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THE IMPACT OF THE WWI AGRICULTURAL BOOM AND BUST ON FEMALE OPPORTUNITY COST AND FERTILITY

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ABSTRACT

Using variation in crop prices induced by large swings in demand World War I, we examine the fertility response to increases in crop revenues during the period 1910-1930. Our estimates from samples utilizing both complete count decennial census microdata and newly collected county-level data from state health reports indicate that a doubling of the agricultural price index reduced fertility by around 8 percent both immediately and in the years following the boom. We further document that this effect was more pronounced in more agrarian areas and where the labor intensity of agriculture was more intense. Extensive robustness checks and analysis of potential mechanisms indicate that the decrease in fertility was driven by increased female opportunity costs which dominated any household income effects resulting from the price boom.

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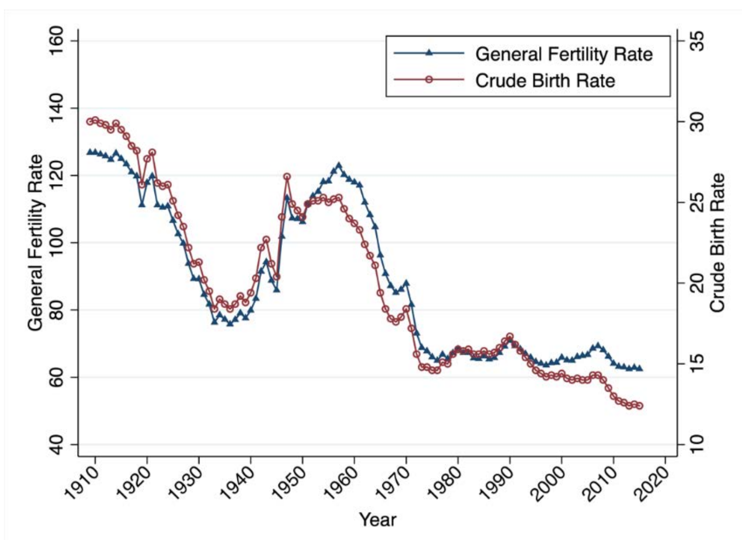
1. Introduction

A recurring theme in economics and demography is that as economies develop, a demographic transition will occur, whereby declining fertility rates accompany rising incomes (Galor, 2005; Jones and Tertilt 2009; Galor and Weil, 2000, Bruekner and Schwandt, 2015). Theory makes sharp predictions regarding the relationship between fertility, income shocks, and changes in the opportunity cost of time (Becker, 1960; Becker and Lewis; 1973; Becker and Tomes, 1976; Rosenzweig, 1977), however, empirical evidence is needed to understand whether pure income shocks or changes in the opportunity costs of time dominate in specific contexts. A recent literature has developed that takes advantage of exogenous variation in income, employment, or wealth to identify their impact on fertility (Lindo, 2010; Ananat et al., 2013; Huttunen and Kellokumpu, 2017, Black et al., 2013, Yonzan et al., 2020; Fishback, Haines, Kantor, 2007; Lovenheim and Mumford, 2013; Dettling and Kearney, 2014; Bailey and Collins, 2013; Lewis, 2018; Fujii and Shonchoy, 2020; Wanamaker, 2012; Schaller, 2016). This literature has broadly shown that increases in the male wage or income leads to increases in fertility, while increasing female wages result in reductions in fertility. Much of the recent literature focuses on identifying the impacts of these shocks in advanced economies, however, developing nations are the most likely to experience demographic transitions in the near future given their reliance on agriculture. In the developing context it has been more challenging to utilize aggregate shocks to identify the impacts on fertility, although there are several notable exceptions (Schultz, 1985; Alam and Portner, 2018; Corno, Hildebrandt, and Voena, *forthcoming*). One challenge in predicting what will occur in developing nations is the lack of quality data over a long time horizon. History provides one avenue to better understand how individuals and families adjust to large aggregate shocks, both in the immediate aftermath and following a period of adjustment (Collins and Margo, 2007, Hornbeck, 2012, Hornbeck and Naidu, 2014; Hanlon, 2017; Feir, Gillezeau, and Jones, 2019; Kantor and Whalley, 2019; Boustan, Kahn, Rhode, and Yanguas, 2020).

In this paper we draw on rich, historical data from the United States to understand how fertility responds to aggregate shocks that change both incomes and the relative opportunity costs of women. Specifically, we take advantage of a period of significant agricultural price variation, the agricultural commodity boom and bust in the United

States surrounding World War I (WWI). During the period we study, 1910-1930, both the General Fertility Rate and Crude Birth rate fell by approximately 29 percent (Figure 1), which is as large a decline as the Baby Boom was a boom. The agricultural price variation induced by international events combined with large changes in fertility make it an ideal setting to explore the link between income shocks and fertility. The agricultural commodity boom in WWI was entirely unexpected as fields in Europe were destroyed. Additionally, the magnitude of the shock was massive. U.S. agricultural exports doubled in the second half of the 1910s, in some cases prices more than doubled, and agricultural receipts increased by 70 percent (Henderson et al., 2011). Farmers expected the boom to persist, as documented by rising land prices, and the agricultural bust following the Treaty of Versailles also came as a surprise as Europe rapidly recovered post war.[4] Finally, female wages in agriculture were impacted by the crop price boom-and-bust, which we argue makes changes in female opportunity costs, rather than household income, the driving mechanism for the fertility response.

Figure 1 - United States Fertility Rate 1909-2015



Notes: National Vital Statistics System data (NVSS, 2017). The general fertility rate is the number of births per 1,000 women aged 15-44. The crude birth rate is the number of annual live births per 1,000 people in the country's population.

[4] In the years following WWI, significant price volatility remained, at least in part as a result of increased international competition and new domestic policy (i.e., Capper-Volstead Act, 1922, Fordney-McCumber Tariff, 1922).

The first few decades of the 20th Century United States have generally been understudied due to limited data availability. The federal government did not begin recording births until 1915 with the creation of the Birth Registration Area (BRA), which was not complete until Texas joined in 1933. To gain insight in the pre-war period, we digitize annual county-level birth tabulations from available state health reports prior to a state's entry into the BRA to push back the series to 1910 for twenty-six states. The longer panel enables us to capture any variation in pre-trends that might confound our estimate of the fertility-income relationship. Additionally, we turn to the complete count Population Census for the years 1910, 1920, and 1930 to compare birth outcomes for women who would have been differentially exposed to the agricultural boom and bust, netting out common locational and cohort effects.

To empirically identify the relationship between changes in fertility and changes in agricultural income, we complement our birth data with a measure of annual county-level agricultural crop revenue. Specifically, we follow Rajan and Ramcharan (2015) and Jaremski and Wheelock (*forthcoming*) to construct a county-level agricultural price index. The index combines pre-war crop production bundles at the county-level with aggregate crop specific price shocks to generate our key source of spatial-temporal variation. We use the index as our measure of agricultural income because annual county-level crop receipts are otherwise unavailable. We then estimate the relationship between the agricultural price index and fertility, controlling for a rich set of covariates, location specific fixed effects, and time fixed effects. Under the assumption that that the initial crop specialization patterns are driven by agro-climatic variables and that national crop specific prices are driven by international events and climate shocks, variation in the agricultural price index is exogenous to the measures of birth and fertility, thus permitting a causal interpretation for our estimates.

Our estimates consistently show that the agricultural boom reduced fertility in the short-run, measured by annual county-level births, as well as in the medium and long-run, measured by counts of children from the population census. In the short-run, we estimate that annual county-level births fell by 4-8 percent when crop prices doubled. Evaluated at the average value of the agricultural index, this 2.9 percent decline in births indicates that agricultural price variation explains about 10 percent of the overall decline in fertility between 1910 and 1930. This estimate is robust to a variety of potential confounds, including county-level WWI induction rates and exposure to the Spanish Influenza Pandemic. Turning to the complete count census samples, we estimate that doubling the price index reduced the number of children under the age of 5 for women in prime child bearing years by 0.189. This estimate translates to an 8.9 percent relative decrease when evaluated at the mean number of children and mean agricultural index value. Using a proxy for long-run fertility, we estimate a 6 percent relative decline in the total number of children in the home, indicating that fertility was not merely delayed or retimed. Combined, these results suggest that in our context, the increased opportunity costs that women faced more than offset any potential income effects.

After documenting the net decline, we further explore the underlying mechanisms. In particular, we examine mechanisms that explore variation in opportunity cost and the direct costs of raising children. Dettling and Kearney (2014) document differential effects for renters (who experience real price increases) and property owners (who experience wealth shocks). We explore owner/renter heterogeneity in our setting by interacting the 1910 county level farm owner-operator rate with the agricultural price index and find that ownership contributes to increases in fertility, although not enough to offset the overall decline. Given recent work in development economics that explores differential child investment based on the labor intensity of crops (Kruger, 2007; Cogneau and Jedwab, 2012), we also explore how the labor intensity of agriculture impacts our estimates. We document that price shocks in areas with higher labor intensity of agriculture lead to larger declines in fertility, supporting the conclusion that the primary driver of our findings is the increased opportunity cost of women's time. Further, we show that young women delayed marriage in response to the price boom and that the delay is increasing in the labor intensity of agriculture.

Our paper contributes to multiple strands of literature. First, our paper complements the aforementioned body of work that seeks to understand the causal relationship between economic shocks and fertility. Second, we view our work as contributing to the broader modern development economics literature that relates agricultural and natural resource shocks on families and family structure (Beegle, Dehejia, and Gatti, 2006; Kruger, 2007; Akresh, 2009; Cogneau and Jedwab, 2012). Our estimates show that agricultural commodity price shocks can also affect the size of the family, which closely relates to Schultz (1985) and Corno, Hildebrandt, and Voena (*forthcoming*). Finally, our work contributes to a recent historical literature that seeks to understand the economic impacts of WWI, joining a growing body of work that explores family formation in Europe (Abramitzky, Delavande, and Vasconcelos, 2011; Vandenbrouke, 2014; Gay, 2019; and Boehnke and Gay, 2020). In the US context, our work joins that of Rajan and Ramcharan (2015) and Jaremski and Wheelock (2018) who study expansions of credit and banking during the interwar period, as well as work by Moser and Voena (2012) who study the future path of innovation following wartime policy changes to intellectual property rights.

Improving our understanding of how aggregate shocks affect income and the opportunity costs of women remains a particularly important goal for researchers and policy makers alike. Interactions between shocks and opportunity costs can impact the efficacy of programs that are designed to encourage family formation and can have lasting impacts on entitlement programs which are based on multigenerational demographic projections. While our estimates are time and place specific, understanding how changes in gender-specific opportunity costs and the relative substitutability of production inputs impact family formation are undoubtedly no less relevant in the 21st century.

2. World War I Agricultural Boom and Bust

WWI created an unprecedented demand shock for American agricultural products. As war raged in Europe, wheat production fell by over 50 percent in both France and Italy, oat production dropped 59 percent in Germany, and livestock plummeted to a quarter of their prewar levels in Denmark (Nourse, 1924). Distress in ocean shipping further increased demands for US products that were less exposed on ocean shipping lanes than goods originating from more distant sources, such as Argentina and Australia. In response, prices for American goods rose sharply. In Figure 2, we highlight the annual variation in crop prices for 11 major crops in the United

States, relative to their price in 1910.[5] In some cases, prices for crops such as potatoes and tobacco increased over 300% during the period 1914-1919.

As a result of the rapid increase in agricultural prices, production on the home front ramped up. Between 1914 and 1919, 30 million new acres of land were brought into production (a 9 percent relative increase).[6] Given the sharp increase in prices and the increased production, the aggregate the value of crops harvested in the United States more than doubled over the same time horizon.[7]

The rapid increase in production strained input markets. Labor was particularly scarce in rural areas as people flocked to cities to take advantage of the wartime manufacturing boom. While America's war effort was modest in WWI relative to World War II in terms of manpower, over 4 million young men were drafted to become doughboys, further tightening the labor market.

Here we provide two examples of programs that were initiated to address the farm labor shortage during the war years, the Women's Land Army and the Boys Working Reserve. At its peak, the Women's Land Army recruited upwards of 20,000 women from colleges and universities work on farms while living in camps (SSA, 1942). The USDA also developed programs in conjunction with states to send young boys to work on farms during the summer months. In 1917, the U.S. Department of Labor created the *Boys Working Reserve*. In 1918 for example, the Boys Working Reserve placed 210,000 16-20 year-old boys on farms for summer employment (US. Dept. of Labor, 1919). In Massachusetts during the summer of 1918, Boys Working Reserve camps provided 9,226 weeks of labor, with an average of 651 boys working each week.

