UC Riverside
UC Riverside Electronic Theses and Dissertations

**Title**
Three Essays in Applied Microeconomics: Evaluating Pathways to Improving the Human Condition

**Permalink**
https://escholarship.org/uc/item/7cp6x45g

**Author**
Sarshar, Vilma Helena

**Publication Date**
2014

Peer reviewed|Thesis/dissertation

eScholarship.org Powered by the California Digital Library
University of California

UNIVERSITY OF CALIFORNIA
RIVERSIDE

# Three Essays in Applied Microeconomics: Evaluating Pathways to Improving the Human Condition

A Dissertation submitted in partial satisfaction
of the requirements for the degree of

Doctor of Philosophy

in

Economics

by

Vilma Helena Sarshar

December 2014

Dissertation Committee:
Dr. Mindy Marks, Co-Chairperson
Dr. Steven Helfand, Co-Chairperson
Dr. David Fairris

Copyright by
Vilma Helena Sarshar
2014

The Dissertation of Vilma Helena Sarshar is approved:

Committee Co-Chairperson

Committee Co-Chairperson

University of California, Riverside

**Acknowledgements**

I thank Mindy Marks and Steven Helfand for their exceptional advice and guidance and also for their patience. I thank David Fairris for his excellent feedback. I would also like to thank Todd Sorensen, without whom the first chapter in this dissertation would likely not exist. Thank you, as well, to Jorge Aguero, Susan Carter and Richard Sutch, and to all the professors at the economics department with whom I have worked and who are too many to mention. Thank you, also, to Gerd Sparovek of ESALQ and the Secretariat of Agrarian Restructuring (SRA) in the Ministry of Agrarian Development (SRA/MDA) of the Brazilian Government for access to the panel dataset used in Chapter 2. Thank you to Michael Carter and USAID BASIS for the grant with which the data used in Chapter 3 was obtained. And my greatest thank you goes to the interviewed families in Northeastern Brazil for their hospitality and friendship.

This dissertation is dedicated to my loving parents, Mara and Larry Ybarrondo, who taught me to persevere in the face of life's challenges and without whose help none of this would be possible, and to my dear husband, Mohammad, soothing and loving pair, for your constant support and encouragement. Também está dedicado à minha linda tia Mariza, pelos brigadeiros deliciosos e conselhos sábios e sem dúvida, à minha vovó querida e preciosa, pelas orações e por seu amor sem fim. I thank my brothers and sister (yes, that's you, Karina), and Jackie Hubbard, Sheetal Bharat and Ana Sofia Bruzon for their advice, support and friendship. And thank you to all my amazing friends and family who have helped me through this most delectable adventure.

سپاسگزارم از خانواده ی گل ایرانیم.

ABSTRACT OF THE DISSERTATION

Three Essays in Applied Microeconomics:
Evaluating Pathways to Improving the Human Condition

by

Vilma Helena Sarshar

Doctor of Philosophy, Graduate Program in Economics
University of California, Riverside, December 2014
Dr. Mindy Marks, Co-Chairperson
Dr. Steven Helfand, Co-Chairperson

Chapter one examines the long-term impact of the state prohibitions of alcohol in the United States. Between 1851 and 1920, thirty-four states enacted statewide prohibitions of alcohol at different times. Making use of the variation in state prohibition as a natural experiment, the adult labor market and educational outcomes of cohorts exposed to state prohibition during the critical early development period is examined. Female cohorts exposed to alcohol-reduced environments during the critical interval from the prenatal period and up to three years of age are shown to have increased labor force participation and increased income in 1960. The results are mainly driven by exposure in the prenatal period. No results were found for male cohorts, which might be explained by the selective prenatal mortality of the frailest male cohort members unexposed to state prohibition. Chapter two provides an impact evaluation of the Brazilian National Land Credit Program. Making use of a panel dataset and a pipeline control group, the chapter evaluates the impact of the program on the outcome variables of agricultural production and earned income, using both difference in differences and individual fixed effects models. Because beneficiaries acquired land at different times, the heterogeneous effect of additional years of land ownership is investigated. The findings suggest that the program is successful in increasing beneficiaries' agricultural production and earned income, but only after four years of land ownership. Once the repayment of the loan is taken into consideration, however, the benefits of the program largely go to making debt payments and improving the net worth of the beneficiaries rather than to raising current household expenditures. Chapter three evaluates the impact of the Brazilian National Land Credit Program on the heights of beneficiary children. Making use of a family fixed effects model, the program is shown to significantly increase the height for age z-scores of beneficiary children exposed to parents' land ownership in the second, third or fourth year of life. The increases in height for age z-scores are likely attained through the mechanism of increased nutritional security that could result with the acquisition of land through the program.

## TABLE OF CONTENTS

## LIST OF FIGURES

## LIST OF TABLES

### Chapter 1 Tables



### Chapter 2 Tables

**Chapter 3 Tables**

## INTRODUCTION

The consumption of alcohol became a problem in the United States once increasing industrialization and urbanization lead to a collapse of traditional societal roles that encouraged moderation. Americans consumed the equivalent of two bottles of 80-proof liquor per capita per week by the beginning of the nineteenth century. Temperance movements emerged to encourage both a more stable familial environment and a more punctual and reliable workforce. By the mid-nineteenth century, campaigns for the prohibition of alcohol began in almost every state. Between 1851 and 1920, thirty-four states enacted prohibitions of alcohol at different times. While many scholars have evaluated the contemporaneous effects of prohibition, the first chapter of this dissertation is a unique evaluation of the long-term impact of the state prohibitions of alcohol.

Given the negative impact of alcohol on the early development of human life, this chapter evaluates whether cohorts exposed to state prohibitory regimes during their early development had improved adult labor market outcomes. Since randomization of parental alcohol use is impossible and unethical, the variation in state prohibitions of alcohol can serve as a natural experiment to evaluate the causal impact of parents' drinking on the quality of early development environments. While the argument could be made that prohibition and non-prohibition states were very different, the states were all shown to be following parallel trends.

The early childhood development hypothesis in the economics literature purports that adult outcomes are determined, in part, by events in early childhood. Even more concretely, the fetal origins hypothesis in the biology literature states that diseases in later life are programmed according to environmental shocks *in utero.* Because of these well-established links between early development and adult outcomes, the impact of spending early development under prohibition was measured by assessing total income, labor force participation and years of schooling using the 1960 U.S. Census and cohorts born from 1905 to the fourth quarter of 1916.

In accordance to the neuroscience literature, the critical period of early development was defined as the *in utero* period and the first three years of life. Prohibition exposure was then defined as the proportion of each cohorts' early development period that was spent under state prohibition. Using a difference in differences methodology, the positive effect of the state prohibitions of alcohol on early development environments was shown. Female cohorts fully exposed to state prohibition during early development saw an increase in total income in 1960 by eleven percent, and an increase in labor force participation by eight percent. These results for females were significant at the ten and five percent levels, both calculated using bootstrapped standard errors in order to ensure the results were not being caused by serial correlation in the outcome variables. The results were robust to different specifications of the critical early development period, to different delineations of cohorts included in the sample, and to the exclusion of states that enacted prohibition before the twentieth century.

Neither result was robust, however, to the inclusion of state specific time trends. Nevertheless, a more flexible model was estimated with indicator variables that represented having spent at least one quarter under state prohibition for different intervals of early development. While the total income result was driven by exposure *in utero* and exposure from one to two years of age, the results were again not robust to the inclusion of the state specific time trends. The result for labor force participation was driven by exposure *in utero* and the effect was robust to the inclusion of the state specific time trends. While the inclusion of the trends could capture omitted variables that could be causing the increases in total income and labor force participation, they could also be absorbing all of the variation in the outcome variable and exacerbating attenuation bias.

No effects were found for male cohorts from exposure to state prohibition in early development. Since effects for females were mostly being driven by exposure to state prohibition *in utero*, the absence of significant effects for males could be explained by the culled cohort hypothesis. A confirmed evolutionary mechanism exists that causes pregnant females to selectively and spontaneously abort frail male fetuses when under stress, since the frail male fetus would be less likely to eventually reproduce than a strong male fetus, or a weak or strong female fetus. Under the likely assumption that pregnant women in non-prohibition states were subject to more environmental stress than pregnant women in prohibition states, on average, male cohorts from wet states would be stronger than male cohorts from dry states. Thus, the positive effects of better early development environments with prohibition exposure would be unobserved at the cohort level.

Since female cohorts were not subject to the same evolutionary mechanism, the positive effects of exposure to state prohibitions during early development were clear. The mechanisms by which exposure to state prohibition could cause improved environments are varied. Decreasing parental alcohol use directly impacted prenatal environments by decreasing the instances of Fetal Alcohol Exposure that occurred with maternal drinking. Postnatally, children's conditions would be improved indirectly through environmental changes due to decreased drinking by either parent. Less drinking by parents would provide a better household environment (decreased domestic violence and parents with unimpaired judgment), together with a positive income shock as household resources would no longer be used for purchasing alcohol. Another mechanism, actually shown in this chapter, is that state prohibition decreased the crude birth rate. A decreased crude birth rate would imply that unintended pregnancies were decreased—a result that would lead to better planned and more nurtured cohorts.

In order to ensure that the positive results found for female cohorts were not spurious, a placebo difference in differences estimation was run. State prohibitions were coded to have fictitiously started ten years after their actual start dates. The 1970 U.S. Census was used and the sample included in the estimation was cohorts born from 1915 to those born in the fourth quarter of 1926 (the original sample delineation plus ten years). As expected, no significant effects were found for either male or female cohorts from this fictitious prohibition exposure. The insignificance of the results in the placebo test suggested that the significant effects found for females in the true estimation were most likely due to prohibition exposure in early childhood, and not omitted variables or differences in birth state specific time trends. While this chapter does not recommend a return to the prohibition of alcohol, the policy implication is clear—reducing parental alcohol use while a child is in early development will lead to long-term positive impacts on the child's future labor market outcomes.

The second chapter of this dissertation pertains to the alleviation of rural poverty. The world is home to over two billion human beings living in a state of deep deprivation, most of them in rural areas. Policies facilitating access to land, accompanied by investments in productive infrastructure, access to credit, technology and markets, can potentially assist these poor rural households to develop and sustain a non-poor standard of living. One such policy initiative is the poverty alleviation line (*Combate à Pobreza Rural*, CPR) of the Brazilian National Land Credit Program (*Programa Nacional de Crédito Fundiário*, PNCF). The PNCF-CPR supplies loans for the purchase of land from willing sellers in addition to grants for infrastructure, technical assistance and loans for productive capital. The second chapter of this dissertation is an impact evaluation of the CPR line of the PNCF in the Northeast of Brazil on the outcome variables agricultural production and earned income, both at the household per capita level.

To initiate the procedure for participating in the PNCF, poor farmers formed into associations, verified eligibility of association members, and were then enrolled in the program. With the help of technical assistants, they created a productive project for a property intended for purchase. Based on the eligibility of the land and the viability of the productive project, the loan was approved or denied. When it was approved, the enrolled members of the association became beneficiaries of the PNCF. When the loan was denied, either a new productive project was created or a new property was sought out for purchase, and the enrolled members of the association continued in the pipeline of the program. Once the loan was approved and the land acquired, beneficiaries had a twenty-four month grace period to commence repayment of the loan, and depending on the principal, had either fourteen or seventeen years to repay.

The data used to perform this impact evaluation was a panel dataset acquired in 2006 and 2010, of randomly selected beneficiaries of the PNCF and, as a control group, randomly selected enrolled members of the PNCF's pipeline. The control group, or pipeline *non*-beneficiary (PNB) group, was selected from the same or neighboring municipalities as the beneficiaries. In order to take advantage of the panel nature of the data, the main analysis in this chapter exclusively used households that were interviewed in both periods. Between the first and second period, two important changes took place. First, forty-two percent of the sample was lost due to attrition. Second, forty-four percent of the PNB group acquired land and became beneficiaries.

A battery of tests was performed in order to ascertain whether the patterns to attrition were random. The findings suggested that nonrandom patterns to the attrition did exist. The patterns identified in the PNB group indicated that the PNB members with higher-than-average outcome variables were attriting from the sample, while in the beneficiary group the opposite pattern was observed. If the control group appeared to be stronger in terms of outcome variables than it counterfactually would have been without attrition and the treatment group appeared to be weaker than it counterfactually would have been, then the attrition biased the estimates of the impact of treatment downward. In order to correct for any nonrandom patterns to attrition, attrition weights were used. The attrition weights are the inverse probability of retention and their use ensures a higher weight to individuals in the balanced panel that were similar to attritors, thus making the balanced panel appear more similar to the original sample. Once the attrition weights were used, the results of the impact evaluation were only strengthened.

With respect to the movement of associations from the PNB group to the beneficiary group, probit models and mean comparisons provided suggestive evidence that no patterns to the movement existed, thus confirming the validity of the use of the pipeline control group. Using this pipeline control group, and thereby ensuring that application to the program was constant across treatment and control groups, multiple identification strategies were used. The first strategy was to use a difference in differences model that captured any time-invariant differences between the treatment and control groups, and also controlled for time-trends that were common to both groups. Taking the difference in differences methodology a step further, individual fixed effects were introduced in order to net out any time-invariant unobserved characteristics at the individual level. Both the difference in differences and individual fixed effects methodologies were used with a binary treatment variable that indicated an individual had acquired land at any time before 2010. In addition, because different beneficiaries were acquiring land at different times, indicator variables for the number of years of land ownership were included as a substitute to the binary treatment variable. The preferred specification of this chapter was the individual fixed effects estimation using the indicator variables for the number of years of land ownership, grouped in three or less years of ownership, four years of ownership and five to six years of ownership.

The results for the preferred specification indicated that, given enough years on the land, beneficiaries had increased values of agricultural production and earned income.

Agricultural production was defined as the total value of agricultural production (including animal production), whether sold, stocked, exchanged or consumed and earned income was defined as the value of net agricultural production—total agricultural production minus variable costs—plus income earned in the labor market and from self employment activities. With four years of land ownership, beneficiaries increased their agricultural production by 47 percent when compared to the baseline average for beneficiaries, significant at the five percent level. Beneficiaries with five to six years of land ownership increased their agricultural production by 102 percent, significant at the one percent level, and their earned income by 35 percent, significant at the five percent level. No significant effects were observed for beneficiaries with three or less years of land ownership for either outcome variable nor for beneficiaries with four years of land ownership for earned income. These findings suggest that beneficiaries required sufficient time on their land to realize adequate returns on their investments.

Because of the amount of time required for beneficiaries to significantly increase earned income, the repayment of the debt was a burden to beneficiaries in the early years. According to the rules stipulated in the operational manual of the program and the principal of each beneficiary, the installment each beneficiary was required to pay in 2010 was calculated. Once the installments were subtracted from the earned income of beneficiaries in 2010, beneficiaries of the program were statistically worse off than the pipeline non-beneficiaries of the program. The beneficiaries that paid their installments were forgoing increases in current consumption in order to increase the household's long-term net wealth. While such a strategy is well known to the middle class, for individuals living on approximately two dollars per person per day, such a sacrifice is overly detrimental to the households' welfare. The policy implications are that the grace period should be extended and the loans amortized over a greater number of years. Given such changes, the PNCF could serve as a model for rural poverty reduction policy in other areas across the globe.

The third chapter takes the impact evaluation from the second chapter a step further by analyzing the effect of the PNCF-CPR on the height-for-age z-scores (HAZ) of beneficiary children in the Northeast of Brazil. The early childhood development hypothesis predicts that events in early development will partially determine adult outcomes. If the children of the rural poor sustain sufficient negative shocks in early development, the intergenerational transmission of poverty will be more likely to occur. In order to evaluate beneficiary children's early development environments, the best ex-post estimate of childhood welfare is the child's HAZ. Measured HAZ at any age will show a deficit if there was a period of growth retardation due to poor nutrition during the earliest years because it is difficult to compensate for early growth failure. Stunting, or a HAZ of less than negative two, is highly predictive of low IQ and low cognitive and educational performance.

Research estimating correlations between children's HAZ and program effects has the potential to be confounded due to the existence of omitted variables at the family level. Because of this potential for biased findings, the preferred methodological approach in this chapter was a family fixed effects model, which eliminated time invariant characteristics at the family level. In order to estimate the family fixed effects model, the author measured the heights of 531 siblings in 211 families of both beneficiary and pipeline non-beneficiary status. While the heights of some families identified as having two or more children under the age of ten were not obtained, they were missing for a wide variety of reasons. These varied reasons together with probit models that attempted to predict which families were not measured lead to the conclusion that the height data was missing at random.

Children's HAZ was calculated using the average and standard deviations of height by month of age and gender obtained from the Brazilian Family Expenditures Survey (Pesquisa de Orçamentos Familiares) of 2008-2009. In addition to revealing early nutritional deficits, the HAZ also served to eliminate any upward time trend in the heights of children in Brazil. Siblings' exposure to their parents' land ownership was calculated as the proportion of quarters that the child spent under their parents' land ownership during the critical period for height growth. An estimation using indicator variables for age at land acquisition revealed that siblings were still benefitting from exposure to parents' land ownership at age three. Thus, the critical period was delineated as the prenatal period and up to age four. The relationship between exposure to parents' land ownership and HAZ was highly nonlinear and therefore a second order polynomial was used to estimate the impact of exposure.

Using the family fixed effects model and the sample of beneficiary children only, the impact of exposure to parents' land ownership was found to increase when proceeding from no exposure up to two years of exposure and to subsequently decrease for increases beyond two years of exposure. This result was likely influenced by the choice of the curvilinear functional form. To relax the limitations associated with specifying a functional form, the exposure variable and its squared term were substituted for a series of indicator variables for different levels of exposure. The first category was being exposed for more than zero but less than four quarters, the second was being exposed for four or more quarters but less than eight quarters, and so on, with the last category for exposure from sixteen to nineteen quarters—the last three quarters equivalent to being exposed in the prenatal period. In this specification, all levels of exposure to parents' land ownership led to increases in HAZ for beneficiary children. The effects were only significant, however, for children with more than zero but less than twelve quarters of exposure. Being exposed up to four quarters increased siblings' HAZ by 0.323, significant at one percent. Children in this category would have spent at least one quarter of their fourth year of life exposed. Being exposed for four to eight quarters, or spending at least one quarter of the third year of life exposed, increased HAZ by 0.281, significant at the five percent level. Exposure for eight to twelve quarters, or at least one quarter of the second year of life exposed, caused the greatest increase in HAZ at 0.351, significant at the one percent level.

Significant effects were not observed for children with more than twelve quarters of exposure. These children would have been exposed for at least one quarter *in utero* or at least one quarter in the first year of life. It is possible that the pregnant and lactating mother did not increase her caloric intake despite greater nutritional security after land acquisition and instead ensured the increased caloric intake of the older children that ate independently. The policy implications are twofold. First, information about the fetus' and nursing infants' nutritional welfare through better nutrition for pregnant and lactating mothers needs to be more widely disseminated in the rural Northeastern region of Brazil. Second, the PNCF succeeded in substantially improving the nutritional security of siblings exposed to parents' land ownership. Through predictions of improved adult outcomes emerging from positive nutritional shocks during the earliest years of development, fewer intergenerational transmissions of poverty will occur. This result predicts that the PNCF will have a long lasting and positive impact on future generations of beneficiaries of the program.

# CHAPTER 1:
# Under the Influence of Prohibition:
# An Analysis of the Adult Outcomes of Cohorts in Early Development During State Prohibitions of Alcohol in the United States

*Come Home Father*
*'tis the*
*Song of Little Mary*
*Standing at the bar-room door,*
*While the shameful midnight revel*
*Rages wildly as before.*
*Father, dear father, come home with me now,*
*The clock in the steeple strikes one;*
*You said you were coming right home from the shop*
*As soon as your day's work was done;*
*Our fire has gone out, our house is all dark,*
*And mother's been watching since tea,*
*With poor brother Benny so sick in her arms*
*And no one to help her but me,*
*Come home! come home! come home!*
*Please father, dear father, come home.*

-Henry Clay Work, 1864

## 1.1 INTRODUCTION

From the days of Plymouth Rock, alcoholic beverages have been consumed in the United States. Alcohol consumption continued without much controversy until after the Revolutionary War. Increasing industrialization and urbanization led to a collapse of traditional societal roles that encouraged moderation, leading to an increase in both the consumption of alcohol and the concerns over the consumption of alcohol (Hanson, 1995). The last half of the eighteenth century is considered to be the most intemperate era of American history (Sinclair, 1962). By the beginning of the nineteenth century, Americans consumed the equivalent of approximately two bottles of 80-proof liquor per capita per week (Okrent, 2010). Due to the concerns surrounding the need for a punctual and reliable workforce, in addition to women's desire for a more stable familial environment, a strong anti-alcohol sentiment emerged in the United States in the mid-nineteenth century (Porter, 1990). This sentiment grew into state and national movements and by the end of the nineteenth century, a full fledged campaign against alcohol was taking place.

Between 1851 and 1920, thirty-four states enacted statewide prohibitions of alcohol at different times. State-level prohibition substantially decreased the consumption of alcohol in the short term, as proxied by drunkenness arrests (Dills, Jacobson and Miron, 2004). With the persistent actions of the Anti-Saloon League, state prohibition closed 1800 saloons in Colorado and 900 in Oregon in 1916, 3500 saloons in Indiana and 3285 in Michigan in 1918, and so on. The success stories of prohibition states, together with local sentiment, led more and more states to enact prohibition—culminating finally in the enactment of National Prohibition in 1920. Since then, many scholars have analyzed the contemporaneous effects of the prohibition of alcohol.[1] This paper, on the other hand, is the first to evaluate the long-term impact of the prohibition of alcohol in the United States.

Making use of the variation in state prohibitions of alcohol, the adult outcomes of cohorts exposed to state prohibition during the critical early development period is examined. The critical early development period is defined as the *in utero* period and the first three years of life. Exposure to prohibition is defined as the proportion of the critical early period that is spent under state prohibition. A priori, there is reason to believe that exposure to state prohibition in early development would have a positive effect on adult outcomes. The fetal origins hypothesis suggests that chronic diseases later in life may be programmed in very early life, according to the environment. Recent research affirms that this hypothesis has graduated to accepted biology, albeit the specific details of the hypothesis remain controversial (Adair and Prentice, 2004). More generally, the early childhood development hypothesis states that adult outcomes are determined, in part, by events in early childhood (Nelson, 2000 and Almond and Currie, 2011). More favorable environments in early development will impact health and cognitive ability, which will, in turn, impact future outcomes (Card, 1999 and Currie and Madrian, 1999).

The mechanisms by which limited availability of alcohol improved adult outcomes of cohorts in early development during prohibition are varied. The child could

---

[1] Warburton (1932) presented a general overview of the economic effects of Prohibition, Dills, Jacobson and Miron (2004) examined the effect of prohibition on the consumption of alcohol, Levine and Reinarman (1991) drew lessons from alcohol prohibition in order to inform current drug policy, Timberlake (1963) analyzed prohibition and the progressive movement, Clark (1988) summarized the effects of prohibition and so on.

be impacted directly by reduced maternal drinking through reduced fetal alcohol syndrome, or indirectly through environmental changes due to decreased drinking by either parent. Less drinking by parents would provide a better household environment, together with a positive income shock as household resources would no longer be used for purchasing alcohol. In addition, less alcohol causes unintended pregnancies to decrease which leads to better planned cohorts. Using the US Decennial Census of 1960 and cohorts born from 1905 to the fourth quarter of 1916, a difference in differences approach yielded significant results. The effects are positive and significant for the variables of total income and labor force participation for female cohorts in early development during state prohibitions. While the results are highly robust to alternative specifications, the effects disappear when including birth state specific time trends.

The original contributions of this analysis using exposure to state prohibitions of alcohol to assess improvements in early development are varied. First, this paper provides a way of estimating the causal impact of parental alcohol use. The absence of an experimental design with respect to parental alcohol use requires that some form of exogenous variation exist, and the prohibition of alcohol enacted in different states at different times serves as a natural experiment. Second, as mentioned above, this paper is the first to analyze the long-term impact of the state prohibitions of alcohol. Third, while many other papers exist that investigate the early childhood development hypothesis, few do so with respect to substance abuse. This paper has important implications in health economic policy, including whether improving conditions in the early development period cause improved adult outcomes.

Section 1.2 of the paper provides background information on the state prohibitions of alcohol, the mechanisms by which prohibition could improve early development environments and provides a discussion on the critical early development period. Section 1.3 presents the empirical strategy, and section 1.4 discusses the data used and the descriptive statistics. The paper then proceeds with the results of the empirical estimation and robustness checks (Section 1.5 and 1.6). An additional estimation showing that state prohibitions decrease the crude birth rate is presented in Section 1.7 and the paper concludes in Section 1.8.

## 1.2 BACKGROUND

### 1.2.1 Prohibition

The temperance movement that eventually culminated in National Prohibition had its origins in the early nineteenth century (Cherrington, 1920). As early as 1833, the state of Georgia allowed a local option law by which two counties decided to enact prohibition. In 1843, the Territorial Legislature of Oregon enacted a general prohibitory law. Similar laws were adopted in Delaware, Maine, New Hampshire and Michigan, although not all proved successful. These very early adopters turned the attention of temperance advocates towards the possibility of state prohibitory amendments to state constitutions. In almost every state, campaigns for prohibition began in 1851, the year Maine's legislature first enacted prohibition.[2]

By 1893, thirty states and territories had at the very least voted on the idea of state prohibition. Only six states—Maine, Kansas, North Dakota, South Dakota, Vermont

[2] Maine's 1851 prohibition was repealed in 1856 and enacted again in 1857.

and New Hampshire actually succeeded. The partisan nature of the prohibition question was to blame for the failed attempts of other states. As such, in 1893 the Anti-Saloon League (ASL) emerged with the intention of creating a non-partisan, inter-denominational temperance movement. Having observed the repeated failures of prohibition without local enthusiasm, the ASL focused on a program intended to garner support. First, they worked locally to create a sentiment in favor of prohibition by encouraging people to voluntarily abstain from alcohol. Through pamphlets, newspapers and door-to-door campaigning, the ASL and other temperance organizations spread the sentiment that alcohol was harmful. Also assisting in creating the sentiment in favor of prohibition was Scientific Temperance Instruction. This “scientific instruction” was an effective strategy of the Women’s Christian Temperance Union (WCTU) that taught schoolchildren to abstain from drinking alcoholic beverages. It was present in all states by 1901, and began in Massachusetts in 1878 (Zimmerman, 1999). Following this local campaign to change public sentiment, the ASL proceeded to wait for the sentiment to crystalize into a majority public opinion. Only then would the final step be taken to pursue prohibitory legislation.

The visionaries of the ASL realized that state prohibition would only be realized with local support and therefore focused their attentions on achieving local option victories. By 1906, thirty states had local option laws by which townships, municipalities or counties could vote for local-level prohibition. Their campaign was so successful that seventy percent of all townships were under prohibition by local option, and over 1,500 counties. More than one third of the U.S. population was living under prohibition in

1906. Most prohibitory local option laws existed in rural areas where the ASL attacked liquor forces at the point of least resistance (Anti-Saloon League, 1914). This rural support was essential as it would be added to the less abundant urban support, and together, enough votes to enact state prohibition would be attained. While the ASL was many times responsible for state prohibition campaigns, depending on the political climate in some states, prohibition might actually have been enacted without the leadership of the ASL.

Oklahoma was the first state to enact prohibition in the twentieth century.[3] Both the WCTU and the ASL were very active in Oklahoma Territory in the decade preceding prohibition. The adoption of prohibition was facilitated by the already present prohibition of the distribution of intoxicating liquors in Indian Territory and also by the protestant population that supported the WCTU and ASL. Victory was achieved for the temperance movement when Oklahoma wrote prohibition into its new constitution (Franklin, 1971). The enactment in Oklahoma resulted in much agitation in all other states, creating frenzied campaigns for statewide prohibition. The order in which states enacted prohibition depended primarily on local sentiment and a myriad of other factors such as, whether the liquor business had strong ties in the community, racial tensions, willingness of local politicians, election dates, enactment dates, a willing judiciary, majority votes for prohibition and so on.[4]

---

[3] Although Georgia was the first state to adopt prohibition in the twentieth century, and Oklahoma second, Oklahoma's enactment of prohibition preceded the enactment in Georgia.
[4] The enactment dates, given the legislative adoption of state prohibition, could be anywhere from immediate to two years after adoption.

Table 1.1 summarizes the quarter and year of the adoption and enactment of state prohibition laws. In addition, the table indicates the method by which the state adopted prohibition—either through a referendum or through a statute passed in the state legislature. The penultimate column displays the mean exposure to state prohibition during early development for all cohorts born in each state. Exposure to state prohibition is measured as the proportion of the critical early period (the prenatal period and the first three years of life) spent under state prohibition. Thus, the mean displayed in Table 1.1 is the mean individual exposure of all people born in each state. The final column indicates the number of observations by state of birth. There were three waves of prohibition enactments—the first in the nineteenth century, the second from 1907 to 1909, and the third from 1914 to 1920. The states passing prohibition in the nineteenth century—and retaining that prohibition into the twentieth century—were Maine, Kansas, North Dakota and New Hampshire.[5] From 1907 to 1909, Georgia, Mississippi, North Carolina, Oklahoma and Tennessee enacted prohibition.

[5] New Hampshire repealed its 1855 prohibition in 1903, South Dakota repealed its 1889 prohibition in 1897, and Vermont repealed its 1853 prohibition in 1902. All states rescinded prohibition in favor of local option laws.

**Table 1.1: State Prohibition Dates, Enactment Types, Mean Exposure to Prohibition and Number of Observations by Birth State**

| State | Passed | Enacted | Type | Exposure | N |
|---|---|---|---|---|---|
| Alabama[†] | Q1 1915 | Q3 1915 | Statute | 0.23 | 4590 |
| Arizona | Q4 1914 | Q1 1915 | Referendum | 0.32 | 416 |
| Arkansas | Q1 1915 | Q1 1916 | Referendum | 0.18 | 3433 |
| Colorado | Q4 1914 | Q1 1916 | Referendum | 0.20 | 1227 |
| Florida | Q4 1918 | Q1 1919 | Referendum | 0.01 | 1537 |
| Georgia[a] | Q3 1907 | Q1 1908 | Statute | 0.89 | 5018 |
| Idaho | Q1 1915 | Q1 1916 | Referendum | 0.22 | 691 |
| Indiana | Q1 1917 | Q2 1918 | Statute | 0.04 | 4328 |
| Iowa | Q1 1915 | Q1 1916 | Statute | 0.19 | 3868 |
| Kansas | Q4 1880 | Q4 1880 | Referendum | 1.00 | 2927 |
| Kentucky[b] | Q4 1919 | Q1 1920 | Referendum | 0.00 | 4297 |
| Maine | Q2 1851 | Q2 1851 | Statute | 1.00 | 1081 |
| Michigan | Q4 1916 | Q2 1918 | Referendum | 0.04 | 4980 |
| Mississippi | Q1 1908 | Q1 1909 | Statute | 0.79 | 3547 |
| Montana | Q4 1916 | Q4 1918 | Referendum | 0.02 | 718 |
| Nebraska | Q4 1916 | Q2 1917 | Referendum | 0.08 | 2179 |
| Nevada | Q4 1918 | Q4 1918 | Referendum | 0.01 | 86 |
| New Hampshire[†] | Q2 1917 | Q2 1918 | Statute | 0.04 | 571 |
| New Mexico | Q4 1917 | Q4 1918 | Referendum | 0.01 | 535 |
| North Carolina | Q4 1908 | Q1 1909 | Referendum | 0.81 | 5048 |
| North Dakota | Q4 1889 | Q4 1889 | Referendum | 1.00 | 1344 |
| Ohio | Q4 1918 | Q2 1919 | Referendum | 0.01 | 7685 |
| Oklahoma | Q4 1907 | Q4 1907 | Referendum | 0.90 | 3561 |
| Oregon | Q4 1914 | Q1 1916 | Referendum | 0.21 | 959 |
| South Carolina | Q3 1915 | Q4 1915 | Referendum | 0.21 | 3045 |
| South Dakota[†] | Q4 1916 | Q3 1917 | Referendum | 0.07 | 1146 |
| Tennessee | Q1 1909 | Q3 1909 | Statute | 0.76 | 4370 |
| Texas[c] | Q2 1919 | Q2 1919 | Statute | 0.01 | 8561 |
| Utah | Q1 1917 | Q3 1917 | Referendum | 0.07 | 906 |
| Vermont[†] | Q4 1852 | Q4 1852 | Referendum | 0.00 | 515 |
| Virginia | Q3 1914 | Q4 1916 | Referendum | 0.12 | 3882 |
| Washington | Q4 1914 | Q1 1916 | Referendum | 0.18 | 1627 |
| West Virginia | Q4 1912 | Q3 1914 | Referendum | 0.34 | 2622 |
| Wyoming[d] | Q4 1918 | Q3 1919 | Referendum | 0.00 | 246 |

Notes: [†]Alabama's 1907 prohibition was repealed in 1911, New Hampshire was also under prohibition from 1855 to 1903, South Dakota was also under prohibition from 1889 to 1897 and Vermont repealed its 1852 prohibition in 1902. Most dates were obtained from Cherrington (1920a) and Pickett, Wilson and Smith (1917), and in the few cases that the Cherrington and Picket et al. dates did not coincide, then the Cherrington date was used. Other sources for dates missing from Cherrington (1920a) and Picket et al. (1917) were [a]New York Times (1907), [b]Cherrington (1920b), [c]Hazel (1942) and [d]Henley (1919).

Each of these early twentieth century enactors passed prohibitory legislation for a list of reasons so varied and through historical paths so unique, that no one explanation for why they each enacted suffices. For example, the impetus for Georgia to adopt prohibition by statute was the Atlanta Race Riot of 1906, where white mobs originated in bars and saloons and killed dozens of African Americans, wounded many more and caused significant property damage. Whites feared that unemployed African American saloon-goers were the cause of the rising crime rates (Crowe, 1968). In Tennessee, feelings between the wets and the drys had reached explosive heights by 1908 and the murder of the leader of the dry movement was the spark that created the victory for prohibition by public referendum in 1909 (Lacy, 1965). In Mississippi, the political environment was crucial to the prohibition campaign—only once the political support was present did the various local factions come together in a unified campaign that eventually achieved state prohibition. Prohibition in Mississippi was actually achieved 3 years before the ASL opened an office in the state (Szymanski, 2003).

In the last wave of prohibition enactment, from 1914 to 1920, a multitude of states enacted prohibition, many overcoming earlier failed attempts. These earlier attempts failed due to a variety of reasons. In some states, governors vetoed prohibition laws, such as in Alabama and Utah. In other states, despite a strong local sentiment, liquor forces were strongly influencing politicians, such as in Texas and Kentucky. Some state judiciaries declared the prohibition law unconstitutional, as in Texas in 1918. In many states, the prohibition question would be delayed by a lack of majority votes, again despite strong local sentiments, such as in Ohio, Missouri, Florida, Oregon, and Arkansas.

Lewis (2008) identified wet voter turnout as an important determinant in the passing of prohibition laws in the twenty-four out of thirty-three states that adopted prohibition through referenda. He posits that local option victories significantly contributed to a reduction in the presence of saloons. While dry voters had the ASL and other temperance organizations around which to rally, wet voters depended on the saloons as headquarters for activity against the temperance movement. Once saloons were abolished in many counties due to local option prohibition, there was no longer any force to rally wet voters and ensure they went to the polls. On the other hand, local option victories proved to dry voters that their efforts were not in vain and so dry voter turnout was secured.

