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Author
Hill, Matthew

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Essays on Marriage and the Economy: 1930-1960

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ABSTRACT OF THE DISSERTATION

Essays on the Marriage and the Economy: 1930-1960

by

Matthew Jonathan Hill

Doctor of Philosophy in Economics

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Professor Dora Costa, Chair

Chapter 1 examines the relationship between marriage and the economy during the 1930s.The Great Depression provides an ideal setting to examine the impact of economic downturns and recoveries on marriage outcomes. Using microeconomic data, I find that during the Great Depression, a standard deviation decrease in retail sales per capita, my proxy for local GDP, lowered a woman's probability of marriage by 18 percent. During the first few years of the crisis, the effect of GDP on marriage rates operated largely through high male unemployment.

Chapter 2 explores the impact of housing on marriage in the postwar baby boom period. The U.S. experienced an unprecedented increase in fertility during the baby boom. The elevated birth rates from 1946 to 1964 were driven in part by a shift toward more universal marriage; marriage rates increased by 25 percent from 1930 to 1950 and the average age of marriage fell by two years. This chapter argues that growth in the supply of housing after World War II contributed to the expansion of marriage during this period. Specifically, the paper estimates the effect of additional building permits at the city level on individual marriage outcomes. An instrumental variable approach is used to address endogenous permit location. I

find a standard deviation increase in permits to a city increased the probability of marriage in that city by 13 to 16 percent over a two-year period. The estimates suggest that the growth in housing supply in the late 1940s can explain about 33 percent of the difference in marriage rates between 1930 and 1950. Overall, the increase in housing supply can account for nearly ten percent of the baby boom.

Chapter 3 re-examines the Easterlin hypothesis with microeconomic data. Easterlin argues relative income was the primary driver of the post war baby boom. I test this hypothesis using state income data in conjunction with individual level data from the U.S. census. I find relative income did positively impact completed fertility. However, this effect largely operates through a cohort effect and relative income can explain at most fifty percent of the baby boom.

The dissertation of Matthew Jonathan Hill is approved.

William R. Summerhill

Naomi R. Lamoreaux

Leah Platt Boustan

Dora Costa, Committee Chair

University of California, Los Angeles

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VITA

Matthew Jonathan Hill received a Bachelors of Science in Applied Mathematics in 2003 from the University of California at Los Angeles. He received a Masters of Arts in Economics in 2008 and a Masters of Arts in U.S. History in 2011, both from University of California at Los Angeles. He was awarded the UCLA Dissertation year fellowship in 2011-2012 and the UCLA Center for Economic History Fellowship in 2009-2010 and 2010-2011. He received the UCLA Chancellor's Prize in 2008-2009 and the Economics Department award for outstanding teaching assistant in 2009. In 2012, he gave presentations at Royal Holloway, University of Mannheim, Pompeu Fabra, University of San Francisco, and the Bureau of Economic Analysis. He has also presented at the All-UC/ Berkeley Economic History Graduate Student Workshop (2011), the All-UC/ Caltech Economic History Conference (2011) and the UCLA Applied Microeconomic and Economic History Proseminars.

Chapter 1

Love in the Time of the Depression: The Effect of Economic Downturns on the Probability of Marriage

1.1 Introduction

Recent business cycle fluctuations have had no effect on marriage rates. While marriage rates fell in the most recent recession (2007-2009), they fell by the same amount during the boom years of 2004 to 2006. However, over the twentieth century female labor force participation and marriage markets have changed dramatically. The relationship between marriage and the business cycle may have changed over time as well. This paper examines the nature of the relationship between marriage and GDP in the early twentieth century by focusing on biggest economic calamity in the period: the Great Depression.

I begin by documenting the time series correlation between GDP and marriage rates from 1873 to 1994. I show that from 1873 to 1960 there is a strong positive correlation between marriage and GDP. After 1960 the correlation breaks down, coinciding with the entry of married women in the labor force. The time series relationship is suggestive of a causal relationship between GDP and marriage. However, there could be another phenomena, such as a changing age structure or low immigration, influencing the aggregate trends in both GDP and marriage. Furthermore, the time series relationship does not reveal the mechanisms through which GDP affects the marriage rate.

Microeconomic data allows me to understand the influence of countervailing trends underlying a decline in

GDP. Cultural norms of the 1930s held that women should leave the labor force upon marriage (Goldin 1990) and that new couples should establish their own household. Therefore high male unemployment may have reduced the pool of marriageable men. In addition to high male unemployment, uncertainty about a potential mate's future income and obligations to financially help one's family may have also caused women to delay marriage. However, other factors associated with falling GDP would push the propensity to marriage in the opposite direction. Single women faced increased unemployment in the Great Depression. An unemployed woman would have had limited opportunities for future income because new jobs were scarce and government relief was targeted at families and the elderly.[1] Thus, decreased female labor force opportunities may have increased women's probability of marriage. Falling home prices may have done so as well. Home prices fell substantially over the 1930s: an average of 30 percent in the major metropolitan areas (Haines 2005). In recent times, falling home prices have been linked to increases in marriage rates (Lauster 2006) because cheaper houses decrease the costs associated with starting a new household.

The 1930s provides an ideal setting to test the impact of GDP on marriage rates. The Depression engendered significant variation in local GDP across space and time. Spatially, the Great Depression was most severe in the Midwest, where the Dust Bowl destroyed lives and livelihoods, and also in the mountain states, where over-reliance on single industries crippled local economies. Conversely, the South Atlantic states had a milder contraction due to higher trend growth rates of employment in the region (Rosenbloom and Sundstrom 1999). There were significant differences in local GDP over time in the Great Depression as well. On average, cities experienced a 30 percent drop in GDP from 1929 to 1933 followed by 20 percent recovery to 1937 before the recession of 1937 struck. And by 1939, most cities had returned to pre-Depression GDP figures. I exploit this variation over time and space in a duration model and a linear probability model to estimate the effect of local GDP, housing prices, and unemployment rates on the probability of marriage for young women.

Employing a variety of data sets to untangle the impact of economic considerations on the marriage decision, I find local GDP (proxied by retail sales per capita) can explain most of the change in marriage rates over the 1930s. I also examine the specific channels through which GDP can influence marriage. I find that decreased labor force opportunities for women (via high unemployment) led to higher female marriage probabilities. Conversely, decreased male labor force opportunities led to lower female marriage probabilities. The fixed costs of household formation also influenced marriage probabilities: as lower home prices increased the chances a woman married.

This paper adds to previous works that find evidence of a strong correlation between historical marriage rates and economic conditions. Haines (1996) suggests resource abundance was the cause of early age

[1]See Goldin (1990) pg 111-112 and Fishback (2005)

at marriage in the North American British colonies. Fitch and Ruggles (2000) propose that the economic expansions of 1890-1930 and 1945-1960 are responsible for the falling marriage ages in those periods. Working in the time period before 1930, Cvrcek (2010) finds that more marriage was associated with better jobs and larger pools of marriageable men.

The paper proceeds as follows: Section 2 documents the long run trends in the correlation between GDP and marriage rates and also discusses marriage specifically within the context of the Great Depression. Section 3 presents my two estimation methods. The first method estimates the effect of GDP on marriage during the Depression, while the second method decomposes the effect of GDP into specific economic factors. Section 4 details the data sets used in the estimations. Section 5 reports the results of the estimations. Section 6 discusses the implications of the estimates and section 7 concludes.

1.2 Trends

In order to place the Great Depression in context, I report the long-run trends in GDP and marriage rates in the United States. Figure 1, a scatter plot of de-trended marriage rates and de-trended GDP per capita in the United States from 1887 to 1960, shows a positive relationship between GDP and marriage rates. The trend-line is upward sloping with above-trend GDP years corresponding to years with marriage rates above trend. However, in more recent data this positive relationship breaks down. Figure 2 is the same scatter plot for the years 1960 to 1994. The data for this later period exhibits no clear relationship and the trend-line has a slope close to zero.

When I more formally examine the relationship between GDP and marriage rates, I find a positive relationship until 1960. After 1960 the relationship becomes statistically insignificant (see Table 1). My results are based on the following equation:

$$mr_t = (a + b \cdot trend)gdp_t + \varepsilon_t \tag{1.1}$$

where mr_t is the de-trended marriage rate in year t and gdp_t is the de-trended log GDP per capita in year t and trend is a linear time trend. Using the Cochrane-Orcutt correction for first-order autocorrelation, I estimate the parameters for the entire period and for various sub-periods. I do not include war years, as the relationship between GDP and marriage rates may be obscured by the absence of large populations of marriageable men. As Table 1 shows, the correlation between the two variables is significant in every specification except the most recent period (1960-1994). The trend variable is insignificant except for the post-WWII era: here it is negative and significant. Taken in concert, these results suggest a positive relationship between GDP and marriage rates until WWII. After WWII the relationship trended downward eventually becoming insignificant in the post 1960 era.

The erosion of the relationship between GDP and marriage rates after 1960 coincides with the rise in married women's labor force participation rates. In 1950 the labor force participation rate of married white women age 25-34 was 23 percent by 1980 this figures more than doubled to 56 percent. In contrast, the labor force participation rates of single women rose by only 11 percentage points in that same period: from 51 to 62 percent.[2] Prior to the 1960s, married women typically entered the labor force only if their husband could not provide for the family. However, by the end of the twentieth century many women resembled men in their attachment to the labor force. As women have become less reliant on men for their economic security, fluctuations in male incomes should have less of an effect on the female marriage decision. In addition, the optimal timing of marriage has changed for women. Many career-oriented women postpone marriage until graduation from college or completion of a professional/graduate program. (Goldin and Katz 2000) For these women marriage timing depends more on the lengths of internships and graduate programs rather than on the business cycle. The Great Depression struck before these changes took place: when a family's economic status was still tied to the husband and only 12.4 percent of urban married white women worked. It was a period when economic misfortunes of men should have still affected household formation.

Data on age at first marriage obtained from the 1940 US Census suggests that economic fluctuations during the Great Depression did influence marriage. Age at first marriage stayed fairly constant from 1925 until 1932 and then increased sharply until 1935 and remained high until 1937 (see Figure 3). Marriage rates in the 1930s decreased until 1934 and then increased until 1937. A likely explanation for the rise in age at first marriage from 1932 to 1935 is some of the women who had forgone marriage during the marriage downturn of 1930-1933 married in the recovery period from 1933-1937. These women would be a few years older and would thus push upwards the average marriage age from 1934-1937.[3] Qualitative primary sources echo the trends observed at the national level: women delayed marriage during the downturn. They delayed because of the shortage of men who could support a wife and for the sake of their family. Women in Chicago families interviewed by Ruth Cavan and Katherine Ranck explained, "The boys have no jobs," and, "I want a man with a job."[4] Meanwhile a mother, wary of losing her working daughter's income, relayed, "I hope she will not marry for two years as the family needs her help." Falling incomes in general also took their toll on marriage. A woman in the Chicago study has a boyfriend but they must delay until "he can support her."

[2] Goldin (1990) for all figures, page 129

[3] There also could be other explanations for the trend. For instance, increased education or increased labor force participation could explain the rise in age at first marriage. However, the trend is suggestive of marriage delay.

[4] All quotes from The Family and the Depression (1938).

Men in the sample complain about barely being able to cover their own expenses let alone support a wife. An Oakland study conducted by Glen H. Elder, Jr. yields similar experiences: widespread delay amongst his sample of men and women. Young Oakland women blame their inability to entice desirable men on financial constraints, noting they could not afford nice dresses or keep up proper grooming habits. Meanwhile, young Oakland men also had difficulties finding jobs and affording courtships.[5]

The primary sources also contain evidence of countervailing trends that would increase marriage. In Studs Terkel's oral history of the Great Depression, one woman associated marriage with escape "I thought maybe if I got married I could eat hamburgers and hot dogs all night... anything would be better than coming home and sleeping on the floor."[6] Many of the people profiled also complained about falling home prices. "All I have left is a house... worth $750,000. I don't what I could get for if I tried to sell it," commented one former millionaire. A woman of more modest means related, "Our house was sold. It was considered the most attractive house in town... it even had a music library. Imagine my shock when it was sold for $5,000 in back taxes." These voices reinforce what is found in the aggregate home price data: young couples could get a bargain on housing.

The primary sources and the aggregate time series suggest a relationship between GDP and marriage, but both have shortcomings. The primary sources are taken from a small subsample of people and therefore are not representative of the national experience. And while the aggregate patterns demonstrate a correlation between GDP and marriage, the two patterns could be unrelated and merely a product of differing regional patterns. Hence, I use microeconomic data to more firmly link marriage decisions to GDP and identify the important countervailing trends contained within an economic downturn.

1.3 Empirical Strategy

I employ two methods to estimate the effect of GDP on marriage in the Great Depression. First, I use a Cox proportional hazard model to estimate the effect of retail sales (my proxy for GDP) on a woman's marriage probability. The hazard model estimates marriage probabilities for each age. Therefore, comparisons between subjects are made between women of the same age who face similar baseline probabilities rather than between women with disparate ages who might face different marriage prospects. I supplement the hazard model with a linear probability model with two cohorts (before the Great Depression and during the Great Depression). This two-cohort specification allows me to untangle the underlying trends within GDP by incorporating economic variables measured at low frequencies, including home prices and unemployment.

[5]see Children of the Great Depression (1974).
[6]See Hard Times (2000).

For the Cox proportional hazard model, I limit my sample to white women who were of marriage age between 1928 and 1940. A woman enters my sample when she becomes at risk for marriage. I set this starting date to age 14. I chose 14 as a starting point because this is the earliest legal marriage age for a several states and I observe almost no marriage before this age in my sample.[7] The hazard model ends (or fails) upon a women's marriage. I subdivide the twelve-year period into two-year increments. Specifically, I assign a woman a 1 (i.e. a failure) if she married in a two-year period and a 0 (i.e. non-failure/survival) if she did not. I use two-year blocks to limit the measurement error in both the marriage year calculation and the interpolation of the GDP variable. The two-year periods in my data are 1928-29, 1930-31, 1932-33, 1934-35, 1936-37 and 1938-39. I estimate the following model:

$$h(t|x_i, y_{jt}) = h_0(t) exp(\alpha_j + \delta_t + x_i^{'}\beta + y_{jt}^{'}\gamma) \tag{1.2}$$

where t indexes age and $h_0(t)$ is the baseline hazard ratio for individuals of a given age,[8] α_j is a city fixed effect, δ_t is a year fixed effect, x_i are the individual controls[9] and y_{jt} are the city level variables for city j in which individual i lives in time t . The city level variables include the following city controls: percent black, percent foreign born, and percent urban. I include these controls because they all affect the size and functioning of marriage markets for a given city. The city variables of interest are work relief spending per capita, building permits per capita and retail sales per capita, where retail sales proxies local GDP.[10] The city fixed effect (αj) controls for any other time invariant aspects of a city. For example, religious groups in specific cities may put pressure on women to marry. The year fixed effect controls for nationwide trends in marriage (i.e., if there was an upwelling of optimism following Roosevelt's election). In tandem, these fixed effects allow me to identify the effect of local GDP (i.e. retail sales) on marriage via variation within cities over time.

The above specification requires biennial observations for the economic variables. This excludes two factors of interest, employment and home prices. Employment data is not available for all sectors in all years and home price data does not cover the entire time period.[11] Both factors are potentially important influences on the marriage decision. An increase in the male unemployment rate reduces the number of marriageable men and therefore decreases the female marriage probabilities. Conversely, an increase in the female unemployment rate should widen the pool of men which women will find acceptable to marry because women now have fewer outside options for economic support. Most couples set up a new household upon

[7] Tennessee and Mississippi are the only states in which a woman could legally marry before the age of 14.
[8] The variable of interest: retail sales satisfies Schoenfeld test for proportionality.
[9] High school educated, and foreign born dummies.
[10] Private charitable spending and other government spending are also included.
[11] There is data for the manufacturing, retail and wholesale industries for multiple years. I do not include this because it only partially represents employment. The results are robust to inclusion of the data.

marriage. This involves the purchase or rental or a dwelling. Hence, lower home prices will decrease the fixed costs of marriage and therefore increase the likelihood of marriage for women.

