# Easterlin Revisted: Relative Income and the Baby Boom

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This paper reexamines the first viable and a still leading explanation for mid-twentieth century baby booms: Richard Easterlin's relative income hypothesis. He suggested that when incomes are higher than material aspirations (formed in childhood), birth rates would rise. This paper uses microeconomic data to formulate a measure of an individual's relative income. The use of microeconomic data allows the researcher to control for both state fixed effects and cohort fixed effects, both have been absent in previous examinations of Easterlin's hypothesis. The results of the empirical analysis are consistent with Easterlin's assertion that relative income influenced fertility decisions, although the effect operates only through childhood income. When the estimated effects are contextualized, they explain 12 percent of the U.S. baby boom.

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# 1 Introduction

In the middle of the twentieth century, the United States and other developed countries experienced a prolonged period of elevated birth rates. The first viable and a still leading explanation for these fertility increases came from Richard A. Easterlin in 1966. Easterlin hypothesized that the key factor driving these baby booms was the relative income of individuals of childbearing age. He suggested that when incomes are higher than material aspirations (formed in childhood), birth rates would rise. The cohort responsible for the baby boom would have had high relative income given that they entered adulthood in the prosperous post-war period and yet their material aspirations would be low because their childhood spanned the Great Depression. In this paper, I will reassess Easterlin's hypothesis with state income data that allows me to utilize a large sample of individuals from the U.S. census.

Easterlin's hypothesis proposes that individuals will develop material aspirations based on their childhood experience. If their adult income surpasses their material aspirations, they will feel richer and thus have more children. In Easterlin's model, one's income relative to one's aspirations is more important to the fertility decision than one's absolute income. Therefore, cohorts raised during hardship and entering adulthood in a period of prosperity would have higher birth rates than those who grew up in an affluent period but whose adulthood occurred during economically depressed years. His initial papers on the topic provided some empirical evidence for the theory by quantifying relative income as the income of households with a head aged 14-24 as a percent of income of households with a head aged 45-54 five years prior. In the aggregate time series Easterlin's relative income measure is highly correlated with the total fertility rate from 1950 to 1980.[1] Prior to World War II, detailed household income data was not available, and Easterlin used male employment as a proxy for

[1] An extension of Easterlin's hypothesis is that relative cohort size should also impact fertility because relative cohort size is related to relative income. Small cohorts face less labor force competition and thus will earn higher wages at younger ages. Several papers have examined the relationship between cohort size and fertility (see Waldorf and Byun 2005 for a review of these studies). In this paper I focus on only the relative income aspect of Easterlin's hypothesis.

relative income. This employment-based measure is highly correlated with the total fertility rate (TFR), as well. The positive relationship between relative income and fertility in the U.S. aggregate time series forms the bedrock of support for Easterlin's hypothesis. However, this aggregate relationship does not necessarily prove Easterlin's hypothesis because a third latent variable (such as marriage rates or educational attainment) could be driving both phenomena.

Easterlin's work has been the impetus for hundreds of studies, as researchers have tried to evaluate the validity of the hypothesis in a variety of contexts. Macunovich (1998) conducted a comprehensive review of seventy-six published papers and ultimately affirmed the hypothesis. According to Macunovich, about two-thirds of the surveyed research supports Easterlin's hypothesis. She surmises that methodological differences drive the results of the dissenting one third. She stresses that Easterlin must be taken on his own terms, a true test of his hypothesis should use age-specific, objective measures in the construction of his relative income variable. Furthermore, she emphasizes that relative income should be used alone and not in conjunction with absolute income due to collinearity issues. In this paper, my relative income variable will follow Macunovich's prescriptions and the cohorts in my sample will be those responsible for the baby boom and bust. In short, I will use measures of relative income as conceptualized by Easterlin and I will test its effect on the cohorts whose fertility behavior was the impetus for the Easterlin hypothesis.

This paper adds to the existing literature by testing Easterlin's hypothesis with a large individual data set and uses variation in annual state incomes during childhood to test the impact of relative income on fertility outcomes. My data and empirical strategy allow me to address criticisms of other papers that test the Easterlin hypothesis. I construct two main measures of relative income, the first is designed to proxy for the relative income measure used by Easterlin and the second is grounded in the psychology literature. I then test whether relative income at the state level impacts the completed fertility of women from the 1970, 1980 and 1990 censuses. Most importantly, I include birth-year fixed effects to control for unmeasured nationwide factors that would have influenced all people born in the same year. These birth cohort fixed effects significantly reduce the explanatory power of the relative income measure similar to Easterlin's. However, the relative income measure based in the psychology literature (my preferred measure) is robust to the inclusion of birth state and birth year fixed effects. The preferred relative income measure can explain 12 percent of the baby boom. Critics of the Easterlin hypothesis point to the fact that fertility has not cycled since the baby boom. I show that the primary influence of relative income in the baby boom was through the channel of low childhood incomes generated by the Great Depression. I also show that the effect of relative income fades for later birth cohorts. Given that there has not been income variation to the extent of which was engendered by the Great Depression, it is feasible that relative income (albeit childhood income is the important component) could have been a driving factor in the baby boom but also not a salient factor in fertility decisions for later cohorts.

This paper is most similar to two other studies: Lindert (1978) and Maxwell (1991). Lindert also used state level income and estimated the effect of income twenty years prior divided by current state income on number of children per woman. Lindert's results were favorable to the Easterlin hypothesis. I also find support for the Easterlin hypothesis, but I improve upon Lindert's methods by using annual state income while Lindert relied on state level income at decade intervals. Further, I am able to contextualize the estimates within the baby boom. Similar to my approach, Maxwell used individual level data from a national survey (the National Longitudinal Survey) and examined the effect of relative wages on fertility. She also found support for the Easterlin hypothesis. Maxwell used variation between cohorts while I estimate the effect of relative income based on variation both between and within a birth cohort. Further, Maxwell is concerned with only age at first birth while I estimate the effect on a range of fertility outcomes.

In recent years, several new theories about the baby boom have emerged. Doepke, Hazan, and Moaz (2008) suggest that increased female labor force competition from older women who had entered the labor force during WWII decreased the age of marriage (thereby increasing birth rates) for women who entered adulthood after WWII. In a 2010 paper, Albanesi and Olivetti emphasize the role that falling maternal mortality can have in short term birth rate increases. In addition, Hill (2014) estimates that ten percent of the baby boom can be attributed to the lower cost of housing in the period. With the proliferation of new explanations for the baby boom, it is useful to revisit Easterlin's original ideas and assess their plausibility.