With respect to the motivation behind prohibition, some scholars argue that state prohibition was simply an attempt by the native-born, pietistic rural population to impose sobriety on cultural groups that accepted drink as a part of normal life (Gusfield, 1967). Other scholars, however, viewed state prohibition as one of many progressive reforms that were an attempt by the middle class to feel politically relevant (Blocker, 1976). Others still argue that state prohibition advocates were concerned about the pragmatic effects of saloons and liquor—such as crime, political corruption and other social ills (Timberlake, 1963 and Clark, 1976). Perhaps all of these theories are correct, depending on local sentiments. Nonetheless, despite the motivation of temperance movement activists, the actual enactment of prohibition depended on wet and dry voter turnouts, local option victories, temperance movement activity, Scientific Temperance Instruction, liquor business ties in the community, willingness of local politicians and judiciaries, enactment dates and a variety of historical accidents.

Once states enacted prohibition, they reported merchants selling more goods, more accounts being paid, savings deposits increasing, more houses being purchased, and that the reduction of costs of police and court systems more than made up for lost taxes from saloons, even allowing the mayor of Charleston, for example, to decrease the tax rate. Washington reported a decrease in arrests for all causes by 46 percent, when comparing the last wet year to the first dry year, a result repeated in many other prohibition states, including substantially decreased number of homicides, suicides and wife whippings. Mining output in West Virginia was calculated to have increased by 111 percent comparing the three months before and three months after prohibition at the White Oak Coal Company, and the manager ascribes this increase to more productive workers due to less drink. Similar reports come from a multitude of states (Henley, 1919).

Perhaps due to the social success of state prohibitions, in January of 1920, the efforts of the ASL finally culminated in National Prohibition's enactment. According to Okrent (2010), National Prohibition was not well enforced. Because state prohibition provides more variation and was considered to be more binding, the sample is delineated in such a way as to exclude the variation emerging from the National Prohibition of alcohol, which is discussed more below. Despite the patterns of prohibition onset, after the Great Depression, it became evident that National Prohibition was not successful in eliminating the consumption and production of alcohol. The wet movement gained increasing support; wets argued that repealing prohibition would generate business in the private sector and taxes for the government. As a result, National Prohibition was rescinded through the 21$^{st}$ amendment to the constitution (Kyvig, 1979).

### 1.2.2 Mechanisms

The most direct mechanism through which prohibition would affect children in early development is through decreased fetal alcohol exposure (FAE) and therefore decreased fetal alcohol syndrome (FAS). Expectant mothers at the time did *not* know the negative impact of alcohol on their unborn children. It was only in 1973 that the connection between FAE and mental deficits was established (Jones and Smith, 1973). With state prohibition, expectant mothers would have less access to alcohol and their decreased consumption would, in turn, decrease occurrences of FAS. FAS is characterized by prenatal/postnatal growth retardation, central nervous system damage (mental retardation), and abnormal facial features. The fetus is affected by alcohol because embryonic cells that are destined to become brain neurons are increasingly susceptible to damage with alcohol metabolism. The metabolism of alcohol generates free radicals which can kill brain cells at critical times of development—the first trimester of a pregnancy. FAS and FAE are associated with children having difficulty with language and memory, difficulty discerning spatial relationships amongst objects, having problems paying attention, having slower, less efficient information processing, struggling with abstract thinking such as planning and organization, and finally, a diminished brain size and disproportionate reduction in specific brain structures (Eustace et al. 2006).

The specific amount of alcohol that would cause FAS is unknown. According to the National Institutes of Health, clinically significant deficits are not common in children whose mothers drank less than 5 drinks per occasion, once per week. However, a study in 2004 determined that ingestion of 2 cocktails per week causes nerve cell death in fetuses.[6] To be sure, the amount of alcohol required to produce adverse effects varies greatly from person to person. Thus, it is recommended that expectant mothers abstain entirely from drinking alcohol.

Besides the consequences to the fetus resulting from maternal drinking, the fetal environment can also be affected by paternal drinking. Similarly, the child's postnatal environment can be affected by paternal or maternal drinking. Drinking is associated with increased risk taking behavior, impaired judgment and response time, increased mortality, and increased domestic violence. Increased risk-taking behavior is evidenced by Carperter (2007), that concludes that heavy alcohol use causes increases in nuisance and property crimes amongst men aged 18-20, using variation from Zero Tolerance laws. Furthermore, in a meta-analysis of alcohol and crime, Carpenter and Dobkin (2011) determine that there is sufficient evidence to conclude a causal relationship between alcohol use and crime commission. The impaired judgment and decreased response time that follow alcohol consumption is best shown in assessing the impact of driving under the influence. Levitt and Porter (2001) find that any alcohol in the blood makes drivers

[6] Olney's findings were reported at the 2004 annual meeting of the American Association for the Advancement of Science (AAAS).

seven times more likely to cause a fatal crash while being legally drunk causes drivers to pose a risk 13 times greater than sober drivers.

With respect to increased mortality, using a regression discontinuity design around the legal drinking age, Carpenter and Dobkin (2009) find that a 10 percent increase in the number of drinking days for adults just turning 21 results in a 4.3 percent increase in mortality. In addition, alcohol use is a strong correlate of domestic violence. Markowitz (2000) uses panel data and individual fixed effects to show that a 1 percent increase in the price of an ounce of pure alcohol would decrease the probability of being a victim of severe wife abuse by 5.34 percent. Moreover, a 10 percent increase in beer tax reduced the likelihood of severe parental violence against children by 2.3 percentage points (Markowitz and Grossman, 2000). Increased risk-taking behaviors, impaired judgments and response times, and domestic violence could cause serious adverse shocks to sensitive early childhood environments.

Studies on the children of alcoholics show that they are reported to have higher rates of injury, poisoning and admissions to hospitals (Bijur et al., 1992). They oftentimes lack consistent caretaking, are physically and emotionally neglected and exposed to greater rates of violence and abuse (Weinberg, 1997). Exposure to parental alcoholism (both prenatally and postnatally) is associated with immune deficits that increase vulnerability to infectious diseases and cancer (Gottesfeld and Abel, 1990). While these studies focus on the extreme case of parental alcoholism, some of these negative effects are also likely to be observed with parental drinking.

In addition to improved early childhood environments, the onset of prohibition might also have altered household earnings. Multiple authors find that moderate drinking leads to a significant earnings premium and that heavy drinking leads to a significant earnings penalty (Barrett 2002, Kenkel and Ribar 1994, Hamilton and Hamilton 1997 and Renna, 2007). If prohibition at least *limited* the availability of alcohol, then occurrences of binge drinking would decrease. Besides the possible increase in earnings, with the onset of prohibition, less household resources would go towards the purchase of alcohol. These increased household resources might then be redistributed towards children and expectant mothers, thereby further improving early development environments.

A final mechanism by which prohibition could improve adult outcomes of cohorts in early development during the time is by generating more nurtured cohorts—by decreasing the number of unintended pregnancies. Chesson, Harrison and Kessler (2000) use a fixed effects model and find that sexually transmitted disease rates decrease with increases in alcohol taxes—a $1 increase in the per-gallon liquor tax reduces gonorrhea rates by 2.1 percent, and a beer tax increase of $.20 per six-pack reduces gonorrhea rates by 8.9 percent. They conclude that more restrictive alcohol policy reduces alcohol consumption, which in turn decreases risky sexual activity, a well-established hypothesis in the psychology literature. Before the advent of modern birth control methods, if risky sexual activity decreases with reduced alcohol consumption then unwanted pregnancies would also decrease. Indeed, Naimi et al. (2003) find that binge drinking is associated with an increase in unintended pregnancies.

If the state prohibitions of alcohol were binding, even if imperfectly, many negative effects to cohorts in early development at the time would have been reduced. The decreased FAS, decreased risky parental behavior, increased instances of parents with unimpaired judgment, decreased domestic violence, increased and redistributed household resources and more planned cohorts would all have contributed to improving early development environments.

### 1.2.3 Critical Early Development

The literature on early childhood development suggests that *in utero* and early childhood environments impact adult outcomes. A review of the neuroscience literature suggests that the most important period for the development of the human brain is the *in utero* period and the first three years of life.[7] The vast majority of neurons are present by the seventh month of gestation. A consensus exists that the appearance of the brain structure is similar to that of adults by two years of age and all major fiber tracts can be observed by three years of age. Although the prefrontal cortex continues to develop into adolescence, the peak of its development takes place at two to three years of life. As such, any shocks to the developmental process of the brain during this critical period will impact future outcomes. Developmental biologists make the value of investments in early childhood most clear—Wadington (1957) describes human development as proceeding along the branches of a tree. Although changes in the developmental trajectory can take place with the sprouting of a new branch, substantially altering the course of development becomes more and more difficult with the passing of time. As such, programs that invest

[7] See Baars and Gage (2007), Casey et al. (2005), Huttenlocher and Dabholkar (1997), Bourgeois (2001), and Shaw et al. (2006)

in the earliest periods of life are likely to alter the main branch from which an individual develops.

Many papers exist that evaluate impacts of positive and negative shocks during early development. Focusing on the *in utero* environment, Black et al. (2007) and Behrman and Rosenzweig (2004) use twins' data to show that increasing birth weight leads to improved future outcomes. Almond (2006) and Almond, Edlund and Palme (2007) both show that large-scale negative environmental shocks *in utero* significantly worsen labor market and educational outcomes. Nilsson (2008) finds that adults *in utero* during a period of laxer beer sale laws in Sweden had worsened labor market and educational outcomes.

With respect to the postnatal environment, Bleakley (2007) shows that being infected with hookworm during childhood decreases adult wages and returns to schooling. Another paper finds that having health problems or conduct disorders in early childhood is associated with an increased likelihood of being on welfare and a decreased likelihood of completing grade 12 for adults (Currie et al., 2009). Currie and Widom (2009) show that adults that were maltreated as children have less years of schooling, lower IQ, less earnings, and a lower likelihood of being employed, having a skilled job or owning a vehicle. A different paper by Currie and Tekin (2006) shows that adults that are maltreated as children are significantly more likely to commit crimes. These papers focusing on the long run impacts of shocks to early development, together with the neuroscience literature, provide very strong evidence that early development does indeed have a lasting impact on future outcomes.

### 1.3 EMPIRICAL STRATEGY

This paper aims to estimate the long-term impact of being exposed to state prohibitions of alcohol during early development. Even if imperfectly, state prohibitions of alcohol limited the availability of the substance and as such altered the home and uterine environments for cohorts in early development at the time. Research estimating correlations between early health and adult outcomes has the potential to be thoroughly confounded due to the existence of omitted variables. There can be an array of omitted variables, such as a nurturing family, educated parents, or wealthy parents that could be causing an overestimation of the impact of healthy early environments on adult outcomes. For example, a nurturing family would provide a healthy *in utero* and early childhood environment, and investments in health, schooling and so on. This would contribute positively to adult outcomes. When simply estimating an Ordinary Least Squares regression in the presence of these omitted variables, the impact of the early environments on adult outcomes would be biased upward. Thus, some form of exogenous variation would be ideal to assist in eliminating the presence of omitted variables. The variation occurring at the state and year level with the enactments of state prohibitions between 1851 and 1920 creates an excellent natural experiment to evaluate the impact of altered *in utero* and early childhood environments on adult outcomes.

Since different states adopted state prohibition at different times, the preferred methodology is a difference in differences model. The difference in differences approach exploits cross-state variation in prohibition to avoid attributing to prohibition influences from unmeasured variables common to all observations from a particular birthplace or cohort. This technique would entail estimating the following equation:

$$outcome_{ics}=\beta prohibitionexposure_{cs}+\delta stateborn_{s}+\gamma yearborn_{c}+\lambda X_{ics}+\theta trend_{cs}+\varepsilon_{ics} \qquad (1.1)$$

where i indexes individual, s indexes state of birth and c indexes cohort. The outcome variables analyzed are total income, labor force participation and years of schooling, all defined in detail in Section 1.4 below. The *prohibitionexposure* variable measures the exposure to prohibition during the critical early development quarters (those from the prenatal period and the first three years of life, for a total of 15). It is defined as the number of quarters exposed, which varies by cohort and state of birth, divided by the number of critical quarters. *Stateborn* is a fixed effect for state of birth and *yearborn* is a cohort fixed effect. The inclusion of the fixed effects for state of birth captures any variation in the outcome variables caused by time invariant characteristics of individuals born in the same state. The year of birth or cohort fixed effects, on the other hand, captures the variation in the outcome variables caused by a common time trend to which individuals born in all states were exposed.

*X* are control variables—race, quarter of birth, current state of residence in 1960 and a dummy variable for being conceived in a state-year where women could vote. Race and quarter of birth are included in order to capture any differences in adult outcomes due to individual characteristics. State of residence in 1960 is included in order to capture any variation in the outcome variables due to the current state of residence. The dummy variable for suffrage exposure, for being conceived after women's suffrage, is included since the suffrage movement existed concurrently with the temperance movement and any changes in the outcome variables due to exposure to women's suffrage should be controlled for. This variable is included to mitigate any effects on the outcome variables coming from the empowerment of women that occurred with women's suffrage.

The final variable included, *trend*, is a birth state specific time trend. When including these trends, the identification of the effect of prohibition exposure comes from whether the exposure itself led to deviations from the trend. For a variety of reasons discussed below, there are advantages and disadvantages to including the trends. The regressions are estimated separately for males and females and sample weights are used in every specification. Standard errors are clustered at the level of state of birth, and in any case where a significant effect was observed, standard errors were then bootstrapped in order to correct for potential downward biases due to serial correlation in the outcome variables (Bertrand et al., 2004).[8]

The period of development defined as the critical early development quarters, for the purposes of calculating the *prohibitionexposure* variable, are the twelve quarters after birth and the three quarters before birth, for a total of fifteen quarters.[9] In an attempt to delineate the treatment and control groups more precisely, two considerations were made. The first is that quarter of birth and quarter of enactment of state prohibition must be taken into account to reduce the measurement error associated with inaccurately ascribing treatment to the untreated and vice versa (as opposed to the less precise technique of using enactment and birth *years* without taking the quarter into consideration). The

[8] Standard errors were bootstrapped using 500 repetitions.
[9] In Section 1.6, a robustness check is performed by defining the critical period in different ways. The positive and significant results seen below are robust to alternative specifications of the critical early development quarters.

second is that different individuals may have had different potential exposures to alcohol-free environments during the critical period of early childhood, which motivates the use of an intensity of treatment approach.

This approach demarcates three categories of the intensity of treatment. The first category is being completely unexposed to state prohibition. This category includes cohorts in a state without state prohibition and cohorts in states with state prohibition, but that completed three years of age before the enactment of state prohibition. In this case, the *prohibitionexposure* variable takes the value of zero. The second category, being partially exposed to state prohibition, entails having been exposed for at least one quarter, but less than 15 quarters, during this critical period. This would be the case for cohorts that were conceived sometime before the enactment of state prohibition, but completed three years of age after the enactment. For these partially exposed cohorts, the *prohibitionexposure* variable assumes a value between zero and one. For the last category of the intensity of treatment, the *prohibitionexposure* variable assumes the value of one, and the cohort was fully exposed during the critical *in utero* and early childhood quarters. Being conceived concurrent with or after the quarter of enactment of state prohibition means an individual was fully exposed. The use of the *prohibitionexposure* variable, defined in this way, makes the tacit assumption that the effects of exposure to an alcohol-free environment are constant throughout the critical quarters.

In order to allow for the effects of exposure to vary for different intervals during the early development period, a more flexible approach is also taken. Instead of defining exposure as the proportion of critical quarters spent under prohibition, a series of indicator variables are used. This approach entails substituting the *prohibitionexposure* variable above by indicator variables for exposure at different intervals in early development. The first indicator variable used equals one if the cohort was exposed for at least one quarter to state prohibition during the three quarters of the *in utero* period. The second variable represents having been exposed for at least one quarter during the first year of life—from birth up to completing one year of age. The third indicator variable is for having been exposed for at least one quarter from one year of age up to completing two years of age, and so on, up to the completion of five years of age. All other outcome, control and fixed effects variables remain the same as in the equation above.

### 1.4 DATA AND DESCRIPTIVE STATISTICS

The data used for this estimation is the United States Decennial Census of 1960, available from IPUMS. The 1960 census was chosen because it is the first census to include a variable indicating quarter of birth, which allows for a more precise mapping of year of birth, using age. In addition to being the first U.S. census to include quarter of birth, cohorts that went through the period of early development during state prohibitions of alcohol were of prime working age during the 1960s; they were 43 to 54 years of age. Using the 1970 census and onwards could be problematic due to selective mortality of the most feeble members of the cohort. Lastly, it is possible to precisely map where individuals were born using the variable of place of birth.[10]

[10] Only individuals born in the 48 United States at the time are kept in the sample, both foreign born and those with state of birth unspecified are dropped, in addition to the exclusion of Washington DC.

Individuals' year of birth can be unambiguously defined as the age subtracted from 1960 for quarter one births, and the age minus one subtracted from 1960 for births in quarters three and four. For those born in quarter two, the year of birth cannot be unambiguously identified. Because the census is recorded in April, it is impossible to know whether an individual with a quarter two birth had already had his or her birthday in 1960. As such, these ambiguous individuals born in quarter two are dropped from the estimation. The year and quarter of conception of the remaining sample is defined to be three quarters previous to the year and quarter of birth.

The cohorts included in the analysis are those born from 1905 in quarter three to individuals born in 1916 in quarter four.[11] Quarter three of 1905 is chosen as the first cohort because it is the first partially exposed cohort observed in the data; the first state prohibition of the 20th century was enacted in quarter four of 1907 (in Oklahoma). 1916 quarter four is chosen as the last cohort in order to include only the variation from state prohibition. This last cohort completes three years of age before the onset of National Prohibition. The variation from National Prohibition is excluded since it was not binding in states where state prohibition had not already been in place (Okrent, 2010). Figure 1.1 displays the proportion of cohorts exposed to state prohibition by year of birth, for the limited cohorts in the analysis. Because it is not known exactly which families changed their behavior due to prohibition, this is an "intent-to-treat" approach.

---

[11] In Section 1.6, robustness checks are performed by adding and subtracting cohorts included at either cutoff. The results shown below are mostly robust to different delineations of the sample.

Figure 1.1: Exposure to State-Level Prohibition by Cohort

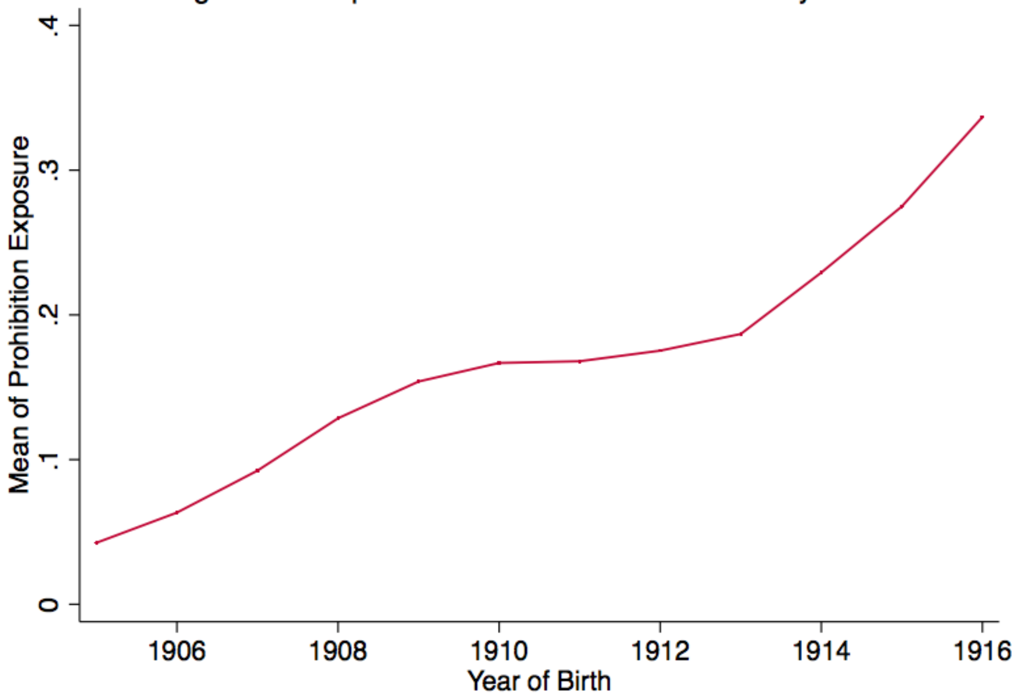



Table 1.1 displays the summary statistics for cohorts by sex and their exposure to state prohibition. Unexposed cohorts are those that completed three years of age in states with prohibition, but prior to prohibition enactment, in addition to cohorts born in wet states. Partially exposed cohorts are those that were conceived before the enactment of state prohibition but completed three years of age after the enactment. Fully exposed cohorts, on the other hand, are those that were conceived after the enactment of state prohibition. Comparisons using Student's t-tests are made between cohorts that are unexposed and cohorts that are partially exposed, and between cohorts that are unexposed and cohorts that are fully exposed.

**Table 1.2: Means (and Standard Deviations) by Sex and Prohibition Exposure**

| | N | Prohibition Exposure | Income | LFP | Years of Schooling | White | Suffrage Exposure |
|---|---|---|---|---|---|---|---|
| **Males** | 62269 | *Unexposed* | 5803.08 | 0.94 | 11.12 | 0.93 | 0.05 |
| | | | (4461.66) | (0.24) | (3.52) | (0.25) | (0.22) |
| | 8586 | *Partially Exposed* | 4964.60*** | 0.93*** | 10.49*** | 0.83*** | 0.11*** |
| | | | (4074.96) | (0.25) | (3.83) | (0.37) | (0.31) |
| | 10459 | *Fully Exposed* | 4634.95*** | 0.93*** | 10.14*** | 0.80*** | 0.05 |
| | | | (3920.01) | (0.26) | (3.84) | (0.40) | (0.23) |
| **Females** | 64210 | *Unexposed* | 1449.99 | 0.47 | 11.30 | 0.93 | 0.05 |
| | | | (2186.75) | (0.50) | (3.16) | (0.25) | (0.22) |
| | 9034 | *Partially Exposed* | 1191.43*** | 0.46 | 10.90*** | 0.82*** | 0.11*** |
| | | | (1806.23) | (0.50) | (3.43) | (0.38) | (0.31) |
| | 11108 | *Fully Exposed* | 1200.20*** | 0.49*** | 10.78*** | 0.79*** | 0.06*** |
| | | | (1823.27) | (0.50) | (3.45) | (0.41) | (0.23) |

Notes: Stars indicate the mean for partially exposed or fully exposed is different from unexposed: ***Significant at 1%; **Significant at 5%; *Significant at 10%. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure is the proportion of the critical quarters from the prenatal period up to 3 years of age exposed.

Total personal income is defined as pre-tax personal income or losses from all sources for the previous year. As can be seen in Table 1.2, both male and female cohorts that are partially and fully exposed to prohibition during early development have significantly lesser incomes in 1960 than those cohorts unexposed to prohibition in early development. Labor force participation (LFP) is a dichotomous variable indicating whether a person participated in the labor force. Male cohorts exposed to any degree to state prohibition display less LFP than unexposed male cohorts, although the difference is too small to be economically significant. Partially exposed and unexposed female cohorts did not display statistical differences, while the difference between unexposed and fully exposed women is statistically significant at the one percent level.

Years of schooling is obtained from the highest grade achieved variable, and coded to account for partially completed years of schooling. As with income, fully exposed and partially exposed cohorts of both male and female cohorts have significantly less years of schooling than unexposed male and female cohorts. White is a variable indicating the proportion of the cohort that is Caucasian. Unexposed cohorts are significantly more Caucasian, on average, than partially and fully exposed cohorts. Suffrage exposure indicates whether an individual was conceived in a state and year in which women could vote. It can be seen that partially exposed cohorts of both sexes had significantly more exposure to women's suffrage than the unexposed cohorts and the fully exposed cohorts.

Table 1.3 uses values from 1920 to reveal the zeitgeist of cohorts' birth states. The means by state from the 1920 census are matched to birth state in the 1960 dataset. Unexposed cohorts of both sexes were born to significantly smaller families. States where the unexposed cohorts were born had more Caucasians and also more urban inhabitants in 1920, defined as residing in a city or incorporated place of 2,500 inhabitants or more. Unexposed cohorts were born in states that had three to four times more foreigners, defined as having at least one foreign parent. Interestingly enough, unexposed cohorts actually attended school less than their partially exposed and fully exposed counterparts. There was no income recorded in the 1920 census, but according to the Duncan Socio-Economic Index (SEI)—a measure of occupational status based on the income level and educational attainment associated with each occupation—unexposed cohorts were also born in states that had a higher socio-economic standing, on average.

Lastly, unexposed cohorts were born to parents that participated more in the labor force. All these differences between unexposed and partially exposed, and between unexposed and fully exposed cohorts are statistically significant at the one percent level.

**Table 1.3: Means (and Standard Deviations) of State of Birth Values in 1920**

| **Prohibition Exposure** | **Family Size** | **White** | **Urban** | **Attend School** | **Foreign** | **SEI** | **LFP Male** | **LFP Female** |
|---|---|---|---|---|---|---|---|---|
| *Unexposed* | 4.85 | 0.92 | 0.54 | 0.23 | 0.39 | 10.77 | 0.90 | 0.24 |
| | (0.39) | (0.12) | (0.20) | (0.02) | (0.21) | (6.00) | (0.02) | (0.05) |
| *Partially Exposed* | 5.19*** | 0.79*** | 0.31*** | 0.25*** | 0.16*** | 8.50*** | 0.89*** | 0.22*** |
| | (0.47) | (0.18) | (0.14) | (0.03) | (0.18) | (4.92) | (0.02) | (0.06) |
| *Fully Exposed* | 5.36*** | 0.77*** | 0.25*** | 0.26*** | 0.12*** | 8.02*** | 0.88*** | 0.23*** |
| | (0.39) | (0.18) | (0.08) | (0.03) | (0.18) | (4.46) | (0.02) | (0.06) |

Notes: The mean by state is calculated from the 1920 census and matched to birth state. Stars indicate the mean for partially exposed or fully exposed is different from unexposed: ***Significant at 1%; **Significant at 5%; *Significant at 10%. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure is the proportion of the critical quarters from the prenatal period up to 3 years of age exposed.

Using the 1920 census and the comparison of means, it seems that exposed cohorts' birth states are, in fact, statistically different from unexposed cohorts' birth states. Nevertheless, when exploring the trends of these variables over the time period 1880 through 1920, using the census data from 1880, 1900, 1910 and 1920, it appears that prohibition and wet states are mostly following parallel trends. Figures 1.2 through 1.4 display the trends for the birth states of cohorts unexposed, partially exposed and fully exposed. The variables shown are SEI, LFP for males and LFP for females. The trends represent the change in the variables in states by cohort exposure.

Figure 1.2: Birth State Time Trends
Duncan Socio-Economic Index

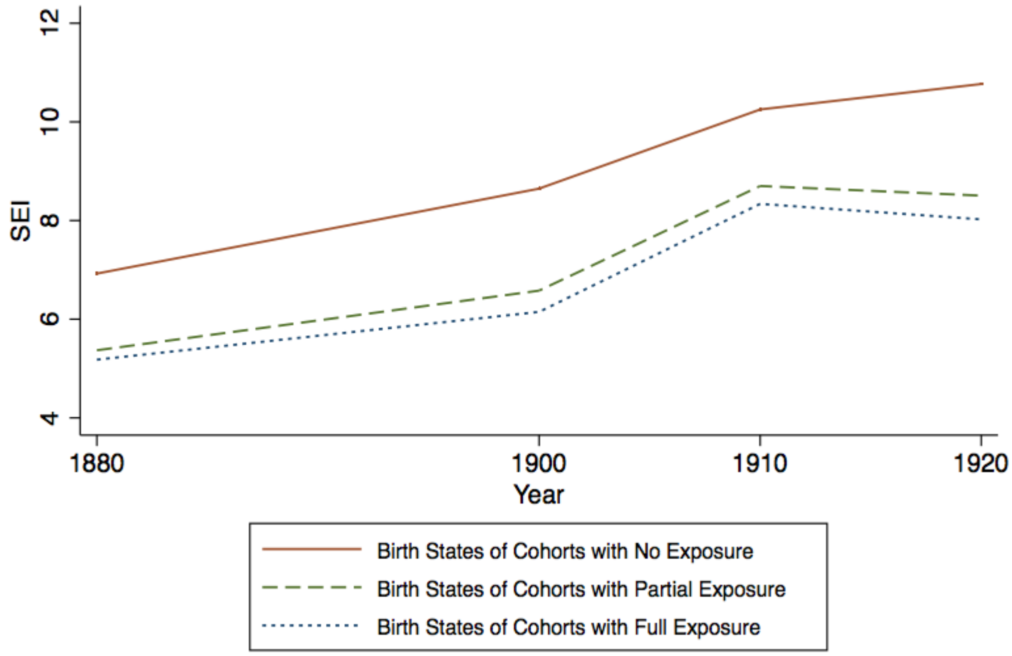
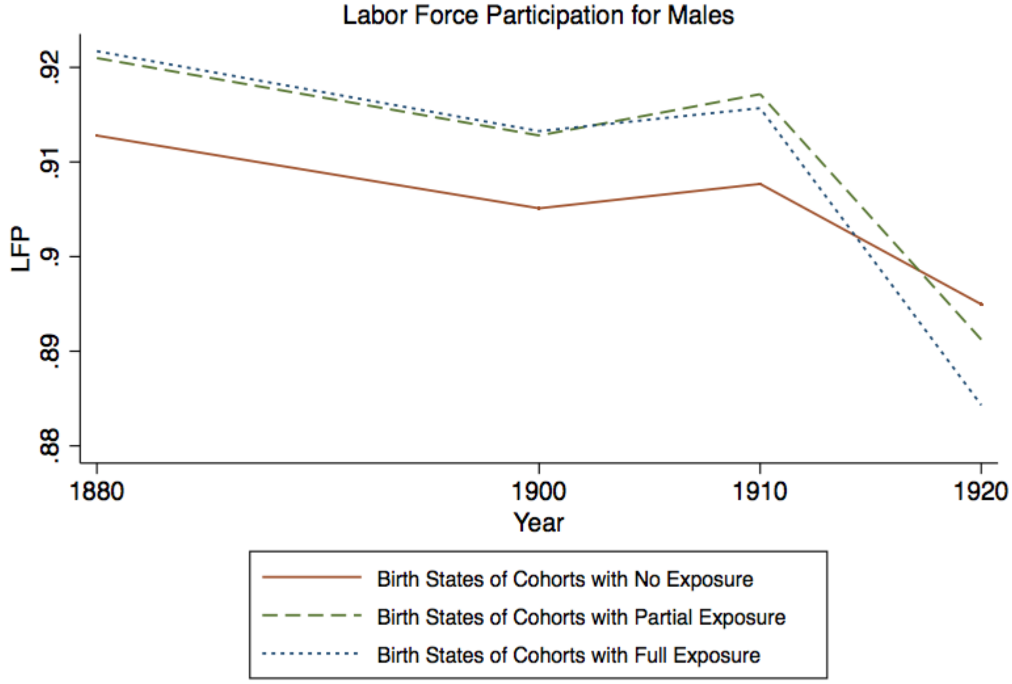
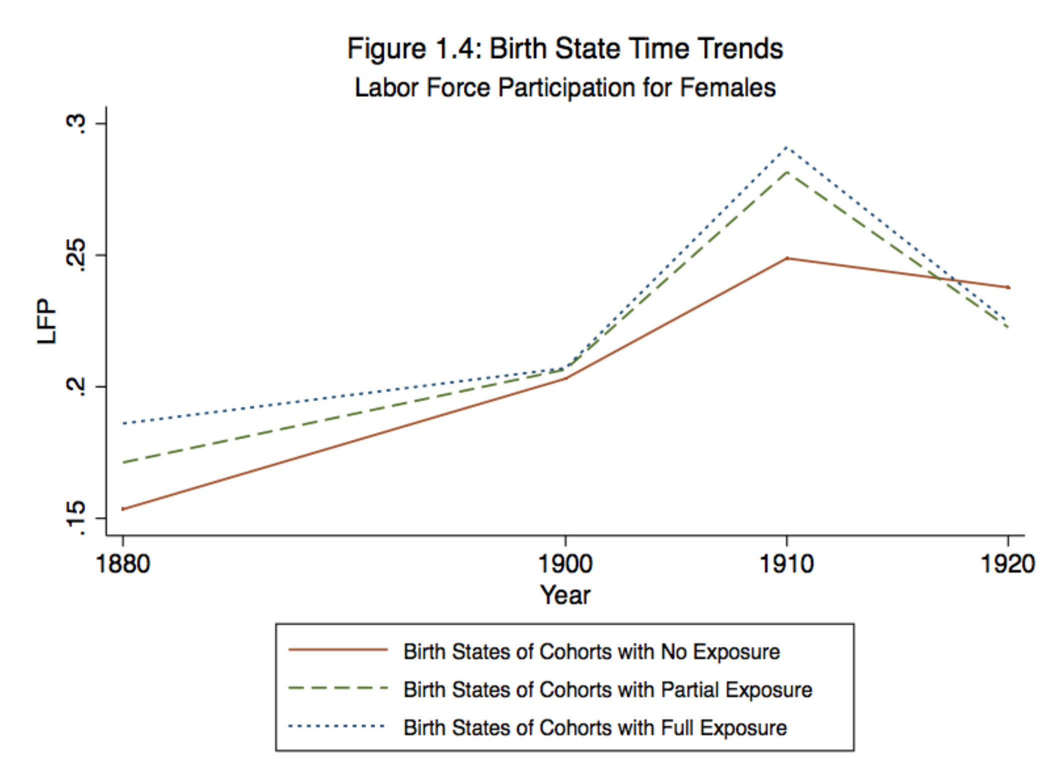



Figure 1.3: Birth State Time Trends
Labor Force Participation for Males






Although not shown, the variables family size, proportion white, urbanization, school attendance and immigrants all display mostly parallel trends.[12] In Figure 1.2, the trends in the SEI variable indicate that the birth states of unexposed, partially exposed and fully exposed cohorts were following parallel trends. In Figures 1.3 and 1.4, the variable LFP displays an interesting pattern for males and females. For males, LFP decreases during the period, possibly due to decreases in LFP amongst older males (Costa, 1998). While unexposed cohorts' birth states decrease less, all states show a decrease in LFP. For females, an increase in LFP is observed. The rate of increase for all states appears to be similar. There was an overcount of unpaid family agricultural workers in 1910, which accounts for the sharp increase in that year (Goldin, 1986).

[12] School attendance appears to become parallel among the birth states only after 1910.

Disregarding this unusual spike, the trends for all birth states of unexposed, partially exposed and fully exposed cohorts do appear to be very similar. These figures show that, despite the sharp differences observed in Tables 1.2 and 1.3, the birth states of the cohorts grouped by level of exposure were following sufficiently parallel trends.

## 1.5 ECONOMETRIC RESULTS

Table 1.4 summarizes the results of the difference in differences estimation for females, while Table 1.5 displays the results for males. All estimations are done separately for males and females to estimate the heterogeneous effects by gender. Column one represents the case where no control variables are included, aside from the birth state and birth year fixed effects. In column two, both race and birth quarter are included as control variables, in order to capture any differences in adult outcomes due to individual characteristics. State of current residence in 1960 is included in column three, in order to capture any variation emanating from differences in states. In column four, exposure to women's suffrage is included to capture any variation in the outcome variables coming from changes in early development environments caused by women's suffrage. And finally, in column five state specific time trends are included.

