result might change if exposure to bracero exclusion were considered to begin in 1962 rather than 1965, and that the result might change if the measure of exposure used a different pre-exclusion year or month. He does not actually conduct any such tests.

These claims are incomprehensible, given that the original CLP paper already

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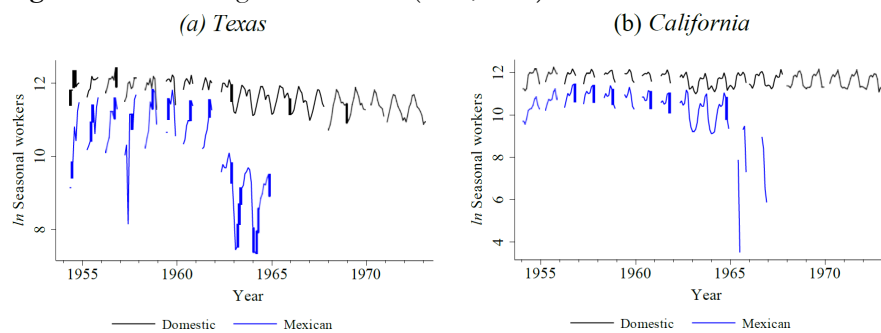
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tests and rejects them. The original paper discusses how “[t]he Kennedy administration began the process of bracero exclusion in March 1962” (CLP 2018, 1469). It discusses how that caused a pre-1963 decline in bracero exposure, which is prominent and extremely clear in the original paper’s Figure 2 Panel A (ibid., 1476). That is why the original paper exhaustively shows that the assumption of treatment in 1965 does not matter:

1. It re-runs the difference-in-differences regressions with 1962 as the treatment year (CLP 2018, A-24–A-25⁴ Tables A7 and A8).
2. It reports full event-study regressions, with a *separate coefficient on each year* so that the interested reader can choose any year as their preferred treatment year (ibid., A-20–A-21 Figures A1 and A2).
3. It reports fixed-effects regressions with no assumption of a before-or-after period (ibid., 1480 Figure 4, A-18 Tables A3 and A4).

For even greater transparency, the original paper’s Figure A3 (CLP 2018, A-22) graphs the raw data, making it as clear as can be that major shocks happened in 1962 and 1965, not before that, and that the choice of peak month is not relevant. Parts of that existing figure, for the most important states, are reproduced here in Figure 1.

Figure 1. Detail of Figure A3 in CLP (2018, A-22)



These raw data show a large, sudden disappearance of braceros in 1962 and another in 1965. They show no substantial change in the hiring of U.S. workers—in *any* month of the year, relative to *any* pre-exclusion base year. This result is obviously not sensitive to the precise choice of pre-treatment year or month as the measure of exposure. These and dozens of other robustness checks have been publicly available in the CLP appendix since April 2017, over a year before the

4. Citations to page numbers A-1 through A-39 of CLP (2018) refer to the paper’s appendix ([link](#)).

paper was published.

CLP (2018) not only posted all of their data and replication code online but went further to post scans of all of their primary archival sources online ([link](#)). If one has concerns about the robustness of the CLP results to changes of specification, those concerns can be tested with trivial effort. Kaestner presents no reanalysis of any kind.

Mechanism

Second, Kaestner conjectures that the mechanism of CLP's result might not be induced technical advance. He states that he does not find that mechanism plausible (Kaestner 2020, 5). The fruitful course for a researcher with concerns about the "plausibility" of this mechanism would be to gather data to test their hypothesis and conduct such a test.

That is what Shmuel San (2019) does in striking new research. Using bibliometric analysis of patent filings before and after bracero exclusion, San shows that immediately after exclusion there was a large surge of innovation in production technology for crops that had depended heavily on bracero labor—but not for other crops. This strongly rejects the hypothesis that bracero exclusion had no effect on labor-saving innovation, corroborating the mechanism for which CLP offered only, as they described it, "suggestive" evidence (2018, 1483).

The hard work of gathering historical data advances economists' knowledge of historical events. Speculation about "plausibility" does not.

Unauthorized migration

Finally, Kaestner speculates that if the excluded bracero workers had been immediately replaced by a vast wave of unauthorized migrant workers from Mexico, that would offer a different mechanism for the null effects on U.S. workers' wages and employment. This is hypothetically true but would require a massive historical event that did not occur and for which Kaestner offers no evidence.

As CLP (2018) discuss at length, 99.87 percent of the last bracero workers in 1964 were recorded returning to Mexico, at a time when unauthorized migration from Mexico was close to zero. The pre-exclusion braceros did not simply stay on illegally in the United States; they went home. That means that if the excluded braceros were replaced by unauthorized workers in 1965, they would have to cross the border anew.

