Do the Children of Guest Workers Enjoy Positive Effects of Paternal Migration Later in Life? Evidence from the Bracero Program

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Abstract: The Bracero Program was a historical guest worker program between Mexico and the United States that saw the temporary migration of nearly five million agricultural workers to the United States. Guest worker programs benefit the host country with relatively cheaper labor, and the sending communities with influxes of cash earned abroad. The Bracero Program provides an opportunity to understand the long term impact of such a policy on the children of the guest workers. I compare the adult outcomes of those children who were treated with exposure to the program to those children who were not exposed. The method I use is a family fixed effects model that compares siblings born at different times relative to their father's migration to the United States as a bracero. Those born before were less affected by the benefits of participation in the program than were those born after. I find some evidence that those children that were more exposed to their father's migration under the Bracero Program had greater completed education. There is no discernible effect on employment at the time of the survey, although there is suggestive evidence that exposure might have allowed children to move out of agricultural work (i.e., occupational upgrading). However, identification rests on a small number of families and individuals and so most results are too imprecise to draw definite conclusions. Positive effects in the long run provide further evidence of guest worker programs as good development policy. Imprecise results combined with a question of this importance suggest that more work is needed to improve the identification.

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Introduction

The Bracero Program was a guest worker program between Mexico and the United States from 1942 until 1964. It allowed for the temporary migration of nearly five million agricultural workers. They worked under contract for a short period of time at a set wage, and then returned to Mexico with money in their pockets.[1] Looking at the political climate of today, much of the debate over comprehensive immigration reform in the United States includes some discussion of the implementation of a guest worker program. Economists cite short term successes for contemporary guest workers as a reason to believe that these programs would be good development policy.[2] I perform an appraisal of the development impacts of a historical guest worker program to better understand whether or not we can expect long run benefits for participants of the more recent programs.

One way to assess the long run benefits of the program is to look at the eventual adult outcomes of the children of those who migrated to the United States as braceros. Our concept of economic development has changed from one of just increases in per capita income to a more comprehensive paradigm of human development (e.g., improvements in health, education, and other forms of human capital).[3] If this program resulted in investments that created sustained increases in human capital (e.g., health, education, etc.) for the children of participants, then one could claim it was, and continues to be, a long run success in Mexico. In this paper, I answer whether or not paternal migration to the United States as a bracero improved long run outcomes for the subsequent children of those migrants in the form of sustained, significant advantages in human capital outcomes.

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[1] See Galarza (1964), Kosack (2016), Craig (1971), and Anderson (1976), among others, for a description of the Bracero Program.

[2] See Clemens and Pritchett (2013), Kosack (2016), and Gibson and McKenzie (2010).

[3] Consider, for example, the United Nation's Millennium Development Goals (MDGs) (http://www.un.org/millenniumgoals/).

In order to answer this research question, I compare the adult outcomes (e.g., years of education, occupation, etc.) for those who were "treated" by exposure to the Bracero Program as a child in their formative years to those who were not. An identification strategy to eliminate potential selection bias and endogeneity in the estimation of the effect of paternal migration on children's outcomes is required. Specifically, I employ a family fixed effects model. I consider a child treated with exposure to the program if their father migrated to the United States as a bracero during their formative years of development, and untreated if they were too old at the time their father migrated. This framework utilizes individual-level migration histories for braceros and individual-level characteristics for the children of bracero migrants that can all be found in the Mexican Migration Project (MMP) database. I use family fixed effects models to control for all household-level characteristics that affect both the propensity to migrate as a bracero and to invest in the education of children in order to obtain plausibly unbiased estimates of the effect.

One contribution of this work is to produce a more complete understanding of the historical impact of the Bracero Program. Most of the work in other literatures takes a very negative view of this program, citing abuses against workers, the creation of large scale, undocumented migration, and corruption in the administration of the program.[4] More recent work, however, shows evidence of the short term, positive impacts of the program for those who participated and their families.[5] This project highlights additional benefits to create a more comprehensive, historical appraisal of the Bracero Program.

[4] See Galarza (1964), Massey and Liang (1989), Reichert and Massey (1982), Rosas (2011), and Sandos and Cross (1983) for examples highlighting negative impacts on participants.

[5] See Kosack (2016) for a review regarding the Bracero Program, as well as Gibson and McKenzie (2010) for the positive impacts of more recent guest worker programs.

A second contribution is to provide policymakers with evidence of the positive development impacts of good migration policy. Guest worker programs, both historical and contemporary, have short term, positive impacts on participants and their households.[6] However, no one has assessed the long run impact of these programs on development in the sending communities. This is an example of a problem that history has an advantage in solving. Since the program was in place several decades ago, I can fill this gap by looking at long run effects; namely, the eventual adult outcomes of those who were exposed as children. If the positive effect of the program can be shown to have been sustained over a long period of time, then one can argue its benefits are even greater than previously imagined. This is an important step in assessing whether or not guest worker programs can be good development policy tools today.