## 2 Relative Income in a Theoretical Framework

In this section, I present a simple model that will be instructive in understanding how relative income might affect fertility decisions. The model is similar to Becker's model (1981) on the demand for children, except I abstract from the demand for child quality and strictly examine the demand for quantity. Agents tradeoff consumption ($c$) and children ($n$), with preferences as follows:

$$u(c, n) = \alpha_i \ln c + \beta_i \ln(1 + n)$$

Where $\alpha_i$ and $\beta_i$ are taste parameters for consumption and children, respectively, and vary by individual ($i$). The budget constraint (where $p_c$ is the price of consumption and $m$ is income) is given by

$$p_c c + n \leq m$$

The optimal amount of children is then

$$n = \frac{\beta_i m - \alpha_i}{\alpha_i + \beta_i}$$

Easterlin argued that individuals who are raised in economic downturns will have a lower taste for material goods, here represented by $\alpha_i$. Relative income ($r$) can be then conceptualized as a proxy for the ratio of current income $m$ to $\alpha_i$. The optimal amount of children in terms of relative income is then:

$$n = \frac{\beta_i r - 1}{1 + \frac{\beta_i}{\alpha_i}}$$

The number of children demanded ($n$) is increasing in relative income $r$; this is the Easterlin hypothesis. However, it is useful to observe that relative income will have no effect on children demanded if an individual has no taste for children ($\beta_i = 0$), these individuals will set $n = 0$. Empirically, we will only observe an effect of relative income for those at a corner solution if changes in $r$ and/or $\beta$ are sufficiently large enough to induce an interior solution, otherwise the individual will remain at the corner. The fact that $n$ is a discrete variable potentially attenuates the effects for those at corner solution as well, because an optimal choice of a fraction of a child is not feasible.

## 3 Data

My individual level data come from the 1970, 1980, and 1990 censuses. These were the census years where the variable of interest, number of children ever born, was available. The census provides a large sample of the birth cohorts of interest and contains other individual level control variables, including various location variables, which allow me to place women over time. I restricted the sample to women age 40 or over, in order to capture individuals who had either passed their child-bearing years or were close to the end of their child bearing years.

Table 1 shows the summary statistics for the women in my sample culled from the 1970, 1980, and 1990 censuses. The majority of the observations come from the 1980 and 1990 census; these were 5 percent samples whereas the 1970 census was a 1 percent sample. The average age of women in the sample is 51.3 and the women's first marriage occurred at 21.4, and only 5.6 percent of the sample never married. There is significant variation in the number of children ever born: the mode is 2, but over 14 percent of women report never having a child and over ten percent report having more than 5 children.

I merge my sample of women with state income data. The information on state income comes from the Bureau of Economic Analysis for the years 1938 to 1990. Data from 1919 to 1938 are from Martin and Nathan (1939) adjusted by Fishback (2008).[2] To account for inflation, I further adjusted the income numbers by the CPI.[3] Figure 1, a plot of the highest income state, the median income state, and lowest income state, displays some of the variation in the data. States exhibited similar trends; incomes fell in the Great Depression and then underwent a dramatic increase in the post-war boom. However, there were differences in the severity of the income swings. The Depression hit the Mountain West region hardest and was milder in the South (Rosenbloom and Sundstrom 1999). Variation between states has generally fallen over time, reaching its apex in 1934 with a standard deviation of 0.38 between states, and dropping to 0.15 in 1987.

A major challenge in combining the census data with the state data is ascertaining a woman's state of location over her lifetime. I limit the sample to women who can be reliably located for their childhood and childbearing years. The census has information on a woman's state of birth, her spouse's state of birth (if the spouse is present in the household), each child's state of birth (if the child is present in the household), her own state of residence five years prior, and her current state of residence. I assumed a woman's state of birth was the state where she spent the bulk of her childhood, and used this state as the childhood state. In assigning an adult state, I chose the mode of her children's birthplace because this would have been the state where she made the majority of her fertility decisions. If no children birthplace information was present, I used state of residence five years prior as the adult state. I classified individuals into four groups. Group 1 contains individuals whose childhood and adult state I am confident that I have correctly chosen based on the available state information. Group 2 contains individuals whose childhood state alone I am confident

[2]The Martin data covers 1919 to 1938, while the BEA data begins in 1929. Fishback regressed the BEA data on the Martin data for the years 1929-1938 and then used the estimates to obtain predicted GDP based on the Martin data for the years 1919 to 1938.

[3]Ideally, a state price index should be used to adjust the state income data. Information on state price levels are only available at 10 or twenty year intervals for the period in question. However, Mitchener and McLean (1999) show that state price levels from 1920 to 1960 (the birth years of my sample) have the lowest standard deviation of any period in US history.

I have correctly chosen. Group 3 contains individuals whose adult state alone I am confident that I have correctly chosen. Group 4 contains individuals about whose childhood and adult state I am unsure. I exclude Group 4 from the sample given the difficulties in determining the state and therefore the proper childhood and adult economic experience. Group 4 willl also be comprised of individuals who have moved between states several times and thus a given state's income will be a poor proxy for their economic experience. Table 2 provides the specific details on the process through which I derived these distinctions and the percent of women in each category. For the remainder of the paper I will refer to these groups as migration group 1, 2, 3, and 4.

With any retrospective assignment of individuals to locations there will be some migration bias introduced. In my sample, the childhood state is proxied by the state of birth. Individuals whose families move while they are very young will be misclassified. If the migration is random, any estimates on the effect of relative income will be attenuated. If families move from poor to rich states, then any estimated effect of relative income will be downwardly biased because individuals will be misclassified with higher relative income. For the adult income state, I use the mode of a woman's children's birthplace. For women where this information is available, the adult state will be reasonably accurate because the state where she made her child-bearing decisions was known. For adults where this information is not available (i.e. the woman has no children in the household), I rely on current state or state 5 years prior. In the case of adult income, migration from a low income state to a high income state will also downwardly bias my coefficient estimates, because I will again assign a higher relative income than their true relative income. Moves from low to high income states generate the same downward bias in both the adult and child income cases because in the child case, I observe the individual before the move, while for the adult case, I observe them after the move. Given that migration is more likely to occur from low to high income per capita states (Davies, Greenwood, Li (2001), Fishback, Horrace, Kantor (2006)), any migration bias introduced by misclassification will downwardly bias the estimated effect of relative income.

# 4 Measuring Income Experience

## 4.1 Childhood Income

Central to Easterlin's hypothesis is the role of income within the household during one's childhood. He argues that individuals form material aspirations based on their childhood experience and that these aspirations will influence later fertility decisions. The hypothesis was somewhat controversial at the time because it emphasized shifting preferences. However, in the context of economic development, it is not unrealistic to believe that people in different eras will have very different aspirations. A person in 1900 might aspire to have indoor plumbing, while a person in 2000 would take indoor plumbing as a given. The psychology literature suggests that material attachment can indeed be formed early in one's childhood experience.[4] Furthermore, the long run impact of childhood experience on other factors has empirical support. Using Dutch data from 1815-2000, Van den Berg et al. (2009) shows that being born during an economic downturn increases the mortality rate later in life. Similarly, Fishback and Thomasson (2014) find that the Great Depression in the U.S. had long run health consequences. And Malmendier and Nagel's (2011) work suggests that individuals adjust their aversion to risk after experiencing stock market shocks.

How should we measure one's childhood income experience? In this paper, I will use income per capita in state of birth during an individual's childhood years. State level income will be a proxy for the average income experience of individuals who grew up in a given state. I use state level income because of data restrictions, it is the lowest level of annual data available for the birth cohorts in question.[5] While the state level data might be a poor proxy for the income experience of some members of my sample, it will not suffer from the

[4]See Holbrook and Schindler (1989) for entertainment tastes and Robinson et al for work on children and brand attachment.

