E Pluribus, Pauciores (Out of Many, Fewer): Diversity and Birth Rates

David H. Solomon
(Boston College)

First Draft: July 2024
This Draft: January 2025

Abstract: In the United States, local measures of racial and ethnic diversity are robustly associated with lower birth rates. A one standard deviation decrease in racial concentration (having people of many different races nearby) or increase in racial isolation (being from a numerically smaller race in that area) is associated with 0.064 and 0.044 fewer children, respectively, after controlling for many other drivers of birth rates. Racial isolation effects hold within an area and year, suggesting that they are not just proxies for omitted local characteristics. This pattern holds for many racial groups, is evident in different vintages of US census data (including before the Civil War), holds internationally, and persists when diversity is instrumented by immigration shocks. Diversity is associated with lower marriage rates and marrying later. These patterns are related to homophily (the tendency to marry people of the same race), as the effects are stronger in races that intermarry less and vary with sex differences in intermarriage. Similar patterns exist with income decile, though the effects are half to two-thirds smaller than race share effect. The rise in racial diversity in the US since 1970 explains between 20% and 44% of the decline in birth rates during that period.

Contact at david.solomon@bc.edu. I would like to thank Bill Cready, John Griffin, Umit Gurun, Jordan Nickerson, Eric So, Robert VerBruggen, James Weston, and seminar participants at Boston College, CUHK Shenzhen, PHBS, and the University of Western Australia for helpful comments and suggestions. All remaining errors are my own.

Since the middle of the 20th Century, the United States has experienced two major demographic changes. The first is a large increase in racial diversity. This is dramatic at the national level, but varies considerably at the local level. This trend arose both through large increases in immigration, and policies designed to reduce racial segregation. The second major change is a considerable decline in birth rates. Births per woman (total fertility rate, or TFR) have fallen by more than half, from approximately 3.6 total births per women in 1960, down to an all-time-low of 1.64 in 2020, which falls below the replacement birth rate needed to sustain the population. Declining fertility during a period of economic growth is prima facie surprising (Becker 1960). This is especially so for the recent declines since 2007, which are puzzling and hard to explain quantitatively (Kearney, Levin, Pardue 2022).

I pose a simple question which, to my understanding, has not been previously considered – are these two facts related? I argue that they are. I present evidence that birth rates are robustly lower in areas of greater local racial and ethnic diversity, after controlling for a wide array of potential confounding variables. I consider two slightly different aspects of racial variation within a community. The first, racial concentration, is a Herfindahl index of racial groups within a local area (as in Putnam 2007). Intuitively, this captures the difference between an area with many groups in small proportions, versus mostly one major group, and maps to what is often just termed "diversity". The second measure is a consequence of racial diversity at the individual level, which I call "racial isolation" – the race share of the population for that person. Intuitively, this captures how many people in your area are "like you" in racial and ethnic terms.

There are strong reasons from the economics of marriage to predict that higher diversity may result in reduced birth rates. As the number of people of different races in each area has increased, people have fewer encounters with others of their own race. Various studies document that people on average have a preference for homophily – they prefer to marry those with similar characteristics, particularly people of the same race (Bedi 2000, Hwang 2012, and many others). These preferences are evident even in reproductive technologies, when the choice is just over traits for one's children, rather than the choice of partner (Daniels and Heidt-Forsythe 2012). If the number of potential same-race partners drops in an area, then either one incurs higher search costs to find a good match, or the quality of matches decreases, or both.

While the evidence for homophily is large, the possibility that this may have implications that link the rise in diversity and the decline in birth rates does not seem to have been considered.

Using US census and American community survey data since 1850, I find that both race Herfindahl and race share variables are robustly associated with higher birth rates. Being in a more racially concentrated area, and being part of a larger group within that area, are both associated with having more children at the time of survey, almost irrespective of the level of controls. With full controls, a one standard deviation increase in race share predicts the average woman aged 18-40 has 0.064 more children, with *t*-statistics generally above 5. For race Herfindahl, in my preferred specification a one standard deviation increase is associated with 0.044 more children. I construct these variables at the finest geographic level available– city, then county, then detailed metro area. The baseline race definition uses the census' broad racial classification plus a separate category for Hispanic/Latino ethnicity.

The use of granular panel data combined with high dimensional fixed effects and demographic controls considerably narrows the set of plausible explanations for my findings. For instance, the use of state-by-year fixed effects helps mitigate concerns that the negative link between fertility and diversity is attributable to general economic or cultural attributes of a state. The use of race-by-state and race-by-time fixed effects precludes many explanations about general racial differences within a state. I control explicitly for demographics (education, income, citizenship, employment, marital status), demographics interacted with state and year fixed effects, local area attributes (population, college fraction, income, fraction recently moved to the area, employment, age), and local area attributes interacted with year fixed effects. The effect is large and highly significant in every specification. At a minimum, the most obvious omitted variables and their associated explanations do not seem to explain the effect.

Because the Herfindahl measure is constructed as the sum of the squared fractions of each group, it is necessarily positively correlated with race share. The Herfindahl measure has the same value for everyone in an area that year, regardless of their race. However, even within a specific area and year, an individual's race share can vary further based on whether they belong to a more or less populous racial group relative to the area's overall racial composition. This means that for race share, local-area controls can be replaced by an area-by-year fixed effect. If more diverse communities are bigger, richer, denser, have higher costs of raising children, or any other omitted factors, these are absorbed in this specification. Only variation *within* a local area and year is used, comparing larger and smaller groups within the area (after controlling for patterns in that race-by-state, race-by-year, etc.).

Racial isolation effects on birth rates survive these area-by-year fixed effects, and their inclusion does not greatly change the parameter estimates. The effects of racial isolation are not due to any general omitted area characteristics that apply to all residents, but vary with the size of one's racial group. The consistent results for racial isolation and racial concentration suggest that they reflect a similar fundamental process, although directly testing this is difficult. While I do not explicitly argue for a causal interpretation, I do not preclude one either. Consistent with causality, when I instrument for race share using county-level immigration shocks that arise from pre-existing ethnic networks (as in Burchardi et al. (2024)), I continue to observe a positive and statistically significant relationship between racial isolation and birth rates. These immigration shocks, which stem from historical settlement patterns, provide plausibly exogenous variation in local racial composition. However, establishing the validity of the exclusion restriction – the assumption that immigration shocks affect birth rates only through their impact on demographic composition – is challenging. Conceptually, it is unclear whether it is more accurate to say that the immigration shock *causes* the increase in diversity, or the immigration shock *is* the increase in diversity, in a purely mechanical sense. In the latter view, the main question is what pathway it is affecting, such as economic opportunities, culture, institutional structures, or social dynamics. To better understand the mechanisms behind the main correlation, I employ targeted sub-tests designed to isolate specific factors and rule out alternatives, while acknowledging the inherent complexity of the relationship between diversity and birth rates.

Importantly, the negative association between diversity and birth rates is present throughout U.S. history. It holds before the 21st century, before the Civil Rights Act, before World War 2, before the 20th Century, and, most surprisingly, before the Civil War. That is to say, racial isolation significantly lowers birth rates even in periods when slavery was legal, in the 1850 and 1860 censuses, with a one standard deviation increase in race share being associated with 0.33 more children. The parsimonious explanation is that whatever is driving the effect must be broadly present in many eras. These results militate strongly against explanations that focus on specific individual events in the history of US race relations.

Second, the effects are present for many different racial groups. In the tightest specification, race share has positive and significant effects at the 10% level or better for seven out of ten groups (whites, blacks, native Americans, Chinese, Japanese, other, and two races – other Asian/Pacific Islander, Hispanic and three or more races are insignificant). In this specification, whites show the fourth smallest magnitude effect. Across specifications, the most uniformly positive and significant results are for whites, native Americans, and two races. The effect is not limited to whites or any single racial group, nor is it obviously attributable to black/white race relations. A likely explanation should apply to many different races.

Third, the findings are unlikely to be driven by selection effects related to mobility. For example, one possible explanation for the results is if younger people live in diverse areas but then move to racially homogenous areas when they have children. I redo the analysis for women who have not been geographically mobile - those living in the same state they were born in, or who have not moved in the years prior to the ACS survey. Across all subsamples, I observe consistent effects, suggesting that selective migration patterns are not the primary explanation for the results.

Fourth, I find that the result is present outside the United States, using international census data for countries that record racial classifications. Racial diversity is strongly associated with lower birth rates in Africa (South Africa, Mozambique, Zimbabwe), and also in a small sample of UK data. Central and South American countries show mixed evidence, with some having strong negative effects of diversity (Ecuador, El Salvador), others having significant positive effects (Brazil), and a number being insignificant or inconsistent in sign. These results do not reveal an obvious pattern of what drives the variation in effects across countries, but suggest that explanations unique to the U.S. are unlikely to be sufficient.

Next, I explore specific predictions of homophily. While homophily is a general pattern, it is unlikely that all races have the same revealed preference for same-race marriage at all points in time. If interracial marriage is more common for a given race and year, racial isolation should matter less for fertility. Second, within a race and year, interracial marriage rates also differ by sex, as women of a given race may “marry out” of their race at higher rates than men, or vice versa. This predicts different effects across the sexes – if women of a given race marry out more frequently, then racial isolation effects will bite more for men of that race than for women (as the men are more dependent on same-race women than those women are dependent on them). I find both predictions borne out in the data. More intermarriage reduces the effect of racial isolation on fertility, and more intermarriage by women relative to men reduces the racial isolation effect for women of a race relative to men of that race. This is strongly consistent with homophily playing an important role in the main effects, and is not easily explainable by other channels.

To further test if the results are due to the difficulty of finding a desired partner, I examine other relationship outcomes. A one standard deviation increase in race share is associated with a 1.2 percentage point higher probability of a woman being currently married, a 1.2 percentage point higher probability of having ever being married, and a lower age of first marriage (by 2.3 months). It is somewhat negatively related to the probability of divorce, though the effects are weaker. Diversity effects do not appear to be limited to the narrow costs of raising children, but also to the difficulty of finding a martial partner.

Another prediction of homophily is that if people have preferences for similar partners along other demographic dimensions, I ought to find demographic share effects for other variables. The evidence here is more mixed – I find robust positive effects for income decile share that are around half to two thirds as large as the race share effect, consistent with the homophily among income levels documented in in Greenwood et al (2014). However, other variables like education and age do not show the same effects.

An alternative mechanism for the results is social trust. As Putnam (2007) describes: *“[I]n more diverse settings, Americans distrust not merely people who do not look like them, but even people who* do*. … Diversity seems to trigger not in-group/out-group division, but anomie or social isolation.”.* Reduced social trust could contribute both to the difficulty of finding a partner, and choices over the number of children. While the predictions and metrics of trust are not as sharp as for homophily, I find that state level social trust measures are positively related to birth rates, and including them in the regressions reduces the race share effect by around a quarter to a third. This holds both using Putnam (2007) survey measures of generalized trust, or Facebook social capital data from Chetty et al. (2022) (e.g. local volunteering rates).

Further evidence that homophily is unlikely to be the entire explanation comes from the fact that both race Herfindahl and race share measures show separate effects when included in the same regression. This holds even when the Herfindahl index is calculated only among races other than that of the individual in question. This helps us rule out the possibility that the Herfindahl index is merely capturing non-linear effects of race share. Under homophily, the main question is the availability of potential same-race matches in one's vicinity, which is captured by the race share measure. It is unclear why, after conditioning on this, variation in the concentration of other races should matter, whereas under social trust, this aspect is important. That is, under homophily, if whites are 60% of the population in an area, this determines their chances of meeting and marrying each other, and it makes no difference whether the remaining 40% is a single race, or many races. Empirically, this variation matters (although it is subsumed by area fixed effects, and thus hard to tightly distinguish from other area traits). The importance of racial concentration does not point to social trust specifically, but it is consistent with it, and is difficult to explain with homophily alone.

The final tests link time-series evidence of declining fertility rates in the U.S. to changes in diversity, related to the initial motivating facts. The level of identification for time series changes is much weaker, but because the patterns are so stark and so poorly explained in quantitative terms, it is an interesting question whether diversity has enough bite to potentially be an important driver of overall birth rates. Simple time-series regressions show that the decline in average racial isolation explains 44% of variation in U.S. TFR since 1971. If the effects of diversity on TFR are estimated from panel versions of the cross-sectional tests, in my preferred specifications the predicted TFR decline due to race share is 20-22% of the actual decline. Even with the most conservative sets of controls, the cross-sectional specifications explain 7.7% of the time series decline since 1971 (after controlling for race). In recent years, the methodologies diverge much more, however. Diversity explains considerable variation in birth rates over long horizons, and potentially is an important driver of the most puzzling changes in recent years.

These findings imply that diversity and birth rates have some fundamental tension between them. Homophily appears to contribute to this relation, as does social trust (though the evidence is more circumstantial), but these may not be the only driving factors. While it ought to go without saying, it bears emphasizing that nothing in this paper implies any value judgments about either the left-hand-side variable or the right-hand-side variable. Opinions on both vary wildly. There are people who like diversity and people who dislike it (mostly, in both cases, for reasons other than birth rates). Opinions on birth rates range from it being a crisis that they are too low (e.g., due to worries about the solvency of social benefit programs, or overall population declines), to being a crisis that they are too high (e.g., for environmental reasons). As a result, reasonable people may interpret the implications of these findings quite differently. I simply assert that understanding the drivers of these variables is crucial as long as they are important for *some* reason (as seems likely). Public policy is better-made when decision makers understand the relevant tradeoffs between key variables than when they do not understand them. Even if the patterns I document merely reveal other underlying factors that are not tied to differences in racial isolation directly, they are important to understand, as diversity and birth rates are some of the most important demographic changes of our age.