The inclusion of these trends proxies for potentially omitted variables that are unique to specific states that vary over time. At the same time, the inclusion of state specific time trends absorbs much of the variation in the outcome variable, in addition to exacerbating the problem of attenuation bias and therefore biasing the key coefficients towards zero (Donohue and Levitt, 2008). In cases where statistical significance is observed, standard errors are bootstrapped to ensure the effect is caused by prohibition exposure and not by serial correlation in the outcome variables. Clustered standard errors are displayed in parentheses in all cases, and their bootstrapped counterparts are displayed in brackets wherever the p-value using the clustered standard errors indicated statistical significance. The level of significance displayed in the tables is calculated using the bootstrapped standard errors.

Positive and significant effects are found for the outcome variables of total income and labor force participation for female cohorts in early development during state prohibition, shown in Table 1.4. In the case of total income, the positive and significant effects are robust to the inclusion of the control variables of birth quarter, race, state of current residence and suffrage exposure. In the specification in column four, the results suggest that female cohorts exposed to state prohibition in early development have an increased total income by $154, significant at ten percent, an increase of eleven percent compared to the mean of total income for all females, $1389.[13] Nonetheless, once including birth state specific time trends, the effect decreases substantially in magnitude and loses statistical significance. For labor force participation, the positive and significant results are robust to the inclusion of all control variables, but not to the inclusion of the birth state specific time trends. The result in column four suggests that female cohorts in early development during state prohibition had increased LFP by 0.038, significant at five percent or approximately by eight percent as compared to the mean for all females, 0.47.

[13] All dollar values are reported in 1960 dollars.

**Table 1.4: Effects for Female Cohorts from Prohibition Exposure**

| | Total Income | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Prohibition Exposure | 161.219** | 163.779** | 155.290* | 154.153* | 16.195 |
| | (60.769) | (60.378) | (60.030) | (60.373) | (72.604) |
| | [79.619] | [80.483] | [79.408] | [80.158] | [-] |
| | **Labor Force Participation** | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Prohibition Exposure | 0.038** | 0.039*** | 0.038** | 0.038** | 0.021 |
| | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) |
| | [0.015] | [0.015] | [0.015] | [0.017] | [0.017] |
| | **Years of Schooling** | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Prohibition Exposure | 0.063 | 0.032 | 0.018 | 0.016 | -0.035 |
| | (0.093) | (0.082) | (0.082) | (0.082) | (0.095) |
| N | 84352 | 84352 | 84352 | 84352 | 84352 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1960 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure is the proportion of the critical quarters from the prenatal period up to 3 years of age exposed.

There are two explanations for the decrease in magnitude and loss of statistical significance observed with the inclusion of the trends. One possibility is that there exist important omitted variables captured by the state specific time trends that are varying in the same way as the state prohibitions of alcohol. If this is the case, then these omitted variables are causing the increases in total income and LFP, and the effects of prohibition exposure cannot be disentangled from the effects of these omitted variables. A second explanation is that, because of the substantial increase in the number of control variables with the inclusion of the trends, the key coefficient is biased towards zero due to the exacerbated effect of attenuation bias. Either possibility is plausible, yet the lack of robustness of the effects with the inclusion of these trends suggests that the positive and significant effects found without the inclusion of the trends should be regarded with caution. With respect to the outcome variable years of schooling, no significant effects of the exposure to state prohibition during early development are observed.

Table 1.5 summarizes the results for male cohorts in early development during state prohibition. No statistically significant effects of being exposed to state prohibition in early development are found for male cohorts for any of the outcome variables once control variables are included, and using the bootstrapped standard errors to calculate the test statistic and p-value. Nevertheless, when using the clustered standard errors to calculate the test statistic, significant effects at five to ten percent were found for exposure to state prohibition for total income, in columns one through four. These clustered standard errors, however, are biased downward because of serial correlation in the outcome variable over time (Bertrand et al., 2004). The bootstrapped standard error corrects for this bias and the significance disappears in columns two through four. The standard errors were bootstrapped precisely to avoid any type I errors and in the case of total income for males, it appears that the serial correlation in male incomes over time is, in fact, what drives the significance once control variables are included.

**Table 1.5: Effects for Male Cohorts from Prohibition Exposure**

| | Total Income | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Prohibition Exposure | 223.567* | 201.674 | 185.987 | 193.993 | 84.936 |
| | (101.424) | (98.584) | (99.226) | (98.365) | (121.084) |
| | [135.584] | [142.156] | [130.631] | [135.029] | [-] |
| | **Labor Force Participation** | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Prohibition Exposure | 0.003 | 0.002 | 0.002 | 0.002 | -0.009 |
| | (0.008) | (0.008) | (0.008) | (0.008) | (0.010) |
| | **Years of Schooling** | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Prohibition Exposure | -0.040 | -0.082 | -0.090 | -0.088 | -0.184 |
| | (0.106) | (0.102) | (0.103) | (0.104) | (0.139) |
| N | 81314 | 81314 | 81314 | 81314 | 81314 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1960 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure is the proportion of the critical quarters from the prenatal period up to 3 years of age exposed.

The results for the more flexible specification using indicator variables to estimate the effect of exposure at different intervals during the early developmental period are shown in Tables 1.6 through 1.11. The indicator variables represent spending at least one quarter of each interval under state prohibition. The variables can indicate which intervals of the early developmental period are driving the positive results seen above. In Table 1.6, the effects for female cohorts on total income are displayed. Having spent at least one quarter of the *in utero* period under state prohibition results in positive and significant increases in income. This effect is robust to the inclusion of the control variables, but not to the inclusion of the state specific time trends. The effect in column 4 shows that spending at least one quarter *in utero* under state prohibition increases female cohorts' income by $133, an increase of ten percent, significant at five percent. Female cohorts that spent at least one quarter of their second year of life under state prohibition also saw significant increases in income, although only by eight percent, significant at the five percent level. The other intervals—the first, third, fourth and fifth year of life—showed no significant effects. Since only the *in utero* period and the second year of life display significant effects, it is likely that the positive and significant effect found in Table 1.4 for total income is being driven by exposure during these specific periods.

In Table 1.7 the results for labor force participation for female cohorts are displayed. The only interval for which positive and significant effects are found is exposure during the *in utero* period. The effects are highly significant at the one percent level, regardless of the inclusion of state specific time trends. Having been exposed to state prohibition for at least one quarter during the *in utero* period leads to increases in labor force participation for females by ten percent, significant at the one percent level. This finding suggests that the positive and significant effect observed for LFP in Table 1.4 is being driven by exposure during the prenatal period. Table 1.8 displays the effects of exposure on years of schooling for females. No significant effects for this variable are found.

**Table 1.6: Effects on Total Income for Female Cohorts from Prohibition Exposure Using Indicator Variables for Different Intervals of Exposure**

| | Total Income | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| In Utero Exposed | 128.452** | 131.545** | 133.213** | 132.573** | 48.035 |
| | (43.048) | (43.288) | (48.701) | (49.094) | (51.379) |
| | [57.978] | [60.426] | [55.539] | [58.579] | [-] |
| Exposed Birth to 1 Year Old | -79.510 | -80.963 | -83.495 | -83.538 | -100.012 |
| | (65.545) | (65.247) | (68.063) | (68.006) | (64.531) |
| Exposed 1 to 2 Years Old | 118.614** | 117.392** | 105.082** | 105.046** | 87.154 |
| | (50.479) | (50.640) | (48.767) | (48.811) | (53.847) |
| | [51.384] | [52.536] | [48.317] | [48.811] | [-] |
| Exposed 2 to 3 Years Old | -35.597 | -34.226 | -26.358 | -26.157 | -33.582 |
| | (66.309) | (66.215) | (65.959) | (65.880) | (65.116) |
| Exposed 3 to 4 Years Old | 19.241 | 18.070 | 14.550 | 14.422 | 9.767 |
| | (44.313) | (44.809) | (46.511) | (46.546) | (49.449) |
| Exposed 4 to 5 Years Old | 30.455 | 33.612 | 36.831 | 36.124 | -7.886 |
| | (51.162) | (51.125) | (50.810) | (50.849) | (56.734) |
| N | 84352 | 84352 | 84352 | 84352 | 84352 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1960 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure in each interval is defined as having spent at least one quarter of the interval under state prohibition.

**Table 1.7: Effects on LFP for Female Cohorts from Prohibition Exposure Using Indicator Variables for Different Intervals of Exposure**

| | Labor Force Participation | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| In Utero Exposed | 0.048*** | 0.047*** | 0.046*** | 0.046*** | 0.041*** |
| | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) |
| | [0.014] | [0.014] | [0.015] | [0.015] | [0.015] |
| Exposed From Birth to 1 Year Old | -0.023 | -0.022 | -0.021 | -0.021 | -0.024 |
| | (0.018) | (0.018) | (0.018) | (0.018) | (0.016) |
| Exposed From 1 to 2 Years Old | 0.022 | 0.022 | 0.020 | 0.020 | 0.015 |
| | (0.014) | (0.014) | (0.014) | (0.014) | (0.016) |
| Exposed From 2 to 3 Years Old | -0.015 | -0.015 | -0.014 | -0.014 | -0.018 |
| | (0.015) | (0.016) | (0.016) | (0.016) | (0.015) |
| Exposed From 3 to 4 Years Old | 0.011 | 0.013 | 0.012 | 0.012 | 0.010 |
| | (0.012) | (0.012) | (0.012) | (0.012) | (0.014) |
| Exposed From 4 to 5 Years Old | 0.003 | 0.002 | 0.003 | 0.003 | -0.010 |
| | (0.010) | (0.009) | (0.010) | (0.010) | (0.010) |
| N | 84352 | 84352 | 84352 | 84352 | 84352 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1960 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure in each interval is defined as having spent at least one quarter of the interval under state prohibition.

**Table 1.8: Effects on Years of Schooling for Female Cohorts from Prohibition Exposure Using Indicator Variables for Different Intervals of Exposure**

| | Years of Schooling | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| In Utero Exposed | 0.043 | 0.059 | 0.066 | 0.064 | 0.026 |
| | (0.079) | (0.080) | (0.078) | (0.078) | (0.093) |
| Exposed From Birth to 1 Year Old | 0.029 | 0.016 | 0.004 | 0.004 | -0.012 |
| | (0.114) | (0.110) | (0.107) | (0.107) | (0.106) |
| Exposed From 1 to 2 Years Old | 0.012 | 0.005 | -0.010 | -0.010 | -0.003 |
| | (0.069) | (0.066) | (0.065) | (0.065) | (0.062) |
| Exposed From 2 to 3 Years Old | 0.002 | -0.014 | -0.003 | -0.002 | -0.015 |
| | (0.103) | (0.097) | (0.099) | (0.099) | (0.102) |
| Exposed From 3 to 4 Years Old | 0.005 | -0.005 | -0.013 | -0.013 | -0.025 |
| | (0.110) | (0.105) | (0.110) | (0.110) | (0.113) |
| Exposed From 4 to 5 Years Old | -0.057 | -0.050 | -0.041 | -0.043 | -0.060 |
| | (0.064) | (0.063) | (0.063) | (0.064) | (0.060) |
| N | 84352 | 84352 | 84352 | 84352 | 84352 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1960 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure in each interval is defined as having spent at least one quarter of the interval under state prohibition.

The results for male cohorts are displayed in Tables 1.9 through 1.11. In Table 1.9, no significant effects are observed for total income, although the magnitude of effects for being exposed at least one quarter during the first year is large. Again, using the clustered standard errors would result in spurious effects significant at ten percent in columns one, three and four. With respect to the outcome variable labor force participation, no significant effects are observed for male cohorts in Table 1.10. Table 1.11 displays the effects for male cohorts on years of schooling. While the exposure *in utero* and also in the first year of life display negative albeit not significant effects, one effect appears positive and significant. Male cohorts having spent at least one quarter of their fifth year under state prohibition see an increase in years of schooling by one percent, at the ten percent level of significance. This effect suggests that these cohorts were able to deviate above and beyond the trend of years of schooling as compared to cohorts unexposed. Nonetheless, no other significant effects are observed for years of schooling in any other specification in this chapter.

**Table 1.9: Effects on Total Income for Male Cohorts from Prohibition Exposure Using Indicator Variables for Different Intervals of Exposure**

| | Total Income | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| In Utero Exposed | -2.193 | 8.434 | -7.217 | -1.665 | -71.137 |
| | (108.637) | (111.684) | (111.678) | (111.960) | (107.196) |
| Exposed From Birth to 1 Year Old | 148.589 | 134.599 | 169.989 | 171.077 | 131.655 |
| | (82.925) | (83.648) | (89.001) | (89.253) | (93.321) |
| | [106.251] | [-] | [107.351] | [111.568] | [-] |
| Exposed From 1 to 2 Years Old | 28.856 | 47.096 | 18.306 | 18.978 | 9.340 |
| | (92.930) | (96.299) | (92.057) | (92.107) | (94.973) |
| Exposed From 2 to 3 Years Old | 25.389 | -1.765 | -22.961 | -25.278 | -17.594 |
| | (80.019) | (72.042) | (65.114) | (64.509) | (62.913) |
| Exposed From 3 to 4 Years Old | -1.998 | -11.905 | -9.058 | -8.061 | -8.628 |
| | (91.203) | (88.104) | (93.401) | (93.615) | (101.585) |
| Exposed From 4 to 5 Years Old | -20.025 | -24.803 | -10.888 | -4.757 | 10.837 |
| | (121.475) | (118.049) | (121.758) | (122.020) | (126.333) |
| N | 81314 | 81314 | 81314 | 81314 | 81314 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1960 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure in each interval is defined as having spent at least one quarter of the interval under state prohibition.

**Table 1.10: Effects on LFP for Male Cohorts from Prohibition Exposure Using Indicator Variables for Different Intervals of Exposure**

| | Labor Force Participation | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| In Utero Exposed | -0.001 | -0.001 | -0.001 | -0.001 | -0.008 |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) |
| Exposed From Birth to 1 Year Old | 0.002 | 0.001 | 0.002 | 0.002 | 0.001 |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) |
| Exposed From 1 to 2 Years Old | 0.005 | 0.006 | 0.005 | 0.005 | 0.005 |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) |
| Exposed From 2 to 3 Years Old | -0.003 | -0.004 | -0.004 | -0.004 | -0.005 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.006) |
| Exposed From 3 to 4 Years Old | -0.007 | -0.007 | -0.007 | -0.007 | -0.007 |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) |
| Exposed From 4 to 5 Years Old | 0.008 | 0.007 | 0.007 | 0.007 | 0.008 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| N | 81314 | 81314 | 81314 | 81314 | 81314 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1960 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure in each interval is defined as having spent at least one quarter of the interval under state prohibition.

**Table 1.11: Effects on Years of Schooling for Male Cohorts from Prohibition Exposure Using Indicator Variables for Different Intervals of Exposure**

| | Years of Schooling | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| In Utero Exposed | -0.037 | -0.030 | -0.028 | -0.027 | -0.126 |
| | (0.087) | (0.076) | (0.075) | (0.075) | (0.077) |
| Exposed From Birth to 1 Year Old | -0.095 | -0.109 | -0.099 | -0.098 | -0.074 |
| | (0.073) | (0.064) | (0.063) | (0.063) | (0.070) |
| | [-] | [0.089] | [-] | [-] | [-] |
| Exposed From 1 to 2 Years Old | 0.006 | 0.023 | 0.003 | 0.003 | 0.045 |
| | (0.097) | (0.095) | (0.090) | (0.090) | (0.102) |
| Exposed From 2 to 3 Years Old | 0.060 | 0.025 | 0.022 | 0.021 | 0.048 |
| | (0.092) | (0.087) | (0.087) | (0.087) | (0.081) |
| Exposed From 3 to 4 Years Old | 0.050 | 0.039 | 0.040 | 0.040 | 0.090 |
| | (0.103) | (0.098) | (0.100) | (0.100) | (0.108) |
| Exposed From 4 to 5 Years Old | 0.030 | 0.018 | 0.033 | 0.034 | 0.145* |
| | (0.070) | (0.060) | (0.060) | (0.060) | (0.060) |
| | [-] | [-] | [-] | [-] | [0.075] |
| N | 81314 | 81314 | 81314 | 81314 | 81314 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1960 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure in each interval is defined as having spent at least one quarter of the interval under state prohibition.

One final consideration with respect to the estimated effects of the exposure to prohibition is measurement error and attenuation bias. Measurement error in independent variables will bias the coefficients towards zero. Although the enactments of state prohibitions are measured with accuracy, not all wet states were completely untreated. Most states had local option laws by which counties or municipalities forbade the sale or consumption of alcohol, or the presence of saloons. While there is no reason to suspect this measurement error differentially affected male cohorts more than female cohorts, it is possible that the effects found for females are lower bounds and that no effects were found for males due to attenuation bias.

Furthermore, since effects for females seem to be mostly driven by exposure in the prenatal period, the differential effects of modified uterine environments by sex should be investigated. It is widely accepted that the male fetus is more sensitive to negative shocks in utero (Catalano and Bruckner, 2006). Byrne et al. (1987) found that male fetuses are 25 times more likely to be spontaneously aborted than female fetuses. As an innate evolutionary mechanism, when under environmental stress, pregnant females spontaneously abort frail male fetuses that would reproduce worse than a weak female fetus, and thus allow for a new pregnancy to begin—yielding either a female or a more robust male (Trivers and Willard, 1973). If the frailest male fetuses are selectively miscarried, then the remaining male cohort is stronger, on average. This phenomenon is known as the culled cohort hypothesis (Schenck-Gustafsson et al., 2012). Various papers find that when environmental stressors take place, the surviving male cohorts have better health and cognition.[14]

Considering that male cohorts in wet states would be subject to greater environmental stressors than male cohorts in prohibition states, it is possible that the frailest male fetuses were selectively miscarried thereby producing stronger-than-average male cohorts. These culled male cohorts in wet states could be the reason why no effects

---

[14] For example, Bruckner and Nobles (2013) find that male cohorts in utero during the September 11$^{th}$, 2001 attacks scored greater cognitive ability at 24 months than cohorts born before the attacks, significant at one percent.

of exposure to state prohibition are found for males. Since frail female fetuses are not "culled," female cohorts *in utero* in wet states would be carried to term more often and therefore would exhibit more instances of the effects of FAE, on average.

## 1.6 ROBUSTNESS CHECKS

In order to ensure that the estimates presented are robust to alternative specifications and that a causal interpretation is appropriate, certain robustness checks can be performed. Since positive and significant effects were only found for total income and labor force participation for female cohorts, and, in one case, total income for male cohorts, only these results will be displayed in the robustness checks tables. Once again, the level of significance is calculated using bootstrapped standard errors, shown in brackets, while the clustered standard errors are shown in parentheses.

The first robustness check is intended to show that the results are robust to changes in the specification of the early critical period. In the definition of the exposure variable above, the critical early development quarters were defined as the prenatal period and the first three years of life. Exposure was then calculated as the proportion of critical quarters spent under state prohibition. Table 1.12 presents estimation results that use different specifications for the critical early development quarters. In every specification, all control variables are included. The state specific time trend is also included in the second column of each panel.

**Table 1.12: Robustness Check – Effects from Prohibition Exposure Using Different Definitions of the Critical Early Development Quarters**

| | | Critical Early Period: | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | In Utero Only | | In Utero & Up to Age 1 | | In Utero & Up to Age 2 | | In Utero & Up to Age 4 | |
| | **Total Income** | | | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Females** | Prohibition Exposure | 137.499** | 38.597 | 139.448** | 23.060 | 136.896** | 0.501 | 163.521** | 19.918 |
| | | (44.310) | (59.976) | (49.740) | (64.650) | (56.599) | (70.302) | (65.897) | (77.601) |
| | | [62.726] | [-] | [65.597] | [-] | [65.494] | [-] | [74.845] | [-] |
| | **Labor Force Participation** | | | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Females** | Prohibition Exposure | 0.033** | 0.025 | 0.038*** | 0.031** | 0.037*** | 0.024* | 0.038** | 0.019 |
| | | (0.010) | (0.012) | (0.011) | (0.012) | (0.011) | (0.011) | (0.013) | (0.013) |
| | | [0.014] | [-] | [0.013] | [0.015] | [0.014] | [0.015] | [0.015] | [-] |
| | **Total Income** | | | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Males** | Prohibition Exposure | 143.093 | 13.215 | 176.407 | 54.743 | 203.682* | 93.542 | 174.951 | 74.122 |
| | | (94.216) | (101.783) | (98.957) | (111.281) | (95.556) | (108.067) | (96.272) | (131.751) |
| | | [-] | [-] | [108.740] | [-] | [107.824] | [-] | [112.783] | [-] |
| | N for Females | 84352 | 84352 | 84352 | 84352 | 84352 | 84352 | 84352 | 84352 |
| | N for Males | 81314 | 81314 | 81314 | 81314 | 81314 | 81314 | 81314 | 81314 |
| | All Control Variables | Y | Y | Y | Y | Y | Y | Y | Y |
| | Birth State Time Trend | N | Y | N | Y | N | Y | N | Y |

Notes: Cohort FE, state of birth FE, birth quarter, race, state of residence in 1960 and the suffrage exposure dummy are included in all columns. Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure is the proportion of the critical period exposed.

The first panel displays the results when defining the critical early period exclusively as the *in utero* period. The second panel shows the results when the definition of the critical period includes the prenatal period and the first year of life, while the third panel's definition additionally includes the second year of life. In the final panel, results are displayed for a definition of the critical period that includes the prenatal period, and up to the fourth year of life—up to age four. Regardless of the definition of the critical period, total income for females remains positive and significant at the five percent level in the specification without the state specific time trends. Once the trends are included, the significance disappears, as in the main regressions.

For the variable labor force participation, in the specification without the state specific time trends, effects remain positive and significant at the level of five to one percent. In the specifications where the definitions of the critical period are up to age one, and up to age two, the effects of LFP continue positive and significant even with the inclusion of the state specific time trends. As for total income for males, only one specification where the critical period is defined as being up to age two displays significant effects at the ten percent level, without the inclusion of the state specific time trends. These results suggest that the positive and significant findings for labor force participation and total income for females are not sensitive to the choice of the early critical period.

The second robustness check explores different delineations of the sample. In the estimations above, the first included cohort was that born in the third quarter of 1905, because they were the first partially exposed cohort to state prohibition. On the upper bound, the last cohort included was that born in quarter four of 1916, since this would exclude any variation coming from National Prohibition. To ensure the positive and significant effects found above are not due to the way the sample is delineated, Table 1.13 explores changes to the lower bound of the delineation while keeping the upper bound constant as cohorts born up to the first quarter of 1917. Table 1.14 explores changes to the upper bound while keeping the lower bound constant as cohorts born from the third quarter of 1905. In both tables, all control variables are used in every specification, while the state specific time trend is included in the second column of each panel.

The first panel of Table 1.13 displays the results when the first cohort included in the sample is changed to those born in the third quarter of 1903 and the second panel to those born in the third quarter of 1904. The third and fourth panels display the results when the first cohort included in the sample was born in the third quarter of 1906 and 1907, respectively. In all cases without the inclusion of the state specific time trends, the effects for female cohorts on total income and labor force participation remain positive and significant. When the first cohort included in the sample is born in the third quarter of 1907, the effect for LFP remains significant even with the inclusion of the state specific time trends. For male cohorts, no significant effects are observed for total income, regardless of the delineation of the lower bound. Although not shown, no effects are found for years of schooling for either female or male cohorts, or for LFP for males for any delineation of the lower bound.

**Table 1.13: Robustness Check – Effects from Prohibition Exposure Using Changing Sample Delineations at the Lower Bound**

| | | First Cohort Included Born In: | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | 1903Q3 | | 1904Q3 | | 1906Q3 | | 1907Q3 | |
| | **Total Income** | | | | | | | | |
| **Females** | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Prohibition Exposure | 136.944* | 16.854 | 147.822* | 25.519 | 172.101* | 57.395 | 196.111* | 146.590 |
| | | (55.669) | (76.425) | (56.087) | (71.038) | (75.611) | (91.371) | (80.096) | (110.036) |
| | | [73.791] | [-] | [77.443] | [-] | [90.926] | [-] | [99.424] | [-] |
| | **Labor Force Participation** | | | | | | | | |
| **Females** | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Prohibition Exposure | 0.038*** | 0.024 | 0.041*** | 0.023 | 0.043** | 0.036 | 0.040** | 0.045* |
| | | (0.009) | (0.011) | (0.010) | (0.011) | (0.014) | (0.015) | (0.016) | (0.023) |
| | | [0.014] | [0.016] | [0.015] | [0.016] | [0.017] | [0.022] | [0.018] | [0.024] |
| | **Total Income** | | | | | | | | |
| **Males** | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Prohibition Exposure | 114.198 | 61.976 | 186.145 | 84.211 | 227.730 | 152.977 | 212.728 | 146.489 |
| | | (83.306) | (147.732) | (91.192) | (132.168) | (114.424) | (169.021) | (136.003) | (255.781) |
| | | [-] | [-] | [122.555] | [-] | [159.369] | [-] | [164.246] | [-] |
| | N for Females | 94279 | 94279 | 88342 | 88342 | 76025 | 76025 | 69600 | 69600 |
| | N for Males | 90660 | 90660 | 85087 | 85087 | 73380 | 73380 | 67159 | 67159 |
| | All Control Variables | Y | Y | Y | Y | Y | Y | Y | Y |
| | Birth State Time Trend | N | Y | N | Y | N | Y | N | Y |

Notes: Cohort FE, state of birth FE, birth quarter, race, state of residence in 1960 and the suffrage exposure dummy are included in all columns. Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample upper bound is limited to cohorts born up to 1916 quarter 4 and prohibition exposure is the proportion of the critical quarters from the prenatal period up to 3 years of age exposed.

Table 1.14 explores changes to the upper bound of the sample delineation, while keeping the lower bound constant. The first panel of Table 1.14 displays the results when the last cohort included in the estimation was born in the fourth quarter of 1915. In this specification, significant effects are only observed for total income for females. This is to be expected since 20 of the 32 prohibition states enacted in or after 1916. The absence of positive and significant effects for LFP for females in the first panel indicates that the effects seen above in the main regressions are likely being driven by exposure to state prohibition in these states enacting in or after 1916.

The second and third panels of Table 1.14 display the results when the last cohort included was born in the fourth quarter of 1917 and 1918, respectively. Including cohorts born up to and including the fourth quarter of 1917 strengthens the positive and significant effects for LFP and total income for females. This is likely due to including exposure from states that enacted later. Results including cohorts born up to and including the fourth quarter of 1918 remain positive and significant for LFP and total income for females, although the effects decrease as compared to the specification in the second panel. In the last panel of Table 1.14, the last cohort included in the sample was born in the fourth quarter of 1919. Total income and LFP continue to be positive and significant in this final specification. While contracting the upper bound results in no significant findings in the first panel, this is expected since most of the variation in state prohibition happens after the cutoff in panels one. The findings are, however, robust to the inclusion of more cohorts along the upper bound of the sample delineation. While the results are not shown, no effects are found for years of schooling for either males or females, or for LFP for males, regardless of the delineation of the upper bound.

**Table 1.14: Robustness Check – Effects from Prohibition Exposure Using Changing Sample Delineations at the Upper Bound**

| | | Last Cohort Included Born In: | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | 1915Q4 | | 1917Q4 | | 1918Q4 | | 1919Q4 | |
| | **Total Income** | | | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Females | Prohibition Exposure | 97.428* | -64.277 | 165.626** | 16.870 | 148.764* | 6.269 | 145.833* | 7.577 |
| | | (54.289) | (80.375) | (65.008) | (66.810) | (62.781) | (58.241) | (59.687) | (50.007) |
| | | [88.454] | [-] | [84.239] | [-] | [83.422] | [-] | [75.665] | [-] |
| | **Labor Force Participation** | | | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Females | Prohibition Exposure | 0.014 | -0.018 | 0.041*** | 0.020 | 0.028* | 0.010 | 0.031** | 0.012 |
| | | (0.011) | (0.011) | (0.013) | (0.012) | (0.012) | (0.010) | (0.011) | (0.008) |
| | | [-] | [0.020] | [0.016] | [0.016] | [0.015] | [-] | [0.014] | [-] |
| | **Total Income** | | | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Males | Prohibition Exposure | 182.030 | 80.181 | 172.717 | 104.973 | 154.326 | 109.501 | 109.238 | 47.983 |
| | | (99.942) | (135.920) | (77.360) | (107.723) | (64.421) | (93.065) | (67.058) | (88.794) |
| | | [166.615] | [-] | [117.406] | [-] | [114.792] | [-] | [-] | [-] |
| | N for Females | 76499 | 76499 | 92560 | 92560 | 101372 | 101372 | 109794 | 109794 |
| | N for Males | 73736 | 73736 | 89108 | 89108 | 97405 | 97405 | 105295 | 105295 |
| | All Control Variables | Y | Y | Y | Y | Y | Y | Y | Y |
| | Birth State Time Trend | N | Y | N | Y | N | Y | N | Y |

Notes: Cohort FE, state of birth FE, birth quarter, race, state of residence in 1960 and the suffrage exposure dummy are included in all columns. Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample lower bound is limited to cohorts born from 1905 quarter 3 and prohibition exposure is the proportion of the critical quarters from the prenatal period up to 3 years of age exposed.

The third robustness check has to do with the timing of state-level prohibition. Since most states enacted prohibition in the twentieth century, the earliest enactors in the nineteenth century might be somehow different than those enacting later. All cohorts from these three states that enacted prohibition in the nineteenth century, Maine, Kansas and North Dakota, are fully treated by state prohibition since they were all conceived after enactment. In order to ensure the results are not dependent on these states that are perhaps different from other states enacting in the twentieth century, Table 1.15 displays the results excluding cohorts born in these states. Once again, effects for female cohorts for total income and LFP remain positive and significant without the inclusion of the state specific time trends. With the exclusion of these states, positive and significant effects are also observed for male cohorts for total income, significant at ten percent. Although not shown, no effects are observed for years of schooling for cohorts of either sex, nor for LFP for males.

**Table 1.15: Robustness Check – Effects from Prohibition Exposure Excluding Birth States that Enacted Prohibition Before 1900**

| | | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|---|
| **Total Income** | | | | | | |
| **Females** | Prohibition Exposure | 164.822** | 167.463* | 159.587** | 157.559* | 13.480 |
| | | (61.230) | (60.915) | (60.339) | (60.181) | (72.438) |
| | | [78.216] | [87.328] | [78.049] | [85.273] | [-] |
| **Labor Force Participation** | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) |
| **Females** | Prohibition Exposure | 0.039** | 0.040** | 0.039** | 0.039** | 0.021 |
| | | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) |
| | | [0.016] | [0.016] | [0.015] | [0.016] | [0.017] |
| **Total Income** | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) |
| **Males** | Prohibition Exposure | 231.186* | 210.550* | 195.766* | 199.568* | 93.386 |
| | | (102.117) | (99.268) | (99.934) | (99.685) | (121.139) |
| | | [125.748] | [132.864] | [130.755] | [133.593] | [-] |
| | N for Females | 81610 | 81610 | 81610 | 81610 | 81610 |
| | N for Males | 78704 | 78704 | 78704 | 78704 | 78704 |
| | Cohort FE | Y | Y | Y | Y | Y |
| | State of Birth FE | Y | Y | Y | Y | Y |
| | Birth Quarter Dummies | N | Y | Y | Y | Y |
| | Race Dummies | N | Y | Y | Y | Y |
| | State of Residence in 1960 | N | N | Y | Y | Y |
| | Suffrage Exposure Dummy | N | N | N | Y | Y |
| | Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1905 quarter 3, to cohorts born in 1916 quarter 4 and prohibition exposure is the proportion of the critical quarters from the prenatal period up to 3 years of age exposed.

The final robustness check is a "placebo" difference in differences estimation. In this placebo test, state prohibitions were coded to have fictitiously started ten years after their actual start dates. The sample included in the estimation is cohorts born from the third quarter of 1915 to those born in the fourth quarter of 1926. This sample delineation is the original delineation plus ten years. The 1970 census was used in order to have a sample that was the same age (43-54) as the true sample. This placebo test entails estimating the equation:

$$outcome_{ics}=\beta fakeprohibitionexposure_{cs}+\delta stateborn_{s}+\gamma yearborn_{c}+\lambda X_{ics}+\theta trend_{cs}+\varepsilon_{ics} \quad (1.2)$$

where all variables are equivalent to those mentioned above for the main regressions, with the important exception that these cohorts were *not* exposed to the patterns of state prohibition onset that the true sample was exposed to. Thus, the variable *fakeprohibitionexposure* is the proportion of quarters fictitiously spent under state prohibition out of the 15 critical early development quarters. Since there was no real exposure, no significant effects should be found. Tables 1.16 and 1.17 display the results for female and male cohorts, respectively. No significant effects are found for any variable for either female or male cohorts. The insignificance of the results in the placebo test suggests that the trends between exposed and unexposed cohorts are, in fact, parallel.

**Table 1.16: Robustness Check – Effects for Female Cohorts from Fictitious Prohibition Exposure Using the 1970 Census**

| | **Total Income** | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Fictitious Prohibition Exposure | 82.994 | 85.541 | 74.671 | 71.632 | -182.688 |
| | (94.696) | (95.474) | (97.914) | (97.398) | (121.348) |
| | **Labor Force Participation** | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Fictitious Prohibition Exposure | 0.007 | 0.006 | 0.006 | 0.006 | -0.010 |
| | (0.011) | (0.011) | (0.012) | (0.012) | (0.013) |
| | **Years of Schooling** | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Fictitious Prohibition Exposure | 0.044 | 0.035 | 0.027 | 0.024 | 0.038 |
| | (0.083) | (0.084) | (0.085) | (0.085) | (0.106) |
| N | 94270 | 94270 | 94270 | 94270 | 94270 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1970 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1915 quarter 3, to cohorts born in 1926 quarter 4 and fictitious prohibition exposure is the proportion of the critical quarters from the prenatal period up to 3 years of age "exposed."

**Table 1.17: Robustness Check – Effects for Male Cohorts from Fictitious Prohibition Exposure Using the 1970 Census**

| | Total Income | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Fictitious Prohibition Exposure | 283.767 | 164.587 | 135.173 | 129.981 | 185.148 |
| | (169.856) | (174.045) | (181.443) | (183.014) | (233.338) |
| | [291.707] | [-] | [-] | [-] | [-] |
| | **Labor Force Participation** | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Fictitious Prohibition Exposure | 0.014 | 0.011 | 0.011 | 0.011 | -0.009 |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.009) |
| | [0.009] | [0.010] | [0.010] | [0.010] | [-] |
| | **Years of Schooling** | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Fictitious Prohibition Exposure | 0.211 | 0.144 | 0.139 | 0.138 | 0.038 |
| | (0.078) | (0.081) | (0.075) | (0.075) | (0.123) |
| | [0.129] | [0.144] | [0.136] | [0.143] | [-] |
| N | 87553 | 87553 | 87553 | 87553 | 87553 |
| Cohort FE | Y | Y | Y | Y | Y |
| State of Birth FE | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | Y | Y | Y | Y |
| Race Dummies | N | Y | Y | Y | Y |
| State of Residence in 1970 | N | N | Y | Y | Y |
| Suffrage Exposure Dummy | N | N | N | Y | Y |
| Birth State Time Trend | N | N | N | N | Y |

Notes: Significance is calculated using bootstrapped standard errors, where present: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors are displayed in parentheses, while bootstrapped standard errors are displayed in brackets. The sample is limited to cohorts born from 1915 quarter 3, to cohorts born in 1926 quarter 4 and fictitious prohibition exposure is the proportion of the critical quarters from the prenatal period up to 3 years of age "exposed."