[5]Some data exists at the city level for the 1930s, however, it is difficult to ascertain in which city women lived during the 1930s from the location data in the 1970, 1980 and 1990 censuses.

endogeneity issues associated with individual level measures like family income or parent's education. For instance, there is substantial evidence that children are not a normal good (Jones and Tertilt 2006); thus, poorer families may have more children. Furthermore, fertility preferences are partially derived from one's parents (Murphy and Knudsen 2002). For this reason, one might observe a mechanical negative relationship between childhood household income and later life fertility and then incorrectly ascribe that relationship to relative income. Biases of this sort will not be present in the state income. Further, state income will also include any peer effects that might influence material aspirations. In general, state income can be thought of as an instrument for an individual's household conditions.

What age do one's material aspirations form? While there is nothing definite from the psychology literature about the age at which material aspirations form, there has been research on other types of aspirations. The psychology literature suggests that preferences for certain brands can develop at young ages, career aspirations form around age 10 to 11, and tastes for popular entertainment form in high school.[6] For this paper I demarcated the ages when material aspirations emerged as age 6 to age 12. I began with age six because this is the age that psychologists claim children usually become concrete-operational (Bahn 1986), meaning they can grasp more than one concept at a time and begin to use logic. Psychologists agree that reaching this stage is a key point in cognitive development. For these reasons, I used age 6 as a plausible starting point for the formation of material aspirations. I ended with age 12 because I judged material aspirations to be most similar to career aspirations and the literature suggests those aspirations are largely formed by age 12. To form a measure of relative income, I took state income during one's early adulthood (age 19-23) divided by state income during the time of aspiration formation (age 6-12). The log of this ratio is hereafter referred to as RI1 and is my preferred relative income measure.

I also construct another measure, called relative income 2 (hereafter RI2) that uses the 0-4 age range for childhood income instead of 6-12. RI2 will have a twenty year gap between the

[6] See Hartung, Porfeil, Vondracek (2005) and Trice (1991) and Cook, Church, Ajanaku, Shadish, Kim, and Cohen (2008) on career aspirations.

adult and child measure and thus will be the closest relative income measure to that which Easterlin used throughout his work. In the bulk of his papers on the subject, Easterlin used income of household heads age 45-54 five years prior as the childhood experience of household heads age 14-24. He argued that individuals age 45-54 would be the parents of people age 14-24 and that five years prior would cover the time when individuals age 14-24 were still in the parental home. The principle difference between the state income data and Easterlin's national data is that the state income data are not age specific while Easterlin's data are broken down by age. Therefore, there is no perfect analog for Easterlin's measure in the state data, but RI2 has a similar year gap and, like Easterlin's measures, is closely related to fertility in the time series.

### 4.2 Adult Income

The second component of relative income is adult income. In the construction of RI1 and RI2, I have used income at the state level during early adulthood (19-23). The age range is chosen principally to match Easterlin's age range of 14-24 year-old household heads. For two reasons, I also assert that this age range is the appropriate bracket to proxy the adult income environment under which fertility decisions are made. Firstly, the average age of first birth is 21.9 to 22.2 for women in the birth year cohorts covered by my sample.[7] The average age at first birth has been shown to be the most important factor for completed fertility; thus, the age range 19-23 includes the period during which the crucial component of completed fertility is determined (Bumpass et al. 1978). Secondly, recent work by Oreopoulos, von Wachter and Heisz (2012) suggests that conditions upon labor-force entry have long term effects. Consequently, the economic environment at age 19-23 will be a proxy for the earnings of individuals for the remainder of the child-bearing years.[8]

While timing of first birth is the most crucial component of completed fertility, higher

[7] source: Vital Statistics of the United States, 2003.

[8] Or we could assume that individuals have limited foresight. In either case, current conditions will be a predictor for both the actual and perceived economic conditions for the duration of one's adulthood.

relative income at later ages could potentially influence fertility as well. Couples may plan for $x$ number of children, but a high relative income realization towards the end of their childbearing years may convince couples to have $x + 1$ children. I therefore construct a fourth and final measure of relative income (RI3) that uses income at ages 26-30 as the adult income measure instead of the ages 19-23. If relative income impacts fertility such that it causes couples to add children after a certain point rather than inducing women to start their childbearing years sooner, then RI3 will be a more salient measure of the effect of relative income than RI1 or RI2.

In Easterlin's papers, the relative income measure was highly correlated with total fertility rate in the aggregate time series. Figure 2 shows how the aforementioned relative income measures relate to fertility from 1919 to 1955. The plot shows average number of children, RI1, RI2, and RI3 by birth year. Of the three measures, RI2 most closely matches birth increases in the aggregate time series and therefore is perhaps the best analog for the Easterlin relative income measure in practice. However, based on the psychology literature, RI1 and RI3 remain the best analogs for relative income in terms of theoretical support.

# 5 The Effect of Relative Income on Fertility Outcomes

## 5.1 Completed Fertility, Intensive and Extensive Margin

In this section I examine the effects of relative income on completed fertility, the intensive fertility margin and extensive fertility margin. I begin with the effect of relative income on a woman's completed fertility. The measure of completed fertility used is children ever born reported by women in the 1970, 1980, and 1990 U.S. censuses. Given that children ever born is a non-negative count variable, I follow the standard practice in the literature and estimate a Poisson regression (Cameron and Trivedi 1998). Table 3 shows the marginal effects of the variable of interest (relative income) on children ever born.[9] I report the results for the

[9]other controls included are adult income state fixed effects, urban dummy, race, education, age, age-squared, still married dummy, and census year fixed effects. By including education, I am assuming the education decision occurs before the fertility

four different versions of relative income. Recall that relative income 1 (RI1) is log (average state income per capita at ages 19-23/ average state income at ages 6-12). Relative income 2 (RI2) is log (average state income per capita at ages 19-23/ average state income at ages 0-4). Relative income 3 (RI3) is log (average state income per capita at ages 26-30/ average state income at ages 6-12). Panel A shows the results for migration group 1, and Panel B shows the results for migration group 1, 2, and 3. The results are largely supportive of the Easterlin hypothesis, relative income is positively associated with completed fertility in all of the specifications except for two and the correlation is statistically significant.[10]

Panel A, row 1 shows the results with RI1 as the relative income measure and the sample is migration group 1. In column (1) when birth year fixed effects are not included, RI1 has a negative effect on children ever born. This result is not surprising given the lack of correlation between RI1 and children ever born in the aggregate time series. Once I control for birth year fixed effects in column (2), RI1 has the expected positive and significant effect on completed fertility proposed by Easterlin. A 25 percent increase in relative income (which is approximately a standard deviation increase) raises the number of children ever born by 0.12, a 4.5 percent increase in completed fertility. The effect of RI1 remains significant when birth state fixed effects are included in column (3).

The marginal effects of RI2 on children ever born are presented in row 2. In column (1), the estimated marginal effect suggests a 25 percent increase in RI2 will increase the number of children ever born by 7.2 percent. The effect is decreased significantly with the inclusion of birth year fixed effects and becomes statistically indistinguishable from zero when birth state fixed effects are included. RI2 was the relative income measure most correlated with children ever born in the aggregate time series; therefore, it is not unexpected that its effect on fertility is reduced with the inclusion of cohort fixed effects. The extent of the reduction

decision. However, I acknowledge that education may be endogenous to fertility if early child-bearing causes women to exit schooling. The results are robust to excluding education from the regressions. The results are also robust to limiting the sample to women who are married after the age of 19, i.e. women who would have plausibly made the decision to continue their education before they made any fertility decisions.