My second estimation method is a linear probability model incorporating housing and employment data. I pool two cohorts of women to estimate the model. The first cohort was age 19-22 and not yet married in 1926, and the second cohort was age 19-22 and not yet married in 1930. The outcome variable is whether the woman married in the subsequent four years (1926-1929 for the first cohort, 1930-1933 for the second cohort). Note that the first cohort does not experience the Great Depression during their prime marriage year window while the second cohort does.

I estimate the following equation:

$$M_{ij} = \alpha_j + cohort_i + x_i^{'}\beta + y_j^{'}\chi + \varepsilon_{ij} \tag{1.3}$$

Where M_{ij} is a dummy equal to 1 if individual i in city j was married in the relevant four year period, α_j is a city fixed effect, cohorti is a cohort fixed effect, x_i is a vector of individual characteristics that may impact the marriage decision, and y_j is a vector of city variables for the relevant four year period. The city fixed effect controls for any time invariant differences between cities in their marriage culture, while the cohort fixed effect controls for any national time trend. Therefore the vector of the coefficients of interest χ, the effects of a city's economic characteristics on the marriage probability, is identified by economic changes within a city over time.

1.4 Data

1.4.1 Sources

The core of my dataset is sample-line women from the 1940 census (my data includes no males because only women were asked the sample questions). This subsample was asked at what age they married. For these women I constructed a probable marriage year.[12] In addition to age at marriage, the census contains all the relevant background characteristics including race, education, age, and nativity status.

I then merged this individual data with city level data for the city in which the person lived. Primarily, I use a detailed panel data set compiled by Fishback, Haines, and Kantor (2007) on 114 cities for the years

[12]This calculation is not precise as marriage year is measured Jan. to Dec. whereas age runs from birthday to birthday. In the empirical specifications I use a dummy variable equaling one if the woman married in a two-year period. This should mitigate some of the measurement error. Also any misclassification of marriage year should not be systematic in any one direction. Therefore my estimates will be attenuated and not upwardly or downwardly biased.

1929 through 1939.[13] These 114 cities include 87 of the largest 100 cities in terms of population and account for 37 percent of the total populace.

This data set includes retail sales data for several years during the Depression. No income data exists for the Great Depression; therefore I use retail sales as a proxy for local macro-economic activity. There is ample evidence showing retail sales are a good proxy for economic activity. Fishback, et al. (2007) found annual national aggregates of retail sales to have correlations above .99 with total personal expenditures for the time period 1929 to1969. Within the Great Depression, Fishback, et al. (2007) show correlations between retail sales and state-level per capita income to be .87, .89, .88 and .90 for the years 1929, 1933, 1935, 1939 (these were the only years for which retail sales was available). [14]

Government relief spending during the period could have also influenced marriage rates. The federal government greatly increased relief spending during Roosevelt's New Deal. In the first year of the New Deal, relief spending per capita rose from $30 to $48. The city panel dataset includes data on relief programs by year. It is classified as either general relief or work relief. General relief includes non-work related spending, such as aid to the disabled and blind,[15] while work relief is spending focused on providing jobs to unemployed persons.[16] This classification is useful because the two types of spending might have different effects on marriage. General relief might have no effect on marriage because it primarily flows to people not of marriageable age.[17] Work relief, on the other hand, might increase marriage probabilities because it potentially gives young single unemployed men a source of income.

The panel dataset also contains other control variables that might affect the marriage market in a given city. Marriage markets are typically confined within ethnic groups. To account for this I include the variables percent black and percent foreign born. I also include percent urban because a woman might be exposed to more potential mates in denser cities.

I am able to control for unemployment and house prices for a subsample of cities. Prior to 1940, standardized measures of unemployment do not exist because the modern definition of unemployment was developed during the 1930s. I use the 1931 Special Census on Unemployment because it contains unemployment figures closest to the modern definition. However, this census was only conducted in 19 cities and only in 1931.[18]

[13] I had to combine some cities in order for the census data on metropolitan areas to match this city data. Among the census sample there were also cities that had no observations, i.e. no one lived in them in the census subsample. In the end my sample included 90 of the 114 cities.

[14] The original sources for the Retail Sales variable: for the years 1929 and 1939 - ICPSR file number 0003. For 1933 and 1935 - the U.S. Department of Commerce, Bureau of Foreign and Domestic Commerce (1936, 1939).

[15] These include programs such as the SSA and general relief under FERA.

[16] These include programs such as the CWA, WPA, public works projects and work relief under FERA.

[17] The direction of the effect is by no means unambiguous. General relief spending might actually increase marriage because after receiving relief an individual does not have to care for their elderly relative. Work spending might disproportionately go to family men thus having no effect on the unemployment of single men.

[18] There were two other unemployment studies conducted in the 1930s. The 1937 Special Census of Unemployment classifies both truly unemployed persons and WPA workers as unemployed hence these figures are probably over estimates. The unemployment relief census in 1933 only counted people on relief. Thus the 1933 census underestimates unemployment.

I use housing data for 13 cities from 1926 to 1934. In 1937 the Department of Commerce released a Financial Survey of Urban Housing. The survey asked homeowners when they bought their house and for how much. From the homeowner's response, the Department of Commerce constructed annual home price data for the surveyed cities starting in 1889 and ending when the survey was conducted in 1934. The methodology of the survey has sample selection issues, i.e. homeowners who are still living in older houses might be from the lower end of the income distribution. I use only the data from 1926 through 1934, because this portion of the data is the most accurate due to its proximity to the time of the survey.

I also control for new building permits. Building permits, like home prices, might serve as a measure for access to housing. The Bureau of Labor Statistics recorded new building permit issues annually for cities of 100,000 or more in the United States. These data have a few limitations. Firstly, the permit data documents residential building within a city's limit. Unincorporated land is not included; therefore not all of the construction undertaken around a city is captured. Also very little building was done during the Great Depression. The new permits exhibit a very low variance across time and place.

Table 2 reports the means for the economic variables of interest by year for sample of women used in the duration model. The patterns observed in the city variables are consistent with the Depression experience. Local GDP and new building permits follow a similar pattern, falling from 1929 to 1933 and then rising thereafter. Federal work relief spending grew steadily during the 1930s reflecting the increased role of the federal government in society.

1.4.2 Mapping Individual data to City Location

My empirical strategy relies on locating women in the city that they lived in from 1929 to 1940. The census reports place of residence for all women in 1940. Furthermore, the 1940 census asked women where they lived five years prior. For women who lived in the same city in both years, I assume that they lived in that city for all the years between 1935 and 1940. For women who moved and married between 1935 and 1940, I use their husband's location in 1935 and their marriage year to determine a probable migration year.[19] Thus, I can locate most of the population of interest (women who married in the 1930s) from 1935 to 1940.[20]

Placing women from 1930 to 1935 is more difficult because the only information available is location in 1935. I assume a woman lived in the same city from 1930 to 1935. This introduces potential biases. For instance, consider a woman who moves from a low GDP to a high GDP city in 1933 and then marries a man

[19] For example, take a woman married in 1937 to a man who had the same 1940 city as her but a different 1935 city. Then I assume she married after migration and assign 1936 as the probable migration year.

[20] For all other women I assign them to their 1940 location from 1936 to 1940 and their 1935 location for all years prior to 1935.

she met from her new city in 1934. I would incorrectly record her as unmarried in the new high GDP city 1930 to 1933 when she was unmarried in her old low GDP city. A wrong assignment of this type would bias the estimate of the local GDP effect downward. Similarly migration from a high GDP city to a low GDP city would bias the coefficient on GDP upward. Table 3 reports the migration of women from 1935 to 1940. On average, single women moved from cities with low retail sales to cities with high retail sales. Thus, if the migration patterns from 1930-1935 were similar to those from 1935-1940 then assigning women to incorrect cities from 1930 to 1935 will downwardly bias my estimates on the effect of GDP.

To mitigate the possible migration bias, I also perform the empirical analysis on two sub-samples with less potential migration. The first subsample uses data only post-1935. This completely removes any of the problems of the pre-1935 misallocation. The second sample is cities with low in and out migration rates from 1930 to 1940.[21] This subsample will have fewer female migrants than the full sample.

1.5 Results

1.5.1 Hazard Model

Table 4, which gives the results of the Cox proportional hazard model estimation from equation (2), shows that local GDP had a significant positive impact on women's probability of marriage. Column 1 contains the estimates for the whole sample of cities over the entire time period. The results suggest a one standard deviation increase in retail sales would increase a women's marriage probability by 11 percent above the baseline probability for her age. Or even more dramatically, if a women living in the lowest retail sales city in 1937 (Johnstown, PA) were to move to the highest retail sales city (San Francisco, CA), her chances of marriage would increase by 60 percent. Columns (2) and (3) show the results for samples with less migration bias.[22] The coefficient estimates are larger in these two columns (a standard deviation increase in retail sales now increases marriage probability by 15-20 percent instead of 11 percent), suggesting migration bias is downwardly biasing the main result in the first column.

The effect of GDP on marriage is stronger for less educated women. During the Great Depression, uneducated men were more likely than educated men to experience employment shocks. Due to assortative

[21] Migration data is from Haines, et al. (2005)

[22] In general, the retail sales coefficients estimates are smaller in specifications that only include the pre-1935 period. I attribute these smaller estimates to migration bias. Specifications using cities with low migration or with only women who currently live in the same state as their birthplace have coefficient estimates that are comparable to the estimates for the later period. One might be concerned the smaller coefficient estimates stem from omitted variable bias and not migration bias. Such omitted variables would be positively correlated with retail sales but negatively correlated with marriage. These include female unemployment or housing declines. If I run the estimation on just cities with high female unemployment or large housing price declines I find larger coefficient estimates on retail sales, not smaller. This suggests omitted variables are not biasing the estimates downward rather it is migration issues that lead to the smaller estimates for the pre 1935 data.

mating, uneducated women should be more sensitive to these employment shocks than educated women. Columns 4, 5, 6 repeat the analysis from columns 1, 2, 3 for women without a high school education. The coefficient on retail sales rises in this subsample. A standard deviation increase in retail sales now increases marriage probability by 15 percent. This result suggests that GDP fluctuations exert more influence on marriage outcomes for uneducated women than on outcomes for educated women.[23]

Under Roosevelt's New Deal, federal spending skyrocketed. I classify the spending into two categories: work spending and relief spending. Work spending created jobs for citizens while relief spending primarily went to the elderly and widowed mothers. In theory, work spending should influence marriage because it transforms males into attractive mates via employment. In all the specifications, work spending had a negative effect on marriage.[24] The effect becomes significant in specifications where the migration bias is minimized. The probable reason for the negative coefficient is that the effect of GDP not accounted for by retail sales contaminates the estimate on work spending. Relief spending was targeted at areas with poorer economic conditions, hence the negative association between work spending and marriage. Attempting to disentangle relief spending from the GDP effect is beyond the scope of this paper. My results indicate, prima facie, government work spending is not associated with higher marriage rates during the Great Depression.

Education is the most important individual characteristic affecting the marriage decision. The estimates suggest a woman with a high school education or more is 40 percent less likely to marry at a given age than a woman who lacks a high school diploma. Goldin (2006) posited that the reason divorce rates rose in the second half on the twentieth century was the increased labor force opportunities for women. I observe a evidence of a similar pattern during the Great Depression– educated women had more labor force opportunities and thus were not driven to marriage in the way an uneducated women with few labor force options might have been.

Building permits per capita do not affect a woman's probability of marriage. Building permits are indicative of an expanded housing stock and therefore, a likely decline in housing prices. I expected access to housing to be positively correlated with marriage. Although this effect is not found in the building permit data, it is possible the relationship does still exist. It may not manifest itself in building permits because there is very little variation in new permits across time and space in the Depression. New building was simply not undertaken in the time period. Because of this lack of construction, housing prices might be a better indicator of the relationship between housing and marriage.

[23] Instead of splitting the sample I could interact education with retail sales. If I do so with the full sample, the coefficient on retail sales is 0.40 with a standard error of 0.225 and the coefficient on the interaction is .18 with a standard error of .074.

[24] The coefficients on other spending and private charity are insignificant in all specifications.

1.5.2 Linear Probability Model

Table 5, which gives the results of the linear probability estimation (equation 3), shows that falling home prices made women more likely to marry. Column (1) reports the results of the estimation on the subsample with housing data available.[25] While this subsample is relatively small (only 8 cities are in the sample), I find a negative and significant effect of a housing price changes on marriage. A standard deviation decrease (9 percent decline) in the percent change in housing prices increased a woman's marriage probability by 2 percentage points from 1930 to 1933.

Table 5, column (2), shows the effect of unemployment on the probability of marriage.[26] As theory predicts, female unemployment and male unemployment have opposite effects on the probability of marriage for women. Higher male unemployment shrinks the pool of marriageable men and lowers the women's probability of marriage. Conversely, higher female unemployment drives women to marriage. Specifically, I find a 10 percentage point increase in male unemployment would lower the marriage probability of women by 13.5 percentage points while a 10 percentage point decrease in female unemployment would increase female marriage probability by 10 percentage points. Finally, the negative coefficient on retail sales in this specification suggests most of the effect of local GDP operates through unemployment.

1.6 Implications

This section uses my preferred estimates of the relationship between GDP and marriage rates to explain the marriage patterns over the 1930s. The marriage rate fell by about 20 percent from 1928-1929 to 1932-1933. My estimates suggest GDP accounts for about 90 percent of this downturn. Retail sales, my proxy for GDP, fell by 35 percent from 1929 to 1933.[27] According to the estimates from the hazard model, a retail sales decline of this magnitude would lower the baseline -probability of marriage by 19 percent over a two-year period.[28] I use the 1930 census to aggregate the individual-level change in marriage probability up to a change in the national marriage rate. The 1930 census has age at first marriage, so I was able to construct marriage rates by age for the two-year period of 1928-1929.[29] I then lowered the marriage rates by 19 percent in each age group to obtain the overall predicted marriage rate for 1932-1933. Figure 4, which gives the

[25]There are more cities with housing price data, but all these had in-migration of over 10%, and I did not want to include them in the sample.

[26]The unemployment sample is larger than the other two samples for two reasons. 1) I do not limit the sample to low migration cities (the results are unchanged if I do, however). 2) The cities chosen for the 1931 Special Census on Unemployment were amongst the largest in the U.S.

[27]GDP declined by slightly less than retail sales: about 30 percent.

[28]Using the coefficient from table 3, column 1 and the mean per capita retail sales from 1929 of 1.10.

[29]The constructed national 1928-1929 marriage rate from the 1930 census of 149 marriages per 1000 unmarried women is roughly equivalent observed 149.6 marriages per 1000 women reported in the Historical Statistics of the U.S.

results of the above exercise, shows the predicted 1932-33 marriage rate is 19 percent lower than 1928-29 rate. The actual 1932-33 marriage rate is 21.5 percent lower than the 1928-1929 rate, therefore retail sales account for roughly 90 percent of this decline.

By 1936-1937 the marriage rate had rebounded to about 90 percent of its pre-Depression levels. My estimates suggest retail sales (the GDP proxy) can account for about two thirds of the rebound. In 1937, GDP had risen to about 95 percent of its pre-Depression value. With an economic rebound of this magnitude, the predicted marriage rate for 1936-1937 is 142 marriages for every 1000 unmarried women, a 20 percent increase from 1932-33. The actual marriage rate, as reported by the Historical Statistics of the United States, increased by 30 percent from 117 marriages per 1000 unmarried women to 153.

During the baby boom years, the marriage rate was 19 percent higher than 1928-1929. My estimates suggest the rate should have been even higher. GDP per capita more than doubled from the 1930s to the 1950s. Retail sales per capita also more the doubled, they increased by 120 percent from 1930 to 1950. I again repeat the above exercise to determine the effect of this retail sales increase on marriage rates. I report the results in figure 4. My estimates suggest this increase would cause marriages to increase by 27 percent from 1928-29 to 1950-51.[30] This out-of-sample over prediction is most likely due to marriage rates being correlated with GDP deviations from trend rather than with raw GDP per capita.