This new work builds on previous studies I have conducted to identify the short run impacts of the Bracero Program.[7] We know from these studies that braceros used their monies earned abroad to invest in themselves and in their children in the very short run. The bigger question, however, remains: Were these very temporary, albeit large, influxes of cash into the household sufficient to make the necessary investments to change lives for the better in the long run? If that is the case, then guest worker programs are powerful tools in conducting development policy.

Background

For an in depth description of the Bracero Program, its operation, administration, and institutional history, please see Kosack (2016). The Bracero Program was a guest worker

[6] See Antman (2011), Antman (2015), Hanson and Woodruff (2006), Hildebrandt and McKenzie (2005), McKenzie and Rapoport (2011), Woodruff and Zenteno (2007), and Yang (2008) for studies of the impacts of migration. See Kosack (2016) and Gibson and McKenzie (2010) for the short term impacts of guest worker programs.

[7] See Kosack (2016) for two specific examples of studies that identify the positive, short run impacts of the program on migrants and their children.

program between the United States and Mexico that lasted for 23 years, from 1942 until 1964. It was born out of a labor shortage in the United States, stemming from World War II. An increase in the demand for agricultural products and decrease in the supply of workers in the fields, both resulting from the war, spurred farmers to demand some action. The Bracero Program, although starting as a wartime program, obviously lasted well beyond. This program was actually a series of several bilateral agreements signed between the two nations over this time period. This is important because it provides variation over time in the operation and administration of the program. Over the lifetime of this program, between four and five million agricultural workers migrated to the United States to work temporarily as braceros, making it the largest guest worker or temporary worker program in the migration history of the United States.

The Bracero Program was a bilateral effort between Mexico and the United States that was designed to operate in a very specific way. Farmers or farmer associations in the United States would request the opportunity to recruit workers from Mexico, citing difficulties in hiring domestic labor at the prevailing wage. If the request was certified by the government in the United States, the request for workers would be sent down to a recruitment center in Mexico. In Mexico, individuals who desired to work temporarily in the United States would travel from their home to the nearest recruitment center. Either they would go with a *permiso* from the local government official, or they would simply go hoping to be granted a contract. After arriving at the recruitment center, having undertaken the journey at their own cost, they would wait in long lines to be called for a contract. The recruitment centers in Mexico are where worker met contract.

Once the worker obtained a contract, he would be transported at the cost of the employer to his place of employment in the United States. There he would work for the contracted amount of time, at the specified wage and doing the specified tasks. Other benefits were supposed to be provided, including room and board, insurance, transportation costs to the United States and back to the recruitment center in Mexico, etc. Once the contract was completed (anywhere from three to nine months) the worker would be transported back to Mexico. He could either wait in line for another contract, or decided to return to his family. There is very little wage evidence that exists for these braceros, although the best guess is that the daily wage in the United States was nearly seven to nine times what the worker could have earned back in Mexico. Much of the literature available focuses on the abuses that occurred as part of this program. Those did occur and they are well-documented. This paper, however, will focus on the benefits that emerged, despite those abuses, from the process described above.

Empirical Strategy

One of the biggest problems when estimating the effect of migration on some outcome is the obvious endogeneity problem coming from self-selection into migration; those that migrate or those who had parents who migrated are likely different than those who did not. In order to overcome the bias in the estimates from this source of endogeneity, I utilize a family fixed effects model, following the methodology described by Antman (2012).

In this paper, I rely on the timing of the father's first migration to the United States under the Bracero Program, comparing children that are born before their father made this trip to children who were born after their father made that trip. To enrich the analysis, I look at the effect across different types of migration during this time period. By including a household or family fixed effect in the regression, I compare the outcomes of siblings who were born before their father took his first trip as a bracero (or other type of migrant) to those who were born after their father took that trip. This methodology will control for any potentially confounding, omitted factors that are constant across the household (i.e., across all individuals in the household).

I include additional controls in the regression to account for other factors that might bias the results. For example, birth year fixed effects are added to account for cohort effects in educational attainment. Not including these would tend to create a positive bias in the estimates as children born after their father migrated are, by construction, more likely to be born later and those born later are more likely, as a result of education trends in Mexico, to achieve more. Finally, controls that vary across siblings are included, such as sex and birth order.

I estimate an ordinary least squares equation with the elements described above, including household fixed effects, year-of-birth fixed effects, and sibling-variant controls. The regression that is estimated is described below in Equation 1:

$$\begin{aligned} EdYrs_{i,h,b} &= \beta_0 + \beta_1 NotBorn\_Bracero_{i,h,b} + \beta_2 NotBorn\_Undoc_{i,h,b} \\ &\quad + \beta_3 NotBorn\_Other_{i,h,b} + X_{i,h,b}^{T}\alpha + \gamma_h + \mu_b + \varepsilon_{i,h,b} \end{aligned} \tag{1}$$

The estimated coefficient, β_1, gives the relationship between being born after a father makes his first migration to the United States as a guest worker under the Bracero Program and the number of years of education eventually attained (or other human capital outcomes). If there are no sibling-specific shocks that are correlated with both educational attainment (or other outcomes) and the timing of migration relative to birth, then this estimated relationship can be interpreted causally.