[10] I follow the recommendations of Betrand, Duflo, and Mullainathan (2004) and cluster standard errors at the birth state level.

suggests that RI2 is primarily a proxy for a cohort's fertility experience. However, taking the Easterlin hypothesis on its own terms, it is debatable whether the appropriate effect is the estimate with or without cohort and birth state fixed effects. If relative income is the defining aspect of a particular cohort or state, then including fixed effects in the specification will remove much of the identifying variation in relative income. Conversely, by excluding cohort fixed effects, all other explanatory variation between cohorts will be included in the estimated effect of relative income. Other potential cohort effects, such as peer influences and exposure to World War II, would be subsumed in the relative income effect. Further, by excluding birth state fixed effects, other time invariant effects on fertility of particular birth states or regions are included in the estimated effect of relative income. These state of birth effects may include the general health environment or educational infrastructure that may influence the preference for quantity of children. (Aaronson, Lange, and Mazumder 2011, Bhalotra, et al. 2012) For the purposes of this paper, the specification using RI1 with cohort and birth state fixed effects is my preferred specification, while the specification absent cohort fixed effects with the relative income measure (RI2) can be viewed as the method most closely approximating the approach used in Easterlin's research.

Row 3 shows the results with RI3 as the relative income variable. RI3 uses an adult income measure from later in an individual's life cycle (26-30). The coefficient estimates with RI3 are slightly larger than those with RI1, although the differences are not statistically different. These results suggest that relative income affects women both at the beginning and middle of their child bearing years.

In column (4), I examine the effects of relative income on the intensive margin. I limit the sample to women who have had at least one child. The effects for this group are stronger than those for the whole sample. The marginal effects are 20 to 40 percent larger than those reported in column (3). This suggests that relative income impacts completed fertility through the intensive margin and has zero or a negative effect on the extensive margin. A 25 percent increase in RI1 increases children ever born by 0.11 and a 25 percent increase in

RI3 will add an additional 0.14 children to women on average. Similar to column (3), no effect is found for RI2.

Panel B replicates the estimations from panel A with migration group 2 and 3 in addition to migration group 1. The estimated effects of interest remain statistically significant although the point estimates are slightly smaller. As this sample contains more individuals who are misallocated in either their birth or adult state, it is likely that the observed attenuation of the estimates is due to migration bias.

While relative income is positively related to both overall fertility and fertility conditional on having at least one child, a positive relationship is not found on the extensive fertility margin. Table 4 shows the estimated marginal effects from a probit model where the dependent variable is equal to 1 if the individual had at least one child. The results are either statistically insignificant or small in magnitude. My preferred relative income measure, RI1, is negatively correlated with the extensive fertility margin when birth state and year fixed effects are included (column 3). A twenty five percent increase in relative income lowers the probability of having at least one child by less than 0.7 percentage points, which is less than a one percent effect on the overall probability. In all the specifications, a standard deviation increase in relative income never has more than a one percent effect in the probability of having at least one child.

The estimates suggest that relative income has no meaningful effect on the extensive fertility margin, but a significant positive effect on the intensive margin. Several recent studies (Aaronson, Lange, and Mazumder 2011, Bhalotra, et al. 2012) have found differential effects on the intensive and extensive margins. In these studies, the difference arises from child quality price changes. A decrease in the price of child quality will negatively impact the intensive margin because of the substitution effect between child quantity and child quality of children. However, positive effects of a quality price decrease may be found on the extensive margin because this substitution effect will not exist at a corner solution. The case of relative income is different to that of a quality price decrease. In the Easterlin model, a relative income increase lowers the preference weight for consumption, and thus increases the relative weight of other goods in the utility function, one of which may be children. Therefore, relative income should increase fertility along both the extensive and intensive margins, and yet no (or a negative) effect is found on the extensive margin. It could be the case that childless women have a preference for no children, and thus may have a low weight for children in their utility function.[11] Childless women may also be childless because they or their partner are incapable of having children. In either case, a lowered preference for consumption goods will not necessarily lead to an increase in child quantity and this would explain the lack of an effect. The baby boom was not only a product of increased births for women; it was also the result of a reduction in childlessness (Bailey, et al 2013). My results suggest relative income only affects women who had already decided to have children and therefore relative income can only partially explain the baby boom.

Figure 3 illustrates the relationship between relative income and fertility in more detail. The figure shows the results of a series of regressions where the dependent variable equals 1 if the woman had more than X children conditional on having at least X children. The variable of interest is relative income, panel A shows the results when RI1 is used, and panel B shows the results for RI3. The x axis plots the number of children. The y axis is the percentage effect a 30 percent increase of relative income on the probability that a woman had more than X children, conditional on having at least X children. For instance, in Panel A the point associated with the x value ">3" means that a 30 percent increase in relative income increased the probability a woman had more than 3 children (conditional on having at least 3 children) by 4 percent. In other words, women with 3 children were 4 percent more likely to have more than 3 children if they lived in a state with 30 percent higher relative income. The results suggest relative income has little effect on the decision to move from 0 to 1 children or from 1 to 2 children. The effects of relative income are concentrated on the higher order births, the decision to have more than 2, more than 3, and more than 4

[11] A low utility weight for children may be due to a lack of preference for children, or due to a high cost of children-bearing, either because of foregone earnings or fear of maternal mortality.

children. After 5 children, the sample size is small and the estimates imprecise. The results are instructive in that they illustrate the channel through which relative income influences completed fertility: relative income increases the number of births only for women who had planned to have at least 2 children. In panel B, the results are slightly stronger when RI3 is the variable of interest instead of RI1. RI3 is constructed using an adult measure of income from the ages 26 to 30, while RI1 uses the ages 19-23 as the adult income. Because the ages 26-30 are the ages at which most women in the time period will decide to have a third or fourth child, it is not surprising that RI3 is more closely related to higher order birth decisions.

### 5.2 Marriage Outcomes

In this section I report the effect of relative income on other marriage outcomes. Changes in marital behavior were an important factor in the baby boom. Age of first marriage fell by 2 years and marriage rates increased by 25 percent from 1930 to 1950. Hill (2014) shows that these shifts in marriage patterns alone can account for 30 percent of the difference in crude birth rates from 1930 to 1950. For the baby boom birth cohorts, marriage is the first step towards child-bearing. By examining the influence of relative income on marriage, we can understand if part of the overall relative income effect is due to its impact on marriage. Table 5 shows the results from a probit regression where the dependent variable equals 1 if the individual never married. Relative income is negatively associated with never marriage. When fixed effects are included, a 25 percent increase in relative income decreases the probability of a woman never marrying by 0.6 - 0.8 percentage points (a 10 to 14 percent effect), depending on the relative income measure used.