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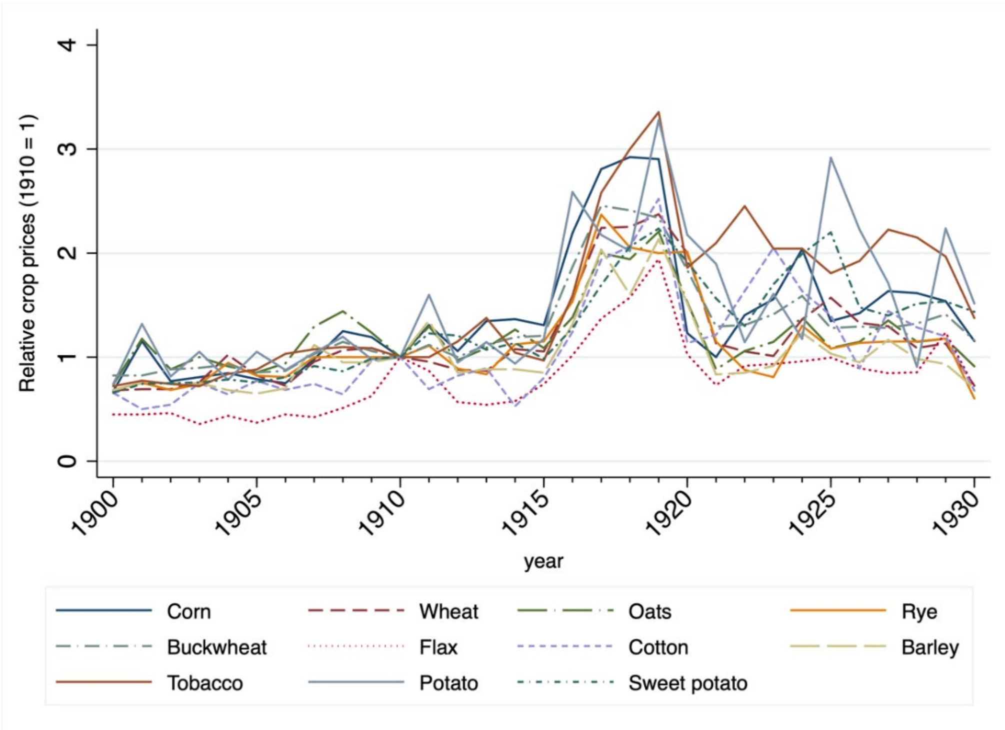
[5] We follow Rajan and Ramcharan (2012) and Jaremski and Wheelock (*forthcoming)* in the selection of crops that will form the basis of our agricultural price index. These crops include: Corn, Wheat, Oats, Barley, Rye, Buckwheat, Flax, Cotton, Tobacco, Potato, and Sweet Potato.

[6] Olmstead, Alan L. and Paul W. Rhode, "Cropland – acreage harvested and indexes of cropland use and production per acre: 1910–1990." Table Da661-666 in *Historical Statistics of the United States, Earliest Times to the Present: Millennial Edition,* edited by Susan B. Carter, Scott Sigmund Gartner, Michael R. Haines, Alan L. Olmstead, Richard Sutch, and Gavin Wright. New York: Cambridge University Press, 2006. http://dx.doi.org/10.1017/ISBN-9780511132971.Da661-1062

[7] Acquaye, Albert K. A. , Julian M. Alston and Philip G. Pardey , "Agricultural output – gross and net value, by crop and livestock: 1910–1998." Table Da1063-1081 in *Historical Statistics of the United States, Earliest Times to the Present: Millennial Edition,* edited by Susan B. Carter, Scott Sigmund Gartner, Michael R. Haines, Alan L. Olmstead, Richard Sutch, and Gavin Wright. New York: Cambridge University Press, 2006. http://dx.doi.org/10.1017/ISBN-9780511132971.Da1063-1265

The program also placed an additional 1,600 boys on individual farms (Committee on School Boys, 1919). Finally, there were also efforts to employ idle urban labor in nearby farms.

Figure 2 - Crop Price Variation 1900-1930



Notes: National price variation for the 11 crops used in the crop index. Data from Carter et al. (2006). Prices are relative to 1910 baseline.

Despite these programs initiated by the USDA, the labor shortage led to increases in the nominal wages for agricultural workers. Between 1914 and 1919, farm wages increased from $22 a month to $43 a month (US Dept. of Labor, 1945). While no aggregate data are reported by the Census Bureau or the USDA regarding gender specific wages, there are anecdotes of women making wage advances. First, the USDA explicitly pushed for wage equality between men and women at the peak of the war (USDA, 1918). In California, women working under the direction of the Women's Land Army were paid a minimum wage of $2/day or the market wage, whichever was greater (Appendix Exhibit 1). Based on reports by the USDA, published in newspapers across the country in 1918, the wage paid to the Women's Land Army was equivalent to the daily wage that included room and board (Appendix Exhibit 2). To the extent that there were real wage increases for women in agriculture, this would tend to increase the opportunity cost of child bearing, at least in the short-run.

As a result of high wages, farmers looked to mechanize. Between 1913 and 1930, tractor adoption rates were most rapid in 1918 and 1919. In these years, the number of tractors in service grew by 66 and 85 percent respectively. There has never been another period in US history with such rapid tractor diffusion. Even in levels, the growth in tractors at the end of WWI rival any other period in the first half of the 20th Century (Carter et al., 2006).[8] While early tractors were not the general purpose machines they are today (Gross, 2018), they allowed farmers, especially those in the upper Midwest, to substitute capital for labor. The diffusion was indeed concentrated in the grain belts. For example, over 9,000 tractors were put into service in 1919 in Iowa alone, accounting for approximately 10 percent of all tractors nationally (New York Times, 1919). In Ohio, the number of tractors on farms doubled between 1918 and 1919 (Chicago Daily Tribune, 1919). In some cases, such as in Pennsylvania, the Pennsylvania Committee on Public Safety and Defense purchased 40 tractors to ensure that the labor supply shortage would not interfere with planting corn and oats (Adams County Independent, 1918). Theoretically, how mechanization impacts fertility is ambiguous (Rosenzweig, 1977).

The agricultural boom in the US was short-lived as it was followed by an abrupt and unexpected bust. The fields of Europe recovered quickly following the signing of the Treaty of Versailles. For example, Buyst and Franaszek (2010) report that crop specific yields recovered for most of Europe by 1922. Even Russia, in the midst of a civil war, was able to increase its agricultural output to pre-war levels by the mid-1920s (Markevich and Harrison, 2011). Throughout the 1920s, agricultural commodity prices continued to fluctuate, although to a lesser extent than the WWI period.

3. Data

To estimate the relationship between the WWI agricultural boom/bust cycle and fertility, we combine data from the county-level agricultural Census with two different

[8] Olmstead, Alan L. and Paul W. Rhode, "Farm machinery and equipment: 1910–1998." Table Da623-634 in *Historical Statistics of the United States, Earliest Times to the Present: Millennial Edition*, edited by Susan B. Carter, Scott Sigmund Gartner, Michael R. Haines, Alan L. Olmstead, Richard Sutch, and Gavin Wright. New York: Cambridge University Press, 2006.

samples detailing fertility. First, we construct a newly digitized dataset of county-level birth counts that predate the Federal Birth Registration Area (BRA), sourced from state health reports between 1910 and 1930 for 26 states. Second, we turn to the complete count public use data from the US Population Census for the years 1910, 1920, and 1930 (Ruggles et al., 2020).

Population and Agricultural Data

To create the agricultural price index at the county-level, we follow Rajan and Ramcharan (2015) and Jaremski and Wheelock (*forthcoming*). We begin by collecting county-level output for 11 crops (corn, wheat, oats, barley, rye, buckwheat, flaxseed, cotton, tobacco, Irish potatoes, and sweet potatoes) from the 1910 Census of Agriculture (Haines, Fishback, and Rhode, 2018). We then multiply each county's 1910 crop output $Q_{i,c,1910}$, by the crop's annual national price, $P_{i,t}$, drawn from Carter et al. (2006) to compute the annual county-level crop revenue.[9] Finally, we normalize the annual county-level crop revenue by the average county-level revenue for the period 1908 and 1914, using the average crop price, $\overline{P_i}$.

$$CropIndex_{c,t} = \frac{\sum_{i=1}^{11} Q_{i,c,1910} \times P_{i,t}}{\sum_{i=1}^{11} Q_{i,c,1910} \times \overline{P_i}}$$

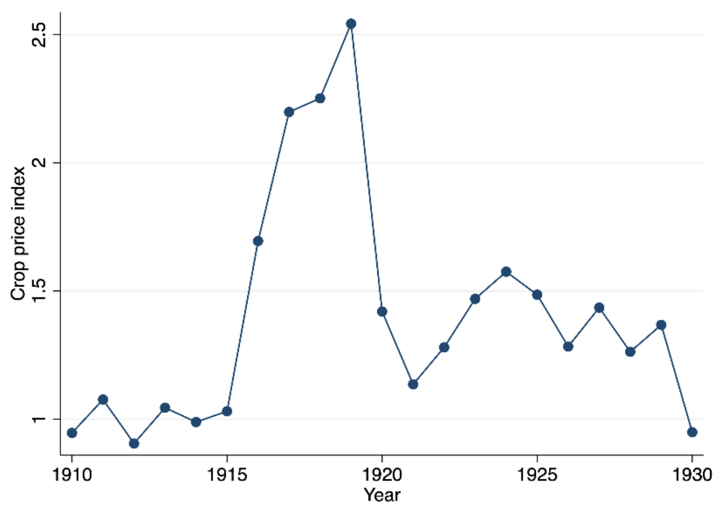
By fixing the output at the 1910 value, and using national prices, we ensure that the variation we exploit is exogenous to the decisions of local farmers. For instance, we do not have to be concerned with the potential of endogenous crop mixes in response to the movement in prices.

In Figure 3, we highlight the time variation in the index. Beginning in 1915, the time when agriculture began to collapse in Europe, crop prices begin to rise dramatically, reaching a peak in 1918, with prices increasing by over 250 percent. Following the signing of the Treaty of Versailles in 1919, agricultural prices fell dramatically, yet remained above their pre-WWI level. Given prewar agricultural

[9] Recent work by Goldsmith-Pinkham, Sorkin, and Swift (*forthcoming*) has debated the use of Bartik (1991) style variables, noting that their validity depends on the exogeneity of the initial shares, in our case $Q_{i,c,1910}$. In our context, agricultural productivity and crop choice was largely driven by agro-climatic variables, such as precipitation, temperature, soil type, and the biological pest environment.

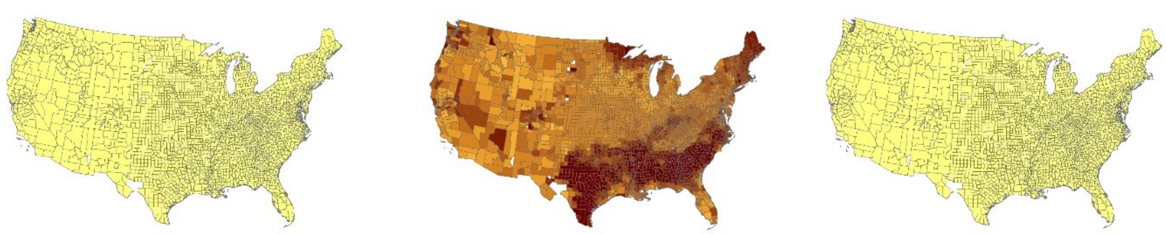
production and crop specialization patterns, there was significant heterogeneity across space in terms of the local intensity of the agricultural boom. Cotton, Irish Potatoes, Tobacco, and Flaxseed all experienced price increases exceeding 300%. Thus, areas such as the Southeast, where cotton and tobacco are grown, experienced relatively larger shocks than the Midwest or West. Similarly, portions of Minnesota and North Dakota, experienced relatively larger price spikes due to their production of flax. In Figure 4, Panels A-C, we highlight the spatial temporal variation in the index for the years 1914, 1919, and 1930.

Figure 3 - Agricultural Price Index Time Variation 1910-1930



Notes: The average crop index over our analysis timeframe. The index is a function of the county's baseline crop mix from 1908 – 1914 and national price fluctuations (Carter et al., 2006).

Figure 4 - Spatial Variation in Agricultural Price Shock



(a) 1914 (b) 1919 (c) 1930

Notes: Three snapshots of the geographic variation in the crop index over our analysis timeframe. WWI caused demand shocks to be concentrated in different counties based on their preexisting crop composition. Data from the Agricultural Census and Carter et al. (2006).

In addition to the data from the Census of Agriculture, we also merge several county-level economic and demographic variables from the 1910 population census (Haines, 2010). The primary variables we use include the population, percent non-white, percent urban, percent aged 6-14, percent illiterate, and the value of manufactured goods per capita.