Data

The data used to implement this methodology come from the Mexican Migration Project (MMP), a database maintained through a joint effort by Princeton University and the University of Guadalajara. This is a survey of households in Mexico in 161 different communities. The communities are not chosen at random as they are selected for being in high-migration areas, but the households are selected randomly within the communities. Thus, it should not necessarily be thought of as a representative sample for all of Mexico. This is a very rich database, with information collected on the head of household and his or her family, including detailed migration histories, demographic information, educational outcomes, occupational outcomes, and much more. I use the information in this database to construct my sample.

For this paper, I create a sample of individuals who had a father who was born prior to 1946.[8] Households are eliminated from my sample if the head of household is not a male. Households are also eliminated if they have any missing data for key variables. After identifying the targeted households in the survey, I coded treatment by calculating the age at father's first migration for each child with a father who migrated (i.e., year of birth subtracted from year of father's migration). Using this calculated age at father's first migration, I categorize individual children according to how old they were at exposure (e.g., not born, 0-5 years old, 6-11 years old, 12-17 years old, and over 18 years old). Finally, I drop all children who are not yet adults (i.e., younger than 18). This process yields a combined sample of adult children from migrant and non-migrant families consisting of 25,837 individuals in 5,058 different households.

I use the data in the MMP to construct a number of different variables to be used in the analysis. First, I construct variables that will be used as controls in the analysis. The sex of the individual child is used to create a dummy variable that takes a value of one if the child is a male. The birth order of each individual child is determined based on his or her year of birth in relationship to the years of birth for each sibling. Second, I construct a number of different outcome measures for each individual child. Years of educational attainment is given in the

[8] This age cutoff was used because it is the youngest that a father could be in the survey and still be an adult during the time of the Bracero Program.

survey. The MMP also codes the occupation of each individual. I use this two construct two different employment outcomes. One such measure is a dummy variable that takes a value of one if the child holds some sort of occupation at the time of the survey. This is used to measure the effect of paternal bracero migration on employment in general. Another such measure is a dummy variable that takes a value of one if the child holds an occupation as an agricultural worker at the time of the survey. This is used to measure the effect of paternal bracero migration on occupational mobility, as most of these fathers were agricultural workers themselves.

I present the summary statistics for my entire sample in Table 1. The children of had, on average, about eight completed years of education. Sixty one percent of the sample was employed and about twelve percent of the sample worked in agriculture, at the time of the survey. Children of migrant fathers were, on average, about half of a year old at the time of their father's first migration, and most of the sample was born after their father made his first migration.[9] Moreover, 50 percent of the adult children in this sample are male and 50 percent are female. The average child in the sample was the fourth child born in his or her family. This not only paints a picture of what the children in the sample looked like, but also provides a good comparison against which to scale the estimated effect.

The use of family fixed effects means that the only individuals identifying the treatment effect are those who come from families with siblings born both before and after their father made the trip to the United States as a bracero. One should therefore be interested in the descriptive statistics for this group. In the case of this paper, we have a group of such families for each of the three categories of migration that I identify. However, the main focus of the paper is on bracero migration, and so I will concentrate on that group for now. In Table 2, I present

[9] Remember, age at first migration can be negative if the child is not yet born at the time of the migration episode.

similar summary statistics for these 640 individuals in 90 families. I show in that this subgroup is slightly less educated and slightly more likely to be employed in agriculture than the entire sample. We should be concerned about the fact that the effect of interested will be identified from such a small subset of the sample for two reasons: (1) external validity and (2) imprecision in the estimates. This is a tradeoff, however, and the benefits of overcoming selection bias make it a worthwhile endeavor.

Potential Concerns with this Approach

Although this family fixed effects approach is quite useful in eliminating potentially confounding, omitted factors that are constant across the household, there are some concerns that should be addressed. First of all, the MMP is not a nationally representative survey and so, even if the effect is causal, it is important to remember that it is not the average treatment effect for any random individual chosen from the Mexican population. Second, this analysis only takes into account the first trip to the United States by the father in the household. One trip could have resulted in multiple subsequent trips and children born later would have benefited from them as well. Thus, it is not clear whether the first trip is causing the increase or if the effect is resulting from the sum of benefits over multiple trips. These will be addressed further in the section on future work and extensions.

Results

The results from the estimation of Equation 1 on the number of years of education attained are presented in Table 3. All of the models I present include controls for year of birth, birth order, age, and sex. In the first column, I present the model for the entire sample with no household fixed effects. Studies of the impact of migration on children's outcomes often highlight differences by gender. Column 2 shows the results from a regression that restricts the analysis to males only. Column 3 shows the results from a regression that restricts the analysis to only females. In Columns 4 through 6, I repeat the analyses, but include the household fixed effects to correct for unobserved heterogeneity that could bias the results. Standard errors across all specifications are corrected for heteroscedasticity.

