Rainfall risk, fertility and development: Evidence from farm settlements during the American demographic transition*

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Abstract

I analyze whether variation in rainfall risk played a role for the demographic transition. The hypothesis is that children constituted a buffer stock of labor that could be mobilized in response to income shocks. Identification relies on fertility differences between farm and non-farm households within counties and over time. The results suggest that in areas with a high variance in rainfall the fertility differential was significantly higher than in areas with a low variance in rainfall. This channel is robust to other relevant forces and the spatial correlation in fertility. The effect disappeared as irrigation systems and agricultural machinery emerged.

Key words: Rainfall risk; Insurance; Fertility; Demographic transition.

JEL codes: J13, N51, O12, Q12

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1. Introduction

In developing economies geography can pose important threats to agricultural households especially if adequate technology and institutions such as irrigation systems or formal insurance are missing. In the absence of irrigation systems for example rainfall variability forces farm households to either undertake measures to reduce the exposure to risk ex-ante, by diversifying production or by choosing low-risk low-return technologies (Dercon, 1996; Dercon and Christiaensen, 2011; Porter, 2012), or to make sure that once a rainfall shock occurs the household can rely on some smoothing mechanism or some form of insurance. Smoothing might be possible if the household has access to durables, savings and credits (Rosenzweig and Wolpin, 1993; Udry, 1994; Morduch, 1995) and insurance might be organized within the own social network (Rosenzweig and Stark, 1989; Townsend, 1994; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006), but both rarely work perfectly. This is well documented in the literature (Dercon, 2004). Other mechanisms include labor reallocation (Kochar, 1999; Rose, 2001; Minale, 2018).

This article deals with the question if in such a context farm households also adjust their demand for children. Fertility decisions have been explored as a way of ensuring old-age security (e.g. Hohm, 1975; Nugent, 1985; Jensen, 1990; Rendall and Bahchieva, 1998), but almost never whether they are also made in view of insuring income shocks that occur much earlier in life and which are hard to predict such as an income shock due to a shortage in rainfall, a crop disease or a case of serious illness. This fertility motive was first discussed by Cain (1981, 1983) and, more generally, by Pörtner (2001). The idea is that children can be used to smooth consumption over time by providing additional labor in the event of a shock. Children can also free up the time of older household members by taking on household chores such as cooking, cleaning or taking care of their younger siblings. De Vany and Sanchez (1979), Cain (1990) and Dasgupta (1995) provide descriptive evidence in support of this motive for the context of developing countries. More robust evidence comes from Pörtner (2011) who uses the geographical variation in hurricane risk across municipalities in Guatemala to examine the effect of risk-exposure on fertility in a cross-section of households. He finds that an increase in risk leads to higher fertility in households with land.

In this paper, I focus on the United States. I empirically analyze to what extent the exposure to rainfall risk shaped fertility patterns among American farmers in the second half of the nineteenth and first half of the twentieth century and whether the emergence of risk-mitigating devices such as irrigation systems, agricultural machinery and financial services boosted the demographic transition by reducing the need to have many children. Although the mechanism is unlikely to be the only force in the demographic transition, it could be an important element to understand differences in the pace of the fertility decline in farm and non-farm households and across different climate zones.

Identification relies on fertility differences between farm and non-farm households within counties and over time. Counties differ in their rainfall variability which is calculated using historical time series of monthly rainfall data for 4x4km grid points. The analysis accounts for county fixed-effects, state-specific time-effects as well as spatial correlation in fertility levels. However, as the empirical design does not allow to fully rule out any unobserved variable bias the results should not be interpreted in a strictly causal sense, but they are quite robust to alternative specifications, various robustness checks and placebo tests.

This study adds to the literature that has explored households' strategies to cope with anticipated weather risks ex-ante in a poor agrarian context (see e.g. Dercon, 1996; Dercon and Christiaensen, 2011; Porter, 2012). Examining the role of the insurance motive for the American demographic transition is particularly interesting as at the time most rural households generated their income from agriculture, particularly households in the Central region of the country, and hence had to cope with ample risks and uncertainties of which many were linked to geography. The most well-known crises people had to go through were the widespread death of cattle in 1886, the influenza pandemic in 1918, the agricultural recession in 1920 and the immense drought in the 1930s, also known as the dust bowl (Thomson and Whelpton, 1933).

This study also adds to the vast body of literature that has analyzed the determinants of the demographic transition in the US.[1] It is also related to two other recent papers that looked at households' behavior and risk during the nineteenth century in America. Ager and Ciccone (2018) show that religious membership strongly varied with risk exposure, suggesting that Americans at that time also tried to cope with risks through informal insurance schemes that were set up within religious communities. Basso et al. (2014) show that fertility decisions by Americans (using data from eight Northeastern states) varied according to the degree of

[1] See e.g. Yasuba (1962), Forster and Tucker (1972), Easterlin (1976, 2000), Vinovskis (1976), Guest (1981), Bourne Wahl (1992), Steckel (1992), Haines (2000), Greenwood and Seshadri (2002), Hacker (2003), Haines and Hacker (2006), Johnson and Rathge (2006), Jones and Tertilt (2006), Curtis White (2008), Bleakley and Lange (2009), Wanamaker (2012), Aaronson et al. (2014), Hansen (2014), Hansen et al. (2014), Lahey (2014) and Ager et al. (2015) among many others.

financial development, i.e. in those areas where the financial system was already more advanced, fertility rates were much lower. The authors interpret their findings as evidence in support of the old age insurance motive.

The remainder of the paper is organized as follows. Section 2 provides context and a conceptual framework. Section 3 presents the data. Section 4 outlines the empirical identification strategy. Section 5 presents the results and various robustness checks. Section 6 concludes.

2. Context and conceptual framework

2.1 Context

This study focuses on the period between the Civil War and the Great Depression. Whereas the Northeast, the Mid-Atlantic, the Southeast and the West were already quite densely settled, the South, North and Midwest were still subject to substantial population movements (Gallaway et al., 1974). The fertility transition was relatively advanced in New England, but in the rest of the country, particularly in the South, the total fertility rate was well above four children per woman (Jones and Tertilt, 2006). Fertility was generally higher in new settlement regions than in old settlement regions even when only considering rural areas. Easterlin (2000) argued that this can be explained by land abundance and high returns in agricultural production in newer settlement regions which in turn allowed for the generous bequeathing to many children. In contrast, in older areas, land started to become scarce and farmers became increasingly concerned about providing for their children and hence reduced fertility. Similar arguments were made by Yasuba (1962) and Forster and Tucker (1972). Sundstrom and David (1988) and Bourne Wahl (1992), among others, however argued that fertility rather declined in these areas as a result of improved labor market opportunities outside agriculture which reduced children’s incentives to stay on the farm. The value of children as old-age security assets consequently diminished.

In the second part of the nineteenth century, the country was, with the exception of the Northeast, still very rural, with more than half of the population working in agriculture, typically on small family-owned farms relying only on horse and manpower (Perelman, 1973; Dimitri et al., 2005; Dempster and Isaacs, 2014). This started to change rapidly at the beginning of the twentieth century as innovations such as the tractor, new varieties and cultural practices, fertilization and other sophisticated inputs emerged which increased productivity substantially and allowed for larger farms (Cochrane, 1979; Olmstead and Rhode, 2001, 2002; Dimitri et al., 2005). According to Olmstead and Rhode (2001), the horsepower provided by tractors rose from roughly 2.5 million hp in 1920 to 11 million hp in 1930 and was one of the great labor-saving innovations in the twentieth century. Since 1900, new technologies and the development of rural infrastructure brought farming households closer to markets for labor and capital, as well as goods and services. This positive dynamic was abruptly stopped with the start of the Great Depression during which prices for food and cash crops collapsed.

In the second half of the nineteenth century it was normal for children to help on farms. Children also increasingly worked in industry where such opportunities existed. Many children combined work with schooling (Walters and O'Connell, 1988). Compared to households in the Northeast region, child work was somewhat more common in the Midwest and substantially more common in the Southern states (Horan and Hargis, 1991). School enrolment in the Southern states was substantially lower. Laws regulating child labor existed in some states but were typically only strictly enforced after the Great Depression to avoid children taking the jobs of adults (US Department of Labor, 1968). The key idea of this paper is that in the event of shocks, children constituted a buffer stock of labor. This mechanism is discussed in more detail in the following sub-section.

2.2 Conceptual framework

In a context in which formal insurance mechanisms are absent, poor rural families that are exposed to serious risks such as rainfall shortages, hurricanes, flooding or an epidemic need to develop strategies to cope with such shocks. The literature shows that historically, households have developed many different strategies. Ex-ante, households may diversify their economic activities to ensure that if one activity is hit by a shock at least one alternative income source remains (Dercon, 1996; Dercon and Christiaensen, 2011; Porter, 2012). However, this, of course, prevents full specialization and the consequent realization of economies of scale. Ex-post, households may sell assets, dissave or obtain credit (Rosenzweig and Wolpin, 1993; Udry, 1994; Morduch, 1995). Selling assets may erode the future income base if these assets are used in production. Dissaving and obtaining credit require some banking services which, again, are often not available in poor rural areas, just as they were not available in the 19th century in the countryside in many industrializing countries. Informal risk sharing networks would only work to the extent that the persons providing the support lived sufficiently far away to avoid being affected by the same shock (Rosenzweig and Stark, 1989; Townsend, 1994; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006). Households can also increase their labor supply or hire more labor (Kochar, 1999; Kijima et al. 2006; Minale, 2018; Rose, 2001).

Hence increasing the number of children as an ex-ante measure to increase the potential labor force might be a useful strategy, as children in rural areas can take on tasks quite early in life. They can, for instance, engage in herding, cooking, cleaning or taking care of their younger siblings (Walters and O'Connell, 1988). By engaging in household chores, they can also free up the time of other household members who can then offer their labor on others' farms or in the non-agricultural sector. Once they have grown up and formed their own households, children can pay remittances (Horan and Hargis, 1991). How much they can pay may depend on the number of children parents have and the children's human capital.

As a conceptual framework it is useful to think in terms of three periods. In each of these periods households face uncertainty about their income. In the first period, parents decide on the number of children and their education and raise their children through to secondary school age. In the second period parents continue to raise their children and possibly continue to send them to school, but the children increasingly contribute to the household income either by engaging in household chores and thus freeing the time of older household members who can use this for farm or non-farm activities, or by participating themselves in farm and non-farm activities. In the third period, the children are grown up, have moved out of their parents' household and formed their own. Yet they continue to support their parental household through remittances in cash or in kind, or by providing labor whenever needed. Parental productivity may go down during this period. The children's ability to generate income will also depend on parental investments in their education as education is assumed to be useful for employment in the industrial and service sectors, but less so in agriculture where manpower is more important.

Raising children is assumed to be costly in the first period, but in the second period benefits will increasingly outweigh costs and in the third period children provide only benefits without generating any further costs. However, the cost of raising children will be relatively low on farms compared to urban households since food is cheaper and often self-produced, housing is cheap and childcare costs are low (Becker, 1981). Moreover, the literature shows that there are substantial economies of scale in raising children, i.e. the second, third and consecutive children cost only small fraction of the first. Henderson (1950), for instance, estimates for the

1930s in the UK that the second child costs only two thirds of the first. Espenshade and Calhoun (1986) estimate in poor American families that the cost of the second and third child is less than half of the first. Moreover, in times of economic hardship, expenses on children can, of course, also be reduced, for instance by withdrawing them from school or cutting non-essential expenditures. At the same time, the potential benefits of having children are high, as farms offer plenty of opportunities, even for adolescent boys and girls, to engage in productive activities (Rosenzweig, 1977). In fact, if parents are risk averse, even a negative return might increase welfare, if having children reduces income variance. Parents may also simply derive utility from having children.

Hence, based on these assumptions, parents in farming households will invest in the number of children as insurance against shocks in the second and third period. In other words, in the context at hand, it is assumed that children constitute, at least in farming households, a cheap, trusted and flexible labor buffer. There is also plenty of evidence from developing countries today that children's labor supply is a common ex-post coping strategy (see e.g., Beegle et al., 2006; Edmonds, 2006; Guarcello et al., 2010; Duryea et al., 2007; Landman and Frölich, 2015). Fitzsimons (2007) for example, shows for Indonesia that children in 'high risk' villages are less educated and fulfill an insurance role in their households. Jacoby and Skoufias (1997) provide evidence for India that seasonal fluctuations in schooling are the consequence of self-insurance. Parents in non-farm households, on the other hand, may instead invest in the education of their children and favor fewer children as they face higher costs in raising children and fewer opportunities to use their children's time productively.

Obviously, children may also die before they reach adolescence. This may lead parents that are risk averse and expect to lose one or several children to choose to have more children, so they can be sure to reach their target (as in Pörtner, 2001). In the literature this is called "hoarding" and the empirical evidence seems to confirm that this is a frequent reaction in high mortality environments (Wolpin, 1998). Mortality makes the investment in children somewhat uncertain but overall this strategy does not seem to offer much more uncertainty than other informal risk-coping strategies.

Hence, the first research hypothesis is that, whilst controlling for farm assets, farm households in areas with a high exposure to rainfall risk, and hence a higher variance in income, have more children than farm households in areas with a low exposure to rainfall risk and hence a lower variance in income. The second research hypothesis is that the emergence of risk-mitigating devices and labor-saving machinery reduces this effect as it reduces the income variance and the return on child labor. The assumption is that these hypotheses hold even after accounting for a possible risk-related income level effect on fertility. It is important to note that what is postulated here is a link between the perceived exposure to risk and fertility, and not between actual income shocks and a possible fertility response.

3. Data

I use four different data sources: population census data, agricultural census data, data from the survey on banks and bank deposits and geographic information, especially rainfall data. I present each source in turn.

Historical population census data is available from the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 1990). I use the data from the years 1870, 1880, 1900, 1910, 1920 and 1930. IPUMS provides 1% samples in each year. Additionally, for 1880 IPUMS provides a 10% sample and for 1900 and 1930 a 5% sample each. Hence for 1880, 1900 and 1930 I use the larger samples.[2] The 1890 census records were destroyed by fire and flooding and are not available for analysis. From the census data I draw individual-level information about women's fertility, their age, their education, their spouse's education, their migration background and their location. The number of children ever born is unfortunately only available for 1900 and 1910 and then again for 1940 onwards. For earlier years there is just the total number of children living in the household or the number below the age of five living in the household. I follow Basso et al. (2014) and compute for each 15 to 39-year-old woman the number of children below the age of five living in the household. Using child-woman ratios as a measure of fertility is quite common in the historical literature (see e.g. Becker et al., 2013).[3] Other authors have constructed retrospectively fertility data by using the number of children ever born reported in the 1940 census. Yet, in my case this is not a preferable option as women may have changed the county between the birth of their children and 1940. In addition, selective mortality could bias the results as many women have not survived until 1940.

[2] IPUMS recently also released preliminary versions of full count data for the years 1880 to 1930. Since this data is preliminary, does not include the year 1870 and does only provide a sub-set of all control variables deemed necessary for the econometric analysis, I stick to the samples. Yet, I provide robustness checks using the full count data.

[3] The conditions under which this is a valid approximation are discussed in the Online Appendix A (Figure S.1).

Given that during the observation window the economic and social context was very different for white and black families, I use for the analysis only information about white women. I also limit my sample to ever married women and focus on marital fertility. Moreover, since I consider the exposure to rainfall risk not very relevant for urban households, I also limit my data set to rural areas.[4] I also exclude the states of Alaska and Hawaii and the population living in military camps.