In panel B, I examine the effect of relative income on another important marriage outcome: age at first marriage.[12] In theory, relative income might impact age at first marriage because a woman with high relative income will feel richer and more prepared for marriage than a

[12]The sample size is reduced because age at first marriage is only available for the 1970 and 1980 censuses.

woman with low relative income who might wish to work for a few extra years in order to build up savings before marriage. The results show that relative income is negatively associated with age of marriage. The estimate in column (3) with migration group 1 suggests that a 25 percent increase in relative income decreases the age at first marriage by 2.2 months. Conditional on covariates, a decrease in age of first marriage by 1 year is associated with 0.08 more children on average. Therefore a decrease in age of first marriage by 2.2 months would increase a woman's completed fertility by 0.015 children, which is 12 percent of the overall effect of relative income on completed fertility.

### 5.3 Decomposing the Relative Income effect

In this section I try to understand who was affected by relative income and which component of relative income matters. Table 6 shows the effect of relative income on the intensive margin by education groups (less than high school, high school, and more than high school) for my preferred relative measure, RI1. The effect of relative income is decreasing in education. I observe the strongest effects for lowest education group and no effect for the highest education group. A plausible explanation for these differential effects is women with less education come from families more likely to be impacted by swings in the aggregate economy. Individuals with more education are more likely to come from wealthy families and these families would have been better suited to weather the economic turbulence of the depression. Thus, childhood state income will be a poor proxy for the variation in their income experience during childhood. Poorer families, on the other hand, are more likely to suffer job loss during downturns and job recovery during economic upswings, therefore the aggregate economy will be a better proxy for their economic experience.

An aspect (and a prediction Easterlin himself made) of Easterlin's hypothesis is fertility patterns should cycle. Large birth cohorts will have low relative income and beget small birth cohorts who will then have high relative income and then beget another large birth cohort, and so on. As Easterlin's critics have pointed out, this cycling of fertility has not happened. Fertility rates have not increased since the baby boom, rather there has been a steady decrease in childbearing since 1964. In rows 4 and 5 of table 6 I examine the effects of relative income by birth cohort. I find effects for women born before 1940, but not after. There are several explanations why we might observe no relative income effect for later birth cohorts. Fertility patterns changed dramatically after 1960 (Bailey et al. 2013). The birth control pill became widely available, resulting in a convergence in the distribution of childbearing around the mode of 2. Married women also entered the labor force in larger numbers, increasing the opportunity cost of children. Increasing returns to education led to a shift away from quantity and towards quality in the demand for children. All of these trends would have diminished the influence of relative income on fertility, and contributed to the insignificant effect found for post 1940 birth cohorts.

Which components of relative income matter for fertility? In table 7 I show the result of poisson regressions on the intensive fertility margin, when childhood and adult state income are entered separately, rather than as a ratio. Contrary to Easterlin's hypothesis, high adult income (as proxied by state income) is negatively associated with children ever born, while childhood income has the predicted effect. When childhood income and adult income are included in the regression, childhood income is significantly more influential in the completed fertility than adult income, which is incorrectly signed. It should be noted that Easterlin objected to using adult and child income in the same regression due to collinearity issues, however, adult income is negatively related to fertility even in the regression without childhood income. The estimates on the effect of childhood income are comparable to the estimates found in table 3, which suggests that the primary driver of the previous results was childhood income and not relative income. This finding complicates the relationship between fertility and relative income, suggesting the observed effect is due to the high correlation between relative income and childhood income.

### 5.4 Robustness

The goal of this paper is to revisit the Easterlin hypothesis and ascertain whether the hypothesis holds with detailed individual-level data that was unavailable to Easterlin and other researchers at the time of the hypothesis's formulation and examination. The results have shown that the hypothesis has empirical support. Relative income is positively related to fertility outcomes, however the effect operates primarily through childhood income. In this section, I explore potential omitted variable biases in order to ascertain the robustness of the correlation between relative income and fertility.

Relative income could be a proxy for another phenomenon at the state level that directly influences fertility. To the extent that such phenomena are time invariant or affect the nation equally, birth state and birth year cohort fixed effects will control for them. However, relative income may be correlated with state or region trends that vary over time, such as compulsory schooling laws or education expenditures that will affect the demand for quantity of children. In table 8, I attempt to account for these trends. In column (1) and (2), I add a regional time trend to the estimation. Relative income still has a positive and significant effect on completed fertility. The results are attenuated which suggest that relative income is a proxy for other regional trends that impact fertility. In a recent paper, Stephens and Yang (2014) show that across a number of outcomes, estimates of causal effects become insignificant when birth year fixed effects are allowed to vary by region. In column (3) and (4), I allow the birth year fixed effects to vary by region. The estimated effect of relative income does not change significantly. Overall, the results suggest omitted variables may bias the estimated relative income effect, but there still exists a strong positive relationship between relative income and fertility.

The evidence presented in this paper shows that relative income is highly correlated with completed fertility. The relationship is driven by low education individuals who had a low childhood income experience and were born before 1940. I find limited evidence that relative income impacts fertility after 1940, most likely due to the dramatic changes in female labor force participation and birth control technology. The bulk of the relative income effect is due to the massive swings in income generated by the Great Depression and recovery. The precise mechanism through which relative income is related to fertility is an open question. The relationship could indeed be driven by low childhood income resulting in a lowered taste for material goods, as Easterlin hypothesized. However, I cannot rule out other channels such as relative income causing a lower valuation for quality with respect to quantity in children, or low childhood income increasing the perceived value of children as elder care insurance.

## 6 Relative Income and the Baby Boom

In this section, I quantify the potential impact of relative income on the baby boom. For this exercise, I must assume the estimated effects of relative income on fertility are causal.[13] I compare the 1929-1930 birth cohort, a cohort that experienced the baby boom, to the birth cohort 10 years prior: 1919-1920, a cohort that had largely completed fertility at the onset of the baby boom. On average, in my sample, completed fertility rose by 0.50 children between the two cohorts, from 2.60 children to 3.10 children. Relative income 1 rose from an average of 0.34 for the 1919-1920 birth cohort to 0.50 for the 1929-1930 birth cohort. If we use the preferred estimate for the effect of relative income on completed fertility from table 3, row 1, column 3, an increase of 0.16 in relative income corresponds to a completed fertility increase of .06. Thus, increase in relative income explains 12 percent of the difference in completed fertility between the two cohorts. If I use the specification most closely approximating Easterlin's (RI2 without birth year or birth state fixed effects), I find a much larger effect of relative income. RI2 increases from 0.39 in 1919-1920 to 0.77 in 1929-1930. The estimated coefficient from Table 3, row 2, column 1 suggests an increase in RI2 of this magnitude would

[13] Estimating causality of the relationship would require random variation in childhood income conditional on controls. Finding such variation or proving its existence in the Great Depression is outside the scope of this paper.

add 0.29 extra children per woman, 58 percent of the difference in completed fertility between the two cohorts. This large effect would lead one to conclude that relative income is a primary driver of the baby boom. However, recall that the estimated effect of RI2 is decreased by 70 percent when birth year fixed effects are added to the specification. Therefore, the large effect of RI2 on fertility is primarily due to the correlation between the two variables over time. For this reason, the 12 percent estimated effect of relative income on the baby boom is a more realistic estimate of relative income's impact.