2. Literature Review

This paper is related to literature on the economics of fertility. The seminal work of Becker (1960) explained the decline in fertility during industrialization in the 19th century partly by the declining value of children for agricultural work.[1] Becker and Lewis (1973) propose a quantity-quality tradeoff theory between having more children and investing more resources (e.g., education) into each one. Recent surveys by Doepke and Tertilt (2016), Greenwood et al. (2017), and Doepke et al. (2022) describe the determinants of fertility. Women's decisions to have children are related to their labor market opportunities (Adsera 2005), and thus affected by drivers such as taxation (Guner et al. 2012, Bick and Fuchs-Schündeln 2017, and Borella et al. 2021), and access to education (Black, Devereux and Salvanes 2008, McCrary and Royer 2011). Fertility rates are also related to government spending on early childhood education programs (Olivetti and Petrongolo 2017), which function as a form of childcare, and such access is especially important for the decision to have multiple children (D'Albis et al. 2017).

---

[1] This coincided with increasing economic growth, thus the prima facie puzzling inverse relationship between income and fertility. In recent decades, high-income countries no longer exhibit a negative correlation between income and fertility (Hazan and Zoabi 2015 and Bar et al. 2018).

Other research considers the role of family planning. Goldin and Katz (2002) argue that improved access to birth control for single women in the 1970s increased women's incentive to invest in a career and delay marriage and childbearing. Kearney and Levine (2009) find similar fertility effects from Medicaid subsidies of contraception, and Myers (2017) finds fertility effects from abortion access. Finally, other papers have examined costs of family formation, including child car seat laws (Nickerson and Solomon 2024), and mortgage deregulation (Hacamo 2021). Fertility is also affected by cultural influences like social attitudes to mothers working, (Kleven et al. 2019) and TV shows (Kearney and Levine 2015).

Relative to this literature, my paper makes several contributions. First, existing theories have considerable difficulty explaining the large and consistent decline in fertility in the US since 2007. As Kearney, Levine and Pardue (2022) describe it: "*The Great Recession contributed to the decline in the early part of this period, but we are unable to identify any other economic, policy, or social factor that has changed since 2007 that is responsible for much of the decline beyond that."* I answer this challenge, and provide an explanation that is both new to the existing literature, and potentially helps explain recent declines. Unlike most existing birth rate drivers, diversity is a property of a *local area,* whereas many others are individual-level costs or benefits more directly related to child-raising and its substitutes.

Second, the findings provide an alternative explanation for the empirical demographic pattern whereby immigrants from high-fertility countries tend to converge over time to lower native levels (Dubuc 2012, Parrado and Morgan 2008, Sobotka 2008, Mulder and Wagner 2001, White, Moreno, and Guo 1995, Adsera and Ferrer 2015). The existing literature has mostly emphasized cultural transmission. My results suggest an alternative – immigrants are typically moving from places where they have a high race share, to places where they have a low race share, and so subsequent generations are expected to have lower fertility.

This paper also contributes to the research on homophily and matching in partner traits. As well as the previous cited literature on same-race preferences, there is also evidence of assortative matching based on income (Chiappori, Salanie and Weiss 2017, Greenwood et al. 2014, Fernandez et al. 2005, Schwartz and Mare 2005, and Chiappori et al. 2022). My paper shows an important implication of such matching -

when there is more local diversity along that dimension, marriage rates and birth rates are lower. While most of the evidence is about racial diversity, I find evidence for income-based diversity effects as well.

This paper is also related to the literature in political economy on the relationship between ethnic diversity and social trust. This documents negative connections between ethnic diversity and favorable outcomes, such as civic engagement (Costa and Kahn 2003), the provision of public goods (Alesina et al., 1999), and self-reported trust levels (Putnam 2007). Dinesen et al. (2020), in their comprehensive meta-analysis, document a significant negative correlation between ethnic diversity and social trust across 1,001 estimates derived from 87 studies. They argue that the consistent negative correlation across various types of social trust aligns with Putnam's (2007) theory of anomie (social isolation), which posits a universal decline in trust across diverse social settings. Surprisingly, none of these studies discussed in the meta-analysis examine fertility as an outcome. I contribute to these studies by documenting a new social outcome of diversity, and show a link to both trust and homophily as potential drivers.

2. Data and Variable Construction

2.1 Data Sources

Census data is obtained from IPUMS, a service of the Minnesota Population Center, which aggregates and standardizes census data from both US and international sources. U.S. census data is taken from decennial censuses from 1850 to 2010, plus yearly vintages of the American Community Survey (ACS) from 2000 through 2021. 1960 is excluded due to lacking local geographic information. International census data is taken from IPUMS International, for nearly all samples where race data is non-missing (United Kingdom, Mozambique, South Africa, Zimbabwe, Costa Rica, Cuba, El Salvador, Jamaica, Brazil, Colombia, Ecuador, Uruguay – I exclude very small samples from Suriname and Saint Lucia). U.S. Total Fertility Rate data and economic indicators (unemployment, GDP growth, inflation) are taken from the FRED website of the Federal Reserve Bank of St. Louis.

2.2 Main Variables

The main results of the paper relate local levels of racial diversity to birth rates. To do so, I have to unpack each of the component pieces – "birth rates", "local", "racial" and "diversity". For "birth rates", I mostly refer to the number of children a woman has living in her house at the time of the survey. This is not specifically a rate, but the results (in most specifications) control flexibly for age, among many other variables. Later, I turn the numbers and ages of children into annual birth outcomes (as in Nickerson and Solomon (2024)), when I can match birth decisions to area-level race data for the year of conception.

By "local", I refer to the community where the person resides, acknowledging that there is no single universally correct or optimal way to define this. With arbitrarily fine data, one could evaluate the different effects of houses on the same street, in the neighborhood, the town, the county, and the state. With public census data, things are complicated on two dimensions. First, the collection of different geographic levels varies over time. As the Appendix describes, the 1850 census collects information on city and detailed metro area. County information first starts in 1950, and metro area information ends in 2011. Some samples measure both city and county, others measure only one or the other. Even when multiple levels are available simultaneously, they do not nest each other, as there are cases of multiple counties within a city, and multiple cities within a county. City is generally a finer measure, however. Taking respondents where both city and county data are non-missing, the average number of cities per county is 5.35, while the average number of counties per city is 1.10. I thus take as the baseline geography measure:

-First city, if this is available

-If no city information is available, then county (if this is non-missing)

-If neither city nor county is available, then detailed metro area.

-If none of these are available, the observation is dropped in the main analysis.

When I consider state-level diversity variables, because these do not require finer geographical information, I include all residents of a state (even if they lack other geographic information). I omit women living in group quarters, although the results are essentially unchanged if they are included.

Second, "racial". There are numerous different ways to classify race. When using U.S. data, I follow the census race classifications. For broad race measures, they currently include nine categories – white, black/African American, American Indian or Alaska native, Chinese, Japanese, other Asian or Pacific Islander, other race, two major races, and three or more major races. In addition, they also ask about Hispanic or Latino ethnicity, which interacts with the above. So, one can be Hispanic white, Hispanic black, Hispanic other, or any other combination.[2]

The aim here is to map to how people construct their own identity. As the primary grouping, I put all Hispanic/Latino respondents in a single, separate category. In this respect, the shorthand use of "race", unless otherwise qualified, refers to these ten groupings (the nine census broad race groups, plus a tenth for Hispanic/Latino). The understanding is that this combines aspects of both race and ethnicity, in terms of how people construct their sense of identity. Including Hispanics/Latinos as a single group implicitly assumes that their sense of what it means to be surrounded by people "like them" covers other Hispanic/Latino people (rather than, say Hispanic whites feeling that Hispanic other are a different group). Some categories are unsatisfactory for this purpose no matter how it is done – it seems unlikely that "three or more major race" respondents only feel a sense of similarity with other "three or more major race" people, who may have entirely different combinations of race. All these measures are imperfect, and I later explore a number of other definitions, but using alternative definitions does not materially affect the results.

Finally, the last metric is diversity, measured as racial concentration and racial isolation. Both are linked by the idea of being surrounded by people who differ from you. Importantly, I do not mean "diversity" merely as a shorthand for "not white". This alternative conception aligns more closely with the race control variables themselves. I consider two specific aspects of diversity. First, racial concentration - whether there are many different groups who are each a small fraction of the population. This is measured using a Herfindahl index of racial concentration – the sum of the squares of the fraction of the population made up by each race in that area and year. For racial isolation, I focus on the concept of being a small fraction of the population. I measure this using the race share variable, which represents the proportion of

[2] In 2021, the most common race labels chosen by Hispanic/Latino respondents are "other" or "two major races", due to U.S. classifications lacking a racial category corresponding to Amerindians from Central and South America. Census race definitions in other countries include categories of "Indigenous" in Brazil, Colombia, Costa Rica, Ecuador, El Salvador and Uruguay, and mixed-race versions like "Mestizo" in Ecuador, El Salvador, and Uruguay.

the local population with the same race as the woman in question. This is mechanically related to race Herfindahl, as shares are always zero or positive, and so race share squared (the addition to a Herfindahl) goes up with race share. The principal difference is that a race Herfindahl applies to everyone in an area, and so does not have any variation within an area and year. In this sense, while the Herfindahl measure maps most closely to the common usage of "diversity", it is necessarily hard to disentangle from other attributes in that area and year that greater variation in races may be associated with.

Given these considerations, I opt to use race share variable as the primary measure of racial diversity. There is a maintained assumption, which is hard to test, that race share and race Herfindahl are capturing similar underlying concepts – that is, that being a small racial group within your area draws on the same underlying mechanism as living in an area with many other different races. The consistency in the direction of the results obtained using both variables strengthens the overall findings, even if the specific underlying mechanisms captured by each measure may differ. In practice, the results of the paper work similarly under either measure. The main difference is that race share allows for the addition of an area by time fixed effect. That is, I can control for all possible drivers of the number of children in a given area and year, and focus only on the difference between being part of a large racial group versus a smaller one.

Finally, in the base specifications, I measure race share and Herfindahl for the population aged eighteen and over, so that the number of children is not mechanically linked to attributes of those children. Results are similar if all ages are used to construct the diversity measures.

3. Results

3.1 Base Effects of Diversity on Birth Rates

I begin by relating diversity and racial isolation to birth rates. The main specification is:

$$\textit{Number of Children}_{i,j,t} = \mathrm{a} + \mathrm{b}_1 * \textit{RaceShare}_{i,j,t} + \mathrm{b}_2 * \textit{Controls}_{i,j,t} + e_{i,j,t}$$

Observations are taken for a woman i, living in area j, in year t. The list of controls varies according to specification, and I introduce them as they are added. Table 2 Panel A presents the baseline results of race share on number of children. Column 1 is a univariate regression with no controls. In this specification, *RaceShare* is positively associated with the number of children, with a coefficient of 0.159, significant at the 1% level with a *t*-statistic of 2.84 (with standard errors clustered by state and year). This regression includes years dating back to 1850. In terms of the economic magnitude, a one standard deviation increase in *RaceShare* is associated with the woman having 0.052 more children, on average.

Column 2 adds controls for *Race* (that is, the nine census racial groups plus a tenth for Hispanic/Latino). *RaceShare* is necessarily correlated with race itself, as being from a more numerous racial group (e.g., whites) makes it more likely that you will live nearby more people of the same race. When this is controlled for, the effect becomes larger and much more significant – the coefficient is now 0.708, with a *t*-statistic of 6.89. Intuitively, once I control for the fact that whites have a high race share in general, and low birth rates in general, the effect of *RaceShare* increases greatly. That is, comparing two women of the same race shows a large effect of *RaceShare* on their number of children. I report two effect sizes. The first is the effect of one unconditional standard deviation of *RaceShare* (from column 1) multiplied by the coefficient. This is 0.230 more children, in this case. The second calculates the effect of a conditional standard deviation in *RaceShare.* That is, I regress *RaceShare* on the same fixed effects in the regression (here, just *Race*), and compute the standard deviation of the residuals. One standard deviation of this is associated with 0.123 more children. The difference between these two measures is approximately whether one uses all the variation in *RaceShare* (and assume it has the same effect as the aspects already controlled for), or whether one just uses the part remaining after stripping out the controlled-for components.

Column 3 adds fixed effects for *State* and *Year*. The coefficient is reduced to 0.435, but the *t*-statistic is similar at 6.92. The unconditional and conditional effects of a one standard deviation increase in *RaceShare* are 0.141 and 0.067 more children, respectively. Column 4 adds fixed effect controls for various demographic variables, collectively referred to as *Demographics*. This includes *Race* as before, but also categories for the woman's age, marital status, nationwide deciles of income that year, employment status, education, and citizenship status. With the full set of demographic controls, the earliest observations are in 1980. As before, the coefficient is reduced, to 0.310, but the *t*-statistic is increased to 7.30. Unconditional and conditional standard deviation effects are now 0.101 and 0.049 more children, respectively.

Column 5 keeps the *Demographics* variables, but replaces the *State* and *Year* fixed effects with an interacted *State-Year* fixed effect. The coefficient is now 0.241, with a *t*-statistic of 8.58. Column 6 replaces the baseline *Demographics* variables with interactions of *Demographics**State* and *Demographics**Year* (in addition to the *State-Year* effects). The coefficient increases to 0.291 with a *t*-statistic of 7.03.

Column 7 adds two new sets of controls related to local area metrics. First, I add dummy variables for the area type (county, city or metro area). Second, I calculate other metrics averaged at the local area: the fraction employed, the fraction college educated, average age, average income decile, and a *z*-score for the fraction of people who have moved in the last one or five years, depending on data availability.[3] I collectively refer to these as *Area Traits*. While most of the underlying variables are category classifications, when taking area averages these become continuous, and so for simplicity I use linear effects. Adding these variables reduces the coefficient to 0.204, with a *t*-statistic of 6.58. The effect of an unconditional and conditional standard deviation of *RaceShare* is now 0.066 and 0.026 children, respectively.