My estimates also have implications for understanding trends in fertility rates. The birth rate fell by 12 percent from 1929 to 1933. My estimates suggest that GDP induced marriage delay alone would decrease birth rates by 4 percent from 1929 to 1933.[31] Married women have higher birth rates than unmarried women; thus if there is widespread delay, birth rates will decrease. I again use the 1930 census to understand the magnitude of the effect of the 1929-1933 retail sales decline on fertility via the marriage channel. I first construct 1929 birth rates by age and marital status by linking women to the age in months of their youngest child in their household.[32] I then build a predicted birth rate for 1933 in the following fashion: I hold within age/marital status birth rates constant and move married women into the unmarried category to simulate the marriage delay from 1929-1933. For each age group I break the married women into three groups: married in the last 2 years, married in the last 2-4 years, and married more than 4 years ago. I lower the number of women in the married less than 2 years ago group by 18 percent to reflect the lowered marriage probability from 1932- 1933. I then lower the number of women in the married 2-4 years ago group by 10 percent to reflect the lower marriage probability from 1930-1931. I do not change the number of women married more

[30] If log (GDP) is used instead of GDP the results are unchanged.

[31] GDP will also likely have a direct effect on fertility. This paper concerns marriage, so I am only interested in the effect of GDP on fertility through marriage.

[32] The constructed birth rate after I adjust for infant mortality is close to the actual birth rate for women age 20-44, before age 20 the census birth rate is lower due to the fact I only observe children who remained in the household. If women gave their children up for adoption or if the child lived with relatives I do not observe them.

than 4 years ago. The result of this exercise is a predicted birth rate in 1933 of 72.1 births per 1000 women age 20-44. The observed birth rate in 1929 is 75.1 births per 1000 women age 20-44.[33] Therefore, the retails sales (a GDP proxy) decline from 1929-1933 indirectly lowered birth rates via the marriage channel by 4 percent via marriage delay. The observed birth rate from Historical Statistics of the United States is 66.3 births per 1000 women age 20-44. Therefore, GDP induced marriage delay can account for about a third of the total decline in the crude birth rate.

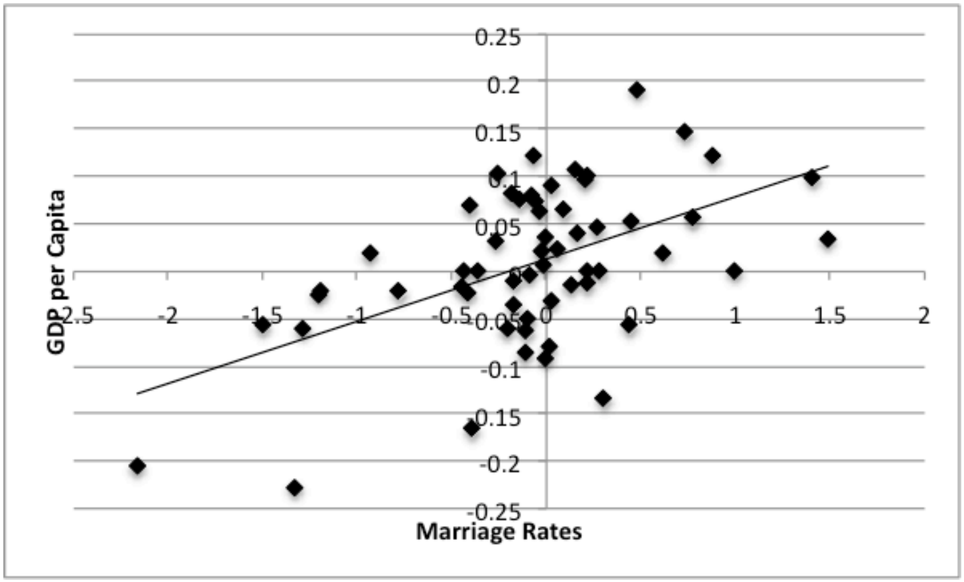
1.7 Conclusion

GDP strongly influenced the probability that a woman married during the Great Depression. My estimates suggest the thirty percent downturn in GDP from 1929 to 1933 lowered a woman's probability of marriage by 15 percent over a two-year period. The chief factor was high male unemployment. An increase in local male unemployment of ten percentage points would lower a women's probability of marriage by one fourth over a four-year period. However, the effect of GDP on marriage is by no means unambiguous. Higher female unemployment and lower home prices, trends associated with low GDP, positively impacted marriage rates.

In recent times societal changes have altered the traditional roles of spouses and muddied the effect of the economy in marriage. The birth control pill has given women control over their fertility schedule. Women can now avoid shotgun weddings and plan pregnancies to accommodate careers. No longer does the husband need to specialize in market production and the wife in home production. The marriage rate did fall in the recent 2008 recession; however, this decline is part of an overall downward trend in marriage rates. The retreat from marriage over the past thirty years has been remarkably steady. The marriage rate dropped an average of .13 percentage points per year from 1980 to 2009. The figures from the most recent recession are merely a continuation of that trend. The marriage rate fell from 7.3 per 1,000 people in 2007 to 6.8 per 1,000 people in 2009: a decline is similar to the .4 percentage point decline in boom years of 2004 through 2006. Therefore, while GDP certainly affected marriage in the Depression, it appears to have little impact on marriage in this most recent recession.

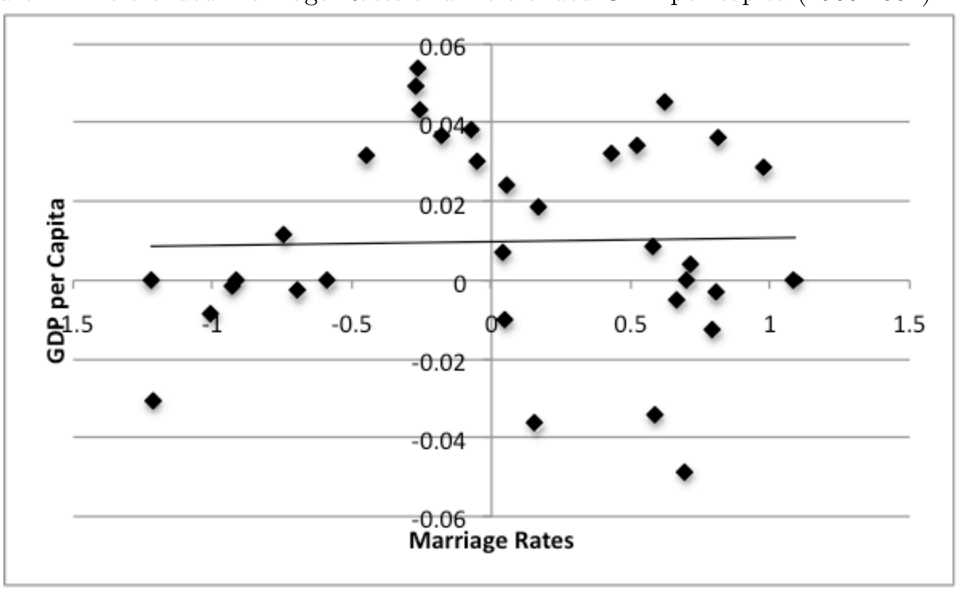
[33] All figures adjusted upward to account for infant mortality. I use infant mortality from Fishback, Haines, and Kantor (2007) of 60 deaths per 1000 births.

Figure 1 – De-trended Marriage Rates and De-trended GDP per capita (1887-1960, WWI and WWII excluded)



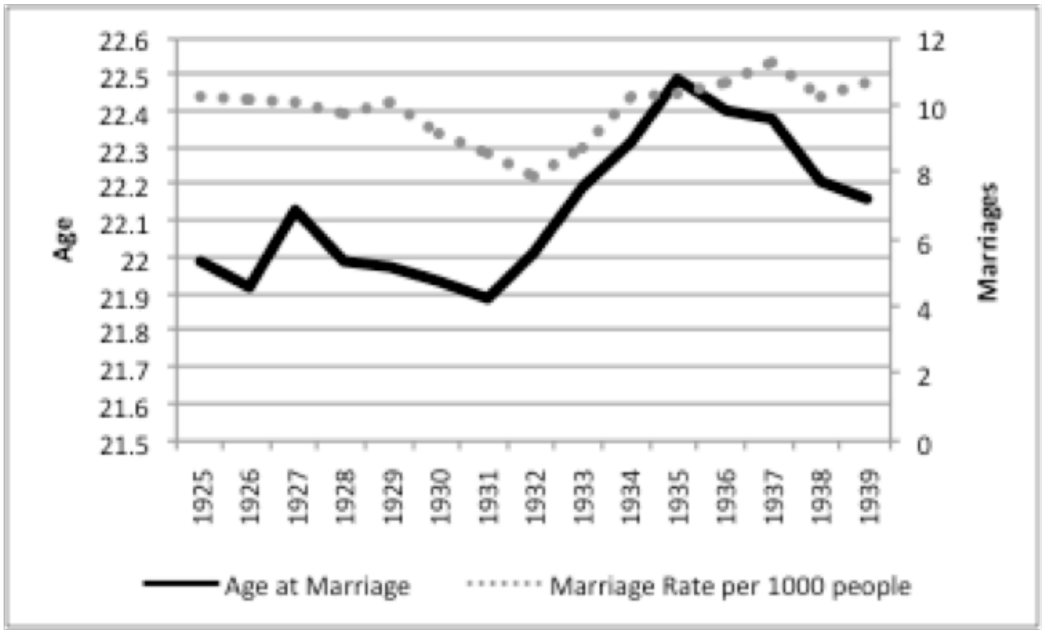
source: Historical Statistics of the United States, U.S. Department of Commerce

Figure 2 – De-trended Marriage Rates and De-trended GDP per capita (1960-1994)



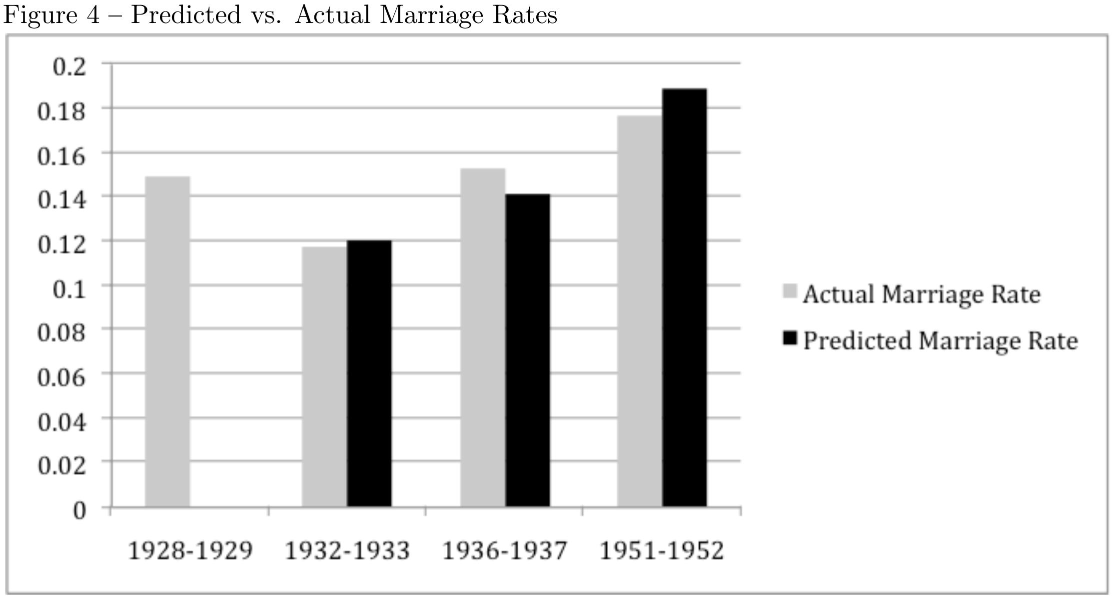
source: Historical Statistics of the United States, U.S. Department of Commerce

Figure 3 – Average Age at First Marriage for Women (1925-1939)



source: 1940 census (sample line women) on age at first marriage, Marriage rate from Historical Statistics of the United States, U.S. Department of Commerce

Figure 4 – Predicted vs. Actual Marriage Rates



note: marriage rate is per unmarried woman age 18-35

Table 1 – Time Series Relationship between GDP and Marriage Rates

Period	a	b
1873-1994(whole)	3.82***	-0.062
	(.637)	(.188)
1873-1915(pre-WWI)	3.12***	0.014
	(.566)	(.027)
1920-1939(inter-war)	6.54***	-0.250
	(1.73)	(.486)
1949-1960(post war)	9.49**	-1.58**
	(3.47)	(.627)
1960-1994(recent)	1.71	0.143
	(1.68)	(.484)

Notes: *** significant at the 99% level, ** significant at the 95%. The estimating equation is 1.1. One can think of a as the average effect of GDP on marriage rates in the time period and b as how much the effect changed from the start of the period until the end of the period. No specification contains the years (1916-1919) or (1940-1946).

Table 2 – Means of City Economic Characteristics by Year

	Building Permits per 1000 people	Retail Sales per Capita	Federal Work Relief spending per capita	Percent of women in sample with H.S. education	Percent of women in sample foreign born
1929	3.9	0.97	0.80	0.48	0.063
	[.321]	[.019]	[.111]	[.012]	[.007]
1931	1.4	0.79	3.75	0.50	0.053
	[.140]	[.017]	[.460]	[.013]	[.006]
1933	0.33	0.63	21.1	0.52	0.046
	[.038]	[.014]	[.940]	[.013]	[.005]
1935	0.52	0.77	36.3	0.54	0.041
	[.077]	[.016]	[1.77]	[.013]	[.005]
1937	1.6	0.93	33.1	0.55	0.039
	[.209]	[.018]	[1.85]	[.012]	[.005]
1939	2.3	0.95	43.8	0.52	0.038
	[.250]	[.018]	[2.06]	[.012]	[.005]
Number of cities	70	90	90	90	90

Notes: Standard error of the mean in parentheses.