The estimate from the fixed effects regression in Column 4 suggests that there is an economically and (marginally) statistically significant relationship between a father's trip as a bracero and educational attainment of subsequently born children. Interpreting the estimate in Column 4, I can say that children in a family born after their father went to the United States under the Bracero Program attain 0.499 years more education than those siblings who were born before he left. Comparing this estimate to the mean for the sample, one can conclude that being born after the father's trip increases educational attainment by about 6.1 percent over the average in the entire sample and 7.8 percent over the average in the subgroup of families identifying the effect. As long as there are no other sibling-specific shocks correlated with both the timing of migration and educational attainment, the positive relationship between bracero migration and eventual educational attainment can be interpreted as causal. Additional observations from this table of results are consistent with a priori predictions. For example, female children attain fewer years of education than males. Furthermore, the results in this Table are split out by sex. The results suggest that benefits of bracero migration to eventual educational attainment of children accrue to male children, but not necessarily to female children. This could be because additional income earned is more likely to be invested in male children, because female children are more likely to be called upon to "fill in" when fathers migrate and leave the home, or some combination thereof. The estimates on bracero migration are all larger in Columns 4 through 6 than they are in Columns 1 through 3, suggesting negative selection on unobservables into bracero migration. Finally, undocumented migration appears to have a similar impact on educational attainment as bracero migration.

In Table 4, I present the regression results for employment. Again, in Columns 1-3, I include results without family fixed effects for the whole sample, only men, and only women. Columns 4 through 6 include family fixed effects. The fixed effects results indicate that there was no statistically significant relationship between a father migrating as a bracero and subsequent children being employed. A preferred approach would be to test whether or not exposure to bracero migration improved the *type* of employment.

I take a step toward that in Table 5, presenting regression results for agricultural employment, conditional on being employed. The majority of braceros were employed in agriculture. If children's exposure to paternal bracero migration caused them to be less likely to be employed in agriculture, one could interpret that as occupational upgrading. Again, in Columns 1-3, I include results without family fixed effects for the whole sample, only men, and only women. Columns 4 through 6 include family fixed effects. The point estimates in the fixed effects regressions in Columns 4 and 5 may suggest that exposure to the bracero program provides boys with the opportunity to seek employment outside of agriculture. However, give the problems with precision, these estimates are not statistically significant.

Different Ages of Exposure

I also run a similar regression to the one described in Equation 1, but I include additional age bins instead of just an indicator for not born in order to look at the effect across different ages of exposure. Results from this estimation are presented in Tables 6, 7, and 8. The omitted age group is those children who were 18 or older when their father took his first trip under the

Bracero Program. All regressions include the same controls previously described. The six columns in each table follow the patterns I described before.

Looking at the results of the fixed effects regressions in Table 6, the first thing that one notices is that the problems of imprecision mean that all estimates concerning bracero migration are statistically insignificant. However, there are some interesting patterns that emerge in the point estimates. For example, the impact of paternal bracero exposure on education falls with the number of years of exposure, suggesting that it is most helpful for children when they are very young. In fact, the estimates for oldest groups exposed are negative (and even younger for males). This could be because paternal bracero migration for older children could, instead of encouraging greater education, paved a way for those children to follow in their father's footsteps and lead a life as a migrant worker. Such an activity would not reward a Mexican education and so could lower the number of years of education.

In Table 7, the fixed effects regressions show a negative impact of paternal bracero exposure on employment. Some of these estimates (especially for boys) are statistically significant. I find this result to be puzzling. One reason for such a result (especially when children are getting more education) is that children who were exposed spend longer in school and so seek employment later as adults. Another reason for such a result (especially for males) is that children exposed follow in their fathers' footsteps and work as seasonal migrant laborers. Thus, they either do not have formal employment in the United States, or they are not employed during the season when the survey is conducted.

In Table 8, contrary to the results from Table 5, we see a higher propensity to be employed in agricultural work as a result of paternal bracero exposure. However, even though the point estimates are positive, they are almost uniformly, statistically insignificant. These results are consistent with, not a story of occupational upgrading, but rather one of intergenerational persistence in employment choice. Obviously, much more work is needed here to more fully explore the occupational choices of the children of the bracero migrants.

Conclusions, Future Work, and Extension

The initial analysis in this project indicates that the positive relationship between a father's migration to the United States as a bracero and the human capital attainment of their children is one that holds in the long run. Children that benefited from the money coming back from the United States end up with more education as adults than those who did not. The results on employment and occupational choice are mixed and merits further investigation. In general, the choice of identification strategy, which relies on 640 children of bracero migrants in 91 different families, is one that trades off precision for internal validity. We can be confident that unobservable family characteristics correlated with the selection into migration are accounted for, but the lack of precision makes the interpretation of the results difficult.

In order to further explore this relationship and the question described in this paper, I will extend the analysis. I plan to test the plausibility of the causality of the relationship by utilizing a natural experiment in the history of the program. Exogenous variation in the opening and closing of recruitment centers over space and time will be exploited to confirm the relationship with a different identification strategy and a different dataset. Namely, I will use a nationally representative survey to compare cohorts born in times and places where fathers had easy access to bracero contracts to cohorts born in times and places where fathers did not. This approach will extend the analysis to cover a larger portion of the population in Mexico and will be free from any bias in the coefficients from that arise from sibling-specific shocks correlated with both migration and human capital investment. Also, it will provide a much larger sample that should not suffer from the problems of statistical power that this sample does with so few families identifying the main effect.