Column 8 replaces the *Area Type* variables with dummies that split each area type into population buckets (i.e., *Area Type**Population Group*), where the grouping is either halves, quintiles or deciles, depending on the number of observations.[4] I compare populations within each type of area, as a city population may not reflect the same meaningful "size" as the surrounding metropolitan area. The coefficient is now 0.160, with a *t*-statistic of 5.00. Unconditional and conditional standard deviation changes in *RaceShare* result in 0.052 and 0.019 more children. Column 9 uses time-varying area controls, replacing *Area Traits* with *Area Traits**Year* and *Area Type**Population Group**Year*. Column 10 adds an *Area* fixed effect (e.g., for Cook County, Illinois). The coefficients and significance are very similar to before.

[3] Different census years list either whether the respondent has moved in the last year, or the last five years. I first compute the average of each of these at the local area level. To make these comparable across years, I convert each into a *z*-score across all local areas that year. If both are available, I average the two.

[4] Each category (county, city, metro area) is split into population percentiles based on the number of respondents that year from the area. If there are 20 or fewer area type observations in that year (e.g., fewer than 20 cities in the data that year), areas are split into high and low populations. If there are between 21 and 100 area type observations, they are split into quintiles. If there are more than 100, they are split into deciles. Each grouping is a separate set of dummies.

Finally, I absorb all variation at the level of the area and year in Column 11, adding in *Area*Year* fixed effects. These replace all of the other area level controls (*Area Traits*Year*, and *Area Type*Population*Year*), as well as the *State*Year* fixed effects. In this specification, the only controls are *Demographics*(State, Year)* and *Area* Year*, with everything else being absorbed. The coefficient is largely unchanged from before, being 0.197 with a *t*-statistic of 5.88. An unconditional and conditional standard deviation increase in *RaceShare* results in 0.064 and 0.020 more children, respectively.

With *Area*Year* fixed effects, I control for many general properties of an area and year that might influence birth rates. All the variation comes from differences in *RaceShare* between different groups in an area (i.e., comparing racial groups that are more numerous in that area versus less numerous). Because I also have *Race*Year* and *Race*State* (as part of the *Demographics*(State, Year)),* I am also comparing each group with the overall birthrate of that racial group in that state, and in that year. For example, if I consider Detroit, MI in 2007 (which is predominantly black), blacks in the city will have more children relative to blacks in Michigan generally, or blacks in 2007 generally, and whites in Detroit will have fewer children. Meanwhile, in Ann Arbor, which is predominantly white, the pattern will be reversed – whites will have more children than elsewhere in the state and year, and blacks will have fewer children. Other individual-level differences in the populations are controlled for in *Demographics*(State, Year*).

Because of this, the results cannot be attributed to any area-level traits that might be associated with diversity in general. That is, if more diverse areas are richer or poorer, have more jobs or fewer, are denser or sparser, or anything else – all of this is controlled for by the *Area*Year* effects. Racial isolation is now separate from the general level of diversity of an area, which is also absorbed. It is notable that the final step of *Area*Year* fixed effects changes the results very little. Controlling parametrically for the other aspects of the area produces very similar results to flexibly controlling for it with fixed effects.

Because the *RaceShare* variable includes both the "lots of different races in an area, each being small" aspect, and the "you personally are from a smaller group" aspect, I next turn to a version that captures only the first aspect. In Panel B, I replace the person's race share with a Herfindahl index of the different races in that area and year. In column 1, without controls, *RaceHerfindahl* positively predicts birth rates, with a coefficient of 0.590 and a *t*-statistic of 5.33. The unconditional effect of a one standard deviation increase in *RaceHerfindahl* (i.e., an area becoming more racially concentrated) is associated with 0.125 more children. The effect increases in magnitude and significance when adding race controls in column 2. Column 3 adds *State*Year* fixed effects, and the coefficient is 0.664, similar to the univariate specification, now with a *t*-statistic of 15.02. Adding *Demographics*(State, Year)* in column 4 reduces the effect to a coefficient of 0.342, and a *t*-statistic of 14.95. Unconditional and conditional standard deviation changes in *RaceHerfindahl* are associated with 0.073 and 0.044 more children, respectively. Adding *Area Type* and *Area Traits* reduces the effect somewhat to 0.207 in column 5. Adding *Area Traits*Year* and *Area Type*Population* Year* in column 6 gives an effect of 0.121, with a *t*-statistic of 3.91, and effect sizes of 0.026 and 0.011 for unconditional and conditional standard deviation increases in *RaceHerfindahl.*

Next, I add *Area* fixed effects. Relative to Panel A, it is less clear what the right level of controls is. In the limit, adding in *Area*Year* will absorb all the variation, so this is not possible. Nonetheless, even when absorbing the average level of *RaceHerfindahl* via an *Area* fixed effect*,* I still find a positive (albeit smaller) and significant effect of 0.037, with a *t*-statistic of 4.16. The unconditional and conditional effects of a one standard deviation change in *RaceHerfindahl* are now 0.008 and 0.001 more children, respectively.

In Panel C, I include both *RaceShare* and *RaceHerfindahl* in the same regression. The specifications are the same as those in Panel B. In general, both variables show positive effects that are not subsumed by the other. The only exceptions are in column 1 (with no controls), where *RaceShare* loads negatively when race controls are absent, and in columns 6 and 7, where the addition of *Area* fixed effects and the *RaceShare* variable means that *RaceHerfindahl* is either zero or negative. In general, coefficients are somewhat reduced relative to the specifications with only one or the other variable, which makes intuitive sense given that the two variables have decent overlap, both conceptually and empirically.

One potential concern with Panel C is that *RaceHerfindahl* could be picking up non-linear effects of *RaceShare,* rather than a separate effect of concentration. Because *RaceHerfindahl* is made up of the sum of squared race shares, if *RaceShare* has additional effects beyond the linear specification I use, this may lead to *RaceHerfindahl* having measured effects even if racial concentrations do not matter directly. To rule out this possibility, in Panel D I replace the *RaceHerfindahl* with a different version, *OtherRaceHerfindahl*, which is the Herfindahl index just computed across all *other* races than the respondent's. This alternative version is orthogonal to the respondent's own *RaceShare*, and represents the concentration of the remaining races in the population. The results are similar to Panel C, but somewhat stronger – the negative effect of *RaceShare* in column 1 disappears (and it is now positive and significant), while the zero and negative coefficients in columns 6 and 7 become positive and zero, respectively.

While *RaceHerfindahl* appears to be an important separate driver of birth rates, it is harder to distinguish from other area-level effects. This is seen in column 7 in Panels C and D, where including an area fixed effect causes both *RaceHerfindahl* and *OtherRaceHerfindahl* to lose their significance. In other words, when both the area average level of *RaceHerfindahl (*or *OtherRaceHerfindahl)* is controlled for by an area fixed effect, *and* the level of *RaceShare* is controlled for, the remaining variation in racial concentration does not drive fertility. Recall that *both* sets of controls are necessary, however – in Panel B, *RaceHerfindahl* on its own still has significant effects with an area fixed effect included (but with *RaceShare* absent). For this reason, I argue that the bulk of the evidence supports the conclusion that racial concentration matters, over and above the level of the respondent's own racial share in the population. Nonetheless, a reader who is skeptical of what racial concentration is measuring absent the inclusion of an area fixed effect may not be convinced of a separate role for racial concentration. For this reason, in the remainder of the paper, I mostly focus on the *RaceShare* variable, due to the ability to add *Area*Year* fixed effects and get a tighter interpretation of what the variable measures. The results of the paper are generally similar if *RaceHerfindahl* is used instead, absent the inclusion of area fixed effects.

In the appendix, I show the robustness of this result to various methodological variations. Table A1 Panel A uses alternative definitions of race for the *RaceShare* variable. Panel B constructs *RaceShare* at different geographical levels. Panel C examines different weighting approaches. Panel D investigates the effect of women's ages, addressing concerns about children leaving home and potential conflation of delayed childbearing with lower fertility. The results are robust under these variations. Later in the paper (in Table 12), I show that the results hold if the data at the time of the survey are turned into a panel of implied birth decisions each year, as part of calculating effects on total fertility rate and time series effects. Finally, in the appendix I use simulations to show that the results are not due to any mechanical effects of the correlation between the race dummies and *RaceShare*, such as under multicollinearity.

3.2 Mobility and Selection

I now turn to tests designed to shed light on what the baseline result of the paper is measuring. One class of explanation is selection effects based on mobility. When people have children, or are thinking about having children, they may desire to be in areas with more people of their own race, even if that does not directly affect how many children they have. This could come from a direct preference for being around people of the same race (a social form of homophily), or being drawn to particular amenities in an area that are more favored by one race over another. If these are complements to having children, then people might relocate because of the child choice, rather than the child choice being affected by the diversity.

To test this, in Table 3 I re-run the tests using various metrics of women who are less likely to have moved. If a woman has not moved at all, then it is not a concern than she moved based on diversity and fertility decisions. Information on mobility-related questions is collected unevenly over years, so I measure mobility in different ways, limiting the sample to women who are less likely to be mobile. All regressions include controls for *Demographics*(State, Year),* and *Area*Year*.

When limiting the sample to various proxies for women who are less mobile, I still find positive and statistically significant effects at the 1% level in all specifications. Column 1 limits the sample to women living in their state of birth. Recall that the coefficient on *RaceShare* in the analogous specification (Table 2 Panel A Column 11) is 0.197. For women living in their state of birth, the coefficient is slightly lower, at 0.161. Column 2 limits the sample to women who haven't moved in the past year. The effect is similar to Table 2, at 0.200. Column 3 limits the sample to women who haven't moved in the past five years, and finds a somewhat lower coefficient of 0.124. Column 4 takes women who either haven't moved in the past year, or haven't moved in the past five years (with surveys generally asking either one question or the other, but not both). The effect is 0.191. Finally, if any of the three measures of being less mobile is grounds for inclusion, the effect is 0.190, with a t-statistic of 5.42. The robustness of the effects across all subsamples suggests that while mobility and selection may contribute to the effect (as seen in the slightly lower coefficients in some specifications) are unlikely to be the primary drivers of the main result.

3.3 Time Periods

Next, I consider the effect across different time periods. While this is not a direct test of a specific mechanism, the very long time period of the data allows us to implicitly test the importance of different theories. For instance, one might imagine that the effect is concentrated in the Obama presidency, or the Civil Rights Act, or during Reconstruction. In Table 4, I evaluate the baseline effect in different periods of the U.S. census dating back to 1850. The set of controls is limited by the available in the early periods – to ensure comparability, in all years I use the controls available in 1850. In Panel A, this is *State*Year,* and *(Age, Race)*(State, Year)*. In Panel B, I also include an *Area*Year* fixed effect.

The periods studied are 1850-1860 (column 1), 1870-1890 (column 2), 1900-1940 (column 3), 1950-1970 (column 4), 1980-1990 (column 5), and 2000-2021 (column 6). In Panel A, I find large and significant results in all specifications. The coefficients in Panel A are generally decreasing across over time, from 1.889 in 1850-1860, to 0.515 in 2000-2021. However, due to the smaller number of observations in the early period, the significance is lower, with 1850-1860 having a t-statistic of 2.01, significant at the 10% level (with all other periods significant at the 1% level). If magnitudes are measured in terms of marginal effects of a one standard deviation change in race share, the largest effect is in 1850/60 with 0.33 children, decreasing to a marginal effect of 0.158 in 2000-2021. The higher variation in *RaceShare* in later years offsets some of the decrease in coefficients, so the difference in marginal effects is not as large.

Panel B includes *Area*Year* fixed effects. Now the first several columns are no longer statistically significant, with significance being stronger starting in 1950. Interestingly, the coefficients now somewhat increase over time, although they are stable from 1950 onwards. As a result, it is not clear what to infer about the magnitude of the effect over time, as the answer depends on what level of controls is applied.

Despite being not directly tied to a particular theory, Table 4 greatly constrains the possible explanations. If the relation is presumed to be driven by the same cause across time, then that cause must be operating before the Civil War, during Reconstruction, during the Gilded Age, during both World Wars, during the Civil Rights Era, at the end of the Cold War, and throughout the 21st century (when between-area variation is included). Even if only within-area variation is used, the cause must be present since 1950. Theories that emphasize contemporary aspects of race relations, regardless of the specific aspect they focus on, will generally face challenges in explaining the pervasive presence of this effect throughout U.S. history. The parsimonious explanation is that the driver of the effect must be ubiquitous across historical periods.

3.4 Effects Across Races

Next, I examine how the baseline effect varies across different racial groups. While the main results control for race (and its interactions with state and year) as a determinant of birth rates, here I focus on the interaction effects. Many theories about the impact of diversity primarily emphasize black/white race relations, and concepts like the historical legacy of slavery. An important test for such theories, if they are the primary drivers of the observed effect, is what prediction they make for other racial groups. By examining the interaction effects across a wide range of racial groups, I can better assess the applicability and explanatory power of theories that predominantly focus on specific racial dynamics.

I consider these possibilities in Table 5. I examine similar specifications to Table 2 Panel B (though, as I include race interactions, all specifications require *Race* fixed effects). When thinking about the effect across different races, there are two ways to consider the effect:

-Does it hold in the most stringent specification (i.e., with *Area*Year* fixed effects?)

-Does it hold across a wide range of different specifications?

To begin with the most stringent specification, Table 5 Column 7 shows the results for interactions of *RaceShare* with all ten racial groups, after adding controls for *Demographics*(State, Year)* and *Area*Year*. As these ten racial groups encompass all the possible categories, the ten interactions subsume the base effect, so each coefficient indicates whether *RaceShare* has a significant effect on birth rates within that racial group. The results show that the effect is positive and significant at the 10% level for 7 out of 10 groups (with only Hispanic, other Asian / Pacific Islander, and three or more races not being significant). In terms of magnitude, whites show the fourth smallest effect, and two races and other have the largest.