The Census of Agriculture provides data for the same years as the population census. I use the average value of farmland and buildings, the average farmland area per farm and the average value of implements and machinery per farm. For the years 1900 and 1910 there is also information about the share of irrigated land.

I remove from the data set those observations where information on key variables is missing.[5] The upper part of Table 1 shows the composition of the total sample used for analysis, in total 945,038 observations. It can be seen that the number of covered counties increases over time as, at the beginning of the observation window, many counties were not yet settled. Since county borders are not constant over time, this study works also with county-groups for which borders do not change. This concept is explained in Section 4.

[Table 1 about here]

The lower part of Table 1 describes the sample over time by showing the mean of various women's, household's and county characteristics in each census year. The used fertility measure declines by about 23% over the entire observation window.[6] Table 1 further shows that literacy increases over time. At the end of the observation window, female literacy exceeds literacy levels of household heads, largely due to a cohort effect. The share of farming households at first increases and then, at the beginning of the twentieth century, declines. The size of farms, the value of farmland and farm buildings and the value of machinery and implements also continuously rise. The increase in the value of machinery and implements is particularly pronounced after 1900, documenting the increasing mechanization of agriculture. The decline of the value of farmland, machinery and buildings in 1930 mirrors

[4] The term 'rural' generally denotes places with less than 2,500 inhabitants. There is a slight variation over time with respect to the coding within the New England states.

[5] The only information that is not available for all women is the information that comes from the Census of Agriculture. The fact that a few counties were not covered removes 35 counties from the sample.

[6] A set of maps tracking the fertility decline in rural areas across counties over time can be found in the Online Appendix B, Figure S.2.

the start of the Great Depression. For the years 1920 and 1930, data from the Survey of banks and bank deposits allows to explore the impact of financial market development on fertility.

Finally, rainfall data comes from the PRISM data set. For the US it provides monthly rainfall data for the period 1895 to 1980 for 4 x 4 km grid points.[7] Using the minimal Euclidian distance, I attributed each rainfall station to a specific county-group and computed for each county-group and each month the average rainfall. From this I computed the average rainfall during the growing season (assumed to last from March to November[8]) for each year. Based on these means, I computed the variance in rainfall for each county-group. For both average rainfall and the variance in rainfall I use log rainfall to capture relative rather than absolute deviations from the mean. As an alternative measure of rainfall risk, I also computed the probability that a drought will occur, i.e. the probability that in a specific year rainfall will be less than 75% of the long-term average rainfall (growing season) in that county-group.

[Figure 1 about here]

Figure 1 shows a set of maps documenting the distribution of rainfall, rainfall variation and droughts across the US (counties). Rainfall levels are particularly high in the Northeast, along the Atlantic coast and in the Southeast more generally. The area to the left of the 100° West meridian is very dry, yet rainfall is relatively high in Colorado, the Rockies and in the Northern part of the Pacific coast. Rain-fed agriculture was, without irrigation, barely possible in the West; livestock farming was the dominant activity. In the latter half of the nineteenth century the US experienced major droughts in 1856-1865, 1870-1877 and in 1890 and 1896 (Herweijer et al., 2006). In the first half of the twentieth century the dust bowl, i.e. a period of severe dust storms that caused enormous damage to farmland, farm yields and cattle, stands out. It started in the early 1930s, i.e. outside my observation window, and lasted several years (Worster, 1979).

[7] The dataset uses whatever station networks and data sources are available for the relevant period. The rainfall time series were modelled using climatologically-aided interpolation (CAI), i.e. the Parameter-elevation Regressions on Independent Slopes Model (PRISM), which used the long-term average pattern as a first guess of the spatial pattern of climatic conditions for a given month or day. CAI is robust to wide variations in station data density, which is necessary when modeling long-term series. Data is based on monthly modelling. Removing those counties from the sample for which rainfall data is not available reduces the sample size by 79 counties.

[8] This is obviously only a rough assumption as the growing season varies across regions and it depends on the weather conditions, especially temperature conditions, in a specific year. Yet, if I use the entire year as reference period the results of this paper hold, but the fit is not as good (see Online Appendix I, Table S.7).

4. Empirical specification

To identify the effect of rainfall risk on fertility I use the pooled sample of all census data covering the period from 1870 to 1930. I regress fertility on the county-group-specific rainfall variance controlling for rainfall levels, individual, household and county characteristics in each census year and accounting for state-specific time-effects in fertility. The indices *i*, *c*, *s* and *t* stand for the individual, the county-group, the state and time respectively. Since rainfall variability should, at least directly, only matter for farm households, I interact rainfall and rainfall variability with being in a farm household allowing me to control for county-group fixed-effects. Hence identification relies on the differential effect of rainfall variability on fertility between farm and non-farm households.[9] Non-farm households in rural areas include for example carpenters, construction workers, millers, mine workers, carriers, mechanics, teachers and traders among others. Moreover, because the effect of rainfall variability may also change over time, for instance because agriculture becomes less dependent on rain, I also interact rainfall and rainfall variability with time. Hence, the full model reads:

$$\begin{aligned} fert_{icst} &= \beta_1(rain_{cs} \times farm_{icst}) + \beta_2(rain_{cs} \times farm_{icst} \times T_t) \\ &+\beta_3(rainvar_{cs} \times farm_{icst}) + \beta_4(rainvar_{cs} \times farm_{icst} \times T_t) \\ &+\beta_5(rain_{cs} \times T_t) + \beta_6(rainvar_{cs} \times T_t) \\ &+\rho W fert_{cst} + X'_{icst}\beta_7 + C'_{cst}\beta_8 + \vartheta_{cs} + \beta_9\left(\gamma_s \times T'_t\right) \quad + \varepsilon_{icst} \quad (1) \end{aligned}$$

where *rain* stands for the average yearly rainfall level (in log) in the growing season, *rainvar* for the average yearly rainfall (in log) variance in the growing season, the matrix *X* for a set of individual and household controls including age, education, education of the household head, being born in a different county or abroad and whether the household is a farming household. The matrix *C* stands for time-varying county-level controls such as the average value of farmland and buildings, machinery and implements and the average size of farmland per farm, *T* stands for year effects (census years) and γ_s for state effects. The parameter ϑ_c stands for county-group fixed-effects. In fact, as county borders changed over time due to the simple shift of county borders, the split of a county into two or more counties, the merger of counties

[9] Note that for 1870 and 1880 IPUMS coded a household as "farm household" if it contained at least one person with the occupation "farmer". In later years the coding was based on the enumerator's explicit identification of a farm (cultivated land, production and use of agricultural labor). My results below hold, if I exclude the years 1870 and 1880 from the estimation suggesting that the particular definition of farm households in 1870 and 1880 does not drive my results (see Appendix J, Table S.8).

into one county or a combination of both I work with county-groups, i.e. counties are grouped in a way that for the group as a whole there is no change of the border over time. This procedure does not eliminate any county from the sample it just means that instead of introducing fixed-effects at the county level, fixed-effects are introduced at the county-group level.

To account for spatial correlation of fertility, I also estimate specifications, where the spatially lagged fertility, *Wfert*, is included among the controls. To do so, I first estimate a spatial weight matrix using the geographical information of latitude and longitude (Kondo, 2017). This matrix is then used to compute a county-specific spatially lagged variable that accounts for spatial dependencies across counties.[10]

Since rainfall variability shows also some variation over time, I also use a specification, where rainfall variability is not calculated over the entire observation window covered by the PRISM data, but over ten-year intervals around the census year, i.e. for the census of 1900 for the period 1895 to 1905 and for the census of 1910 for the period 1905 to 1915 and so on. This measure is supposed to reflect the rainfall conditions around a specific census year. Obviously, the virtue of this specification is that it creates within-county-group variation in rainfall-variability over time which can be used in addition for identification:

$$fert_{icst} = \beta_1(rain_{cst} \times farm_{icst}) + \beta_2(rainvar_{cst} \times farm_{icst}) +$$
$$+\rho Wfert_{cst} + X'_{icst}\beta_7 + C'_{cst}\beta_8 + \vartheta_{cs} + \beta_9\left(\gamma_s \times T'_t\right) + \varepsilon_{icst} \quad (2)$$

Since the PRISM data only starts in 1895, this specification does not use the census data of 1870 and 1880 and therefore it is not my preferred estimation method. Yet, Equation (2) can also be estimated including county-group-specific fixed-effects for agricultural and non-agricultural households which can in principle further reduce any possible bias due to omitted group-specific traits.

[10] I do not use household weights in my regression as my sample consists of white married women in the age group 15-39 for which these weights are not appropriate. Moreover, using weights has only limited value added as my regressions control for county group fixed effects and a whole range of socio-economic characteristics. Yet, if I do use weights my results do not change, this is shown in the Online Appendix K, Table S.9.

5. Results

A precondition for seeing an effect of rainfall variability on fertility that can be traced back to households' need for insurance, is that rainfall is a determinant of agricultural production. Although it has been shown many times in the literature, I nevertheless verified that this link also holds true in my data. For this purpose, I regressed the county average value of production per farm in year *t* on rainfall during the growing season in year *t* controlling for average farm size, the average value of land per acre, the average value of machinery and implements per farm, state-specific period effects and county-group fixed-effects. I find an elasticity of 0.37, i.e. one percent more rainfall during the growing season increases the value of output by 0.37%. The results are shown in the Online Appendix C, Table S.1. In what follows I therefore assume that agricultural production and hence agricultural income varies with rainfall. The results also show that county-group-specific rainfall variability in itself, i.e. the extent to which rainfall typically varies from one year to another is in turn not a determinant of the value of production in year *t* once rainfall in year *t* is controlled for (Table S.1, cols. (4)-(6)). Rainfall variability is measured here as the variance in rainfall over the period covering the five years prior and five years following the census year. Hence, if below I find an effect of rainfall variability on fertility controlling for the rainfall level, it is unlikely that this effect is driven by a simple income effect. I now turn to the estimation of Equation (1).

5.1 Main results

Table 2 shows eight different specifications where each specification estimates the effect of rainfall variability on fertility. Col. (1) does not yet account for county-group fixed-effects and state-time effects, it just includes general time effects. Col. (2) controls for county-group fixed effects. Col. (3) estimates year-specific effects of rainfall variability on fertility. Col. (4) allows in addition for state-specific time effects. Col. (5) accounts for a spatial dependence of fertility levels across counties and hence uses exactly the specification shown in Equation (1). Col. (6) uses instead of rainfall variation the risk of drought measurement. Col. (7) includes also urban areas in the analysis and col. (8) removes women from the sample who were born in another state than their current state of residence or abroad. Table S.2 in the Online Appendix D re-estimates cols. (2)–(4) at the county-group level, i.e. with county-group averages. Using alternative specification allows to examine the sensitivity of the results with respect to different sets of controls and estimation methods. It will become apparent from the following discussion that the key results are very robust.

[Table 2 and Figure 2 about here]

The results in col. (1) show that the general effect of rainfall variability on fertility is negative, but the interaction effect with being in a farm household is positive and over-compensates the general negative effect. Hence, rainfall variability is associated with an increase in fertility in farm households but not in non-farm households. The negative effect for non-farm households might be due to the fact – as implied by my theoretical framework - that in non-farm households there is little use for child labor and hence children are not an adequate mean to cope with the (indirect) effects of rainfall shocks, they are too costly and parents have to rely on other coping strategies and have rather less children than non-farm households that are less exposed to such shocks. In the following specifications identification relies on that difference between farm and non-farm households within county-groups and hence all county-group-specific characteristics that do not vary over time and do not impact farm and non-farm households differently are controlled for through the introduction of county-group fixed-effects.

In farm households the effect of rainfall variability on fertility is across all specifications significant positive. Col. (2) suggests if rainfall variability is increased from a level that corresponds to the mean in the tenth percentile to the mean in the ninetieth percentile the number of children below the age of five in farm households increases on average by 0.09 or 9.2% of the mean relative to non-farm households. This is about half of the difference in fertility between literate and illiterate women. Such a shift in rainfall variability corresponds, for example, to a move from Chippewa, Michigan to Culberson, Texas.

Cols. (3) and (4) reveal an interesting time pattern: The effect of rainfall variability on fertility observed for farm households is reduced as time progresses. Whereas the estimated effect is 0.830 in 1870 it decreases by about 60% to 0.338 in 1930 (col. (4)). This means that if rainfall variability is increased, again, from a level that corresponds to the mean in the tenth percentile to the mean in the ninetieth percentile the number of children below the age of five in farm households relative to non-farm households increases in 1870 on average by 12% and in 1930 by only 5%, hence the fertility differential that is due to rainfall risk has almost disappeared by the end of the observation window.

During the period 1870 to 1930 the US experienced substantial social and economic change. In very arid areas, irrigation systems were built, formal safety nets and financial services became available and farms became more and more capital intensive. It also coincides with a massive expansion in the use of electricity. All this might have reduced the role of children in coping with rainfall risks. Whether this hypothesis is coherent with the empirical evidence will be explored in more detail below. This time pattern is also robust to the inclusion of a spatial lag, i.e. if the spatial dependence of fertility levels across counties is taken into account (col. (5)). Figure 2 shows the time pattern in a graph. It can be seen that the rainfall variability effect is relatively constant between 1870 and 1900 and then starts to decline. The results do also not change qualitatively if instead of rainfall risk the risk of drought is used (col. (6)). If rainfall during the non-growing season is used to conduct a placebo test, the estimated effects associated with rainfall variance are much smaller and partly insignificant which is again in support of my results above (see Online Appendix E, Table S.3). Including urban households into the sample does also not significantly change the results used (col. (7)). However, if estimated for urban households alone (see Online Appendix F, Table S.4) the effect of rainfall variance is insignificant, which is in line with the theoretical framework which suggests that children as an insurance were too expensive in urban areas. Finally, if migrants are removed from the sample the rainfall variability effect even increases (col. (8)). This makes it unlikely that my estimates are driven by selective migration. This potential bias will be further discussed below. The coefficient associated with rainfall variability and being in a farm household is fairly constant over columns (3) to (8). The results are also qualitatively similar if a count data model is used. They do also not change if fixed-effects are not introduced at the county-group level but at the county level (results not shown in Table). To deal with changing borders over time, this specification works with the county borders of 1870 for all subsequent years (to the extent that this is possible). The results do also not change if full count data is used. This is demonstrated in the Online Appendix G, Table S.5, using the period 1880 to 1930 for which next to the samples, IPUMS does also provide (preliminary) full count data at least for a subset of all necessary variables.

Before turning to further robustness checks, I will briefly comment on the effects associated with rainfall levels and some of the other control variables. The general effect of rainfall levels on fertility is insignificant in both farm and non-farm households (col. (1)). However, as time progresses the effect of rainfall levels on fertility is significantly positive and larger in farm households than in non-farm households. But this effect is very small. As expected, literate women have lower fertility than illiterate women. The fertility rate of women who were born in a different state is not significantly different. Yet, women who were born abroad have on average an additional 0.25 children below the age of five in their household. Interestingly in col. (8) the farm effect turns negative suggesting also that migrant farms had significantly more children than non-migrant farms. I also estimated alternative specifications where I controlled for the exact region or country of origin of the woman, but this did also not change the main results (see Online Appendix H, Table S.6). Fertility also decreases as the value of land and buildings per acre and farm size increases. The value of implements and machinery per farm alone has an insignificant or only small negative effect. Other specifications where I controlled for additional geographical traits such as the distance to the next river, longitude, latitude and temperature all interacted with time have also not led to different results (not shown in Table). [11]

To conclude, the results above suggest that farm households increased their fertility with increasing rainfall variability. Yet this effect started to decrease at the beginning of the 20th century. Before I explore the underlying time pattern in more detail, I conduct several robustness checks to provide further support to my main findings.