A final word can be said about the variation of the onset of prohibition in different states. Extensive research into the topic suggests that the timing of enactments of prohibition was random to a certain degree, considering all the factors involved. The precise date of enactment of state prohibition depended on local option victories, willingness of local politicians and judiciaries, temperance movement activity, wet and dry voter turnouts, Scientific Temperance Instruction, liquor business ties in the community, the actual enactment dates and a variety of historical accidents. Most states that enacted prohibition in the later periods went through a multitude of initial failed attempts, due to not having enough votes, not having political support, strong liquor interests, and so on. This evidence is suggestive that the onset of prohibition in different states was indeed exogenous, to a certain extent.

## 1.7 ADDITIONAL ESTIMATIONS

Besides estimating the effects of early developmental exposure to an alcohol reduced environment, an additional estimation can shed light on one of the mechanisms by which prohibition exposure increased adult female total income and labor force participation. One of the possible mechanisms by which state prohibition can increase the outcome variables of cohorts in early development at the time is by decreasing unintended pregnancies and thereby generating cohorts that are better planned and therefore more nurtured. Although we cannot directly measure whether a pregnancy is intended or unintended, the crude birth rate—the number of births per 1,000 population—can be used as a proxy. The dataset used to obtain the crude birth rate is the *Vital Statistics Rates in the United States 1900-1940* (U.S. Public Health Service, 1947). Table 1.18 displays the mean crude birth rate by year and the proportion of states having enacted prohibition in the *preceding* year for the limited number of states for which the *Vital Statistics* are available.[15]

[15] These states are California, Connecticut, Delaware, Indiana, Kansas, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Nebraska, New Hampshire, New Jersey, New York,

**Table 1.18: Mean Crude Birth Rate and the Proportion of States with Prohibition Enacted the Preceding Year**

| Year | CBR | N | Prohibition Enactment In Preceding Year |
|---|---|---|---|
| 1915 | 23.546 | 11 | 0.184 |
| 1916 | 23.733 | 12 | 0.225 |
| 1917 | 24.024 | 21 | 0.388 |
| 1918 | 24.452 | 21 | 0.469 |
| 1919 | 22.252 | 23 | 0.592 |
| 1920 | 23.708 | 24 | 0.653 |
| 1921 | 24.218 | 28 | 1.000 |

Notes: The crude birth rate is the number of live births per 1000 population. See text for the subset of states for which data is available. Not all states had observations in every year. Prohibition equals one in a given state year if prohibition had been enacted the previous year. Source for CBR: U.S. Public Health Service (1947).

In order to estimate the near contemporaneous effect of prohibition on the crude birth rate, the following equation was estimated, using the years 1915-1921:

$$cbr_{s,y+1}=\beta_1 prohibitionlag_{sy}+\beta_2 state_s+ \beta_3 year_y+\varepsilon_{sy} \tag{1.3}$$

where s indexes state, and y indexes year. The variable *cbr* is the crude birth rate, which varies by state and year, the variable *prohibitionlag* is an indicator variable that varies by state and year and equals one if prohibition was enacted the preceding year, *state* is a fixed effect for state and *year* is a fixed effect for year. The state fixed effect captures any variation in the crude birth rate that is caused by time invariant characteristics of states, while the year fixed effect captures the time trend of the crude birth rate common to all

North Carolina, Ohio, Oregon, Pennsylvania, Rhode Island, South Carolina, Utah, Vermont, Virginia, Washington and Wisconsin. Not all states had observations in every year in the sample.

states. A lagged prohibition variable is used because prohibition in a given year would have an effect on the birthrate of the following year.

**Table 1.19: Effects of Prohibition on the Crude Birth Rate of the Following Year**

| | (1) | (2) |
|---|---|---|
| Prohibition Lag | 0.226 | -0.741** |
| | (0.571) | (0.352) |
| N | 133 | 133 |
| Year Fixed Effect | N | Y |
| State Fixed Effect | N | Y |

Notes: Significance was calculated using robust standard errors: ***Significant at 1%; **Significant at 5%; *Significant at 10%. See text for the subset of states for which data is available. Source for CBR: U.S. Public Health Service (1947). Years included in the estimation: 1915-1921.

As can be seen in Table 1.18 above, the average crude birth rate in 1921 was 24.2 births per 1,000 population. Table 1.19 displays the results for the effect of lagged prohibition on the crude birth rate. The results show that, for the limited sample of states included in the estimation, prohibition is statistically significant at five percent in reducing the crude birth rate by 0.74 births per 1,000 population, or three percent when compared to the mean for 1921. Prohibition, or the limited availability of alcohol, was likely reducing the crude birth rate by decreasing occurrences of unintended pregnancies. In this way, prohibition was generating cohorts that were more planned—women were consciously having children in more optimal conditions. In the spirit of Donohue and Levitt (2001), prohibition might have been influencing adult labor market outcomes of cohorts in early development at the time through this mechanism of generating cohorts that were more wanted.

## 1.8 CONCLUSION

The state prohibitions of alcohol had a positive impact on early development environments. Although not perfectly, state prohibitions limited the availability of alcohol and therefore reduced Fetal Alcohol Exposure, improved household safety and nurturance, created cohorts that were more planned, and increased the amount of resources available to the expectant mother and child. By improving these early development environments, state prohibitions had an unintended long-term impact; they affected adult labor market outcomes of female cohorts in early development during the time.

The main estimation in this paper shows that female cohorts in early development during state prohibitions had increased total income by eleven percent, significant at the ten percent level and increased labor force participation by eight percent, significant at the five percent level. These results for total income for female cohorts are highly robust to alternative specifications, but not to the inclusion of state specific time trends. While the trends could be capturing important omitted variables that vary in the same manner as the state prohibitions of alcohol, they also absorb most of the variation in the outcome variable and exacerbate the problem of attenuation bias. The results for females for LFP are also highly robust to alternative specifications, and also to the inclusion of state specific time trends in the specification using indicator variables. These results for females appear to be driven mostly by *in utero* exposure to state prohibitions. No significant effects were found for male cohorts in early development during state prohibitions. The absence of significant findings for male cohorts could be explained by the selective prenatal mortality of the weakest members of male cohorts unexposed to state prohibition, known as the culled cohort hypothesis. In addition, due to the measurement error associated with local option laws, it is likely that the effects found for female cohorts are lower bounds of the impact of prohibition.

These findings have important policy implications—investing in fetal and early childhood health has significant long-term effects and may critically alter the developmental branch of affected individuals. Since developmental biologists argue that it becomes increasingly difficult to alter a developmental trajectory with the passing of time—an argument that is corroborated by the science of neurological development—policy should be geared towards improving the earliest period of life. Although this paper does not recommend a return to prohibition, policy advising parents to eliminate or reduce alcohol consumption during their children's early developmental period could lead to improved long-term outcomes.

**REFERENCES**


Aaron, Paul and David Musto. (1981). "Temperance and Prohibition in America: A Historical Overview." In *Alcohol and Public Policy: Beyond the Shadow of Prohibition*, eds. Mark H. Moore and Dean R. Gerstein. Washington D.C.: National Academy Press: 127-181.

Abel, Ernest. (1997). "Maternal Alcohol Consumption and Spontaneous Abortion." *Alcohol and Alcoholism*, 32(3): 211-219.

Anti-Saloon League. (1914). *Proceedings, Fifteenth National Convention of the Anti-Saloon League of America*. Westerville: American Issue Publishing House.

Adair, Linda S. and Andrew M. Prentice. (2004). "A Critical Evaluation of the Fetal Origins Hypothesis and its Implications for Developing Countries." *The Journal of Nutrition*, 134: 191-193.

Almond, D. (2006). "Is the 1918 Influenza Pandemic Over? Long-term Effects of In-utero Influenza Exposure in the Post-1940 U.S. Population." *Journal of Political Economy*, 114 (August): 612-712.

Almond, D., L. Edlund and M. Palme. (2007). "Chernobyl's Subclinical Legacy: Prenatal Exposure to Radioactive Fallout and School Outcomes in Sweden." *NBER Working Paper* No. 13347.

Almond, Douglas and Janet Currie. (2011). "Human Capital Development Before Age 5." *Handbook of Labor Economics*, Volume 4: 1315-1486.

Angrist, Joshua and Jorn-Steffen Pischke. (2009). *Mostly Harmless Econometrics.* Princeton: Princeton University Press.

Baars, B and NM Gage. (2007). *Cognition, Brain And Consciousness: An Introduction To Cognitive Neuroscience*. London: Elsevier Academic Press.

Bader, Robert Smith. (1986). *Prohibition in Kansas*. Lawrence: University Press of Kansas.

Barrett, Garry F. (2002). "The Effect of Alcohol Consumption on Earnings." *The Economic Record*, 78(1): 79-96.

Berger, Mark C. and Paul Leigh. (1988). "The Effect of Alcohol Use on Wages." *Journal Applied Economics*, 20: 1343-1351.

Behr, Edward. (1996). *Prohibition*. New York: Arcade Publishing.

Behrman, Jere and Mark Rosenzweig. (2004). "Returns to Birthweight." *The Review of Economics and Statistics*, 86(2): 586-601.

Bertrand, Marianne, Esther Duflo and Sendhil Mullainathan. (2004). "How Much Should We Trust Difference-in-Differences Estimates?" *Quarterly Journal of Economics*, 119: 248-275.

Bijur, PE., M. Kurzon, M. Overpeck and P. Scheidt. (1992). "Parental alcohol use, problem drinking, and children's injuries." *AMA* 267: 3166-3171.

Black, S.E., P.J. Devereux and K.G. Salvanes (2007). "From the Cradle to the Labor Market? The Effect of Low Birth Weight on Adult Outcomes." *Quarterly Journal of Economics*, 122(1): 409-439.

Bleakley, Hoyt. (2007). "Disease and Development: Evidence from Hookworm Eradication in the South." *The Quarterly Journal of Economics*, 122(1): 73-117.

Blocker, Jack S., ed. (1979). *Alcohol, Reform and Society*. Westport: Greenwood Press.

Bouchery, Ellen E., Henrick J. Harwood, Jeffrey J. Sacks, Carol J. Simon and Robert D. Brewer. (2006). "Economic Costs of Excessive Alcohol Consumption in the U.S." *American Journal of Preventive Medicine*, 41(5): 516-524.

Bourgeois, J.P. (2001). "Synaptogenesis In The Neocortex Of The Newborn, The Ultimate Frontier For Individuation?" In *The Handbook of Developmental Cognitive Neuroscience*, eds. Charles Nelson and Monica Luciana. MIT Press, Cambridge: 23-33.

Bray, Jeremy W. (2005). "Alcohol Use, Human Capital, and Wages." *Journal of Labor Economics*, 23(2): 279-312.

Brunn, Stanley and Thomas Appleton Jr. (1999). "Wet-Dry Referenda in Kentucky and the Persistence of Prohibition Forces." *Southeastern Geographer,* 39(2): 172-189.

Bruckner, T.A. and J. Nobles. (2013). "Intrauterine Stress and Male Cohort Quality: the Case of September 11, 2001." *Social Science Medicine*, 76(1): 107-114.

Byrne, J., Warburton, D., Opitz, J. M. and Reynolds, J. F. (1987). "Male Excess Among Anatomically Normal Fetuses in Spontaneous Abortions." *American Journal Medical Genetics*, 26: 605–611.

Card, David. (1999). "The Causal Effect of Education on Earnings." *Handbook of Labor Economics*, 3(30): 1801-1863.

Carpenter, Christopher. (2007). "Heavy Alcohol Use and Crime: Evidence from Underage Drunk-Driving Laws." *Journal of Law and Economics*, 50(3): 539-557.

Carpenter, Christopher and Carlos Dobkin. (2009). "The Effect of Alcohol Consumption on Mortality: Regression Discontinuity Evidence from the Minimum Drinking Age." *American Economic Journal: Applied Economics*, 1(1): 164-182.

Carpenter, Christopher and Carlos Dobkin. (2011). "Alcohol Regulation and Crime." in *Controlling Crime: Strategies and Tradeoffs*, eds. Philip J. Cook, Jens Ludwig, and Justin McCrary. Chicago: University of Chicago Press: 291-329.

Case, Anne, Angela Fertig and Christina Paxson. (2005). "The Lasting Impact of Childhood Health and Circumstance." *Journal of Health Economics*, 24: 365-389.

Casey, B. J., N. Tottenham, C. Liston and S. Durston. (2005). "Imaging The Developing Brain: What Have We Learned About Cognitive Development?" *Trends in Cognitive Science*, 9(3): 104–110.

Catalano, Ralph and Tim Bruckner. (2006). "Secondary Sex Ratios and Male Lifespan: Damaged or Culled Cohorts." *Proceedings of the National Academy of Sciences of the United States*, 103(5): 1639-1643.

Cherrington, Ernest. (1920a). *The Evolution of the Prohibition in the United States of America*, Westerville: American Issue Press.

Cherrington, Ernest, ed. (1920b). *The Anti-Saloon League Yearbook*, Westerville: The Anti-Saloon League of America.

Chesson, Harrell, Paul Harrison and William J. Kassler. (2000). "Sex Under the Influence: The Effect of Alcohol Policy on Sexually Transmitted Disease Rates in the United States." *Journal of Law and Economics,* vol. XLIII.

Clark, Norman H. (1976). *Deliver Us From Evil: An Interpretation of American Prohibition*. New York: W .W. Norton & Company.

Clark, Norman. (1988). *The Dry Years*. Seattle: University of Washington Press.

Costa, Dora. (1998). "The Evolution of Retirement." In *The Evolution of Retirment: An American Economic History, 1880-1990,* ed. Dora Costa. Chicago: University of Chicago Press: 6-31.

Crowe, Charles. (1968). “Racial Violence and Social Reform—Origins of the Atlanta Riot of 1906.” *The Journal of Negro History,* 53(3): 234-256.

Currie, Janet and Erdal Tekin. (2006). “Does Child Abuse Cause Crime?” *NBER Working Paper* 12171.

Currie, Janet and Cathy S. Widom. (2009). “Long-Term Consequences Of Child Abuse And Neglect On Adult Economic Well-Being.” Manuscript, September 2009.

Currie, Janet, Mark Stabile, Phongsack Manivong, and Leslie L. Roos. (2009). “Child Health and Young Adult Outcomes.” *NBER Working Paper* 14482.

Currie, Janet and Brigitte Madrian. (1999). “Health, Health Insurance and the Labor Market.” *Handbook of Labor Economics*, 3(50): 3310-3416.

Dee, Thomas S. and William N. Evans (2003). “Teen Drinking and Educational Attainment: Evidence from Two-Sample Instrumental Variables Estimates.” *Journal of Labor Economics*, 21(1): 178-209.

Dills, Angela, Mireille Jacobson and Jeffrey Miron. (2004). “The Effect of Alcohol Prohibition on Alcohol Consumption: Evidence from Drunkenness Arrests.” *Economics Letters*, 86: 279-284.

Dills, A. and J. Miron. (2003). Alcohol Prohibition and Cirrhosis. *NBER Working Paper* No. 9681.

Donohue, John J., III, and Steven D. Levitt. (2001). “The Impact of Legalized Abortion on Crime.” *The Quarterly Journal of Economics,* 116(2): 379–420.

Donohue, John J., III, and Steven D. Levitt. (2008). “Measurement Error, Legalized Abortion, and the Decline in Crime: A Response to Foote and Goetz.” *The Quarterly Journal of Economics,* 123(1): 425–440.

Edwards, Ellen, Rina D. Eiden, and Kenneth E. Leonard. (2004). “Impact Of Fathers’ Alcoholism And Associated Risk Factors On Parent–Infant Attachment Stability From 12 To 18 Months.” *Infant Mental Health Journal*, 25(6): 556–579.

Eustace, Larry, Duck-Hee Kang and David Coombs. (2006). “Fetal Alcohol Syndrome: A Growing Concern for Healthcare Professionals.” *Journal of Obstetric, Gynecologic and Neonatal Nursing*, 32(2): 215-221.

Fisher, Irving. (1930). *The Noble Experiment*. New York: Alcohol Information Committee.

Franklin, Jimmie. (1971). *Born Sober*, Norman: University of Oklahoma Press.

Goldin, Claudia. (1986). "The Female Labor Force and American Economic Growth, 1890-1980." In *Long-Term Factors in American Economic Growth*, eds. Stanley Engerman and Robert Gallman. Chicago: University of Chicago Press: 557-604.

Gottesfeld, Zehava and Ernst Abel. (1991). "Maternal and Paternal Alcohol Use: Effects on the Immune System of the Offspring." *Life Sciences*, 48: 1-8.

Gusfield, Joseph R. (1968). "Prohibition: The Impact of Political Utopianism." In *Change and Continuity in Twentieth Century America: The 1920s*, ed. John Braeman. Columbus: Ohio State University Press: 257-308.

Hamilton, V. and B.H. Hamilton. (1997). "Alcohol and Earnings: Does Drinking Yield a Wage Premium?" *Canadian Journal of Economics,* 30(1): 135-151.

Hanes Walton, Jr. and James E. Taylor. (1971). "Blacks and the Southern Prohibition Movement." *Phylon*, 32(3): 247-259.

Hanson, David J. (1995). *Preventing Alcohol Abuse: Alcohol, Culture and Control.* Westport: Praeger.

Hazel, Sybal. (1942). *Statewide Prohibition Campaigns in Texas* (Unpublished Dissertation). Texas Technological College, Lubbock.

Henley, Lillian, ed. (1919). *Bulletin of Public Affairs Information Services.* New York: H.W. Wilson Co.

Huttenlocher, PR and AS Dabholkar. (1997). "Regional Differences in Synaptogenesis in Human Cerebral Cortex." *Journal of Comparative Neurology,* 387: 167-178.

Isaac, Paul. (1965). *Prohibition and Politics*. Knoxville: University of Tennessee Press.

Jones, Kenneth and David Smith. (1973). "Recognition of the Fetal Alcohol Syndrome in Early Infancy." *The Lancet,* 302(7836): 999-1001.

Kenkel, D.S. and D.C. Ribar. (1994). "Alcohol Consumption and Young Adults' Socioeconomic Status." *Brookings Papers on Economics Activity: Microeconomics*: 119-161.

Kline, Jennie, Zena Stein, Patrick Shrout, Mervyn Susser and Dorothy Warburton. (1980). "Drinking During Pregnancy and Spontaneous Abortion." *The Lancet*, 316(8187): 176-180.

Kobler, John. (1973). *Ardent Spirits*. New York: G. P. Putnam's Sons.

Kyvig, David E. (1979). *Repealing National Prohibition*. Chicago: University of Chicago Press.

Lacy, Eric Russell. (1965). "Tennessee Teetotalism: Social Forces and the Politics or Progressivism." *Tennessee Historical Quarterly*, 24(3): 219-240.

Levine, Harry G. (1985). "The Birth of American Alcohol Control: Prohibition, the Power Elite and the Problem of Lawlessness." *Contemporary Drug Problems,* Spring: 63-115.

Levine, Harry and Craig Reinarman. (2004). *Alcohol Prohibition and Drug Prohibition. Lessons from Alcohol Policy for Drug Policy*. Amsterdam: CEDRO.

Levitt, Steven D. and Jack Porter. (2001). "How Dangerous Are Drinking Drivers?" *Journal of Political Economy*, 109(6): 1198-1237.

Lewis, Michael. (2008). "Access to Saloons, Wet Voter Turnout, and Statewide Prohibition Referenda, 1907-1919." *Social Science History*, 32(3): 373-404.

Lewis, Michael. (2009). "Cultural Norms and Political Mobilization: Accounting for Local and State-Level Liquor Laws, 1907-1919." *Journal of Cultural Geography*, 24(2): 31-52.

Markowitz, Sara. (2000). "The Price of Alcohol, Wife Abuse and Husband Abuse." *Southern Economic Journal*, 67(2): 279-303.

Markowitz, S. and M. Grossman. (2000). "The Effects of Beer Taxes on Physical Child Abuse." *Journal of Health Economics*, 19: 271–282.

McCarty, Jeanne. (1980). *The Struggle for Sobriety*. El Paso: Texas Western Press.

Miron, Jeffrey and Zwiebel, Jeffrey. (1991). "Alcohol Consumption During Prohibition." *The American Economic Review*, 81(2): 242-247.

Naimi, Timothy, Leslie E. Lipscomb, Robert D. Brewer and Brenda Colley Gilbert. (2003). "Binge Drinking in the Preconception Period and the Risk of Unintended Pregnancy: Implications for Women and Their Children." *Pediatrics*, 111(1): 1136-1141.

Nelson, Charles A. (2000). "The Neurological Basis of Early Intervention." In *Handbook of Early Childhood Intervention*, eds. Jack Shonkoff and Samuel Meisels. Cambridge: Cambridge University Press: 204-230.

New York Times. (1907). "Prohibition for Georgia." (July 31)..

New York Times. (1915). "Prohibition Gains 4 States this Year." (June 22)..

Nilsson, J Peter. (2008). "Does a Pint a Day Affect your Child's Pay? The Effect of Prenatal Alcohol Exposure on Adult Outcomes." *IFAU Institute for Labour Market Policy Evaluation Working Paper*.

Okrent, Daniel. (2010). *Last Call: The Rise and Fall of Prohibition*. New York: Scribner.

Olson, Heather, Mary J. O'Connor and Hiram E. Fitzgerald. (2001). "Lessons Learned From the Study of the Developmental Impact of Parental Alcohol Use." *Infant Mental Health Journal*, 22(3): 271–290.

Ostrander, Gilman. (1957). *The Prohibition Movement in California.* Berkeley: University of California Press.

Pickett, Deets, Clarence Wilson and Ernest Smith, eds. (1917). *The Cyclopedia of Temperance, Prohibition and Public Morals.* New York: Methodist Book Concern.

Plant, Moira. (1997). *Women and Alcohol*. London: Free Association Books.

Renna, Francesco. (2007). "The Economic Cost of Teen Drinking: Late Graduation and Lowered Earnings." *Health Economics*, 16: 407-419.

Ruggles, Steven, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. (2010). *Integrated Public Use Microdata Series: Version 5.0* [Machine-readable database]. Minneapolis: University of Minnesota.

Schenck-Gustafsson, Karin, Paula DeCola, Donald Pfaff, and David Pisetsky, eds. (2012). *Handbook of Clinical Gender Medicine."* Basel: S. Karger AG.

Shaw, P., D. Greenstein, J. Lerch, L. Clasen, R. Lenroot, N. Gogtay, A. Evans, J. Rapoport, and J. Giedd. (2006). "Intellectual Ability and Cortical Development in Children and Adolescents" *Nature*, 440: 676–679.

Sinclair, Andrew. (1962). *Prohibition: The Era of Excess*. New York: Little Brown.

Sournia, Jean-Charles, ed. (1990). *A History of Alcoholism*. Trans. by Nick Hindley and Gareth Stanton. Oxford: Basil Blackwell.

Spiegler, Danielle. (1985). “American Indians and Alaska Natives.” In *Alcohol Use among U.S. Ethnic Minorities*. Washington, DC: US Department of Health and Human Services.

Szymanski, Ann-Marie. (2003). “Beyond Parochialism: Southern Progressivism, Prohibition, and State-Building.” *The Journal of Southern History*, 69(1): 107-136.

Thornton, Mark. (1991). *The Economics of Prohibition*. Salt Lake City: University of Utah Press.

Timberlake, James. (1963). *Prohibition and the Progressive Movement*. Cambridge: Harvard University Press.

Trivers, R. L. and D. E. Willard. (1973). "Natural Selection of Parental Ability to Vary the Sex Ratio of Offspring." *Science,* 179(4068): 90–92.

U.S. Public Health Service. (1947). *Vital Statistics Rates in the United States, 1900-1940.* Washington, D.C.: Government Printing Office.

Waddington, C.H. (1957). *The Strategy of the Genes; a Discussion of Some Aspects of Theoretical Biology.* London: Allen & Unwin.

Warburton, Clark. (1932). *The Economic Results of Prohibition*. London: P. S. King and Son.

Weinberg, Naimah. (1997). “Cognitive and Behavioral Deficits Associated with Parental Alcohol Use.” *Journal of American Academy of Child and Adolescent Psychiatry,* 36:9: 1177-1186.

Wells, Jonathan. (2000). “Natural Selection and Sex Differences in Morbidity and Mortality in Early Life.” *Journal of Theoretical Biology*, 202: 65-76.

Whitener, Daniel Jay. (1945). *Prohibition in North Carolina, 1715-1945*. Chapel Hill: University of North Carolina Press.

Wickersham, George W. (1931). *Enforcement of the Prohibition Laws*. Washington D.C.: U.S. Government Printing.

Windham, Gayle, Laura Fenster and Shanna Swan. (1992). “Moderate Maternal and Paternal Alcohol Consumption and the Risk of Spontaneous Abortion.” *Epidemiology*, 3(4): 364-370.

Zimmerman, Jonathan. (1999). *Distilling Democracy: Alcohol Education in America's Public Schools, 1880-1925.* Lawrence: University Press of Kansas.

# CHAPTER 2:
# A Matter of Time:
# An Impact Evaluation of the Poverty Alleviation Line of the Brazilian National Land Credit Program

## 2.1 INTRODUCTION

At the beginning of the 21st century, the rural areas of developing countries were home to nearly 900 million people living on less than one dollar per day, and over two billion people living on less than two dollars per day (World Bank, 2007). Households are more likely to be chronically poor when they have low levels of assets (Bird et al., 2002; Carter and Barrett, 2006). Policies that facilitate access to land—one of the most important assets in rural areas—may be able to assist poor rural households to develop and eventually sustain a non-poor standard of living. While important, land acquisition by itself is often insufficient to eradicate poverty; supporting infrastructure, access to credit, technology, and markets are also essential in order to elevate asset returns (Deininger, 1999). One program that provides beneficiaries with subsidized loans to purchase land from willing sellers, as well as assistance with complementary investments, is the Brazilian National Land Credit Program (*Programa Nacional de Crédito Fundiário*, PNCF). Between 2002 and 2012, the PNCF had over 90,000 beneficiaries. This paper provides an impact evaluation of this program.

There are few impact evaluations of land transfer programs, and the debate surrounding their effectiveness has been highly politicized (Deere and Medeiros, 2007). In an evaluation of South Africa's Land Redistribution for Agricultural Development program, Keswell and Carter (2014) provide one of the most credible studies. They conclude that living standards initially decrease with land transfers, but after three years of land ownership, living standards increase by fifty percent. We seek to contribute to this debate by providing evidence from a similar program in a different part of the world.

Because we utilize panel data to evaluate this program, in contrast to the single cross-section used for the South Africa study, fewer assumptions are required to address potential concerns caused by the unobservable characteristics of the participants.

This paper evaluates the Rural Poverty Alleviation line (*Combate à Pobreza Rural*, CPR) of the PNCF on the outcome variables of agricultural production and earned income, using both difference in differences and individual fixed effects models. Because beneficiaries acquired land at different times, the heterogeneous effect of additional years of land ownership is investigated. The paper uses a panel dataset from 2006 and 2010 of beneficiaries randomly selected from program participants and a control group randomly selected from the program's pipeline. Because both treatment and control groups applied to the program, and were verified to be eligible, the use of a pipeline control group helps to reduce concerns over unobservable differences between the two groups. Concerns related to the influence of unobservables are further tested by the inclusion of a proxy for the "eagerness" of groups in applying to the program (Agüero et al., 2009). Finally, the use of a fixed effects model removes unobservable individual characteristics that are time invariant. While panel data has many advantages, there was also considerable attrition in this panel. Attrition tests provide mixed evidence on whether or not it was random. Given the possibility of non-random attrition, the models were re-estimated with weights to correct for attrition. The results of the paper were only strengthened.

The paper finds that the Poverty Alleviation Line of the Brazilian National Land Credit Program (henceforth PNCF-CPR) has a significant impact on the outcome variables of program participants. Yet the benefits of land ownership only start to appear after a certain amount of time. While there is no statistically significant impact on agricultural production or earned income in the first three years of land ownership, after five to six years of program participation, production and income rise by 102 percent and 35 percent respectively. These are important gains for households living at around US$2 per day, most of whom qualify for the *Bolsa Família* conditional cash transfer program in Brazil. Because the PNCF-CPR program requires the repayment of the loan, however, a more complete evaluation of its effectiveness in reducing rural poverty must take the burden of the debt into consideration. When this is done, the results suggest that the benefits of the program largely go to making debt payments and improving the net worth of the beneficiary households rather than to raising current household expenditures. If the beneficiaries' income continues to grow at the rate observed in the first five to six years of land ownership, it is likely that they will soon reach a level at which they can simultaneously meet their debt obligations and raise their standard of living.

In Section 2.2 of the paper, background information on the program and dataset are provided. The methodology is described in Section 2.3 and descriptive statistics are presented in Section 2.4. Section 2.5 contains a discussion of the main econometric results. Section 2.6 provides a battery of robustness checks, including an analysis of attrition. Section 2.7 analyzes beneficiaries' ability to repay the PNCF loan, and Section 2.8 offers conclusions.

## 2.2 BACKGROUND AND DATA

Market Assisted Land Reform (MALR) began as a pilot project in Colombia in 1994. It was then implemented in South Africa, Brazil, Honduras, El Salvador, Guatemala, Mexico, Malawi, and the Philippines. MALR was first implemented in an experimental fashion in Brazil in 1997 as a joint effort of the World Bank and the Brazilian government. Due to the success of earlier projects, the PNCF was created in 2003. The program functions by providing subsidized loans to poor farmers to purchase land from willing sellers. There are two main lines of credit within the PNCF, each of which is aimed at different target populations. The analysis in this paper is limited to the Rural Poverty Alleviation line (CPR), in order to assess the ability of the program to reduce poverty in the Northeast of Brazil. This is the poorest region of the country, and over half of the rural poor reside there. By 2012, this line of the program had 48,000 beneficiaries.[16]

The PNCF-CPR aims to promote access to land and to provide infrastructure on the acquired lands. There are a series of eligibility requirements for enrollment, including earning less than R$9,000 (US$5,049) per year, having assets totaling no more than R$15,000 (US$8,415), not owning enough land to sustain a family, and having at least five years of experience as a farmer.[17] Individuals apply to this line of the program by forming an association with other interested individuals. Once all of the eligibility requirements are verified, the eligibility of the land intended for purchase is checked. The most important eligibility criteria with respect to the property are that it not be eligible for expropriation through state led land reform, and that the property's price be similar to

---

[16] The other line of the PNCF is the Consolidation of Family Farming (CAF), which has a higher income cap for eligibility than the CPR. Another important difference is that CAF makes individual loans, while CPR makes group loans. CAF has been more important in other regions of the country.

[17] The values are according to the CPR Manual of 2009 and the dollar values were calculated with the January 2010 exchange rate of R$1 to US$0.561.

those of other properties in the same region, using the Ministry of Agrarian Development's Land Market Monitoring System as a guide.

After ensuring that both the association and the land meet the eligibility requirements, a productive project for the land is analyzed and the loan is approved or rejected. The maximum amount of the loan per beneficiary was R$40,000 (US$22,440) in 2009, however, each region of the country had different caps associated with local market prices. In addition to 14-17 year loans made for the purchase of land, the program makes infrastructure grants available to the association, which can be used to build houses and community infrastructure, or to purchase capital for agricultural production. In an effort to create an incentive for the land price to be negotiated as low as possible, the R$40,000 cap applies to the sum of the grant and loan. Thus, the smaller the loan component, the larger is the grant component. After the acquisition of the land, technical assistance is provided.

In 2012, the Ministry of Agrarian Development published a report in Portuguese of an impact evaluation of the PNCF (Sparovek, 2012). That evaluation used a similar dataset as this paper, but a different methodology throughout. The authors used propensity score matching together with a difference in differences approach. Unlike the results presented here, they found no impact on monthly monetary income or gross agricultural production. The differing results the reader will find below are most likely due to the fact that they did not distinguish the heterogeneous effect of additional years of land ownership, nor did they systematically account for the changing number of family members over the two waves of the survey. Differences in the datasets used could also matter. Our sample has 39% percent more observations, largely because we do not exclude the non-beneficiaries who became beneficiaries between the two waves of the panel.

The dataset used in this impact evaluation is a two period panel, collected in 2006 and 2010. We were involved in the creation of the questionnaires used in both periods, in addition to the data collection process in the second period. The data were thoroughly cleaned to ensure that no observations were wasted.[18] The treatment group of this dataset was randomly selected from members of beneficiary associations through stratified random sampling, by municipality, association and member. We call these beneficiaries (B). The control group was drawn from members of associations in the program pipeline—those that were enrolled in the program, were deemed eligible as program participants, but had not yet acquired their land. These pipeline non-beneficiaries (PNB) were selected from the same or neighboring municipalities as the randomly selected projects of beneficiaries. As will be explained below, many pipeline non-beneficiaries acquired land between the baseline and follow-up periods, and thus transitioned into the treatment group.

In the baseline period, the reference period for beneficiaries was the twelve months *prior to the acquisition of land*, which changed from project to project. In order to minimize potential measurement error due to recall, the universe of projects that was used to sample from was restricted to those projects that had been created in the thirteen

---

[18] For example, if sex was missing but name was not missing, sex could be inferred from the name since Brazilian names are generally unambiguous with respect to sex. Similarly, if the land purchase date was missing for one beneficiary but not for others in the same association, the missing value could be corrected.

months prior to the fieldwork in 2006. For the pipeline non-beneficiaries in both the baseline and the follow-up, and for the beneficiaries in the follow-up, the reference period was the twelve months *prior to the interview*.

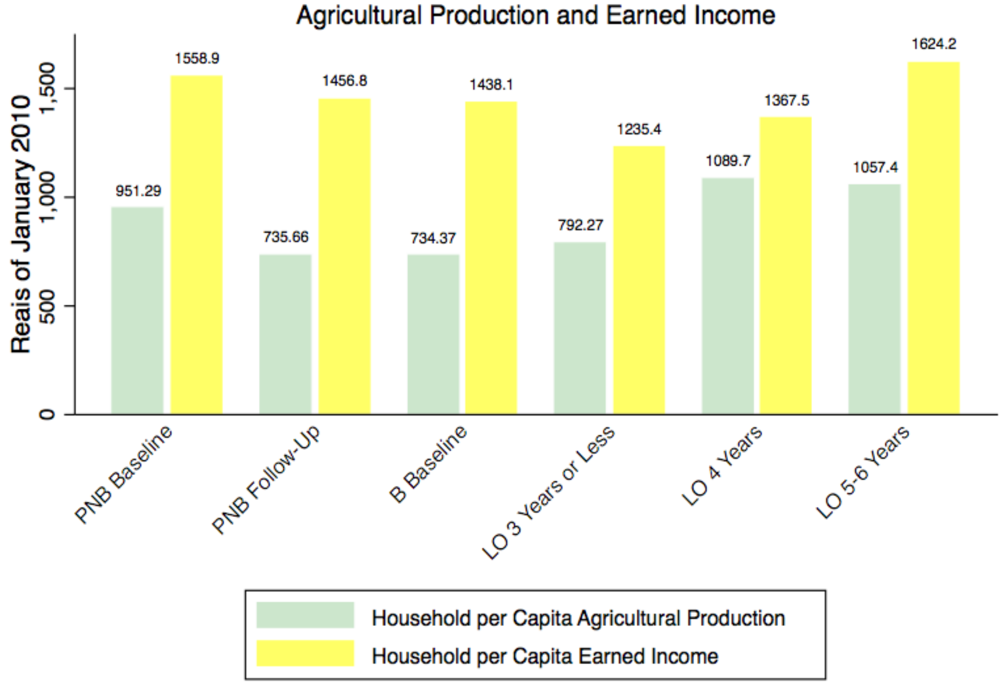
The original sample had 1335 households; of these, about 42% attrited. In order to take advantage of the panel nature of the data, the main analysis in this paper uses a sample of 773 households that were interviewed in both periods. Attrition is subsequently dealt with in detail in the section on robustness checks. When weighted regressions are estimated to correct for attrition bias, the main results in the paper are only strengthened.