When considering the consistency of the effect across different specifications, the picture is somewhat different. For white respondents, the effects are positive and significant in every specification, which is unsurprising, given the baseline result for all respondents is very strong, and whites constitute the largest racial group in the sample. By contrast, the consistency of the effect varies more across specifications for other racial groups, suggesting potential differences in the robustness of the relationship between racial diversity and birth rates depending on the level of controls and racial group being examined. Results are generally positive and significant for Native Americans/Indians, blacks, and two races. Effects are generally positive but not always significant for Hispanic, other and three or more races, with the only significant values being positive. Other Asian/Pacific islander is insignificant in all specifications. Japanese and Chinese switch from positive and significant to negative and significant across specifications.

The interpretation of the most stringent specification is the clearest, namely that the effect is present in some degree for a large majority racial groups once the largest number of other alternative drivers of birth rates are accounted for. However, the interpretation of the other specifications is less clear, as it requires either taking a definitive stance on the precise (and smaller) number of controls that should be included, or simply considering the generality of the results. Even in this case, it is not clear what explanation would show the most reliable results for whites, blacks, native Americans, and two races.

3.5 International Results

Another class of explanation is attributes that are unique to U.S. history or U.S. race relations. An important test of such theories is whether the results are present in other countries. To test this, I use IPUMS international data, for nearly all countries that collect race information, excluding only Saint Lucia and Suriname where the small sample sizes make geographic measurements challenging. Panel A examines African countries (Mozambique, South Africa, Zimbabwe) plus the UK (the only European country I observe). Panel B examines Central American countries (Costa Rica, Cuba, El Salvador, Jamaica). Panel C examines South American countries (Brazil, Colombia, Ecuador, Uruguay). IPUMS codes geography at coarse and fine levels, which roughly corresponds to states and sub-state units (cities, counties, etc.). I measure *RaceShare* at the finest level available, usually fine geography, but sometimes coarse geography.

I measure race using whatever definitions the country collects, as described in the appendix. When multiple samples are available for a given country, I add *Year* interactions where applicable. For each country, where possible I use three specifications:

i) *Coarse Geography* Year* and *Demographics.*

Coarse geography approximately corresponds to state. *Demographics* includes: an urban dummy, race, marital status, age, educational attainment, and employment status (depending on availability)

ii) *Coarse Geography*Year, Demographics*(Coarse Geography, Year), Ln Population Density*Year*

I also include the natural log of population density, interacted with year.

iii *Fine Geography *Year, Demographics*(Coarse Geography, Year), Ln Population Density*Year*

Panel A examines the effects for the UK and African countries, and finds a generally positive relationship, except when using *Fine Geography*Year* effects. The UK is unusual, having only a single sample and only coarse geography measures (so *RaceShare* is at the coarse geography level). Standard error clustering is also at the coarse geography level, or at the fine geography level where available (the small number of time periods makes clustering by time either inadvisable or outright impossible). Column 1 includes *Demographics* and *Ln Population,* and finds a positive and significant effect for the UK. This disappears in column 2, when *Coarse Geography* controls are added (which, recall, are at the same level as the *RaceShare* variable, so are more equivalent to the fine geography controls in other countries). In columns 3, 6 and 9 I find positive and significant effects for *RaceShare* in Mozambique, South Africa, and Zimbabwe, when controlling for *Coarse Geography*Year* and *Demographics.* In columns 4, 7 and 10, these remain positive and significant, and increase in magnitude, when controls are added for *Demographics*(Coarse Geography, Year)* and *Ln Population Density*Year.* When controls for *Fine*

*Geography*Year* are added (looking only at variation between races in the same area, like Table 2 Panel A Column 11), the effect is insignificant in Mozambique and Zimbabwe, but still significant in South Africa.

Panel B examines Central America. Costa Rica and Cuba show mixed results: zero in the first specification, negative and marginally significant when adding *Demographics*(Coarse Geography, Year)* and *Ln Population Density*Year*, but positive and significant when adding *Fine Geography*Year*. El Salvador shows a similar pattern to Panel A – positive and significant in the first and second specifications, insignificant with *Fine Geography*Year* effects. Jamaica shows insignificant effects in all specifications.

Panel C shows the effects for South America. Brazil shows the only reliably negative effects of *RaceShare* across all specifications. These are small in magnitude compared with other countries, but the large number of observations (over 16 million) makes them significant. Colombia shows insignificant results in all specifications. Ecuador shows the Panel A pattern, of being positive and significant in the first two specifications, but insignificant when *Fine Geography *Year* controls are added. Uruguay is negative and significant in the first two specifications, but positive and significant in the third.

Overall, these results show that the effect in international data is not as ubiquitous as in the U.S., but neither is it limited only to U.S. data. The number of countries where the results are "some positive and significant, none negative and significant" is six, whereas "some negative and significant, none positive and significant" is only one. The patterns across countries do not tell an obvious story. While U.S. race relations often focus on interactions between black and white populations, the results are present in the UK (which has far fewer blacks), and African countries (which are overwhelmingly black), but not in Jamaica (also overwhelmingly black) nor Brazil (which has a large black population by U.S. definitions, but also a rather different conception of racial categories). I consider the question of understanding all these sources of variation to be interesting, but beyond the scope of the paper. Instead, these results serve as an indication that the results likely arise from forces that occur in other counties as well, but not universally so.

3.6 Potential Explanations – Marriage and Divorce

Next, I explore the implications of racial diversity on marriage outcomes. One possibility is that the effects may be narrowly related to the costs of raising children. If instead the effect comes from broader relationship effects, then I might expect to see impacts on marriage and divorce rates. These additional relationship outcomes are consistent with both trust and homophily explanations. At a minimum, if the effects extend beyond childbearing decisions to marriage formation and dissolution, it suggests that racial diversity affects family structures and dynamics more than just through direct costs of child-raising.

I analyze this question in Table 7. In Panel A, I consider whether a woman is currently married at the time of survey. Using the same specifications as in Table 2 Panel B, a higher *RaceShare* is associated with a greater likelihood of the woman being currently married, significant at the 1% level in all specifications. The effect of an unconditional standard deviation change in *RaceShare* on the probability of being married ranges from 1.2 to 6.2 percentage points (columns 7 and 1 respectively). In Panel B, I consider whether a woman was ever married (the variable equals one if the woman is married, divorced or widowed, and zero if she is single). The effects here are generally similar in magnitude and significance to Panel A. A one unconditional standard deviation increase in *RaceShare* is associated with higher chances of ever being married by 1.2 to 6.2 percentage points. Panel C examines age at first marriage. This is for a subset of the data, as I limit the sample to women who are currently married, and who have only been married once (as others lack data on the age of first marriage). The effects here have the largest statistical significance of the four panels. An unconditional one standard deviation increase in *RaceShare* is associated with getting married between 6.6 months earlier (column 1) and 2.3 months earlier (column 7).

Panel D examines the probability of being divorced, conditional on getting married. The dummy variable now equals one if the woman is divorced, and zero if she is married or widowed (with single now being omitted). While there are some effects of *RaceShare* on lower divorce rates in the early specifications, the effects are smaller and less consistent, and specifications with tighter controls show no effect. A one standard deviation change in *RaceShare* results in divorce probability ranging from 1.9 percentage points lower (column 1) to 0.4 percentage points higher (column 2). Overall, these results reinforce that diversity is negatively associated with marriage rates, particularly whether and when you get married, more so than whether you get divorced. This reinforces the conclusion that diversity is associated with broader relationship effects, rather than just child-rearing costs, narrowly defined.

3.7 Sex and Race Differences in Interracial Marriage

Next, I consider one of the channels that might contribute to a causal interpretation of the main result. In particular, I consider the role of homophily in relationship preferences. There is considerable evidence that people generally prefer to marry someone similar to them. Similarity in race is one of the strongest of these (e.g., Hwang 2012). The evidence from sperm donation suggests a preference for same-race traits in donors (Daniels and Heidt-Forsythe 2012), even when the man will not be present in the woman's life (and thus correlated aspects like partner income cannot be driving the choice). If people on average have a preference for marrying someone of the same race, it is plausible that the fraction of the population around them of the same race is an important determinant of the chances of finding a suitable marriage partner. The results linking diversity to marriage outcomes, not just child-rearing, are consistent with broader effects of diversity on finding a suitable partner, consistent with homophily.

It is tempting to attempt to address this problem by controlling for whether the woman married someone of a different race, but this is unlikely to be sufficient. If the pool of same-race partners shrinks, one may end up marrying someone of the same race, but of a worse quality match than might have been obtained in a larger pool. The challenge is that proxies for the quality of a match are difficult to observe and quantify, and so controlling for interracial marriage may not fully capture the impact of a reduced pool of same-race partners. I instead turn to two additional tests. In particular, the homophily preference for marrying within one's own race is unlikely to be uniform across all racial groups and historical periods. To take a stark example – the census category of "three or more races" is unlikely to be a source of strong homophily, whereby people of three or more races only want to marry someone else of three or more races (regardless of what those three races actually are), rather than someone of two races (classified as a different racial group), or someone of one of those three racial groups, or anyone else. More broadly, different racial groups likely have different norms about the importance of marrying someone of the same race. If one is from a group where interracial marriage is strongly discouraged, then it should be of larger importance to live around more people of the same race, so as to have a larger dating and marriage pool.

One minor complication is that the level of interracial marriage is somewhat mechanically related to *RaceShare* itself. For instance, if whites are 90% of the married population in a state, it is not possible for them to have a large interracial marriage rate (while the 10% remaining population could, in principle, all marry someone of a different race). Instead, I compute the abnormal intermarriage rate, by comparing the intermarriage rate nationally for that race and year, with the simulated distribution if all married people that year paired up randomly. I compute 1000 simulations of random pairings, and compute a *z*-score of the actual intermarriage rate, minus the simulation average, divided by the simulation standard deviation. I interact this variable with *RaceShare*. I add the univariate version of the abnormal interracial marriage variable in specifications that lack *Race*Year* fixed effects, which subsume the abnormal intermarriage rate.

Intermarriage rates may also reflect differences across races that matter for other reasons. For this reason, I turn to a second, sharper prediction, namely sex differences in intermarriage rates. In particular, men and women of the same race may "marry out" of their race at different rates. Each man that marries a woman of a different race reduces the pool of marriageable men for women of his own race. If the population sizes of the sexes are roughly equal, then neither sex will have a surplus of potential partners as a baseline. This highlights the essential aspect that intermarriage rates for men increase the pressure on women of the same race, and vice versa. Unlike the previous tests, any overall traits that are common to both men and women of that race, however arising, should not affect this rate.

I measure this by calculating the interracial marriage gender ratio for each race and year. I divide the fraction of married women of that race who have a husband of a different race by the corresponding fraction of married men of that race who have a wife of a different race. For these tests in Table 8 on intermarriage rate, and sex differences in intermarriage rate, I take both men and women ages 18-40 (as opposed to the other tables, which only include women). I replace the controls for *State*Year, Demographics*(State, Year)* and *Area*Year* with interactions with *Sex,* so that these effects can vary between men and women (i.e., I have *Sex*State*Year, Sex*Demographics*(State, Year),* and

*Sex*Area*Year* respectively). The only exception is that I cannot include *Race*Sex*Year* because this would absorb the variation I am using, so instead I use *Race*Year* and *Race*State* as before.

These results are presented in Table 8. The first four columns show the effect of the abnormal intermarriage rate. I find that higher levels of abnormal intermarriage are associated with a lower effect of *RaceShare.* That is, *RaceShare*AbnormalIntermarriageRate* is negative and significant in three of the four specifications. The interpretation is that when both men and women of that race are more likely to marry people of different races, it makes less difference to their average number of children whether they are living near people of the same race or not. The only exception is when I add controls for *Sex*Area*Year*, when the effect becomes smaller and insignificant.

Next, I consider sex differences. The variables of interest are *RaceShare*IntermarriageSexRatio* and *RaceShare*IntermarriageSexRatio*Male*. The former estimates the effect of greater female out-marriage on the *Race Share* effect for women of that race. The latter is the additional effect of female out-marriage on *RaceShare* for men of that race (relative to women). This triple interaction term is the key prediction, and in columns 5-8, *RaceShare*IntermarriageSexRatio*Male* is positive and significant in all specifications. When women of a race marry out at higher rates than men, the number of children that men of that race have is more affected by whether they live in a high *RaceShare* area, relative to how much *Race Share* affects the number of children for women of that race. The *RaceShare*IntermarriageSexRatio* coefficient is negative but not generally significant.

3.8 Trust

Next, I turn to the second major theory that could explain the results, namely social trust. Higher levels of racial diversity are associated with lower direct levels of trust (i.e., survey answers as to whether you can generally trust people), and also with various other aspects of social capital.[5] It is plausible that

[5] As Putnam (2007) describes: *"In areas of greater diversity, our respondents demonstrate:*

- *Lower confidence in local government, local leaders and the local news media.*
- *Lower political efficacy – that is, confidence in their own influence.*
- *Lower frequency of registering to vote, but more interest and knowledge about politics and more participation in protest marches and social reform groups*

some or all of these social capital factors are related both to the likelihood of people finding a suitable marriage partner, and their choice of how many children they would like to bring into the world. The predictions for this hypothesis are not as sharp as those for homophily. However, the two most straightforward aspects are that i) trust levels should be positively associated with birth rates, and ii) controlling for trust levels should reduce the effect of *RaceShare*.