5.2 Robustness checks

Shorter term rainfall variability

The estimations above are based on county-group-specific rainfall variability calculated for the period 1895 to 1980. I now re-estimate this model using county-group-decade-specific rainfall variability as specified in Equation (2). The advantage of this specification is that it allows not only to use within-county-group variation between farm and non-farm households but to use also within-county-group variation in rainfall variability over time. This allows for example to include also county-group effects for farm and non-farm households separately. For each census year and each county-group I calculate the rainfall variability for the period starting five years prior to the census year and ending five years after the census year. Again, the intention is to capture the rainfall conditions in the period 'around' the census year.

[11] Distance from the centroid of a county-group to the next river was calculated using a shapefile from the US Geological Survey. It contains all rivers and lakes in North-America. Only rivers that are longer than 100 miles or alternatively 500 miles were considered. Yet, the data refers to rivers today, the exact course of a river might have been slightly different at the end of the 19th century.

Alternatively, one could also use for example rainfall variability over the ten years preceding the census to ensure to capture only weather conditions that prevailed before a birth was reported. This has also been done but did not change the results reported below. Since rainfall data is only available for 1895 onwards, the estimation of Equation (2) does only use the census data of the years 1900-1930. Table 3 shows the results. The column "Recap" re-estimates the specification of Table 2, col. (2) for the sub-period 1900-1930 and hence can serve as a comparison or benchmark. Cols. (1) to (4) use county-group-decade-specific rainfall variability: Col. (1) without rainfall-farm interactions, col. (2) with rainfall-farm interactions, col. (3) includes in addition state-specific time-effects and col. (4) uses county-group fixed-effects for farm and non-farm households separately.

[Table 3 about here]

I find that the overall effect of rainfall variability on fertility is clearly positive and somewhat larger than the one identified using the specification with the time-constant rainfall variability (see col. (1) and col. "Recap"). As rainfall variability increases, rural households tend to have more children. Col. (2) shows that this effect is driven by farm households, as the linear effect is insignificant. If state-specific time-effects are included, the effect of rainfall variability shrinks somewhat but is still sizeable and statistically significant. The effect is also robust to the inclusion of farm and non-farm-specific fixed-effects (col. (4)). Rainfall levels do not have any significant effect fertility neither in general nor in farm households specifically. I also ran regressions with lags and leads of rainfall and rainfall variability where one would expect the leads to have a much smaller effect than current values and lags (see Online Appendix L, Table S.10). In these regressions both lagged and future rainfall variability interacted with farming are insignificant, but the coefficient associated with future rainfall variability is much smaller than the coefficient associated with lagged rainfall variability. Current rainfall variability interacted with farming remains highly significant. Hence, overall these results support the findings above. Fertility in farm households increases with rainfall variability.

Timing effects

Next, I divided the sample into a subsample with all women younger than 25 years and a subsample with all women 25 years and older allowing to check whether the rainfall risk effect is only driven by younger women. This could be the case if in fact rainfall variability did not induce parents to have more children but only to have them earlier. For older women that bias, if it exists, should have vanished. The results and further explanations are provided in the Online Appendix M including Table S.11. The estimates show that splitting the sample by women's age does not change the previous results and hence it can be ruled out that the rainfall risk effect on fertility is only driven by women in farm households who have their children earlier.

Endogenous cropping patterns

The results might also be biased if increased variability in rainfall causes rural households to grow a different set of crops. If these crops are more labor-intensive, it may explain why I find higher fertility rates in areas with a high rainfall variability. Table S.12 in the Online Appendix N shows that the inclusion of cropping patterns as control variables does not change my results with respect to rainfall variability.

Rainfall risk-induced child mortality

The results above would be biased if the increased fertility in counties with higher rainfall variability was partly a response to increased infant and child mortality induced by the rainfall variability. If droughts led to shortages of food and diseases that caused children to die more frequently, parents in counties with high rainfall variability may have increased their fertility to compensate (Olsen, 1980). Unfortunately, mortality data for that period of time is scarce and hence it is not possible to control for infant and child mortality systematically. Nevertheless, to further rule out that such a bias drives the results, I use the census data of 1900 and 1910, where women were not only asked about the number of children in their household but were also asked to report the number of children ever born and the number of children that survived. With this data I test whether child mortality was correlated with fertility, whether the mortality effect off-sets part of the rainfall variability effect and whether rainfall variability in turn can explain child mortality. The results are shown in the Online Appendix O (Table S.13). These results make it unlikely that the effect of rainfall variability is transmitted through rainfall risk-induced child mortality.

Rainfall risk and returns to education

Another potential concern is that differences in rainfall risk lead to differences in returns to education and, in consequence, also to returns to quantity (i.e. the number of children). In this case the higher fertility was not the result of risk mitigation but the response to low returns to quality and hence high returns to quantity. This argument is not very different from the one tested in this paper as the underlying hypothesis is that rainfall-risk increases the return to quantity. Yet, to exclude that the effect only goes through education, I re-estimated Equation (1) controlling for the children's year and county-specific school enrolment rate, i.e. I tested whether there is still a positive association between rainfall variability and fertility, once the effect of parental educational investments on fertility is controlled. The results are discussed in Online Appendix P (Table S.14). Based on the findings it is unlikely that the effect of rainfall variability on fertility dominantly passes through reduced educational investments.

Endogenous Migration

Unobserved preferences that may have determined both the destination of settlers and fertility also present a threat to the identification used in this paper, i.e. if parents with a preference for fewer children migrated systematically to areas with lower rainfall variability. I provide a detailed discussion of this possible bias in the Online Appendix Q and argue that it is unlikely that endogenous migration is important. Moreover, the estimates presented in Table 2 show that the rainfall variability effect is robust to the inclusion of variables controlling for "having been born in a different state" and "having been born abroad". The results also hold if like in col. (8) all women that migrated are excluded from the sample. Hence, based on these findings, I believe that selective migration is not the main driver of the results shown in this paper.

5.3 The effect of risk-mitigating technologies and institutions

The results above suggest that the effect of rainfall risk on fertility is reduced as time progresses. In this section I explore whether this phasing out can be explained by the adoption of risk-mitigating technologies and the emergence of risk-mitigating institutions. I focus on the adoption of irrigation systems, the use of physical capital more generally and the spread of formal banks. For all three it can be argued that they reduce the need of parents to have many children to cope with rainfall risks. Unfortunately, data for the availability of irrigation systems and formal banks is only available for selected years. The availability of irrigation systems is measured by the share of agricultural land that is irrigated in a county. Physical capital is measured by the value (ln) of machinery in agriculture in constant prices in a county. Access to financial institutions is measured by the total number of banks per county. All three are time varying variables. Maps showing the spatial distribution of irrigation systems, agricultural machinery and banks over time can be found in Online Appendix R (Figures S.3 - S.5).

Figure S.3 shows that irrigation was used particularly in the arid West, but also spread to the western and southern parts of the Central region as time progressed. The use of machinery increased rapidly at the beginning of the twentieth century, especially in the Great Plains (Fig. S.4). This can be explained by several innovations that became available for large scale adoption, such as tractors and reapers (Olmstead and Rhode, 2001). It is also due to the ongoing electrification which started in 1880, and the increasing availability of credit, both of which made it possible for households to use more machinery. Banks were especially concentrated in New England and the northeastern Central states. Because of the 1929 financial crisis and a serious disruption of the banking system by wide-spread mortgage defaults by small farmers and corresponding bank failures there were fewer banks in 1930 than in 1920 (Fig. S.5) (Libecap and Hansen, 2002). In 1929 and 1930 alone almost 2,000 banks failed, mainly due to a run on banks during which millions of depositors throughout the country withdrew their savings (Wicker, 1996; Klein 2001).

To explore the potential effect of risk-mitigating technologies (I), i.e. irrigation, machinery and access to financial services, I re-estimate Equation (1) as follows:

$$\begin{aligned} fert_{icst} = {} & \beta_1(rain_{cs} \times farm_{icst}) + \beta_2(rain_{cs} \times farm_{icst} \times I_{cst}) \\ & + \beta_3(rainvar_{cs} \times farm_{icst}) + \beta_4(rainvar_{cs} \times farm_{icst} \times I_{cst}) \\ & + \beta_5(rain_{cs} \times I_{cst}) + \beta_6(rainvar_{cs} \times I_{cst}) + \beta_7(farm_{icst} \times I_{cst}) + \beta_8 I_{cst} \\ & + \beta_9(rain_{cs} \times T_t) + \beta_{10}(rainvar_{cs} \times T_t) \\ & + \rho W fert_{cst} + X'_{icst}\beta_{11} + C'_{cst}\beta_{12} + \vartheta_{cs} + \beta_{13}(\gamma_s \times T'_t) + \varepsilon_{icst} \quad (3) \end{aligned}$$

If the emergence of risk-mitigating technologies indeed reduces the need of farm households for more children to cope with rainfall shocks, I should find $\beta_4 < 0$.

Table 4 shows the results in each case with two alternative specifications regarding time effects. Cols. (1) and (2) show that the effect of rainfall risk on fertility is significantly reduced for women in farming households in counties where farms adopted irrigation systems when compared to counties where irrigation systems were not available. The size of the estimated coefficients in col. (2) implies that the fertility differential due to rainfall variability between farm and non-farm households disappears if the share of irrigated land in a county exceeds 23%. Among those counties that have irrigation systems in place, 23% corresponds to the level in the 30th percentile of the distribution. The fertility effect is also lower in counties where more machinery is used. The estimate col. (4) implies that the fertility differential shrinks by 50% if counties in the ninety-fifth percentile of the distribution of machinery are compared with counties in the fifth percentile. Finally, cols. (5) and (6) show the results where rainfall variability is interacted with the access to financial services. Although the sign of this interaction is also negative, the effect is economically small and statistically insignificant. The results do not change much if instead of the absolute number of banks per county the number of banks per farm per county is used as explanatory variable. Hence, based on this specification it is hard to say whether financial services could not or were not used for risk coping or whether the available measures of access to financial services are simply too crude. Yet, overall the results in Table 4 seem to support the idea that farming households used children to insure themselves against rainfall shocks but that this effect disappeared as other risk-mitigating technologies became available, at least this seems to be the case for irrigation and machinery.[12] It is also interesting to note that in farm households the increasing adoption of irrigation and machinery reduced not only the effect of rainfall variability on fertility, it also reduced the effect of rainfall levels on fertility in farm households.

[Table 4 about here]

The results in this section could of course be biased if the emergence of these risk-mitigating technologies and services was endogenous to fertility decisions. This would be the case if, for instance, farming households with a preference for fewer children sorted themselves into areas where risk-mitigating technologies and institutions were available or if they at least, adopted these technologies and institutions earlier than others. The used controls can only imperfectly account for such unobserved heterogeneity. See Online Appendix Q for a more detailed discussion to what extent endogenous migration could be a threat to identification.

[12] Guest and Tolnay (1985) show that in the early 1900s, increasing farm mechanization increased education investment suggesting that mechanization reduced the return on child labor (see also Goldin and Katz, 1999).

6. Conclusion

This paper explored the role of rainfall risk in the American demographic transition. Geographic-induced risk has so far only received little attention as a driver of fertility. My findings provide support for the hypothesis that in a context in which formal insurance devices are largely absent, fertility may respond to a need to insure risks ex-ante. I find that in the late nineteenth century in the United States, agricultural households that were exposed to extreme rainfall variability, controlling for rainfall levels, had higher fertility than agricultural households that were exposed to lower levels of rainfall variability. As expected, rainfall variability does not increase fertility in non-agricultural households. This effect is robust to a wide range of controls including county-group level heterogeneity, state-specific time effects and spatial lags. The effect is also found if changes in the climate, i.e. the variation of rainfall variability within county-groups over time are used for identification. Going from the tenth to the ninetieth percentile in the distribution of rainfall variability increases the fertility differential between farm and non-farm households by about 12% in the late 19th century. Interestingly, the effect starts to decrease at the beginning of the 20th century, suggesting that the insurance function of children is diluted as more capital is used in agriculture and alternative risk management devices emerge. And indeed, using data on the use of irrigation systems and agricultural machinery, it can be shown that the fertility effect decreases as these aspects gain importance. Hence, access to risk-mitigating devices significantly contributed to the demographic transition in the US.

The observational character of the data do not allow me to deal with all possible confounding factors and hence the findings are not necessarily causal, but they are quite robust to alternative specifications and lead, overall, to a consistent story. I can also show that the effects are not driven by rainfall variability-induced child mortality or a quantity-quality trade off triggered by differences in the returns to education between areas with low and high rainfall variability.

The findings from this paper cannot only contribute to a better understanding of the demographic transition in the US, they can also enrich demand-side theory of fertility and models of long-term growth with endogenous population. The findings also have potentially important implications for Sub-Saharan Africa, where many households must cope with rainfall shortages, crop diseases, price shocks, natural disasters, health shocks and conflicts. As formal risk management devices are usually absent, these risks may partly drive fertility and keep it at high levels. It may explain why, in some regions, especially in poor rural semiarid or arid areas of Sub-Saharan Africa, the demographic transition has often not even started or, if it has started, progresses only slowly or has stalled (Bongaarts and Casterline, 2013).

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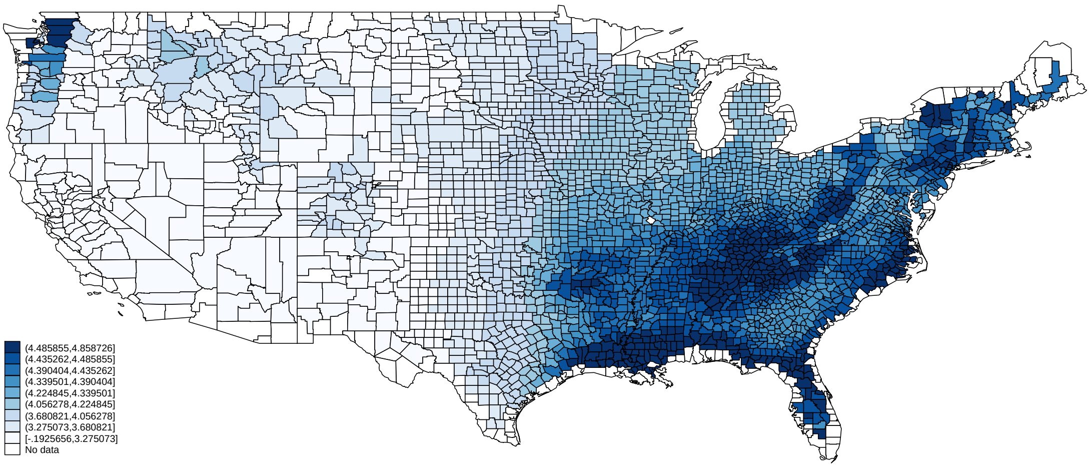
Tables and Figures