The balanced panel has 367 pipeline non-beneficiaries and 406 beneficiaries in the baseline period. By the time of the follow-up period, 162 of the pipeline non-beneficiaries had acquired land. Because of this expected change, the final count of pipeline non-beneficiaries is 205, and the final count of beneficiaries (defined as having been observed to acquire land during the sampling period) is 568. Because different associations of beneficiaries acquired land at different times, we explore the impact of the duration of exposure to treatment on outcomes. For this analysis, we divide the beneficiaries in the follow-up period into groups based on the number of years of land ownership. Specifically, in the follow-up period three groups are defined: beneficiaries with three or less years of land ownership, beneficiaries with four years of land ownership and beneficiaries with five or six years of land ownership.

The number of people in member households was found to be decreasing significantly between the baseline and the follow-up period. In the baseline period, both pipeline non-beneficiaries and beneficiaries had between 4.7 and 4.8 people per household. In the follow-up period, the pipeline non-beneficiaries had 4.5 people per household while the beneficiaries had 3.9. Because of these changes, it is important to use outcome variables measured in household per capita (HHPC) units. The primary outcome variables that were analyzed were earned income and agricultural production, which will be defined in detail in Section 2.4. In all cases, the income and production variables were deflated to Reais of January of 2010. Figure 2.1, below, displays mean agricultural production and earned income, in HHPC units, by period, status and number of years of land ownership. This simple analysis of means displays an important pattern that this paper addresses: the positive—or U-shaped—relationship between the value of outcome variables and the number of years of land ownership.

Figure 2.1: Mean of Outcome Variables

Agricultural Production and Earned Income



## 2.3 EMPIRICAL STRATEGY

In this section, a description of the methodology is presented. Two models are estimated: Difference-in-Differences (with the fixed effects at the level of the group) and individual Fixed Effects (with the fixed effects at the level of the individual enrolled member). Using these models, we additionally incorporate an analysis allowing for the heterogeneity from additional years of land ownership. While the individual FE model is superior, we estimate the DD model for comparison, and because a number of robustness checks can only be estimated in this framework.

When attempting to evaluate the impact of a program, an important problem to address is selection bias. If a program is not randomly assigned, one can assume that individuals who are more eager, able or otherwise more likely to benefit from a program will apply. A possible income increase following participation in the program might then be attributable, at least in part, to the qualities of the applicants, as opposed to the effectiveness of the program. We employ three different strategies in an effort to avoid this bias and arrive at the causal impact of the program.

First, by using a pipeline control group (Ravallion, 2008), *application* to the program is held constant across treatment and control groups. In principle, any unobserved characteristics that motivate people to apply to the program are held constant across both groups, thereby reducing selection bias. In addition, since the program depends on individuals forming groups in order to acquire the loan for land purchase, it is likely that variation of unobservables across individuals within each group will increase the degree of randomness of the treatment. There is still concern that there might be some unobserved characteristics of individuals that influence the timing of application or, given application, influence whether or not they will receive land. As shown below, there is suggestive evidence that receiving land after enrolling in the program appears to be random. Thus, unobserved characteristics do not appear to influence the timing of the acquisition of land. In the robustness checks section, we also include a proxy for the eagerness of beneficiaries, to assess whether early applicants to the program were perhaps more motivated for success. The analysis serves as additional evidence that it is, in fact, land acquisition and not unobserved characteristics of enrolled members that are driving the results.

Second, a Difference-in-Differences (DD) estimation technique is used in order to remove any time-invariant differences between the treatment and control groups, and also to eliminate time-trends that are common to both groups. The DD technique has been useful in estimating the causal impact of policy interventions in numerous studies by modeling the fixed effects at the level of the group. The approach entails estimating the equation:

$$Y_{ist} = \alpha+\beta T_t+\gamma S_s+ \pi(T^*S)_{st}+\delta X_{ist}+\varepsilon_{ist} \tag{2.1}$$

where Y is either agricultural production or earned income, T is an indicator variable that equals one in the follow-up period, S is an indicator variable that equals one if the enrolled member has acquired land, T*S is an indicator variable that equals one if *both* T is equal to one *and* S is equal to one, and X is a vector of control variables. In addition, i, s and t index individuals, status of treatment (beneficiary or pipeline non-beneficiary) and time (baseline or follow-up period). The estimate of the effect of acquiring land via the

PNCF-CPR, then, is $\pi$. In order to avoid possible bias, potentially endogenous time varying controls are kept at baseline levels. The DD identification strategy relies on $E(\varepsilon_{ist}|S, T, X_{ist})=0$. In other words, there can be no omitted factors that are causing both the growth in the outcome variable and the treatment status.

Third, another way of dealing with the issue of selection bias is to difference out individual level unobserved characteristics that are fixed through time by using a fixed effects (FE) model. The error term $\varepsilon_{it}$ can be decomposed into time-invariant ($u_i$) and time-varying ($\eta_{it}$) components. Thus we have:

$$Y_{it} = \alpha_t + \pi L_{it} + \delta X_{it} + u_i + \eta_{it} \tag{2.2}$$

where $\eta$ is normally distributed, and L is an indicator variable that equals one if the enrolled member received land before the follow-up period. If we lag this equation by one period and take the difference between the two, we have:

$$\Delta Y_i = \Delta\alpha + \pi\Delta L_i + \delta\Delta X_i + \Delta\eta_i \tag{2.3}$$

In this way, the time invariant unobservable characteristics at the individual level are differenced out (Wooldridge, 2002). Using an individual fixed effects model can result in a more accurate estimate of the impact of treatment because assignment into treatment is more likely to be random given the removal of time invariant unobserved and observed characteristics *and* the pipeline control group *and* the group nature of the program design. Because of this, the FE model is the preferred specification as it is most likely to arrive at the causal impact of the program.

In the techniques mentioned so far, treatment was modeled with a binary variable. The implicit assumption was that the average impact of treatment was the same for all beneficiaries of the program. However, as mentioned in the previous section, different associations obtained land at different times. One can suppose that the intensity of treatment increases with the amount of time a member is exposed to treatment, thus leading to a greater impact (King and Behrman, 2009). One approach to allow for heterogeneity by year of land acquisition is to estimate an intensity of treatment DD model:

$$Y_{ist} = \alpha+\beta T_t+\gamma S_s+\pi_3 LO3_{ist}+\pi_4 LO4_{ist}+\pi_5 LO5_{ist}+\delta X_{ist}+\varepsilon_{ist} \quad (2.4)$$

Where LO3 is an indicator variable for land ownership for three years or less, LO4 is for land ownership for four years, and LO5 is land ownership for five to six years. These indicator variables measure the effect of increasing years of land ownership without assuming that the impacts increase in a linear fashion. For the FE model, the equivalent intensity of treatment equation is:

$$Y_{it} = \alpha_t+ \pi_3 LO3_{it}+ \pi_4 LO4_{it}+ \pi_5 LO5_{it} +\delta X_{it}+u_i+\eta_{it} \quad (2.5)$$

The standard errors for the regression coefficients throughout the paper are calculated with corrections for clustering to allow for the possibility of heteroskedasticity across projects or correlation of errors across time within a geographical region. The errors were clustered at the level of the project for beneficiaries and many pipeline non-beneficiaries, and at the level of the municipality for pipeline non-beneficiaries when the project code—that could uniquely identify an association—was missing.[19] There were a total of 200 clusters.

## 2.4 DESCRIPTIVE STATISTICS AND VALIDITY OF THE PIPELINE

The outcome variable agricultural production was defined as the total value of agricultural production (including animal production), whether sold, stocked, exchanged or consumed. As can be observed in Table 2.1, beneficiaries had less agricultural production than pipeline non-beneficiaries in the baseline period, significant at five percent. In the follow-up period, beneficiaries that had owned land for four or more years had substantially more agricultural production than the remaining pipeline non-beneficiaries. Thus, their productive opportunities appear to have increased as a result of land ownership. Earned income, the second outcome variable, was defined as the value of net agricultural production—total agricultural production minus variable costs—plus income earned in the labor market and from self-employment activities. There is no statistical difference between the average earned income of pipeline non-beneficiaries and beneficiaries in either period, regardless of the length of land ownership.

**Table 2.1: Mean Production and Income Variables by Beneficiary Status and Number of Years of Land Ownership**

| | Baseline Period | | Follow-up Period | | | |
|---|---|---|---|---|---|---|
| | **PNB** | **B** | **PNB** | **B≤3 Years** | **B 4 Years** | **B 5-6 Years** |
| Agricultural Production | 951.29 | 734.37** | 735.66 | 792.27 | 1089.65** | 1057.36* |
| Earned Income | 1558.87 | 1438.06 | 1456.81 | 1235.37 | 1367.50 | 1624.16 |

Notes: Stars indicate mean is different from PNB group by period. ***Significant at 1%; **Significant at 5%; *Significant at 10%. All values in household per capita Reais of January 2010. Pipeline non-beneficiary is abbreviated as "PNB" and beneficiary as "B." In the follow-up period, the beneficiaries are grouped according to the number of years of land ownership.

[19] Clustering all at the level of municipality results in slightly larger standard errors, but the levels of statistical significance remain the same.

Control variables were used to capture differences in the outcome variable that were due to baseline characteristics of the enrolled members as opposed to the acquisition of land via the PNCF-CPR. Basic demographic and location variables were used (age, sex, race, marital status and urban status), in addition to education and experience (years of schooling and years of experience as a farmer). As can be seen in Table 2.2, among these individual characteristics, beneficiaries statistically differ from the pipeline non-beneficiaries only in sex composition and urban status—the beneficiary group being more female and less urban than the pipeline non-beneficiary group. These individual characteristics were included in the estimations since they might influence treatment status and the outcome variables.

**Table 2.2: Mean Control Variables by Beneficiary Status**

| | Baseline Period | |
|---|---|---|
| | **Pipeline NB** | **Beneficiary** |
| **Individual Characteristics** | | |
| Age | 37.62 | 36.56 |
| Sex | 0.71 | 0.86*** |
| White | 0.20 | 0.17 |
| Married | 0.82 | 0.79 |
| Urban | 0.27 | 0.21* |
| Years of Schooling | 4.02 | 4.33 |
| Years of Experience | 23.37 | 22.70 |
| **Social Capital Variables** | | |
| Position Held | 0.42 | 0.57*** |
| Frequency of Meeting | 2.29 | 2.17* |
| Trust | 2.81 | 2.70** |
| **Individual Agricultural Variables** | | |
| Technical Assistance | 0.05 | 0.07 |
| PRONAF | 0.32 | 0.25* |
| **Regional Agricultural Variables** | | |
| Yield of Corn | 1.18 | 0.90*** |
| Daily Agricultural Wage | 12.81 | 12.22** |

Notes: Stars indicate mean of beneficiary group is different from pipeline non-beneficiary group. ***Significant at 1%; **Significant at 5%; *Significant at 10%.

Measures of baseline social capital in the association were used in an attempt to capture the effects of social capital on the outcome variables. More socially cohesive associations will likely be predictive of both participation in the program and the success of the eventual projects. The first social capital variable, position held, is an indicator variable that equals one if the member held a position in the leadership of the association. While the beneficiaries held more positions in their associations than the pipeline non-beneficiaries, the other social capital variables display the opposite pattern. Beneficiaries had fewer meetings and less trust in other association members. Frequency of meetings shows how frequently association members met, while trust is a variable that indicates the amount of trust that the enrolled member had in other association members. Since there was less social cohesiveness in the beneficiary group, it is unlikely that these variables explain their success.

Agricultural variables were also included since they may influence both treatment status and the outcome variables. Technical assistance and PRONAF are individual level agricultural variables that indicate whether enrolled members received technical assistance and whether they received additional loans from a credit program for family farmers. While technical assistance is statistically equivalent for both groups, pipeline non-beneficiaries did receive more family farming loans, which is consistent with their higher levels of agricultural production in the baseline. The regional agricultural variables included are yield of corn and daily agricultural wage. State level corn yields (tons/hectare) proxy for time varying geo-climactic characteristics.[20] Pipeline non-beneficiaries found themselves in areas with more favorable geo-climactic conditions; this is, once again, consistent with their higher levels of agricultural production in the baseline period. Pipeline non-beneficiaries also occupied areas that had higher agricultural wages in the baseline.

**Table 2.3: Comparison of Means within Pipeline**
*(Mean of New Beneficiaries - Mean of Remaining Non-Beneficiaries)*

| | |
|---|---|
| **Production and Income Variables** | |
| Agricultural Production | No Significant Difference |
| Earned Income | No Significant Difference |
| **Individual Characteristics** | |
| Age | No Significant Difference |
| Sex | Difference>0*** |
| White | No Significant Difference |
| Married | Difference<0* |
| School | No Significant Difference |
| Experience | Difference<0** |
| Urban | Difference<0* |
| **Social Capital Variables** | |
| Position Held | Difference>0* |
| Meet | Difference<0* |
| Trust | Difference<0* |
| **Individual Agricultural Variables** | |
| PRONAF | No Significant Difference |
| Technical Assistance | Difference>0** |
| **Regional Agricultural Variables** | |
| Daily Agricultural Wage | Difference<0*** |
| Yield of Corn | Difference<0*** |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%.

When using a pipeline control group, there should not be any unobserved characteristics that influence which enrolled members receive treatment after application (Ravallion, 2008; Angrist, 1998). While this is impossible to prove, a few basic tests serve as evidence that receiving land after applying for the PNCF-CPR loan appears to be

[20] Tons of corn and hectares harvested were obtained from the Brazilian Institute of Applied Economic Research (ipeadata.gov.br) to calculate yield of corn by state.

random. First, a comparison of means indicated that there is no statistically significant difference in the baseline between the outcome variables of pipeline non-beneficiaries that go on to acquire land and those non-beneficiaries that remain in the pipeline in the follow-up period (see Table 2.3). While some observables differences exist, these can be controlled for in the estimations. Second, probit regressions were run attempting to predict which pipeline non-beneficiaries go on to acquire land between the baseline and the follow-up periods. As can be seen in Table 2.4, where the dependent variable is an indicator variable that equals one if the member acquired land sometime between the baseline and follow-up period, earned income and agricultural production fail to significantly predict the movement into the treatment group, regardless of the inclusion of control variables.[21] Furthermore, it is likely to be the case that the timing of treatment was random at the level of the individual member because treatment occurred at the level of the association. As such, these tests provide suggestive evidence that unobserved characteristics of pipeline non-beneficiaries are not influencing which ones go on to receive land.

[21] The estimation in Table 2.4 is done with baseline values only and the sample is limited to the balanced panel of pipeline non-beneficiaries and non-beneficiaries who became beneficiaries after the baseline period. A separate estimation was run including attritors, and the results are equivalent.

**Table 2.4: Probits for the Probability of Acquiring Land Between the Baseline and Follow-up Periods**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Agricultural Production | -0.0001 | | -0.0000 | |
| | (0.0001) | | (0.0001) | |
| Earned Income | | -0.0000 | | 0.0001 |
| | | (0.0001) | | (0.0001) |
| N | 349 | 349 | 349 | 349 |
| State FE | N | N | Y | Y |
| Individual Controls[1] | N | N | Y | Y |
| Social Capital Controls[2] | N | N | Y | Y |
| Individual Agricultural Controls[3] | N | N | Y | Y |
| Regional Agricultural Controls[4] | N | N | Y | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include technical assistance and PRONAF. [4]Regional Agricultural controls include daily agricultural wage and yield of corn. The sample is all pipeline non-beneficiaries from the balanced panel in the baseline with the exception of 4 dropped because of missing independent variables, 6 dropped because they were outliers, and 8 observations for the state of Espirito Santo were dropped because they predicted failure completely.

## 2.5 ECONOMETRIC RESULTS

This section presents the results for the different specifications used. Outliers for each outcome variable were excluded from their respective regressions and were detected by plotting the residuals against the fitted values from the regressions.[22] The panel on the left of Tables 2.5 and 2.6 show the results for the difference in differences estimation. Time and status dummies, along with municipal fixed effects, were included in all specifications. The first column shows the results from a specification without any control variables, with the exception of municipal fixed effects. Starting with the second column, individual level controls are included—age, sex, race, marital status, years of

[22] In the case of agricultural production, ten outliers were detected, for a total of twenty observations dropped to maintain the balanced panel. In the case of earned income, nine of those ten were also outliers, plus an additional four for a total of thirteen, for a total of twenty-six dropped.

schooling, years of experience as a farmer and urban status—all kept at baseline levels. Baseline social capital controls are included in the third column and both individual and regional agricultural controls in the fourth. Because technical assistance and PRONAF loans reflect individual choices, they are kept at baseline levels. The daily agricultural wage and the yield of corn, in contrast, are allowed to vary over time. These variables are exogenous because they refer to geographical levels that are much larger than the individuals in the treatment and control groups. This last specification that utilizes all available controls is the preferred DD specification.

The individual fixed effects results are presented in the right panel of Tables 2.5 and 2.6. Since the individual fixed effects model is estimated by taking the first difference over time, only time varying control variables remain in the model. The first specification includes no control variables. In the second specification in column six, the difference in the daily agricultural wage and the difference in the yield of corn remain in the model in order to capture variation in the outcome variables that are due to time-varying characteristics of the surrounding environment. This second specification, with the inclusion of viable time-varying controls, is the preferred specification for the FE model. In fact, since the FE model differences out time invariant individual level unobserved characteristics, it is the preferred specification of the paper.

**Table 2.5: Effects on Agricultural Production from Land Ownership**

| | | Difference in Differences | | | | Fixed Effects | | |
|---|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | | (5) | (6) |
| **Binary** | Time*Status | 427.71** | 427.04** | 427.87** | 405.43** | Land Owner | 421.76** | 403.01** |
| | | (184.03) | (184.77) | (184.88) | (181.99) | | (175.66) | (168.52) |
| | Time | -257.83 | -258.20 | -258.90 | -81.13 | | | |
| | | (160.69) | (161.33) | (161.43) | (745.49) | | | |
| | Status | -120.01 | -177.67 | -219.41 | -177.60 | | | |
| | | (142.34) | (137.10) | (139.16) | (135.08) | | | |
| | | (1) | (2) | (3) | (4) | | (5) | (6) |
| **Intensity of Treatment** | Land Owner ≤3 Years | 293.72 | 291.68 | 305.73 | 301.56 | Land Owner ≤3 Years | 179.89 | 155.44 |
| | | (246.38) | (243.74) | (244.43) | (237.93) | | (254.09) | (247.94) |
| | Land Owner 4 Years | 381.34* | 379.55* | 384.07* | 363.88* | Land Owner 4 Years | 363.86* | 343.90** |
| | | (195.38) | (197.50) | (197.69) | (196.30) | | (192.13) | (184.57) |
| | Land Owner 5-6 Years | 616.65*** | 619.26*** | 603.23*** | 582.77*** | Land Owner 5-6 Years | 710.67*** | 749.53*** |
| | | (203.21) | (203.32) | (203.33) | (204.75) | | (196.49) | (199.74) |
| | Time | -257.68 | -258.02 | -258.71 | -82.99 | | | |
| | | (160.81) | (161.46) | (161.56) | (750.75) | | | |
| | Status | -99.89 | -159.11 | -201.13 | -165.45 | | | |
| | | (142.60) | (136.92) | (139.32) | (134.94) | | | |
| | N | 1522 | 1522 | 1522 | 1522 | | 1522 | 1522 |
| | Municipal FE | Y | Y | Y | Y | | N | N |
| | Individual Controls[1] | N | Y | Y | Y | | N | N |
| | Social Capital Controls[2] | N | N | Y | Y | | N | N |
| | Individual Agricultural Controls[3] | N | N | N | Y | | N | N |
| | Regional Agricultural Controls[4] | N | N | N | Y | | N | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual agricultural controls include technical assistance and PRONAF. [4]Regional agricultural controls include daily agricultural wage and yield of corn. The sample contains all observations in the balanced panel with 20 outliers dropped and 4 dropped because of missing values. All values in household per capita Reais of January 2010.

Table 2.5 displays the results for the dependent variable agricultural production. In the panel on the top left, the binary case, the estimation shows significant and positive effects of receiving land, robust to the inclusion of different controls. In the first column, which only includes municipal fixed effects, receiving land via the PNCF-CPR increases beneficiaries' agricultural production by R$428 (US$240) per person during the last year as compared to the pipeline non-beneficiaries. The estimated coefficients change little in the remainder of the specifications, and are not statistically different from each other. The coefficient remains the same when individual level controls and social capital controls are included stepwise in columns two and three. In the preferred specification in column four, the coefficient decreases slightly to R$405 (US$227) per person in the household. All estimated coefficients in the first row of the left panel of Table 2.5 are statistically significant at the five percent level.

The bottom left panel of Table 2.5 displays the results for agricultural production using the intensity of treatment estimation. It shows the effects of the program for increasing number of years of land ownership. The pattern is clear: increasing years of land ownership increase the magnitude of the estimates. The coefficients on being a landowner for three years or less are all positive, but none are statistically significant. The coefficients on being a landowner for four years are all positive and significant at ten percent. Finally, the coefficients on being a landowner for five to six years are all of a much larger magnitude than the coefficients for the previous two categories and all become significant at the one percent level. The preferred specification in column four indicates that owning land via the PNCF-CPR for five to six years increases per capita agricultural production in the last year by R$583 (US$327). Given the limitations of the data, we cannot know for certain if the returns will continue to increase at an increasing or decreasing rate, or at what point they might plateau, with additional years of land ownership. Nevertheless, the results based on up to six years of ownership suggest that acquiring land via the PNCF-CPR will likely have an increasing effect on agricultural production over time.

The results for the binary individual FE model in the top right panel of Table 2.5 show almost identical effects as the difference in differences model. The binary case shows significant and positive effects on agricultural production of being a beneficiary of the program. The coefficient is robust to the alternative specification with time varying controls. In the intensity of treatment estimation in the bottom right panel, the expected pattern is observed. Increasing years of land ownership are associated with increased magnitudes of the effects on agricultural production per member in the household. Nonetheless, the coefficients are only significant for landowners for four years and for landowners for five to six years. From the preferred specification in column six, it can be concluded that being a beneficiary of the PNCF-CPR for five to six years increases agricultural production per person in the household by an average of R$750 (US$421) in the last year. This result is significant at the one percent level.

**Table 2.6: Effects on Earned Income from Land Ownership**

| | | Difference in Differences | | | | Fixed Effects | | |
|---|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | | (5) | (6) |
| **Binary** | Time*Status | 81.85 | 82.15 | 82.31 | 43.43 | Land Owner | 99.65 | 87.49 |
| | | (172.78) | (173.23) | (173.46) | (182.59) | | (165.12) | (171.46) |
| | Time | -114.14 | -115.21 | -115.40 | 120.83 | | | |
| | | (139.68) | (140.14) | (140.33) | (760.18) | | | |
| | Status | 27.06 | -47.66 | -84.38 | -34.31 | | | |
| | | (152.31) | (141.84) | (144.66) | (147.15) | | | |
| | | (1) | (2) | (3) | (4) | | (5) | (6) |
| **Intensity of Treatment** | Land Owner $\leq$3 Years | -43.09 | -50.79 | -42.22 | -39.67 | Land Owner $\leq$3 Years | -120.93 | -112.03 |
| | | (206.11) | (200.65) | (200.78) | (196.60) | | (207.01) | (203.80) |
| | Land Owner 4 Years | -43.87 | -36.81 | -32.28 | -53.65 | Land Owner 4 Years | -25.46 | -19.69 |
| | | (193.50) | (194.96) | (194.19) | (207.71) | | (193.57) | (204.63) |
| | Land Owner 5-6 Years | 435.84** | 426.84** | 411.82** | 324.90 | Land Owner 5-6 Years | 519.69** | 501.19** |
| | | (207.04) | (206.68) | (207.54) | (221.72) | | (204.58) | (227.66) |
| | Time | -114.07 | -115.07 | -115.24 | 157.08 | | | |
| | | (139.79) | (140.24) | (140.44) | (777.45) | | | |
| | Status | 58.88 | -19.04 | -54.76 | -16.66 | | | |
| | | (152.43) | (141.91) | (144.50) | (146.43) | | | |
| | N | 1516 | 1516 | 1516 | 1516 | | 1516 | 1516 |
| | Municipal FE | Y | Y | Y | Y | | N | N |
| | Individual Controls[1] | N | Y | Y | Y | | N | N |
| | Social Capital Controls[2] | N | N | Y | Y | | N | N |
| | Individual Agricultural Controls[3] | N | N | N | Y | | N | N |
| | Regional Agricultural Controls[4] | N | N | N | Y | | N | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual agricultural controls include technical assistance and PRONAF. [4]Regional agricultural controls include daily agricultural wage and yield of corn. The sample contains all observations in the balanced panel with 26 outliers dropped and 4 dropped because of missing values. All values in household per capita Reais of January 2010.

For the outcome variable earned income (Table 2.6), all binary models show a positive effect of being a beneficiary of the program, although the coefficients are not statistically significant. The estimation results that allow for the heterogeneous effects of additional years of land ownership are shown in the lower panels of Table 2.6. In both the DD and individual FE models, the impact of the PNCF-CPR only becomes positive and significant at 5% for beneficiaries with five or six years of land ownership (with the exception of column four). Based on the preferred FE specification in column six, the estimated impact of the PNCF-CPR on earned income is R$501 (US$281) per person in the household in 2010, significant at five percent.

## 2.6 ROBUSTNESS CHECKS

In order to ensure that the estimates presented are robust to alternative specifications and that a causal interpretation is appropriate, a variety of robustness checks were performed. First, in order to investigate whether some unobserved trend could be causing spurious findings, a placebo test was run. Second, there exists a concern that there could be some characteristics of the earliest beneficiaries that were causing their agricultural production and earned income to grow more. In order to control for this, an eagerness variable in the spirit of Agüero et al. (2009) was included in the estimation. Third, sample attrition and potential attrition bias was analyzed.[23]

Despite the apparent validity of using the pipeline control (Table 2.4), it could be possible that the beneficiaries were subject to different trends than the pipeline non-

[23] A test of the parallel trends hypothesis would also have been appropriate, but we do not have data from a prior period. We considered testing parallel trends at a municipal level with an auxiliary dataset, but this too was not feasible because the beneficiary and control individuals were largely drawn from the same locations.

beneficiaries. The estimated parameters, then, could be reflecting these different unobserved trends, instead of accurately estimating the impact of the program. In order to provide suggestive evidence that the identification strategy is indeed valid, a placebo test was run. This placebo test entails estimating the regressions above on a variable that the PNCF-CPR should not have any effect on. If there were unobserved trends affecting the beneficiaries and not the pipeline non-beneficiaries, then the results could display a similar pattern to those in Table 2.5 and 2.6. The chosen variable for the placebo test is total transfers, which include old age pensions, *Bolsa Família* conditional cash transfers, other government transfers and private transfers. There is no reason to expect that access to land via the PNCF-CPR should affect this variable. As can be seen in Table 2.7, regardless of the estimation technique, no significant effects are observed on total transfers in either the binary or intensity of treatment case. This provides some evidence that it is unlikely that the beneficiaries and pipeline non-beneficiaries were exposed to different group-specific time trends.

**Table 2.7: Robustness Check – Effects on Total Transfers from Land Ownership**

| | | Difference in Differences | | | | Fixed Effects | | |
|---|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | | (5) | (6) |
| Binary | Time*Status | 47.04 | 57.16 | 56.61 | 77.71 | Land Owner | 29.14 | 29.69 |
| | | (57.51) | (55.42) | (55.45) | (57.91) | | (55.23) | (56.42) |
| | Time | 71.22 | 74.71 | 75.15 | -192.22 | | | |
| | | (48.43) | (46.64) | (46.65) | (207.27) | | | |
| | Status | -77.82 | -88.83 | -81.54 | -95.64* | | | |
| | | (57.59) | (56.80) | (57.11) | (57.09) | | | |
| | | (1) | (2) | (3) | (4) | | (5) | (6) |
| Intensity of Treatment | Land Owner ≤ 3 Years | 46.87 | 73.92 | 67.17 | 93.48 | Land Owner ≤3 Years | 75.72 | 93.13 |
| | | (78.37) | (74.60) | (73.76) | (75.64) | | (77.54) | (77.90) |
| | Land Owner 4 Years | 80.27 | 79.04 | 79.16 | 110.67* | Land Owner 4 Years | 23.62 | 32.72 |
| | | (63.98) | (61.14) | (61.13) | (64.02) | | (60.88) | (63.00) |
| | Land Owner 5-6 Years | -24.76 | -1.66 | 0.51 | -7.41 | Land Owner 5-6 Years | 9.38 | -31.63 |
| | | (79.88) | (75.86) | (76.17) | (81.46) | | (81.60) | (89.42) |
| | Time | 71.25 | 74.69 | 75.13 | -206.93 | | | |
| | | (48.47) | (46.67) | (46.70) | (214.20) | | | |
| | Status | -83.05 | -93.16 | -85.87 | -100.50* | | | |
| | | (58.25) | (57.40) | (57.70) | (57.68) | | | |
| | N | 1494 | 1494 | 1494 | 1494 | | 1494 | 1494 |
| | Municipal FE | Y | Y | Y | Y | | N | N |
| | Indvidivual Controls[1] | N | Y | Y | Y | | N | N |
| | Social Capital Controls[2] | N | N | Y | Y | | N | N |
| | Individual Agricultural Controls[3] | N | N | N | Y | | N | N |
| | Regional Agricultural Controls[4] | N | N | N | Y | | N | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual agricultural controls include technical assistance and PRONAF. [4]Regional agricultural controls include daily agricultural wage and yield of corn. The sample contains all observations in the balanced panel with 48 outliers dropped and 4 dropped because of missing values. All values in household per capita Reais of January 2010.

In addition to the placebo test, another model was estimated to address the concern that there may be differences within the beneficiary group itself. It could be that the earliest beneficiaries were simply more motivated (as indicated by their early participation in the program), which is what caused them to have increased levels of the outcome variables in the intensity of treatment specifications. To the extent that this reflects a time invariant characteristic at the individual level, the FE model will address this concern and generate unbiased estimates of the program impact. The same cannot be said of the DD model. In order to analyze this hypothesis directly in the context of the DD model, a proxy for "eagerness" was created in the spirit of Agüero et al. (2009). Eagerness was defined as the average contract date of projects in a given municipality minus the individual's contract date. If the enrolled member was eager, the difference between his or her date of contract and the average date of contract of projects in the same municipality was negative, while the difference was positive for less eager, or tardy, applicants.

**Table 2.8: Robustness Check – Effects from Land Ownership with Eagerness of Beneficiary Application in Intensity of Treatment Difference in Differences Estimation**

| | Agricultural Production | | | | Earned Income | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Land Owner $\leq$ 3 Years | 246.55 | 255.88 | 269.90 | 265.15 | -124.89 | -102.02 | -92.87 | -88.56 |
| | (249.03) | (246.18) | (246.13) | (238.24) | (203.40) | (198.27) | (198.11) | (193.48) |
| Land Owner 4 Years | 385.41* | 382.71* | 387.23* | 368.48* | -36.92 | -32.36 | -27.90 | -47.54 |
| | (196.34) | (198.46) | (198.66) | (198.19) | (193.70) | (195.24) | (194.58) | (208.61) |
| Land Owner 5-6 Years | 638.11*** | 635.40*** | 619.45*** | 606.82*** | 473.04** | 449.97** | 434.81** | 357.38 |
| | (206.57) | (206.41) | (206.42) | (212.38) | (209.35) | (208.57) | (209.67) | (226.44) |
| Time | -257.11 | -257.59 | -258.29 | -103.43 | -113.07 | -114.46 | -114.65 | 129.45 |
| | (160.81) | (161.45) | (161.55) | (756.31) | (139.74) | (140.21) | (140.39) | (778.98) |
| Status | -281461 | -217021 | -218619 | -220990 | -485438* | -309200 | -307894 | -295852 |
| | (311825) | (301477) | (298694) | (306166) | (289049) | (269986) | (265620) | (271667) |
| Eagerness | 0.28 | 0.22 | 0.22 | 0.22 | 0.49* | 0.31 | 0.31 | 0.30 |
| | (0.31) | (0.30) | (0.30) | (0.31) | (0.29) | (0.27) | (0.27) | (0.27) |
| N | 1522 | 1522 | 1522 | 1522 | 1516 | 1516 | 1516 | 1516 |
| Municipal FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Individual Controls[1] | N | Y | Y | Y | N | Y | Y | Y |
| Social Capital Controls[2] | N | N | Y | Y | N | N | Y | Y |
| Individual Agricultural Controls[3] | N | N | N | Y | N | N | N | Y |
| Regional Agricultural Controls[4] | N | N | N | Y | N | N | N | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual agricultural controls include technical assistance and PRONAF. [4]Regional agricultural controls include daily agricultural wage and yield of corn. The sample contains all observations in the balanced panel with 20 outliers dropped for AP, 26 outliers dropped for EI and 4 dropped for both because of missing values. All values in household per capita Reais of January 2010.

The inclusion of the eagerness variable affected the coefficients of interest in the same way in both the agricultural production and earned income regressions. The inclusion of the variable decreased all coefficients for landowners with three or less years on their land, while increasing the coefficients for landowners with four or five to six years on their respective lands. In the preferred specification in column four, both the significant coefficients (for LO 4 years and LO 5-6 years) increased slightly compared to the same model without eagerness, although the changes were not statistically significant. The same pattern is observed for the earned income coefficients in column eight. Thus, while the eagerness of the early applicants might be a small piece of the story, it is not the principal reason why the earliest beneficiaries had increased agricultural production and earned income.

### 2.6.1 Attrition

In the follow-up phase of the data collection process, thirty six percent of the original beneficiaries, and forty seven percent of the original pipeline non-beneficiaries were not interviewed again, for a variety of reasons. These reasons range from withdrawing from the program, refusing the interview, death, coding errors, enumerators not being able to locate the individual (or in some cases entire associations), the enrolled member living in another city or state, to the enrolled member being out of town during the enumerator's visit. There were also 162 pipeline non-beneficiaries that acquired land after the baseline period and were re-interviewed as beneficiaries. As such, they exited from the pipeline non-beneficiary sample, potentially leaving it unrepresentative of how it was constituted in the baseline. Finally, a small number of observations had to be excluded due to missing values in the follow-up period.[24]

Attrition can be a serious threat to inference using panel data because it can cause the original random sample to become unrepresentative of the treatment and control groups as individuals exit from the panel over time. This can lead to biased parameter estimates of the impact of treatment. These potential biases, however, depend not on the magnitude of attrition but on whether the attrition was non-random. Specifically, if there was systematic selection on characteristics of the enrolled members even after conditioning on observed covariates, bias could ensue. In this section, a number of tests indicate that attrition was random in this dataset, while others suggest that it was not. In order to control for the potential bias resulting from nonrandom attrition, the inverse probabilities of retention (*i.e.*, non-attrition) are used as weights (Fitzgerald, Gottschalk and Moffitt, 1998).