Table 3 - Summary Statistics for the Cities Migrants Move To and From (1935-1940)

	Migrant's Original City		Migrant's New City	
	Mean	Std. Dev.	Mean	Std. Dev.
Percent Change in Retail Sales 1934-1937	.386	.125	.393	.125

Note: data on Retail Sales from Fishback, Haines, and Kantor (2007) and migrant data is from the 1940 Census

Table 4 – Hazard Model: Economic Factors Influencing the Probability of Marriage

	Full Sample			Less than H.S. Educated		
	Entire Period 1929-1939 (1)	1935-1939 (2)	Low Migration Cities (3)	Entire Period 1929-1939 (4)	1935-1939 (5)	Low Migration Cities (6)
Retail Sales	0.54**	0.93**	0.77***	0.73***	1.04**	0.95**
	[.22]	[.38]	[.300]	[.27]	[.41]	[.41]
Per Capita Work Spending	-0.0012	-0.003	-.004***	-.002	-.004***	-0.005***
	[.007]	[.002]	[.001]	[.001]	[.002]	[.001]
Building Permits per capita	-4.73	7.01	3.47	-12.03	-0.65	-6.08
	[11.9]	[15.00]	[14.74]	[12.89]	[16.67]	[16.09]
Dummy=1 for foreign born	-0.033	-0.066	-0.030	0.034	0.01	0.022
	[.030]	[.045]	[.028]	[0.030]	[.045]	[.033]
Dummy=1 if H.S. educated	-0.44***	-0.44***	-0.44***			
	[.023]	[.032]	[.028]			
Number of unique cities	62	62	37	62	62	37
Number of individuals	89617	67812	70271	49959	36157	40643

*significant at the 10% level, **significant at the 5% level, *** significant at the 1% level. Notes: Table shows estimates from Equation 2 in the text. Numbers are the actual coefficients from the hazard model estimation and are not hazard ratios. The standard errors (in brackets) are clustered the metropolitan area level. Low migration cities are those with migration less than +/- 5% between 1930 and 1940. The work variable includes all government spending on work projects (public grants, CWA, WPA)

Table 5 – The Effect of Unemployment and Housing on Marriage (Equation 1.3)

	Housing Sample (8 Cities)			Unemployment Sample (19 Cities)		
		Means			Means	
	(1)	1926-1929	1930-1933	(2)	1926-1929	1930-1933
Retail Sales per Capita in period	1.92**	1.05	0.69	-0.32	1.17	0.76
	[0.66]	(.105)	(.124)	[.227]	(.137)	(.131)
Total Per Capitia Work Spending in the perio	0.009**	0	15.2	0.0007	0	21.1
	[.003]	(0)	(8.17)	[.0007]	(0)	(13.9)
%Change in Housing prices over period	-0.21*	0.01	-0.34			
	[.092]	(.141)	(.062)			
Male Unemployment in period				-1.35***	0.095	0.257
				[.415]	(.016)	(.053)
Female Unemployment in period				1.02*	0.095	0.175
				[.511]	(.016)	(.037)
Mean Marriage Dummy	0.41			0.42		
R^2	0.08			0.03		
Number of Observations	669			5769		

*significant at the 10% level, **significant at the 5% level, *** significant at the 1% level. Notes: Standard errors (in brackets) are clustered by metropolitan area. For the means columns, standard deviations are in parentheses. The two periods are 1926-1929 and 1930-1933. In the case of unemployment and retail sales, data from 1929 is used for the first period and data from 1931 is used for the second period. While the two years are close together temporally, given large economic changes occurred between 1929 and 1931 the two years are reasonable proxies for the four year time periods they represent. Separate male and female unemployment figures were unavailable for 1926-1929, so total unemployment was used for both male and female unemployment in that period. Work spending is total federal spending over the entire period. Other controls: Education dummy, race dummy, foreign born dummy, percent black in city, percent urban in city, private charitable spending over the period, building permits per capita over the period.

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Chapter 2

Homes and Husbands for All: Marriage, Housing, and the Baby Boom

2.1 Introduction

During the baby boom (1946-1964) an average of 4.2 million babies were born per year, compared to 2.4 million per year in the 1930s. From 1946 to 1964, annual fertility rates were 30 to 50 percent higher than they were in the pre-war era. This increase in fertility was unprecedented in the United States. Fertility rates had been falling by 8 percent per decade between 1800 and 1940 and declined by 10 percent per decade between 1965 and 2005. The rise in birth rates in the baby boom is a result of two trends: first, there was an increase in birth rates within marriage and second, marriage itself became more universal. Marriage rates increased by 25 percent from 1930 to 1950, and the average age of marriage fell by two years.

Explanations for the baby boom have focused on the increase in birth rates within marriage. The primary hypotheses for this rise in fertility are that young couples who had come of age during the Great Depression had a preference for children over material goods (Easterlin), that improvements in household technology lowered the cost of children (Greenwood et al.), and that declines in maternal mortality led to an initial increase in the quantity of children (Albanesi and Olivetti). In contrast to the previous literature, I examine the cause of the rising marriage rates.

I argue that a burst in new housing construction, after a near construction freeze during the Great Depression (1929-1940) and World War II (1941-1945), can account for 33 percent of the observed change in marriage patterns from 1930 to 1950. After World War II, new residential housing boomed; 1.3 million residential units were built in 1950, over triple the number constructed in any depression-era year. However, the magnitude of this housing boom varied from city to city. I use a newly digitized dataset on permits per capita as a proxy for housing supply and exploit the variation between cities to estimate the effect of new permits per capita on marriage outcomes. I find that a standard deviation increase in permits per capita in a given year raised the probability of marriage by 6 to 8 percent in the subsequent year.

In theory, couples in areas where more homes are built should marry sooner, ceteris paribus, than couples in areas with fewer homes because of the long-standing cultural norm in Western societies that couples obtain their own housing upon marriage. Even in the pre-industrial age, the majority of Northern European households were made up of nuclear families (Hajnal, 1962). By the mid-twentieth century, this norm had not changed; one married couple per household remained the standard arrangement. For instance, in 1940, 93 percent of married twenty-five year-olds had their own household while just 44 percent of their non-married peers lived on their own. Given the expectation of setting up a household upon marriage, lower housing prices, due for example to increased housing supply, should raise marriage rates and speed transitions into marriage.

A potential concern in estimating the effect of permits per capita on marriage is that new building permits may be a response to marriage. Builders may observe marriage rates rising and build homes to accommodate the rush of new couples. Thus, a positive correlation between building permits and marriage outcomes in the cross section may be driven by construction spurred by new marriages rather than the effect of new construction on marriage. I employ a fixed effects specification that should mitigate the bias from reverse causality because in this specification marriage outcomes in a two-year period are paired with permits per capita in the previous two-year period. However, if builders are forward looking, the bias from reverse causality will still be present in this specification.

A second potential concern is that omitted variables may bias the estimates of the effect of permits per capita on marriage outcomes. For instance, a local labor demand shock might drive new construction as builders respond to recent migrants. The same demand shock also might increase entry into marriage because new jobs and/or increased job security provides the economic stability that facilitates marriage. To address the potential bias of the estimated effect of permits per capita on marriage rates caused by omitted variables and reverse causality, I construct an instrument for annual permits. The instrument is built by allotting aggregate annual permits to the sample cities based on two weights that are unrelated to contemporaneous labor demand shocks. The first weight is a geographic weight: cities that are more geographically constricted receive fewer permits than less constricted cities. For example, if a city can only build on half of its land because of geographic features like marshland or steep slopes, it receives a geographic weight of .5. The second weight is a population weight. A population growth weight is the appropriate weight because permits represent the flow of new housing construction, rather than the existing stock. However, population growth may also be correlated with omitted variables, such as local labor demand shocks. Thus, ten-year lagged predicted population growth is used as the weight, where population growth is predicted by region and average annual temperature. Essentially, I use three time-invariant factors (region, geography, and temperature) unrelated to local labor demand shocks to generate an annual instrument that exploits national variation in housing demand.

This paper contributes not only to the baby boom literature but also to the emerging literature on the effect of housing supply. Recent work suggests several cities have increasingly restricted their housing supply through regulation.[1] The immediate effect is that the cost of housing has risen dramatically in these areas over the past 20 years. An artificially restricted housing supply has indirect effects as well: Glaeser, Gyourko and Saiz (2008) show restricted supply leads to more volatile prices; also higher housing costs may drive away firms that are unwilling to pay higher wages. This paper shows that restricting housing supply will lead to lower marriage rates and higher ages at first marriage. Cities with more housing regulations already have lower rates of in-migration; my results suggest that these cities will also have lower levels of fertility as well.

The paper proceeds as follows: Section 2 provides background on the expansion of marriage in the period and the state of the postwar housing markets. Section 3 outlines the conceptual framework for the effect of housing costs on marriage rates. Section 4 details the datasets used in the empirical estimation. Section 5 presents the instrumental variable analysis used in the paper and the results of the estimation. Section 6 discusses the magnitude of the estimates and the role they played in the baby boom. Section 7 concludes.

2.2 Background on the 1950s Baby Boom and Housing Market

2.2.1 Marriage and the Baby Boom

From 1930 to 1950, marriage rates increased by 25 percent and the average age at first marriage for both men and women fell by two years. These marriage trends have significant fertility implications because married women have higher birth rates than single women. The difference in birth rates between single and married women is even more pronounced in the baby boom period because single motherhood was a rare event.[2] Furthermore, contraception was less reliable in the 1950s than the present.[3] Thus, when women married younger, they were likely to have more children because they faced a higher risk of pregnancy for more years.

How large a role did the marriage trends of the period play in the baby boom? To connect the expansion of marriage to increased birth rates more formally, I present a decomposition of the change in the crude

[1] See Glaeser, Gyourko, and Saks (2005), Saiz (2010), and Glaeser and Ward (2009)

[2] The birth rate for single women was 6 times higher in 1998 than it was in 1950. (Carter et al. 2006)

[3] Oral contraceptives were not introduced until 1960.

birth rates from 1930 to 1950. This decomposition shows the share of the aggregate increase in crude birth rates that can be explained by the changes in marriage patterns. I decompose the difference in crude birth rates as follows:

$$cbr_{50} - cbr_{30} = \sum_{am=54}^{AM} (cbr_{am50} - cbr_{am30})\rho_{am50} + \sum_{am=54}^{AM} (\rho_{am50} - \rho_{am30})cbr_{am30}$$

where am indexes each distinct demographic group, and groups are defined by age and marital status. Ages 14 through 40 are included in the decomposition, yielding 54 total groups. The variable, ρ_{amy}, is the share of individuals in demographic group am in year y. The first term gives the difference in crude birth rates attributable to rising birth rates within groups. The second term measures the difference in crude birth rates due to a shift in the proportions of the population in each demographic group, i.e., the difference in crude birth rates due to increases in the proportions married at given ages. The total difference in crude birth rates between 1930 and 1950 is 35 births per 1000 women aged 14-40. The calculation yields a value of 25 for the first term and 10 for the second term. Thus, marriage patterns alone account for 29 percent of the difference in crude birth rates between 1930 and 1950. If the calculation is done for 1960, marriage patterns account for about 25 percent of the difference in crude birth rates between 1930 and 1960.

Very few papers tackle the cause of the marriage boom in the postwar period. Doepke, Hazan, and Maoz (2007) argue that young women after World War II faced stiff labor force competition from both older women who had been employed during the war effort and returning male veterans. The authors posit that with few job opportunities, young women were then driven to marriage. This hypothesis is inconsistent with two facts. Firstly, Albanesi and Olivetti (2009) show that the labor force participation of mothers actually rose in the period. If women were driven to marriage because of lack of jobs, why did their labor force participation increase? Secondly, the Doepke, Hazan and Maoz hypothesis suggests that young women should face stiffer labor force competition in states with higher war mobilization rates and be driven to marry earlier than in states with lower war mobilization rates. However, states with the highest rates of war mobilization had the highest ages of marriages for the cohort of women most affected by labor force competition after World War II.[4] Other papers not directly related to the marriage boom offer clues as to why marriage increased in the postwar period. Hill (2011) finds evidence that marriage rates were sensitive to GDP prior to 1960, and Loughran (2002) shows that women's propensity to marry increases when the wage distribution of men is more equal. The 1950s was both an era of prosperity and the period in U.S. history when the wage distribution was most equal. These two factors likely played a role in the increase of marriage rates after World War II; however, there may be other trends that also positively influenced marriage rates, such as the

[4] I define the cohort most affected to be women who were in high school during World War II and therefore too young to have worked during World War II.

growth in the housing supply.

2.2.2 Housing and Building Permits

In 1945, on the eve of the baby boom, the U.S. faced a housing shortage. A report to Congress estimated that the country needed 3,000,000 homes to accommodate the returning veterans and their prospective families.[5] The Great Depression and World War II had dampened construction for the previous 15 years. A Senate report scolded, "The lack of decent housing within economic reach of all American families may once have been a national scandal. It is now a national tragedy."[6] The U.S. government was concerned enough about the dearth of housing that it formed a new agency, "The Office of the Housing Expediter," to help handle the crisis.

On the housing demand side, the number of people who could afford to purchase a home after World War II increased. In 1944, Congress passed the G.I. Bill, officially the Servicemen's Readjustment Act, which included several provisions specifically related to housing. Through the Veterans Administration mortgage insurance program, veterans were granted access to home loans with low interest rates and very low down payments.[7] The housing provisions had widespread uptake; from 1944 to 1952, 2.4 million veterans received a federally insured home loan and about 35 percent of all home mortgages were federally insured. Fetter (2010) estimates that 70 percent of the increase in homeownership from 1940 to 1960 among 26 year-olds and 32 percent of the increase for 32 year-olds can be attributed to G.I. Bill benefits. The G.I. Bill's influence went beyond guaranteeing loans; it changed the features of mortgages by proving to private lenders that loans with extended terms and low down payments were economically feasible. By 1947, home loans had a very different structure than the home loans of twenty years prior. Compared to 1920, the average loan in 1947 was almost twice as long, had a 30 percent lower interest rate, and a 30 percent smaller down payment. These changes in home loans can be thought of as an advancement in mortgage technology that lowered the overall cost of housing. The types of mortgages propagated after World War II are especially relevant in the context of the baby boom because home loans with less stringent savings and credit constraints enabled couples to transition into marriage at earlier ages.

I argue that permits are a good proxy for the cost of housing. Permits per capita will capture the differences in supply elasticity between cities. In the postwar period, demand for housing was shifting upward as all cities increased in size and the GI provisions lowered the cost of housing for buyers. Builders responded to this demand shift. Large scale developers, like Levitt, constructed whole new cities and

[5] Congress, House, Committee of the Whole, Director of Housing Stabilization. 79th Cong., 2nd Sess. Report No. 1580.

[6] Congress, Senate, Committee on Banking and Currency, Veterans' Emergency Housing Act of 1946. 79th Cong., 2nd Sess. Report No. 1130.

[7] see Green and Wachter (2005)

enterprising manufacturers, like Lustron, provided mail order pre-fabricated houses. Figure 1, a plot of building permits per capita from 1933 to 1958, shows new permits per capita increased six fold from 1945 to 1950. However, cities varied in their ability to respond to the increased demand for housing. Places with ample land and/or the presence of entrepreneurial developers would have been able to rapidly increase their housing stock, while more constricted, low supply elasticity cities would have been slower to respond. In these more constricted cities, home prices would have been higher, as sellers could extract a premium from buyers. Table 1, a regression of home prices on permits, shows that this was the case. There is a strong negative correlation between new housing permits from 1944-1949 and home prices in 1950. A 60 percent increase in permits per capita (the standard deviation between cities) is associated with a six percent decrease in prices.

2.3 Theoretical Framework

In this section, I present a simple model to clarify the relationship between marriage and housing. Consider a three-period model representing a woman's early, mid, and late twenties. Women are assumed to be similar, thus assortative mating is not a component of the model as it is in Burdett and Coles (1997) and Bloch and Ryder (2000). This is done because I am not interested in who marries who rather I am concerned about the timing of marriage. This decision is also informed by the historical context: there was much less assortative mating in the 1950s than the present (Schwartz and Mare 2005).[8] In each period in which a women is single, she receives a draw from a distribution of potential mates. If she accepts the draw she remains married to the mate for all periods thereafter, i.e., there is no divorce. The goal of the model is to find an expression for the reservation value in period t (r_t) above which she will accept her mate draw, and to examine the effects of housing costs on r_t. If she rejects the mate, a woman receives zero utility for that period. If she accepts the mate, she gets utility equal to $(1-\phi(t))x_t$.[9] Where x_t is the value of her draw from period t and $\phi(t)$ represents the cost of housing. Let $\phi(t)$ be decreasing in t, reflecting the fact that housing is less costly at older ages because credit is more readily available and older men have more savings. For simplicity, I assume $\phi(1)=c$, $\phi(2)=\frac{c}{2}$, $\phi(3)=0$. Intuitively, women weigh the trade-off between accepting a mate in the current period and holding out for a better option. Housing costs act as a tax on current consumption so women are more likely to wait when costs are high. It is straightforward to show the reservation value in period 3, r_3, is zero. In period 1, if the woman accepts her draw, her utility is:

[8] Also the late 1940s had a much lower skill premium than the present (Juhn 1999) and Fernandez, et al. (2005) show assortative mating decreases when the skill premium falls.