All in all, this paper is an important step in better understanding how guest worker programs affect human capital development in the long run, telling a more complete history of migration between the United States and Mexico, and showing how migration policy can also make development policy when implemented correctly.

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Figures and Tables

Table 1 – Summary Statistics for the Entire Sample

Variable	Observations	Mean	Min	Max
Year Born	25,837	1965.3	1907	1998
Birth Order	25,837	4.0	1	18
Age at Migration	4,245	0.4	-46	62
Years of Education	25,837	8.2	0	23
Female	25,837	0.5	0	1
Employed	25,837	0.61	0	1
Agricultural Employee	25,837	0.12	0	1

Table 2 – Summary Statistics for "Treated" Families

Variable	Observations	Mean	Min	Max
Year Born	640	1958.5	1924	1980
Birth Order	640	4.6	1	13
Age at Migration	640	-2.8	-25	29
Years of Education	640	6.4	0	22
Female	640	0.5	0	1
Employed	640	0.6	0	1
Agricultural Employee	640	0.19	0	1

Table 3 – Regression Results for Years of Education

VARIABLES	(1) edyrs	(2) edyrs	(3) edyrs	(4) edyrs	(5) edyrs	(6) edyrs
notborn_bracero	-0.00982	0.0620	-0.0779	0.499*	0.756*	0.313
	(0.132)	(0.195)	(0.178)	(0.265)	(0.401)	(0.342)
notborn_undoc	0.109	0.148	0.0690	0.448*	0.928***	0.0544
	(0.125)	(0.185)	(0.171)	(0.244)	(0.303)	(0.343)
notborn_other	2.179***	2.071***	2.282***	0.134	0.573	-0.359
	(0.216)	(0.338)	(0.278)	(0.490)	(1.096)	(0.563)
birthorder	-0.111***	-0.111***	-0.110***	0.0802***	0.0811**	0.0721**
	(0.0117)	(0.0166)	(0.0164)	(0.0237)	(0.0329)	(0.0333)
youngest	0.886***	0.837***	0.947***	-0.0570	-0.0256	-0.00612
	(0.0802)	(0.114)	(0.113)	(0.0609)	(0.100)	(0.0915)
oldest	0.359***	0.348***	0.394***	0.435***	0.431***	0.389***
	(0.0793)	(0.114)	(0.111)	(0.0510)	(0.0832)	(0.0750)
female	-0.203***			-0.164***		
	(0.0502)			(0.0388)		
age	0.0297***	0.0312***	0.136***	-0.0439	-0.0461	1.382
	(0.00331)	(0.00482)	(0.0206)	(0.193)	(0.152)	(0.946)
agesq	2.87e-06***	3.02e-06***	-0.00153***	4.39e-06	4.61e-06	-0.000681*
	(3.30e-07)	(4.81e-07)	(0.000284)	(1.92e-05)	(1.52e-05)	(0.000404)
Constant	5.689***	-0.254	9.160***	11.02***	11.42***	8.031***
	(0.0646)	(0.331)	(0.325)	(0.988)	(0.789)	(1.351)
Observations	25,837	12,795	13,042	25,837	12,795	13,042
R-squared	0.095	0.082	0.114	0.060	0.051	0.097
Number of famid				5,058	4,587	4,480

Robust standard errors in parentheses; Birth year fixed effects included

*** p<0.01, ** p<0.05, * p<0.1

Table 4 – Regression Results for Employment

VARIABLES	(1) employed	(2) employed	(3) employed	(4) employed	(5) employed	(6) employed
notborn_bracero	0.0227*	0.0275***	0.0160	-0.00547	0.000159	0.0185
	(0.0122)	(0.0105)	(0.0222)	(0.0270)	(0.0243)	(0.0489)
notborn_undoc	-0.0127	0.0159	-0.0301	0.0422	0.0771**	0.0164
	(0.0127)	(0.0136)	(0.0204)	(0.0312)	(0.0315)	(0.0486)
notborn_other	0.0300	-0.00600	0.0638*	0.00599	-0.0623	-0.119
	(0.0228)	(0.0227)	(0.0374)	(0.0638)	(0.0927)	(0.0958)
birthorder	-0.00515***	0.00158	-0.0118***	-0.000764	-0.00429	-0.00186
	(0.00116)	(0.00128)	(0.00188)	(0.00313)	(0.00361)	(0.00513)
youngest	-0.00310	-0.0512***	0.0452***	-0.0212**	-0.0346***	-0.0130
	(0.00819)	(0.00927)	(0.0132)	(0.00932)	(0.0110)	(0.0158)
oldest	-0.00560	-0.0149**	0.00472	0.00494	-0.0177**	0.0176
	(0.00720)	(0.00711)	(0.0126)	(0.00718)	(0.00780)	(0.0133)
female	-0.611***			-0.617***		
	(0.00471)			(0.00641)		
age	0.00233***	0.00548***	0.0114***	0.0180	0.0369	-0.0953
	(0.000322)	(0.000333)	(0.00257)	(0.0449)	(0.0385)	(0.0893)
agesq	-2.32e-07***	-5.46e-07***	-0.000170***	-1.80e-06	-3.68e-06	-0.000188***
	(3.20e-08)	(3.31e-08)	(3.50e-05)	(4.48e-06)	(3.84e-06)	(6.32e-05)
Constant	0.968***	0.659***	0.819***	0.798***	0.914***	0.143
	(0.00637)	(0.0227)	(0.0404)	(0.0929)	(0.130)	(0.233)
Observations	25,837	12,795	13,042	25,837	12,795	13,042
R-squared	0.398	0.053	0.018	0.433	0.052	0.009
Number of famid				5,058	4,587	4,480