A number of steps were taken in order to analyze whether there was any pattern to the attrition in the data. The first test conducted was a simple comparison of base year means, and a Student's t-test of the differences between those enrolled members that disappeared and those that remained (Alderman et al. 2001). Because we suspected that there may be heterogeneous patterns to attrition, all comparisons and tests were done first for the full sample, then by group. Table 2.9 summarizes the findings of the mean comparison by group. Looking at the case of the full sample, attritors seemed to be younger, more white, less married, had more schooling, less experience as farmers and were more urban. They had less trust in other members of their association than non-

[24] The "attritors" that were dropped because of missing values only represent 1.6% of the total number of attritors.

attritors, and lived in areas where there was a lower daily wage. While there are many significant differences in observable variables, within the full sample and the beneficiary group there was no evidence that attritors had systematically different outcome variables at baseline. Nevertheless, it appears that the weakest of the pipeline non-beneficiaries, in terms of agricultural production, were attriting at the one percent level of significance. In other words, pipeline non-beneficiaries with lower levels of agricultural production were more likely to disappear from the sample after the baseline period. This would result in a pipeline non-beneficiary group that appeared stronger than it would have, had the attritors remained in the sample. This pattern of attrition would lead to a *downward* bias on the estimated coefficients of program impact. Because this comparison of mean group characteristics shows significant differences, nonrandom attrition is suspected.

Following Alderman et al. (2001) and Chawanote and Barrett (2014), we next implemented a BGLW test (Becketti, Gould, Lillard, and Welch, 1988) by estimating the following equation with the sample of attritors and non-attritors using baseline data only:

$$Y_i=\alpha+\beta attrition_i+\delta X_i+\mu(X_i*attrition_i)+\varepsilon_i \qquad (2.6)$$

where "attrition" is an indicator variable that equals one if the enrolled member attrited between the baseline and follow-up periods, Y are the outcome variables agricultural production and earned income, X are control variables and "attrition" is multiplied with the control variables to create interaction terms.[25] Table 2.10 shows that none of the coefficients on the attrition dummy are significant, regardless of the inclusion of control

[25] The included control variables are identical to those used in the main regressions. Due to multicollinearity, race was substituted by the variable "white" (an indicator variable that equals one for Caucasians and zero otherwise) and municipality was not interacted with the attrition variable.

variables, indicating that attrition is not significant in determining either agricultural production or earned income. Nevertheless, an F-test of the joint significance of the coefficient on the attrition dummy and the coefficients on the control variables interacted with the attrition dummy shows that these variables are jointly significant, for agricultural production in the full sample and earned income in both sub-samples. In this way, we conclude that the coefficients on the explanatory variables differ between individuals who disappear from the panel and those who do not. This result indicates that there was nonrandom attrition and thus the use of attrition weights is warranted.

The final test to assess whether attrition was random were a series of probit regressions with the dependent variable equal to one if the enrolled member left the sample between the baseline and follow-up period, and zero otherwise.[26] At the ten percent level of significance and when not conditioning on observed covariates, the weaker pipeline non-beneficiaries in terms of agricultural production appear to have an increased probability of attrition (Table 2.11). For the beneficiary group, when including control variables, it appears that the stronger beneficiaries with respect to agricultural production have an increased probability of attrition. If the lower-than-average producers leave the pipeline non-beneficiary sample, the pipeline non-beneficiary group appears to be stronger than it actually is, and the reverse is true for the beneficiary group. A stronger pipeline non-beneficiary group coupled with a weaker beneficiary group would lead to estimated parameters that would be biased downward. In order for the balanced panel to produce unbiased estimates, the use of attrition weights is required. Due the particular

[26] State fixed effects are used instead of municipal fixed effects due to multicollinearity.

pattern of attrition described above, it is likely that the use of attrition weights will increase the estimated impact of the program.

**Table 2.9: Robustness Check – Attrition Mean Comparison**
*(Mean of Attritor – Mean of Non-Attritor)*

| | **Full Sample** | **Pipeline NB** | **Beneficiary** |
|---|---|---|---|
| **Production and Income Variables** | | | |
| Agricultural Production | NSD | Difference<0*** | NSD |
| Earned Income | NSD | NSD | NSD |
| **Individual Characteristics** | | | |
| Age | Difference<0*** | Difference<0* | Difference<0*** |
| Sex | NSD | Difference>0*** | Difference<0* |
| White | Difference>0*** | NSD | Difference>0** |
| Married | Difference<0*** | Difference<0* | Difference<0*** |
| School | Difference>0** | Difference>0* | Difference>0* |
| Experience | Difference<0*** | Difference<0*** | Difference<0*** |
| Urban | Difference>0*** | Difference>0** | NSD |
| **Social Capital Variables** | | | |
| Position Held | Difference<0* | NSD | NSD |
| Meet | NSD | Difference<0* | NSD |
| Trust | Difference<0*** | Difference<0*** | Difference<0* |
| **Individual Agricultural Variables** | | | |
| PRONAF | Difference<0** | Difference<0** | Difference<0** |
| Technical Assistance | Difference>0** | Difference>0** | NSD |
| **Regional Agricultural Variables** | | | |
| Daily Agricultural Wage | Difference<0*** | Difference<0*** | NSD |
| Yield of Corn | NSD | Difference<0*** | NSD |

Notes: NSD is the abbreviation for "no significant difference." ***Significant at 1%; **Significant at 5%; *Significant at 10%. The sample contains all observations in the baseline period with 7 outliers dropped for AP, 7 outliers dropped for EI (6 of which are common to both AP and EI) and 12 dropped because of missing values.

**Table 2.10: Robustness Check – BGLW Test for Attrition Analysis**

| | | Agricultural Production | | | Earned Income | | |
|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| **Full Sample** | Attrition | -116.64 | 57.77 | 1699.73 | -32.77 | 54.94 | 2485.67 |
| | | (111.46) | (98.39) | (1451.44) | (115.78) | (109.26) | (1579.50) |
| | *(F-Stat)* | | | 1.59* | | | 1.15 |
| | *(p-value)* | | | 0.08 | | | 0.32 |
| | N | 1316 | 1316 | 1316 | 1316 | 1316 | 1316 |
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| **PNB Group** | Pipeline NB Attrition | -320.93 | -111.60 | 1003.70 | -191.89 | -106.97 | 1975.93 |
| | | (216.67) | (103.45) | (1831.76) | (218.48) | (130.21) | (2321.52) |
| | *(F-Stat)* | | | 1.25 | | | 1.94** |
| | *(p-value)* | | | 0.24 | | | 0.02 |
| | N | 678 | 678 | 678 | 678 | 678 | 678 |
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| **B Group** | Beneficiary Attrition | 52.47 | 203.22 | -2837.05 | 118.43 | 88.49 | -1109.15 |
| | | (151.97) | (127.92) | (2332.10) | (108.48) | (90.94) | (1503.90) |
| | *(F-Stat)* | | | 1.17 | | | 1.76** |
| | *(p-value)* | | | 0.30 | | | 0.05 |
| | N | 638 | 638 | 638 | 638 | 638 | 638 |
| Municipal FE | | N | Y | Y | N | Y | Y |
| Individual Controls[1] | | N | Y | Y | N | Y | Y |
| Social Capital Controls[2] | | N | Y | Y | N | Y | Y |
| Individual Agricultural Controls[3] | | N | Y | Y | N | Y | Y |
| Regional Agricultural Controls[4] | | N | Y | Y | N | Y | Y |
| Interaction | | N | N | Y | N | N | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual agricultural controls include technical assistance and PRONAF. [4]Regional agricultural controls include daily agricultural wage and yield of corn. The sample contains all observations in the baseline period with 7 outliers dropped for AP, 7 outliers dropped for EI and 12 dropped for both because of missing values. All values in household per capita Reais of January 2010.

**Table 2.11: Robustness Check – Probits for the Probability of Attrition**

| | | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| **Full Sample** | Agricultural Production | -0.0000 | | -0.0000 | |
| | | (0.0000) | | (0.0000) | |
| | Earned Income | | -0.0000 | | -0.0000 |
| | | | (0.0000) | | (0.0000) |
| | N | 1315 | 1315 | 1315 | 1315 |
| | | (1) | (2) | (3) | (4) |
| **PNB Group** | Agricultural Production | -0.0001* | | -0.0001 | |
| | | (0.0001) | | (0.0000) | |
| | Earned Income | | -0.0001 | | -0.0000 |
| | | | (0.0001) | | (0.0000) |
| | N | 677 | 677 | 677 | 677 |
| | | (1) | (2) | (3) | (4) |
| **B Group** | Agricultural Production | 0.0000 | | 0.0001* | |
| | | (0.0000) | | (0.0000) | |
| | Earned Income | | 0.0000 | | 0.0000 |
| | | | (0.0000) | | (0.0000) |
| | N | 638 | 638 | 638 | 638 |
| | State FE | N | N | Y | Y |
| | Individual Controls[1] | N | N | Y | Y |
| | Social Capital Controls[2] | N | N | Y | Y |
| | Individual Agricultural Controls[3] | N | N | Y | Y |
| | Regional Agricultural Controls[4] | N | N | Y | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual agricultural controls include technical assistance and PRONAF. [4]Regional agricultural controls include daily agricultural wage and yield of corn. The sample contains all observations in the baseline period with 8 outliers dropped (6 common to both AP and EI and 1 unique to each) and 12 dropped because of missing values.

The attrition weights, or the inverse probabilities of retention, are estimated using baseline-level data only and defined by:

$$w(z,x)=[Pr(A=0|z,x)/Pr(A=0|x)]^{-1} \tag{2.7}$$

where Pr(A=0) is the probability of retention, x are the control variables used in the estimation and z are auxiliary variables that affect the attrition propensity, can be related to the density of the outcome variables conditional on the control variables, and yet are not in the original regressions (Fitzgerald et al., 1998). Two variables were used as the auxiliary variables—previous ties with association members and whether the enrolled member had lived in a different city in the past ten years. Previous ties with the association members equals one if the enrolled member was friends with, or related to, members of the association before the association formed. As can be seen in Table 2.12, previous ties to association members is highly predictive of retention amongst pipeline non-beneficiaries, and significant at the 10% level in the full sample. Whether the enrolled member lived in a different city in the past ten years is predictive of retention in the full and beneficiary samples, but only when additional controls are not included.

The intuition behind the attrition weights is that they give more weight to enrolled members that have similar initial characteristics to enrolled members that disappear from the sample than to enrolled members with characteristics that make them more likely to remain in the sample. Because the nonrandom patterns of attrition were found to be different in the pipeline non-beneficiaries and beneficiary groups, the attrition weights were calculated separately for each group. The weights ranged from 0.54 to 3.2, with a mean of 1.01. With the inclusion of attrition weights, Tables 2.13 and 2.14 show that all estimated parameters of the impact of treatment unambiguously increase in the preferred specification where all available control variables are included. This result confirms that attrition is not the source of the positive and statistically significant findings reported in Section 2.5, and confirms that the patterns observed in attrition were biasing downward—even if only slightly—the estimates of program impact.

**Table 2.12: Robustness Check – Probits for the Probability of Retention Using Z-Variables**

| | | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|---|
| **Full Sample** | Different City 2006 | -0.20** | | -0.14 | | -0.13 |
| | | (0.10) | | (0.10) | | (0.10) |
| | Previous Ties 2006 | | 0.39*** | | 0.24* | 0.23 |
| | | | (0.14) | | (0.14) | (0.14) |
| | N | 1315 | 1315 | 1315 | 1315 | 1315 |
| | | (1) | (2) | (3) | (4) | (5) |
| **PNB Group** | Different City 2006 | -0.28 | | -0.16 | | -0.11 |
| | | (0.17) | | (0.16) | | (0.16) |
| | Previous Ties 2006 | | 0.70*** | | 0.60** | 0.58** |
| | | | (0.25) | | (0.26) | (0.27) |
| | N | 677 | 677 | 677 | 677 | 677 |
| | | (1) | (2) | (3) | (4) | (5) |
| **B Group** | Different City 2006 | -0.26** | | -0.19 | | -0.19 |
| | | (0.12) | | (0.13) | | (0.13) |
| | Previous Ties 2006 | | 0.39** | | 0.15 | 0.13 |
| | | | (0.17) | | (0.17) | (0.17) |
| | N | 638 | 638 | 638 | 638 | 638 |
| | State FE | N | N | Y | Y | Y |
| | Individual Controls[1] | N | N | Y | Y | Y |
| | Social Capital Controls[2] | N | N | Y | Y | Y |
| | Individual Agricultural Controls[3] | N | N | Y | Y | Y |
| | Regional Agricultural Controls[4] | N | N | Y | Y | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual agricultural controls include technical assistance and PRONAF. [4]Regional agricultural controls include daily agricultural wage and yield of corn. The sample contains all observations in the baseline period with 8 outliers dropped (6 common to both AP and EI and 1 unique to each) and 12 dropped because of missing values.

**Table 2.13: Robustness Check – Effects on Agricultural Production from Land Ownership With and Without Attrition Weights**

| | | Difference in Differences | | Fixed Effects | | |
|---|---|---|---|---|---|---|
| | | (1) | (2) | | (3) | (4) |
| Binary | Time*Status | 428.56** | 405.43** | Land Owner | 425.28** | 403.01** |
| | | (178.62) | (181.99) | | (167.66) | (168.52) |
| | Time | -235.51 | -81.13 | | | |
| | | (593.78) | (745.49) | | | |
| | Status | -185.42 | -177.60 | | | |
| | | (133.27) | (135.08) | | | |
| | | (1) | (2) | | (3) | (4) |
| Intensity of Treatment | Land Owner ≤3 Years | 304.50 | 301.56 | Land Owner ≤3 Years | 159.01 | 155.44 |
| | | (234.83) | (237.93) | | (244.49) | (247.94) |
| | Land Owner 4 Years | 390.76** | 363.88* | Land Owner 4 Years | 371.44** | 343.90** |
| | | (185.95) | (196.30) | | (177.36) | (184.57) |
| | Land Owner 5-6 Years | 605.93*** | 582.77*** | Land Owner 5-6 Years | 753.39*** | 749.53*** |
| | | (208.32) | (204.75) | | (206.63) | (199.74) |
| | Time | -235.71 | -82.99 | | | |
| | | (592.50) | (750.75) | | | |
| | Status | -171.76 | -165.45 | | | |
| | | (133.13) | (134.94) | | | |
| | N | 1522 | 1522 | | 1522 | 1522 |
| | **Attrition Weights** | **Y** | **N** | | **Y** | **N** |
| | Municipal FE | Y | Y | | N | N |
| | Individual Controls[1] | Y | Y | | N | N |
| | Social Capital Controls[2] | Y | Y | | N | N |
| | Individual Agricultural Controls[3] | Y | Y | | N | N |
| | Regional Agricultural Controls[4] | Y | Y | | Y | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual agricultural controls include technical assistance and PRONAF. [4]Regional agricultural controls include daily agricultural wage and yield of corn. The sample contains all observations in the balanced panel with 20 outliers dropped and 4 dropped because of missing values. All values in household per capita Reais of January 2010.

**Table 2.14: Robustness Check – Effects on Earned Income from Land Ownership With and Without Attrition Weights**

| | | Difference in Differences | | Fixed Effects | | |
|---|---|---|---|---|---|---|
| | | (1) | (2) | | (3) | (4) |
| Binary | Time*Status | 92.46 | 43.43 | Land Owner | 124.07 | 87.49 |
| | | (177.67) | (182.59) | | (167.99) | (171.46) |
| | Time | -272.42 | 120.83 | | | |
| | | (685.81) | (760.18) | | | |
| | Status | -46.96 | -34.31 | | | |
| | | (149.89) | (147.15) | | | |
| | | (1) | (2) | | (3) | (4) |
| Intensity of Treatment | Land Owner ≤3 Years | -19.11 | -39.67 | Land Owner ≤3 Years | -93.19 | -112.03 |
| | | (193.46) | (196.60) | | (196.83) | (203.80) |
| | Land Owner 4 Years | -1.30 | -53.65 | Land Owner 4 Years | 28.06 | -19.69 |
| | | (198.24) | (207.71) | | (197.03) | (204.63) |
| | Land Owner 5-6 Years | 387.35* | 324.90 | Land Owner 5-6 Years | 512.44** | 501.19** |
| | | (217.71) | (221.72) | | (225.06) | (227.66) |
| | Time | -240.41 | -157.08 | | | |
| | | (693.09) | (777.45) | | | |
| | Status | -27.82 | -16.66 | | | |
| | | (149.01) | (146.43) | | | |
| | N | 1516 | 1516 | | 1516 | 1516 |
| | **Attrition Weights** | **Y** | **N** | | **Y** | **N** |
| | Municipal FE | Y | Y | | N | N |
| | Individual Controls[1] | Y | Y | | N | N |
| | Social Capital Controls[2] | Y | Y | | N | N |
| | Individual Agricultural Controls[3] | Y | Y | | N | N |
| | Regional Agricultural Controls[4] | Y | Y | | Y | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include position in association, frequency of meetings and trust in association. [3]Individual agricultural controls include technical assistance and PRONAF. [4]Regional agricultural controls include daily agricultural wage and yield of corn. The sample contains all observations in the balanced panel with 26 outliers dropped and 4 dropped because of missing values. All values in household per capita Reais of January 2010.

## 2.7 REPAYMENT

While the regressions and supporting robustness checks showed that the PNCF-CPR is successful in increasing beneficiaries' agricultural production and earned income after four years of land ownership, an important factor to consider is their ability to repay the PNCF-CPR loans, and also the effects of the program once accounting for repayment. A few policies facilitate repayment. First, if the principal is above R$15,000, beneficiaries have up to seventeen years to repay. For smaller loans, the repayment period is limited to fourteen years. Second, the grace period is twenty-four months, and the annual interest rates vary between two and five percent depending on the principal. In the first year of repayment—the beginning of the third year of land ownership—the beneficiaries with a principal of less than R$15,000 are only required to pay the interest accrued on the loan during the first two years (MDA, 2009).[27] In addition, in the semi-arid regions of the Northeast of Brazil, there is a forty percent discount on all installments made on or before the due date. In the rest of the Northeast, the discount is thirty percent for on-time payments. Lastly, there is an additional ten percent discount on installments for associations that are able to negotiate the price of the land below what the predicted price would have been using the land price monitoring system. The cap for the discounts is R$1,000 per installment. Given these two discounts, it is likely that a high share of beneficiaries should be able to repay their loans. What follows is not an analysis of the percentage of beneficiaries that actually paid. It is an analysis of the percentage that should have had enough income to meet their loan obligations.

As can be seen in Table 2.15, in 2010, there were 88 beneficiaries in the third year of the program, 320 beneficiaries in the fourth year, and 148 beneficiaries in the fifth to sixth year. Looking only at the cases with both discounts, depending on the year of land ownership, 79 to 84 percent of beneficiaries could repay given their earned income, and 91 to 92 percent could repay once transfers are included.[28] This would leave beneficiaries

[27] The beneficiaries with loan amounts above R$15,000 must repay the interest and also the first installment.

[28] Although including government monetary transfers no longer allows us to strictly measure the ability of beneficiaries to repay given increases in income *due to the program*, transfers such as old age social

with 64 to 75 percent of their earned income after repayment, and 74 to 83 percent of their income including transfers. Nevertheless, many beneficiaries do not secure these discounts and have a more difficult time repaying.

**Table 2.15: Beneficiaries' Ability to Repay PNCF Loan**

| | | Percent Able to Pay | | | Share of EI Used for Payments | | |
|---|---|---|---|---|---|---|---|
| | | *Discount Type* | | | *Discount Type* | | |
| Year | N | No Discount | On-time Discount | Both Discounts | No Discount | On-time Discount | Both Discounts |
| 3 | 88 | 66% | 81% | 84% | 40% | 35% | 36% |
| 4 | 320 | 73% | 78% | 79% | 37% | 28% | 26% |
| 5-6 | 148 | 79% | 84% | 84% | 38% | 27% | 25% |
| | | | **Including Transfers** | | | | |
| 3 | 88 | 82% | 90% | 91% | 31% | 26% | 26% |
| 4 | 320 | 86% | 90% | 91% | 27% | 19% | 18% |
| 5-6 | 148 | 89% | 92% | 92% | 28% | 19% | 17% |

Notes: Numbers are for those beneficiaries who would have enough earned income to make the payment, not those who actually did make payments.

The above analysis suggests that there appears to be a relatively high share of beneficiaries who should be able to repay their loans. Nevertheless, the payments range from 25 to 40 percent of beneficiaries' earned income, depending on the level of discount. Considering that these are very poor families, these payments might be too burdensome, and substantially decrease their quality of life. In order for the burden of the debt to be minimized, the grace period could be extended so as to give the beneficiaries sufficient time to adapt to their new circumstances and acquire enough earned income to pay the debt with greater ease. In this regard, a more forward looking analysis suggests that—if the program impacts continue to grow at the rate observed in the first six years of

security benefits and the conditional cash transfer program *Bolsa Família* represent an important share of income in the rural Northeast. Helfand et al. (2009) report that social security transfers accounted for 23 percent of income in the rural Northeast in 2005. In this dataset, the number is slightly higher—at 25 percent.

land ownership (Table 2.14)—beneficiaries should be able to make debt payments more and more easily.

Although the beneficiaries' ability to repay is an important consideration, an equally pressing issue is the effectiveness of the program in poverty reduction. While the analysis in Section 2.5 and 2.6 showed that the program has a significant impact on agricultural production and earned income, that analysis did not address the repayment of the loan. In order to address this missing piece, the value of the installment due was calculated for each beneficiary and then subtracted from earned income in the follow-up period. Regressions were re-estimated using the updated earned income, and attrition weights were also used to ensure the representativeness of the sample.

Table 2.16 shows the results for the intensity of treatment estimation including control variables (equivalent to column four in Table 2.14). The results indicate that once repayment is included in the analysis beneficiaries no longer enjoy an increase in their current welfare because the gains to earned income are being used for repayment. With both discounts, beneficiaries in the fourth, fifth or sixth year of land ownership now display no significant effects on earned income. Beneficiaries in the fourth year with only one or no repayment discounts, and all beneficiaries with three or less years of land ownership display negative and significant effects of being a beneficiary of the program, once repayment is taken into account. As such, while the program works to increase the earned income of beneficiaries, once repayment is taken into consideration, the beneficiaries in the first four years of the program are statistically *worse off* in terms of current welfare than the pipeline non-beneficiaries. For beneficiaries with five to six years of land ownership, all of the gains in earned income go towards paying the debt and increasing the net wealth of the household, rather than toward improving current welfare. Since the results become less negative and eventually positive with increasing number of years of land ownership, it is likely that improvements in this situation are only a matter of time.

**Table 2.16: Effects on Earned Income from Land Ownership Subtracting Repayment in Intensity of Treatment Fixed Effects Estimation**

| | No Discount | One Discount | Both Discounts |
|---|---|---|---|
| Land Owner $\leq$ 3 Years | -716.40*** | -512.57** | -492.48** |
| | (256.66) | (235.82) | (235.96) |
| Land Owner 4 Years | -558.49*** | -356.78* | -329.24 |
| | (205.27) | (200.80) | (200.69) |
| Land Owner 5-6 Years | -77.81 | 137.17 | 169.05 |
| | (242.01) | (234.11) | (233.12) |
| N | 1516 | 1516 | 1516 |
| Attrition Weights | Y | Y | Y |
| Regional Agricultural Controls[1] | Y | Y | Y |

Notes: ***Significant at 1%; **Significant at 5%; *Significant at 10%. Clustered standard errors in parentheses. The value of repayment was calculated uniquely for all beneficiaries depending on principal, interest and year in the program. These individually calculated repayment values were then subtracted from each beneficiaries' EI and the estimations were performed as before. [1]Regional agricultural controls include: daily agricultural wage and yield of corn. The sample contains all observations in the balanced panel with 26 outliers dropped and 4 dropped because of missing values. All values in household per capita Reais of January 2010.

## 2.8 CONCLUSION

The primary objective of the Poverty Alleviation Line of the Brazilian National Land Credit Program (PNCF-CPR) is to promote the creation of productive activities which, in turn, increase the income and wellbeing of the beneficiary population. This impact evaluation confirms that the program achieves the first part of this objective—to create productive activities—through the evidence of increased agricultural production for program beneficiaries. The results for agricultural production are highly significant and robust to alternative specifications, indicating that the program increases agricultural production after the first three years of land ownership. Using the preferred specification of the fixed effects model, we conclude that relative to the control group agricultural production increases by an average of R$750 (US$421) per person in households with five to six years of land ownership. This represents an increase of 102 percent relative to the baseline production of beneficiaries.

With regard to the welfare of the beneficiary population, earned income is a superior indicator because it accounts for the fact that beneficiaries might increase agricultural production by substituting away from labor market earnings. The analysis of earned income revealed that positive and significant effects only appear for the most seasoned beneficiaries. Relative to the control group, the fixed effects model shows that earned income increased by R$501 (US$281) per person in households with five to six years of land ownership. This increase of 35 percent relative to the baseline income of beneficiaries indicates that the program also appears to achieve the second part of its primary objective, but exclusively for beneficiaries with more than four years of land ownership. The income gain is roughly equivalent to what a poor household would have received in 2010 through the conditional cash transfer program *Bolsa Família* in exchange for ensuring that two children remained in school.

Once repayment of the PNCF-CPR loan is factored into the analysis, however, it appears that beneficiaries face a trade-off between current welfare and asset accumulation. The impact of the program net of loan payments becomes negative and significant in the early years, and only becomes positive (but not significant) with five to six years of land ownership. Thus, although the beneficiaries' earned income increased as a result of participation in the program, most of this gain goes to making debt payments for the land. In effect, their current income net of payments was no higher than income in the control group, but beneficiaries were increasing their net wealth. A more forward looking analysis suggests that—if the program impacts continue to grow at the rate observed in the first five to six years of land ownership—beneficiaries should be able to make debt payments and improve current welfare simultaneously. This is the cautiously optimistic scenario. The alternative, at least for a share of the beneficiaries, is to fall into arrears on their payments, thereby losing access to the on-time discount and to PRONAF family farm credit.

The results of this study have important implications for policy. First, since beneficiaries only see significant income gains as of the fifth year of land ownership, the grace period should be extended beyond two years to allow sufficient time for productive projects to mature. Second, policy should facilitate improved access to technical assistance and PRONAF loans, which contribute to the success of productive projects and thus to the beneficiaries' repayment capacity. Third, even with five to six years of land ownership, beneficiaries have not achieved a level of earned income that permits both a higher level of welfare and the ability to repay the loan. This problem could be overcome by spreading debt repayment over a longer horizon in order to reduce the burden of annual payments. Instead of the current fourteen to seventeen years, loans could be amortized over twenty to thirty years. Alternatively, payments of principal could grow more gradually in the initial years of the loan, tracking the expected path of income growth. Fourth, the conclusion that positive effects on income grow with time and become statistically significant as of the fifth year of land ownership underscores the importance of conducting medium term impact evaluations of asset transfer programs, rather than restricting attention to the first few years of program impacts.

The general conclusion, then, is optimistic, but cautious. The PNCF-CPR can provide a pathway out of poverty by transferring assets to the poor. There is a positive impact on earned income, which appears to be growing rapidly after the first few years of land ownership. But repayment in the early years is an issue. Beneficiaries require sufficient time on their newly acquired land to realize adequate returns on their investments. It would seem, then, that the PNCF-CPR—and asset transfer programs more generally—is a viable option for rural poverty reduction, but positive and significant results are only achieved in a matter of time.

## REFERENCES


Agüero, Jorge, Michael Carter and Ingrid Woolard. (2009). "The Impact of Unconditional Cash Transfers on Nutrition: The South African Child Support Grant." *Working Paper* 39, International Policy Centre for Inclusive Growth.

Alderman, Harold, Jere Behrman, Hans-Peter Kohler, John A. Maluccio, and Susan Cotts Watkins. (2001). "Attrition in Longitudinal Household Survey Data." *Demographic Research*, 5: 79-124.

Angrist, Joshua. (1998). "Estimating the Labor Market Impact of Voluntary Military Service Using Social Security Data on Military Applicants." *Econometrica,* 66(2): 249-288.

Angrist, Joshua and Jorn-Steffen Pischke. (2009). *Mostly Harmless Econometrics.* Princeton: Princeton University Press.

Barrett, Christopher. (2003). "Rural Poverty Dynamics: Development Policy Implications." *Working Paper*, Department of Applied Economics and Management Cornell University.

Becketti, Sean, William Gould, Lee Lillard, and Finish Welch. (1988). "The Panel Study of Income Dynamics after Fourteen Years: An Evaluation." *Journal of Labor Economics*, 6(4): 472-492.

Bird, Kate, David Hulme, Karen Moore and Andrew Shepherd. (2002). "Chronic Poverty and Remote Rural Areas." *Working Paper No 13*, Chronic Poverty Research Centre.

Carter, Michael and Dylan Fitz. (2008). "Attrition in the PCT Studies." Mimeo.

Carter, Michael and Christopher Barrett. (2006). "The Economics of Poverty Traps and Persistent Poverty: An Asset-Based Approach." *Journal of Development Studies*, 42(2): 178–199.

Chawanote, Chayanee and Christopher B. Barrett. (2014). "Farm and Non-Farm Occupational and Earnings Dynamics in Rural Thailand." *Working Paper.*

de Janvry, Alain, Jean-Philippe Platteau, Gustavo Gordillo and Elisabeth Sadoulet. (2001). "Access to Land and Land Policy Reforms." In *Access to Land, Rural Poverty and Public Action*, A. de Janvry, G. Gordillo, J. Platteau and E. Sadoulet (eds.). Oxford: Oxford University Press.

de Janvry, Alain, Elisabeth Sadoulet and Wendy Wolford. (2001). "The Changing Role of the State in Latin American Land Reforms." In *Access to Land, Rural Poverty and Public Action*, A. de Janvry, G. Gordillo, J. Platteau and E. Sadoulet (eds.). Oxford: Oxford University Press.

Deere, Carmen Diana and Leonilde Servolo de Medeiros. (2007). "Agrarian Reform and Poverty Reduction, Lessons from Brazil." In *Land, Poverty and Livelihoods in an Era of Globalization*, Akram-Lodhi, Borras and Kay (eds). New York: Routledge.

Deininger, Klaus (1999). "Making negotiated land reform work: initial experience from Brazil, Colombia, and South Africa." *World Development*, 27(4): 651-672.

Deininger, Klaus. (2001). "Negotiated Land Reform as One Way of Land Access: Experiences from Colombia, Brazil, and South Africa." In *Access to Land, Rural Poverty and Public Action*, A. de Janvry, G. Gordillo, J. Platteau and E. Sadoulet (eds.). Oxford: Oxford University Press.

Deininger, Klaus, and Gershon Feder. (2001). "Land Institutions and Land Markets." In *Handbook of Agricultural Economics*, Gordon Rausser and Bruce Gardner (eds.). Amsterdam: Elsevier.

Deininger, Klaus. (2003). *Land Policies for Growth and Poverty Reduction*. Washington, D.C.: World Bank and Oxford University Press.

Eastwood, Robert, Michael Lipton and Andrew Newell. (2010). "Chapter 65 Farm Size." *Handbook of Agricultural Economics*, 4: 3323-3397.

FECAMP. (2007). *Estudo de Avaliação de Impact do Programa Cédula da Terra (PCT)*. Campinas: FECAMP.

Finan, Frederico, Elisabeth Sadoulet, and Alain de Janvry. (2005). "Measuring the Poverty Reduction Potential of Land in Rural Mexico," *Journal of Development Economics*, 77: 27-51.

Fitzgerald, John, Peter Gottschalk, and Robert Moffitt. (1998). "An Analysis of Sample Attrition in Panel Data: The Michigan Panel Study of Income Dynamics." *The Journal of Human Resources*, 33(2): 251-299.

Griffin, Keith, Aziz Khan, and Amy Ickowitz. (2000). "Poverty and the Distribution of Land." Mimeo.

Grossi, Mauro Eduardo Del, Jose Garcia Gasques, Jose Graziano da Silva, and Junia Cristina P.R. Conceicao. (2000). *Estimativas da Famílias Potenciais Beneficiárias de Programas de Assentamentos Rurais no Brasil*. Brasília: Instituto de Pesquisa Econômica Aplicada.

Hausman, J. (2001). "Mismeasured Variables in Econometric Analysis: Problems from the Right and Problems from the Left." *Journal of Economic Perspectives*, 15(4): 57-67.

Heckman, James J. (1979). "Sample Selection Bias as a Specification Error." *Econometrica*, 47(1): 153-162.

Helfand, Steven M., Rudi Rocha and Henrique E. F. Vinhais. (2009). "Pobreza E Desigualdade De Renda No Brasil Rural: Uma Análise Da Queda Recente." *Pesquisa e Planejamento Econômico*, 39(1): 67-88.

Hoffmann, R. and M. G. Ney. (2008). "A Recente Queda da Desigualdade de Renda no Brasil: Análise de Dados da PNAD, do Censo Demográfico, e das Contas Nacionais." *Economica* 10(1): 7-39.

IFAD. (2011). *Rural Poverty Report*. Rome: International Fund for Agricultural Development.

Imbens, Guido and Jeffrey Wooldridge. (2009). "Recent Developments in the Econometrics of Program Evaluation." *Journal of Economic Literature*, 47(1): 5-86.

Keswell, Malcolm and Michael Carter. (2014). "Poverty and Land Redistribution." *Journal of Development Economics*, 110(C): 250-261.

King, Elizabeth M. and Jere R. Behrman. (2009). *Timing and Duration of Exposure in Evaluations of Social Programs*. Oxford: Oxford University Press.

Lamb, Russell L. (2003). "Inverse Productivity: Land Quality, Labor Productivity, and Measurement Error." *Journal of Development Economics*, 71: 71-95.

Maluccio, John A. (2004). "Using Quality of Interview Information to Assess Nonrandom Attrition Bias in Developing-Country Panel Data." *Review of Development Economics*, 8(1): 91-109.

Maluccio, John. (2000). "Attrition in the Kwazulu Natal Income Dynamics Study, 1993-1998." *FCND Discussion Paper*, 95.

MDA. (2009). *Programa Nacional de Crédito Fundiário Manual de Operações*. Brasília: Ministério do Desenvolvimento Agrário.

Mueller, Bernardo, Lee Alston, Gary D. Libecap, and Robert Schneider. (1994). "Land, Property Rights and Privatization in Brazil." *Quarterly Review of Economics and Finance*, 34: 261-280.

Moffitt, Robert. (2003). "Causal Analysis in Population Research: An Economist's Perspective." *Population and Development Review*, 29(3): 448-458.

Navarro, Zander. (2009). "Expropriating Land in Brazil." In *Agricultural Land Redistribution: Toward Greater Consensus*, H. Binswanger-Mkhize, C. Bourguignon, R. van den Brink (eds.). Washington D.C.: World Bank.