Annual County Level Birth Data

Our first measure of fertility is comprised of county-level birth counts constructed from a combination of Birth Registration Area data and State Board of Health Reports sources. We begin with county-level birth data reported by the Federal Birth Registration Area (Eriksson, Niemesh, and Thomasson, 2018). The Federal Birth Registration Area (BRA) was formed in 1915, and, at its inception included 10 states, primarily in the Northeast and upper Midwest. States joined the BRA following an application and certification process, whereby the US Census Bureau verified that the state in question accurately recorded 95 percent of births. The BRA was not complete until 1933, when Texas joined. To supplement these data, we collected and entered county-level birth data using a combination of state health department annual reports, state vital statistics annual reports, and state board of health monthly and quarterly bulletins published prior to a state's entry into the BRA back to 1910. To appear in the sample, a state must not have missing data for more than 1 year between 1910 and its entry into the BRA. We are able to construct a county-level birth panel covering 26 states between 1910 and 1930 (see Appendix Figure 2).[10] In Appendix Table 1 we report the year that each state in our sample entered the BRA and the years for which we collected and coded the state-level reports.

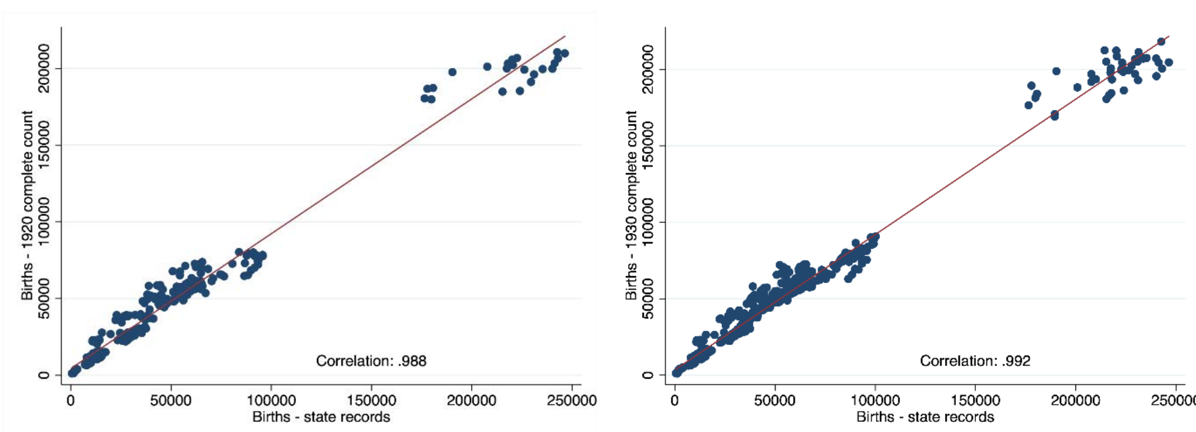
The advantage of the county-level data are that they allow us to test for instantaneous responses in fertility associated with the WWI boom and bust. However, these data also have limitations. First, the states that appear in our sample tend to be located in the northeast, upper Midwest, and Pacific Coast. Thus, our county-level sample does not use the most extreme variation stemming from large swings in cotton

[10] Each state set their own policy in regards to the tracking of vital statistics. In many cases, states did not pass enabling legislation early enough to begin the certification process to enter the Census Birth Registration Area or Death Registration Area during our sample period.

and tobacco prices. Secondly, given the reporting accuracy requirements to enter the BRA, an obvious concern with the use of pre-BRA data is its reliability (i.e., measurement error).

Eriksson, Niemesh, and Thomasson (2018) point out that even after states entered the BRA, there are discrepancies between the US Population Census and the BRA birth counts. They also document that the most sever discrepancies occur in the South, which is largely missing from our sample. To examine the quality of reporting in our sample, we aggregate the county-year observations to the state-year level and correlate the state-year totals to the state-birth cohort totals from the complete counts of the population Census. In Figure 4, Panel A, we highlight the correlation between the state health report sample (x-axis) and the 1920 complete count Census (y-axis). The correlation between these two data sources is 0.988. Similarly, in Figure 5, Panel B, we highlight the correlation between the state health report data the 1930 Census. In 1930, the correlation is 0.992.[11]

Figure 5 - Correlation between Births reported in State Health Reports and State of Birth in 1920 and 1930 Censuses



(a)1920 **(b) 1930**

Notes: Newly digitized birth counts from state health reports compared to complete count census data.

[11] These data reflect a correction made to the Eriksson, Niemesh, and Thomasson (2018) data. In the original data, the birth counts for New York City are underreported by an order of magnitude. We have corrected these drawing on data from Fifty-First Annual Report of the State Department of Health for the Year Ending December 31, 1930: Volume 2 Division of Vital Statistics. Burland Printing Co. New York. 1931. In Appendix Figure 3, we highlight how this correction affects the correlation.

While there is a strong correlation between the county-level data and the Census data, there still may be a lingering concern that the reporting quality is suspect. Our estimates would be biased if measurement error is systematically correlated with movements in agricultural commodity prices. To alleviate these types of measurement concerns, our regressions include a variety of county-level covariates, county fixed effects, year fixed effects, and state-year fixed effects.

Our county-level dataset consists of 27,969 county-year observations. In several specifications, we restrict this sample, dropping counties with populations above the 90th percentile of the 1910 population. We do this to ensure that our estimates are driven by changes in agricultural commodity prices and are not driven by other economic consequences of WWI such as industrial growth in urban centers. Table 1 includes summary statistics of the county-level dataset. Unsurprisingly, removing the right tail of the population distribution reduces the mean number of annual births per county, manufacturing output per capita in 1900, and the fraction of the population classified as urban in 1910. Otherwise, the restriction does not drastically affect the variable means. As mentioned above, we supplement the available control variables with extensive fixed effects.

Table 1 - Summary Statistics, County-Level Data

	Full Sample		Population Restricted Sample	
	Mean	Std. Dev.	Mean	Std. Dev
Annual births	932	(2739)	412	(302)
Index 1-year lag	1.37	(0.45)	1.37	(0.45)
Fraction owner-operated farm	0.75	(0.13)	0.74	(0.13)
Fraction farm land	0.71	(0.13)	0.71	(0.29)
Fraction non-white	0.05	(0.12)	0.06	(0.16)
Fraction urban	0.21	(0.26)	0.15	(0.20)
Fraction age 6 to 14	0.19	(0.03)	0.19	(0.03)
Fraction illiterate	0.04	(0.04)	0.04	(0.05)
Manufacturing output per cap	89.9	(123)	64.3	(93.4)

Note: There are 27,969 observations for the full sample and 23,976 for the restricted sample. The 90th percentile for county population in 1910 was 48,116. Controls for race, age, and literacy are available for 27,944 county-year observations in the unrestricted sample and 23,976 observations in the restricted sample. The fraction of the population classified as urban is available for 27,797 and 23,804 observations for the full and restricted samples, respectively. Manufacturing output is available for 27,274 and 23,281 observations for the full and restricted samples, respectively. Each fraction variable is measured in 1910 while manufacturing output is measured in 1900.

Individual Level Dataset

Given the trade-offs of the county-level dataset, we turn to the complete count microdata from the 1910, 1920, and 1930 Census (Ruggles et al., 2020). In this sample, we focus on women aged 14-49, not living in group quarters. Across the three waves of the census, this amounts to over 49 million woman-year observations. In several specifications we will limit our attention to women who were more likely of child bearing age (under 35), of which there are approximately 25 million observations in the sample. As would be expected, women in the under 35 sample have more children under the age of 5 and less total children than the full sample. We will further consider similar population restrictions, dropping women living in the most populated counties based on 1910 population counts. The rural, under age 35 sample contains just over 12 million women. Importantly, restricting the sample to rural areas increases the probability that a woman is living on the farm from 25 percent in the under 35 sample to 46 percent in the rural sample. Women under the age of 35 living in less populated counties have more children and are less likely to be white, likely reflecting both the importance of child labor on farms and our pre-Great Migration period of analysis.

Table 2 - Summary Statistics, 1910-1930 Complete Count Data

	Complete Count (1)	Under 35 (2)	Under 35 and population restriction (3)	Ages 30—40 (4)	Ages 30—40 and population. restriction. (5)
Married	0.92 [0.26]	0.96 [0.20]	0.96 [0.19]	0.93 [0.25]	0.94 [0.23]
Age	34.0 [8.37]	27.1 [4.49]	26.7 [4.65]	34.5 [2.88]	34.4 [2.88]
White	0.89 [0.31]	0.88 [0.33]	0.84 [0.37]	0.90 [0.31]	0.86 [0.35]
Rural	0.63 [0.48]	0.64 [0.48]	0.97 [0.16]	0.61 [0.49]	0.97 [0.17]
On farm	0.26 [0.44]	0.25 [0.44]	0.46 [0.50]	0.25 [0.43]	0.48 [0.50]
5 year index	1.44 [0.42]	1.44 [0.43]	1.42 [0.44]	1.44 [0.42]	1.42 [0.44]
10 year index	1.24 [0.27]	1.23 [0.28]	1.22 [0.29]	1.24 [0.27]	1.22 [0.29]
Children under 5	0.61 [0.84]	0.85 [0.90]	0.93 [0.92]	0.65 [0.85]	0.75 [0.89]
Total children	2.19 [2.02]	1.76 [1.65]	1.97 [1.76]	2.54 [2.08]	2.95 [2.21]
County pop. in 1910	298,183 [617,923]	286,467 [608,816]	23,499 [10,435]	315,312 [634,304]	23,745 [10,497]
Owner fraction in 1910	0.63 [0.19]	0.63 [0.19]	0.61 [0.21]	0.64 [0.19]	0.63 [0.20]
Observations	49,135,132	25,491,494	12,040,980	18,153,900	7,888,122

Notes: Complete count census data for 1910-1930. Standard deviations in brackets. Population restriction removes residents in counties which were in the top 10 percent of the population distribution in 1910 (population over 47,733).

4. Empirical Strategy

In what follows, we seek to estimate the reduced form relationship between a measure of fertility and the agricultural index. A naïve regression of births on income would likely produce biased results for two reasons. First, births may have a causal impact on income. This is particularly a concern in agriculture where children provide labor for the family. Second, omitted variables which affect both income and fertility could bias the estimates in a naïve regression; health and productivity are two examples of important characteristics which cannot be directly measured yet surely play a role in determining births and income.

To address these concerns, we exploit exogenous variation in the prices of different crops. The primary specification when using the county-level dataset derived from state health reports is:

$$LogBirths_{c,t} = \alpha + \phi Index_{c,t-1} + X'_{c,t}\beta + \tau_t + \sigma_c + \epsilon_{c,t}. \qquad (1)$$

The dependent variable is the log number of births in county c and year t. The primary coefficient of interest is ϕ which should capture the lagged effect of the previous year's crop index on county births. We use the lagged value of the index rather than the contemporaneous value given the nine month gestation period. Year (τ) and county (σ) fixed effects account for national differences over time and stationary differences between counties, respectively. The vector X is comprised of interactions between the year fixed effects and various baseline county-level characteristics. These controls are measured in 1910 and include the fraction of the population who is non-white, between the ages of 6 and 14, illiterate, and living in urban areas with over 2500 residents. We also interact year fixed effects with county-level manufacturing output per capita in 1910. By interacting these baseline observable characteristics with year fixed effects, this specification should net out differential trends in these dimensions which may otherwise be picked up by the lagged crop index.

Estimating the impact of the crop index on births using decennial census data requires alternative specifications due to the timing of the data collection and variable availability. Ideally, we would be able to use a measure of completed fertility, however this variable is not available in either the 1920 or 1930 Census. [12] The two primary dependent variables we use from the complete count data are the total number of

[12] We are primarily interested in how agricultural prices affected fertility, making the "children ever born" variable another relevant outcome. Unfortunately, this variable is only available in the 1910 and 1940 censuses.

children in the household and the number of children under age 5. Given the available data, our specification compares women of the same age across Census years, netting out time specific and county specific effects. Because the available variables represent the cumulative fertility response over multiple years, each woman must be assigned a cumulative or average crop index. When investigating the impact on children under the age of 5, we use the average county index over the last 5 years. When focusing on the total number of children we use the average county index over the previous decade. The general specification when using the census data is:

$$Kids_{i,c,t} = \alpha + \phi AvgIndex_{i,c,t} + X'_{i,c,t}\beta + \tau_t + \sigma_c + \epsilon_{i,c,t}. \qquad (2)$$

The observations are at the mother (i), county of residence (c), and census year (t) level. The index variable is one of those described above and is assigned to each mother based on her age and county of residence. Census year fixed effects (τ) and county fixed effects (σ) are included, as are fixed effects for age, marital status, race, size of residence location (based on total population), and whether the mother lives on a farm. The dependent variable is either the woman's total number of kids at the time of the census or the number of children under age 5 at the time of the census.

Both equation (1) and (2) are intended to address the same underlying question: how did the agricultural commodity boom (and subsequent bust) impact fertility decisions? A priori, it is unclear whether the estimates from both specifications should point in the same direction. For instance, fertility could immediately fall during the war time boom due to increased labor scarcity and increased opportunity cost of childbearing. However, at the war's conclusion, as labor demand subsides, and the supply constraint is lessoned as men return from Europe, women may simply retime children, resulting in no net change in total children in the long-run. Similarly, the sudden wealth shock could lead to an immediate increase in child bearing, while in the long-run there is no change in the total number of children born.