I consider two ways of measuring trust. The first is the measure in the 2006 Social Capital Benchmark Survey of Putnam (2007), where respondents are asked "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?". I take the state level number responding "people can be trusted" as a fraction of those giving this answer or "you can't be too careful" (omitting responses of "it depends", "don't know" or refusal to answer). As I have this information only for 2006, I apply it to all years for that state. As a result, in these regressions, I cannot include state fixed effects or state interactions, and the controls are *Demographics*Year*. In this respect, the coefficients have fewer controls than prior specifications. Nonetheless, the interest is how much these state metrics explain fertility, and how much they reduce the effect of *RaceShare* under this level of controls.

I present these results in Panel A. Because the trust metrics are state level, I compare them to both state level versions of *RaceShare* (in columns 1-6), and with the baseline local version (in columns 7-12). Finally, because the trust measure is only from a single year, I also limit the sample to years increasingly matched to the timing of the trust measure – all years (columns 1 and 2), 2001-2010 (columns 3 and 4), and 2006 only (columns 5 and 6). I find the first prediction confirmed – in all specifications, states with higher levels of general trust (*StateLevelTrust)* in 2006 have higher birth rates, when controlling for

---

• Less expectation that others will cooperate to solve dilemmas of collective action (e.g., voluntary conservation to ease a water or energy shortage).
• Less likelihood of working on a community project.
• Lower likelihood of giving to charity or volunteering.
• Fewer close friends and confidants.
• Less happiness and lower perceived quality of life.
• More time spent watching television and more agreement that 'television is my most important form of entertainment'."

*Demographics*Year*. Second, I find that controlling for *StateLevelTrust* also reduces the effect of *RaceShare(State)* and *RaceShare(Base).* Columns 1, 3 and 5 compute the coefficient on *RaceShare(State)* for the years in question and observations where I can match *StateLevelTrust* data. Columns 2, 4 and 6 show the same coefficient once *StateLevelTrust* is controlled for. In column 2, the effect of *RaceShare(State)* is reduced by 21% once I add trust controls. In the 2001-2010 sample, the reduction is slightly larger, at 24%, in column 4. In 2006 alone, the reduction is slightly larger still, at 26%. Columns 7 to 12 show similar effects, albeit smaller reductions, when the baseline geography definitions are used. This is consistent with broad state-level trust measures having less ability to drive out geographically tighter measures of diversity.

In Panel B, I examine a different set of county-level social capital metrics using Facebook data, from Chetty et al. (2022). I focus on the three major measures of that paper – the volunteering rate (the fraction of people who participate in a volunteer organization), friendship clustering (the chances that, if A and B are friends, and C is friends with B, that A is also friends with C), and economic connectedness (the share of above median income friends by people with below median income). These measures are at the county level, so I compare them with county-level versions of *RaceShare*. I compare the baseline *RaceShare* variable in the same periods and counties with social capital measures, then add the three social capital measures. I do this for all years (columns 1-2), 2011-2021 (columns 3-4) and 2021 only (columns 5-6).

Comparing the univariate *RaceShare* effect with the version with all three social capital measures included, the coefficient is reduced by 39%, from 0.318 to 0.200 in the full sample (with similar effects in other year ranges). Volunteering rates have directionally positive effects in all specifications, but are only significant when only using 2021 data. Friendship clustering and economic connectedness are negative but insignificant. These results are consistent with social trust being a contributing explanation for the main result, explaining between 20-37% of the *RaceShare* effect on birth rates, depending on the trust measure.

3.9 Diversity Along Other Dimensions

Next, I examine the effect of population shares for other demographic variables. This is a robustness test for whether *RaceShare* proxies for a person's similarity to others in their area along other, correlated dimensions. It also examines a related prediction of homophily, whether people have higher birth rates if they are surrounded by similar people along other dimensions, or only race. Conceptually, homophily preferences can differ by trait. Examining more variables helps distinguish whether homophily over race is just one example among many traits, versus being the primary aspects of marital homophily preferences.

I construct analogous *[Variable]Share* variables for other measures of demographic similarity. In Table 10 Panel A, I consider education, income decile, and age. Because shares depend on how coarsely the groups are defined, coefficients are not directly comparable. For age, I calculate the share as the fraction of the population that is between two years younger and ten years older than the woman. For education, I use census education categories. In Panel A, *EducationShare* has a positive sign, but loses significance with additional controls. Income is most similar to race in its effects. *IncomeDecileShare,* in columns 5-8, shows a positive and statistically significant effect in all specifications, including with *Area*Year* fixed effects.. The unconditional effect of a one standard deviation increase in *IncomeDecileShare* ranges from 0.024 more children in column 5 to 0.028 more children in column 6. In columns 9-12, *AgeShare* shows inconsistent effects across specifications. In Panel B, I consider the fraction of people overlapping in foreign versus native birth, and citizenship status. Both *USBornShare* and *CitizenShare* shows positive and significant effects in the first two columns, but insignificant negative effects once more controls are added. The fact that income works similarly to race suggests that race is not unique as a variable where homogeneity is associated with higher fertility, but not all demographic variables have the same effect.

In Panel C, I consider these variables together. Importantly, *RaceShare* is positive, significant, and of a similar magnitude in all specifications. While the effect of these other variables alters the *RaceShare* coefficients in early specifications where few other variables are controlled for, with the *Area*Year* fixed effects in column 4, the *RaceShare* coefficient is 0.191, very close to the 0.197 in the equivalent specification in Table 2 Panel A column 11. The effect of *RaceShare* is about two and a half times as large as *IncomeDecileShare*, with effect sizes between 0.061 and 0.073 additional children, compared with 0.022 to 0.023 additional children for *IncomeDecileShare*. A number of the variables that show weak or inconsistent "univariate" effects (i.e., as the only *[Variable]Share* variable) show different patterns after controlling for other aspects of similarity. *CitizenShare* is now large and economically significant, but *USBornShare* is negative and significant, as is *EducationShare.* If the main result is driven by homophily, then a number of variables have nuanced effects, whereby a trait that is desirable at a univariate level may be undesirable once other correlated aspects of matches are controlled for (or vice versa).

The fact that income share is the next most reliable measure is consistent with the considerable evidence for assortative matching based on income (Chiappori, Salanie and Weiss 2017, Greenwood et al. 2014, Fernandez et al. 2005, Schwartz and Mare 2005, and Chiappori et al. 2022). It is less obvious that this represents an explicit preference for homophily (i.e., it is not clear that lower income people explicitly prefer their partners to also have low income). The result can also arise in matching models like Becker (1971), where everyone wants a richer partner, but has to be richer themselves to attract them. Even in this model (which lacks homophily), higher diversity may still lower marriage and birth rates, if the rich view their local low-income partner possibilities as being worse than the outside option of remaining single.

3.10 Immigration Shocks

The paper's primary focus extends beyond just identification to understanding the underlying drivers of the correlation between racial diversity and birthrates. I take this focus because diversity itself is unlikely to be a fundamental driver of birthrates; rather, it appears to be a factor that influences more basic mechanisms, such as trust and ability to find marital partners.

A key challenge in the analysis is ruling out the possibility that this correlation stems from omitted variables, where diversity emerges as a byproduct of more fundamental economic factors affecting birthrates. For example, an economic boom might simultaneously increase employment opportunities (raising the opportunity cost of childbearing) while attracting a more racially diverse population. While the previous tests – including area-by-year fixed effects and analyses across periods, races, and marital outcomes ‑ have largely addressed such simple explanations, I conduct an additional test examining whether plausibly exogenous changes in diversity levels produce effects consistent with the base results.

For this analysis, I rely on the county-level immigration shocks from Burchardi et al. (2024). Their methodology employs a multi-step approach: first estimating predicted 1975 ancestry shares for each county and national origin using national migration patterns and historical settlement patterns, then combining these predicted shares with subsequent national inflows to estimate county-specific migration shocks in five-year intervals. I convert these migrant numbers to population percentages by dividing by county census/ACS survey respondents. I then use these immigration shocks over the previous five years to instrument for the race share in the county in a given survey year, and use these in an instrumental variables regression to examine the effect on the number of children.

Table 11 presents these results, using the control structure from Table 2 Panel B. I run an instrumental variables regression, where I instrument for race share in that county and year by immigration shocks over the previous five years (in tens of thousands of new residents), divided by the number of respondents. Due to county-matching requirements, these regressions use county-level *RaceShare* observations. *RaceShare* has a positive and significant effect on birth rates when instrumented by immigration shocks. The significance mirrors Table 2 Panel B: without controls, the *t*-statistic is 2.12 (clustering standard errors by year and state). Adding race controls and race- and state-by-year controls increases both magnitude and significance (*t*-statistics of 4.42 and 6.74 respectively). Results are positive and significant until including area fixed effects in column 7, where significance dissipates. Notably, the magnitudes exceed those in the non-instrumented versions from Table 2 Panel A.[6] These findings support racial diversity as a driver of birthrates rather than merely a correlation arising from other factors.

In the Appendix, I report the results of the first stage regressions for the IV regression, along with various diagnostic tests. I find strong negative effects of immigration shocks on *RaceShare* in all

[6] The larger magnitude of IV coefficients compared to OLS estimates is consistent with a few potential mechanisms. First, the IV strategy identifies a Local Average Treatment Effect (LATE) from complier populations – those whose diversity levels are actually affected by immigration shocks – who may be more responsive to changes in racial composition. Second, the larger IV coefficients could indicate that measurement error in racial diversity measures biased the OLS estimates toward zero. Finally, the pattern may suggest that omitted variables in the OLS specifications were masking some of the true relationship between diversity and birthrates. While the magnitude difference warrants careful consideration of instrument strength and validity, the first-stage F-statistics and extensive robustness checks support the reliability of these IV estimates.

specifications, with highly significant *t*-statistics (ranging from -16.38 to -4.05) suggesting that the instruments are capturing meaningful variation in the endogenous variable. Moreover, the diagnostic tests strongly indicate that the instruments are not weak. Both Cragg-Donald F-statistic and Kleibergen-Paap F-statistic are above the Stock-Yogo critical value of 16.38, generally well above, suggesting that weak instrument bias does not threaten the IV estimates. These results provide confidence in the instruments' ability to meaningfully explain variation in the endogenous variables.

As noted previously, thinking about the exclusion restriction here is difficult, because diversity is not itself a direct mechanism that affects birthrates, but rather something that affects more fundamental mechanisms. Indeed, the idea that immigration only affects birth rates through the diversity channel is not especially well defined, because in some sense the increase in immigration *is* the increase in diversity in a quite mechanical sense, rather than being the cause of it. Instead, the instrumental variables help rule out the alternative that diversity is only operating as the *result* of some underlying other trend that is having the direct effect on birth rates – when the increase in diversity happens for mostly exogenous reasons, the result is the same. Meanwhile, the other subtests help to shed more substantive light on what this correlation actually represents in terms of underlying mechanisms.

3.11 Time Series Evidence

In this section, I consider directly the motivating question I began with – the time series changes in overall fertility and diversity. While the Table 2 specification without controls implicitly touches on this, it is informative to examine the broader question using simpler methods. Specifically, I seek to quantify how much of the overall change in fertility could plausibly be attributed to increased diversity and decreased racial isolation.[7] In Table 12 Panel A, the dependent variable is the total fertility rate for the US since 1971, from the St Louis Fed FRED database. I relate this to the average of the *RaceShare* variable in the year before (when most conception decisions would have been made). Because I am only able to test the

[7] In the time series tests, it is not possible to distinguish between the effects of national averages of *RaceShare* and *RaceHerfindahl*, as the time series correlation is 0.99.

relationship for years when I have census diversity information in the year prior (i.e., when conception decisions were made), this gives us TFR observations every ten years from 1971 to 2001, and annual observations in 2004 and from 2006 onwards. I include as controls various economic variables lagged by a year: inflation, GDP growth, and unemployment. In column 1, the univariate effect of average *RaceShare* is 1.440, with a *t*-statistic of 3.86. There are two ways to think about economic magnitude here. First, the R-squared of the regression is 0.439, indicating that a substantial amount of year-to-year variation is explained. Secondly, I consider the full time-series change over the period (a drop in TFR of 0.602), versus the predicted change in TFR based on the changes in average *RaceShare* and the regression coefficient, and get a predicted change of 0.393. That is, the variable explains 65.3% of the overall drop in fertility.[8]

Column 2 adds economic controls. The effect increases in both magnitude and significance, and now explains 117% of the overall decline. Because the level of geographic measurement varies over the sample, columns 3, 4 and 5 show the effect of average *RaceShare* measured for cities only, counties only, and states. The effect is insignificant for cities only, but significant for the remainder. Predicted changes are 245.8%, 85.2% and 117.0% of actual declines respectively. Columns 6-10 limit the sample to 2006 onwards, where consecutive annual data allows for Newey-West standard errors, here with a lag of five years. The effects are larger in coefficient and significance during this period. The univariate R-squared in column 6 is 88.6%. The predicted changes are between 94.8% and 115.3% of the actual changes over the same period.

In Figure 1, I plot visually the levels of state-level averages of *RaceShare* (which are less sensitive to different definitions of geography being collected over time) and TFR for the following year. This figure shows the drivers of the two sets of results – in long horizons, the large drop in TFR in the early 1980s without a large drop in *RaceShare* leads the R-Squared to decline, whereas in the recent years the changes in TFR and Race Share track each other very closely, hence the high R-squared. Some data points are

[8] The analysis reported in the first column uses a time series with T=21 and N=1, which is typical for many macroeconomic studies. While this sample size allows for basic time series modeling, it limits the complexity of models I can reliably fit, as the number of observations decreases or the number of covariates increases. I am mindful of the potential for overfitting, particularly as I approach a "small T, large N" scenario, which can lead to unreliable estimates and poor out-of-sample performance. I acknowledge these constraints and interpret the results conservatively, focusing on robust trends and avoiding overly complex specifications to mitigate the risk of overfitting.

puzzling, such as the low average *RaceShare* in 2000. As Table 1 shows, this year had both fewer respondents, and fewer geographic areas reported than surrounding years, and this may affect the result. Despite this potential data limitation, the overall relationship remains remarkably strong.