Evidence against immediate replacement of the braceros

In other words, immediate replacement of the braceros by unauthorized migrants would require a new, sudden flood of approximately 100,000 new unauthorized migrants crossing the border immediately in 1962 and every year thereafter—plus an additional flood of 200,000 more, immediately in 1965 and every year thereafter. This is the event that Kaestner would need to document to support his claim.

That event is imaginary. Researchers who wish to posit such a historical event would need to provide an explanation for all of the following:

- Stanford historian Ana Raquel Minian (2018, 25) documents from primary sources in Mexico that most of the former braceros remained in Mexico for several years: “A quarter million braceros who had been in the United States flooded into Mexican border cities,” she writes of 1965. “Many of them elected to remain in the northern borderlands instead of returning to their hometowns in the interior of the country because they believed that the program would be renewed.” In those areas, she shows, “Unemployment rates mushroomed...reaching almost 50 percent of the population.” This is inexplicable if unauthorized migrants immediately replaced the braceros.
- John McBride (1963) narrates, firsthand and in detail, the elimination of the bracero workers from cotton farms in the Lower Rio Grande Valley of Texas—directly on the Mexican border. He does not mention unauthorized Mexican workers replacing the braceros at all. This is inexplicable if hundreds of thousands had suddenly begun pouring over the border.
- The U.S. Employment Service published contemporary investigations of how farms were adapting to bracero exclusion in Texas (Hood 1966) and Michigan (Mitchell 1966). Neither of them mentions illegal migration in any form.
- The U.S. Secretary of Labor (1966) reports a detailed contemporaneous investigation into how farmers nationwide were adjusting to bracero exclusion. It does not mention illegal migration at all. It does report widespread crop losses, especially in California where bracero employment was highest.
- The U.S. Department of Agriculture conducted a nationwide, detailed review of the response of farms to bracero exclusion, state by state and crop by crop (Metzler et al. 1967). It reports systematic labor shortages and massive in-state and nationwide efforts to recruit teenagers or

indigenous people to replace the braceros. It reports that growers typically were not able “to recruit a labor force which would take over the jobs formerly performed by the braceros.” It does not mention illegal migration at all, except to note that there was little of it: In Texas, it says, “Both legal and illegal immigration from Mexico has been reduced to a minimum” (Metzler et al. 1967, 14). A typical note reports: “When no braceros were available in 1965, the cucumber growers had great difficulty in obtaining a labor supply” (ibid., 15). None of these statements would make sense if growers had quickly replaced the bracero workers.

- Massey and Pren’s (2012) bibliometric analysis of U.S. newspapers reveals no substantial rise in mentions of illegal migration until just before 1970. This accords with our Figure 2 here. When illegal migration became substantial later on, in the 1970s, it was widely discussed in newspapers.

In other words, if the major historical event posited by Kaestner occurred, it went somehow unnoticed by officials of the USDA, Employment Service, and Department of Labor charged with studying the issue at the time, and unnoticed by journalists at the time, and unnoticed by Texas cotton farmers at the time, and it has been somehow overlooked by historians since. This possibility is remote. All of this is already discussed in CLP (2018).

Kaestner presents no new evidence for his claim that this vast change occurred. He simply reproduces the same graph that is already in the original paper’s appendix (CLP 2018, A-26 Figure A4). That graph from the CLP paper already shows that illegal migration rose after bracero exclusion. What is at issue is whether it rose so quickly and so greatly that labor markets could not react. CLP write that “border apprehensions of Mexicans did not substantially rise *in the years immediately after exclusion*” (2018, 1481, emphasis added).

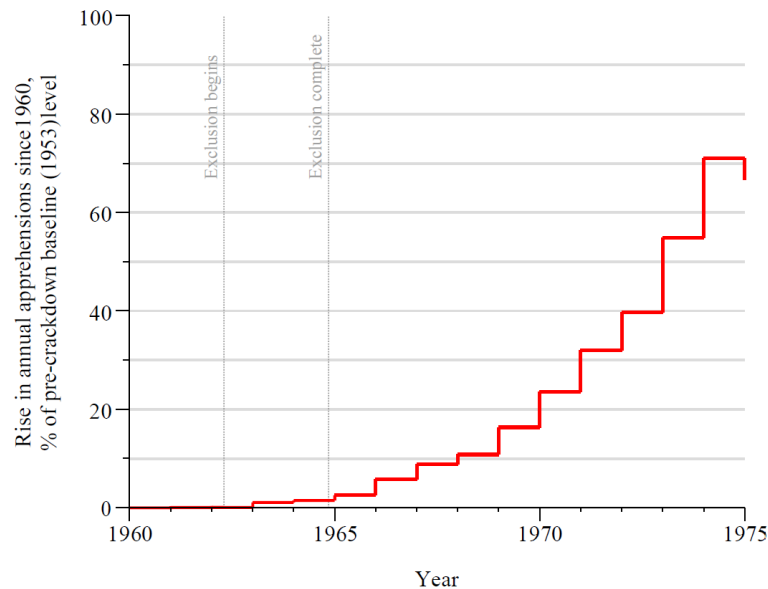
The slow rise in illegal migration in the 1960s

As the CLP appendix also already shows, the rise in illegal migration remained low for several years after the 1962 and 1965 exclusions of braceros. Labor markets had plenty of time to react.