5. Estimates

Below we present our estimates from several different empirical specifications. We proceed by discussing our estimates in the short-run by examining the county-year sample and the complete count sample that focuses on children under 5 in the household. We then discuss several robustness checks that address many of the potential confounds. After describing the short-run estimates, we outline our estimates over a longer time horizon (10 years), where it is possible for families to adjust along multiple dimensions. After highlighting our main empirical estimates, we explore some of the underlying mechanisms and heterogeneity along key dimensions: the cost of children and labor intensity.

5.1 Short-Run Fertility Effects of Agricultural Boom

In Table 3, we report our estimates from equation (1) and equation (2). In Panel A, we report the baseline results using the county-level birth sample. Panel B includes the estimates using the number of children under age 5 as the outcome of interest. In each case, we report standard errors in parenthesis, clustered at the county level.[13] Using the county-level sample, we find that on average, a doubling of the agricultural price index results in 4.4 percent fewer births in a county. After dropping urban counties, we find that births fall by 5.2 percent for a doubling of agricultural prices. When we include our set of county level covariates, the magnitude our estimate increases to a decline of 8.03 percent. Given an average of 412 births per rural county, our estimates suggest that there were between 21 and 33 fewer births per county-year given a doubling in agricultural prices. Evaluating at the average agricultural index value throughout the period, our estimates suggest a 2.9 percent reduction in fertility. Given the 29 percentage point decline in aggregate fertility, 1910-1930, the agricultural price volatility explains about 10 percent of the overall decline. In Appendix Table 2, we document heterogeneity in the fertility response by focusing on subsamples of the data that are increasingly agricultural. There we show that the magnitude of the decline is increasing in the share of the county acreage in farms.

[13] We have also computed Conley (1999) standard errors for the county sample using a distance cutoff of 100 miles. The main result of Panel A remains statistically significant at the 10% level despite larger standard errors. We use county-level standard errors in order to be consistent across datasets. For the complete count estimates we have also computed the more conservative state level standard errors and find no meaningful differences.

Table 3 - Baseline Estimates, Fertility 1910-1930

Panel A: Y = ln(County Births)	(1)	(2)	(3)	(4)
Ag. Crop Index	-0.0442*	-0.0523**	-0.0803***	-
	(0.0253)	(0.025)	(0.0267)	-
Population Restriction		Y	Y	-
Controls			Y	-
Observations	27,969	23,976	23,976	-
Panel B: Y = #Children Under 5				
Average Ag. Crop Index	-0.113***	-0.203***	-0.189***	-0.029***
	(0.015)	(0.023)	(0.022)	(0.016)
Controls	Y	Y	Y	Y
Age Restriction		Y	Y	Y
Population Restriction			Y	Y
Same States as County Sample				Y
Observations	49,088,311	25,465,610	12,037,770	4,910,064

Notes: Panel A estimated using the county-level birth records. Panel B uses the complete count for 1910-1930. Every regression includes county and year fixed effects. Panel A controls include separate interactions between year fixed effects and baseline fractions of population who are non-white, between the ages of 6 and 14, and living in an urban area, as well as interactions with a baseline manufacturing output per capita. Panel B controls include dummy variables for age, race, marital status, farm status, and population size of place of residence. The population restriction drops the counties which are in the top 10 percent of population in 1910. The age restriction in Panel B is for women under the age of 35. Standard errors clustered at the county level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The instantaneous decline in births could simply reflect retiming due to the war disruption. Indeed, a model that allows for household savings would predict that the number of children born should fall in the current period when female wages rise, but the increase in savings would result in more children in the future. We now turn to the complete count data samples in Panels B. In Panel B, we report the estimated impact of the average agricultural price index over the previous 5 years on the number of children under the age of 5 in the household. We estimate that a doubling of the price index over a 5-year period results in 0.113 fewer children under the age of five in the home. During our sample period, the average 5-year price index was 1.44 and the average number of children under age 5 was 0.61. Thus, on average, we estimate that the agricultural boom reduced the number of young children by about 8.1 percent relative to the mean. For younger women, who are more likely to be of child bearing age, we estimate a larger effect. For an average doubling of the index over 5 years, the coefficient increases to

0.203. Given slightly higher rates of young children in the home (0.85), the relative decline in young children among women under age 35 is 10.5 percent. For these same young women living in rural areas, the effect is similar, we find an 8.9 percent decline in children under age 5. Thus, in the intermediate run, there seems to be no evidence that the estimates from the county-level data reflect retiming alone.

One caveat to interpreting the estimates between panels A and B is that the complete count data include more states, and in particular, regions of the country where the agricultural boom was much more pronounced (i.e., the South). To ensure comparability between the complete count sample and the county sample estimates, we restrict the complete count sample to only include states that appear in our annual county-level panel. We report these estimates in Panel B, Column (4). When the sample is restricted, we find that the coefficient remains negative and statistically significant (-0.029), however, the magnitude falls. Still, the smaller estimate implies a 1.3 percent reduction in the relative fertility rate. There are two likely explanations for the difference in coefficient magnitude. The first is that the counties in our restricted sample are those who experience less severe swings in crop prices. The second is that the labor intensity of agriculture is lower in the restricted sample, which may dampen the opportunity cost channel.

Our findings are strikingly similar to those reported by Schultz (1985), who examines the impacts of commodity price shocks to crops that utilize gender specific labor in Sweden (butter and grain). Our estimates are also similar in magnitude to those reported by Aaronson, Lange, and Mazumder (2014) for cohorts who were completely exposed to the rollout of Rosenwald schools (7.2-9.5 percent) during roughly the same era in the American South. The estimates are also similar to those reported by Bleakley and Lange (2009), who explore the impact of hookworm eradication in the American South. Our findings depart from that of recent work by Black, Kolesnikova, Sanders, and Taylor (2013), who document increases in fertility during the Appalachian Coal Boom, however in our agricultural setting, the opportunity cost of women's labor is likely changing relative to the male wage, as documented anecdotally in the case of the Women's Land Army, whereas increases in male earnings were the main driver of income in the coal boom. Relative to recent work, our findings are consistent with work by Schaller (2016) who shows that fertility in increasing in the male wage, but decreasing in the female wage.

5.2 Robustness – Complete Count Migration Sample Restrictions

In the complete count sample, our model implicitly assumes that individuals experience the intensity of the boom/bust based on where they are enumerated. However, this assumption may be dubious, for instance, because of internal migration or from immigration (although during and after WWI there were additional immigration restrictions under Immigration Acts of 1917 and 1924). To ensure our estimates are not driven by either internal migration or immigration, we restrict the sample to individuals living in their state of birth. Further, we also estimate regressions where we drop women who were born in a foreign country, given that we do not know when they arrived to the United States. In each case, we maintain the restriction that women are under the age of 35 and are living in non-urban areas. In Table 4, we report the estimates from these specifications, standard errors are reported in parenthesis and are clustered at the county level. In Column 1, we report our baseline estimate of -0.189 from Table 3, Panel B for comparison. In Column 2 we show that restricting the population to individuals who have not left their state of birth has a minimal impact on the estimate, which increases in magnitude to -0.193. In Column 3, we restrict the sample to individuals who were born in the United States. Imposing this sample restriction, we estimate a coefficient of -0.191. In both cases, the estimates are similar to our baseline.

Table 4 - Number of Children Under Age 5, non-migrants

Y = # Children Under 5	(1)	(2)	(3)
Average Ag. Crop Index	-0.189***	-0.193***	-0.191***
	(0.022)	(0.0147)	(0.0135)
Urban Pop. And age Restriction	Y	Y	Y
Restrict to State of Birth		Y	
Exclude Foreign Born			Y
Observations	12,037,770	8,607,892	5,525,761

Notes: Sample uses the complete count for 1910-1930. Each regression includes county and year fixed effects as well as dummy variables for age, race, marital status, farm status, and population size of place of residence. The population restriction drops the counties which are in the top 10 percent of population in 1910. The sample is restricted to women under the age of 35. Column 1 replicates the estimates from Table 3, Panel B, Column 3 which examines rural women aged 35 or less. Column 2 restricts the sample to people living in their state of birth. Column 3 restricts the sample to those born in the United States. Standard errors clustered at the county level: *** p < 0.01, ** p < 0.05, * p < 0.1

5.3 Robustness – Other Potential Confounds

Given the time period, there are several potential confounds that raise concern. First and foremost, our estimates could be the byproduct of a reduction in the availability of prime age males as a result of WWI. Furthermore, as men returned from Europe, America experienced the Spanish Influenza Pandemic (Almond, 2006; Beach et al., 2018) which hit prime age males especially hard, which may have also impacted fertility patterns. If the spread of the flu were correlated with the boom, then our fertility estimates would be biased. Our time period also contains several secular movements that likely alter the returns to children, either through changes in the underlying health risk, or through their future labor market returns. For example, the period we examine is squarely in the middle of the public health and high school movements.

The United States drafted over 4 million men during WWI. Therefore, our estimates could be the result of fewer available partners and not due to the agricultural boom/bust. Further, local draft boards had significant discretion in the administration of deferments, which could be given on the basis of occupation. Thus, it's possible that the agricultural boom and draft were correlated with one another. To limit the impact of the draft on our estimates, we have collected and coded the Final Report of the Provost Marshal General (1920), that reports the number of men drafted in each draft precinct (smaller than counties). We aggregate the WWI precinct level draft numbers to the county-level and directly control for the number of men inducted.

To address the potential impact of the Spanish Influenza we include a measure of county-level exposure. To construct the Spanish Flu exposure measure we return to the state health report sources and county-level vital statistics (Eriksson, Niemish, and Thomasson, 2018) to construct an annual county-level panel of all-cause mortality. The growing literature that assesses the impacts of the Spanish Flu in the United States (Almond and Mazumder, 2005; Almond, 2006; Clay, Lewis, and Severnini, 2018; Beach, Ferrie, and Saavedra, 2018; Brown and Thomas, 2018; Corriera, Luck, and Verner, 2020) has thus far relied on either a cohort exposure design or a measure of excess mortality derived from a sample of cities in the Census Death Registration Area. Our all-cause mortality sample enables us to construct a measure of excess county-level deaths with much greater geographic coverage (as described in Beach, Ferrie, and Saavedra, 2018). We then interact the excess mortality measure with year fixed effects for counties in the 25/26 states for which we were able to collect mortality statistics. The county-level mortality sample and is new to the literature.[14,15] We highlight the spatial variation in excess deaths in Appendix Figure 4.

Following the end of WWI and the subsequent expansion of the franchise to women, federal funds flowed to states via Sheppard-Towner to fund women and infant care. With few exceptions, states accepted the federal funds (Moehling and Thomasson, 2012). Often time these funds were funneled through the states by recently formed or expanded State Health Departments. As a result of the expanded public dollars, as well as privately funded public health efforts that were underway, there was a rapid rollout of County Health Offices (CHO) that administered a variety of preventative care measures, educated the public, invested in clean water, and treated communicable disease. Hoehn-Velasco (2018, 2019) documents that the rollout of CHO's reduced infant mortality and increased later life earnings for treated boys. Outside of the public sector, there were

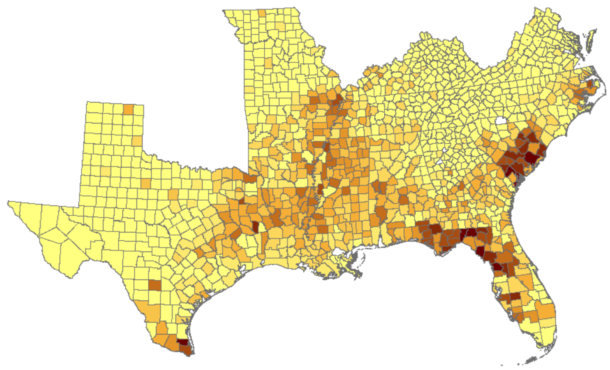
[14] Iowa does not report county-level mortality until 1916, thus we omit Iowa from the sample. Additionally, we also correct the Eriksson, Niemish, and Thomasson (2018) death counts for New York City.
[15] As an alternative, we directly control for the number of deaths in the county-year instead of using the measure of excess mortality.

also major private interventions led by the Rockefeller Sanitary Commission (RSC) that began before the breakout of WWI. In the South in particular, the RSC provided funding and human resources to aid in two eradication campaigns, first hookworm, and then malaria. Bleakley (2007, 2010) and Bleakley and Lange (2009) study how the RSC campaigns affect the returns to schooling and changes in fertility.