Robust standard errors in parentheses; Birth year fixed effects included.
*** p<0.01, ** p<0.05, * p<0.1

Table 5 – Regression Results for Agricultural Employment Outcomes

VARIABLES	(1) agemployed	(2) agemployed	(3) agemployed	(4) agemployed	(5) agemployed	(6) agemployed
notborn_bracero	0.0268	0.0304	0.0155	-0.00271	-0.0161	0.0345
	(0.0166)	(0.0214)	(0.0163)	(0.0397)	(0.0517)	(0.0534)
notborn_undoc	-0.00573	-0.0137	0.0139	-0.0399	-0.0344	0.0423
	(0.0171)	(0.0222)	(0.0160)	(0.0370)	(0.0471)	(0.0357)
notborn_other	-0.0925***	-0.131***	-0.00380	-0.110**	-0.127**	-0.0808
	(0.0205)	(0.0282)	(0.0165)	(0.0559)	(0.0566)	(0.106)
birthorder	0.000771	0.000402	0.00101	0.00334	0.00429	0.00388
	(0.00150)	(0.00196)	(0.00125)	(0.00324)	(0.00427)	(0.00272)
youngest	-0.0108	-0.00846	-0.0179**	-0.00126	0.00863	-0.000116
	(0.00970)	(0.0132)	(0.00729)	(0.00925)	(0.0129)	(0.00691)
oldest	-0.0192**	-0.0262**	-0.000916	-0.0222***	-0.0250**	0.00219
	(0.00914)	(0.0119)	(0.00795)	(0.00767)	(0.0105)	(0.00705)
female	-0.219***			-0.135***		
	(0.00481)			(0.00673)		
age	0.00126***	0.00149**	-0.000561	-0.0164	-0.0118	-0.00678
	(0.000450)	(0.000580)	(0.00234)	(0.0211)	(0.0171)	(0.0467)
agesq	-1.28e-07***	-1.51e-07***	1.65e-05	1.64e-06	1.17e-06	3.57e-05
	(4.49e-08)	(5.78e-08)	(3.49e-05)	(2.10e-06)	(1.70e-06)	(3.97e-05)
Constant	0.937***	0.929***	0.00440	0.998***	1.154***	0.106
	(0.0306)	(0.0395)	(0.0364)	(0.109)	(0.106)	(0.291)
Observations	15,788	11,766	4,022	15,788	11,766	4,022
R-squared	0.072	0.016	0.035	0.053	0.016	0.066
Number of famid				4,686	4,378	2,381

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6 – Education Regression by Exposure Bins

VARIABLES	(1) edyrs	(2) edyrs	(3) edyrs	(4) edyrs	(5) edyrs	(6) edyrs
notborn_bracero	-0.0226	0.0463	-0.0886	0.948	0.259	0.785
	(0.132)	(0.196)	(0.178)	(0.828)	(0.924)	(1.173)
age0to5_bracero	-0.960***	-1.021**	-0.975**	0.467	-0.593	0.534
	(0.285)	(0.416)	(0.384)	(0.817)	(0.911)	(1.152)
age6to11_bracero	-1.081***	-0.974*	-1.182**	0.462	-0.140	0.311
	(0.361)	(0.564)	(0.471)	(0.767)	(0.883)	(1.136)
age12to17_bracero	-0.930***	-1.252*	-0.734**	-0.251	-0.803	-0.409
	(0.314)	(0.731)	(0.349)	(0.663)	(0.917)	(0.845)
notborn_undoc	0.0941	0.131	0.0595	0.884**	1.632***	0.649
	(0.126)	(0.185)	(0.171)	(0.432)	(0.590)	(0.615)
age0to5_undoc	-0.827***	-1.032***	-0.620**	0.420	0.770	0.577
	(0.185)	(0.265)	(0.261)	(0.333)	(0.533)	(0.496)
age6to11_undoc	-0.279	-0.111	-0.481	0.448	0.556	0.546
	(0.208)	(0.289)	(0.300)	(0.284)	(0.432)	(0.453)
age12to17_undoc	-0.256	-0.494	-0.0247	0.455**	0.0550	0.868***
	(0.229)	(0.349)	(0.300)	(0.200)	(0.325)	(0.289)
notborn_other	2.164***	2.055***	2.270***	1.651	3.795**	0.728
	(0.216)	(0.338)	(0.278)	(1.246)	(1.870)	(1.650)
age0to5_other	1.978***	1.767***	2.243***	1.638	3.324**	1.235
	(0.405)	(0.601)	(0.547)	(1.181)	(1.455)	(1.680)
age6to11_other	1.437***	1.216*	1.727**	0.888	2.779*	0.412
	(0.485)	(0.667)	(0.706)	(1.096)	(1.468)	(1.233)
age12to17_other	2.370***	2.787***	1.910**	1.091	2.631**	0.404
	(0.548)	(0.761)	(0.761)	(0.947)	(1.282)	(1.165)
birthorder	-0.110***	-0.112***	-0.109***	0.0800***	0.0798**	0.0728**
	(0.0116)	(0.0166)	(0.0164)	(0.0238)	(0.0330)	(0.0335)
youngest	0.883***	0.838***	0.943***	-0.0546	-0.0241	-0.00319
	(0.0803)	(0.114)	(0.113)	(0.0610)	(0.100)	(0.0914)
oldest	0.359***	0.341***	0.401***	0.437***	0.433***	0.389***
	(0.0792)	(0.113)	(0.111)	(0.0509)	(0.0832)	(0.0749)
female	-0.202***			-0.164***		
	(0.0501)			(0.0388)		
age	0.0293***	0.0308***	0.139***	-0.0418	-0.0430	1.374
	(0.00332)	(0.00484)	(0.0206)	(0.192)	(0.154)	(0.941)
agesq	-2.83e-06***	-2.98e-06***	-0.00157***	4.18e-06	4.30e-06	-0.000680*
	(3.31e-07)	(4.82e-07)	(0.000284)	(1.92e-05)	(1.53e-05)	(0.000407)
Constant	5.694***	-0.217	9.128***	10.89***	11.34***	7.923***
	(0.0649)	(0.332)	(0.325)	(0.998)	(0.799)	(1.353)
Observations	25,837	12,795	13,042	25,837	12,795	13,042
R-squared	0.098	0.085	0.117	0.060	0.053	0.098
Number of famid				5,058	4,587	4,480