[9] In the simulations I will assume the distribution of males is normal, with mean 0 and variance 1. This is done for ease of computation. The implications of the model are valid for any distribution of males.

$$U^{Accept} = (1-c)x_1 + \beta(1-\frac{c}{2})x_1 + \beta^2 x_1$$

If she declines, her utility will be:

$$U^{Decline} = 0 + Pr(x_2 > r_2)\left(\beta(1-\frac{c}{2})E[x_2|x_2 > r_2] + \beta^2 E[x_2|x_2 > r_2]\right) +$$

$$Pr(x_2 < r_2)\left(\beta^2 Pr(x_3 > 0)E[x_3|x_3 > 0]\right)$$

If she declines in the first period, she will accept in period 2 if

$$\left(x_2(1-\frac{c}{2}) + \beta x_2\right) > (\beta Pr(x_3 > 0)E[x_3|x_3 > 0])$$

From the above equations, one can show:

$$r_1 = \left(\frac{1}{1-c+\beta(1-\frac{c}{2})+\beta^2}\right)(Pr(x_2 > r_2)\left(\beta(1-\frac{c}{2})E[x_2|x_2 > r_2] + \beta^2 E[x_2|x_2 > r_2]\right)$$

$$+Pr(x_2 < r_2)\left(\beta^2 Pr(x_3 > 0)E[x_3|x_3 > 0)\right)$$

$$r_2 = \left(\frac{\beta}{\beta+1-\frac{c}{2}}\right) Pr(x_3 > 0)E\left[x_3|x_3 > 0\right]$$

And

$$\frac{dr_1}{dc} > \frac{dr_2}{dc} > \frac{dr_3}{dc} = 0$$

Suggesting that as housing costs (c) rise, so does the reservation value in periods 1 and 2. Figure 2, a simulation of the model, illustrates the difference in marriage rates under different housing cost regimes. When housing costs are low (c=.1), 37 percent of women will marry in period 1 and by period 2, 64 percent of women will have married. When housing costs are high, 31 percent of women will marry in period 1 and 59 percent of women will have married by period 2.

The model has two other key implications. Housing costs affect the reservation wage more in earlier periods, implying that the estimated effect of housing on marriage should be larger for younger age cohorts than for older age cohorts. A second implication is that housing costs should have a larger impact on men with lower x values. In figure 2, the difference in marriage rates between the low housing cost regime and the high housing cost regime is driven by the willingness of women in the low cost regime to accept a man with a lower x_1 value. Men from the higher end of the distribution are unaffected by housing costs; they marry under both regimes. Empirically, one should observe that housing costs have larger effects for men on the lower end of the distribution.

2.4 Data

Information on marriage outcomes is available at both the individual and county level. The individual-level data I use is from the 1950 census. This dataset contains an individual's current marital status and the duration of that status. From this marriage data, I can ascertain whether an individual was single or married in a given year and therefore construct a retrospective marriage history for each individual. The census data also includes an individual's current city and whether the person lived in this city one year prior. The location information allows me to merge individual data with data on the relevant city from other sources. Additionally, the census has background characteristics, including race, education, age, labor-force status, and veteran status. A drawback of the census data is that there may be some migration bias introduced because I assume an individual lived in the same location from 1946 to 1950. However, in the empirical section I report estimates for a sample with less migration bias and show that my results do not change significantly.

After 1950, I need to rely on county-level marriage data because the 1960 Census microdata do not report an individual's location below the level of the state. I use marriage licenses issued in a county as the marriage outcome for the second half of the baby boom period: 1954-1962. This data was available for 66 of the most populous U.S. counties from the U.S. National Office of Vital Statistics' Monthly Vital Statistics Reports.

Building permit data come from the Bureau of Labor Statistics (BLS). Beginning in 1920, the BLS began collecting data on new permits issued in urban areas. The dataset is at the city-level and only includes buildings undertaken within a city's limits. However, new permits within a city are highly correlated with new permits in the surrounding areas (correlation coefficient of 0.74 for the years when data were available, 1962-64). In 1954, the permit data series was taken over by the Census Bureau. The Census Bureau changed the level of observation from the city-level to the county-level by collecting data on towns and unincorporated areas surrounding a central city. I digitized the city-level permit data from 1921 to 1954 and the county level permit data from 1954 to 1964 for use in this paper.

Data on other city and county characteristics are from the 1944, 1948, and 1952 city data books and the

1956 and 1962 county data books. This dataset includes other economic characteristics that might influence the marriage decision, including employed persons and retail sales per capita (a proxy for local GDP). I merge the city data books with the individual-level data from the 1950 census. I chose city-level data to be consistent with the permit data from this time period. I merge the county data books with the post-1954 marriage license and building permit data, which are both also observed at the county-level.

2.5 The Effect of Housing Supply on Marriage

2.5.1 Empirical Design

I examine the effect of the number of new building permits per capita in an individual's city on their marital status in 1950. I estimate a linear probability model for a cross section in 1950.[10] The estimating equation is:

$$Marriage_{ij} = \alpha + \beta \cdot PerCapPermits_j + y_j^{'}\phi + x_i^{'}\delta + \varepsilon_{ij} \tag{2.1}$$

where $Marriage_{ij}$ is a dummy variable equal to 1 if individual i in city j married in the previous three years; it equals 0 if the individual was single in 1947 and remained single through 1950. $PerCapPermits_j$ is the per capita permits in city j from 1947 to 1949, y_j is a vector of city controls including employment, sex ratio and retail sales, and x_i is a vector of individual controls including age, age squared and an education dummy equal to 1 if the individual has 12 or more years of schooling. The sample is composed of white males age 20-27 and white females age 18-25 who were single in 1947. I limit the sample to these particular age groups because they are the prime marriage ages for the respective sexes in the period, by age 25, 78 percent of women were married and by age 27, 72 percent of men were married. The results of the estimation are robust to altering the sample to include other ages around the prime marriage years.

I also employ a city fixed effects specification that has two advantages over the 1950 cross section. Firstly, it controls for time-invariant factors that may influence both marriage outcomes and building permits that are not accounted for by the city controls in the cross-section specification. For instance, politically conservative cities may have more construction due to a more permissive regulatory environment, and they may also have a culture of early marriage due to widely held religious beliefs. Secondly, in this specification I relate contemporary marriage outcomes to lagged permits because there is an inherit lag in the permitting-to-construction process. It takes about one year for a permit to become a house. The use of lags also mitigates issues with reverse causality that stem from construction induced by rising marriage rates. In the fixed-

[10]Results are robust to using a probit or logit specification.

effects specification, the dependent variable is a dummy equal to 1 if the individual married in a two-year period. I use two-year time periods to mitigate measurement error introduced by calculating a marriage year from the census variable: "duration of current marital status." As in the cross section, I limit the sample to individuals who were white, single and age 20-27 (18-25 for females) at the start of each period. The estimating equation is:

$$Marriage_{ijt} = \alpha + \gamma_j + \chi_t + \beta \cdot PerCapPermits_{jt} + x_i^{'}\delta + \epsilon_{ijt} \tag{2.2}$$

where $Marriage_{ijt}$ is a dummy variable equal to 1 if individual i in city j married during period t. $PerCapPermits_{jt}$ is per capita permits from the previous two-year period, for instance if the marriage period is 1948-1949, then permits from 1946-1947 are used. The term, γ_j is a city fixed effect and χ_t is a period fixed effect.

For the second half of the baby boom period when individual data is not available, I perform the empirical analysis at the county level. The estimating equation is:

$$PerML_{jt} = \alpha + \chi_j + \beta \cdot PerCapBP_{jt} + \varepsilon_{jt} \tag{2.3}$$

$PerML_{jt}$ is the marriage licenses per capita in county j in year t, $PerCapBP_{jt}$ is the building permits per capita in county j in year t, and χ_j is a county fixed effect. Here the sample is 66 counties where the data are available from the year 1954 to 1962.

2.5.2 Instrumenting for Building Permits

Estimates of the effect of building permits on marriage outcomes may be biased by three potential sources: reverse causality, omitted variables, and measurement error. As discussed in 5.1, the fixed effect specification addresses the issue of reverse causality. However, estimates from the fixed effect specification may still suffer from omitted variable bias. The city fixed effect will control for the long run economic prosperity of cities but will not account for the presence of a one-period labor demand shock. For instance, the construction of a new factory could influence both marriage outcomes and new building. Finally, measurement error may be present in the building permit data. The Bureau of Labor Statistics only collected permits within a city's limits. Therefore, building outside a city is not included in the data.

To address the potential biases in the estimates, I generate an instrument called predicted permits. The instrument is constructed by allocating aggregate permits to the sample cities based on geographical conditions unrelated to contemporaneous demand shocks. Essentially, I build an annual instrument from a city's time-invariant factors by using variation in the national permit series. The national permits are assigned to cities based a on a city-specific weight. The first component of the weight is a city's physical geographical constraints. Cities vary in their endowment of developable land. A city surrounded by water, like San Francisco, has much less developable land than a city surrounded by flat plains, like Houston. Saiz (2010) built an index of developable land for major U.S. cities. His index is constructed in the following manner: a 50 km circle is drawn around the city center. Then, he calculates what percentage of this circle cannot be built upon. Undevelopable land for construction includes oceans, wetlands, lakes, and steep slopes.[11] Saiz's index shows a wide variation of developable land between cities. The most constrained city is Ventura, CA, with only 20 percent developable land, while the least constrained city (McAllen, TX) has over 99 percent developable land. The amount of developable land will be an exogenous component of a city's housing supply elasticity.[12] Cities with a high percentage of developable land can better respond to demand shocks than cities with low percentages of developable land. Differences in geography should not affect marriage in any plausible way other than through housing. The presence or absence of lakes, oceans, and mountains should not directly induce or dissuade couples from marriage.

The second weight is a population-based weight. A population growth weight is the appropriate weight because permits represent the flow of new housing construction, rather than the existing stock. Current population growth cannot be used as the weight because it will be correlated with contemporaneous demand shocks. Lagged population growth, while uncorrelated with current demand shocks, is problematic because population growth is highly serially correlated. Therefore, I build a predicted population growth weight based on factors that will be orthogonal to short-term demand shocks. Due to the advent of air conditioning, warmer cities began growing faster than colder cities after 1920. Thus, average annual temperature in conjunction with region dummies can be used to predict population growth. I generate a weight called predicted population growth for each of the j cities that is the share of total population growth that city j will account for as predicted by its region and average annual temperature. I then lag the weight by ten years. For example, permits in 1950 will be assigned based on predicted population growth from 1930 to 1940.

The weights are used to construct an instrument (predicted permits) for annual actual permits by the following equation:

$$\hat{P}_{jt} = (\sum_j P_{jt}) \frac{pgr\hat{o}w_{jt-}}{\sum_j pgrow_{jt-}} dev_j$$

[11] Land with above a 15% slope is classified as steep and difficult to build on.

[12] Saiz (2010) calculates the elasticities and finds constrained cities have lower supply elasticities than less constrained cities.

The first term is the sum of all permits from j cities in year t. The second term is the predicted population growth weight for city j. The final term (dev_j) is the percent of developable land in city j. Figure 3, a plot of predicted permits per capita against actual permits per capita, shows that the generated predicted permits are highly positively correlated with actual permits for the years 1944 to 1947 (the main years used in the empirical analysis).

2.5.3 Results

Table 2, the results of estimating (1), shows that building permits per capita from 1947 to 1949 had a significant effect on an individual's probability of marriage in 1950. The OLS estimates suggest that a standard deviation increase in the per capita permits from 1947-1949 increases the probability an individual married in the previous three years by 11 percent for males and 13 percent for females. Columns (2)-(4) report the IV estimates. When lagged population growth is used as the population weight (column 2) in the construction of the instrument, the IV and OLS estimates are statistically indistinguishable. As discussed in 5.2, lagged population growth might not be an appropriate weight; thus column (3) reports the results for when predicted lagged population growth is used as the population weight. These IV estimates are significantly larger than the OLS estimates: a standard deviation increase in building permits per capita raises the probability of marriage over a three-year period by 24 percent for men and 21 percent for women. The difference in magnitude between the OLS estimates (column 1) and the IV estimates (column 3) suggests that measurement error was a larger bias in the estimated effect of building permits than omitted variables. The sample used to estimate equation (1) is a cross section, therefore, I can use the time-invariant weights (geography and lagged predicted population growth) from the construction of the annual instrument as separate instruments. The advantage of using multiple instruments is that the first stage is no longer exactly identified and I can test for the endogeneity of the instruments with over-identification tests. Column (4) reports the results of using geography interacted with region as one instrument and lagged predicted population growth as a second instrument. The estimated effect of permits on marriage in column (4) is not significantly different from column (3) and the instruments pass the over-identification test, which suggests that the instruments are valid.

Table 3, the results of estimating (2), shows that the estimates of the effect of building permits per capita on marriage probabilities are similar between the cross section and fixed effects specifications. In the fixed effects model, the OLS estimates suggest that a standard deviation increase in permits per capita in the previous two years raises the probability of marriage by 7 percent for men and 8 percent for women over a two-year period. As in the cross section, the difference between the OLS and IV estimates with lagged population growth as the weight are not significantly different. When lagged predicted population growth is used as the weight in the construction of the IV, the estimated effect of permits per capita on marriage outcomes doubles. The estimates in column (3) and (6) suggest that a standard deviation increase in permits raises the probability of marriage by 13 percent for men and 16 percent for women over a two-year period. This effect is quantitatively similar to the 24 and 21 percent effect observed over a three-year period in the cross-sectional results. The similar effect of permits per capita on marriage outcomes between the fixed effects and the cross-section specifications suggest reverse causality was not a significant bias in the estimates from table 2.

After 1950, I perform the empirical analysis with county level data. The county level data has some drawbacks compared to the individual level data. The number of observations in these data is smaller than the census, and the variable of interest (marriage licenses) is measured with more error. People may often file for marriage in a different location than where they reside. Therefore, the marriage license counts may not be a perfect measure of marriage in a given area.[13] However, measurement error in building permits is less of an issue in the county sample because the permit series sampling method improves after 1954. As in the individual level data, building permits positively influence marriage. Table 4 shows the results of estimating equation 3. At the county level the size of the effect of permits on marriage is larger than at at the individual level. The OLS estimates (column 1) suggest that a standard deviation increase in permits per capita in a given year raised the marriage licenses per capita by 6 percent in the following year. The IV estimates (columns 2 and 3) are larger in magnitude but not statistically different than the OLS estimates, perhaps because there is less measurement error in the permit series.

2.5.3.1 Entire Period (1926-1949)

I can repeat the estimation of equation (2) from 1928 through 1949 because building permit data are available from the mid-1920s. Like the 1950 census, the 1940 census has both marriage and location information. In the 1940 census there is a question on age of marriage and also a question about an individual's current location and the individual's location five years prior. From these two questions, I construct retrospective marriage histories for persons in this census. I combine the building permit data for a given city with the individual marriage data to estimate the effect of permits on marriage outcomes from 1928 to 1949.[14] Table 5, the results of estimating equation (2) on the whole period (World War II excluded), shows that building permits per capita have the predicted positive effect, but the effect is slightly weaker than it was in the post-war period. The estimates from columns (1) and (2) suggest that a standard deviation increase in

[13] However, if the measurement error is mean independent of the explanatory variables, this issue will not bias the results.

[14] I choose 1928 as the cut-off because going further back introduces significant migration bias in the individual data and the permit series is not complete until the late 1920s. However, the results are robust to different cut-off years.

permits raises the probability of marriage in a two-year period by 6.3 percent for men and 5 percent for women. The lower effect is likely due to the fact that young people were less apt to own a home prior to the GI bill and thus were less sensitive to permit fluctuations. Furthermore, the variance in permits per capita between cities was lower in the 1930s as the Depression slowed new construction everywhere.

The instrumental variable approach cannot be used prior to 1940. I use ten-year lagged population growth as predicted by temperature in the construction of the instrument. Because air conditioning technology only began to diffuse after 1920, the first decade for which temperature predicts city growth is 1920 to 1930. Therefore, I cannot construct the instrument prior to 1940. The instrument constructed with simple lagged population growth as the population weight is also problematic. From 1930 to 1945 there is very little new building, and many cities lost population. Recall, the instrument is based on two weights: geography and population growth. If cities are contracting, the geography weight will not predict permits because geography will not be a check on construction when so little is being undertaken. Figure 4, a plot of the correlation between actual and predicted permits by year, shows that the instrument is not valid for the entire period. The instrument is only correlated with actual permits when the economy is expanding in the late 1930s and late 1940s. However, the IV estimates from the postwar period were always equal to or larger than the OLS estimates. Thus, while I cannot use the IV analysis for the full period, it is reasonable to assume that the OLS estimates are a lower bound for the effect of permits per capita on marriage outcomes from 1928 to 1949 and that potential biases are not causing the observed positive relationship between housing supply and marriage.