Ravallion, Martin. (2008). "Evaluating Anti-Poverty Programs." *Handbook of Development Economics*, 4: 3787-3846.

Sauer, Sergio. (2006). "The World Bank's Market-Based Land Reform in Brazil." In *Promised Land*, Rosset, Patel and Courville (eds). Oakland: Food First Books.

Sparovek, Gerd and Rodrigo Maule. (2009). "Negotiated Agrarian Reform in Brazil." In *Agricultural Land Redistribution: Toward Greater Consensus*, H. Binswanger-Mkhize, C. Bourguignon, R. van den Brink (eds.). Washington D.C.: World Bank.

Sparovek, Gerd , Rodrigo Maule, Alberto Barreto, Sergio Martins and Ludwig Plata. (2003). *Diagnostico dos Projetos do Credito Fundiario e Combate à Pobreza Rural*. Unpublished report. Brasilia: Ministry of Agrarian Development.

Sparovek, Gerd. (2012). *Estudos de Reordenamento Agrário*. Brasília: Ministério do Desenvolvimento Agrário.

Wooldridge, J. (2002). *Econometric Analysis of Cross Section and Panel Data.* Boston: MIT Press.

World Bank. (2007) *World Bank Annual Report.* Washington D.C.: World Bank.

# CHAPTER 3:
# João, José and The Beanstalk:
# A Siblings' Comparison of the Height of Children of Brazilian National Land Credit Program Beneficiaries

## 3.1 INTRODUCTION

In the context of the Early Childhood Development (ECD) hypothesis and the idea of the intergenerational transmission of poverty, impact evaluations of policy initiatives should assess the changes upon the youngest generations that may benefit from a program (Harper et al., 2003). Because of the critical nature of development during the prenatal period and the first few years of life, any positive or negative shocks during this period will have a lasting impact on future development (Nelson, 2000). If programs are not shown to benefit the youngest children, it is possible that some degree of poverty will continue to be transmitted through generations.

The Brazilian National Land Credit Program (*Programa Nacional de Crédito Fundiário*, PNCF) is a Market Assisted Land Reform program in Brazil. It is a mechanism through which landless workers, small farmers and the children of family farmers can obtain land via the market. The primary objective of the PNCF is to promote the creation of productive activities which will, in turn, increase the income and wellbeing of the rural population. The program works by providing subsidized loans to families or groups of families that together seek out and negotiate the purchase of land available through the market. Once the land is purchased, all beneficiaries are eligible for credit to finance infrastructure, production, and technical assistance. Given the highly unequal distribution of land in Brazil, accompanied by the fact that approximately half of the rural population of Brazil lives under the poverty line (Helfand, Rocha and Vinhais, 2009), this program has important policy implications.

It would be paramount to evaluate the impact of the PNCF-CPR on the children of the beneficiaries. If the program has a positive impact on the young children of the beneficiaries, then through the early childhood development hypothesis, these children's future outcomes may be improved. Since this is a recent program, long-term effects cannot be directly measured. However, the short-term impact on the height of children, through the mechanism of nutritional security, could show the effects on children from the acquisition of land. In order to perform this evaluation, the author, together with another enumerator, personally measured the heights of 531 siblings in 211 families in the Northeast of Brazil in 2010. Since family unobserved characteristics would be very important omitted variables when estimating the impact of the program on height, a family fixed effects model is used.

The findings suggest that being exposed to parents' land ownership through the PNCF-CPR has positive and significant effects on the height of children exposed from one to three years of age. One of the unique contributions of this paper is its focus on evaluating the benefit to children from a program that is not primarily intended to benefit the young. Because of the critical nature of the early childhood development period, the author recommends all anti-poverty programs be evaluated in such a way. Because of the intergenerational transmission of poverty, if programs do not benefit the young, then these poverty-stricken children will emerge into adulthood without the "basic capabilities" to pull themselves out of poverty (Sen, 1999). Furthermore, this paper is necessary because the PNCF-CPR is itself an important program, due to the high levels of landlessness and poverty in the Brazilian countryside. An evaluation of this program has critical policy implications throughout the developing world. Once the program is evaluated using something as tangible as the height of children, it can serve as concrete evidence of its effect.

## 3.2 BACKGROUND

### 3.2.1 Early Childhood Development and Height

Measuring the impact of an anti-poverty program on the youngest family members is important because the most critical period for human development is the *in utero* period and the first three years of life. This bold statement can be backed by the facts of neurological development. Fundamentally, human behavior, great, small, successful or wicked, originates in our brain and the neural pathways making behaviors possible critically depend on its development. The vast majority of the neurons in the brain are developed while we are still fetuses in our mothers' womb, and all major communication tracts between neurons are established within the first three years of life. By age two, human brain structure is similar to that of an adult and although the pre-frontal cortex continues to develop into late adolescence, the peak of its development takes place at age two to three (Baars and Gage, 2007, Casey et al., 2005, Huttenlocher and Dabholkar, 1997, Bourgeois, 2001, and Shaw et al., 2006). According to neuroscientist Charles Nelson (2000), it is because of the brain's plasticity in the earliest years of life that any positive or negative shock will have a greater and longer lasting effect on an individual's development. Any intervention taking place once the plasticity of the brain is no longer as great will have less returns, precisely because the brain is less receptive to change due to environmental experiences. It is during the *in utero* phase and up to three years of life that the brain is most susceptible to “experience dependent effects.” The brain continues to be plastic and mutable throughout the life course, but with every year of life, experiences must be repeated more and more times in order for a change to take place at the neuronal level. In contrast, during the earliest years, just a few experiences are sufficient to lead to neuronal pairing.

A multitude of papers exist showing the importance of the early childhood environment. Some papers focus on improved prenatal conditions and unequivocally show that improved *in utero* environments result in cohorts that are more educated, have less disabilities, earn higher wages and so on. Since it is impossible and unethical for the econometrician to randomize *in utero* conditions, most studies of this type make use of natural experiments in policy (Nilsson, 2008) or disease outbreaks (Almond, 2006). Other papers look, instead, at the post-natal environment and again show long-term benefits of early life interventions (Bleakley 2007, Currie et al., 2009). Because of this well established hypothesis that the earliest years matter so much, this evaluation on the impact of the PNCF-CPR on the young children of beneficiaries can determine whether the program will have a lasting effect.

Using height to measure this impact is ideal for a variety of reasons. First, it can serve to proxy for the quality of the earliest developmental period since the critical period for height growth is the *in utero* period and the first year of life (Tanner, Whitehouse and Takaishi, 1966). While height continues to grow past the first year, the growth velocity decreases dramatically from birth and only rises again in puberty. If nutritional shocks occur during the first few years of life, little can be done to compensate for the resulting growth failure (Martorell, 1995, and Branca et al., 1992). Measured height at any age will show a deficit if there was a period of growth retardation due to poor nutrition during the earliest years.

Second, using height can serve to predict future outcomes since stunting (low height for age), is highly correlated with low IQ, and poor cognitive and educational performance (UNICEF, 1998, and Grantham-McGregor et al., 2007). Furthermore, Victora et al. (2008) found that height for age at age two is highly predictive of future human capital attainment. Some papers focusing on the height of children evaluate the success of the PROGRESA program in Mexico. Using a design that relies on the randomized implementation of the program, these papers generally find that height for age for children 0-36 months of age improves with program participation (Gertler, 2004, Neufeld et al., 2005). Another paper evaluates the impact on height for age for South African children beneficiaries of an *unconditional* cash transfer program (Aguero, Carter and Woolard, 2009). This paper finds that increasing the amount of exposure to the cash transfers during the critical nutritional period of 0-36 months of age increases children's height for age z-scores.

### 3.2.2 *Programa Nacional de Crédito Fundiário*

The Brazilian National Land Credit Program (*Programa Nacional de Crédito Fundiário*, PNCF) was created in 2003. The program is a Market Assisted Land Reform initiative that provides subsidized loans to poor families to purchase land. There are two lines of credit within the PNCF, each of which is aimed at different target populations. The first is the *Combate à Pobreza Rural* (CPR), or Rural Poverty Alleviation line and the second is the *Consolidação da Agricultura Familiar* (CAF), or the Consolidation of Family Farming. While the second chapter is limited to families in the CPR line, the sample in this chapter includes children of beneficiaries of both PNCF lines. This section will describe the PNCF, the eligibility requirements for the beneficiaries and the land, the procedure by which families become beneficiaries, and the types of additional credit available to them.

The PNCF aims to promote access to land and to provide basic and productive infrastructure on the acquired lands. The program is run in a de-centralized fashion—each state supervising the program for its municipalities. Rural workers unions, family agriculture associations and other NGOs are also 'partners' in the program, helping with such things as the dissemination of information about the program, accepting applications for entry into the program, verifying the veracity of potential beneficiary claims to eligibility, and providing technical assistance for the families.

To be eligible for the CPR program in 2009, families needed to earn less than R$9,000 (US$5,049) per year, and have assets totaling no more than R$15,000 (US$8,415). For the CAF line, the numbers are R$15,000 and R$30,000 (US$16,830), respectively.[29] They could be owners of land as long as the plot was smaller than what is required to sustain a family. They could not be public employees or beneficiaries of another land reform program, and they must have had at least 5 years of experience as

[29] The values are according to the CPR Manual of 2009 and the CAF Manual of 2005 and are those that applied throughout the period of this study. The dollar values were calculated with the January 2010 exchange rate of R$1 to US$0.561.

farmers. Lastly, eligibility required individuals not to have previously defaulted on any debt and have all proper forms of identification.

The procedure by which families apply to the program and become beneficiaries is that potential beneficiaries must first form a group, called an association, of eligible members. After the association is formed, every member must self-declare that they meet the eligibility requirements stated above and present the necessary documents. In the CAF line, beneficiaries can take out individual loans and bypass the need to establish an association, although many still do acquire larger plots of land through an association.

It is often the case that the associations are formed already with an intended property to purchase. Once all eligibility requirements of the association members are fulfilled, the state supervisory unit of the PNCF then needs to verify the eligibility of the land. The property must not be bigger than 15 fiscal modules, yet must be large enough to provide each member of the association with sufficient land to provide for his or her family.[30] Furthermore, the property cannot be in an area of environmental protection, must be legally titled and not have any legal impediments to sale. The land cannot be owned by any family member of anyone in the association intending to purchase it, and lastly, the price must be similar to those of other properties in the same region.

A productive project is developed by the association members with help from technical assistants, and, after ensuring both the association and the land meet the eligibility requirements, the state supervisory unit of the PNCF analyzes the project and

[30] A fiscal module is a unit of measure that varies across municipalities by taking into account the primary economic activities in a region and the land necessary for such activities to sustain a family. In the Northeast of Brazil, it ranges anywhere from 5 hectares (close to capital cities) to 90 hectares in a half a dozen municipalities. This requirement ensures that land eligible for expropriation through state-led land reform cannot be purchased using a loan from the PNCF.

either approves or rejects the loan. For both lines, the maximum amount of the loan per beneficiary is R$40,000 (US$22,440), however, each region of the country has different caps, associated with local market prices. For both lines, if the principal is above R$15,000, beneficiaries have up to 17 years to repay, if the principal is below R$15,000, beneficiaries have up to 14 years to repay. The grace period is 24 months, and the interest rates vary between 2% and 5% (for CPR) and 3% to 6.5% (for CAF) depending on the principal.

In the CPR line, the entire association is deemed responsible for the repayment of the debt. In other words, if one member of the association does not pay his or her part, it must be paid by the rest of the group, lest the entire association and all its members default, which precludes them from taking out any other loan. This places a great deal of pressure on association members. In addition to this social mechanism to ensure repayment, the program also provides incentives in the form of a 15-40% discount (depending on the region) on the installments if paid on time. If the price of the land is negotiated to be less than predicted, then there is an additional 5-10% discount on installments made on time.

Besides the loan made for the purchase of the land, the program makes infrastructure grants available to the association.[31] These grants can be used for building houses for the beneficiary families, other community infrastructure projects, or even the purchasing of inputs for agricultural production. Again in an effort to create an incentive for the land price to be negotiated as low as possible, the R$40,000 cap applies to the sum

[31] In the case of the CAF line, the money available for infrastructure development is not a grant, but must also be repaid.

of the grant and loan. Thus, in the CPR line, the smaller the loan component, the larger is the grant component. The rural workers' union or local NGOs provide technical assistance to associations to help with the negotiation of the land price. After the acquisition of the land, additional technical assistance is provided to help the associations with agricultural production.

One last component worthy of mention is a related agricultural credit program that is often used in conjunction with the PNCF, the National Program to Strengthen Family Farming (PRONAF). PRONAF is a credit program available to family farmers throughout Brazil, most of whom are not part of the PNCF. PRONAF can provide additional loans to the beneficiaries of the PNCF to acquire capital for agricultural production and to provide working capital. If the association, after the grace period, defaults on their payments for the land, they will no longer be eligible for PRONAF credit.

## 3.3 EMPIRICAL STRATEGY

This paper aims to measure the effects of the PNCF on early childhood wellbeing through its impact on the height of children. Research estimating correlations between children's height and program effects has the potential to be thoroughly confounded due to the existence of omitted variables. There can be an array of omitted variables, such as a more proactive family, more educated parents, or savvier parents that could be causing an overestimation of the impact of a program on children's height. For example, a proactive family would provide a healthy *in utero* and early childhood environment, complete with investments in health, which would increase children's height. This proactive family would also be more likely to self-select for program participation. When simply estimating an Ordinary Least Squares equation in the presence of these omitted variables, the impact of a program on children's height would be biased upward. Thus, some strategy must be employed to arrive at the causal impact of a program on the height of children.

The primary data for this evaluation is a panel dataset collected in 2006 and 2010 that was commissioned by the Brazilian government to evaluate the PNCF. The first strategy employed to arrive at the causal effect on the height of children makes use of the way the primary dataset was drawn. The treatment group was drawn from program beneficiaries and the control group was drawn from the pipeline of the program—from families that had applied to and been deemed eligible for the PNCF but had not acquired land by 2010. Henceforth, families that acquired land through the PNCF will be referred to as beneficiaries and those families still waiting in the pipeline will be referred to as pipeline non-beneficiaries. By using this pipeline control group (Ravallion, 2008), *application* to the program is held constant across treatment and control groups. In principle, this strategy reduces selection bias by holding constant any unobserved characteristics that motivate families to apply to the program.

Nonetheless, even if all families have the same unobserved motivation required for program participation, different families have different observed and unobserved characteristics that influence the height of their children. These family characteristics would continue to be omitted variables biasing the estimation of the effects on height even with the use of a pipeline control group. This bias would undermine the ability to identify the variation in height due to program participation since it would be confounded with variation in height due to family unobserved characteristics. Assuming these family unobserved characteristics are the same for all siblings within a family—all children are treated more or less equally by the parents—then one way to eliminate the confounding factor of family unobserved characteristics is to collect the heights of siblings *within one family* (Ashenfelter and Zimmerman, 1997). Each sibling, born at a different time, will have different levels of exposure to the program, while having the same amount of exposure to the family unobserved characteristics. Acquiring data from multiple siblings in each family allows for the use of a family fixed effects model, thus netting out the effect of unobserved family characteristics.

The effect of the PNCF on the height of children was identified using the following estimating equation:

$$HAZ_{\mathrm{if}} = \alpha + \beta_1 Exposure_{\mathrm{if}} + \beta_2 Exposure^2_{\mathrm{if}} + \beta_3 X_{\mathrm{if}} + \beta_4 Family_{\mathrm{f}} + \varepsilon_{\mathrm{if}} \qquad (3.1)$$

where the subscripts i and f represent individual and family, respectively. *HAZ* is height for age z-score and *exposure* indicates exposure to parents' land ownership during the critical period for height growth—both defined in detail below. *X* are control variables sex, indicator variables that specify the quarter of birth of the child, the yield of corn in the state and year of birth of the child, and the length of time the family owned land on the day of the child's conception. *Family* represents the family fixed effect. When the family fixed effect is included, the sample is limited to the children of beneficiaries only. The inclusion of family fixed effects eliminates any time-invariant family characteristics, both observed and unobserved. The identification of the effect of exposure to the PNCF is based upon the relationship between differences in exposure across siblings and differences in the HAZ across siblings, netting out the effect of family time-invariant characteristics by including the family-level fixed effects.

In order to make use of the control group, another specification is run without the inclusion of family fixed effects. In this specification, only pipeline non-beneficiaries and beneficiaries with some exposure to their parents' land ownership are included in the sample. This is done so that the identification comes from differences across parents' beneficiary status and differences in the HAZ across children. In both specifications, the standard errors for the regression coefficients were calculated with corrections for clustering to allow for the possibility of heteroskedasticity across geographical regions or correlation of errors across birth years within a geographical region. Since treatment, or exposure, in the two preceding estimations is a continuous variable, the critical identification assumption is that the magnitude of exposure is unrelated to unobserved factors that can affect children's height (Aguero, Carter and Woolard 2009 and Hirano and Ibens 2004). Since the date of land acquisition is most likely exogenous to children's height, the use of this continuous treatment estimator is deemed valid, although nothing can be done to test this validity directly. The use of the family fixed effects in the first specification should difference out any family unobserved characteristics that would cause both eagerness in land acquisition, timing of child-bearing and children's nutritional wellbeing.

In order to relax the assumptions regarding the functional form of the relationship between exposure and HAZ, another estimation is run. This estimation entails substituting the *exposure* variable and its squared term above in equation 3.1 with a series of indicator variables for different levels of exposure. The first category is being exposed for more than zero but less than four quarters, the second is being exposed for four or more quarters but less than eight quarters, the third is being exposed for eight or more quarters but less than twelve quarters, the fourth is being exposed for twelve or more but less than sixteen quarters and the last category is for exposure from sixteen to nineteen quarters—the last three quarters equivalent to being exposed in the prenatal period. The sample is limited to beneficiary children only and the family fixed effect is included in every specification. The omitted category in this estimation is the group of siblings with no exposure to parents' land ownership. This final estimation, relaxing the assumptions with respect to the functional form, is the preferred specification of this paper.

In the three preceding estimations, the exposure of children to their parents' land ownership identifies the effect of the program. The definition of the critical period for exposure is of utmost importance to the results of the estimations. In order to accurately assign a critical period in which exposure to parents' land ownership will impact height, a nonparametric regression was run to identify at which ages children of beneficiaries continued to benefit from the acquisition of land. This estimation entailed substituting the *exposure* variable and its squared term in equation 3.1 above for a series of indicator variables identifying the age of children at the date of land acquisition. It is important to note that the previous estimation made the assumption that children four years and older were unexposed while this estimation makes no such assumption. Any effects seen by the age of the child at the date of land acquisition can help to specify the critical period. The sample for this estimation includes beneficiary families only, and makes use of family fixed effects in order to difference out time-invariant family characteristics. Once again, the standard errors are clustered at the level of municipality.

### 3.4 DATA AND DESCRIPTIVE STATISTICS

All families in the primary dataset in 2010 with at least two children younger than ten years old were identified for the measurement of height. There were a total of 255 families in both the beneficiary and pipeline non-beneficiary groups of the PNCF in the Northeast of Brazil in 2010. To be clear, beneficiary members are those that had already received their land, while pipeline non-beneficiary members were families that had passed the eligibility requirements and were enrolled in the program, but were still waiting for the approval of the productive project and the acquisition of land. Heights were collected from 211 of these 255 families. The missing data of these 44 families, in addition to attrition from the primary panel data between 2006 and 2010 is discussed in detail in Section 3.6.

A total of 531 children were measured according to standard anthropometric guidelines (WHO, 1995).[32] These guidelines specify that children over the age of two be measured standing next to a wall, while children younger than two years be laying on a flat surface, with the enumerators applying gentle pressure to the knees of the child in order to measure the accurate length from the top of the head to the soles of the feet. Despite great measures to accurately record height, four observations were found to be

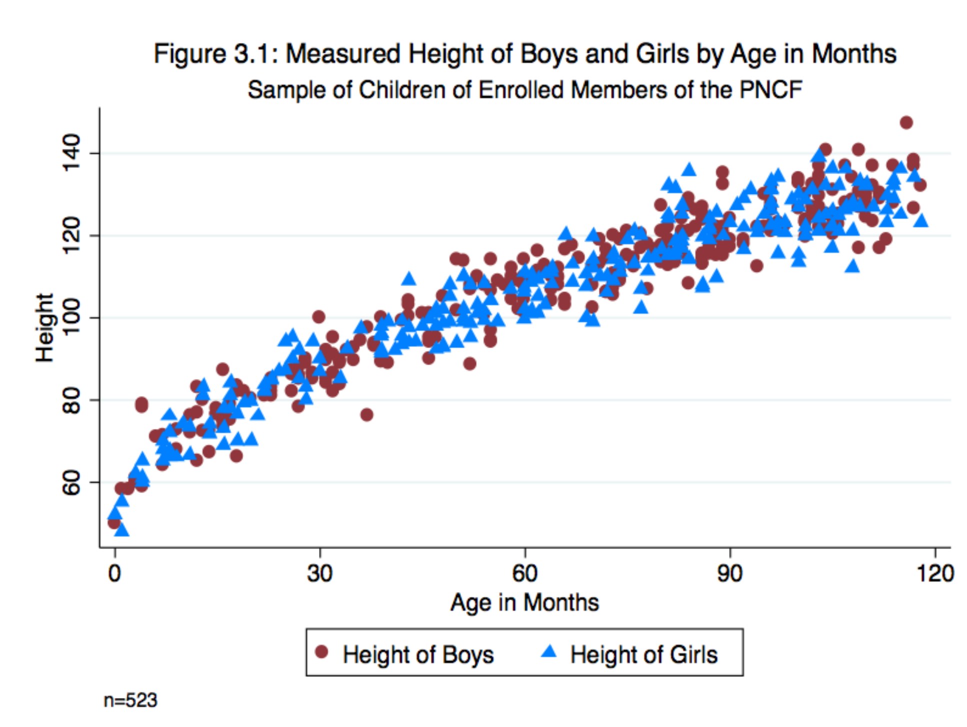
[32] Thirty-two percent of the height data was gathered by the author during the collection of data for the government commissioned impact evaluation. The author gathered the remaining sixty-eight percent in a return visit using a grant from BASIS.

outliers, defined as having an absolute value for the height for age z-score greater than 3.5, and were deleted from the analysis.[33] Table 3.1 displays the number of children per family, while Figure 3.1 displays the height for boys and girls by age in months.

**Table 3.1: Number of Siblings per Family**

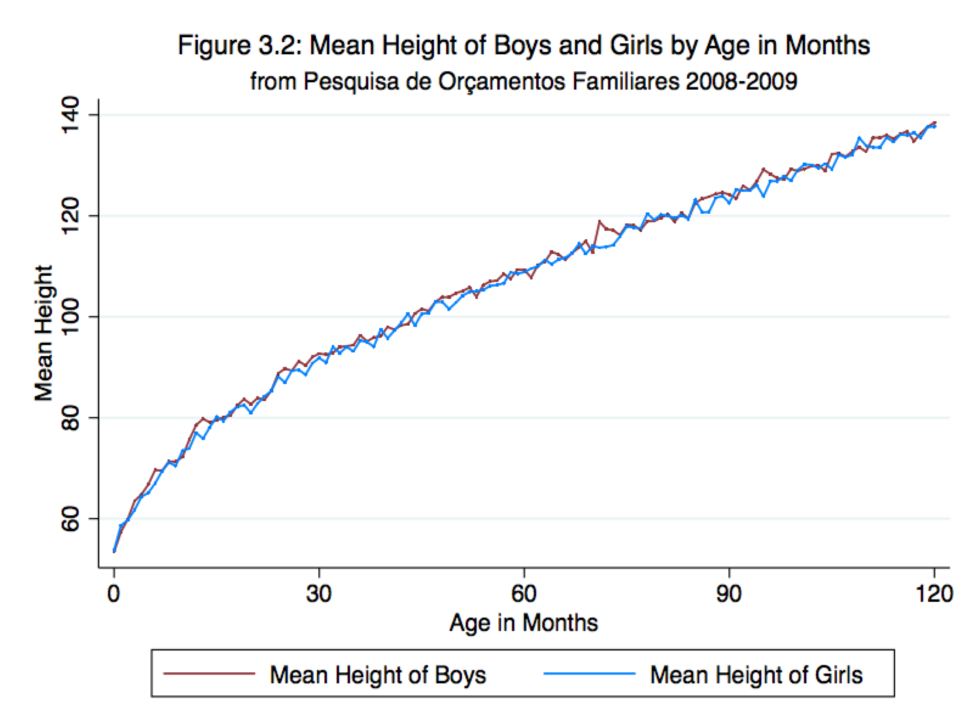
| Number of Siblings | Number of Families | Total Sample Size |
|---|---|---|
| 2 | 127 | 254 |
| 3 | 57 | 171 |
| 4 | 18 | 72 |
| 5 | 4 | 20 |
| 6 | 1 | 6 |
| **Total** | **207** | **523** |

Notes: Four heights were dropped as outliers, together with the heights of their sibling pairs, for a total of 4 families dropped.




[33] The four heights that were deemed implausible were deleted from the analysis (and the heights of the deleted observations' siblings, since the estimation makes use of a family fixed effects model and there were only two children per family in each case). There were other heights that seemed high or low for a given age, but they were judged to be plausible by comparing to the heights of other siblings and parents in the same family, and as such kept in the analysis.

Simply looking at a child's height, however, does not reveal whether the child suffers any developmental lag. In order to assess the quality of the child's growth, one must compare the height to a standard. With this in mind, the mean and standard deviation of the height of children in Brazil was obtained from the Brazilian survey *Pesquisa de Orçamentos Familiares* (POF, the Family Expenditures Survey) of 2008-2009, in order to calculate height for age z-scores (HAZ). The POF provided 28,038 measures of the height of children younger than 10 years old throughout Brazil. The mean and standard deviation of the height of children was calculated by age—measured in months—and sex, using the sampling weights provided by the survey. Age was chosen to be measured in the unit of months to both allow for a precise comparison with children of the same age, and to ensure there were sufficient observations within each chosen unit of time to calculate the average. The lowest number of observations by month of age was 55 for girls and 81 for boys, while the mean number of observations per month of age was 114 and 122, respectively. Thus for each month of life, the average and standard deviation of the height of boys and girls throughout Brazil was calculated and is displayed in Figure 3.2.




The HAZ for each child in the PNCF sample was calculated by subtracting the child's height from the mean for all children with the same sex and same age in months, and dividing by the standard deviation for all children with the same sex and same age in months, using the POF data. Children with a HAZ of lower than negative two are classified as stunted by the World Health Organization (1995). Besides showing whether the children suffer any developmental lag, the HAZ should also serve in removing any upward time trend that may exist in the heights of children in Brazil. Figure 3.3 displays a scatter diagram of z-scores for boys and girls in the PNCF sample by age in months, while Table 3.2 displays the mean HAZ by age in years and sex. There is only one significant difference between male and female children, at two years of age. Nevertheless, the sample size for females is too small to determine if this difference would persist with more data. Since no significant differences between gender were systematically observed, the estimations were performed with male and female children pooled.

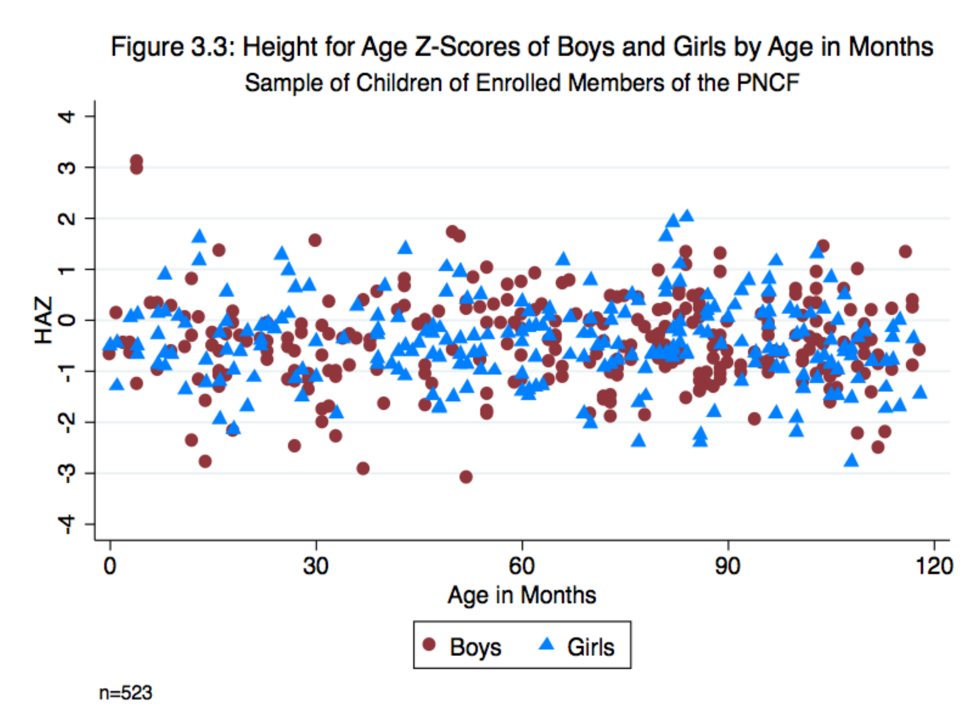



Table 3.2: Mean Height for Age Z-Scores by Age in Years and Sex

| | | Girls | | | Boys | | | | Both | |
|---|---|---|---|---|---|---|---|---|---|---|
| Age | N | Mean | SD | N | Mean | SD | p-value | N | Mean | SD |
| 0 | 18 | -0.3818 | 0.57 | 15 | 0.0990 | 1.30 | 0.1619 | 33 | -0.1632 | 0.98 |
| 1 | 20 | -0.5353 | 0.95 | 26 | -0.6651 | 0.91 | 0.6394 | 46 | -0.6087 | 0.92 |
| 2 | 15 | -0.2635 | 0.92 | 29 | -0.8590 | 0.81 | 0.0335** | 44 | -0.6560 | 0.89 |
| 3 | 22 | -0.3351 | 0.62 | 18 | -0.5017 | 0.98 | 0.5686 | 40 | -0.4211 | 0.80 |
| 4 | 24 | -0.4202 | 0.81 | 23 | -0.2097 | 1.20 | 0.4689 | 47 | -0.3172 | 0.98 |
| 5 | 27 | -0.4819 | 0.79 | 26 | -0.2628 | 0.69 | 0.2852 | 53 | -0.3744 | 0.74 |
| 6 | 35 | -0.2874 | 0.86 | 43 | -0.5199 | 0.67 | 0.1861 | 78 | -0.4156 | 0.77 |
| 7 | 22 | -0.3207 | 1.00 | 36 | -0.4416 | 0.85 | 0.6260 | 58 | -0.3957 | 0.91 |
| 8 | 38 | -0.4381 | 0.86 | 37 | -0.3842 | 0.70 | 0.7682 | 75 | -0.4115 | 0.78 |
| 9 | 21 | -0.8811 | 0.68 | 26 | -0.5144 | 0.93 | 0.1388 | 47 | -0.6783 | 0.84 |

Notes: p-value for the null hypothesis that the mean for girls is equal to the mean for boys: ***Significant at 1%; **Significant at 5%; *Significant at 10%.

Each siblings' exposure to the program was calculated as the proportion of days that the child was exposed to their parents' land ownership during the critical period for linear growth. The critical period for height growth is defined as the *in utero* period and up to four years of age.[34] If a child completes four years of age before the date of his or her parents' acquisition of land, then, for the purposes of this paper, he or she is unexposed to the program. If a child is conceived before the date of land acquisition but completes four years of age after the land acquisition, then his or her exposure is measured as the fraction of days he or she was exposed, divided by the number of days in the critical period (266 days in the prenatal period and 1460 in the postnatal period for a total of 1726). If a child is conceived after the parents' acquisition of land, then his or her exposure is total and the exposure value is equal to one. Of course, only *beneficiaries* of the PNCF have received land, thus the exposure of all children of non-beneficiaries is zero. Table 3.3 displays a frequency distribution of children's exposure, together with the mean and standard deviation of HAZ by exposure. As can be seen, while children with some exposure and full exposure have higher HAZ, the least negative HAZ is for children with only some exposure. Although not shown in the table, the mean HAZ for children with some exposure is statistically different from the mean for beneficiary children with no exposure, significant at the five percent level. There is no statistical difference between beneficiary children with no exposure and those with full exposure. This finding is explored in detail below.

[34] While the standard critical period for linear growth defined in the medical literature is the *in utero* period up to the first or second year of life, a flexible approach using indicator variables for age at land acquisition revealed that children were still benefitting from exposure to parents' land ownership at age three (see Table 3.9). Thus, the critical period was delineated as the prenatal period and up to age four.

**Table 3.3: Levels of Exposure, Mean Height for Age Z-Scores and Frequency**

| Level of Exposure | Mean HAZ | SD | N | Percent of Sample |
|---|---|---|---|---|
| Pipeline Non-Beneficiary Child | -0.5099 | 0.90 | 169 | 32.31% |
| Beneficiary Child with No Exposure | -0.5736 | 0.83 | 89 | 17.02% |
| Beneficiary Child with Some Exposure | -0.3611 | 0.83 | 185 | 35.37% |
| Beneficiary Child with Full Exposure | -0.4060 | 0.87 | 80 | 15.30% |

Notes: Pipeline non-beneficiary children have no exposure.

Examining only the children of beneficiaries, the younger children have higher levels of exposure to the program, since the earliest date of land acquisition took place in 2004 (see Figure 3.4). In Table 3.4, the mean HAZ by age at land acquisition is displayed. This table shows that the largest group of beneficiary children was that conceived after the acquisition of land. In order to decide the best model for analyzing the impact of exposure on HAZ, exposure was divided into five discrete bins. Figure 3.5 displays the relationship between exposure and HAZ. The relationship appears to be highly nonlinear and thus a second order polynomial was used.