## 7 Conclusion

In this paper I reinvestigated the Easterlin hypothesis using census data and state income data. My estimates of the effect of relative income on completed fertility support Easterlin's theory, but call into question the specific channel of the effect. Relative income has a positive impact on a variety of fertility outcomes: completed fertility, age of marriage, and the decisions to have more than 2, 3, and 4 children. The effect of relative income operates through variation in childhood income and is most salient for low education groups and for birth cohorts prior to 1940.

An important advantage of the data sources I used in this paper are the ability to account for cohort and state fixed effects. I tested a number of different methods to construct relative income. I found that the relative income measure guided by the psychology literature is the most robust to inclusion of time and place fixed effects. My relative income measure that most closely matches Easterlin's relative income proxy performs well in the time series but is not robust to time and place fixed effects. This finding suggests that much of the positive effect observed in Easterlin and subsequent authors' works can be explained by other trends specific to birth cohorts. The estimates do not imply that relative income was the primary driver of the mid-twentieth century baby boom.

In the rush to find new and novel explanations we often discard old theories instead of re-evaluating them with modern methods. This paper demonstrates the value of revisiting one such old theory. I have found that there exists empirical support for one of the oldest theories about the baby boom. My estimates show that the Great Depression had long run fertility implications and changes in relative income can explain 12 percent of the post-World War II baby boom. I also find that childhood income is the primary driver of those results and future research into the Easterlin hypothesis should examine the long run fertility effects of childhood economic conditions.

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Figure 1: Log state income per capita (1919-1987)

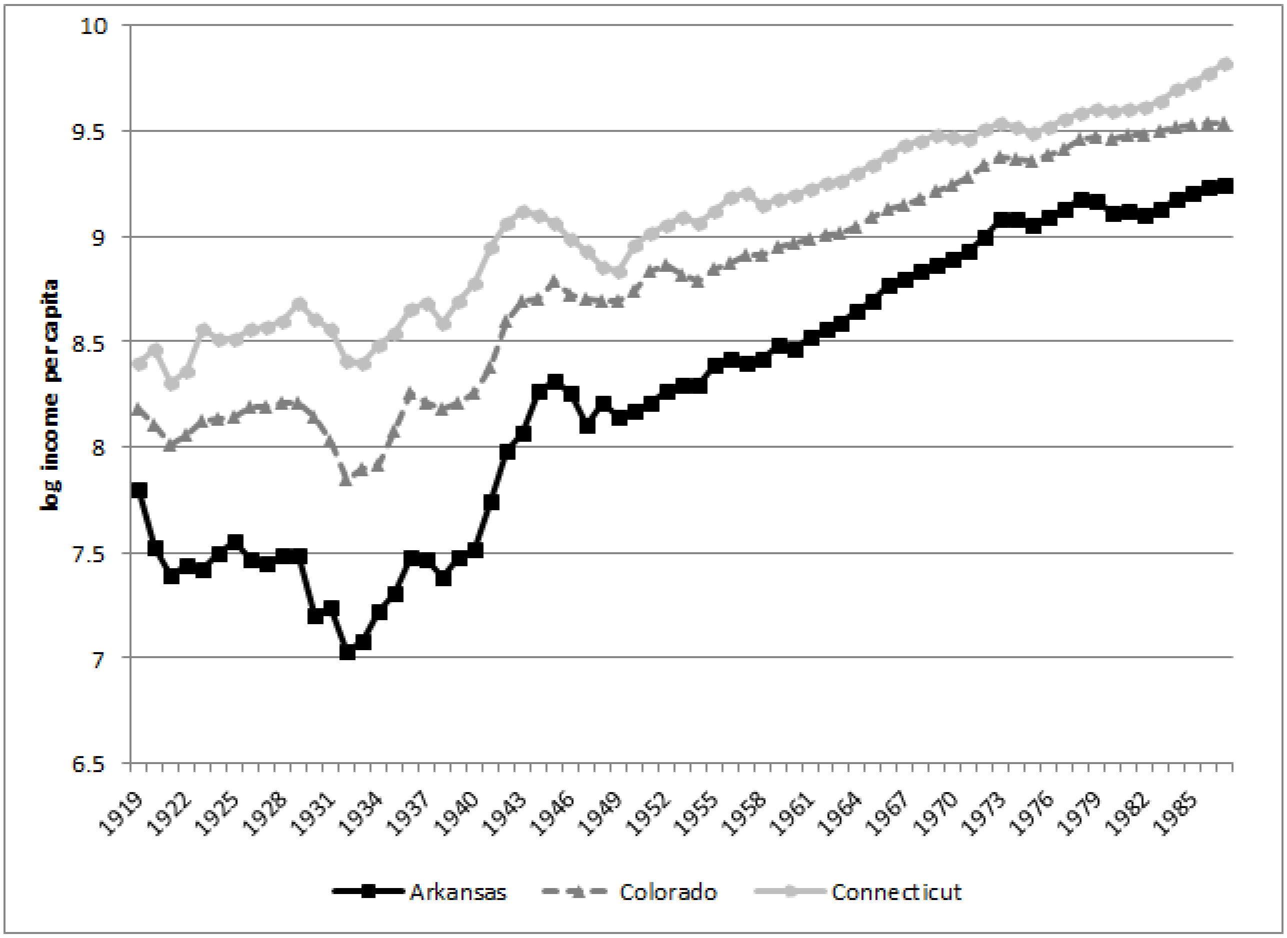


Figure 2: Children ever born and relative income by birth year (1919-1955)