Table 1. Sample composition and descriptive statistics

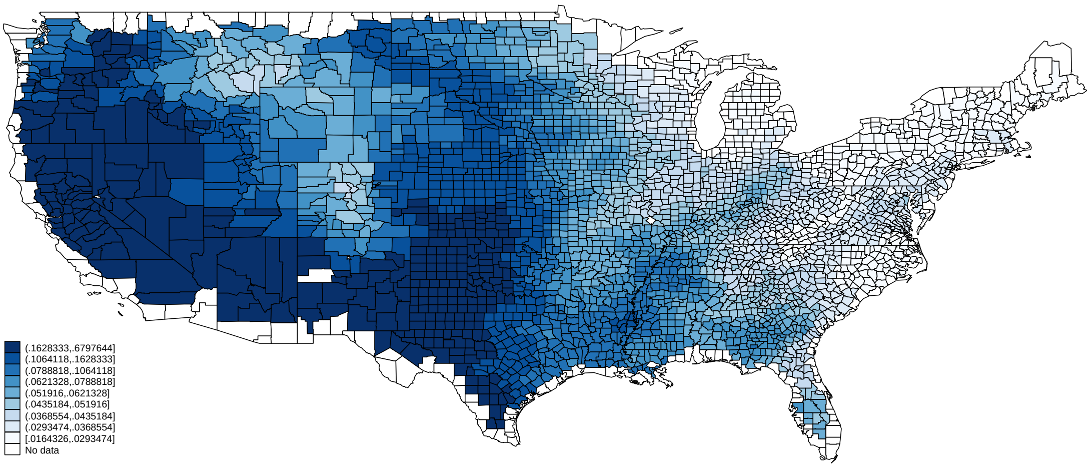
	1870	1880	1900	1910	1920	1930	Total
Sample composition							
Women ever married (15-39)	45,910	346,174	206,193	47,312	49,467	249,982	945,038
Share obs.	0.049	0.366	0.218	0.050	0.052	0.265	1
Counties	1,934	2,250	2,623	2,772	2,913	2,968	n.a.
County Groups	315	327	336	341	344	344	n.a.
States	36	38	44	46	48	48	n.a.
Descriptives							
# children under 5	1.041	1.032	0.990	0.956	0.902	0.804	0.932
Age	28.5	28.7	28.9	28.9	29.2	29.2	28.7
Literate	0.863	0.891	0.927	0.947	0.961	0.975	0.926
Head literate	0.869	0.893	0.919	0.931	0.941	0.953	0.918
Farm hh	0.569	0.575	0.567	0.539	0.570	0.511	0.554
Born in different state	0.273	0.290	0.216	0.233	0.227	0.222	0.249
Born in foreign country	0.109	0.116	0.107	0.096	0.085	0.050	0.094
Av. value farml. & build. p/a[a)]	19.9	17.2	21.0	40.6	73.0	57.3	40.6
Av. farm size (acres)	205.5	186.4	205.9	264.6	305.5	346.3	327.1
Av. value of impl. & machin. per farm[a)]	118.2	95.3	144.6	223.1	618.6	616.5	328.1
Share of land irrigated[a)]			0.040	0.049			0.045
Number of banks[a)]					9.7	6.9	8.3

Notes: White, rural population excluding Alaska, Hawaii and people living in military camps. [a)] County averages (values in constant US dollar, surface in acres), n.a. = not applicable.
Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); own estimations.

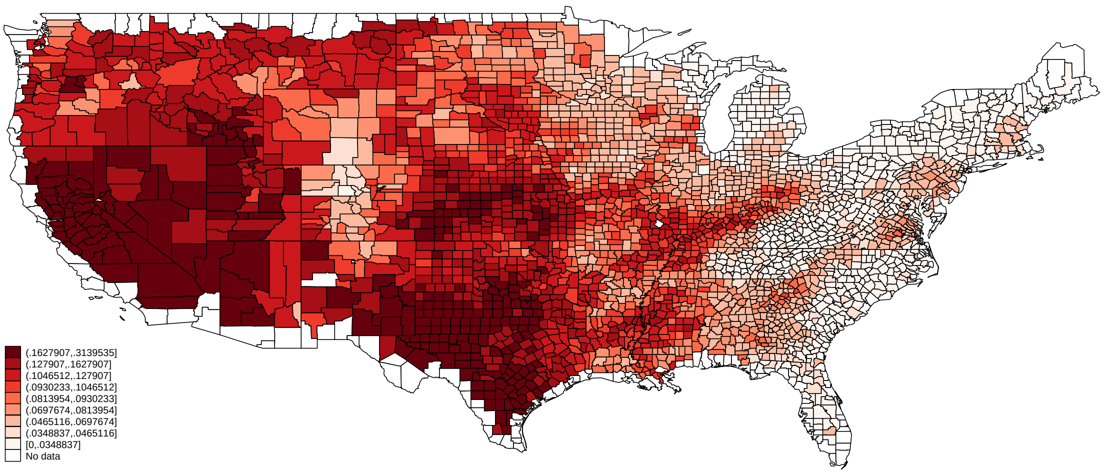
Figure 1. Rainfall levels and rainfall risk



Rainfall level



Rainfall risk



Risk of drought

Notes: Darker areas indicate higher rainfall levels, rainfall risk and risks of drought. White areas indicate areas without rainfall data.
Source: PRISM data set; own estimations.

Table 2. The effect of rainfall variability on fertility, main results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	Basic FE	Year interact.	State-time FE	Spatial lag	Drought	Rural & Urb.	w/o migrants
Rainfall (ln)	0.023							
	(0.016)							
Var Rainfall (ln)	-0.234**							
	(0.097)							
Rainfall (ln) x Farm hh	0.014	0.054**	0.030	0.032	0.031	0.002	0.038	0.075***
	(0.015)	(0.026)	(0.025)	(0.024)	(0.024)	(0.012)	(0.023)	(0.026)
Var rainfall (ln) x Farm hh	0.443***	0.653***	0.992***	0.830***	0.839***	1.157***	0.857***	1.547***
	(0.105)	(0.169)	(0.247)	(0.213)	(0.211)	(0.220)	(0.204)	(0.383)
Rainfall (ln) x Farm hh x 1880			0.020***	0.018***	0.018***	0.018***	0.020***	0.022***
			(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Rainfall (ln) x Farm hh x 1900			0.030***	0.027***	0.028***	0.028***	0.034***	0.034***
			(0.003)	(0.003)	(0.003)	(0.005)	(0.003)	(0.004)
Rainfall (ln) x Farm hh x 1910			0.031***	0.028***	0.028***	0.030***	0.038***	0.035***
			(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.005)
Rainfall (ln) x Farm hh x 1920			0.030***	0.030***	0.030***	0.032***	0.039***	0.039***
			(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.005)
Rainfall (ln) x Farm hh x 1930			0.031***	0.031***	0.031***	0.036***	0.041***	0.039***
			(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Var rainfall (ln) x Farm hh x 1880			-0.052	0.117	0.102	0.061	0.102	-0.309
			(0.143)	(0.112)	(0.110)	(0.208)	(0.119)	(0.343)
Var rainfall (ln) x Farm hh x 1900			-0.159	-0.074	-0.089	-0.154	0.006	-0.655*
			(0.160)	(0.131)	(0.131)	(0.225)	(0.130)	(0.357)
Var rainfall (ln) x Farm hh x 1910			-0.361**	-0.238	-0.252*	-0.337	-0.160	-0.918**
			(0.178)	(0.147)	(0.147)	(0.253)	(0.153)	(0.407)
Var rainfall (ln) x Farm hh x 1920			-0.508***	-0.367**	-0.387**	-0.481**	-0.214	-0.883**
			(0.188)	(0.156)	(0.155)	(0.235)	(0.152)	(0.382)

Table 2 continued …

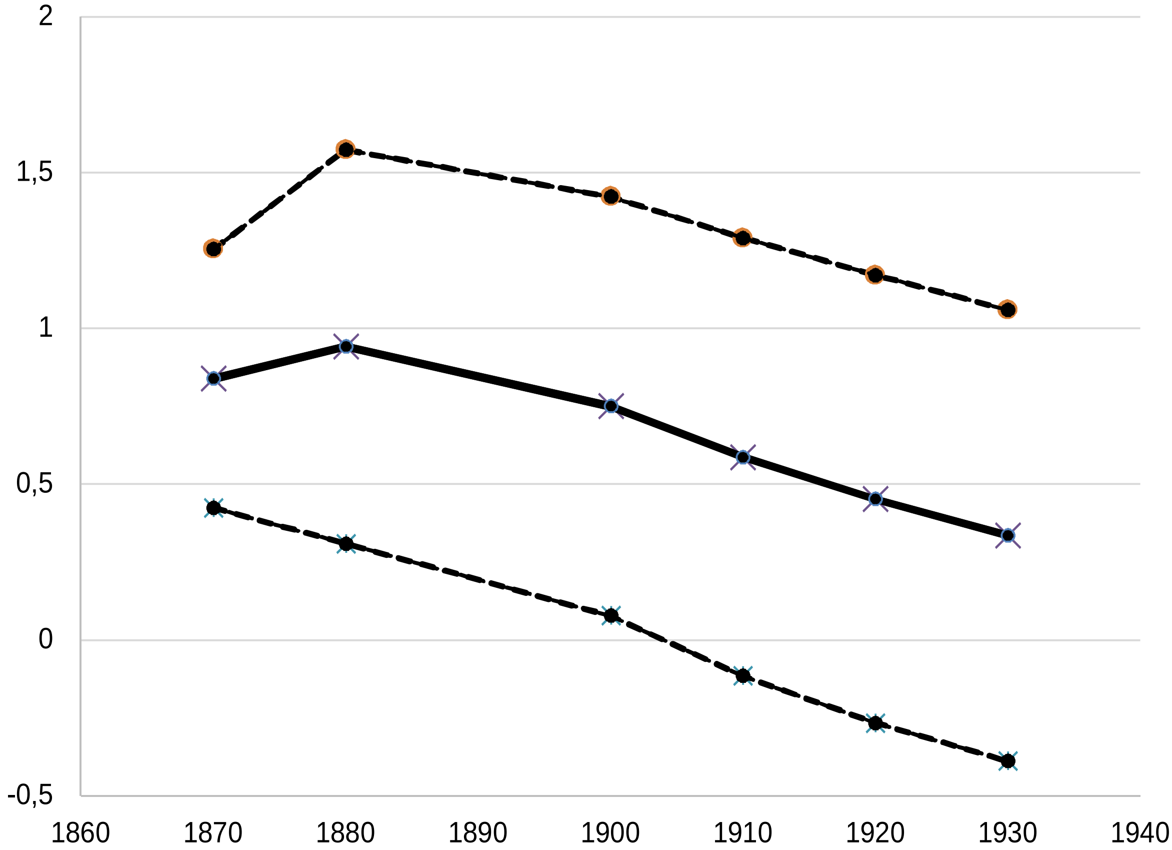
	(1) OLS	(2) Basic FE	(3) Year interact.	(4) State-time FE	(5) Spatial lag	(6) Drought	(7) Rural & Urb.	(8) w/o migrants
Var rainfall (ln) x Farm hh x 1930			-0.638***	-0.492***	-0.504***	-0.703***	-0.421***	-0.893**
			(0.199)	(0.158)	(0.157)	(0.216)	(0.154)	(0.378)
Spatially lagged fertility					0.124***	0.114***		
					(0.013)	(0.014)		
Literate	-0.132***	-0.111***	-0.111***	-0.112***	-0.112***	-0.110***	-0.146***	-0.098***
	(0.005)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Literate head	-0.012**	-0.001	0.000	0.000	0.000	0.007	0.018***	-0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Farm hh	0.052	-0.138	-0.143	-0.143	-0.141	-0.052	-0.150	-0.385***
	(0.070)	(0.119)	(0.113)	(0.111)	(0.110)	(0.051)	(0.106)	(0.116)
Born in a different state	-0.002	0.001	0.001	-0.003	-0.003	-0.001	-0.021***	
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	
Born abroad	0.246***	0.246***	0.250***	0.246***	0.246***	0.250***	0.226***	
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	
Val. land and build. p.a. (ln)	-0.052***	-0.068***	-0.057***	-0.057***	-0.053***	-0.051***	-0.059***	-0.055***
	(0.006)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)	(0.010)
Av. farm size (ln)	0.012*	-0.032***	-0.032***	-0.031***	-0.029***	-0.026***	-0.028***	-0.023**
	(0.007)	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)	(0.010)	(0.011)
Av. value machinery (ln)	-0.053***	0.006	-0.004	-0.008	-0.007	-0.011	-0.008	-0.021*
	(0.007)	(0.010)	(0.011)	(0.010)	(0.009)	(0.010)	(0.010)	(0.012)

Table 2 continued …

	(1) OLS	(2) Basic FE	(3) Year interact.	(4) State-time FE	(5) Spatial lag	(6) Drought	(7) Rural & Urb.	(8) w/o migrants
Age effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-group fixed-effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed-effects	Yes	Yes	Yes					
Rainfall-time-interactions[a)]			Yes	Yes	Yes	Yes	Yes	Yes
State-specific time-effects				Yes	Yes	Yes	Yes	Yes
R-squared	0.078	0.076	0.077	0.081	0.082	0.084	0.090	0.086
Observations	945,038	945,038	945,038	945,038	945,038	945,038	1,532,791	621,393
County-groups		344	344	344	344	344	349	344

Notes: In parentheses standard errors clustered at the county-group level. "OLS" does not include county-group fixed effects. "Basic FE" includes county-group fixed-effects and time-effects. "Year interact." includes Rainfall-farm household-year interactions. "State-time FE" includes state-specific time effects. "Spatial lag" accounts for spatial correlation. "Drought" uses the probability of a drought as a measure of rainfall variance. "Rural & Urban" includes also urban households into the sample. "w/o migrants" excludes women from the sample who migrated to their current state from another state or from abroad. [a)] Rainfall-level time interactions and Rainfall-variance time interactions.
Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

Figure 2. The effect of rainfall variability on fertility over time (farm vs. non-farm households)



Notes: The left axis shows the regression coefficients along with the corresponding 95% confidence intervals drawn from Table 2, col. (5).
Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

Table 3. The effect of rainfall variability on fertility using decade-specific rainfall variability

	Recap.	(1)	(2)	(3)	(4)
Rainfall (ln)		0.016	0.011	-0.048	-0.067
		(0.044)	(0.044)	(0.074)	(0.070)
Var rainfall (ln)		0.543***	0.160	-0.167	-0.066
		(0.145)	(0.188)	(0.238)	(0.209)
Rainfall (ln) x Farm hh	0.036		0.026	-0.033	0.015
	(0.026)		(0.044)	(0.074)	(0.072)
Var rainfall (ln) x Farm hh	0.346**		0.857***	0.440**	0.395*
	(0.161)		(0.174)	(0.220)	(0.213)
Full set of controls	Yes	Yes	Yes	Yes	Yes
County-group fixed-effects	Yes	Yes	Yes	Yes	
County-group farm/non-farm fixed-effects					Yes
Time fixed-effects	Yes	Yes	Yes		
State-specific time-effects				Yes	Yes
R-squared	0.060	0.060	0.060	0.062	0.056
Observations	552,954	552,954	552,954	552,954	552,954
County-groups	344	344	344	344	688

Notes: In parentheses standard errors clustered at the county-group level. The full set of controls includes age group dummies, literacy of the women and of her spouse, whether the household is a farm household, migration status (birth place), average land value per acre, average land size per farm and average value of machinery per farm. "Recap." estimates the specification of Table 2, col. (2) for the period 1900-1930 only.
Source: US population census 1900-1930, agricultural census (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

Table 4. The effect of rainfall variability on fertility, the role of risk mitigating technologies and institutions

	(1)	(2)	(3)	(4)	(5)	(6)
Rainfall (ln) x Farm hh	0.055	0.060	0.389***	0.445***	0.045	0.040
	(0.046)	(0.047)	(0.077)	(0.074)	(0.031)	(0.032)
Var rainfall (ln) x Farm hh	0.850***	0.878***	4.308***	4.555***	0.369	0.391
	(0.291)	(0.294)	(0.528)	(0.527)	(0.243)	(0.242)
Rainfall (ln) x Farm hh x Share irrig. land	-0.597***	-0.608***				
	(0.164)	(0.156)				
Var rainfall (ln) x Farm hh x Share irrig. land	-3.828***	-3.853***				
	(0.836)	(0.805)				
Rainfall (ln) x Share irrig. land	Yes	Yes				
Var rainfall (ln) x Share irrig. land	Yes	Yes				
Share irrig. land x Farm hh	Yes	Yes				
Rainfall (ln) x Farm hh x Value machinery (ln)			-0.071***	-0.080***		
			(0.012)	(0.012)		
Var rainfall (ln) x Farm hh x Value machinery (ln)			-0.696***	-0.738***		
			(0.085)	(0.084)		
Rainfall (ln) x Value machinery (ln)			Yes	Yes		
Var rainfall (ln) x Value machinery (ln)			Yes	Yes		
Value machinery (ln) x Farm hh			Yes	Yes		
Rainfall (ln) x Farm hh x Number of banks					-0.002	-0.002
					(0.003)	(0.003)
Var rainfall (ln) x Farm hh x Number of banks					-0.012	-0.014
					(0.019)	(0.019)
Rainfall (ln) x Number of banks					Yes	Yes
Var rainfall (ln) x Number of banks					Yes	Yes
Number of banks x Farm hh					Yes	Yes

Table 4 continued …

Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes
County-group fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed-effects	Yes		Yes		Yes	
Rainfall level and Var rainfall time inter.	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time-effects		Yes		Yes		Yes
R-squared	0.052	0.053	0.079	0.081	0.052	0.053
Observations	253,505	253,505	945,038	945,038	299,449	299,449
County-groups	341	341	344	344	344	344

Note: In parentheses standard errors clustered at the county-group level. Col. (1) and (2) uses data from the years 1900 and 1910. Cols. (3) and (4) uses data from the years 1870 to 1930. Cols. (5) and (6) uses data from the years 1920 and 1930. The full set of controls includes age group dummies, literacy of the women and of her spouse, whether the household is a farm household, migration status (birth place), average land value per acre, average land size per farm and average value of machinery per farm; in col. (1) and (2) also the share of irrigated land and in cols. (5) and (6) also the total number of banks.
Source: US population census, agricultural census (IPUMS, NHGIS); PRISM Precipitation dataset; Survey on Bank and Bank deposits, 1920 and 1930; own estimations.