To ensure that our estimates isolate the impact of changing agricultural prices rather than a combination of various health initiatives, we specify several robustness checks. First, we estimate a set of regressions controlling for the opening of a CHO at the county level and alternate measures of CHO efforts in the county using the data from Hoehn-Velasco (2019).[16] We have also collected and re-coded the RSF hookworm campaign data at the county level and include measures of the pre-eradication hookworm infection rate, and interact that measure with year fixed effects. To address the potential impact of malaria eradication, we have collected county-level malaria mortality data from a combination of state health reports and state vital statistics reports in 1937. One drawback of measuring malaria intensity in 1937 is that public health efforts ramped up in the early 1930s (Kitchens, 2013), however, the measure is collected before the arrival of DDT and subsequent eradication. The key benefit of measuring malaria in 1937 is that it varies at the county-level, whereas prior work has relied on either state-level measures of malaria (Bleakley, 2010) or imputed data relying on variation in climatic variables when relatively few weather stations existed (Hong, 2007, 2011, 2013), which are effectively collinear with specifications that include state-year effects. In Figure 5, we highlight the spatial variation in malaria and note that our coverage is highly correlated with the implementation of the Malaria Control in War Areas program (precursor to the CDC, see Appendix)[17] and the USPHS's DDT residual spraying program carried out in the post WWII era (Centers for Disease Control, 1948).

[16] These data originally are reported in Ferrell (1932).
[17] https://history.amedd.army.mil/booksdocs/wwii/Malaria/maps/map04.jpg

Figure 6 - 1937 Malaria Mortality per 100,000



Notes: 1937 Malaria mortality at the county level for the state report sample. Data from state health reports and state vital statistics.

Outside of health interventions, there were also targeted education interventions during the period. One major change in the education system was the high school movement and changes to state compulsory education laws. To address any potential changes in the returns to children as educational opportunities change, we include Goldin and Katz's (2008) measure of compulsory schooling. In addition to changes in compulsory schooling laws that varied at the state-year level, there were also targeted interventions that vary at the county-year level. One notable example is the creation of Rosenwald Schools that targeted blacks in the South. There is evidence that these schools improved educational attainment for blacks (Aaronson and Mazumder, 2011) and subsequently impacted fertility and long-run mortality (Aaronson, Lange, and Mazumder, 2014; Aaronson, Mazumder, Sanders, and Taylor, 2017). To ensure that our results are not driven by changes in the returns to education in the South via Rosenwald Schools, we directly control for their presence.

In Table 5, we report the estimates when controlling for these additional confounds. In Panel A, we include the additional controls to our regressions that examine changes in the annual number of births at the county-level, while in Panel B we report estimates using the complete count sample of women under age 35, living in rural areas. In Column 1 we replicate our preferred baseline estimates from Table 3. In Column 2 we report the estimate when restricting to the 26 states in the state health report sample. Column 3 adds controls for the number of men drafted into WWI. In Column 4 we include controls for the exposure to the Spanish Influenza. In Column 5 we include controls that account for the rollout of County Health Organizations. In Column 6, we add controls for the Rockefeller Foundation's Hookworm eradication campaign. In Column 7 we include controls to account for the possible impact of malaria. In Column 8 we report our estimates when controlling for the presence of a Rosenwald School, and in Column 9, we add controls to capture changes in compulsory schooling laws. Moving across the columns, it is clear that the inclusion of these additional controls has little impact on our estimate of the agricultural index on the number of births the following year. Similarly, in Panel B, we report our estimates using the complete count sample with the number of children under age 5 as the outcome of interest. In general, the estimates are qualitatively similar to our preferred specification. When comparing the impact of the Spanish Influenza controls, it is important to compare the coefficient in Panel B, Column 4 to the coefficient reported in Panel B, Column 2, given the underlying differences in the states included in the two main samples. If anything, stronger educational requirements in the form of compulsory schooling laws may reduce the magnitude of the coefficient slightly.

5.4 Long-Run Fertility Effects of Agricultural Boom

We now examine how variation in the price index over the prior 10 years impacts the number of children in the household. A priori, it is not clear if fertility will rise or fall in response to elevated agricultural prices over an extended period. In the long-run, elevated levels of income may allow farmers to adopt new technologies or increase capital intensity to alleviate the strain on labor markets. Alternatively, savings from the boom period may allow families to retime and replace or expand. To the extent that children are complements (substitutes) to the farm capital, the number of children may increase (decrease). Likewise, if farms are more profitable, or pay higher wages, it may influence the decisions of children to stay on the farm longer as they mature, as highlighted by Rosenzweig (1977). Therefore, a change in the number of children in the household could reflect changes in when children exit the household rather than changes in the number of children born.

Table 5 – Short-Run Robustness

Panel A: Y =ln(births)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Ag. Crop Index	-0.0801***	-	-0.0644***	-0.855**	-0.0791***	-0.0722***	-0.0790***	-0.0799***	-0.0838***
	(0.0146)	-	(0.0140)	(0156)	(0.0144)	(0.0146)	(0.0185)	(0.0145)	(0.0143)
WWI Inductions			Y						
Spanish Flu Exposure				Y					
CHO					Y				
Rockefeller Foundation						Y			
Malaria 1937							Y		
Rosenwald Schools								Y	
Goldin and Katz (2003)									Y

Panel B: Y = # children under 5	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Ag. Crop Index	-0.189***	-0.029***	-0.192***	-0.0926***	-0.190***	-0.150***	-0.167***	-0.183***	-0.118***
	(0.0130)	(0.016)	(0.0131)	(0.0156)	(0.0131)	(0.0134)	(0.0132)	(0.0129)	(0.0136)
Same States as County Sample		Y							
WWI Inductions			Y						
Spanish Flu Exposure				Y					
CHO					Y				
Rockefeller Foundation						Y			
Malaria 1937							Y		
Rosenwald Schools								Y	
Goldin and Katz (2003)									Y

Notes: Panel A uses the county-level birth records (N = 23,216) while Panel B uses the 1910-1930 complete count (N = 12,037,770). Every regression includes county and year fixed effects. Panel A controls include separate interactions between year fixed effects and baseline fractions of population who are non-white, between the ages of 6 and 14, and living in an urban area, as well as interactions with a baseline manufacturing output per capita. Panel B controls include dummy variables for age, race, marital status, farm status, and population size of place of residence. The population restriction drops the counties which are in the top 10 percent of population in 1910. The age restriction in Panel B is for women under the age of 35. Each robustness control variable is measured at the county-level. Total WWI inductions are interacted with year fixed effects. Spanish Flu Exposure is measured by the excess mortality from trend, interacted with year fixed effects. In Panel A, (N=21244) due to the loss of observations from Iowa. In Panel B, (N=5,604,615) due to the restricted 25 state sample. CHO is the annual expenditure by County Health Organizations. The Rockefeller Foundation control interacts the percent hookworm infections between 1912-1914 with year fixed effects. The malaria control is described in the text and interacted with year fixed effects. Rosenwald School expenditure is measured annually. Separate dummy variables are used to control for the age at which children could work and how many years of schooling were required for children to receive work permits (Goldin and Katz, 2003). Standard errors clustered at the county-level: *** p < 0.01, ** p < 0.05, * p < 0.1

Measuring the long-run fertility effects with the complete count data recommends slightly different sample restrictions. We would ideally observe completed fertility or the total number of children ever born, but the closest variable collected in each of the Census years from 1910-1930 is the total number of children in the household at the time of the survey. Younger women have not yet finished having children while older women will no longer have all of their children still living at home. For the long-run fertility regressions, we restrict our analysis to women between the ages of 30 and 40. We selected this age window because it will better capture the total fertility of women than the under 35 group. Furthermore, we limit the influence of endogenous household exit of children on our estimates by excluding women who are older than 40. The final two columns of Table 2 show that women between the ages of 30-40 have more total kids in the household but fewer children under the age of five than the under 35 group. They also have more children of any age than the full sample, reflecting the influence of household exit. While the summary statistics suggest that the total number of children for women aged 30-40 is the closest measure to completed fertility reported in the Census, children may have already exited the household at the time of enumeration and some women in the sample have not completed their fertility.

In Table 6, we report estimates that use variation in the average agricultural price index over the previous 10 years, related to the total number of children in the home. Here we find that for a doubling in prices over a 10-year period, there is a 0.29 reduction in total children relative to a sample mean of 2.19 children. Given an average 10-year price index of 1.24, this suggest that on average, there was a 3.17 percent reduction in children. Focusing on women who were between the ages of 30-40, we estimate a negative coefficient of 0.466. Evaluated at the mean number of children and the average price index experienced by women aged 30-40, this implies a 6.05 percent decline in children. Further restricting to rural areas, we estimate a negative coefficient of 0.447, which translates to a 4.97 percent decline in total children. In Column (4), we report the estimate restricting the sample to include the same states used in the county-level annual birth sample. Here the coefficient changes signs, which may be due changes in the composition of the sub-sample or unobservable changes in state policy.[18] Later in the paper we explore sources of heterogeneity some of which may be related to the change in sign. Finally, in Columns (5) and (6), we report estimates from samples that restrict to people living in their state of birth (Column 5), or to mothers born in the United States (Column 6). These restrictions have little impact on the estimated coefficient, alleviating concerns that our results reflect selective migration.

Table 6 - Number of Children in Home

Y = # of children in home	(1)	(2)	(3)	(4)	(5)	(6)
Average Ag. Crop Index	-0.294***	-0.466***	-0.447***	0.285***	-0.537***	-0.545***
	(0.081)	(.098)	(0.046)	(0.066)	(0.0475)	(0.0443)
Age Restriction		Y	Y	Y	Y	Y
Population Restriction			Y	Y	Y	Y
Only States in County Sample				Y		
Live in State of Birth					Y	
Not Foreign Born						Y
Observations	49,088,311	18,136.692	7,886,314	3,534,163	5,258,447	7,315,752

Notes: 1910-1930 complete count census data. Each regression includes county and year fixed effects as well as dummy variables for age, race, marital status, farm status, and population size of place of residence. The population restriction drops the counties which are in the top 10 percent of population in 1910. The age restriction is for women between the ages of 30 and 40. Standard errors clustered at the county-level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

6. Mechanisms

Models of fertility and agricultural production point to a few key mechanisms that would drive our estimated net decline in fertility. First and foremost, a change in the agricultural wage increases the opportunity cost of women's time. While there are no known gender specific wage data for our period, we can at least test for changes in the agricultural wage bill directly and examine two indirect measures that that are strongly correlated with the female wage. First, we can explore how the labor intensity of agriculture in a given location interacts with the fertility response. Second, we can explore how the agricultural shock impacts the timing of marriage.

[18] The estimated coefficient in Table 6, Column 4 is sensitive to the specification. For example with the inclusion of State x Year Fixed effects we estimate a coefficient of -0.274 with a standard error of (0.135).

Fertility would also decline if capital investments were substitutable for child labor. Using data on the value of farm implements at the county level from the agricultural census, we test for changes in capital adoption. Additionally, changes in the costs of raising children could affect the fertility rate. As Dettling and Kearney (2014) and Lovenheim and Mumford (2013) note, housing is one of the largest costs of child rearing. Because the WWI agricultural boom had large impacts on land markets, we explore the potential for heterogeneous impacts for farm owner/operators and renters.

6.1 Agricultural Wages

The Census of Agriculture provides information regarding farm wage bills (which may reflect changes in labor hired or the wage), making it possible to understand how the boom impacted labor costs. Here we test whether the agricultural boom led to higher wage bills for farmers. We construct a county-level panel for the years 1909, 1919, 1925, and 1930 drawing on Haines, Fishback, and Rhode (2018). We consider two different specifications, the first regresses the log of the wage bill on the average agricultural price index over the previous 5 years, while the second regresses the log of the wage bill on a 10-year average of the agricultural price index. In the regressions we control for a variety of important controls interacted with year fixed effects, including: the percent non-white in 1910, share of children in 1910, percent illiterate, percent urban, the value of manufacturing per capita, and, in some specifications, 1910 cotton acreage. We control for cotton acreage in some specifications because cotton was hand-picked during this era and relied on long term relational labor contracts (Alston and Ferrie, 1993), thus, the wage effects may differ.

Table 7 - Agricultural Wage Bill

Y = log wage bill	(1)	(2)	(3)	(4)
Avg. Ag. Index 10 Years	0.300***	0.426***		
	(0.102)	(0.105)		
Avg. Ag. Index 5 years			0.348***	0.403***
			(0.0677)	(0.0738)
Cotton controls		Y		Y

Notes: Data from the 1909, 1919, 1925, and 1930 agricultural census. Each regression has N = 9,745 and includes county and year fixed effects. Each regression includes separate interactions between year fixed effects and baseline fractions of population who are non-white, between the ages of 6 and 14, and living in an urban area, as well as interactions with a baseline manufacturing output per capita. Baseline fraction of crops comprised of cotton controlled for in even columns. Standard errors clustered at the county-level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Our estimates in Table 7 document a sharp rise in the wage bill, ranging from an increase of 30- 34.8 percent nationwide. When we control for cotton acreage, the estimated coefficients increase to 40.3-43.6 percent. While these estimates may sound extreme, anecdotally, they are consistent with backward looking reports by the US Dept. of Labor (1945) that document a doubling of nominal agricultural wages during WWI. While the agricultural wages reported by the Census are not gender specific, these estimates set the stage for increased female labor participation, which would in turn increase the opportunity cost of child bearing.