2.5.3.2 Effects on Different Demographic Groups

The conceptual framework in Section 3 has clear implications about the effects of housing on different demographic groups. Firstly, housing costs should be more important at younger ages because older men are more likely to have built up savings and would be less sensitive to fluctuations in the housing market. In table 6, panel A, columns 1 and 2 show the results of the estimation of equation (1) on younger men (age 20-27), and columns 3 and 4 show the effect for older men (28-35). The OLS estimate of the effect of permits on marriage is 40 percent smaller for older men, and the IV estimate is 25 percent smaller. While the estimates are not statistically different, they suggest that housing cost matters less for older men.

In the simulation of the model, the difference in marriage rates between low housing cost cities and high housing cost cities is driven by the ability of men from the lower end of the distribution to marry in low housing cost cities. Thus, men from the lower end of the distribution should be more affected by housing costs than men at the higher end of the distribution. Intuitively, a high quality man should be able to obtain housing regardless of which housing cost regime he is in, while a low quality man can obtain housing only when costs are low. One dimension of quality is education. Table 6, Panel B, reports the estimated effect of building permits per capita on men with a high school education versus men with less than a high school education. As predicted by theory, the effect of permits per capita on marriage rates for low education men is around 50 percent higher than the effect for high education men.

The model also predicts that people should marry younger in low housing cost regimes. Table 6, Panel C, shows the effect of building permits on men and women's age at marriage. Building permits per capita negatively impact the age of marriage for both men and women, although the OLS estimate for women is not statistically significant. The IV estimates suggest a standard deviation increase in permits per capita lowers the age of marriage for men by 4 months and for women by 6 months.

Another interesting group is African-Americans. In the 1950s, African-Americans had to wait for whites to filter out of new housing before they could purchase homes (Boustan and Margo, 2011). Therefore, the effect of new permits should be muted for blacks in this time period. Table 6, Panel D, shows the estimates of equation 1 for whites and blacks. The OLS estimates are 40 percent smaller for blacks versus whites, and the IV estimates suggest that permits per capita had no effect on black marriage outcomes.

2.5.3.3 Prices

Home prices and building permits have a similar effect on the probability of marriage in 1950. In 2.2, I argue that building permits are a good proxy for the cost of housing in a given city. A more direct proxy for the cost of housing is the median home price within a city. However, while permit data is available annually, home price data is only available for the year 1950. Thus, home prices can only be used in the cross-section specification. Table 7, the results of estimating equation (1) with median home price (in thousands) in 1950 as the variable of interest instead of permits per capita, shows that home prices had a comparable effect to that of permits per capita. The OLS estimates imply that a standard deviation decrease in the median home price raises the probability of marriage by 10 percent for women and 14 percent for men. The IV estimates are about double the magnitude of the OLS estimates: a standard deviation decrease in the median home price raises the probability of marriage by 20 percent for women and 21 percent for men. The fact that a standard deviation change in permits per capita has virtually the same effect on marriage that a standard deviation change in home prices suggests that permits per capita are a valid proxy for the cost of housing.

2.5.4 Migration

Individual migration is a potential bias in the estimated effect of building permits per capita on marriage outcomes. The census contains the location of persons in 1950 and 1949. In the empirical analysis, I assume that a person was located in the city reported in 1949 for the years 1946 to 1948. If individuals marry and then move to cities with higher building permits per capita, I will be over-estimating the effect of permits on marriage. Given that high growth cities (the cities with high in-migration) have over double the permits per capita of lower growth cities, the potential bias is indeed positive.[15] However, the majority (66 percent) of persons age 17-27 who moved to a different county from 1949 to 1950 were single. Therefore, it is much more likely that people move and then marry than marry and then move. As a robustness check, I can estimate equation (1) for the years where location of persons is fully known (1949-1950); this sub-sample should have minimal migration bias. Table 8 shows the results of this estimation. The effects of permits per capita on marriage are larger in this sub-sample than they are in table 2, which suggests that migration bias is not driving the main results of the paper. The OLS estimated effect of a standard deviation increase in permits per capita on the probability of marriage is 13 percent for men and 21 percent for women, while the IV estimated effect is 28 percent for men and 33 percent for women.

2.6 Discussion

In this section, I use my estimates of the effect of housing on marriage to show that growth in the supply of housing can explain nearly 10 percent of the baby boom. Around 750,000 more homes were built between 1947 and 1949 than were built between 1927 and 1929. An increase of this magnitude corresponds to 0.5 more new permits per one hundred people. My estimates suggest that an additional 0.5 permits per hundred persons would raise the marriage probability for women age 18-25 by 4 percentage points and for women age 26-30 by 2 percentage points. Columns (1) and (2) of table 9 report the proportion of women married by age in 1930 and 1950, respectively. Column (3) is my estimate of the proportion of women who would be married in 1930 after the influx of 750,000 new homes. Column (4) is the percentage of the difference between the 1930 marriage rate and 1950 marriage rate accounted for by the housing increase. On average, the new homes can account for about 32 percent of the difference in the proportion married between 1930 and 1950 for women age 20 to 32. According to the demographic exercise I performed in section 2, changes in marriage patterns are responsible for 29 percent of the changes in crude birth rates from 1930 to 1950. These two estimates taken together suggest that about 9.6 percent of the baby boom can be accounted for by growth in the housing supply after World War II.

Is the above thought experiment realistic? The number of permits from 1927 to 1929 was an equilibrium outcome. If 750,000 new homes were built would they be bought/rented? The key difference between the late 1920s and 1940s was that there was an exogenous innovation in mortgage technology that shifted demand

[15] I define high growth cities as cities with 1940 to 1950 population growth over the mean and low growth as cities with growth under the mean.

upward. Federal involvement in the home mortgage sector, originally through the Federal Housing Authority in the late 1930s and then perpetuated by the G.I. Bill after World War II, reshaped home loans. The average home loan in 1947, when compared to 1927, had a longer term, lower down payment and lower fixed interest rate. The G.I. Bill gave a segment of the population a power they did not have before: the ability to buy homes. Fetter (2010) estimates that the G.I. Bill raised home ownership rates for 26 year-olds by 70 percent and for 32 year-olds by 32 percent. To accommodate these increases, over a million new homes would have to have been built. Thus, it is feasible that these 750,000 new homes from 1947 to 1949 were built to serve a portion of the population that had not been able to purchase homes prior to the 1944 passing of the G.I. Bill.

2.7 Conclusion

This paper estimates the effect of housing supply on marriage outcomes during the U.S. baby boom. Specifically, I use permits per capita as a proxy for housing supply and find that a standard deviation increase in permits per capita from 1947 to 1949 increased the probability of marriage by 21-24 percent in that three-year period. My estimates imply that increases in housing supply can explain 32 percent of the difference in marriages rates between 1930 and 1950. The remaining difference can likely be attributed to the strong postwar U.S. economy. In an earlier paper on marriage and the Great Depression, I estimated that a standard deviation increase in GDP raised the probability of marriage by 24 percent over a three-year period (Hill, 2011). Thus, GDP growth and increased housing supply can explain much of the remarkable rise in marriage rates between 1930 and 1950.

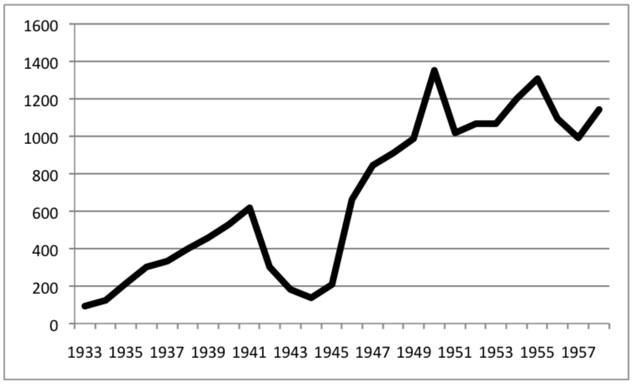
I find that growth in the supply of housing can account for 10 percent of the rise in birth rates between 1930 and 1950 through the channel of increased marriage rates. This result may explain why the baby boom experienced by the U.S. was larger than similar postwar baby booms in other western countries. Most developed countries experienced a baby boom after World War II, but the baby boom was largest in the U.S., Canada, New Zealand and Australia. The urban areas in these four countries had been spared the destruction experienced by European cities and began the post-war period with a largely intact housing stock. Returning soldiers and rising GDPs can probably explain the majority of the baby boom in developed countries. However, increased housing supply may explain why the baby boom was larger in countries with open land and less affected housing stock.

My estimates of the effect of increased housing supply on the baby boom are most likely an underestimate because the growth of housing supply could also account for increased fertility within marriage. For instance, areas with more housing typically have larger dwellings which might enable couples to have more children.

Declines in maternal mortality and rising income are among the factors that could explain the additional portion of the baby boom unaccounted for by housing.[16] In future work I will examine the Easterlin hypothesis that cohorts raised during the hardship of the Great Depression preferred to spend their disposable income on children rather than on consumption goods.

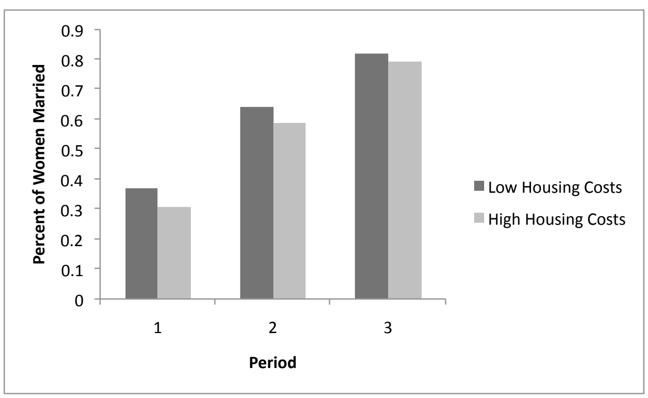
[16]See Albanesi and Olivetti (2010) and Easterlin (1962).

Figure 1 - Building Permits (1933-1958)



source: Bureau of Labor Statistics

Figure 2 - Housing Costs and Marriage Rates



Notes: the distribution of mates is assumed to be $\sim N(0,1)$ and $\beta = .9$, $c = .1$ (low cost) and $c = .9$ (high cost)

Figure 3 - Actual vs. Predicted Permits (1944-1947)

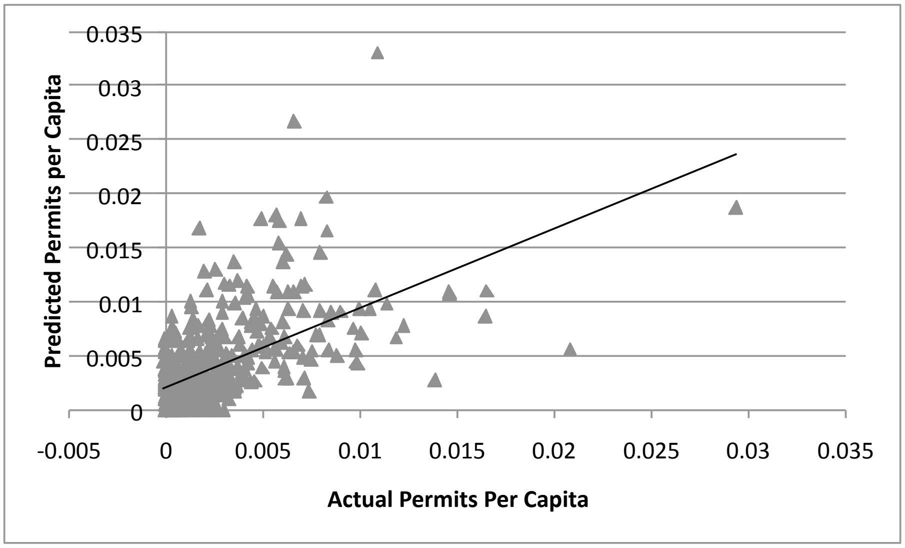
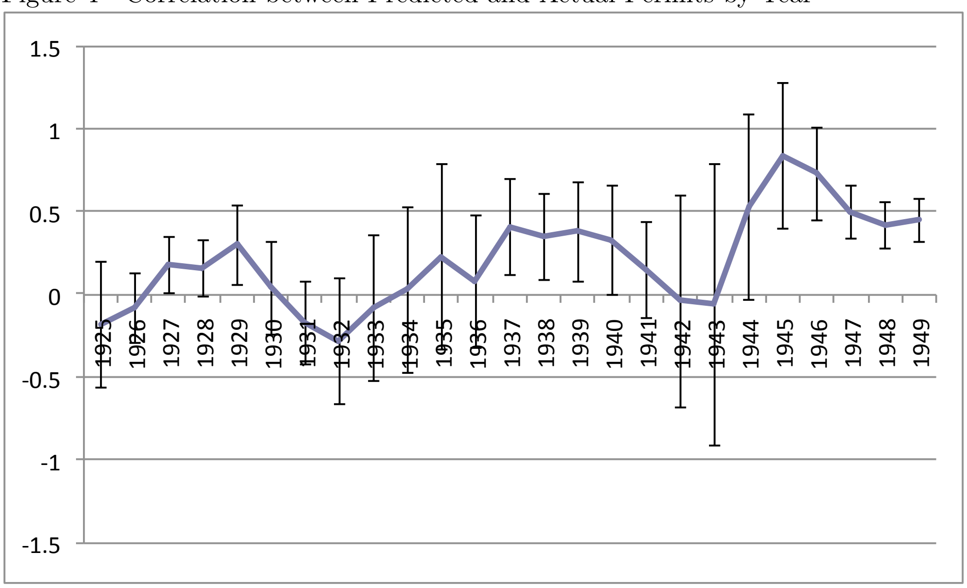


Figure 4 - Correlation between Predicted and Actual Permits by Year



notes: error bars are a 95% confidence interval

Table I - Correlation between Permits and Prices (1950)

	Dep. Variable = log Median Home Price in 1950
log Blding Permits per hundred 1944-1949	-0.092**
	(.035)
Median Income 1950	0.0003***
	(.00007)
Employment Rate	0.649
	(.743)
r-squared	0.26
obs	82

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Other controls included are housing stock per capita (1940) and average rooms per dwelling.

Table II -Effect of Building Permits on Marriage (1950 cross section)

Panel A: Females				
	Dependent: =1 if married			
	(1)	(2)	(3)	(4)
	OLS	IV(lag)	IV(plag)	IV(2 instr)
Building Permits per hundred (47-49)	0.048***	0.058**	0.093***	0.077***
	(.013)	(.024)	(.023)	(.015)
Prcnt Change in Retail Sales (40-50)	0.248***	0.256***	0.284***	0.270***
	(.051)	(.050)	(.055)	(.053)
Prcnt Change in Employment (40-50)	-0.176	-0.040	0.459	0.224
	(.346)	(.463)	(.442)	(.390)
Dummy=1 if in Labor Force	-0.343***	-0.343***	-0.340***	-0.341***
	(.017)	(.017)	(.017)	(.017)
r-squared	0.25			
obs	5247	5247	5247	5247
Mean Dep. Var	0.45	0.45	0.45	0.45
Weak Instr. F Test		20.7	10.9	12
p(Over-Id Test)				0.688
Sample	Age 18-25, single/ married in last 3 yrs			

Panel B: Males				
	Dependent: =1 if married			
	(1)	(2)	(3)	(4)
	OLS	IV(lag)	IV(plag)	IV(2 instr)
Building Permits per hundred (47-49)	0.040***	0.039**	0.092***	0.072***
	(.014)	(.018)	(.025)	(.016)
Prcnt Change in Retail Sales (40-50)	0.219***	0.218***	0.257***	0.242***
	(.065)	(.065)	(.071)	(.067)
Prcnt Change in Employment (40-50)	-0.195	-0.211	0.530	0.249
	(.359)	(.441)	(.581)	(.382)
Dummy=1 if Veteran	0.136***	0.136***	0.137***	0.137***
	(.016)	(.016)	(.016)	(.016)
Dummy=1 if in Labor Force	0.226***	0.226***	0.227***	0.227***
	(.019)	(.019)	(.019)	(.019)
r-squared	0.15			
obs	5027	5027	5027	5027
Mean Dep. Var	0.41	0.41	0.41	0.41
Weak Instr. F Test		22.9	11.3	10.5
p(Over-Id Test)				0.363
Sample	Age 20-27, single/ married in last 3 yrs			

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (1) in the text. Non-whites are excluded. Other controls included are housing stock per capita (1940), sex ratio, education dummy, and foreign-born dummy. Standard errors are clustered by city.