Rainfall risk, fertility and development: Evidence from farm settlements during the American demographic transition

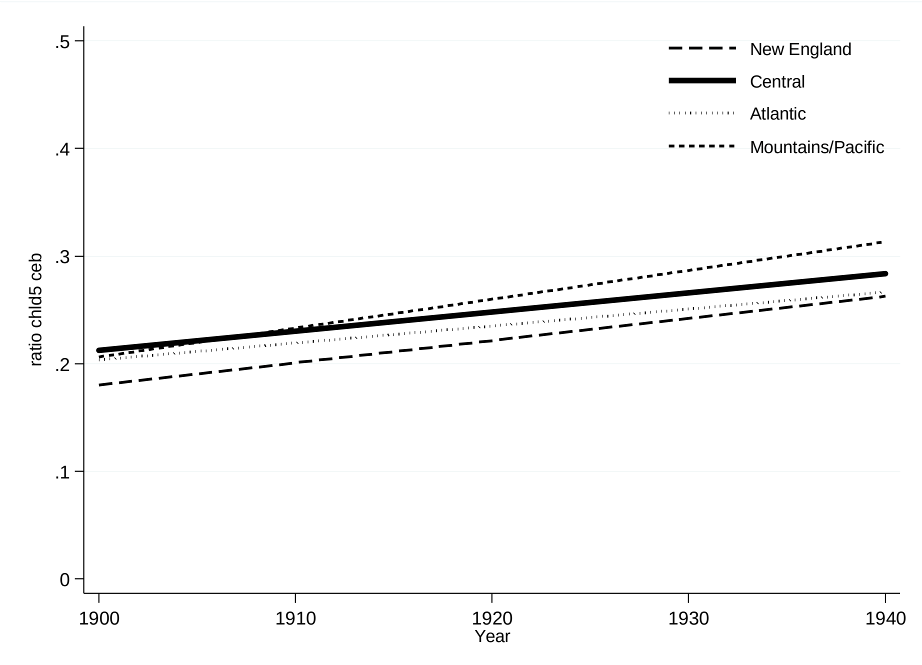
Online appendix – Not intended for print publication

Table of contents

A. Checking the validity of using the child-woman ratio as a measure of fertility

The child woman ratio will follow a similar trend as that for the number of children ever born if, during the demographic transition, women start childbearing later and increase birth intervals proportionally. If, in contrast, as suggested by Tolnay and Guest (1984), women maintain their birth intervals but just stop earlier to end up with fewer children, the number of children below the age of five living in the household will somewhat underestimate the real fertility decline. To check whether this might be a concern I have computed the evolution between 1900 and 1940 for both variables and compared their trends. This is illustrated in Figure S.1 below separately for four regions which together represent the entire country. Indeed, the number of children below five seems to decline a little bit slower than the number of children ever born, but the difference is rather small. The analysis of timing effects in Section 5.2 does also suggest that stopping vs. delaying is not an important source of bias. Finally, it is important to emphasize that it is also less of a concern as I am more interested in fertility differences across space rather than in absolute fertility levels.

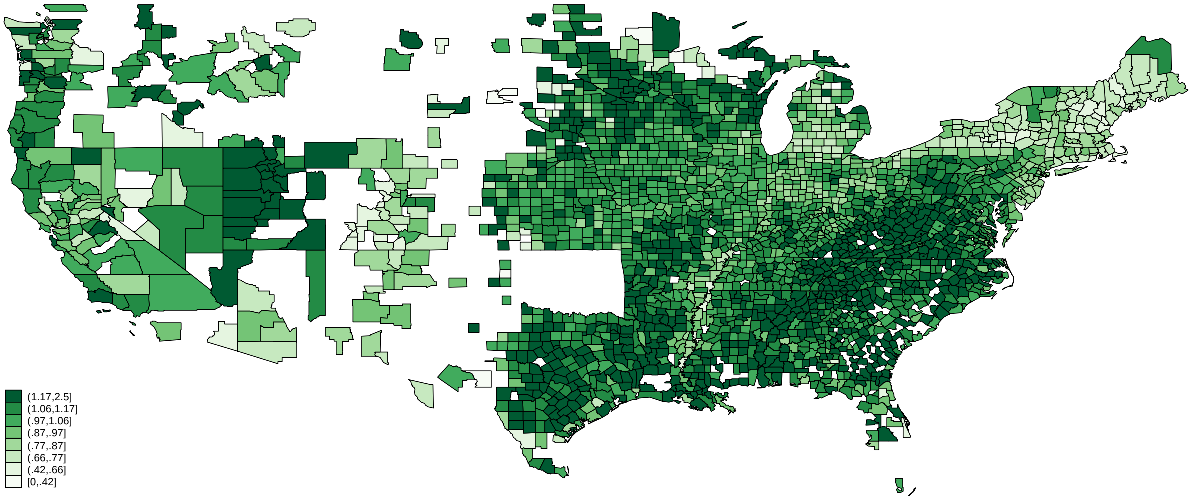
Figure S.1. The evolution of the ratio between children aged under five and children ever born over time



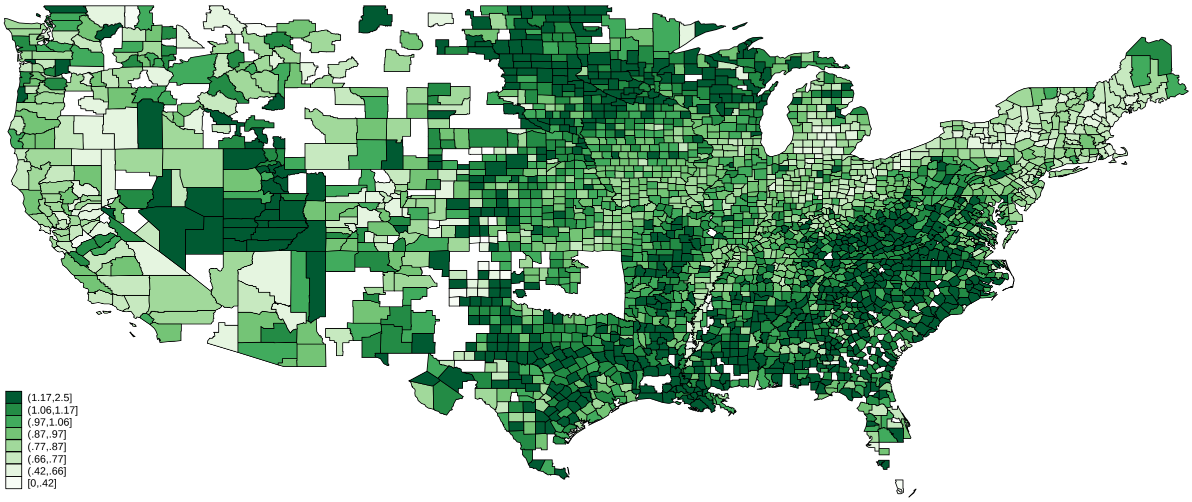
Source: US population census, 1900 and 1940 (IPUMS); own estimations.

B. Fertility across counties and time

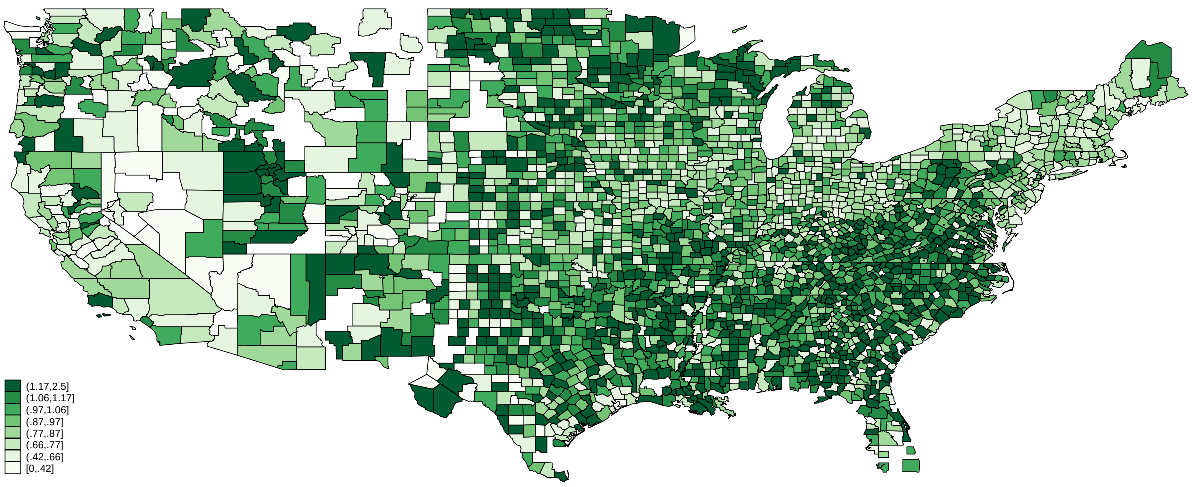
Figure S.2. Children under five per woman ratios of ever married white women (rural areas)



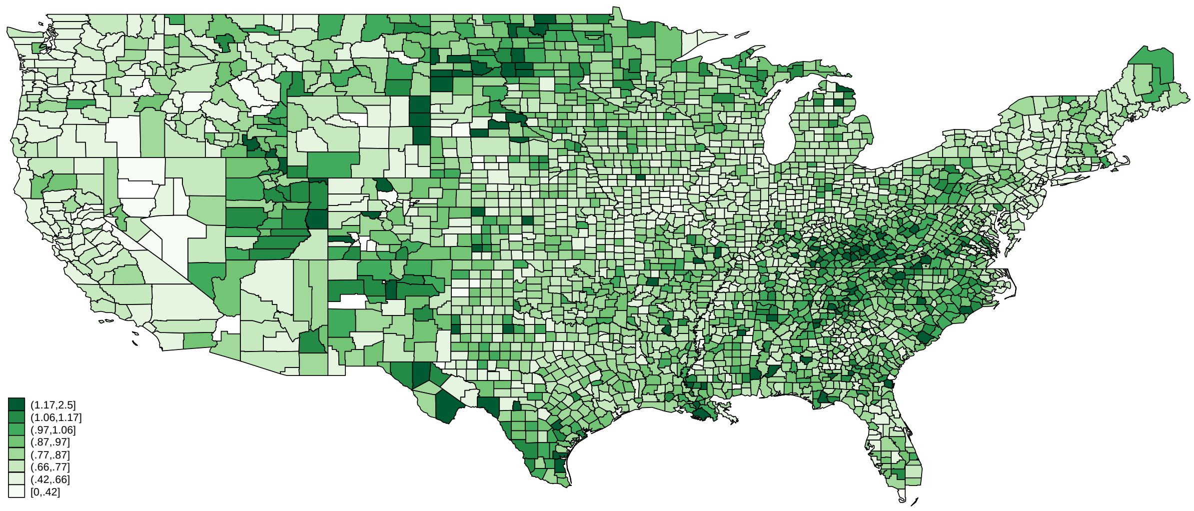
1880



1900



1910



1930

Notes: Each map uses the same scale. White spaces indicate areas where no census data was collected.
Source: US population census (IPUMS); own estimations.