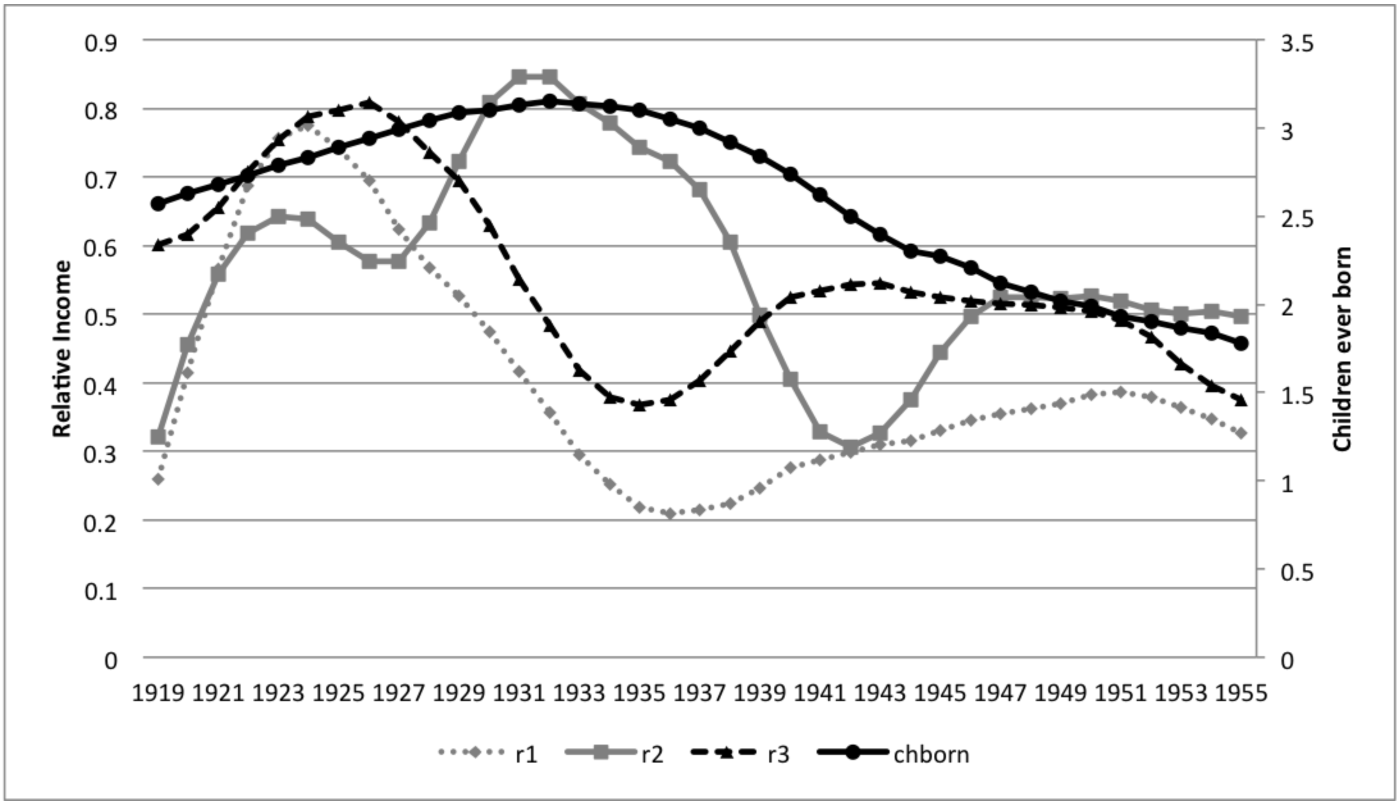
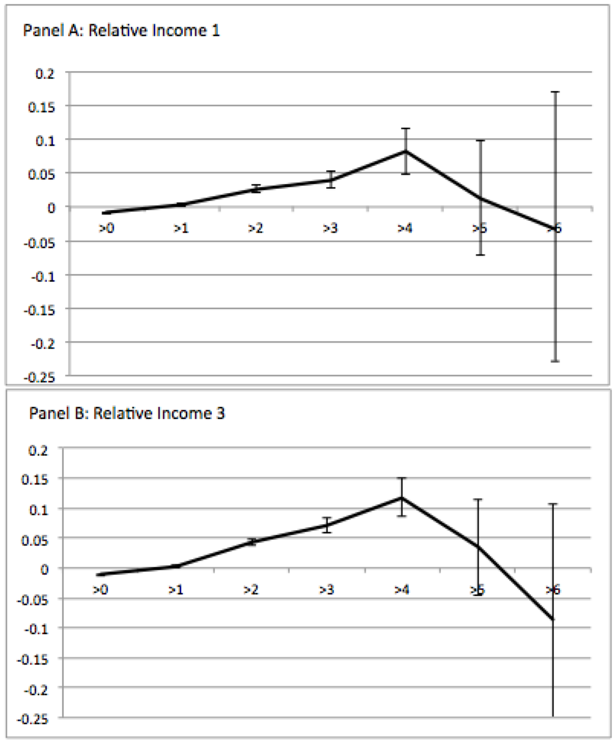


Figure 3: Effect of relative income on the probability of having more than X children




The figure shows the results of a series of regressions where the dependent variable equals 1 if the woman had more than X children conditional on having at least X children. The variable of interest is relative income, panel A shows the results when RI1 is used, and panel B shows the results for RI3. The x axis plots the number of children. The y axis is the percentage effect a 30 percent increase of relative income on the probability that a woman had more than X children, conditional on having at least X children. Error bars are a 95 percent confidence interval.

Table 1: Summary Statistics

| Variable | |
|---|---|
| Age | 51.3 |
| | (11.2) |
| <u>Education</u> | |
| less than HS | 27.0% |
| High School | 40.4% |
| more than HS | 32.6% |
| | |
| Black | 9.9% |
| Urban | 69.3% |
| Never married | 5.6% |
| Age at first marriage | 21.4 |
| | (5.23) |
| <u>Children ever born</u> | |
| 0 | 14.1% |
| 1 | 12.9% |
| 2 | 26.7% |
| 3 | 20.6% |
| 4 | 12.0% |
| 5 | 6.2% |
| 6 | 3.3% |
| 7+ | 4.3% |
| mean | 2.63 |
| | (1.98) |
| <u>observations</u> | |
| 1970 | 460,653 |
| 1980 | 1,741,873 |
| 1990 | 2,536,940 |
| total | 4,739,466 |

Notes: standard deviations in parentheses.

Table 2: Definition of migration groups and percent of sample in each group

| | |
|---|---|
| Group 1 (probable adult and child states identified) | |
| all state locations agree (own bpl, spouse bpl, children's bpl, current, 5 yrs. prior) | 18.9 |
| spouse moved to current state (only spouse bpl differs) | 11.3 |
| moved after children born (current state/5 yrs. prior differs from own bpl, spouse bpl, children bpl) | 1.3 |
| bpl=spouse bpl=current state (children bpl not available) | 14.2 |
| bpl=current state (spouse missing or different, children bpl not available) | 16.5 |
| movers (own bpl=spouse bpl then move to current state=children's bpl) | 2.0 |
| bpl=children's bpl (spouse missing or different, current state differs) | 1.7 |
| Group 2 (probable child state identified) | |
| bpl=spouse's bpl (other state info missing or different) | 4.8 |
| Group 3 (probable adult state identified) | |
| moved to spouse (own bpl differs, but spouse and children have same bpl) | 3.7 |
| moved to spouse (same as previous except no children bpl available) | 3.4 |
| children all born in current state (spouse bpl and own bpl differ) | 6.3 |
| moved to spouse and then moved again (spouse and child bpl the same, but current state differs | 0.6 |
| Group 4 (unable to identify adult or child state) | |
| No matches between own bpl, spouse bpl, children's bpl, and current state | 15.5 |

Table 3: Effect of relative income on children ever born

| | Completed Fertility | | | Intensive |
|---|---|---|---|---|
| Panel A. Migration Group 1 | (1) | (2) | (3) | (4) |
| RI1 (lnc. 19-23/ Inc. 6-12) | -0.113** | 0.473*** | 0.371*** | 0.458*** |
| | (0.055) | (0.044) | (0.087) | (0.095) |
| RI2 (Inc. 19-23/ Inc. 0-4) | 0.765*** | 0.223*** | -0.103 | -0.093 |
| | (0.055) | (0.055) | (0.075) | (0.086) |
| RI3 (Inc. 26-30/ Inc. 6-12) | -0.178*** | 0.511*** | 0.458*** | 0.557*** |
| | (0.066) | (0.047) | (0.088) | (0.090) |
| birth cohort fixed effects | no | yes | yes | yes |
| birth state fixed effects | no | no | yes | yes |
| Panel B. Migration Group 1,2,3 | (1) | (2) | (3) | (4) |
| RI1 (lnc. 19-23/ Inc. 6-12) | -0.068*** | 0.127*** | 0.265*** | 0.364*** |
| | (.029) | (.038) | (.065) | (0.088) |
| RI2 (Inc. 19-23/ Inc. 0-4) | 0.392*** | 0.063* | -0.087 | -0.057 |
| | (0.048) | (0.037) | (0.063) | (0.073) |
| RI3 (Inc. 26-30/ Inc. 6-12) | -0.087*** | 0.142*** | 0.357*** | 0.467*** |
| | (0.037) | (0.037) | (0.061) | (0.067) |
| birth cohort fixed effects | no | yes | yes | yes |
| birth state fixed effects | no | no | yes | yes |

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

Notes: Table shows the marginal effects from a poisson regression where children ever born is the dependent variable. Standard errors (in parentheses) are clustered at the birth state level. Other controls: adult state fixed effects, urban dummy, race, education, age, age-squared, still married dummy, and census year fixed effects. Observations: (Panel A: Columns 1-3 - 1,804,637; Column 4 - 1,658,151; Panel B: row 1-3 - 2,357,539, row 4 - 2,170,441).