In Panel B, the dependent variable is the unadjusted average number of children for all respondents. This is sensitive to other factors like the age profile of the women being sampled, but it has the advantage of being easy to construct back to 1850. In columns 1-4, I find that all geographic measures work as univariate predictors since 1850 with *t*-statistics above 6. R-squared values range from 0.571 to 0.796, and predicted changes as a fraction of actual are 85.8%, 70.4%, and 83.6% for base, city and state respectively (with county being an odd outlier at explaining 3892%, partly due to the smaller number of observations). Finally, in columns 5-8, I repeat the analysis using only decennial observations to mitigate the potential influence of the numerous annual observations available since 2000. The results obtained from this restricted sample are consistent with the previous findings.

3.12 Estimating Time Series Effects from Cross Sectional Tests

The magnitude estimates from time series effects come only from the overall relationship between overall fertility and average race share. While these estimates provide a simple quantification of race share's importance for high-level changes in fertility, they have inherent limitations. Pure time series regressions, with their small number of observations and potentially large number of alternative controls, are inherently limited in their ability to control for other variables. As a result, I next examine how the magnitudes from the time series estimates relate to the magnitudes from the rest of the paper, especially as controls are added.

As a first step, I need to turn the panel regression coefficients into effects for something closer to TFR. This is measured as the sum of age-specific average birth rates across a woman's child-bearing years (usually up to 44, but sometimes up to 49). The base measure counts the number of children the woman has at survey time – that is, the total of births up to that point for that woman. To get the effects on birth rates directly, I first need to augment the dataset into a panel of observations for whether the woman gave birth that year. I use a similar procedure to Nickerson and Solomon (2024), as described in the Appendix. I assign any children in the household to relevant adult-aged woman, and then use the number and ages of those children, along with the year of survey, to construct a panel of birth outcomes each year for that woman. I then match these to area-level measures (race share, and area-level controls) from the year before the birth outcome, to approximately match the time of conception. Other demographic variables are taken only in the survey year. Because of the requirement that *RaceShare* information be available one year before the birth year, I am generally matching birth outcomes for the year after census or ACS surveys are taken. In untabulated results, I also study a version where I match birth year outcomes to the next available area-level information (even if not from the exact matching year), as well as matching diversity measures to the year of birth itself (rather than the year of conception), and find qualitatively similar results.

Table 13 presents the results that explain annual birth outcomes with *RaceShare* measures in the year of conception, a modified version of the analyses from Table 2 Panel A. The dependent variable is now a binary indicator (multiplied by 100) representing whether a woman gave birth in a given year. Both *RaceShare* measures and fixed effects are now based on the birth year, rather than the survey year. Despite these methodological changes, the statistical significance of the results remains largely consistent with Table 2 Panel A, reinforcing the robustness of the findings across different analytical approaches.

Next I compute implied approximate changes to TFR. Because the coefficient represents the effect of *RaceShare* on average birth probabilities for women of ages 18-40, I can multiply this by 23 to get the sum of the effects for those years, and then multiply this by the standard deviation of *Race Share* (unconditional, and conditional on the set of fixed effects) as in Table 2 Panel A. These numbers will differ somewhat from correct TFR numbers because i) I only count women up to age 40 due to the possibility of missing children who moved out of home, and thus I understate the effect on late age births, and ii) I use a sample that weights ages according to their empirical distribution, rather than weighting equally. Nonetheless, with these caveats, I find a sizable impact of *Race Share* on approximate TFR, with magnitudes somewhat larger than in Table 2 Panel A. Excluding column 1, which lacks race controls, the mean effect on TFR across the remaining specifications is that one standard deviation increase in *Race*

Share increases the TFR by 0.089 births per woman. The largest effect is 0.15 births per woman, and the effect from the most stringent estimation in column 11 is 0.076 births per woman.

More importantly for the immediate purpose, I can also use these coefficients to get an alternative estimate of how much the overall changes in *Race Share* can explain the historical TFR changes. That is, I can use the more tightly estimated coefficients to get an estimated magnitude per unit of *RaceShare* after controlling for many factors, and use the overall *RaceShare* changes to see what TFR is implied. To do this, I take the change in average *RaceShare* between 1971 and 2021 (0.273), and multiply this by the coefficient*23/100. Across the 10 specifications that include race controls, this implies an average change in TFR of 0.078. If I scale this by the actual change in TFR over the same period, changes in *Race Share* would explain 12.9% of the overall decline, with the largest estimate being explaining 21.8% of the decline, and the estimate from the most stringent specification being explaining 11.0% of the decline.

The higher R-squared in the time series analysis (44%) compared to the panel-based estimates is likely due to methodological differences. Time series data, dealing with aggregated measures, often yields higher R-squared values due to smoothing of individual variations, fewer degrees of freedom, and potential omitted variables. Panel data, capturing individual heterogeneity, introduces more "noise", but also the ability to include many controls. This flexibility of controls, however, leads to a greater need to take a stance on the correct level of controls for estimating magnitudes. This question of exactly which controls to apply is less pressing in the main tests, which are more concerned with the existence and robustness of the effect than the specific magnitude. As such, for those tests it is prudent to lean towards more controls, to ensure that the effect is still large and robust even when as many possible confounders are taken into account.

But some of the controls are likely to be mediators, not confounders. As a result, controlling for them will artificially reduce the estimated magnitude beyond its true value. For instance, under the homophily explanation, the drop in marriage rates is part of how *RaceShare* reduces birth rates. If so, there is a strong case to prefer specifications that do *not* control for marriage rates (columns 2 or 3, where *Race Share* explains 21.8% and 19.8% of the decline, respectively), otherwise one is controlling for some of the actual pathway by which the effect operates. For this reason, I argue that the 19.8% and 21.8% values are better estimates of the time series effect from the panel data. However, if one is more concerned that marriage rates are also dropping for other reasons, and this just happens to be correlated with rising diversity, one may prefer the other, more conservative estimates. It is notable that even these more conservative estimates show an important role for *Race Share* in explaining long term declines in fertility. Among specifications that control for race, the lowest explanatory power is still 7.7%, and the explanatory power in the most stringent specification is 11.0%.

In the most recent period, however, the differences between the methodologies become larger. Time series regressions for the 2006-onwards-only sample (chosen for its ability to generate Newey-West standard errors) produce a very high R-squared of 88%. However, the declines implied by the panel estimates are 5.2% and 4.7% of the actual in the preferred specifications that do not control for marriage, 3.1% averaged across all specifications, and 2.6% in the tightest specification. In a sense, this decline in explanatory power is mechanical – I compute a single coefficient from all time periods, and as long as the decline in *RaceShare* between 2006 and 2021 is less than the decline between 1971 and 2021 (which is indisputable), then the implied drop in TFR will be lower. By contrast, the high R-squared reflects the fact that the declines in TFR match very closely to the declines in *Race Share* over the same period. This is visually apparent in Figure 1, which plots the underlying datapoints from the time series regression. Despite the large disagreement in magnitude for this period, it is worth noting the almost complete absence of *other* variables able to quantitatively explain the decline in birth rates over this period. This is the conclusion of Kearney, Levine and Pardue (2022), who document the general difficulty of existing variables to quantitatively explain the decline in birth rates since 2007. Increases in racial diversity appear to be a potentially important contributor to this decline, although the magnitude of the effect is unfortunately more sensitive to methodological choices over the recent period than it is for the long horizon tests.

With aggregate time series changes, it is much harder to say for sure what the drivers are, and the ability to make causal statements is very limited. Nonetheless, to the extent that one believes in a potential causal channel from the tighter cross-sectional tests, these also serve to show that the implied magnitudes for the time series changes are considerable, and that changes in diversity may be important variables for helping quantitatively explain the decline in birth rates that I observe, especially longer horizon changes.

4. Conclusion

I document a new stylized fact linking the central demographic changes of our time. Women living in areas of higher racial diversity robustly have fewer children. I do not explicitly argue that this represents a causal relationship, but the obvious non-causal explanations have considerable difficulty explaining the range of facts documented. The effect is present in every period that U.S. census data is easily obtainable, and does not appear to be an artefact of modern race relations. It is present for many different races of women, so is not just related to black/white race relations. It holds (unevenly) in other countries, so while it is not an inevitable human universal, it is also not limited to the U.S. Diversity is not only associated with the direct costs of raising children, but also relationship outcomes like the likelihood a. The findings suggest that the impact of racial diversity extends beyond the narrow scope of childrearing expenses and influences multiple aspects of family formation and stability.

What alternatives are left that fit all the facts above? The strongest of these is preferences for homophily in partner choice, and I present evidence specifically consistent with this, from differences in interracial marriage across races, and between the sexes within a race. The existence of income share effects also supports the homophily explanation, given existing research on income homophily. These additional results are hard to explain under competing theories. More speculative, but potentially also important, is the role of social trust. Putnam (2007) links this to racial concentration, the more direct measure of diversity. The results also show a negative relationship between racial concentration and birth rates, and this generally holds controlling for race share. The relationship between racial isolation specifically and what these other aspects of racial concentration are capturing is an important avenue for future studies.

References

Abascal, M., and Baldassarri, D. (2015). Love thy neighbor? Ethnoracial diversity and trust reexamined. American Journal of Sociology, 121(3), 722-782.

Adsera, A., (2005). Vanishing children: From high unemployment to low fertility in developed Countries. American Economic Review (Papers and Proceedings) 95, 189-193.

Adsera, A. and Ferrer, A., (2015). Immigrants and demography: Marriage, divorce, and fertility. In Handbook of the economics of international migration (Vol. 1, pp. 315-374). North-Holland.

Alesina, A., Baqir, R., and Easterly, W. (1999). Public goods and ethnic divisions. The Quarterly Journal of Economics, 114(4), 1243-1284.

Bar, M., Hazan, M., Leukhina, O., Weiss, D., and Zoabi, H. (2018). Why did rich families increase their fertility? Inequality and marketization of child care. Journal of Economic Growth, 23(4), 427–463.

Bailey, M.J., Hershbein, B., and Miller, A.R. (2012). The opt-in revolution? Contraception and the gender gap in wages. American Economic Journal: Applied Economics, 4(3), 225-254.

Becker, G.S. (1971). A theory of marriage: part I. Journal of Political Economy 81, 813-46.

Becker, G.S., and Lewis, H.G. (1973). On the interaction between the quantity and quality of children. Journal of Political Economy, 81(2, Part 2), S279-S288.

Becker, G.S. (1960). An economic analysis of fertility. In Demographic and economic change in developed countries (pp. 209-240). Columbia University Press.

Bedi, S. (2020). Private Racism. Cambridge University Press.

Bick, A., and Fuchs-Schündeln, N. (2018). Taxation and labour supply of married couples across countries: A macroeconomic analysis. The Review of Economic Studies, 85(3), 1543-1576.

Black, S. E., Devereux, P.J. and Salvanes, K.G., (2008). "Staying in the Classroom and Out of the Maternity Ward? The Effect of Compulsory Schooling Laws on Teenage Births", Economic Journal 118, 1025-1054.

Borella, M., De Nardi, M., and Yang, F. (2023). Are marriage-related taxes and social security benefits holding back female labour supply?. The Review of Economic Studies, 90(1), 102-131.

Burchardi, K. B., Chaney, T., Hassan, T. A., Tarquinio, L. and Terry, S. J. (2024). "Immigration, Innovation, and Growth", American Economic Review (Forthcoming).

Chetty, R., Jackson, M. O., Kuchler, T., Stroebel, J., Hendren, N., Fluegge, R. B., Gong, S., Gonzalez, F., Grondin, A., Jacob, M., Johnston, D., Koenen, M., Laguna-Muggenburg, E., Mudekereza, F., Rutter, T., Thor, N., Townsend, W., Zhang, R., Bailey, M., … Wernerfelt, N. (2022). Social capital I: measurement and associations with economic mobility. Nature, 608(7921), 108–121.

Chiappori, P.-A., Salanie, B., and Weiss, Y. (2017). Partner Choice, Investment in Children, and the Marital College Premium. American Economic Review, 107(8).

Chiappori, P.-A., Fiorio, C., Galichon, A., and Verzillo, S. (2022). Assortative Matching on Income.

Costa, D.L., and Kahn, M.E. (2003). Civic engagement and community heterogeneity: An economist's perspective. Perspectives on Politics, 1(1), 103-111.

Daniels, C. R., & Heidt-Forsythe, E. (2012). Gendered eugenics and the problematic of free market reproductive technologies: Sperm and egg donation in the United States. Signs: Journal of Women in Culture and Society, 37(3), 719-747.

D'Albis, H., Gobbi, P.E., and Greulich, A. (2017). Having a second child and access to childcare: evidence from European countries. Journal of Demographic Economics, 83(2), 177–210.

Dinesen, P.T., Schaeffer, M., and Sønderskov, K.M. (2020). Ethnic diversity and social trust: A narrative and meta-analytical review. Annual Review of Political Science, 23, 441-465.

Doepke, M., Hannusch, A., Kindermann, F., and Tertilt, M. (2023). The economics of fertility: A new era. In Handbook of the Economics of the Family (Vol. 1, No. 1, pp. 151-254). North-Holland.

Doepke, M., and Tertilt, M. (2016). Families in macroeconomics. Chapter 23. In: Handbook of Macroeconomics, vol. 2. North Holland.

Dubuc, S. (2012). Immigration to the UK from High-Fertility Countries: Intergenerational Adaptation and Fertility Convergence. Population and Development Review 38(2), 353-368.

Fernandez, R., Guner, N., and Knowles, J. (2005). Love and money: A theoretical and empirical analysis of household sorting and inequality. The Quarterly Journal of Economics, 120(1), 273-344.