C. Agricultural production regressed on rainfall and rainfall variability

Table S.1. The effect of rainfall and rainfall variability on agricultural production (county average value of production in constant US dollar per farm in *t*), county level estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Rainfall (ln)	0.390***	0.350**	0.370**	0.394***	0.349**	0.374***
	(0.151)	(0.159)	(0.157)	(0.132)	(0.140)	(0.139)
Var Rainfall (ln)				0.208	-0.028	0.249
				(1.651)	(1.657)	(1.560)
Val. land and build. p.a. (ln)	0.037	0.449***	0.114	0.037	0.449***	0.113
	(0.048)	(0.067)	(0.071)	(0.049)	(0.067)	(0.070)
Av. farm size (ln)		0.629***	0.281***		0.629***	0.281***
		(0.031)	(0.036)		(0.031)	(0.036)
Av. value machinery (ln)			0.505***			0.505***
			(0.040)			(0.040)
County-group fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time-effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.575	0.701	0.732	0.575	0.701	0.732
Observations	11,398	11,398	11,398	11,398	11,398	11,398
County-groups	344	344	344	344	344	344

Note: In parentheses standard errors clustered at the county-group level.
Source: US population census 1900-1930, agricultural census (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

D. County-group level regressions

Table S.2. The effect of rainfall variability on fertility, county-group level regression

	(1) Basic FE	(2) Year interact.	(3) State-time FE
Rainfall (ln) x Farm hh	0.042*	0.025	0.028
	(0.025)	(0.025)	(0.026)
Var rainfall (ln) x Farm hh	0.321**	0.328**	0.401*
	(0.162)	(0.166)	(0.241)
Rainfall (ln) x Farm hh x 1880		0.017***	0.016***
		(0.005)	(0.005)
Rainfall (ln) x Farm hh x 1900		0.029***	0.028***
		(0.005)	(0.005)
Rainfall (ln) x Farm hh x 1910		0.030***	0.029***
		(0.006)	(0.006)
Rainfall (ln) x Farm hh x 1920		0.027***	0.026***
		(0.005)	(0.005)
Rainfall (ln) x Farm hh x 1930		0.022***	0.020***
		(0.005)	(0.005)
Var rainfall (ln) x Farm hh x 1880		0.262	0.219
		(0.260)	(0.245)
Var rainfall (ln) x Farm hh x 1900		0.139	0.084
		(0.254)	(0.242)
Var rainfall (ln) x Farm hh x 1910		-0.109	-0.176
		(0.260)	(0.259)
Var rainfall (ln) x Farm hh x 1920		-0.039	-0.103
		(0.225)	(0.217)
Var rainfall (ln) x Farm hh x 1930		-0.046	-0.118
		(0.261)	(0.252)
Literate	0.003	-0.048	-0.064
	(0.103)	(0.109)	(0.115)
Literate head	-0.133	-0.046	-0.090
	(0.124)	(0.129)	(0.127)
Farm hh	-0.066	-0.085	-0.098
	(0.115)	(0.112)	(0.117)
Born in a different state	0.130***	0.115***	0.075
	(0.042)	(0.042)	(0.065)
Born abroad	0.377***	0.400***	0.441***
	(0.060)	(0.064)	(0.079)
Val. land and build. p.a. (ln)	-0.051***	-0.012	-0.015
	(0.016)	(0.016)	(0.021)
Av. farm size (ln)	-0.001	0.013	0.034
	(0.025)	(0.026)	(0.027)
Av. value machinery (ln)	-0.013	-0.037*	-0.045**
	(0.022)	(0.022)	(0.022)

(Table continues next page)

Table S.2 continued …

Literate	0.003	-0.048	-0.064
	(0.103)	(0.109)	(0.115)
Literate head	-0.133	-0.046	-0.090
	(0.124)	(0.129)	(0.127)
Farm hh	-0.066	-0.085	-0.098
	(0.115)	(0.112)	(0.117)
Born in a different state	0.130***	0.115***	0.075
	(0.042)	(0.042)	(0.065)
Born abroad	0.377***	0.400***	0.441***
	(0.060)	(0.064)	(0.079)
Val. land and build. p.a. (ln)	-0.051***	-0.012	-0.015
	(0.016)	(0.016)	(0.021)
Av. farm size (ln)	-0.001	0.013	0.034
	(0.025)	(0.026)	(0.027)
Av. value machinery (ln)	-0.013	-0.037*	-0.045**
	(0.022)	(0.022)	(0.022)
Age effects	Yes	Yes	Yes
County-group fixed-effects	Yes	Yes	Yes
Time fixed-effects	Yes		
Rainfall level and Var rainfall time inter.			Yes
State-specific time-effects		Yes	Yes
R-squared	0.456	0.492	0.577
Observations	3,959	3,959	3,959
County groups	344	344	344

Notes: *** p<0.01, ** p<0.05, * p<0.1. "Basic FE" includes county-group fixed-effects and time-effects. "Year interact." includes Rainfall-farm household-year interactions". "State-time FE" includes state-specific time-effects.

Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

E. Placebo test using rainfall during non-growing season

The table below shows the results if the rainfall levels and rainfall variance of the non-growing season are used. It should be noted that the rainfall variance across all counties in the growing and non-growing season has a correlation coefficient of 0.601. So, one would expect a lower effect, but not an insignificant effect. I re-estimated the specifications used in cols. (1), (2) and (4) of Table 2. I find the following results. In col (1) there is no effect on fertility of rainfall variance during the non-growing season in farm households. In col. (2) if county-group fixed effects are added, I find a very small positive effect in farm households relative to non-farm households, but the effect is less than a sixth of the effect estimated for growing season rainfall (compare with Table 2). If rainfall variance-farm-year-interactions and state-specific time-effects are added, it can be seen, that the general rainfall variance effect in farm households is again only half of the effect estimated for the growing season.

Table S.3. The effect of rainfall variability on fertility, placebo test using rainfall during non-growing season

	(1) OLS	(2) Basic FE	(3) State-time FE
Var rainfall (ln)	-0.107***		
	(0.016)		
Var rainfall (ln) x Farm hh	0.101***	0.100***	0.434***
	(0.017)	(0.027)	(0.100)
Var rainfall (ln) x Farm hh x 1880			-0.151*
			(0.091)
Var rainfall (ln) x Farm hh x 1900			-0.258***
			(0.097)
Var rainfall (ln) x Farm hh x 1910			-0.277***
			(0.099)
Var rainfall (ln) x Farm hh x 1920			-0.386***
			(0.103)
Var rainfall (ln) x Farm hh x 1930			-0.421***
			(0.097)
Full set of controls	Yes	Yes	Yes
County-group fixed effects		Yes	Yes
Time fixed effects	Yes	Yes	
Rainfall level and Var rainfall time inter.			Yes
State-specific time effects			Yes
R-squared	0.078	0.071	0.074
Observations	945,038	945,038	945,038
County groups	344	344	344

Notes: In parentheses standard errors clustered at the county-group level. The set of controls includes those used in Table 2, including rainfall levels and rainfall level-farm household-time interactions.

Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

The time interactions show that that the effect is rapidly declining over time so that the effect is very small from 1880 onwards, i.e. much smaller than in the same years if the growing season rainfall is used (compare with Table 2). Hence, this placebo test supports the main findings of this paper.

F. Placebo test using the urban sample

The table below shows the results if the estimation is done for the urban sample alone. As there are hardly any farm households in urban areas, it is not possible to exploit the within county variation between farm and non-farm households. Hence, I just estimate an OLS regression. It can be seen, that rainfall variability has no effect on fertility, this is in line with the theoretical framework which assumes that children are only a cost-effective insurance device in farm households.

Table S.4. The effect of rainfall variability on fertility, urban areas only (non-farm households)

	Urban sample
Rainfall (ln)	-0.010
	(0.021)
Var rainfall (ln)	-0.079
	(0.154)
Full set of controls	Yes
State-specific time-effects	Yes
R-squared	0.070
Observations	581,691

Notes: In parentheses standard errors clustered at the county-group level. The set of controls includes those used in Table 2.
Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

G. Robustness to using full count data

Recently, IPUMS also published full count data, yet there are several problems implied by using this data for this study, which altogether led to the decision to work with the published sub-samples. In what follows I briefly discuss these other problems, but I also show the results from robustness checks using the full count data. These checks show that the main results do not change if full count data is used.

Using the full count data comes with the following problems: (i) A first problem implied by using full count data is that this data is not available for the year 1870 which is the beginning of the observation window of this study. Given that an important part of my findings is based on the change of the risk-fertility relationship over time as new risk mitigating technologies and institutions emerge, not using the year 1870 would be a serious shortcoming. (ii) Moreover, according to IPUMPS the full-count data for the years 1900, 1910, 1920 and 1930 is still preliminary, so it is not obvious that this is a better choice than using finalized sub-samples. (iii) Several key variables are not available in the full count data sets. Literacy is missing in the 1880 full count data set. Literacy of the household head is missing throughout the years 1880 to 1930. Not accounting for the woman's education and the education of the household head might introduce a serious unobserved variable bias as education may affect both fertility and the ability to respond to rainfall risk.

Table S.5. The effect of rainfall variability on fertility, main results – robustness to the use of full count data

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		Basic FE		State-time FE	
	Sample	Full	Sample	Full	Sample	Full
Rainfall (ln)	0.036**	-0.003				
	(0.017)	(0.013)				
Var Rainfall (ln)	-0.172*	-0.352***				
	(0.101)	(0.088)				
Rainfall (ln) x Farm hh	0.014	0.001	0.054**	0.030	0.046*	0.028
	(0.015)	(0.010)	(0.025)	(0.020)	(0.024)	(0.019)
Var rainfall (ln) x Farm hh	0.417***	0.269***	0.630***	0.389***	0.931***	0.669***
	(0.105)	(0.080)	(0.167)	(0.133)	(0.186)	(0.157)
Rainfall (ln) x Farm hh x 1900					0.010***	0.004***
					(0.002)	(0.001)
Rainfall (ln) x Farm hh x 1910					0.010***	0.004***
					(0.003)	(0.001)
Rainfall (ln) x Farm hh x 1920					0.012***	0.010***
					(0.003)	(0.002)
Rainfall (ln) x Farm hh x 1930					0.014***	0.010***
					(0.002)	(0.002)

(Table continues next page)

Table S.5 continued …

Var rainfall (ln) x Farm hh x 1900					-0.199**	-0.167***
					(0.084)	(0.052)
Var rainfall (ln) x Farm hh x 1910					-0.362***	-0.192***
					(0.102)	(0.064)
Var rainfall (ln) x Farm hh x 1920					-0.482***	-0.289***
					(0.127)	(0.078)
Var rainfall (ln) x Farm hh x 1930					-0.611***	-0.443***
					(0.113)	(0.086)
Literate		n.a.		n.a.		n.a.
Literate head		n.a.		n.a.		n.a.
Farm hh	0.055	0.116**	-0.129	-0.013	-0.127	-0.031
	(0.070)	(0.047)	(0.118)	(0.091)	(0.111)	(0.088)
Born in a different state	-0.010***	-0.013***	-0.003	-0.007*	-0.006*	-0.010***
	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
Born abroad	0.258***	0.252***	0.256***	0.244***	0.259***	0.245***
	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)
Val. land and build. p.a. (ln)	-0.057***	-0.055***	-0.077***	-0.076***	-0.060***	-0.048***
	(0.006)	(0.006)	(0.008)	(0.007)	(0.009)	(0.007)
Av. farm size (ln)	0.017**	0.010	-0.024**	-0.034***	-0.031***	-0.022**
	(0.008)	(0.007)	(0.011)	(0.009)	(0.011)	(0.009)
Av. value machinery (ln)	-0.057***	-0.062***	0.012	0.017*	-0.009	-0.012
	(0.008)	(0.008)	(0.011)	(0.010)	(0.011)	(0.010)
Age effects	Yes	Yes	Yes	Yes	Yes	Yes
County-group fixed effects			Yes	Yes	Yes	Yes
Time fixed effects			Yes	Yes		
Rainfall time interactions [a)]			Yes	Yes	Yes	Yes
State-specific time effects					Yes	Yes
R-squared	0.068	0.062	0.062	0.056	0.064	0.059
Observations	900,538	22,280,639	900,538	22,280,639	900,538	22,280,639
County groups			344	356	344	356

Notes: In parentheses standard errors clustered at the county-group level. “OLS” does not include county-group fixed effects. “Basic FE” includes county-group fixed-effects and time-effects. “State-time FE” includes state-specific time effects. [a)] Rainfall-level time interactions and Rainfall-variance time interactions.

Source: US population census, agricultural census 1880-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations. See also Ruggles et al. (1990).

H. Robustness to the inclusion of the country or region of origin

The main regressions do already control for being born abroad. If I replace this variable by dummy variables for the country or region of origin (using the US as the reference category), the coefficients associated with rainfall variability do not change. This is shown in the table below. Although some of these dummies are statistically significant, i.e. some nationalities are indeed associated with higher/lower fertility levels, this is uncorrelated with the rainfall variance.

Table S.6. The effect of rainfall variability on fertility controlling for the country or region of origin

	(1) w/t nationality mix	(2) with nationality mix
USA		Ref.
Canada		0.147***
		(0.019)
Mexico		0.149***
		(0.039)
Central America		-0.053
		(0.096)
South-America		0.204*
		(0.105)
Northern Europe		0.235***
		(0.012)
UK/Ireland		0.218***
		(0.011)
Western Europe		0.325***
		(0.010)
Southern Europe		0.274***
		(0.018)
Eastern Europe		0.266***
		(0.013)
East Asia		0.294
		(0.245)
Southeast Asia		-0.349
		(0.348)
South Asia		0.099
		(0.134)
MENA		0.184**
		(0.082)
Africa		0.140
		(0.202)
Australasia		0.169
		(0.113)
Other		0.104*
		(0.054)

(Table continues next page)

Table S.6 continued …

Var rainfall (ln) x Farm hh	0.830***	0.838***
	(0.213)	(0.213)
Var rainfall (ln) x Farm hh x 1880	0.117	0.114
	(0.112)	(0.112)
Var rainfall (ln) x Farm hh x 1900	-0.074	-0.081
	(0.131)	(0.131)
Var rainfall (ln) x Farm hh x 1910	-0.238	-0.247*
	(0.147)	(0.148)
Var rainfall (ln) x Farm hh x 1920	-0.367**	-0.379**
	(0.156)	(0.158)
Var rainfall (ln) x Farm hh x 1930	-0.492***	-0.489***
	(0.158)	(0.158)
Full set of controls	Yes	Yes
County-group fixed effects	Yes	Yes
Rainfall level and Var rainfall time inter.	Yes	Yes
State-specific time effects	Yes	Yes
R-squared	0.081	0.081
Observations	945,038	945,038
County groups	344	344

Notes: In parentheses standard errors clustered at the county-group level. Cols. (1) and (2) use the same specification as col. (4) in Table 2. The set of controls includes all controls used in col. (4) in Table 2, including rainfall levels and rainfall level-farm household-time interactions.
Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

I. Robustness to the rainfall reference period

Table S.7. The effect of rainfall variability on fertility, robustness to definition of growing season

	(1) State-time FE growing season	(2) State-time FE all year
Rainfall (ln) x Farm hh	0.032	0.006
	(0.024)	(0.019)
Var rainfall (ln) x Farm hh	0.830***	1.316***
	(0.213)	(0.298)
Rainfall (ln) x Farm hh x 1880	0.018***	0.018***
	(0.003)	(0.003)
Rainfall (ln) x Farm hh x 1900	0.027***	0.029***
	(0.003)	(0.003)
Rainfall (ln) x Farm hh x 1910	0.028***	0.029***
	(0.004)	(0.004)
Rainfall (ln) x Farm hh x 1920	0.030***	0.033***
	(0.004)	(0.004)
Rainfall (ln) x Farm hh x 1930	0.031***	0.035***
	(0.003)	(0.003)
Var rainfall (ln) x Farm hh x 1880	0.117	0.025
	(0.112)	(0.201)
Var rainfall (ln) x Farm hh x 1900	-0.074	-0.372
	(0.131)	(0.236)
Var rainfall (ln) x Farm hh x 1910	-0.238	-0.580**
	(0.147)	(0.252)
Var rainfall (ln) x Farm hh x 1920	-0.367**	-0.921***
	(0.156)	(0.275)
Var rainfall (ln) x Farm hh x 1930	-0.492***	-1.086***
	(0.158)	(0.272)
Full set of controls	Yes	Yes
County-group fixed effects	Yes	Yes
Rainfall level and Var rainfall time inter.	Yes	Yes
State-specific time effects	Yes	Yes
R-squared	0.081	0.074
Observations	945,038	945,038
County groups	344	344

Notes: In parentheses standard errors clustered at the county-group level. Cols. (1) and (2) use the same specification as col. (4) in Table 2. The set of controls includes all controls used in col. (4) in Table 2.
Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

J. Robustness to changing definition of a farm-household between 1880 and 1900

As mentioned in the main paper the definition of a farm household slightly changed over time. For 1870 and 1880 IPUMS coded a household as "farm household" if it contained at least one person with the occupation "farmer". In later years the coding was based on the enumerator's explicit identification of a farm (cultivated land, production and use of agricultural labor). To check the robustness of the results to this change, I re-estimate the specification of col. (4) in Table 2 without using the years 1870 and 1880. The Table below compares both estimates, with and without these two years. It can be seen, that the results stay roughly the same if these two years are excluded from the estimation suggesting that the definition of farm households in 1870 and 1880 does not drive my results.