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7 – Employment Regression Results by Exposure Bins

VARIABLES	(1) employed	(2) employed	(3) employed	(4) employed	(5) employed	(6) employed
notborn_bracero	0.0228*	0.0272***	0.0165	-0.110	-0.432***	-0.00388
	(0.0122)	(0.0105)	(0.0222)	(0.0907)	(0.143)	(0.107)
age0to5_bracero	0.0103	0.0302***	-0.0244	-0.102	-0.439***	-0.0106
	(0.0222)	(0.0103)	(0.0480)	(0.0912)	(0.144)	(0.112)
age6to11_bracero	-0.0261	0.0107	-0.0716	-0.0974	-0.395***	-0.0440
	(0.0358)	(0.0283)	(0.0639)	(0.0890)	(0.144)	(0.100)
age12to17_bracero	-0.178***	-0.288**	-0.0965*	-0.178***	-0.602***	-0.138**
	(0.0642)	(0.125)	(0.0585)	(0.0453)	(0.178)	(0.0564)
notborn_undoc	-0.0128	0.0155	-0.0295	-0.0466	-0.00673	-0.0642
	(0.0128)	(0.0136)	(0.0204)	(0.0626)	(0.0686)	(0.0936)
age0to5_undoc	-0.0284	-0.0222	-0.0334	-0.0892	-0.0952	-0.0862
	(0.0205)	(0.0237)	(0.0320)	(0.0558)	(0.0658)	(0.0807)
age6to11_undoc	-0.0133	0.0114	-0.0378	-0.0969**	-0.0594	-0.0751
	(0.0202)	(0.0207)	(0.0361)	(0.0489)	(0.0517)	(0.0753)
age12to17_undoc	0.0269	-0.000295	0.0559	-0.0205	-0.0196	-0.00691
	(0.0243)	(0.0259)	(0.0382)	(0.0420)	(0.0378)	(0.0596)
notborn_other	0.0296	-0.00658	0.0640*	-0.0142	-0.254	-0.229
	(0.0228)	(0.0227)	(0.0374)	(0.156)	(0.208)	(0.202)
age0to5_other	0.0126	-0.0620	0.104	-0.0362	-0.185	-0.156
	(0.0460)	(0.0537)	(0.0708)	(0.153)	(0.187)	(0.198)
age6to11_other	0.0988	-0.0481	0.280***	0.0696	-0.206	0.0788
	(0.0614)	(0.0607)	(0.0921)	(0.120)	(0.141)	(0.152)
age12to17_other	0.0408	-0.0162	0.112	-0.00511	-0.103	0.155
	(0.0526)	(0.0485)	(0.0965)	(0.105)	(0.0852)	(0.210)
birthorder	-0.00509***	0.00157	-0.0116***	-0.000507	-0.00443	-0.00137
	(0.00116)	(0.00128)	(0.00189)	(0.00314)	(0.00361)	(0.00515)
youngest	-0.00310	-0.0512***	0.0452***	-0.0216**	-0.0348***	-0.0131
	(0.00819)	(0.00927)	(0.0132)	(0.00932)	(0.0110)	(0.0159)
oldest	-0.00565	-0.0149**	0.00536	0.00466	-0.0182**	0.0178
	(0.00720)	(0.00711)	(0.0126)	(0.00718)	(0.00778)	(0.0133)
female	-0.610***			-0.617***		
	(0.00471)			(0.00640)		
age	0.00235***	0.00551***	0.0116***	0.0173	0.0366	-0.0951
	(0.000323)	(0.000334)	(0.00258)	(0.0443)	(0.0381)	(0.0891)
agesq	-2.34e-07***	-5.49e-07***	-0.000173***	-1.73e-06	-3.64e-06	-0.000181***
	(3.22e-08)	(3.33e-08)	(3.51e-05)	(4.41e-06)	(3.79e-06)	(6.38e-05)
Constant	0.968***	0.657***	0.815***	0.812***	0.950***	0.151
	(0.00640)	(0.0228)	(0.0405)	(0.0933)	(0.130)	(0.231)
Observations	25,837	12,795	13,042	25,837	12,795	13,042
R-squared	0.398	0.054	0.019	0.433	0.054	0.010
Number of famid				5,058	4,587	4,480