This is apparent in Figure 2 below. It uses the same data in the CLP appendix and in the comment. The vertical axis is the rise in apprehensions of Mexicans as a fraction of the 1953 level (835,311), and this rise is shown starting from 1960. Three years after the early-1962 exclusion, at the end of 1964, apprehensions at the border had risen by just 2.6 percent of their 1953 level. In 1969, seven years

after exclusion began in 1962—and four years after exclusion was completed in 1965—apprehensions had risen by just 10.8 percent of their 1953 level.

Figure 2. Analysis of the existing Figure A4 in CLP (2018, A-26): The rise of illegal migration was small and slow



1953 is the best comparison year because it is the last full year before the bracero program expanded to almost completely replace the market for unauthorized Mexican migrants (Hernández 2010, 187–191). Hypothetically, the same number of observed apprehensions could understate a larger number of unobserved unauthorized migrants if there were a reason to believe that enforcement effort fell greatly after 1962 or 1965, relative to 1953. But there is no such evidence. As the CLP appendix already discusses, there was no decline in the government’s own metrics of border enforcement staffing and enforcement effort in 1965 or several years thereafter (North and Houstoun 1976, 53). Any given migrant’s probability of apprehension was no lower after exclusion than in 1953: In 1967 it was 52 percent, compared to 49 percent in 1953 (Roberts et al. 2013, Appendix 1 p. 7).⁵

The intent of the exclusion policy was to raise wages immediately, as the simplest economic theory predicts it would, not a decade later when the magnitude

5. Enforcement effort was clearly lower in 1965 than in the final year before bracero visa expansion, 1954—because 1954 was the year of the massive crackdown officially named *Operation Wetback*. This is why we choose 1953 as the comparison year. 1953 is the ideal comparison because, although a similar border-enforcement crackdown had been planned for that year (to be called *Operation Cloud Burst*), it was never actually carried out (Hernández 2010, 183).

of illegal migration became large enough to be an important confounder. When the U.S. Senate (1966, 16–17) Committee on Labor claimed that bracero exclusion had successfully raised farm wages, it used data extending only through October of 1965—just one season after exclusion was complete.

Implications for CLP’s analysis

Beyond ignoring the magnitude of illegal migration, Kaestner ignores major anomalies—also already discussed in CLP—that would need to be explained if the braceros were substantially and immediately replaced by unobserved workers (CLP 2018, A-23–A-26). One is that estimates of “total hired workers” that include unauthorized workers, available for most states, fall proportionately with the decline in braceros (*ibid.*, A-25 Table A9). There would be no such change if the braceros were quickly replaced.

Another anomaly is that there is no correlation between bracero exposure intensity and U.S. workers’ wages or employment controlling for state fixed effects, *prior to exclusion, during the bracero program* (CLP 2018, 1480 Figure 4, A-18 Tables A3 and A4). Suppose that we accept the comment’s assertion that bracero scarcity after 1965 was uncorrelated with wages because unobserved unauthorized workers had instantaneously replaced the braceros. How, then, can we explain the fact that very high variance in bracero scarcity was uncorrelated with wages in the 1950s as well, when (as the comment accepts) unauthorized immigration was nearly eliminated? CLP already ask this. Kaestner is silent.

The role of policy evaluation

We conclude with a broader observation about this comment. When the government intervenes in the labor market to raise wages or employment (such as with a job-training program), economists do not presume that the intervention worked until someone can prove a precisely estimated zero effect. They require evidence of a nonzero effect (Card et al. 2017). When the government intervened to exclude the braceros, its explicit and principal goal was to raise wages and employment for U.S. farm workers (Borjas and Katz 2007). Such an effect must be shown with evidence. Kaestner provides none.

CLP use the government’s own data on wages and employment, precisely the data that the government claimed would show the positive effects of its policy intervention (U.S. Senate 1966, 16–17). Using those data is an appropriate test of the government’s claims about that policy. Kaestner dismisses the data as containing no useful information, but does not suggest any other way of testing

the hypothesis. This amounts to the claim that no such tests exist—that no one can ever know the economic effects of this policy. We look forward to seeing more fruitful approaches in this literature that, in contrast, advance economists’ understanding of historical events.

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