Table 4: Effect of relative income on extensive fertility margin

| Panel A: Migration group 1 | (1) | (2) | (3) |
|---|---|---|---|
| RI1 (lnc. 19-23/ Inc. 6-12) | -0.007 | 0.024* | -0.022*** |
| | (.005) | (.012) | (.006) |
| RI2 (Inc. 19-23/ Inc. 0-4) | 0.040*** | 0.027** | -0.006 |
| | (0.004) | (0.011) | (0.005) |
| RI3 (Inc. 26-30/ Inc. 6-12) | -0.012** | 0.020 | -0.026*** |
| | (0.006) | (0.013) | (0.005) |
| birth cohort fixed effects | no | yes | yes |
| birth state fixed effects | no | no | yes |

| Panel B: Migration group 1,2,3 | (1) | (2) | (3) |
|---|---|---|---|
| RI1 (lnc. 19-23/ Inc. 6-12) | 0.004 | 0.017*** | -0.026*** |
| | (.003) | (.004) | (.005) |
| RI2 (Inc. 19-23/ Inc. 0-4) | 0.028*** | 0.018*** | -0.010** |
| | (0.003) | (0.004) | (0.004) |
| RI3 (Inc. 26-30/ Inc. 6-12) | 0.004 | 0.016*** | -0.030*** |
| | (0.003) | (0.004) | (0.006) |
| birth cohort fixed effects | no | yes | yes |
| birth state fixed effects | no | no | yes |

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

Notes: Table shows the marginal effects from a probit model where the dependent variable equals 1 if the individual had at least one child. Sample is migration group 1, results with group 2 and 3 are comparable. Standard errors (in parentheses) are clustered at the birth state level. Other controls: middle-aged state fixed effects, urban dummy, race, education, age, age-squared, still married dummy, and census year fixed effects. Observations: Panel A - 1,804,637, Panel B - 2,357,539.

Table 5: Effect of relative income on marriage outcomes

| Panel A<br>Dependent Variable=1 if never married | (1) | (2) | (3) |
|---|---|---|---|
| RI1 (migration group 1) | -0.012** | -0.058*** | -0.023*** |
| | (0.005) | (0.013) | (0.004) |
| RI1 (migration group 1,2,3) | -0.022** | -0.040*** | -0.031*** |
| | (0.006) | (0.010) | (0.005) |
| birth cohort fixed effects | no | yes | yes |
| birth state fixed effects | no | no | yes |

| Panel B<br>Dependent Variable=age of marriage | (1) | (2) | (3) |
|---|---|---|---|
| RI1 (migration group 1) | -0.434*** | -0.972*** | -0.736*** |
| | (.057) | (.099) | (.173) |
| RI1 (migration group 1,2,3) | -0.502** | -0.649*** | -0.644*** |
| | (0.078) | (0.102) | (0.142) |
| birth cohort fixed effects | no | yes | yes |
| birth state fixed effects | no | no | yes |

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

Notes: Panel A shows the marginal effects from a probit regression where a dummy=1 if never married is the dependent variable. Standard errors (in parentheses) are clustered at the birth state level. Other controls: adult state fixed effects, urban dummy, race, education, age, age-squared, and census year fixed effects. Observations: (Panel A row 1 - 1,899,440; row 2 - 2,456,042). Panel B shows the results from an OLS regression where the dependent variable is age of marriage. Other controls: adult state fixed effects, urban dummy, race, education, current age, current age-squared, and census year fixed effects. Observations: (Panel B, row 1 - 651,980; row 2 - 861,096).

Table 6: Effect of relative income by groups

| | RI1 (migration group 1) | RI1(migration group 1,2,3) |
|---|---|---|
| Education | | |
| less than HS | 0.901*** | 0.634*** |
| | (0.154) | (0.150) |
| high school | 0.243** | 0.234*** |
| | (0.095) | (0.076) |
| more than HS | -0.058 | -0.031 |
| | (0.106) | (0.087) |
| Birth Year | | |
| 1919-1940 | 0.509*** | 0.403*** |
| | (0.096) | (0.077) |
| 1940-1949 | 0.110 | 0.142 |
| | (0.239) | (0.147) |

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

Notes: Table shows the marginal effects from a poisson regression where the dependent variable is children ever born. Sample used is migration group 1 and women with at least one child. Standard errors (in parentheses) are clustered at the birth state level. All regressions include birth year, birth state, and adult fixed effects. Other controls: urban dummy, race, education, age, age-squared, still married dummy, and census year fixed effects.

Table 7: Separating income measures

| | (1) | (2) | (3) |
|---|---|---|---|
| log income per cap. (age 6-12) | -0.564*** | | -0.468*** |
| | (0.070) | | (0.074) |
| log income per cap. (age 19-23) | | -0.773*** | -0.318*** |
| | | (0.137) | (0.110) |
| birth year fixed effects | yes | yes | yes |
| birth state fixed effects | yes | yes | yes |
| observations | 1658151 | 1658151 | 1658151 |

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

Notes: Table shows the marginal effects from a poisson regression where the dependent variable is children ever born. Sample used is migration group 1 and women with at least one child. Standard errors (in parentheses) are clustered at the birth state level. All regressions include birth year, birth state, and adult fixed effects. Other controls: urban dummy, race, education, age, age-squared, still married dummy, and census year fixed effects.

Table 8: Relative Income and regional trends

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| RI1 (migration group 1) | 0.425*** | 0.227*** | 0.621*** | 0.375*** |
| | (0.103) | (0.083) | (0.024) | (0.031) |
| RI1 (migration group 1,2,3) | 0.211*** | 0.130** | 0.244*** | 0.194*** |
| | (0.048) | (0.066) | (0.011) | (0.023) |
| birth state fixed effects | no | yes | no | yes |

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

Columns (1), (2) show the marginal effects from a Poisson regression where the dependent variable is children ever born. Columns (3) and (4) are coefficient estimates from an OLS regression. All regressions include birth year, birth state, and mid-state fixed effects. Other controls: birth year fixed effects (column 1 and 2), birth year by region fixed effects (column 3 and 4), urban dummy, race, education, age, age-squared, still married dummy, and census year fixed effects.