6.2 Labor Intensity

Until the 1950s cotton and tobacco were primarily hand-picked, while many grain crops were partially or fully mechanized. Women and children played an important role in the cotton harvest, and were often times just as productive as their male counterparts (Olmstead and Rhode, 2018; Logan, 2015). In the spirit of Shultz (1985), this may suggest that the relative wage increase for women working in cotton and tobacco was larger than women working in grain production. Therefore, women in labor intensive locations should experience a larger increase in their opportunity cost of child bearing, resulting in fewer children.

To test this potential mechanism, we construct an index of labor intensity per acre in production. We take the quantities produced of each of the 11 crops that comprise our agricultural price index and multiply them by the number of labor hours used to produce a given quantity, which we draw from the USDA's *Labor Used to Produce Field Crops, Statistical Bulletin No. 346* (USDA, 1964). We then normalize the number of labor hours by the number of acres in production, based on reports in the 1910 agricultural census (Fishback, Haines, Rhode, 2018).[19,20] In Figure 6, we highlight the variation in the labor intensity index. Not surprisingly, places such as the Mississippi River Delta, upper Piedmont of South Carolina, and Tobacco Row in North Carolina exhibit the highest measures of labor intensity per acre in production. To better understand the impact that labor intensity in agriculture has on fertility outcomes, we modify equations (1) and (2) by adding an interaction between the labor intensity measure and the agricultural index.

In Table 8, we report the estimated impact of the agricultural price index and the labor intensity agricultural price index interaction term. As in our baseline, we find that fertility is negatively related to increases in the agricultural price index. We also find that when holding constant the agricultural price index, that there is a further decline in fertility as the labor intensity of agriculture increases. While this is not direct evidence that increases in female opportunity costs are the main mechanism contributing to our estimated decline, it is consistent with the potential mechanism.

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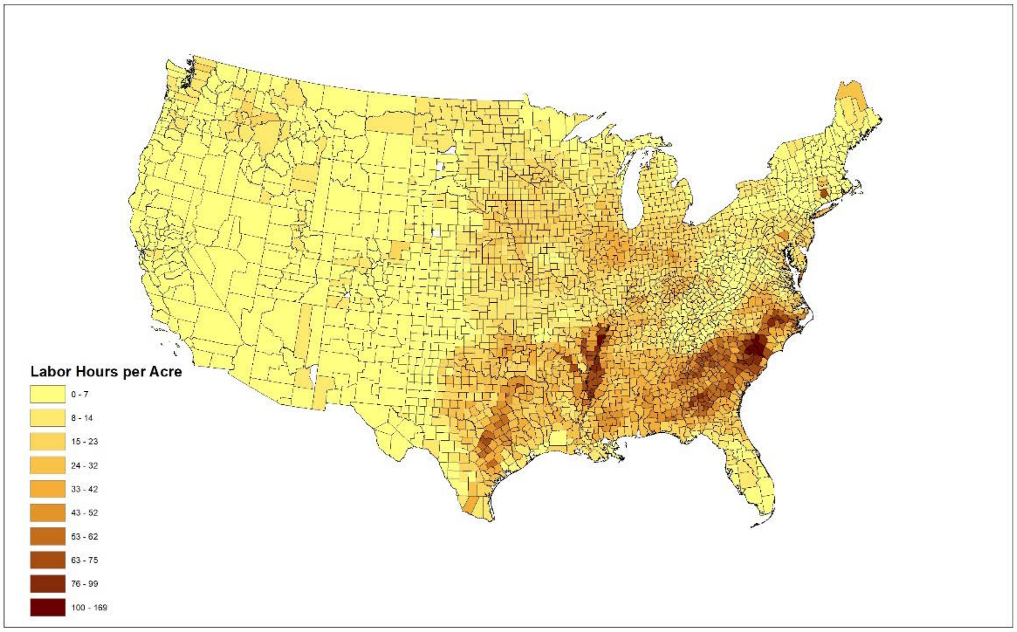
[19] Formally, we define the labor index as follows: $Labor\ Index_{c,1910} = \frac{\sum_{i=1}^{11} Q_{i,1910} \times Hours\ per\ unit_{i,1910}}{Acres_{c,1910}}$

[20] Related work from the modern development literature also highlights the heterogeneous impacts that the labor intensity of crops can have on child investments. For example, work by Kruger (2007) documents that price spikes in raw coffee beans lead to declines in child education. On the other hand, positive price shocks for other crops have been shown to increase the education investments in children (Cogneau and Jedwab, 2012).

Figure 7 - Labor intensity per acre produced, 1910



Notes: The labor intensity index is constructed by taking 1910 output quantities from the Agricultural Census for the 11 crops used to produce the agricultural price index. We then multiply the output by the labor hours required to produce the output using values from the USDA's *Labor Used to Produce Field Crops, Statistical Bulletin No. 346* (USDA, 1964). Finally, we normalize the index by the acres in production in each county in 1910.

Table 8 - Heterogeneity by Agricultural Labor Intensity

	Y= ln(births)		Y = Children Under 5		Y = Total Children	
	(1)	(2)	(3)	(4)	(5)	(6)
Ag. Price Index	-0.080***	-0.068**	-0.189***	-0.0295***	-0.447***	-0.337***
	(0.015)	(0.016)	(0.0130)	(0.003)	(0.046)	(0.052)
Labor Index x Ag. Price Index		-0.0009		-0.0013***		-0.0021***
		(0.0006)		(0.0001)		(0.0005)
Observations	23,216	23,216	12,037,770	12,037,770	7,886,314	7,886,314

Notes: The first two columns use the county-level dataset and the next four columns use the 1910-1930 complete count census data. Columns 3-4 use the under 35 age restriction while the final two columns are restricted to women between the ages of 30-40. The population restriction for each sample drops the counties which are in the top 10 percent of population in 1910. Every regression includes year and county fixed effects. The first two columns include separate interactions between year fixed effects and baseline fractions of population who are non-white, between the ages of 6 and 14, and living in an urban area, as well as interactions with a baseline manufacturing output per capita. Columns 3-6 include dummy variables controls for age, race, marital status, farm status, and population size of place of residence. Standard errors clustered at the county-level: *** p < 0.01, ** p < 0.05, * p < 0.01

6.3 Delayed Marriage

A large literature in labor economics explores the impact that positive wage shocks have on the age of marriage for young women. Increases in the relative wages of women result in marital delay (Becker, 1981; Blau, Kahn, and Waldfogel. 2000; Hankins and Hoekstra, 2011; Jensen, 2012; Salisbury, 2017). Delayed marriage also reduces fertility (Bharadwaj, 2015) by delaying sex or changing the incentives to acquire education. Marriage delay may also lead to changes in intra-household bargaining resulting in smaller families (Ashraf, Field, and Lee, 2014; Hahn et al., 2018).

To test whether the agricultural boom delayed marriage, we take advantage of a question in the 1930 Census that asks women their age at first marriage. Using this measure, we estimate the relationship between the agricultural price index, 1910-1919, and age of first marriage for the subset of women who were of marital age and unmarried at the outbreak of WWI. More specifically, our sample consists of women who were at least 15 years old in 1915. The regressions control for a set of age, race, size of place, and farm status indicator variables.

Table 9 - Marriage Delay

Y = Age of 1st Marriage	(1)	(2)	(3)	(4)
Avg. Ag. Index X Not Married 1915	1.436	4.668***	5.419***	8.535***
	(1.029)	(0.326)	(0.341)	(0.0492)
Avg. Ag. Index X Not Married 1915 X Labor Intensity Index				0.292***
				(0.00346)
Controls	Y	Y	Y	Y
Population Restriction		Y	Y	Y
Marriage Cutoff Restriction			Y	Y
Observations	11,393,987	4,614,974	4,567,536	4,567,536

Notes: The age of first marriage is taken from the 1930 complete count. The average index over 1910-1919 is interacted with a dummy variable for not being married in 1915; each column is restricted to women who were 15 or older in 1915. The population restriction drops the counties which are in the top 10 percent of population in 1910. Column 3 restricts the sample to women who were married by 1930. Standard errors clustered at the county-level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.01$

In Table 9, we report our estimates that relate the agricultural price index to the age of first marriage. Column 1 reports the estimate for all women who were at least 15 years old and unmarried as of 1915. Across the entire population of women, there is no statistically significant delay in marriage. However, many of the young women in this sample are living in cities. In Column 2, we restrict the sample to women living in rural areas and estimate a positive and statistically significant, 4.668 year delay in marriage. During the period 1910-1919, the average of the agricultural index was 1.47, thus, for every year exposed to the average boom, marriage is delayed by 0.22 years. In Column 3, we restrict the sample to women who married by 1930.[21] When we make this restriction the estimated delay in marriage increases to 5.419 years. Scaling the coefficient based on the mean boom 1910-1919, suggests that for every year a women was exposed to the average boom, they delayed marriage by 0.25 years. Our finding of delayed marriage stemming from agricultural shocks is consistent with recent work by Corno, Hildebrandt, and Voena (*forthcoming)*. If increases in opportunity costs are the main driver of marriage delay, marriage should be delayed more in places where agriculture is more labor intensive. In Column 4, we report the estimates of a triple interaction between the agricultural labor index, the crop price index, and an indicator for being unmarried as of 1915. From this specification, it is clear the marriage is delayed even further in places where agriculture is more labor intensive.

6.4 Capital Adoption

While rural areas traditionally lacked credit to finance fixed capital investments, both Rajan and Ramcharan (2015) and Jaremski and Wheelock (*forthcoming)* point out that the WWI Boom increased the availability of lending and new unit banks opened to fund land and capital purchases during the boom. Indeed a doubling of agricultural prices increased lending by approximately 24 percent. Given the tight labor markets, availability of cash, and expanded access to savings, it is possible that farmers were able to increase the rate of capital adoption. As we described previously, the interwar period is the era of most rapid tractor diffusion in the 20th Century. The adoption of additional capital could partially explain the decline in fertility if farm capital substitutes for labor. From the agricultural Census, we take a measure of farm capital, the value of farm implements, and regress it on the average agricultural price index over the previous 5

[21] Non-responses or never-married women are coded as zeros, which explains why the delay increases when they are removed from the sample.

and 10 years. The baseline regressions include controls for county and year fixed effects. In additional specifications, we also control for 1910 cotton acreage, interacted with year fixed effects. We do this because cotton had one of the largest boom/bust cycles, however, due to the available technology, was unable to mechanize until the 1950s.

In Table 10, we report our estimates using the log value of farm implements as the dependent variable. In Column (1) and Column (3) we report the estimates excluding the cotton acreage control. We find that on average there is no statistically significant relationship between the agricultural price index and the value of farm implements. However, when the cotton acreage controls are included in Column (2) and Column (4), the estimated coefficient is positive and statistically significant. The estimate suggests that a doubling of agricultural prices over a 5 and 10-year period increases capital investment by between 36.7 and 42.5 percent. These estimates suggest that outside of the South, capital was rapidly adopted.

Table 10 - Changes in the Value of Farm Implements

Y = ln(value of farm implements)	(1)	(2)	(3)	(4)
Avg. Ag. Index 10 Years	-0.00117	0.425***		
	(0.0878)	(0.0849)		
Avg. Ag. Index 5 years			0.0847	0.367***
			(0.0582)	(0.0551)
Cotton controls		Y		Y

Notes: The value of farm implements is taken from the 1909, 1919, 1925, and 1930 agricultural census. The first two columns use the average agricultural price index over the previous decade while the last two columns use the average index over the previous five years. Each regression has N = 9,751 and includes county and year fixed effects. Each regression includes separate interactions between year fixed effects and baseline fractions of population who are non-white, between the ages of 6 and 14, and living in an urban area, as well as interactions with a baseline manufacturing output per capita. Baseline fraction of crops comprised of cotton controlled for in even columns. Standard errors clustered at the county-level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Earlier in the paper, we found that in our complete count samples, when restricting the sample to the same states that appear in the annual county-level sample, that the estimated magnitudes were smaller (5 Year Average) and at times became positive (10 year). Our estimates regarding the value of implements may suggest that when possible, capital substituted for child labor and alleviated the labor supply shortage in the medium run.

6.5 *Ownership Effects*

To test for the differential impact that owners and renters experienced during the agricultural boom, we modify equations (1) and (2) by including an interaction term that measures the share of farms that are owner-operated in the county in 1910. We report the results of this modified specification in Table 11. Starting with the county-level birth sample in Panel A, we continue to estimate a negative relationship between the agricultural price index and fertility. Focusing on the interaction term, our estimates using the annual county level sample are small and statistically insignificant.