Figure 3.4: Mean Exposure to the PNCF by Age in Months

Sample of Children of Beneficiaries of the PNCF

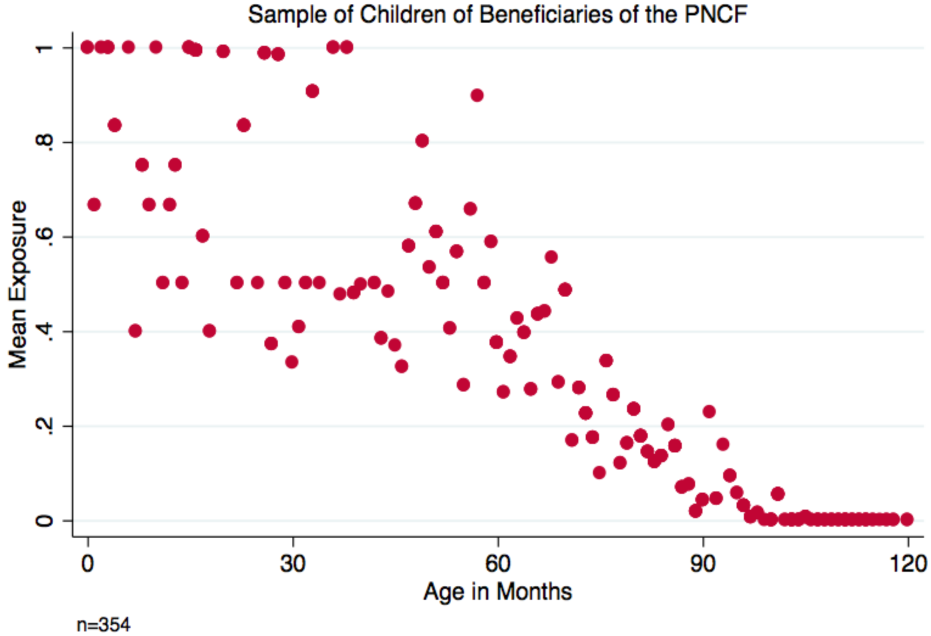
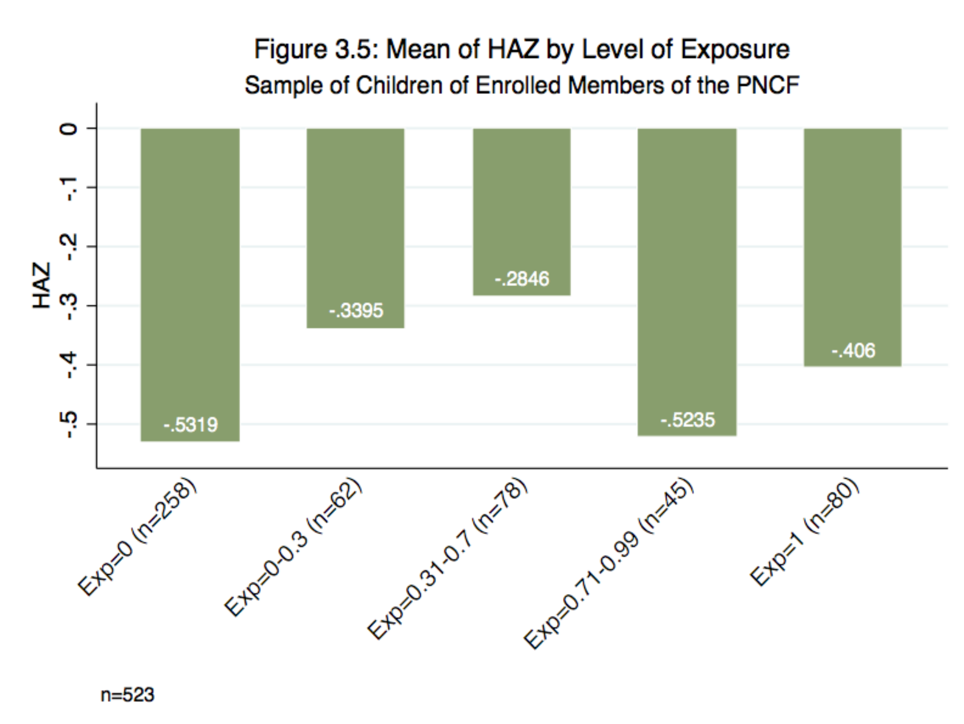


**Table 3.4: Mean Height for Age Z-Scores by Age at Land Acquisition**

| Age at Land Acquisition | Mean HAZ | SD | N |
|---|---|---|---|
| Conceived After Land Acquisition | -0.4119 | 0.88 | 78 |
| In Utero at Land Acquisition | -0.6603 | 0.93 | 27 |
| Less Than One Year Old | -0.3060 | 0.88 | 37 |
| One Year Old | -0.2371 | 0.72 | 37 |
| Two Years Old | -0.3188 | 0.82 | 49 |
| Three Years Old | -0.3678 | 0.80 | 37 |
| Four Years Old | -0.4958 | 0.76 | 41 |
| Five Years Old | -0.5107 | 0.82 | 33 |
| Six Years Old and Older | -0.9249 | 1.01 | 15 |

Notes: Includes beneficiary children only.




## 3.5 ECONOMETRIC RESULTS

Table 3.5 displays the effects on HAZ of exposure to parents' land ownership during the critical period for linear growth—the *in utero* period and up to four years of age. All specifications in the table include the family fixed effects. In column one, neither the squared term nor the control variables are included. In column two, the squared term is included, to show that the model should indeed be curvilinear. Starting in column three, sex is included as a control variable to capture any differences that may be caused by gender. In column four, birth quarter dummies are introduced in order to capture any seasonal variation in HAZ. The yield of corn by year of birth and state is included in column five in order to capture any differences in HAZ cause by time varying geo-climactic conditions. And finally, in the last column the number of months of parents' land ownership on the day of a child's conception is included in order to control for time varying household characteristics.

**Table 3.5: Effects on Height for Age Z-Score from Exposure to Parents' Land Ownership for Beneficiary Children Using Family Fixed Effects**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Exposure | -0.080 | 1.251*** | 1.250*** | 1.230*** | 1.216*** | 1.162*** |
| | (0.105) | (0.324) | (0.323) | (0.317) | (0.326) | (0.366) |
| Exposure Squared | | -1.322*** | -1.322*** | -1.308*** | -1.279*** | -1.206*** |
| | | (0.322) | (0.322) | (0.315) | (0.332) | (0.389) |
| Sex | | | -0.009 | -0.007 | -0.008 | -0.009 |
| | | | (0.072) | (0.071) | (0.071) | (0.071) |
| Yield of Corn | | | | | -0.045 | -0.030 |
| | | | | | (0.130) | (0.132) |
| Length of Land Ownership | | | | | | -0.002 |
| | | | | | | (0.006) |
| N | 354 | 354 | 354 | 354 | 354 | 354 |
| Family Fixed Effects | Y | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | N | N | Y | Y | Y |

Notes: Clustered standard errors in parentheses. ***Significant at 1%; **Significant at 5%; *Significant at 10%. The sample is limited to the children of beneficiaries with 8 outliers dropped and exposure is measured as the proportion of time spent under parents' land ownership from the prenatal period up to four years of age.

Exposure and its squared terms are significant in determining HAZ and the coefficients are robust to the inclusion of different controls. The inclusion of sex in column three has a very small effect on the coefficients. The coefficients on exposure and the quadratic term decrease in magnitude with the inclusion of birth quarter in column four. The inclusion of yield of corn in the penultimate column does not substantially alter the coefficients. Lastly, the inclusion of the length of parents' land ownership in the final column reduces the magnitude of the estimates. Nevertheless, exposure and exposure squared both remain statistically significant at one percent.

Instead of using the siblings without exposure to parents' land ownership as the control group, the next estimation uses the sample of pipeline non-beneficiary children and only the sample of beneficiary children with at least some exposure. In this way, the pipeline non-beneficiary children become the control group for the exposed children, and the family fixed effects are not used. Table 3.6 displays the results for this estimation. Although all coefficients are significant at the ten percent level once the squared term is included, they are all of much smaller magnitude than the coefficients in the siblings' specification.

**Table 3.6: Effects on Height for Age Z-Score from Exposure to Parents' Land Ownership Using Pipeline Non-Beneficiary Children as a Control Group**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Exposure | 0.055 | 0.963* | 0.960* | 0.976* | 0.917* |
| | (0.127) | (0.508) | (0.507) | (0.520) | (0.517) |
| Exposure Squared | | -0.946* | -0.944* | -0.968* | -0.901* |
| | | (0.480) | (0.480) | (0.498) | (0.499) |
| Sex | | | -0.060 | -0.053 | -0.052 |
| | | | (0.085) | (0.083) | (0.084) |
| Yield of Corn | | | | | -0.056 |
| | | | | | (0.092) |
| N | 434 | 434 | 434 | 434 | 434 |
| Family Fixed Effects | N | N | N | N | N |
| Birthquarter Dummies | N | N | N | Y | Y |

Notes: Clustered standard errors in parentheses. ***Significant at 1%; **Significant at 5%; *Significant at 10%. The sample is limited to the children of beneficiaries with at least some exposure (N=271) with 8 outliers dropped and pipeline non-beneficiary children (N=169). Since length of parents' land ownership only exists for beneficiary children, it is not included in this specification. Exposure is measured as the proportion of time spent under parents' land ownership from the prenatal period up to four years of age.

Since these estimated coefficients from Tables 3.5 and 3.6 are derived using a polynomial regression, the predicted effect on HAZ depends on the value of exposure, and thus the coefficients become difficult to interpret. Table 3.7 below uses the final specification from each table (the specification with all available control variables included) and presents the predicted effects on HAZ from incremental increases in exposure. Exposure increases incrementally by years and, in the case of prenatal exposure, by nine months. Because the upper limit of the critical period is four years of age, going from no exposure to one year of exposure means spending the third year of life, just before turning four, under parents' land ownership. Going from one year of exposure to two years of exposure means spending the second and third year of life under parents' land ownership, and so on. In Table 3.7, both the siblings' comparison from Table 3.5 and the beneficiary children and pipeline non-beneficiary children comparison from Table 3.6 show very similar patterns in the predicted change from increasing amounts of exposure. In all cases, the siblings' comparison yields predicted and percentage changes of greater magnitude than the comparison using pipeline non-beneficiary children as the control group. Nonetheless, both predict improvements in HAZ by increasing from zero to one year of exposure, as well as increasing from one year to two years of exposure. Further increases beyond 2 years of exposure lead to progressively larger decreases in HAZ. Going, for example, from four years of exposure to full exposure, decreases HAZ by 38 percent, in the case of the siblings' comparison. Because of the quadratic functional form, these decreasing effects should be expected.

**Table 3.7: Mean HAZ, Number of Observations and Predicted Change on HAZ Calculated Using Quadratic Polynomial Coefficients from Tables 3.5 and 3.6 for Increasing Amounts of Exposure**

| | | Siblings' Comparison | | | | Pipeline NB Control Group | | | |
|---|---|---|---|---|---|---|---|---|---|
| **Exposure Began** | **Amount of Exposure** | **N** | **Mean HAZ** | **Predicted Change** | **% Δ** | **N** | **Mean HAZ** | **Predicted Change** | **% Δ** |
| No Exposure | 0 Years | 89 | -0.5736 | - | - | 169 | -0.5099 | - | - |
| From Age 3 | 1 Year | 37 | -0.3678 | 0.1914 | 45% | 37 | -0.3678 | 0.1533 | 36% |
| From Age 2 | 2 Years | 49 | -0.3188 | 0.0842 | 20% | 49 | -0.3188 | 0.0733 | 17% |
| From Age 1 | 3 Years | 37 | -0.2371 | -0.0236 | -6% | 37 | -0.2371 | -0.0073 | -2% |
| From Birth | 4 Years | 37 | -0.3060 | -0.1320 | -31% | 37 | -0.3060 | -0.0883 | -21% |
| From Womb | 4.75 Years | 105 | -0.4758 | -0.1640 | -38% | 105 | -0.4758 | -0.1150 | -27% |
| Total | - | 354 | -0.4247 | - | - | 434 | -0.4273 | - | - |

Notes: Predicted change calculated using coefficients from the sixth column of Table 3.5 for the siblings' comparison and using coefficients from the fifth column of Table 3.6 for the pipeline non-beneficiary control group specification. The percentage change is calculated by dividing the predicted change by the mean for all observations in each estimation type, displayed in the last row under "Total".

In order to perform a more flexible estimation, where the functional form does not need to be specified, a series of indicator variables were included for different amounts of exposure. Table 3.8 displays the results. The coefficients are highly robust to the inclusion of the control variables sex, birth quarter, yield of corn by state and birth year, and length of parent's land ownership. All columns include family fixed effects and the omitted category is the group of siblings unexposed to parents' land ownership. Observing the coefficients from the fifth column, being exposed up to four quarters increases siblings' HAZ by 0.323 compared to not being exposed to parents' land ownership at all. This is an astounding increase of 76 percent when compared to the mean for all beneficiary children, significant at one percent. Children in this category would have spent at least one quarter of their fourth year of life exposed. Being exposed for four to eight quarters, or spending at least one quarter of the third year of life exposed increases HAZ by 0.281 compared to unexposed siblings, an increase of 66 percent, significant at the five percent level. Exposure for eight to twelve quarters, or at least one quarter during the second year of life exposed, causes the greatest increase in HAZ, at 0.351, or an increase of 83 percent, at the one percent level of significance. While the effects for exposure from twelve to sixteen quarters and from sixteen to nineteen quarters are not significant, they are not negative, as they had been when the estimated functional form was a second order polynomial. These results make it clear that children from the age of one and older were the ones to benefit the most from their parents' acquisition of land.

**Table 3.8 Effects on Height for Age Z-Score by Different Quantities of Exposure to Parents' Land Ownership for Beneficiary Children Using Family Fixed Effects**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Exposed 0+ up to 4 Quarters | 0.349*** | 0.351*** | 0.326*** | 0.331*** | 0.323*** |
| | (0.121) | (0.122) | (0.121) | (0.123) | (0.125) |
| Exposed 4 up to 8 Quarters | 0.296** | 0.297** | 0.285** | 0.287** | 0.281** |
| | (0.136) | (0.136) | (0.132) | (0.133) | (0.132) |
| Exposed 8 up to 12 Quarters | 0.384*** | 0.382*** | 0.353*** | 0.353*** | 0.351*** |
| | (0.133) | (0.135) | (0.134) | (0.136) | (0.135) |
| Exposed 12 up to 16 Quarters | 0.243 | 0.243 | 0.248 | 0.255 | 0.240 |
| | (0.177) | (0.178) | (0.177) | (0.178) | (0.194) |
| Exposed 16 up to 19 Quarters | 0.022 | 0.022 | 0.005 | 0.027 | 0.058 |
| | (0.112) | (0.112) | (0.116) | (0.123) | (0.138) |
| Sex | | -0.015 | -0.015 | -0.016 | -0.017 |
| | | (0.077) | (0.076) | (0.076) | (0.076) |
| Yield of Corn | | | | -0.061 | -0.033 |
| | | | | (0.136) | (0.140) |
| Length of Land Ownership | | | | | -0.003 |
| | | | | | (0.007) |
| N | 354 | 354 | 354 | 354 | 354 |
| Family Fixed Effects | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | N | Y | Y | Y |

Notes: Clustered standard errors in parentheses. ***Significant at 1%; **Significant at 5%; *Significant at 10%. The sample is limited to the children of beneficiaries with 8 outliers dropped and the omitted category is the group of siblings unexposed to parents' land ownership.

The final estimation run was the most flexible one, no functional form was imposed and no assumptions were made surrounding the critical period for exposure. Instead, this regression was run in order to determine at which age children's HAZ no longer seemed to improve from parents' land acquisition. Table 3.9 displays the results for this estimation. The results show that, indeed, the added benefit to HAZ from parents' land acquisition through the presumed mechanism of nutritional security ends at three years of age. Children four and five years old at the date of land acquisition see no significant improvement to their HAZ.

**Table 3.9: Effects on Height for Age Z-Score by Age at Land Acquisition for Beneficiary Children Using Family Fixed Effects**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Conceived After Land Acquisition | 0.235 | 0.245 | 0.257 | 0.273 | 0.457* |
| | (0.261) | (0.260) | (0.255) | (0.251) | (0.249) |
| In Utero at Land Acquisition | 0.134 | 0.145 | 0.149 | 0.153 | 0.121 |
| | (0.281) | (0.273) | (0.268) | (0.268) | (0.273) |
| Less than One Year Old at Land Acquisition | 0.440 | 0.452 | 0.486 | 0.484 | 0.454 |
| | (0.293) | (0.297) | (0.297) | (0.296) | (0.308) |
| One Year Old at Land Acquisition | 0.566** | 0.574** | 0.571** | 0.562** | 0.558** |
| | (0.258) | (0.253) | (0.247) | (0.250) | (0.253) |
| Two Years Old at Land Acquisition | 0.489* | 0.502* | 0.519** | 0.512** | 0.501* |
| | (0.263) | (0.261) | (0.258) | (0.257) | (0.261) |
| Three Years Old at Land Acquisition | 0.538** | 0.553** | 0.554** | 0.552** | 0.544** |
| | (0.261) | (0.261) | (0.249) | (0.248) | (0.253) |
| Four Years Old at Land Acquisition | 0.226 | 0.239 | 0.276 | 0.264 | 0.272 |
| | (0.251) | (0.247) | (0.252) | (0.253) | (0.256) |
| Five Years Old at Land Acquisition | 0.203 | 0.215 | 0.241 | 0.233 | 0.234 |
| | (0.269) | (0.268) | (0.268) | (0.268) | (0.272) |
| Sex | | -0.030 | -0.031 | -0.032 | -0.034 |
| | | (0.074) | (0.074) | (0.075) | (0.077) |
| Yield of Corn | | | | -0.059 | 0.009 |
| | | | | (0.132) | (0.138) |
| Length of Land Ownership | | | | | -0.011 |
| | | | | | (0.007) |
| N | 354 | 354 | 354 | 354 | 354 |
| Family Fixed Effects | Y | Y | Y | Y | Y |
| Birth Quarter Dummies | N | N | Y | Y | Y |

Notes: Clustered standard errors in parentheses. ***Significant at 1%; **Significant at 5%; *Significant at 10%. The sample is limited to the children of beneficiaries with 8 outliers dropped and the omitted category is beneficiary children six years of age and older.

### 3.6 ROBUSTNESS CHECKS

In order to ensure that the estimates presented are robust to alternative specifications and that a causal interpretation is appropriate, a variety of robustness checks are considered. First, when using a fixed effects model, there exists a concern that measurement error in the explanatory variables could lead to less precise standard errors that could lead to insignificant effects (Hausman, 2001). Since a significant effect is found, it is likely providing a lower bound due to the downward bias on the estimate from any potential measurement error in height. Second, even with the use of family fixed effects, there is a concern that heterogeneity in the treatment of siblings exists. The concern is that if one sibling is taller than the other, this sibling might be receiving preferential treatment. There is, unfortunately, nothing that can be done to show this is not the case. Nonetheless, it can be intuitively argued that not all families systematically and purposefully mistreated the children that were not exposed to the PNCF in early childhood. It is more plausible that the observed effect is caused by the conditions the family was living due to program participation.

Third, the original dataset of beneficiaries and non-beneficiaries of the PNCF was a randomly selected sample created in 2006. The heights of children are drawn from the 2010 sample, which suffered 42.1% attrition as compared to the 2006 sample. Attrition is problematic if it has made the 2010 sample non-representative of beneficiaries and pipeline non-beneficiary enrolled members of the PNCF. Nevertheless, the tests conducted in the impact evaluation of the PNCF (see chapter 2 of dissertation) indicate that attrition was mostly random in terms of unobservable characteristics. If anything, attrition led to both beneficiaries with higher-than-average outcome variables and pipeline non-beneficiaries with lower-than-average outcome variables to attrit from the panel. Assuming that HAZ will be correlated with a family's income and production variables, this pattern to attrition actually works against finding positive and significant results for exposure, since the remaining pipeline non-beneficiary group appears stronger than it actually is and the beneficiary group appears weaker than it actually is. This only matters for the estimation using the pipeline non-beneficiary children as the control group. Future work will entail more careful analysis of how attrition from the panel affects the siblings' comparison.

In addition to the attrition from the panel, missing height data exists for the sample of families with at least two children under ten, identified using the data from 2010. 44 families, for a total of 109 children, were not measured due to a variety of reasons. Some families were not measured because the family could not be located (eight cases), others because the nuclear family relocated to a different city (one case) and others still because the family broke apart (four cases). Seven families were not measured since the children were miscoded as being under ten years old, and five families were not measured because the enumeration ended before the collection was complete. Eight families were not measured due to unrecorded reasons. Lastly, eleven families were not measured due to cost considerations—the families were located in the states of Alagoas, Sergipe or Espirito Santo and the cost of visiting three extra states was deemed higher than the benefit of obtaining fourteen additional observations.[35]

This case of "attrition" is unique since the only variable missing is height. Income and production variables, along with some demographic variables are available for all 255 families. Probit regressions were run where the dependent variable was equal to one if the family was missing height data and zero otherwise. Individual characteristics of the

[35] Note that many observations of height were collected from the states of Alagoas and Sergipe during the collection of data for the government commissioned impact evaluation of the PNCF. It was only in the return visit that the decision was made to not revisit these states precisely because most of the families identified had already been measured. With respect to Espirito Santo, only one family needed to be measured in the return visit and as such it was not cost effective to obtain that observation.

head of household were included as controls, as was the household's urban status. Table 3.10 displays the results for the probit regressions.[36] The only significant variables in predicting missing height are age of head of household and "white," which indicates a family is Caucasian. Missing families have heads of household that are older, significant at one percent, and were more Caucasian, on average, significant at five to ten percent. These differences in age and race and other observable characteristics are, of course, less problematic since their effect is netted out using the family fixed effects. A greater problem would exist if the missing data were systematically missing for children with higher or lower-than-average HAZ.

[36] Agricultural production is defined as the total value of agricultural production (including animal production), whether sold, stocked, exchanged or consumed and earned income is defined as the value of net agricultural production—total agricultural production minus variable costs—plus income earned in the labor market and from self employment activities.

**Table 3.10: Probits for the Probability of Missing Height**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Agricultural Production | -0.0000 | | -0.0001 | |
| | (0.0000) | | (0.0001) | |
| Earned Income | | 0.0000 | | -0.0000 |
| | | (0.0000) | | (0.0000) |
| Age | | | 0.0355*** | 0.0344*** |
| | | | (0.0132) | (0.0133) |
| Sex | | | -0.2422 | -0.2292 |
| | | | (0.2699) | (0.2732) |
| White | | | 0.5419** | 0.5093* |
| | | | (0.2668) | (0.2704) |
| Married | | | 0.4179 | 0.3840 |
| | | | (0.2842) | (0.2828) |
| Years of Schooling | | | -0.0090 | -0.0130 |
| | | | (0.0351) | (0.0343) |
| Urban | | | 0.4249 | 0.4461 |
| | | | (0.3696) | (0.3652) |
| N | 254 | 254 | 254 | 254 |
| State FE | N | N | Y | Y |

Notes: Clustered standard errors in parentheses. ***Significant at 1%; **Significant at 5%; *Significant at 10%. The sample is limited to the to the 255 families identified for measure in 2010 with 1 dropped due to missing observation for years of schooling. The demographic characteristics of the head of household were included as control variables. Of the individual characteristics used in chapter 2, years of experience as a farmer was also considered as a control variable however too many values for this variable were missing.

Although the HAZ of missing observations remains unavailable, it can be said that families with increased levels of production and income variables, hence increased wealth, would theoretically provide more nutritionally secure environments for their children. These more nutritionally secure environments would, in turn, lead to higher-than-average HAZ. As can be seen in Table 3.10, neither the income nor production variables are significant in predicting which families are missing height, regardless of the inclusion of control variables. This result serves as suggestive evidence that the HAZ of missing observations would not have been higher or lower than the average HAZ of non-missing observations. Because of this result and also because of the variety of reasons some families were not measured, it is suspected that the data is missing at random.

A final word should be said about the external validity of a siblings' comparison. Since the nature of the study requires the sub-sampling of families with two children or more under the age of ten, the external validity of these findings is limited. These families with at least two children under ten years of age are, of course, different from the general population, and as such the positive effects of the program will be difficult to generalize to all potential beneficiaries of the PNCF (Moffitt, 2003). Even so, the study will be externally valid for the sub-sample of families with children.

### 3.7 CONCLUSION

This paper finds positive effects on the height for age z-scores of children of beneficiaries of the PNCF. When not specifying a functional form, all levels of exposure to parents' land ownership led to increases in HAZ for beneficiary children. The effects were only significant, however, for children with more than zero but less than twelve quarters of exposure, or having been exposed for at least one quarter during the second, third or fourth year of life. Since no significant effects were observed for children with at least one quarter of exposure *in utero* or in the first year of life, it is possible that the pregnant and lactating mother did not increase her caloric intake despite possible increases in nutritional security after land acquisition. Instead, she ensured the increased caloric intake of the older children that ate independently.

This result has an important policy implication. A wider dissemination of information regarding the importance of maternal nutrition during pregnancy and lactation is recommended in the rural Northeastern region of Brazil. Nonetheless, for older children, the PNCF clearly leads to better nutritional security as evidenced by improved height for age z-scores. The early childhood development hypothesis predicts that this result will most likely lead to improved outcomes in later life. In this way, the intergenerational transmission of poverty will be broken and future generations of today's beneficiaries will likely find their way out of poverty. This program is predicted to have long lasting effects in rural northeastern Brazil by improving the conditions of the youngest generations that could benefit from the program.

## REFERENCES


Almond, D. (2006). “Is the 1918 Influenza Pandemic Over? Long-term Effects of In-utero Influenza Exposure in the Post-1940 U.S. Population.” *Journal of Political Economy*, 114 (August): 612-712.

Almond, Douglas and Janet Currie. (2011). “Human Capital Development Before Age 5.” *Handbook of Labor Economics*, Volume 4: 1315-1486.

Agüero, Jorge, Michael Carter and Ingrid Woolard. (2009). “The Impact of Unconditional Cash Transfers on Nutrition: The South African Child Support Grant.” *Working Paper* 39, International Policy Centre for Inclusive Growth.

Ashenfelter, Orley and Alan Krueger. (1994). "Estimates of the Economic Return to Schooling from a New Sample of Twins." *American Economic Review* 84(5): 1157–1174.

Ashenfelter, Orley and David Zimmerman. (1997). “Estimates of the Returns to Schooling from Sibling Data: Fathers, Sons and Brothers.” *Review of Economics & Statistics*, 79(1): 1-9.

Baars, B and NM Gage. (2007). *Cognition, Brain And Consciousness: An Introduction To Cognitive Neuroscience*. London: Elsevier Academic Press.

Bleakley, Hoyt. (2007). “Disease and Development: Evidence from Hookworm Eradication in the South.” *The Quarterly Journal of Economics*, 122(1): 73-117.

Bourgeois, J.P. (2001). “Synaptogenesis In The Neocortex Of The Newborn, The Ultimate Frontier For Individuation?” In *The Handbook of Developmental Cognitive Neuroscience*, eds. Charles Nelson and Monica Luciana. MIT Press, Cambridge: 23-33.

Branca, F., A. Ferro-Luzzi, S.P. Robins and M.H.N. Golden. (1992). “Bone Turnover in Malnourished Children.” *The Lancet*, 340(8834): 1493-1496.

Casey, B. J., N. Tottenham, C. Liston and S. Durston. (2005). “Imaging The Developing Brain: What Have We Learned About Cognitive Development?” *Trends in Cognitive Science*, 9(3): 104–110.

Corcoran, M. (2001). “Mobility, Persistence, and the Consequences of Child Poverty for Children: Child and Adult outcomes.” In *Understanding Poverty*, esd. S. Danziger and R. Haveman. Cambridge, MA: Harvard University Press: 127-161.

Cunha, F., J. J. Heckman, L. Lochner and D. V. Masterov. (2006). "Interpreting the Evidence on Life Cycle Skill Formation." *Handbook of the Economics of Education*, 1: 697-812.

Currie, Janet, Mark Stabile, Phongsack Manivong, and Leslie L. Roos. (2009). "Child Health and Young Adult Outcomes." *NBER Working Paper* 14482.

Duncan, Greg J., W. Jean Yeung, Jeanne Brooks-Gunn and Judith R. Smith. (1998). "How Much Does Childhood Poverty Affect the Life Chances of Children?" *American Sociological Review*, 63: 406–423.

Gertler, Paul. (2004). "Do Conditional Cash Transfers Improve Child Health? Evidence from PROGRESA's Control Randomized Experiment." *The American Economic Review*, 94(2): 336-341.

Grantham-McGregor, S.M., Y.B. Cheung, S. Cueto, P. Glewwe, L. Richter and B. Strupp. (2007). "Developmental Potential in the First 5 Years for Children in Developing Countries." *The Lancet*, 369(9555): 60–70.

Harper, C., R. Marcus and K. Moore. (2003). "Enduring Poverty and the Conditions of Childhood: Lifecourse and Intergenerational Poverty Transmissions." *World Development*, 31(3): 535–554.

Hausman, J. (2001). "Mismeasured Variables in Econometric Analysis: Problems from the Right and Problems from the Left." *Journal of Economic Perspectives*, 15(4): 57-67.

Helfand, Steven M., Rudi Rocha and Henrique E. F. Vinhais. (2009). "Pobreza E Desigualdade De Renda No Brasil Rural: Uma Análise Da Queda Recente." *Pesquisa e Planejamento Econômico*, 39(1): 67-88.

Hirano, K., and G. Imbens. (2004). "The Propensity Score with Continuous Treatments." In *Applied Bayesian Modeling and Causal Inference from Incomplete-Data Perspectives*, A. Gelman and X.L. Meng (eds.). New York: Wiley.

Huttenlocher, PR and AS Dabholkar. (1997). "Regional Differences in Synaptogenesis in Human Cerebral Cortex." *Journal of Comparative Neurology,* 387: 167-178.

Martorell, Reynaldo. (1995). "Results and Implications of the INCAP Follow-up Study." *The Journal of Nutrition,* 125: 1127S-1138S.

Moffitt, Robert. (2003). "Causal Analysis in Population Research: An Economist's Perspective." *Population and Development Review*, 29(3): 448-458.

MDA. (2005). *Programa Nacional de Crédito Fundiário Consolidação da Agricultura Familiar Manual de Operações*. Brasília: Ministério do Desenvolvimento Agrário.

MDA. (2009). *Programa Nacional de Crédito Fundiário Combate à Pobreza Rural Manual de Operações*. Brasília: Ministério do Desenvolvimento Agrário.

Nelson, Charles A. (2000). "The Neurological Basis of Early Intervention." In *Handbook of Early Childhood Intervention*, eds. Jack Shonkoff and Samuel Meisels. Cambridge: Cambridge University Press: 204-230.

Neufeld, Lynnette, Armando García-Guerra, Jef Leroy, María de Lourdes Flores López, Ana Cecilia Fernández Gaxiola and Juan Ángel Rivera-Dommarco. (2005). *Impacto del Programa Oportunidades en Nutrición y Alimentación en Zonas Urbanas De México.* Mexico, D.F.: Instituto Nacional de Salud Pública Dirección de Epidemiología de la Nutrición.

Nilsson, J Peter. (2008). "Does a Pint a Day Affect your Child's Pay? The Effect of Prenatal Alcohol Exposure on Adult Outcomes." *IFAU Institute for Labour Market Policy Evaluation Working Paper*.

Ravallion, Martin. (2008). "Evaluating Anti-Poverty Programs." *Handbook of Development Economics*, 4: 3787-3846.

Sen, Amartya. (1999). *Development as Freedom*. Oxford: Oxford University Press.

Shaw, P., D. Greenstein, J. Lerch, L. Clasen, R. Lenroot, N. Gogtay, A. Evans, J. Rapoport, and J. Giedd. (2006). "Intellectual Ability and Cortical Development in Children and Adolescents" *Nature*, 440: 676–679.

Tanner, J. M., R. H. Whitehouse and M. Takaishi. (1966). "Standards from Birth to Maturity for Height, Weight, Height Velocity, and Weight Velocity: British Children, 1965." *Archives of Disease in Childhood*, 41: 454–471.

UNICEF. (1998). *The State of the World's Children.* New York: Oxford University Press.

Victora, Cesar G., Linda Adair, Caroline Fall, Pedro C Hallal, Reynaldo Martorell, Linda Richter, Harshpal Singh Sachdev. (2008). "Undernutrition 2: Maternal and Child Undernutrition: Consequences for Adult Health and Human Capital." *The Lancet,* 371(9609): 340–357.

WHO. (1995). *Field Guide on Rapid Nutritional Assessment in Emergencies*. Alexandria: World Health Organization.

## CONCLUSION

This dissertation evaluated different periods of time and different regions of the world, but all chapters had a common theme—the human condition and how it can be improved. From the early childhood development hypothesis, and the basic science of human development, the most critical period for the improvement of the human condition is the prenatal period and the first few years of life. It is during this critical period that the human brain develops and therefore any positive or negative environmental shocks will have long lasting impacts. The first and third chapters of this dissertation evaluated changes during the critical early period. The second chapter, while not evaluating changes during the critical early period, did estimate the impact of an anti-poverty program intended to increase the wellbeing of the rural poor.

The first chapter evaluated improving the human condition by reducing the negative effects to children in early development caused by excessive parental alcohol use. Decreased parental alcohol use would directly impact prenatal environments by decreasing the instances of Fetal Alcohol Exposure that would occur with maternal drinking. Postnatally, children's conditions would be improved indirectly through environmental changes due to decreased drinking by either parent. Less drinking by parents would provide a better household environment, together with a positive income shock as household resources would no longer be used for purchasing alcohol. In addition, less alcohol caused unintended pregnancies to decrease which lead to better planned cohorts.

In order to show that the conditions of children were improved with decreased parental alcohol use, the chapter made use of the variations in state-level alcohol prohibitions between 1851 and 1920 as a natural experiment. Thirty-four states enacted state-level prohibition in different years during this period, which allowed for the estimation of a difference in differences model. The improvements in the early development environments for cohorts *in utero* and up to three years of age during state prohibitions were confirmed by increased labor force participation and total income for female cohorts in 1960. In other words, female cohorts exposed prenatally and in the first three years of life to state-level prohibition had improved adult labor market outcomes, as compared to female cohorts in wet states and female cohorts in dry states that completed their early development before the enactment of state prohibition. No effects were found for male cohorts—a result that could be explained through an evolutionary mechanism that caused pregnant females to selectively and spontaneously abort frail members of the male sex when exposed to environmental stressors. Given that pregnant women in wet states were subject to more environmental stressors than pregnant women in dry states, male cohorts from wet states would be stronger, on average, than male cohorts in dry states. In this way, effects of improved early childhood environments from state prohibitions of alcohol were found for female but not male cohorts. The conclusion of the chapter did not recommend a return to alcohol prohibition. Instead, the policy implication was that reducing parental alcohol use while a child is in early development would lead to long-term positive impacts on the child's future labor market outcomes.

The third chapter of the dissertation also had to do with evaluating improvements to the conditions of early development—but through the mechanism of increased nutritional security. As poor parents acquired land through the Brazilian National Land Credit Program, children's nutrition could be improved. In order to evaluate this hypothesis, the third chapter made use of data on the heights of siblings. By comparing siblings with differential exposure to parents' land ownership, unobserved family characteristics that were common to both siblings were netted out through the use of family fixed effects. The findings suggested that children exposed to parents' land ownership in the second, third or fourth year of life had improved height for age z-scores. These improved height for age z-scores predict improved adult outcomes emerging from positive nutritional shocks during the earliest years of development. As mentioned above, improvements in the earliest periods of life have long-term consequences in the improvement of the human condition.

Although no longer focusing on the early developmental period, the second chapter of this dissertation also evaluated a pathway through which the human condition could be improved—through a reduction in rural poverty. The Brazilian National Land Credit Program provides small farmers with loans to purchase land. Using a panel dataset from 2006 and 2010, together with a control group that was drawn from the program pipeline, the second chapter evaluated the effectiveness of this program. Using the panel dataset, both difference in differences and individual fixed effects models were estimated. The results suggested that, after four years of land ownership, beneficiaries' agricultural production and earned income were significantly improved. Nonetheless, since the program operates by providing a loan, the evaluation also took into account the value of the installments for the repayment of the debt. Once the installments were subtracted from the earned income of beneficiaries in 2010, beneficiaries of the program were statistically worse off than the pipeline non-beneficiaries of the program. They were, of course, forgoing current consumption in order to increase the household’s long-term net wealth. While the effectiveness of this program in readily improving the earned income of beneficiaries remained questionable, the effects of the program on agricultural production were highly positive and significant. Not only did the results of the second chapter display this increase in agricultural production, but also the results of the third chapter. Since children of beneficiaries that were exposed to parents’ land ownership had improved height for age z-scores, the increases in agricultural production were significant in improving the nutritional security of the families. This program can therefore be another mechanism through which the human condition can be improved.