Table III - Effect of Building Permits on Marriage 1946-1949 (Fixed Effects)

	Dep. Variable=1 if Married					
	Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV(lag)	IV(plag)	OLS	IV(lag)	IV(plag)
Building Permits per hundred	0.049***	0.069***	0.090***	0.062***	0.053**	0.119***
	(.013)	(.018)	(.026)	(.017)	(.022)	(.031)
r-squared	0.15			0.24		
obs	10395	10395	10395	10514	10514	10514
Mean Dep. Var	0.4	0.4	0.4	0.42	0.42	0.42
F-stat		20.7	32.2		16.5	28.1

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (2) in the text. Non-whites are excluded. Other controls included are a labor force dummy, an education dummy, and a foreign-born dummy. Standard errors are clustered by city and period.

Table IV - Effect of Building Permits on Marriage Licenses 1954-1962

	Dep. Var= Marriage Licenses per Capita		
	OLS	IV(lag)	IV(plag)
Building Permits per Capita prev. year	0.101***	0.156**	0.189***
	(.022)	(.068)	(.073)
F-stat		26.5	22.2
Mean Dep. Var	0.0076	0.0076	0.0076
N	528	528	528

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (4) in the text. Standard errors are clustered by city.

Table V - Effect of Building Permits on Probability of Marriage 1928-1949

	Dep. Var =1 if Married in period	
	Male	Female
	(1)	(2)
Building Permits per hundred people	0.017***	0.016**
	(.006)	(.007)
r-squared	0.1	0.16
obs	46748	49312
Mean Dep. Var	0.22	0.25

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (2) in the text. Non-whites are excluded. Men age 20-27 included, women 18-25 included. Other controls included are a labor force dummy, an education dummy, and foreign-born dummy. Standard errors are clustered by city and period.

Table VI - Effect of Building Permits (by Group)

Panel A: Young vs. Old Men

	Dep Var. = 1 if Married in last 3 yrs.			
	Men Age 20-27		Men Age 28-35	
	(1)	(2)	(3)	(4)
	OLS	IV(plag)	OLS	IV(plag)
Bldg Permits per hundred (47-49)	0.040***	0.092***	0.024*	0.069***
	(.014)	(.025)	(.012)	(.021)
Percent Change in Retail Sales (40-50)	0.219***	0.257***	0.177***	0.202***
	(.065)	(.071)	(.051)	(.048)
r-squared	0.15		0.05	
F-stat		11.3		10.1
obs	5027	5027	2727	2727

Panel B: Low Ed vs. High Ed Males

	Dep Var. = 1 if Married in last 3 yrs.			
	H.S. Ed or Higher		Less than H.S. Ed	
	(1)	(2)	(3)	(4)
	OLS	IV(plag)	OLS	IV(plag)
Bldg Permits per hundred (47-49)	0.038***	0.075***	0.055**	0.117***
	(.011)	(.021)	(.024)	(.039)
Percent Change in Retail Sales (40-50)	0.215***	0.250***	0.213**	0.272***
	(.058)	(.069)	(.099)	(.102)
r-squared	0.16		0.11	
F-stat		11.9		13
obs	3177	3177	1850	1850

Panel C: Age of Marriage

	Dep Var. = Age of Marriage			
	Men, married in last 3 years		Women, married in last 3 years	
	(1)	(2)	(3)	(4)
	OLS	IV(plag)	OLS	IV(plag)
Bldg Permits per hundred (47-49)	-0.163***	-0.228**	-0.133	-0.512***
	(.045)	(.116)	(.093)	(.166)
Percent Change in Retail Sales (40-50)	-0.976***	-1.03***	-0.876**	-1.22***
	(.319)	(.295)	(.399)	(.407)
r-squared	0.04		0.06	
F-stat		11.5		10.7
obs	1778	1778	2031	2031

Panel D: Black vs. White

	Dep Var. = 1 if Married in last 3 yrs.			
	White		Black	
	(1)	(2)	(3)	(4)
	OLS	IV(plag)	OLS	IV(plag)
Bldg Permits per hundred (47-49)	0.041***	0.080***	0.028**	0.002
	(.012)	(.018)	(.012)	(.026)
Percent Change in Retail Sales (40-50)	0.221***	0.247***	0.204***	0.189***
	(.046)	(.048)	(.061)	(.061)
r-squared	0.23		0.15	
F-stat		11.4		5.6
obs	12334	12334	2029	2029

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (1) in the text. Other controls included are housing stock per capita (1940) and sex ratio. Age groups are age 20-27 for males and 18-25 for females unless otherwise noted. Standard errors are clustered by city.

Table VII - Effect of Home Prices on Probability of Marriage (1950)

	Dependent: =1 if married			
	Females		Males	
	(1)	(2)	(3)	(4)
	OLS	IV(plag)	OLS	IV(plag)
Median Home Price	-0.023***	-0.044***	-0.028***	-0.042***
	(.0055)	(.013)	(.0061)	(.012)
Dummy=1 if in Labor Force	-0.343***	-0.343***	0.231***	0.234***
	(.017)	(.017)	(.018)	(.019)
Dummy=1 if Veteran			0.134***	0.133***
			(.016)	(.016)
r-squared	0.25		0.15	
obs	5247	5247	5027	5027
Mean Dep. Var	0.45	0.45	0.41	0.41
Weak Instr. F Test		10.1		10.1

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (1) in the text. Non-whites are excluded. Other controls included are percent change in retail sales (1940-1950), percent change in employment (1940-1950), housing stock per capita (1940), sex ratio, education dummy, and foreign-born dummy. Standard errors are clustered by city.

Table VIII - Effect of Building Permits on Probability of Marriage (1949-1950)

	Dep. Variable=1 if Married			
	Male		Female	
	(1)	(2)	(3)	(4)
	OLS	IV(plag)	OLS	IV(plag)
Building Permits per hundred people (47-49)	0.029**	0.062**	0.051***	0.081***
	(.011)	(.026)	(.015)	(.022)
Percent Change in Retail Sales (40-50)	0.164***	0.188***	0.214***	0.237***
	(.044)	(.053)	(.044)	(.048)
Percent Change in Employment (40-50)	0.045	0.499	0.020	0.424
	(.262)	(.492)	(.379)	(.406)
Dummy=1 if Veteran	0.076***	0.077***		
	(.016)	(.017)		
Dummy=1 if in Labor Force	0.148***	0.150***	-0.250***	-0.247***
	(.014)	(.014)	(.017)	(.018)
r-squared	0.06		0.14	
obs	3878	3878	3930	3930
Mean Dep. Var	0.23	0.23	0.26	0.26
F-stat		10.1		10.7
Sample	Age 20-27, single or married in last 1 yrs		Age 18-25, single/ married in last 1 yrs	

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (1) in the text. Non-whites are excluded. Other controls included are housing stock per capita (1940), education dummy, sex ratio and foreign-born dummy. Standard errors are clustered by city.

Table IX - Proportion of Females Married by Age (30, 50, 30_predict)

	(1)	(2)	(3)	(4)
Age	1930	1950	1930_hat	Percent of difference
20	39.34	46.95	43.34	0.52562418
21	46.59	58.49	50.59	0.33613445
22	54.8	66.68	58.8	0.33670034
23	62.82	74.18	66.82	0.35211268
24	66.98	79.16	70.98	0.32840722
25	72.14	83.5	76.14	0.35211268
26	75.63	85.74	77.63	0.19782394
27	79.26	87.75	81.26	0.23557126
28	81.19	88.85	83.19	0.26109661
29	84.16	91.22	86.16	0.28328612
30	83.21	90.77	85.21	0.26455026
31	87.25	92.89	89.25	0.35460993
32	86.34	92.7	88.34	0.31446541

Notes: Column (3) is the predicted proportion of females married in 1930 if permits were at their 1947-1949 levels. Column (4) is the percent of the difference between (1) and (2) that can be accounted for by the increase in permits.

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Chapter 3

Easterlin Revisited: Relative Income and the Baby Boom

3.1 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 explanation for these fertility increases came from Richard A. Easterlin in 1962. Easterlin hypothesized that the key factor driving these baby booms was the relative income of young people. He suggested that when incomes are higher than material aspirations (formed in childhood), birth rates would rise. In this paper I'll re-assess Easterlin's hypothesis with new state income data that allows me to utilize a large sample of individuals from the U.S. census.

In recent years, several new theories about the baby boom have emerged. Doepke, Hazan, and Moaz (2008) suggest that female labor force competition decreased age of marriage and thus increased birth rates. Greenwood et al. (2005) propose diffusion of household appliances drove the cost of children down. In a 2010 paper, Albanesi and Olivetti emphasize the role that falling maternal mortality can have in short term birth rate increases. And Hill (2012) estimates 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.

Easterlin's hypothesis proposes individuals will develop material aspirations based on their childhood experience. If their adult incomes surpass 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. 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 WWII, detail household income data was not available and Easterlin used male employment to build relative income. This employment-based measure is highly correlated with the 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 imply there is a causal link between the two variables 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 comes down in favor of the hypothesis. According to Macunovich, about two-thirds of the surveyed research supports Easterlin's hypothesis. She surmises methodological differences drives the unsupportive results in the dissenting one third. She stresses Easterlin must be taken on his own terms, a true test of his hypothesis should use age-specific, objective (many studies used subjective interviews about how the interviewee felt about his/her income) measures in construction of Easterlin's relative income variable. Furthermore, she emphasizes that relative income should be used alone and not in conjunction with absolute income due to collinearity issues.

This paper is most similar to two other studies: Lindert (1978) and Maxwell (1991). Lindert also uses state level income and estimates the effect of income twenty years prior divided by current state income on number of children per woman. However, his study only uses state level income at decadal intervals while I'll use annual state income. Maxwell uses individual level data from NLS and examines the effect of relative wages on fertility. However, her study relies on variation between cohorts while I'll estimate the effect of relative income based on variation both between and within a birth cohort.

In this paper I'll add to the existing literature by examining Easterlin's hypothesis with a large individual data set and use variation in annual state incomes during childhood to test the impact of relative income on fertility outcomes. My data allows me to address two main criticisms of other papers that test the Easterlin hypothesis. Firstly, I include birth-year fixed effects to control for unmeasured nation-wide factors that would have influenced all people born in the same year the same way. For instance, a birth cohort raised during economic hardship might mature earlier and thus marry younger, this cohort would have both high

[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 garnish 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 and high birth rates that would produce a non-causal correlation between the two trends in the aggregate time series. Secondly, the census data allows me to link to a person's spouse and include said spouse's childhood state income history. Previous studies did not account for spouse's relative income, a potentially important factor in the fertility decision. The results of my study suggest that an increase in relative income can indeed explain a significant portion of the baby boom, however, this effect diminishes greatly once cohort fixed effects are included and the effect is only present for persons born before 1940.

3.2 Data

My individual level data come from the 1970, 1980, and 1990 censuses. These were the census years where number of children ever born was available. In addition to a large sample size and rich individual controls, the census also contains information on spouses. This allows me to generate a composite household childhood income variable that is an average of a woman's childhood income experience and the childhood income experience of her spouse.

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. 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 postwar 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 it's 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 an individual's state of location over their lifetime. The census has information on a person's state of birth, her spouse's state of birth (if the spouse is present in the household), her children'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 person's state of birth was the state they spent the bulk of their childhood. In assigning an adult state, I chose the mode of a woman's 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

[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.

residence five years prior as the adult state. I classified people into four groups. Group 1 contains people for whom I am confident I have chosen the correct childhood and adult state. Group 2 contains people who moved between childhood and adulthood, but I am still confident that I have correctly placed them in both their childhood and adult state. Group 3 contains people for whom I am only confident about either the adult or child state but not both. Group 4 contains people for whom I am unsure about adult and child state. Table 1 provides more detail for exactly how I made these distinctions.

3.3 Measuring Income Experience

3.3.1 Childhood Income

Central to Easterlin's hypothesis is the role of income within the household during one's childhood. He argues that people form material aspirations based on their childhood experience and that these aspirations will influence later fertility decisions. The hypothesis was somewhat heretical at the time because it emphasized shifting preferences. However, in the context of economic development, it is not unrealistic to believe people in different times 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. And it is reasonable to assume one's baseline level of material attainment is formed during one's childhood experience (although it is a cliché, people do quote prices from childhood quite frequently, e.g. when I was kid X cost X amount). 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 (2008) find that the Great Depression in the U.S. had long run health consequences. And Malmendier and Nagel's (2011) work suggest that persons 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 persons who grew up in a given state. The state level data might be better than individual data because it is less endogenous than household income and contains 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 work done on other types of aspirations. The psychology literature seems to agree that career aspirations form around age 10 to 11, while tastes for popular entertainment form in high school, and preferences for certain brands can develop at young ages.[3] For this paper I demarcated the ages when material aspirations emerged as age 6 to age 12. I began at age six because this is the age that psychologists label children as concrete-operational (Bahn 1986), meaning they can focus on more than one dimension. I ended at 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). This measure is hereafter referred to as RI1.

Other age ranges of childhood may more closely match Easterlin's definition of childhood income experience. 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 people age 45-54 would be the parents of people age 14-24 and that five years prior would cover the time when persons 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. Thus, the drawback of my data are that any breakdown of state income during an individual's lifetime will be the averages of all persons in that state. Therefore, there is no perfect analog for Easterlin's measure in the state data. One option is to try to match Easterlin's age ranges and use income at the state level from age 19-23 relative to income at age 12-16 as the proxy for Easterlin's relative income measure (hereafter referred to as RI2). Another option is to view Easterlin's measure as child's income (as a young adult) divided by parent's income. In this case state income at age 19-23 relative to income at age 0-4 might be the appropriate measure (hereafter referred to as RI3). Here, income at age 0-4 is a proxy for the parent's income during the beginning of the parents work career and perhaps the best proxy for the parent's lifetime earning experience.

In Easterlin's papers, his relative income measure was always highly correlated with total fertility rate in the aggregate time series. Figure 2 shows how well my previously mentioned relative income measures correlate with fertility in the aggregate time series. The plot shows average number of children, RI1, RI2, and RI3 by birth year. Of the three measures, RI3 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, I'd argue based on the psychology literature, RI1 remains the best analog for relative income in theory. The figure also suggests relative income measures are sensitive to their definition, a caveat that should be considered when interpreting results.

[3] See Hartung, Porfeil, Vondracek (2005) and Trice (1991) and Cook, Church, Ajanaku, Shadish, Kim, and Cohen (2008) on career aspirations. See Holbrook and Schindler (1989) for entertainment tastes and Robinson et al for work on children and brand attachment.

3.3.2 Adult Income

The second component of relative income is adult income. In the construction of RI1, RI2, and RI3 I have used income at the state level during early adulthood. The census provides other possible measures of adult income. A first option is total household income. However, total household income is certainly endogenous to completed fertility because of the substitution effect between female wages and fertility. Therefore, a better measure of adult income experience is husband's income. Husband's income is free of the substitution effect but might still suffer from selection bias if women who desire more children seek out high potential earners. Another option is income at the national level. State income during childhood is plausibly exogenous to fertility outcomes because one does not choose one's birthplace. At adult ages, one can choose one's state; therefore, state income may be endogenous to fertility if women who desire more children sort themselves into high-income states. Using national income overcomes the potential endgoeneity of state income. In the results section I will report results for all three measures of adult income: husband, state, and national.