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8 – Agricultural Employment Regression Results by Exposure Bins

VARIABLES	(1) agemployed	(2) agemployed	(3) agemployed	(4) agemployed	(5) agemployed	(6) agemployed
notborn_bracero	0.0286*	0.0332	0.0145	0.300	0.257	0.0231
	(0.0166)	(0.0214)	(0.0164)	(0.333)	(0.410)	(0.0568)
age0to5_bracero	0.101**	0.121**	0.0124	0.279	0.248	-0.0116
	(0.0419)	(0.0489)	(0.0505)	(0.333)	(0.411)	(0.0364)
age6to11_bracero	0.134*	0.156*	-0.0399	0.412	0.387	-0.00461
	(0.0736)	(0.0830)	(0.0301)	(0.341)	(0.409)	(0.0218)
age12to17_bracero	0.276*	0.234	0.229	0.527*	0.437	
	(0.152)	(0.171)	(0.347)	(0.275)	(0.315)	
notborn_undoc	-0.00378	-0.0108	0.0128	-0.0316	-0.0553	0.0355
	(0.0171)	(0.0222)	(0.0160)	(0.0613)	(0.0921)	(0.0318)
age0to5_undoc	0.0211	0.0293	-0.00856	0.0234	-0.0191	-0.00917
	(0.0272)	(0.0364)	(0.0193)	(0.0550)	(0.0885)	(0.0154)
age6to11_undoc	0.0106	0.0233	-0.0245***	-0.0329	-0.0218	0.00176
	(0.0275)	(0.0348)	(0.00418)	(0.0406)	(0.0618)	(0.0106)
age12to17_undoc	0.0658**	0.114***	-0.0293***	-0.0219	-0.0492	-0.000591
	(0.0299)	(0.0440)	(0.00558)	(0.0322)	(0.0442)	(0.00944)
notborn_other	-0.0907***	-0.128***	-0.00481	-0.255**	-0.239	0.000603
	(0.0205)	(0.0283)	(0.0165)	(0.114)	(0.157)	(0.0442)
age0to5_other	-0.0515	-0.0959	0.0285	-0.176*	-0.158	0.0969
	(0.0436)	(0.0584)	(0.0540)	(0.0984)	(0.141)	(0.0936)
age6to11_other	0.00808	0.0401	-0.0415***	0.0249	0.0992	0.00758
	(0.0572)	(0.0909)	(0.0150)	(0.0459)	(0.0895)	(0.0106)
age12to17_other	-0.135***	-0.174***	-0.0217***	-0.0426	0.0107	0.0417
	(0.0352)	(0.0461)	(0.00572)	(0.0555)	(0.0743)	(0.0357)
birthorder	0.000802	0.000466	0.000924	0.00325	0.00439	0.00393
	(0.00150)	(0.00196)	(0.00125)	(0.00323)	(0.00427)	(0.00273)
youngest	-0.0106	-0.00817	-0.0184**	-0.00126	0.00889	-1.82e-05
	(0.00971)	(0.0132)	(0.00731)	(0.00925)	(0.0129)	(0.00696)
oldest	-0.0198**	-0.0270**	-0.00149	-0.0227***	-0.0256**	0.00241
	(0.00913)	(0.0119)	(0.00795)	(0.00767)	(0.0105)	(0.00712)
female	-0.219***			-0.134***		
	(0.00482)			(0.00672)		
age	0.00127***	0.00154***	-0.000638	-0.0162	-0.0119	-0.00789
	(0.000451)	(0.000581)	(0.00234)	(0.0205)	(0.0170)	(0.0469)
agesq	-1.28e-07***	-1.56e-07***	1.67e-05	1.61e-06	1.18e-06	3.82e-05
	(4.49e-08)	(5.79e-08)	(3.50e-05)	(2.05e-06)	(1.69e-06)	(4.06e-05)
Constant	0.937***	0.927***	0.00720	0.988***	1.141***	0.114
	(0.0306)	(0.0395)	(0.0366)	(0.111)	(0.109)	(0.293)
Observations	15,788	11,766	4,022	15,788	11,766	4,022
R-squared	0.074	0.018	0.036	0.055	0.018	0.067
Number of famid				4,686	4,378	2,381

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1