Table S.8. The effect of rainfall variability on fertility, robustness to definition of farm households

	(1) all years	(2) w/t 1870 & 1880
Var rainfall (ln) x Farm hh	0.830***	0.614***
	(0.213)	(0.176)
Var rainfall (ln) x Farm hh x 1880	0.117	
	(0.112)	
Var rainfall (ln) x Farm hh x 1900	-0.074	
	(0.131)	
Var rainfall (ln) x Farm hh x 1910	-0.238	-0.155
	(0.147)	(0.110)
Var rainfall (ln) x Farm hh x 1920	-0.367**	-0.289***
	(0.156)	(0.106)
Var rainfall (ln) x Farm hh x 1930	-0.492***	-0.417***
	(0.158)	(0.103)
Full set of controls	Yes	Yes
County-group fixed effects	Yes	Yes
Rainfall level and Var rainfall time inter.	Yes	Yes
State-specific time effects	Yes	Yes
R-squared	0.081	0.062
Observations	945,038	552,954
County groups	344	343

Notes: In parentheses standard errors clustered at the county-group level. Cols. (1) and (2) use the same specification as col. (4) in Table 2. The set of controls includes all controls used in col. (4) in Table 2, including rainfall levels and rainfall level-farm household-time interactions.

Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

K. Robustness to the use of IPUMS household weights

Table S.9. Robustness check using household weights

	(1) w/t weights	(2) with weights
Var rainfall (ln) x Farm hh	0.830***	0.826***
	(0.167)	(0.172)
Var rainfall (ln) x Farm hh x 1880	0.117	0.112
	(0.167)	(0.165)
Var rainfall (ln) x Farm hh x 1900	-0.074	-0.074
	(0.169)	(0.167)
Var rainfall (ln) x Farm hh x 1910	-0.238	-0.231
	(0.190)	(0.188)
Var rainfall (ln) x Farm hh x 1920	-0.367**	-0.365**
	(0.184)	(0.182)
Var rainfall (ln) x Farm hh x 1930	-0.492***	-0.486***
	(0.164)	(0.161)
Full set of controls	Yes	Yes
County-group fixed effects	Yes	Yes
Rainfall level and Var rainfall time inter.	Yes	Yes
State-specific time effects	Yes	Yes
R-squared	0.091	0.096
Observations	945,038	945,038
County groups	344	344

Notes: In parentheses standard errors clustered at the county-group level. Cols. (1) and (2) use the same specification as col. (4) in Table 2. The set of controls includes all controls used in col. (4) in Table 2, including rainfall levels and rainfall level-farm household-time interactions. Household weights are those provided by IPUMS.
Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

L. Testing the relevance of time lags and leads

Table S.10 shows specifications with lags (col. (2)) and leads (col. (3)). As can be seen lagged rainfall variability is insignificant. Future rainfall variability is also insignificant if interacted with being a farm household. The linear effect is significantly negative.

Table S.10. The effect of rainfall variability on fertility using decade-specific rainfall variability, Test of lags and leads

	(1)	(2)	(3)
Rainfall (ln), lag one decade		-0.242***	
		(0.086)	
Var rainfall (ln), lag one decade		0.219	
		(0.268)	
Rainfall (ln) x Farm hh, lag one decade		-0.077	
		(0.090)	
Var rainfall (ln) x Farm hh, lag one decade		0.431	
		(0.266)	
Rainfall (ln)	-0.048	0.074	0.157
	(0.074)	(0.096)	(0.099)
Var rainfall (ln)	-0.167	0.679**	0.282
	(0.238)	(0.269)	(0.344)
Rainfall (ln) x Farm hh	-0.033	-0.049	-0.088
	(0.074)	(0.100)	(0.099)
Var rainfall (ln) x Farm hh	0.440**	0.905***	0.669**
	(0.220)	(0.290)	(0.321)
Rainfall (ln), lead one decade			-0.181*
			(0.106)
Var rainfall (ln), lead one decade			-1.205***
			(0.288)
Rainfall (ln) x Farm hh, lead one decade			0.075
			(0.104)
Var rainfall (ln) x Farm hh, lead one decade			0.042
			(0.280)
Full set of controls	Yes	Yes	Yes
County group fixed effects	Yes	Yes	Yes
State-specific time effects	Yes	Yes	Yes
R-squared	0.062	0.056	0.056
Observations	558,342	347,137	304,367
County groups	355	354	354

Notes: In parentheses standard errors clustered at the county-group level. The full set of controls includes age group dummies, literacy of the women and of her spouse, whether the household is a farm household, migration status (birth place), average land value per acre, average land size per farm and average value of machinery per farm. Col. (1) estimates the specification of Table 3, col. (3). Col. (2) covers the period 1910 to 1930. Col. (3) covers the period 1900 to 1920.
Source: US population census 1900-1930, agricultural census (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

M. Ruling out timing effects

I divided the sample into a subsample with all women younger than 25 years and a subsample with all women 25 years and older allowing to check whether the rainfall risk effect is only driven by younger women. This could be the case if in fact rainfall variability did not induce parents to have more children but only to have them earlier. For older women that bias, if it exists, should have vanished. The estimates show that splitting the sample by women's age does not change the previous results. For both subsamples, higher rainfall variability is associated with higher fertility. The effect has a similar size for both age groups at the end of the 19th century. Yet, a sizeable decline of the rainfall variability effect as time progresses can only be observed for the older age group. For the younger group the rainfall-time-farm interactions are also negative for more recent years but not statistically significant. Overall, these results make it unlikely that the rainfall risk effect on fertility is only driven by women in farm households who have their children earlier.

Table S.11. The effect of rainfall variability on fertility, ruling out timing effects

	(1)	(2)	(3)	(4)
	Younger	Younger	Older	Older
Var rainfall (ln) x Farm hh	0.808***	0.815***	0.701***	1.068***
	(0.173)	(0.244)	(0.186)	(0.304)
Var rainfall (ln) x Farm hh x 1880		0.204		-0.034
		(0.187)		(0.204)
Var rainfall (ln) x Farm hh x 1900		-0.143		-0.202
		(0.204)		(0.228)
Var rainfall (ln) x Farm hh x 1910		0.110		-0.514**
		(0.234)		(0.242)
Var rainfall (ln) x Farm hh x 1920		0.028		-0.626**
		(0.211)		(0.251)
Var rainfall (ln) x Farm hh x 1930		-0.216		-0.725***
		(0.181)		(0.269)
Full set of controls	Yes	Yes	Yes	Yes
County-group fixed-effects	Yes	Yes	Yes	Yes
Time fixed-effects	Yes		Yes	
Rainfall time interactions[a)]		Yes		Yes
State-specific time-effects		Yes		Yes
R-squared	0.106	0.109	0.060	0.064
Observations	270,182	270,182	674,856	674,856
County-groups	344	344	344	344

Notes: In parentheses standard errors clustered at the county-group level. The full set of controls includes age group dummies, literacy of the women and of her spouse, whether the household is a farm household, migrant status (birth place), average land value per acre, average land size per farm, average value of machinery per farm rainfall levels and rainfall level-farm household-time interactions. [a)] Rainfall-level time interactions and Rainfall-variance time interactions.

Source: US population census, agricultural census 1880-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

N. Robustness to the inclusion of crop choices

Table S.12. The effect of rainfall variability on fertility controlling for crop choices

	(1) w/t crops	(2) with crops
Var rainfall (ln) x Farm hh	0.830***	0.815***
	(0.213)	(0.211)
Var rainfall (ln) x Farm hh x 1880	0.117	0.112
	(0.112)	(0.112)
Var rainfall (ln) x Farm hh x 1900	-0.074	-0.056
	(0.131)	(0.129)
Var rainfall (ln) x Farm hh x 1910	-0.238	-0.224
	(0.147)	(0.145)
Var rainfall (ln) x Farm hh x 1920	-0.367**	-0.360**
	(0.156)	(0.153)
Var rainfall (ln) x Farm hh x 1930	-0.492***	-0.486***
	(0.158)	(0.155)
Wheat		-0.004
		(0.011)
Rye		-0.001
		(0.006)
Indian Corn		0.042*
		(0.024)
Oats		-0.001
		(0.012)
Barley		-0.005
		(0.004)
Buckwheat		0.010*
		(0.005)
Rice		0.020**
		(0.008)
Tobacco		0.005
		(0.004)
Cotton		0.013*
		(0.007)
Potatoes		0.163***
		(0.030)
Full set of controls	Yes	Yes
County-group fixed effects	Yes	Yes
Rainfall level and Var rainfall time inter.	Yes	Yes
State-specific time effects	Yes	Yes
R-squared	945,038	945,038
Observations	0.074	0.075
County groups	344	344

Notes/Sources see next page

Notes: In parentheses standard errors clustered at the county-group level. Cols. (1) and (2) use the same specification as col. (4) in Table 2. The set of controls includes all controls used in col. (4) in Table 2, including rainfall levels and rainfall level-farm household-time interactions.
Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

To address a potential omitted variable bias due to crop choices that are possibly correlated with both rainfall variability and fertility, I created a set of dummy variables for each county and each census year where each dummy variable stands for a specific crop and takes the value one if that crop has been cultivated in that year in that county and zero otherwise. I consider the following crops: wheat, rye, Indian corn, oats, barley, buckwheat, rice, tobacco, cotton and potatoes. The table below shows the results. In the first column I replicate the results of col. (4) in Table 2. In the second col. I re-estimate the same model but include the crop pattern as control. As can be seen the coefficients associated with rainfall variability are almost identical in both estimations. Most of the crop dummies are insignificant. Some are associated with higher fertility, such as Indian corn, buckwheat, rice, cotton and potatoes, but the effects are relatively small and again they do not change the results with respect to rainfall variability.

O. Robustness to rainfall risk-induced child mortality as an alternative channel

The results would be biased if the increased fertility in counties with higher rainfall variability was partly a response to increased infant and child mortality induced by the rainfall variability. If droughts led to shortages of food and diseases that caused children to die more frequently, parents in counties with high rainfall variability may have increased their fertility to compensate (Olsen, 1980).

Unfortunately, mortality data for that period of time is scarce. Death registration procedures were universally in place only after 1933. Until 1910 deaths were only registered in the more industrialized and urbanized states of the Northeast. After 1910, death registries were slowly introduced in the rest of the country (Pope, 2000). Based on the little data that existed, Haines (2008) estimates that the infant mortality rate for white people declined from 176 per 1000 live births in 1870, to 111 in 1900 and 60 in 1930 (Haines, 2008; see also Cutler and Meara, 2001). The general decline in mortality in the late nineteenth century was largely the result of improvements in public health and sanitation (see, e.g., Cutler and Miller, 2005; Cutler et al., 2006; Haines, 2008). Better diets, clothing and shelter also played a role. Later heating and clean cooking made accessible through electrification further lowered infant mortality (Lewis, 2014). Medical interventions to curb specific infectious and parasitic diseases led to significant mortality reductions only well into the twentieth century (Haines, 2008; Fishback et al., 2007). Malaria was a major cause of death until its eradication in 1950 (Hong, 2007).

With the data at hand, it is not possible to control for infant and child mortality systematically. However, there is little indication that mortality was highly correlated with rainfall variability. Mosquito breeding may increase with rainfall but the effect of rainfall variability on breeding is rather ambiguous. Droughts would certainly reduce breeding and hence reduce rather than increase mortality. Famines were very uncommon in the US at that time, given the abundant land resources. Even during the dust bowl of the 1930s, mortality did not reach levels that are typically associated with famines (see, e.g., Worster, 1979; Hansen and Libecap, 2004). This makes it unlikely that rainfall variability led to higher fertility through increased mortality.

Nevertheless, to further rule out that such a bias drives the results, I use the census data of 1900 and 1910, where women were not only asked about the number of children in their household but were also asked to report the number of children ever born and the number of children that survived. I use these two variables to estimate child mortality rates for each year in each county. I calculate the rate separately for farm and non-farm households. With this data I test whether child mortality was correlated with fertility, whether the mortality effect off-sets part of the rainfall variability effect and whether rainfall variability in turn can explain child mortality. Table S.13 shows the results.

Table S.13. The effect of rainfall variability on fertility, ruling out child mortality as the main channel

	(1) Ch. born	(2) Ch. born	(3) Ch. born	(4) Ch. mort.
Rainfall (ln) x Farm hh	0.172		0.169	0.004
	(0.105)		(0.103)	(0.005)
Var rainfall (ln) x Farm hh	2.203***		2.185***	0.026
	(0.751)		(0.743)	(0.036)
County-level child mort.		0.781***	0.747***	
		(0.278)	(0.283)	
Full set of controls	Yes	Yes	Yes	Yes
County-group fixed-effects	Yes	Yes	Yes	Yes
Rainfall level and Var rainfall time inter.	Yes	Yes	Yes	Yes
State-specific time-effects	Yes	Yes	Yes	Yes
R-squared	0.252	0.252	0.252	0.093
Observations	493,693	493,693	493,693	493,693
County-groups	341	341	341	341

Notes: In parentheses standard errors clustered at the county-group level. The estimates use all ever-married white women in rural areas, not just the age group 15-39 as in Table 2 and 3. “Ch. born” stands for the children ever born. “Ch. mort” stands for county-year-level farm and non-farm household-group-specific child mortality. The full set of controls includes age group dummies, literacy of the women and of her spouse, whether the household is a farm household, migration status (birth place), average land value per acre, average land size per farm and average value of machinery per farm.
Source: US population census 1900-1910 (IPUMS); PRISM Precipitation dataset; own estimations.

Col. (1) in Table S.13 re-estimates Equation (1) using the data of 1900 and 1910 only and instead of using the number of children below five in the household it uses the number of children ever born as dependent variable. The differential effect of rainfall variability on fertility between farm and non-farm households is qualitatively similar to the effects shown in Table 2. Col. (2) shows that indeed, as expected, higher child mortality is associated with higher fertility, i.e. parents seem to compensate. An increase in the mortality rate by 10 percentage points would be associated with an increase in fertility by 0.078 children per mother. If as in col. (3) mortality and rainfall variability are introduced jointly, both the effect of mortality and the effect of rainfall variability are more or less unchanged. Finally, col. (4) suggests that rainfall variability does not affect child mortality. Taken together, these results make it unlikely that the effect of rainfall variability is transmitted through rainfall risk-induced child mortality.

P. Robustness to rainfall risk and returns to education as an alternative channel

Another potential concern is that differences in rainfall risk lead to differences in returns to education and, in consequence, also to returns to quantity (i.e. the number of children). In this case the higher fertility was not the result of risk mitigation but the response to low returns to quality and hence high returns to quantity. This argument is not very different from the one tested in this paper as the underlying hypothesis is that rainfall-risk increases the return to quantity. Yet, to exclude that the effect only goes through education, I re-estimated Equation (1) controlling for the children's year and county-specific school enrolment rate, i.e. I tested whether there is still a positive association between rainfall variability and fertility, once the effect of parental educational investments on fertility is controlled.

The census data reports for each child in the household whether this child is enrolled in school or not. In the second half of the 19th century school enrollment rates for 5 to 19-year olds fluctuated around 50% to 60% and started to rise from 1900 onwards quite rapidly. Since older children are likely to have left the household already, I focus in what follows on the age group 6 to 11. For every census year I calculate for each woman the share of her children in that age group that is enrolled in school and average these rates at the county level. Using the year-specific county-level enrollment rate instead of the household level enrollment rates should mitigate endogeneity problems to some extent.

Table S.14 shows the results. The comparison of cols. (1), (2) and (3) shows that the effect of rainfall risk on farm households' fertility is not significantly changed if school enrolment is among the controls. School enrolment itself is negatively correlated with fertility as the quality-quantity trade-off model predicts, however the effect is small and statistically insignificant. Rainfall variability in turn is not correlated with enrolment rates (col. (4)). Based on these findings it is unlikely that the effect of rainfall variability on fertility dominantly passes through reduced educational investments.