3.4 The Effect of Relative Income on Fertility

3.4.1 Separate Income Measures

To determine the impact of childhood and adult income on children ever born for woman observed in census years 1970, 1980, and 1990, I estimate ordinary least squares (OLS) regressions using the following equation:

$$Chborn_{isbj} = X_{isbj}\alpha + ChInc_{sb}\beta + AdInc_{sb}\lambda_s + \phi_j + \gamma_b + \varepsilon_{isbj} \tag{3.1}$$

where the subscripts refer to woman i, born in year b, currently living in state s, observed in census year j. The vector X_{isbj} contains individual controls for race, education, urban/rural status, and ever divorced. $ChInc_{sb}$ is the appropriate childhood income measure for a woman born in state sb, and $AdInc_{sb}$ is a proxy for adult income experience. Birth-year fixed effects (γ_b) were included to control for unmeasured nation-wide factors that would have influenced all people born in the same year the same way. I also added a vector of current state fixed effects (λ_s) to account for unmeasured factors that varied across state but did not vary across time. This state fixed effect will control for cultural influences that might differ between states, for instance if certain states have stronger religious beliefs that encourage high birth rates. Finally, a census year fixed effect (ϕ_j) is included to account for possible differences in surveying between the census years. I limit the sample to woman with at least one child that are likely to have completed their fertility (i.e. over age 35).[4]

Table 2, column 1, the results of estimating equation (1) with only childhood log per capita income in the state of birth from age 6 to 12 as the variable of interest, shows that childhood income has a persistent negative effect on children ever born. I divide my sample into the migration groups outlined in section 2. Table 2, row 1-4 shows the estimated effect separately for women in migration group 1, group 1,2, groups 1,2, 3, and finally the entire sample. The effect is present for all migration groups; a standard deviation decrease in childhood income raises the number of children ever born by about 0.1.

In Table 2 column 2, I add spousal current income to the estimating equation. The findings are again consistent with the Easterlin hypothesis. With the exception of the sample where migration group 4 is included, childhood income has a consistent negative influence on children ever born and spouse's income positively impacts children ever born. However, these income effects are relatively small, a standard deviation increase in childhood income lowers the number of children born by 0.08, and a standard deviation increase in spousal income raises the number of children born by 0.01.

Table 2, columns 5 and 6, show the results of estimating equation (1) with state log income per capita from age 19-23 used as a proxy for adult income instead of spousal income. Childhood income still has a negative effect on number of children born but contrary to Easterlin's hypothesis adult income also has a negative effect. However, this effect might be incorrectly estimated due to the presence of a high degree of collinearity between childhood state income and adult state income. The correlation coefficient between adult and childhood income is as high as .9 for some birth cohorts. Rows 5 and 6 show the estimated effect of income with samples where childhood income and adult income are less correlated. Row 5 uses a sample where persons have a different adult and childhood state, and row 6 displays the results for persons born after 1922 (collinearity between adult and child state income is highest for those born before 1922). In these two samples the estimates conform more to Easterlin's theory, there is a negative effect of childhood income and a positive, albeit insignificant effect of adult income.

3.4.2 Relative Income

Entering childhood and adult incomes separately is not a direct test of Easterlin's hypothesis. He argued that because relative income is constructed using absolute income, then only one or the other should be used when estimating the effect of income on fertility. Table 3, shows the results of estimating equation (1) when the income proxy is a ratio of adult income to child income. I use four different versions of relative income, the three previously mentioned in section 3 (average log per capita income in state at ages 18-23

[4]Results are robust to including women with no children as well.

divided by either average log per capita income in state at ages 6-12 (RI1), average log per capita income in state at ages 12-16 (RI2), or average log per capita income in state at ages 0-4 (RI3)) and average national log per capita income from age 19-23 divided by average log per capita income in state of birth from ages 6-12 (RI4). The estimates reveal that measures of relative income where income from ages 6-12 is used as the childhood income proxy perform accordingly to Easterlin's hypothesis. The effect is sizable: a standard deviation increase in relative income increases children ever born by .2 for RI4 and .1 for RI1, but the effect is halved once cohort fixed effects are included. RI3 has the expected positive effect, but the effect is reversed when cohort fixed effects are included. RI2, has the expected positive effect only when cohort fixed effects are included. It should be noted that RI2 is possibly the most contaminated representation of relative income because the childhood and adult income proxies are measured at close proximity (age 12-16 vs. age 18-23) and thus likely suffer from a high degree of collinearity. The close relationship between the two income measures perhaps can explain the unexpected negative estimated effect of RI3 on fertility.

3.4.3 Cohorts

Does the effect of relative income differ by cohort? Maxwell (1991) found that relative income impacted women born in the years 1923-1929 and 1944-1954 more than other birth cohorts. In theory, relative income may matter only for generations exposed to a severe economic shock, such as the Great Depression. It could be the case that small fluctuations in income will not permanently change one's preferences but a prolonged economic shock will cause one to adjust one's long-term material aspirations. Table 4, which reports the results of estimating equation (1) for different birth cohorts, shows that the positive effect of relative income is only present for women born before 1940. Although I do find an effect for some post-1940 birth cohorts when I perform the estimation on the birth cohorts that Maxwell (1991) found were most affected by relative income. These results suggest relative income did not impact fertility for those born during WWII, but certainly played a role in the fertility decisions of those exposed to the hardships of the Great Depression.

3.4.4 Other Fertility Outcomes

Thus far I have used children ever born as the fertility outcome. In this section I report results with other fertility outcomes. Age of first marriage is a harbinger of completed fertility because women who marry younger have more children on average than women who marry later. Relative income might impact age at first marriage because a woman with high relative income will likely feel richer and more ready for marriage than a woman with low relative income who might wish to work for a few extra years in order to build up savings before marriage. Table 5, the results of estimating equation (1) with age at marriage as the dependent variable, show that RI1, RI2, RI3, and RI4 negatively influence age of marriage. A standard deviation increase in relative income decreases the age of first marriage by about one third of a year. The second row of table 4 shows the results of equation (1) when age at first birth is used as the dependent variable. Relative income might impact age at first birth in a similar fashion to age at marriage: a woman with high relative income might feel more prepared to start a family. Table 5, row 2, reveals that relative income has the predicted negative effect on age at first birth. The average effect is that a standard deviation increase in relative income lowers age of first birth by 0.2 years. Age of first birth has a larger impact on completed fertility than age of marriage (Bumpass et al., 1978) so while the estimated effect of relative income on age of first birth is smaller than the estimated effect on age of first marriage, the overall effect on completed fertility size is potentially larger.

3.5 Relative Income and the Baby Boom

How much of the baby boom can be explained by the rise in relative income? For comparison, I examine the 1930s birth cohort, a cohort that experienced the baby boom, and the 1910s birth cohort, a cohort that did not participate in the baby boom but contained parents of the baby boom birth cohort. On average, completed fertility rose by 0.6 children between the 1910s birth cohort and the 1930s birth cohort. Relative income (log per capita state income in adulthood/ log per capita state income in childhood) rose from an average of 1.2 for the 1910s birth cohort to 1.7 for the 1930s birth cohort. If I use my preferred measure of relative income (RI1), an increase of .05 in relative income corresponds to a completed fertility increase of .135, if the estimate without cohort fixed effects is used, and an increase of .044 if the estimate with cohort fixed effects is used. Thus, relative income can explain 23 or 7 percent of the shift in completed fertility, depending on whether cohort fixed effects are included or not. It should be noted that the estimates with RI3 (the measure of relative income that correlates highly with completed fertility in the time series) are the largest, and suggest that relative income can explain about 57% of the baby boom. However, this result disappears completely when cohort fixed effects are included. And while RI3 does correlate with fertility, it is perhaps a problematic representation of Easterlin's theory because the proxy for childhood income is state income during ages 0-4. And it is hard to understand how material aspirations would be more shaped by ages 0-4 compared to ages 6-12 or 12-17.

3.6 Discussion

In this paper I re-investigated the Easterlin hypothesis using census data and state income data. Overall, my estimates of the effect of relative income on completed fertility support Easterlin's theory. Unsupportive results were found only when I used relative income measures that exhibited a high degree of collinearity. My estimates also reveal that relative income impacted intermediate fertility measures (age of marriage and age at first birth) as well.

While I believe Easterlin's theory has empirical support, I question to what extent it can explain the baby boom. Including cohort fixed effects greatly diminishes the role of relative income in the baby boom (from 23 to 7 percent). An argument can be made that cohort fixed effects should not be included in the estimation because relative income may be the defining aspect of a particular cohort, but then all 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 WWII, would be subsumed in the relative income effect. Thus the relative income effect absent of cohort fixed effects can be viewed as an upper bound for the role of relative income in the baby boom and even this upper bound explains less than half of the baby boom. The Easterlin hypothesis was indeed confirmed in this paper, however, the results suggest it was not the primary driver of the baby boom.

Figure 1 - Log per Capita State Income (1919-1987)

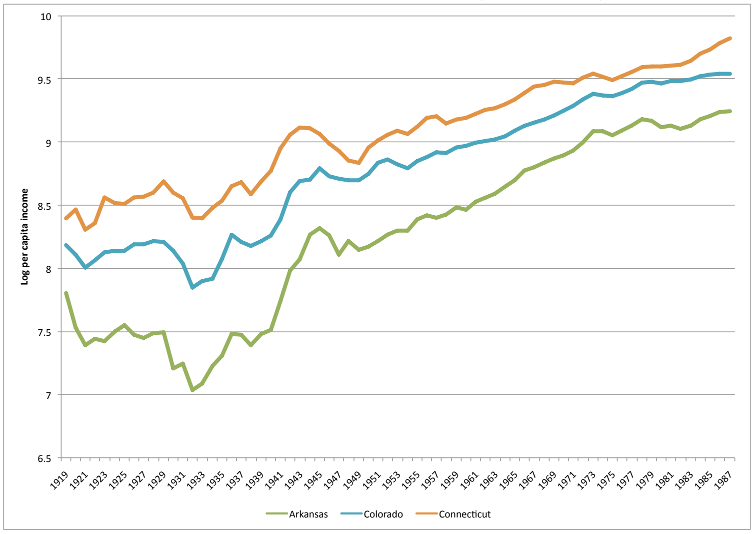

Figure 2 - Children Ever Born and Relative Income

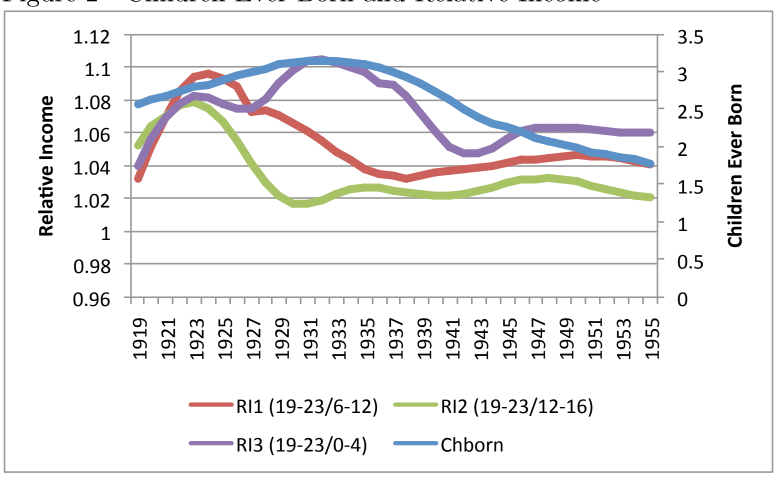

Table I - Definition of Migration Groups

Women in Group 1	
	• All birthplace locations agree (Spouse, Children, Past State, Current)
	• Spouse moved to current state (Only Spouse's birthplace is differs)
	• Moved after kids were born (Only current state or past state is different than birthplaces and children birthplace)
	• Birthplace=Current state (no info on children's birthplace because children not in household)
Women in Group 2	
	• Movers (Spouse and Woman born in same place and then moved to current state where children were born)
Women in Group 3	
	Spouse and woman have the same birthplace but children birthplace and current state is different
	Moved to Spouse (only woman's birthplace different; spouse and children have same birthplace)
	Moved to Spouse (same as above but no info on children)
	Children born in current state but spouse and woman have different birthplace
	Moved to Spouse and then moved again; spouse and children birthplace the same but current state differs.
Women in Group 4	
	No matches between own birthplace, spouse birthplace, children's birthplace and current state

Table II -Effect of Childhood and Adult Income on Children Ever Born

Sample	Child StateInc	Adult None	Child StateInc	Adult SpouseInc	Child StateInc	Adult StateInc
Group 1	-.248***		-0.175***	3.81***	-0.039***	-0.652***
	[.008]		[.009]	[.568]	[.010]	[.018]
Group 1,2	-0.146***		-0.077***	4.16***	-0.045***	-0.433***
	[.006]		[.006]	[.504]	[.006]	[.013]
Group 1,2,3	-0.164***		-0.062***	4.58***	-0.111***	-0.306***
	[.005]		[.005]	[.438]	[.005]	[.011]
Group 1,2,3,4	-0.029***		0.009*	3.38***	0.023***	-0.394***
	[.004]		[.005]	[.406]	[.004]	[.009]
Group 1,2,3 Movers					-.044***	.025
					[.008]	[.018]
Group 1,2,3 born>1922					-0.050***	0.019
					[.006]	[.013]

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (1) in the text. Individual controls included are urban/rural dummy, race, education, and a divorced dummy.

Table III - Effect of Relative Income on Children Ever Born

	RI1		RI2		RI3		RI4	
Group 1	0.880***	2.70***	0.694***	-0.97***	-0.128*	6.84***	1.96***	4.45***
	[.079]	[.051]	[.087]	[.054]	[.074]	[.054]	[.057]	[.044]
Group 1,2	0.236***	1.97***	0.157***	-0.663***	-0.348***	4.50***	1.16***	3.36***
	[.052]	[.040]	[.057]	[.043]	[.049]	[.040]	[.041]	[.035]
Group 1,2,3	0.720***	2.23***	0.708***	-0.46	0.313***	4.25***	1.29***	3.38***
	[.041]	[.033]	[.046]	[.036]	[.038]	[.033]	[.035]	[.030]
Group 1,2,3,4	-0.458***	1.15***	-0.552	-0.783***	-0.656***	2.93***	0.272***	2.25***
	[.033]	[.028]	[.037]	[.030]	[.031]	[.028]	[.029]	[.025]
Cohort Fixed Effects	X		X		X		X	

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (1) in the text. Individual controls included are urban/rural dummy, race, education, and a divorced dummy.

Table IV - Effect of Relative Income on Children Ever Born by Birth Cohort

Birth Cohorts	RI1	RI2	RI3	RI4
1940-1955	-0.779***	-0.701***	-0.675***	-0.710***
	[.078]	[.085]	[.064]	[.073]
1919-1939	0.606***	0.681***	0.060	1.13***
	[.051]	[.056]	[.049]	[.043]
1923-29,1944-54	1.24***	0.870***	0.789***	1.27***
	[.059]	[.065]	[.058]	[.051]

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (1) in the text. Individual controls included are urban/rural dummy, race, education, and a divorced dummy.

Table V - Effect of Relative Income on Age at first Marriage and Birth

Dep. Variable	RI1	RI2	RI3	RI4
Age of marriage	-8.79***	-11.62**	-7.03***	-8.84***
(N=1355462)	[.137]	[.152]	[.129]	[.118]
Age of first birth	-6.08***	-9.04***	-3.87***	-4.76***
(N=691701)	[.246]	[.280]	[.203]	[.221]

Notes: *** significant at the 99% level. ** significant at the 95% level. * significant at the 90% level. Table displays results of estimating equation (1) in the text. Individual controls included are urban/rural dummy, race, education, and a divorced dummy.

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