Fisman, R., Iyengar, S.S., Kamenica, E., and Simonson, I. (2006). Gender differences in mate selection: Evidence from a speed dating experiment. The Quarterly Journal of Economics, 121(2), 673-697.

Goldin, C., and Katz, L.F. (2002). The power of the pill: Oral contraceptives and women's career and marriage decisions. Journal of Political Economy, 110(4), 730-770.

Greenwood, J., Guner, N., and Vandenbroucke, G. (2017). Family Economics Writ Large. Journal of Economic Literature, 55(4), 1346-1434.

Greenwood, J., Guner, N., Kocharkov, G., and Santos, C. (2014). Marry Your Like: Assortative Mating and Income Inequality. The American Economic Review, 104(5), 348–353.

Guner, N., Kaygusuz, R., and Ventura, G. (2012). Taxation and household labour supply. The Review of Economic Studies, 79(3), 1113-1149.

Guner, N., and Ventura, G. (2014). Sorting within and across countries. American Economic Review, 104(3), 349-353.

Hacamo, I. (2021). The Babies of Mortgage Market Deregulation. Review of Financial Studies 34, 907–948.

Hazan, M., and Zoabi, H. (2015). Do highly educated women choose smaller families?. The Economic Journal, 125(587), 1191-1226.

Hwang, W. C. (2013). Who are people willing to date? Ethnic and gender patterns in online dating. Race and Social Problems, 5, 28-40.

Kearney, M. S. and Levine, P.B., 2009. "Subsidized Contraception, Fertility and Sexual Behavior", *Review of Economics and Statistics* 91, 137-151.

Kearney, M. S. and Levine, P.B., 2015. "Media Influences on Social Outcomes: The Impact of MTV's 16 and Pregnant on Teen Childbearing", American Economic Review 105, 3597-3632.

Kearney, M.S., and Levine, P.B. (2022). The Causes and Consequences of Declining US Fertility. Aspen Institute, August.

Kleven, H., Landais, C., Posch, J., Steinhauer, A., and Zweimüller, J. (2019). Child penalties across countries: Evidence and explanations. AEA Papers and Proceedings, 109, 122-126.

McCrary, J., and Royer, H. 2011. "The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth", American Economic Review 101, 158-195.

Mulder, C. H., & Wagner, M. (2001). The connections between family formation and first-time home ownership in the context of West Germany and the Netherlands. European Journal of Population/Revue europeenne de demographie, 17, 137-164.

Myers, C.K. (2017). The power of abortion policy: Reexamining the effects of young women's access to reproductive control. Journal of Political Economy, 125(6), 2178-2224.

Nickerson, J., and Solomon, D.H. (2024). Car seats as contraception. Journal of Law and Economics (Forthcoming)

Olivetti, C., and Petrongolo, B. (2017). The economic consequences of family policies: lessons from a century of legislation in high-income countries. Journal of Economic Perspectives, 31(1), 205-230.

Parrado, E.A. and Morgan, S.P. (2008). Intergenerational fertility among Hispanic women: New evidence of immigrant assimilation. Demography, 45, 651-671.

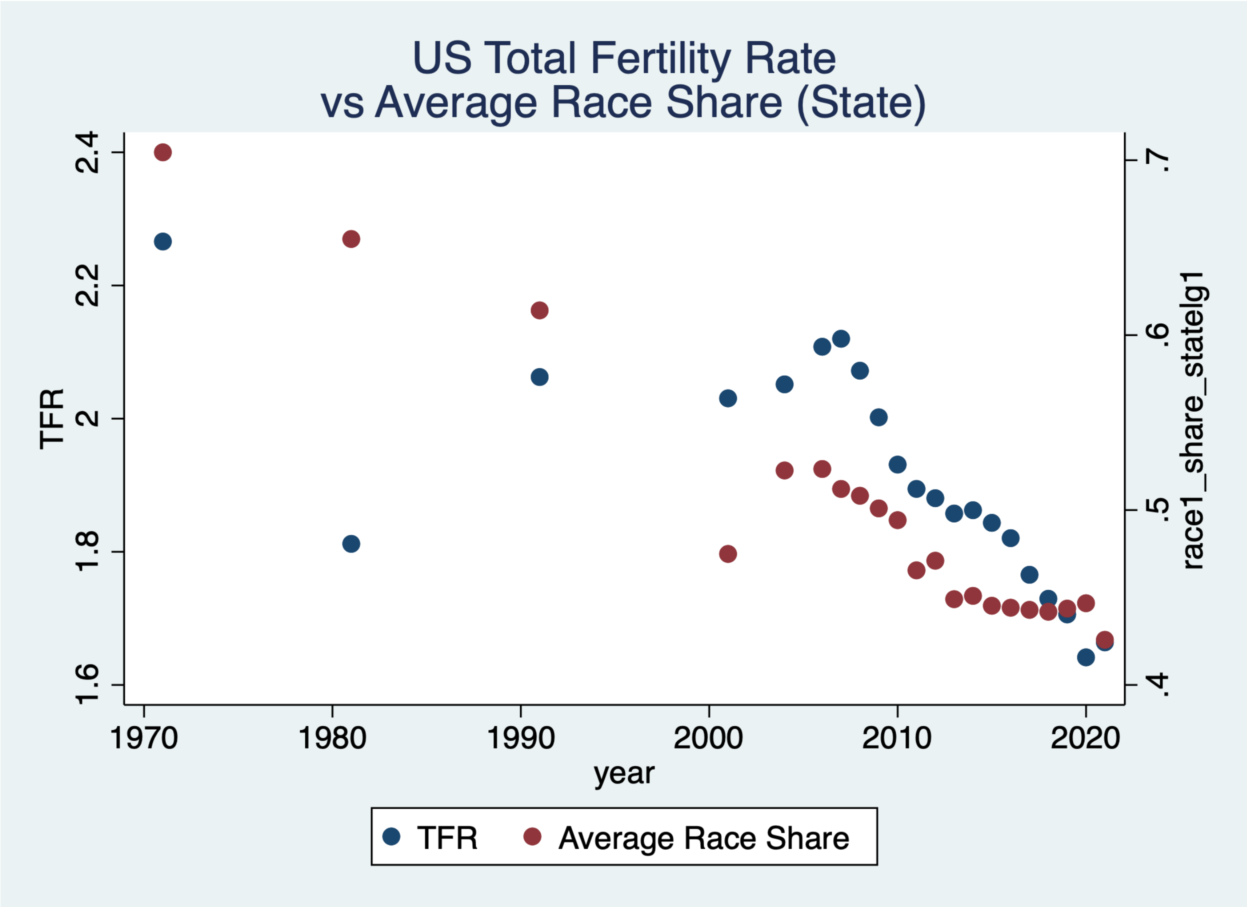
Putnam, R.D. (2007). E pluribus unum: Diversity and community in the twenty-first century the 2006 Johan Skytte Prize Lecture. Scandinavian Political Studies, 30(2), 137-174.

Schwartz, C.R., and Mare, R.D. (2005). Trends in educational assortative marriage from 1940 to 2003. Demography, 42(4), 621-646.

Sobotka, T. (2008). Overview Chapter 7: The rising importance of migrants for childbearing in Europe. Demographic research, 19, 225-248.

White, M. J., Moreno, L., & Guo, S. (1995). The interrelation of fertility and geographic mobility in Peru: a hazards model analysis. *International Migration Review*, *29*(2), 492-514.

Figure 1 – Time Series Changes in Average Race Share and Total Fertility Rate



This figure plots the Total Fertility Rate from the St Louis Fed) for the United States, with the average of state-level measures of *Race Share*, the fraction of the local area population that is of the same race as you. These are computed at the state level, where differences in the geographic definitions and collection of information are less likely to affect time series changes. The *Race Share* variable is lagged by a year relative to TFR, to match the timing of conception decisions, but is shown for the corresponding year of the TFR.

Table 1 – Summary Statistics

This table presents summary statistics for the main variables used in the paper. Data is taken from U.S. census and American Community Survey files from 1850 to 2021, obtained from IPUMS, for all women aged 18-40 at the time of survey. *Number of Children* is the number of children the woman has living at home at the time of the survey. *Race Share* is the fraction of the local area population of the same race as the women, where race is the nine broad racial groups classified by the census, plus a tenth for Hispanic/Latino. *Race Herfindahl* is the sum of squared percentages for each racial group in that local area. Panel B presents breakdowns by year. Local areas are defined as being first city (if available), then county (if available), then detailed metro area. The number of local areas and total respondents is shown, along with race shares for the ten groups in that year. A blank value means that classification was not collected at the time.

Panel A - Whole Sample								
	N	Mean	Std Dev	Min	25th Pct	50th Pct	75th Pct	Max
Number of Children	7,156,888	0.98	1.30	0	0	0	2	9
Age	7,156,888	28.93	6.68	18	23	29	35	40
Race Share	7,156,888	0.557	0.324	0.000	0.231	0.635	0.855	1.000
Race Herfindahl	7,156,888	0.587	0.212	0.177	0.411	0.572	0.763	1.000

Panel B - By Year														
			Race Share	Race Share			Amer.						Three	
Year	N	# Areas	Mn	SD	White	Black	Indian	Chinese	Japanese	Asia Pac.	Other	Two Races	Races	Hispanic
1850	7,418	72	0.917	0.191	0.9520	0.0453								0.0027
1860	12,548	105	0.938	0.168	0.9656	0.0308	0.0002	0.0002						0.0033
1870	18,509	176	0.891	0.212	0.9240	0.0715	0.0001	0.0002						0.0042
1880	26,751	259	0.885	0.217	0.9187	0.0750	0.0000	0.0009						0.0054
1900	60,426	513	0.894	0.209	0.9243	0.0701	0.0001	0.0002	0.0000					0.0053
1910	85,664	693	0.886	0.216	0.9182	0.0739	0.0001	0.0004	0.0008	0.0002				0.0063
1920	98,633	398	0.871	0.227	0.9141	0.0740	0.0002	0.0003	0.0017	0.0005				0.0092
1930	142,954	1,111	0.842	0.246	0.8919	0.0889	0.0004	0.0005	0.0019	0.0005				0.0160
1940	140,756	234	0.834	0.251	0.8904	0.0908	0.0003	0.0005	0.0010	0.0002				0.0168
1950	192,521	259	0.777	0.274	0.8494	0.1169	0.0004	0.0010	0.0017	0.0003	0.0001			0.0301
1970	403,112	187	0.716	0.301	0.7961	0.1290	0.0022	0.0037	0.0052	0.0048	0.0013			0.0578
1980	320,561	511	0.687	0.309	0.7591	0.1334	0.0047	0.0054	0.0040	0.0137	0.0011			0.0784
1990	332,815	565	0.647	0.316	0.7182	0.1234	0.0058	0.0099	0.0045	0.0257	0.0009			0.1118
2000	241,894	159	0.486	0.297	0.5609	0.1504	0.0049	0.0155	0.0042	0.0458	0.0022	0.0192	0.0014	0.1954
2003	20,136	61	0.572	0.323	0.6345	0.0777	0.0067	0.0089	0.0114	0.0502	0.0026	0.0162	0.0037	0.1880
2005	312,914	651	0.555	0.323	0.6213	0.1167	0.0051	0.0149	0.0040	0.0483	0.0031	0.0125	0.0011	0.1731
2006	322,868	650	0.542	0.319	0.6083	0.1210	0.0052	0.0158	0.0036	0.0496	0.0030	0.0134	0.0012	0.1789
2007	324,831	650	0.537	0.318	0.6037	0.1193	0.0049	0.0158	0.0037	0.0517	0.0030	0.0146	0.0012	0.1822
2008	322,150	650	0.529	0.317	0.5959	0.1205	0.0050	0.0159	0.0033	0.0529	0.0028	0.0159	0.0016	0.1863
2009	325,889	650	0.522	0.315	0.5887	0.1220	0.0048	0.0163	0.0033	0.0540	0.0027	0.0170	0.0015	0.1898
2010	328,152	650	0.503	0.301	0.5746	0.1254	0.0051	0.0169	0.0030	0.0555	0.0023	0.0188	0.0018	0.1966
2011	327,101	650	0.500	0.307	0.5620	0.1329	0.0060	0.0181	0.0030	0.0546	0.0022	0.0201	0.0023	0.1989
2012	274,379	504	0.473	0.300	0.5329	0.1304	0.0050	0.0206	0.0033	0.0614	0.0024	0.0208	0.0031	0.2200
2013	280,088	504	0.474	0.300	0.5390	0.1259	0.0049	0.0209	0.0034	0.0618	0.0026	0.0216	0.0035	0.2165
2014	278,666	504	0.468	0.299	0.5336	0.1253	0.0046	0.0223	0.0030	0.0637	0.0026	0.0226	0.0036	0.2187
2015	281,817	504	0.466	0.298	0.5326	0.1222	0.0046	0.0230	0.0029	0.0637	0.0025	0.0230	0.0034	0.2220
2016	282,583	504	0.464	0.297	0.5326	0.1183	0.0043	0.0240	0.0028	0.0647	0.0028	0.0238	0.0038	0.2229
2017	288,755	504	0.461	0.297	0.5321	0.1142	0.0044	0.0244	0.0028	0.0674	0.0029	0.0248	0.0038	0.2231
2018	290,629	504	0.462	0.297	0.5348	0.1111	0.0045	0.0245	0.0028	0.0668	0.0030	0.0252	0.0039	0.2235
2019	287,380	504	0.463	0.299	0.5388	0.1069	0.0044	0.0257	0.0026	0.0680	0.0029	0.0262	0.0037	0.2208
2020	234,678	504	0.440	0.292	0.5181	0.1059	0.0042	0.0271	0.0025	0.0703	0.0047	0.0405	0.0052	0.2215
2021	289,310	504	0.428	0.286	0.5008	0.1039	0.0041	0.0263	0.0025	0.0734	0.0058	0.0438	0.0052	0.2344