Table S.14. The effect of rainfall variability on fertility, ruling out returns to education as the main channel

	(1) Fertility	(2) Fertility	(3) Fertility	(4) Share enroll.
Rainfall (ln) x Farm hh	0.057**		0.057**	0.004
	(0.025)		(0.025)	(0.003)
Var rainfall (ln) x Farm hh	0.648***		0.649***	0.016
	(0.166)		(0.165)	(0.020)
Share enrolled (county)		-0.028	-0.028	
		(0.018)	(0.018)	
Full set of controls	Yes	Yes	Yes	Yes
County-group fixed-effects	Yes	Yes	Yes	Yes
Rainfall level and Var rainfall time inter.	Yes	Yes	Yes	Yes
State-specific time-effects	Yes	Yes	Yes	Yes
R-squared	0.074	0.074	0.074	0.793
Observations	945,038	945,038	945,038	945,038
County-groups	344	344	344	344

Notes: In parentheses standard errors clustered at the county-group level. The "share enrolled" measures the year and county-specific school enrolment rate of children 6 to 11 years old. The full set of controls includes age group dummies, literacy of the women and of her spouse, whether the household is a farm household, migration status (birth place), average land value per acre, average land size per farm and average value of machinery per farm.
Source: US population census, agricultural census 1870-1930 (IPUMS, NHGIS); PRISM Precipitation dataset; own estimations.

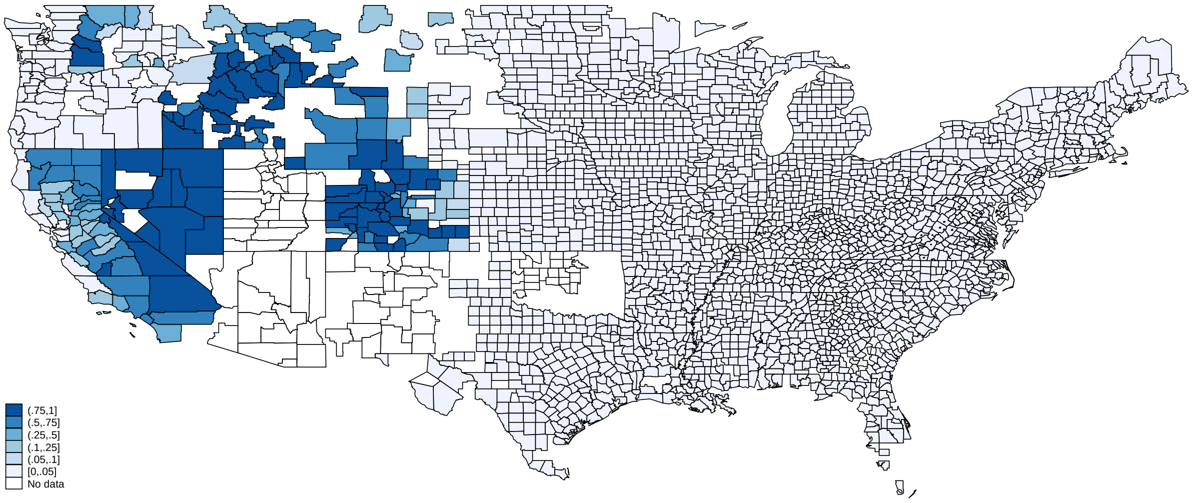
Q. Robustness to endogenous migration

Unobserved preferences that may have determined both the destination of settlers and fertility present a threat to the identification used in this paper, i.e. if parents with a preference for fewer children migrated systematically to areas with lower rainfall variability. Dunlevy (1980) and Dunlevy and Saba (1992) highlight four factors explaining the choice of destination of settlers in the nineteenth century: population density, the availability of land, the distance to the port of entry and prevailing per capita income. The latter was generally seen as an indicator of expected income or job opportunities. Gallaway et al. (1974) provide some evidence that native-born Americans tended to migrate to the less densely populated states whereas immigrants were more inclined to locate in the more densely populated states. The presence of friends and relatives was also an attraction to immigrants (Gallaway et al., 1974). According to Dunlevy (1980), the exact weather conditions played only a minor role (see also Dunlevy and Saba, 1992). He shows that, if anything, some migrants were attracted by destinations that showed a climate like the one they had in their home country. Libecap and Hansen (2002) also argue that settlers had only little means of accessing systematic weather information from other regions. There were also no warnings of droughts. Moreover, the so-called dryfarming doctrine was presented to settlers as a remedy for drought (Libecap and Hansen, 2002). All this is of course not enough to rule out the potential concerns about endogeneity, but at least there is nothing that explicitly suggests that, conditional on other geographic characteristics including rainfall levels, rainfall variance was an important determinant for the destination choice of immigrants and native-born migrants.

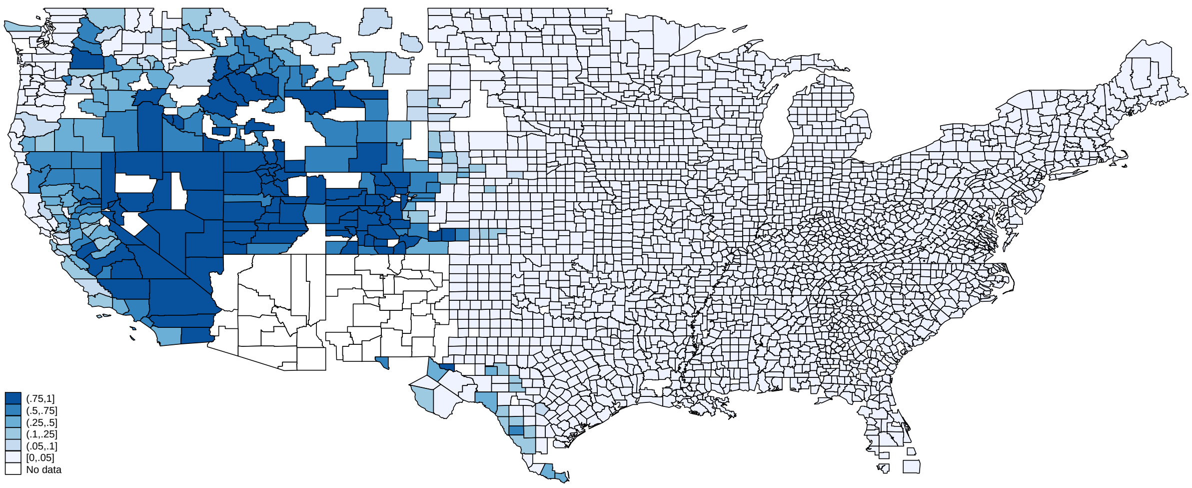
Moreover, the estimates presented in Table 2 show that the rainfall variability effect is robust to the inclusion of variables controlling for “having been born in a different state” and “having been born abroad”. The results also hold if like in col. (8) all women that migrated are excluded from the sample. Hence, based on these findings, I believe that selective migration is not the main driver of the results shown in this paper.

R. The role of risk mitigating factors

Figure S.3. Share of irrigated agricultural land



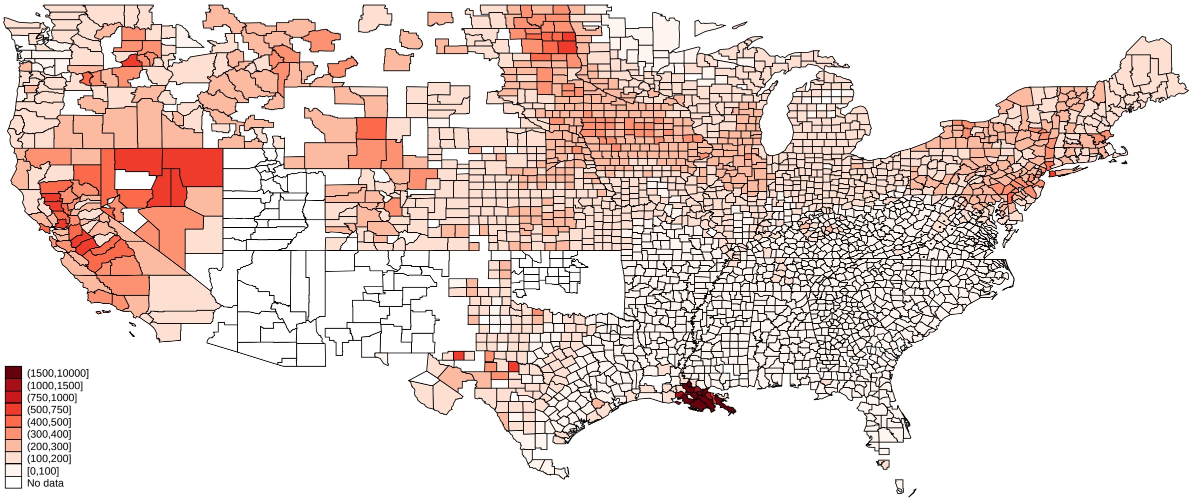
1900



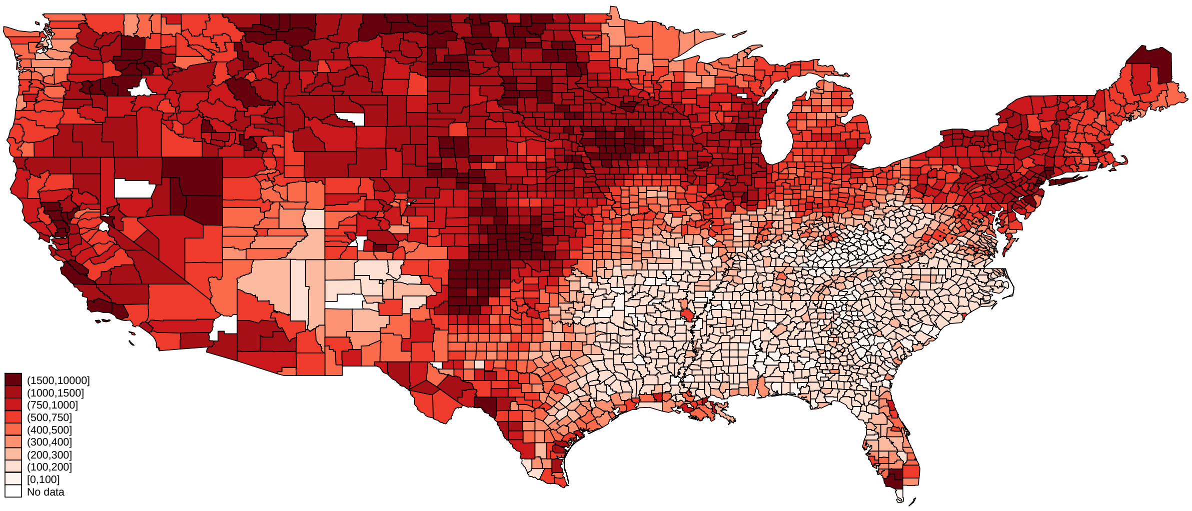
1910

Notes: Darker areas show areas with a higher share of irrigated land. Each map uses the same scale. White spaces indicate areas where no agricultural census data was collected.
Source: US agricultural census, 1900 and 1910 (NHGIS).

Figure S.4. Value of agricultural machinery in use (constant USD)



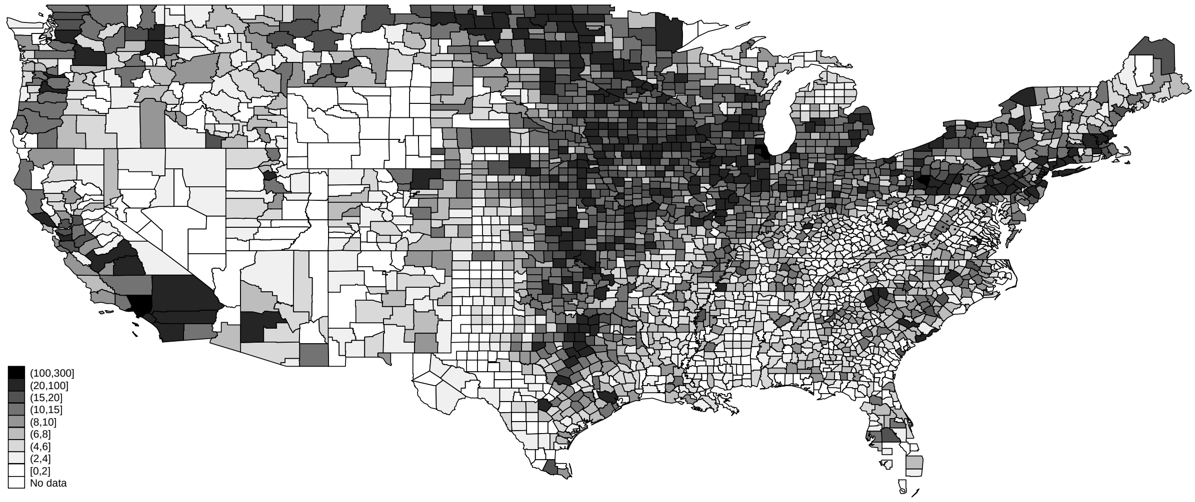
1900



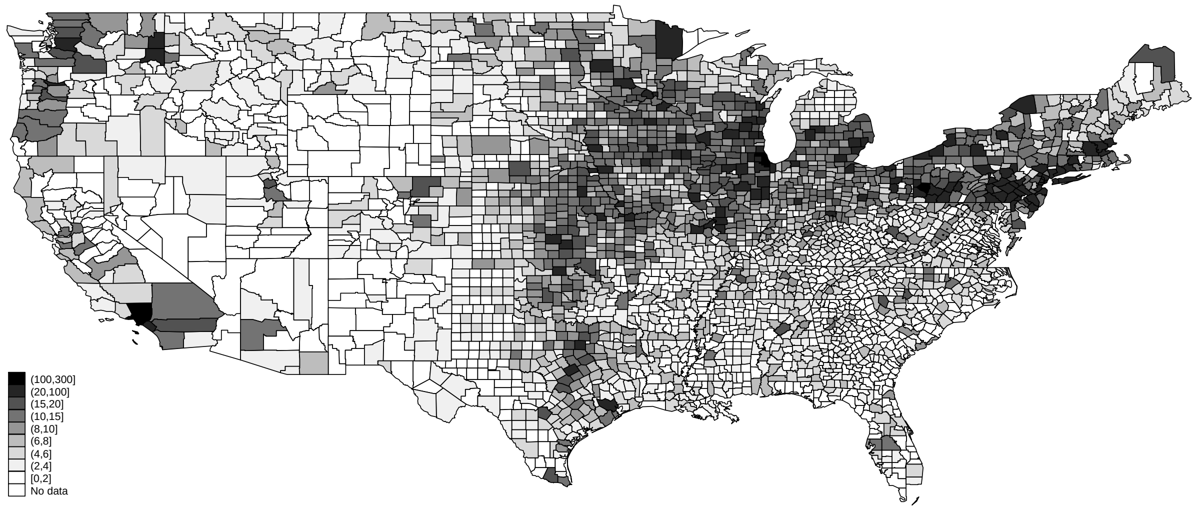
1930

Notes: Darker areas show areas with a higher value of machinery in use. Each map uses the same scale. White spaces indicate areas where no agricultural census data was collected.
Source: US agricultural census, 1900 and 1930 (NHGIS).

Figure S.5. Spread of banks



1920



1930

Notes: Darker areas show areas with more banks. Each map uses the same scale. White spaces indicate areas where no survey data was collected.
Source: Survey on Bank and Bank deposits, 1920 and 1930.

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