In Panels B and C, we report our estimates from the complete count samples that focus on the number of children under age 5 and total children. As before, we continue to estimate a negative and statistically significant relationship between the agricultural index and fertility, but farm ownership effects appear to be important in these lower panels. The coefficients in Panel B indicate that a move from zero percent owner-occupied farms to 100 percent owner-occupied firms would cut the decline in fertility by between half and two-thirds. This suggests that the wealth/income effects are partially offsetting the dominant substitution effect. Similarly, in Panel C, we find that moving from zero to complete ownership also reduces the magnitude of the decline in fertility, however, its effect is smaller over the 10-year time horizon.

Table 11 - Heterogeneous Impacts by % Owner Operator

Panel A: Y = ln(County Births)	(1)	(2)	(3)
Ag. Crop Index	-0.0679*	-0.0814*	-0.0766**
	(0.0352)	(0.0380)	(0.0349)
% Owner Operated X Index	0.0326	0.0397	-0.0045
	(0.0429)	(0.0459)	(0.0433)
Pop. Restriction		Y	Y
Controls			Y
Observations	27,969	23,976	23,239

Panel B: Y = # Children Under 5	(1)	(2)	(3)
Average Ag. Crop Index	-0.146***	-0.240***	-0.198***
	(0.0158)	(0.0221)	(0.0119)
% Owner Operated X Index	0.105***	0.137***	0.134***
	(0.0169)	(0.0188)	(0.00077)
Controls	Y	Y	Y
Under age 35		Y	Y
Rural Restriction			Y
Observations	49,088,311	25,465,610	12,037,770

Panel C: Y = # Children	(1)	(2)	(3)
Average Ag. Crop Index	-0.329***	-0.493**	-0.501***
	(0.0822)	(0.100)	(0.0477)
% Owner Operated X Index	0.0765***	0.0635***	0.0838***
	(0.0152)	(0.0174)	(0.0114)
Controls	Y	Y	Y
Aged 30-40		Y	Y
Rural Restriction			Y
Observations	49,088,311	18,136,692	7,886,314

Notes: Panel A uses the county-level dataset while Panels B-C use the 1910-1930 complete count data. The percent owner operated variable is taken from the 1910 agricultural census and is measured at the county-level. Every regression includes county and year fixed effects. Panel A controls include separate interactions between year fixed effects and baseline fractions of population who are non-white, between the ages of 6 and 14, and living in an urban area, as well as interactions with a baseline manufacturing output per capita. Panels B and C controls include dummy variables for age, race, marital status, farm status, and population size of place of residence. The population restriction drops the counties which are in the top 10 percent of population in 1910. Standard errors clustered at the county-level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

7. Conclusions

Identifying the causal relationship between income and fertility continues to be an important goal for economists, one with substantial implications for demography and policy design. In this paper, we presented evidence that income shocks with the potential to disproportionately change the opportunity costs of women can have significant impacts on fertility decisions. The agricultural boom-and-bust of WWI, which differentially affected the output prices of farms across the US based on their preexisting composition of crops, provided the necessary exogenous variation to study how families responded to income shocks. Analysis using newly-digitized annual birth records revealed that counties with larger price shocks experienced larger fertility declines in the short-run. This pattern was confirmed using an alternative measure of short-run fertility (children under the age of 5) in the complete count Census data. An important follow-on question is whether women simply retimed their fertility to take advantage of temporary wage increases. This does not appear to be the case: results focusing on the total number of children in the household from the complete count support the conclusion that these price shocks lowered the long-run fertility of affected women.

In addition to the main analysis of how the price index affected fertility, we investigated potential mechanisms which could partially explain the estimates. The most direct channel is arguably farm wages, which we found to increase the most in counties most affected by the price shock. As discussed above, there is anecdotal evidence of considerable wage equality in agriculture during this time period, meaning women in our sample faced higher wages, and therefore higher opportunity costs of childbearing, directly. We also found that places with higher labor intensity and lower ability to mechanize had larger negative fertility responses, consistent with the opportunity cost theory. Pushing in the other direction, locations with higher rates of owner-operated farms had relatively weaker negative responses to the price shocks. One way of interpreting this pattern is that the income effect generated by the price shock has a positive impact on fertility but that the substitution effect is overwhelmingly dominant. Lastly, we found that the price shocks led to modest delays in marriage, another possible explanation for lower fertility across multiple time horizons.

Much of the literature on fertility and family size focuses on the theoretical tradeoff between quality and quantity or how costs, both opportunity and explicit, have changed over time. When children are also inputs to the household production function rather than merely inputs to the parental utility function, the decision to have additional children becomes more complex. This additional dimension is still present in agricultural societies today even if many other factors (cultural, institutional, etc.) differ from those in the US in the early 20th century. Although the roles and expectations of children have changed substantially over the last century, parents still consider economic opportunities when deciding how many children to have. As policymakers consider potential responses to the many challenges that declining fertility presents to existing social programs, the results of this paper serve as a reminder that gender-specific changes to opportunity costs are central to the discussion. Put another way, there is more to the relationship between income and fertility than just income effects.

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Appendix Table 1 - State Entry into Birth Registration Area and State Health Report Data

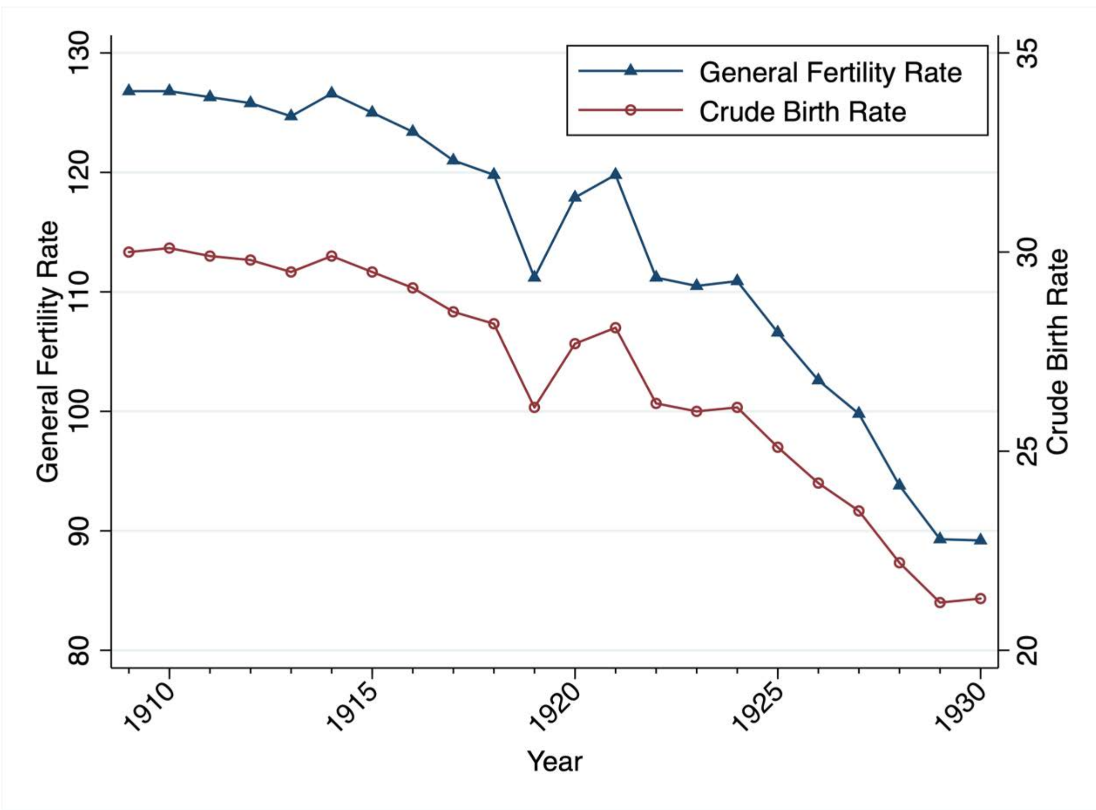
Year of Entry into Birth Registration Area	State	State Health Report Years	Year of Entry into Birth Registration Area	State	State Health Report Years
1915	Connecticut	1910-1914	1917	Indiana	1910-1916
	Maine	1910-1914		Kansas	1912-1916
	Massachusetts	1910-1914		Kentucky	1911-1916
	Michigan	1910-1914		Virginia	1912-1916
	Minnesota	1910-1914		Washington	1910-1916
	New Hampshire	1910-1914		Wisconsin	1910-1916
	New York	1910-1914	1919	California	1910-1918
	Pennsylvania	1910-1914		Oregon	1910-1918
	Rhode Island	1910-1914		New Jersey	1910-1920
	Vermont	1910-1914		Wyoming	1911-1921
1916	Maryland	1910-1915		Iowa	1910-1925
			1925	West Virginia	1910, 1912-1924
				Missouri	1911-1926
			1929	Nevada	1911-1922, 1923-1928
			1932	South Dakota	1910-1930

Appendix Table 2 - Heterogeneity by share of County Acreage in Farms

Y =ln(births)	Baseline	>50% Farms	>75% Farms	>90% Farms
Ag. Price Index	-0.0801***	-0.0697***	-0.0727***	-0.107**
	(0.0146)	(0.0174)	(0.0279)	(0.0473)
Observations	23,239	16,159	9,227	3,607

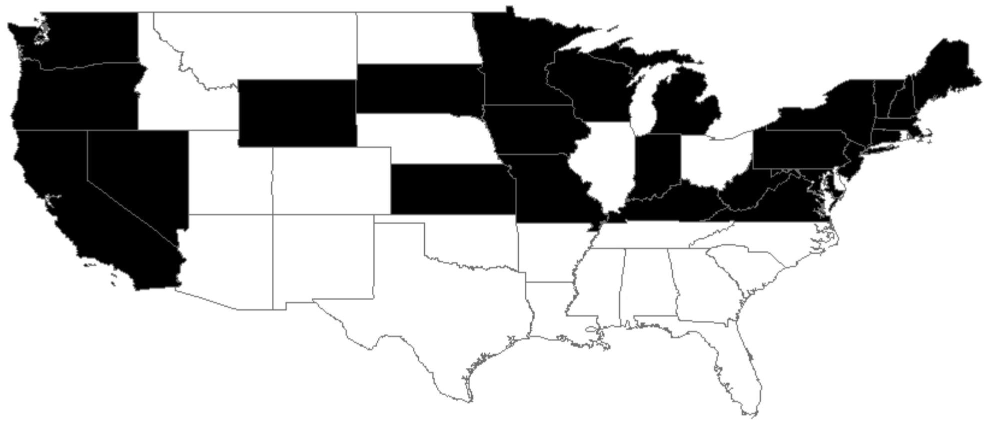
Notes: Estimates using county-level birth records. Restrictions moving from left to right are based on the fraction of land used for farming in the county. Each regression includes county and year fixed effects as well as separate interactions between year fixed effects and baseline fractions of population who are non-white, between the ages of 6 and 14, and living in an urban area, as well as interactions with a baseline manufacturing output per capita. Standard errors clustered at the county-level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendix Figure 1 - US Fertility and Crude Birth Rate, 1910-1930



Notes: National Vital Statistics System data (NVSS, 2017). The general fertility rate is the number of births per 1,000 women aged 15-44. The crude birth rate is the number of annual live births per 1,000 people in the country's population.

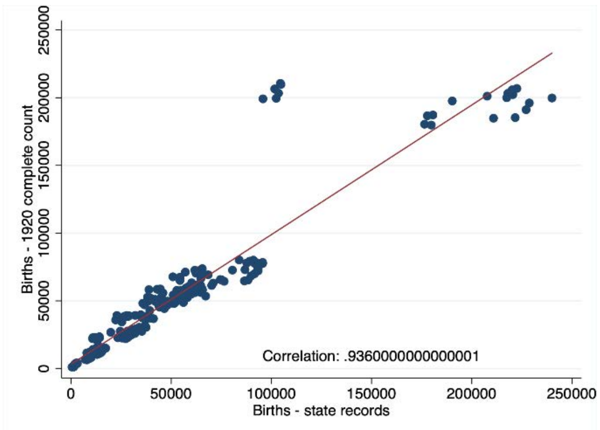
Appendix Figure 2 - States in County Birth Sample



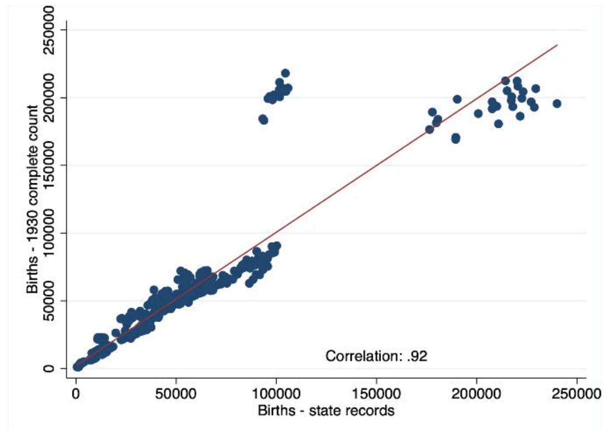
Notes: States shaded in black appear in our county-year birth sample, covering the years 1910-1930.

Appendix Figure 3 - Correlation between Births reported in State Health Reports and State of Birth in 1920 and 1930 Censuses with and without NYC Correction

Panel A: Correlation without NYC Correction

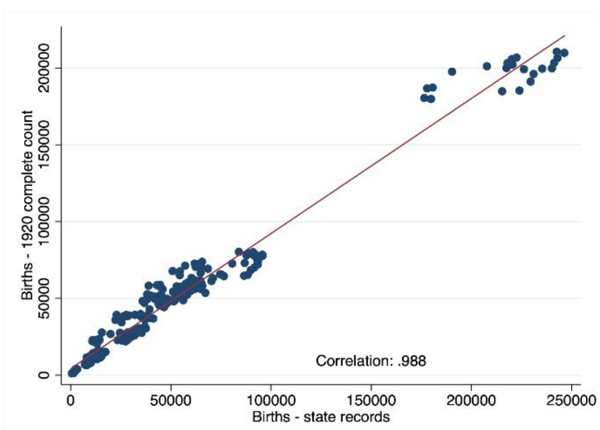


(a) 1920

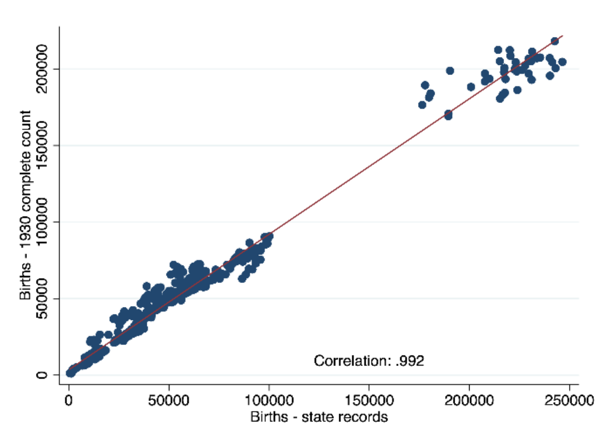


(b) 1930

Panel B: Correlation with NYC Correction



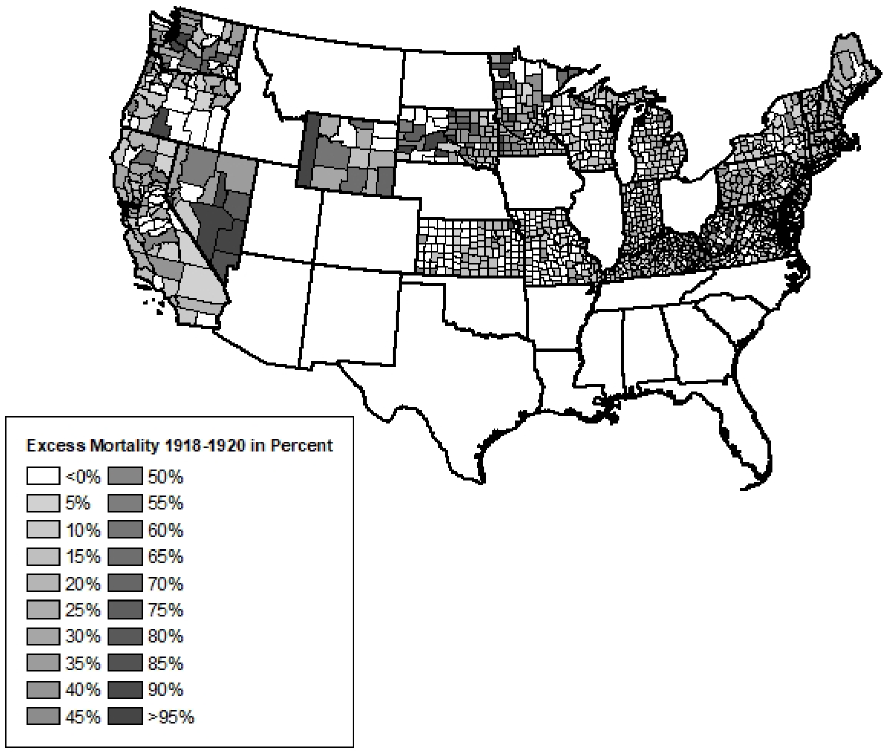
(b) 1920



(b) 1930

Notes: Newly digitized birth counts from state health reports compared to complete count census data. The New York City correction accounts for changes in how the populations of NYC boroughs were recorded across the different datasets. The results are similar with or without this correction.

Appendix Figure 4 – County-Level Excess Mortality 1918-1920, Spanish Influenza



Notes: We follow Beach, Ferrie, and Saavedra (2018) to construct the excess mortality from trend 1918-1920 to capture the local intensity of the Spanish Influenza. The underlying mortality data is drawn from a combination of newly digitized all-cause mortality death county level counts from various state health reports in 25 states and Eriksson, Niemesh, and Thomasson (2018).

Exhibit 1: Female Wage in California – The Californian 1918

WOMEN WILL BE REGISTERED FOR FARM WORK

Task Will Be Accomplished By Women's Land Army of America

JOBS FOR THE JOBLESS

Organization Demands "Going Wages" For All the Workers Sent Out

All able-bodied women over 18 years of age who will be available for farm work in 1919 are to be registered in northern California during the coming winter, if plans made at a conference of the county chairman and advisory board of the women's land army of America, northern California district, are realized.

The women's land army is ready to fill all demands for women workers for farms, packing houses and canneries this season, it was announced after the conference. Mrs. Katherine P. Edson, member of the land army's advisory board and executive officer of the state industrial welfare commission, said that standards of wages, housing and hours had been fixed and that no disturbance of labor conditions would result from the employment of women.

The "going wage" is demanded for the women workers sent out by the army, said Mrs. Edson, and their services will be offered in wage dispute cases only when male workers refuse the awards of United States mediators.

Mrs. Sydney Joseph of Berkeley, chairman, explained that, while a $2 minimum is fixed for women's land army corps workers, they must be given the wage commonly paid male workers. Other regulations announced include: A standard eight-hour day, overtime not to exceed two hour at the rate of one and a quarter times the regular wage, and living conditions conforming to the state housing and immigration commission's standards.

Methods for registering women to assist in harvesting 1919 crops will be determined at later conferences, it was announced.

Exhibit 2: Prevailing Agricultural Wages- Baltimore Sun 1919

HIGHEST FARM WAGES IN HALF CENTURY PAID IN 1918

WAGES for farm labor in the United States have more than doubled since 1902 and have increased 43 to 64 per cent. for the different classes of hiring since 1916, or 53 per cent. for farm labor in general. These comparisons are warranted by the results of a recent investigation made by the Bureau of Crop Estimates, United States Department of Agriculture.

For 1918 the wage rate per month with board was $34.92, without board $47.07; per day in harvest with board $2.65, without board $3.22; per day out of harvest with board $2.07, without board $2.63. These are averages for the United States. The highest rates were in the far West, and next below are those of the west North Central States. The wage rates of the South Atlantic States were lowest and were a little below those of the South Central States, as State-group averages. A record of 53 years of farm wages places 1918 at the top, and far above the highest rates of the half century before 1916. Wage earnings measured by purchasing power may warrant a different statement.

While the wages of farm labor have greatly increased in only two years and have doubled in 16 years, it is interesting to observe that from the time of low-water mark in farm wages, 25 years ago in the great industrial depression of 1893-1897, the rates have increased from 165 to 229 per cent. for the different classes of hirings.

Day wages on farms increased in greater degree than month wages did during the last two years, and this is true for the last 16 years, except for month wages with board. Wages by the day for work outside of harvest have advanced more strongly than for harvest work. Apparently the procurement of

https://blogs.loc.gov/loc/2018/03/world-war-i-the-womens-land-army/

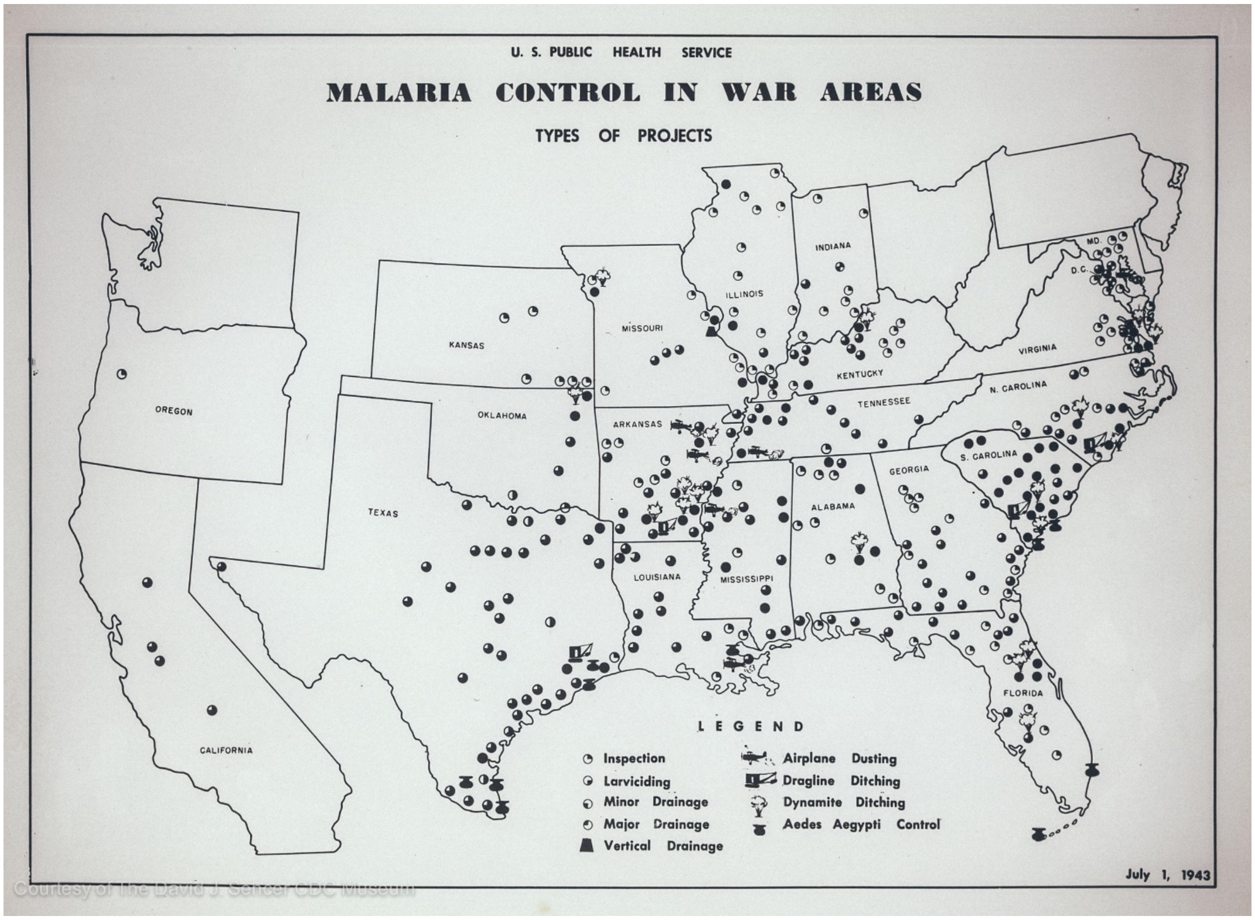
https://www.kcet.org/history-society/the-womens-land-army-farmettes-for-suffrage-during-world-war-i


NATIONAL SERVICE
WOMEN'S
LAND ARMY
"GOD SPEED THE PLOUGH
AND THE WOMAN WHO DRIVES IT"
SERIES W.4
APPLY FOR ENROLMENT FORMS AT YOUR NEAREST POST OFFICE OR
EMPLOYMENT EXCHANGE

BOOKLET OF INFORMATION

U.S. BOYS' WORKING RESERVE

U.S. EMPLOYMENT SERVICE
U.S. DEPARTMENT OF LABOR



U. S. PUBLIC HEALTH SERVICE
MALARIA CONTROL IN WAR AREAS
TYPES OF PROJECTS
OREGON
CALIFORNIA
KANSAS
OKLAHOMA
TEXAS
MISSOURI
ARKANSAS
LOUISIANA
ILLINOIS
INDIANA
KENTUCKY
TENNESSEE
MISSISSIPPI
ALABAMA
GEORGIA
FLORIDA
S. CAROLINA
N. CAROLINA
VIRGINIA
MD
D.C.
LEGEND
Inspection
Larviciding
Minor Drainage
Major Drainage
Vertical Drainage
Airplane Dusting
Dragline Ditching
Dynamite Ditching
Aedes Aegypti Control
July 1, 1943
Courtesy of The David J. Sencer CDC Museum