Table 2 – Number of Children and Racial Diversity

This table presents the baseline relationship between the number of children a woman has and various measures of local levels of racial diversity. Data is taken from the U.S. decennial census from 1850 to 2000, and from the American Community Survey from 2001 to 2021. Observations are taken for women ages 18-40 at the time of survey who have non-missing geographic information for either city, county, or detailed metro area. The dependent variable is the number of children the woman has. In Panel A, the main independent variable is *Race Share*, the fraction of the population in the local area who are of the same race/ethnicity as the woman ("race", as a shorthand). Race is constructed as ten categories, with nine categories for the broad racial groups (if the respondent is not Hispanic or Latino) and a tenth category for Hispanic/Latino. Local area is measured first as county (if present), then city (if county is missing), then metro area (if both city and county are missing). Fixed effects are included as labeled for race, state, year, state-by-year, demographics (age, marital status, education, race, employment status, income decile, and citizenship), demographics by state and year, area type (i.e., county, city or metro), deciles of population within that area type, deciles of population by year, area (where area is measured at the same level as the race share), and area-by-year. Area parametric controls are included as average income decile, average age, fraction employed, and a z-score for the fraction of residents who moved in the last 1 or 5 years (depending on data availability). These are also interacted with year fixed effects. The earliest year for data availability (given the set of controls) is noted. The effect on the number of children of a one standard deviation change in race share is indicated, both for an unconditional one standard deviation change across all observations, and a conditional standard deviation – a one standard deviation change in the residual after first regressing race share on the set of fixed effects in the regression. In Panel B, the race share variable is replaced with a race Herfindahl index for that area and year. In Panel C, both the race share and race Herfindahl index are included. In Panel D, the Herfindahl Index is computed only among races other than the respondent's own race (so it measures concentration among the races other than your own). Standard errors are double clustered by year and state. Coefficients are in the top row, and t-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - Race Share											
	Dependent variable is number of children at time of survey										
Race Share	0.159***	0.708***	0.435***	0.310***	0.241***	0.291***	0.204***	0.160***	0.166***	0.194***	0.197***
	(2.84)	(6.89)	(6.92)	(7.30)	(8.58)	(7.03)	(6.58)	(5.00)	(5.26)	(5.89)	(5.88)
Effect of 1 σ change (unconditional)	0.052	0.230	0.141	0.101	0.078	0.094	0.066	0.052	0.054	0.063	0.064
Effect of 1 σ change (conditional)	0.052	0.123	0.067	0.049	0.038	0.037	0.026	0.019	0.020	0.020	0.020
Race	N	Y	Y	N	N	N	N	N	N	N	N
State	N	N	Y	Y	N	N	N	N	N	N	N
Year	N	N	Y	Y	N	N	N	N	N	N	N
State-Year FE	N	N	N	N	Y	Y	Y	Y	Y	Y	N
Demographics FE	N	N	N	Y	Y	N	N	N	N	N	N
Demographics*(State, Year) FE	N	N	N	N	N	Y	Y	Y	Y	Y	Y
Area Type	N	N	N	N	N	N	Y	N	N	N	N
Area Traits	N	N	N	N	N	N	Y	Y	N	N	N
Area Traits * Year FE	N	N	N	N	N	N	N	N	Y	Y	N
Area Type * Population FE	N	N	N	N	N	N	N	Y	N	N	N
Area Type * Population FE *Year	N	N	N	N	N	N	N	N	Y	Y	N
Area FE	N	N	N	N	N	N	N	N	N	Y	N
Area * Year FE	N	N	N	N	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980	1980	1980	1980	1980
Observations	7,156,888	7,156,888	7,156,887	5,967,596	5,967,596	5,967,585	5,967,585	5,967,585	5,967,585	5,967,585	5,967,585
R-squared	0.002	0.017	0.041	0.313	0.392	0.400	0.402	0.402	0.402	0.404	0.405

Panel B - Race Herfindahl							
	Dependent variable is number of children at time of survey						
Race Herfindahl	0.590***	0.819***	0.664***	0.342***	0.207***	0.121***	0.037***
	(5.33)	(7.76)	(15.02)	(14.95)	(8.76)	(3.91)	(4.16)
Effect of 1 σ change (unconditional)	0.125	0.174	0.141	0.073	0.044	0.026	0.008
Effect of 1 σ change (conditional)	0.125	0.156	0.083	0.044	0.023	0.011	0.001
Race	N	Y	Y	N	N	N	N
State-Year FE	N	N	Y	Y	Y	Y	Y
Demographics FE	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	N	N	N	Y	Y	Y
Area Type	N	N	N	N	Y	N	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * Year FE	N	N	N	N	N	Y	Y
Area Type * Population FE *Year	N	N	N	N	N	Y	Y
Area FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1980	1980	1980	1980	1980
Observations	7,156,888	7,156,888	7,156,887	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.009	0.022	0.042	0.393	0.402	0.402	0.404

Panel C - Race Share and Race Herfindahl							
Race Share	-0.162***	0.262***	0.236***	0.143***	0.157***	0.159***	0.197***
	(-5.02)	(4.65)	(4.81)	(4.68)	(4.24)	(4.54)	(5.85)
Race Herfindahl	0.751***	0.674***	0.530***	0.261***	0.112***	0.023	-0.080***
	(6.05)	(6.15)	(11.57)	(9.13)	(3.56)	(0.62)	(-3.48)
Race	N	Y	Y	N	N	N	N
State-Year FE	N	N	Y	Y	Y	Y	Y
Demographics FE	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	N	N	N	Y	Y	Y
Area Type	N	N	N	N	Y	N	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * Year FE	N	N	N	N	N	Y	Y
Area Type * Population FE *Year	N	N	N	N	N	Y	Y
Area FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1980	1980	1980	1980	1980
Observations	7,156,888	7,156,888	7,156,887	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.010	0.023	0.043	0.393	0.402	0.402	0.404

Panel D - Race Share and Race Herfindahl Across Other Races							
Race Share	0.244***	0.694***	0.528***	0.319***	0.241***	0.182***	0.203***
	(4.42)	(7.17)	(6.88)	(7.57)	(7.17)	(5.61)	(5.37)
Other Race Herfindahl	0.691***	0.569***	0.310***	0.235***	0.094***	0.036*	0.014
	(6.31)	(5.28)	(3.48)	(4.81)	(3.83)	(2.05)	(0.66)
Race	N	Y	Y	N	N	N	N
State-Year FE	N	N	Y	Y	Y	Y	Y
Demographics FE	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	N	N	N	Y	Y	Y
Area Type	N	N	N	N	Y	N	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * Year FE	N	N	N	N	N	Y	Y
Area Type * Population FE *Year	N	N	N	N	N	Y	Y
Area FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1980	1980	1980	1980	1980
Observations	7,114,973	7,114,973	7,114,972	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.014	0.024	0.042	0.393	0.402	0.402	0.404

Table 3 – Mobility

This table examines whether the main effects are due to selection effects based on mobility. I conduct similar versions of the main regressions in Table 2, but limit the sample to various categories of women less likely to have relocated: i) those living in the state they were born in, ii) those who haven't moved in the past year, iii) those who haven't moved in the past 5 years, and combinations of these. Standard errors are double clustered by year and state. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Race Share	0.161***	0.200***	0.124***	0.191***	0.190***
	(5.05)	(5.13)	(3.87)	(5.11)	(5.42)
Selection	Living in State of Birth	No Move in Last Year	No Move in Last Five Years	No Move in Last One or Five Years	Any of Previous
Demographics*(State, Year) FE	Y	Y	Y	Y	Y
Area * Year FE	Y	Y	Y	Y	Y
Clustering	State, Year	State, Year	State	State, Year	State, Year
Observations	3,211,983	3,861,439	423,389	4,284,878	5,100,548
R-squared	0.405	0.406	0.457	0.412	0.407

Table 4 – Different Time Periods

This table examines how racial diversity is associated with number of children in different time periods of US history. Specifications from Table 2 are run separately for i) 1850-1860, ii) 1870-1890, iii) 1900-1940, 1950-1970, 1980-1990, and 2000-2021. Panel A includes state by year fixed effects as well as age and race both interacted with state and year. Panel B also includes area by year fixed effects. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - No Area*Year Fixed Effects						
Race Share	1.889*	1.015***	0.949***	1.054***	0.552***	0.515***
	(2.01)	(2.83)	(3.19)	(3.91)	(6.37)	(5.92)
Period	1850-1860	1870-1890	1900-1940	1950-1970	1980-1990	2000-2021
Effect of 1 σ change (unconditional)	0.335	0.215	0.227	0.310	0.173	0.158
State*Year	Y	Y	Y	Y	Y	Y
(Age, Race)*(State, Year) FE	Y	Y	Y	Y	Y	Y
Area*Year FE	N	N	N	N	N	N
Observations	19,929	105,637	467,974	595,615	653,356	5,314,218
R-squared	0.302	0.252	0.181	0.282	0.261	0.269

Panel B - With Area*Year Fixed Effects						
Race Share	-1.162	0.049	0.069	0.355*	0.338***	0.321***
	(-1.23)	(0.32)	(0.62)	(1.93)	(5.35)	(5.60)
Period	1850-1860	1870-1890	1900-1940	1950-1970	1980-1990	2000-2021
Effect of 1 σ change (unconditional)	-0.206	0.010	0.017	0.104	0.106	0.098
State*Year	Y	Y	Y	Y	Y	Y
(Age, Race)*(State, Year) FE	Y	Y	Y	Y	Y	Y
Area*Year FE	Y	Y	Y	Y	Y	Y
Observations	19,929	105,635	467,969	595,615	653,356	5,314,218
R-squared	0.311	0.267	0.198	0.289	0.273	0.283

Table 5 – Effects by Race

This table examines how the effect of racial diversity on number of children varies with the race of the woman. Observations are taken for women ages 18-40, in US census surveys from 1850 to 2021. Specifications from Table 2 are run with interactions between the baseline race share variable, and then ten racial ethnic groups I consider (from census categories): white, black, native American, Chinese, Japanese, Asian/Pacific Islander, other, two races, three or more races, and Hispanic. Controls are the same as in Table 2. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Race Share * White	1.000***	0.703***	0.327***	0.414***	0.263***	0.264***	0.185***
	(7.38)	(7.46)	(8.62)	(9.74)	(6.18)	(5.90)	(3.70)
Race Share * Black	0.084	0.111	0.136**	0.070**	0.131***	0.137***	0.272***
	(0.76)	(1.26)	(2.22)	(2.50)	(3.33)	(3.75)	(4.24)
Race Share * Native American	0.474***	0.436***	0.705***	0.474***	0.167	0.188*	0.252*
	(3.63)	(4.81)	(9.99)	(4.56)	(1.67)	(1.79)	(1.90)
Race Share * Chinese	-0.506**	-0.015	-0.067	-0.077	0.673***	0.704***	0.957***
	(-2.18)	(-0.07)	(-0.63)	(-0.39)	(4.09)	(4.24)	(7.46)
Race Share * Japanese	0.584**	-0.855***	0.156*	-0.025	0.630	0.681	1.114**
	(2.53)	(-5.18)	(2.06)	(-0.06)	(1.35)	(1.47)	(2.26)
Race Share * Asian / Pacific Islander	-0.259	-0.166	0.053	-0.110	0.171	0.182	0.141
	(-0.90)	(-1.12)	(0.35)	(-0.83)	(1.36)	(1.42)	(0.83)
Race Share * Other	0.710	15.308***	4.475	3.121	3.540	3.700	5.838*
	(0.20)	(3.05)	(1.39)	(0.95)	(1.05)	(1.13)	(2.07)
Race Share * Two Races	0.554	0.854	0.257	1.692**	2.723***	2.879***	2.011***
	(0.47)	(1.60)	(0.60)	(2.19)	(4.20)	(4.33)	(4.34)
Race Share * Three or More Races	2.535***	1.122*	1.381***	-0.435	-0.648	-0.626	-1.003
	(5.44)	(1.75)	(4.40)	(-0.50)	(-0.67)	(-0.64)	(-1.11)
Race Share * Hispanic	0.122	0.093	0.137*	0.137*	-0.013	-0.000	0.158
	(1.38)	(1.13)	(2.03)	(1.73)	(-0.24)	(-0.00)	(1.56)
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Area FE	N	N	N	N	N	N	N
Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	7,156,888	7,156,887	5,967,596	5,967,585	5,967,585	5,967,585	5,967,585
R-squared	0.020	0.042	0.392	0.400	0.402	0.403	0.405

Table 6 – International Results

This table examines the relationship between local racial diversity and the number of children a woman has, for different countries around the globe. All countries with census data on IPUMS that contain information on race are included, Observations are taken for all women age 18-40 at the time of the survey. The dependent variable is the number of children the woman has. The independent variable is the fraction of the local area population of the same race as the woman. Geography is measured at the finest level available (usually "level 2" on IPUMS, generally corresponding to regions within a state, but sometimes "level 1", generally corresponding to a state, if there is no level 2 information). Race is measured according to whatever definition is used in the country in question. Controls are included for level 1 by year (colloquially, "state-year"), demographics, demographics by state and year, log population density by year, and local region (i.e., level 2) by year. Demographics variables include whichever is available for that country, out of urban status, marital status, race, employment status, age, and educational attainment. Full country-level information on race definitions and controls is included in the Appendix. Panel A examines the United Kingdom and three countries from Africa – Mozambique, South Africa, and Zimbabwe. Panel B examines countries from Central America – Costa Rica, Cuba, El Salvador and Jamaica. Panel C examines countries from South America – Brazil, Colombia, Ecuador and Uruguay. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - UK and Africa

Country	UK		Mozambique			South Africa			Zimbabwe		
Race Share	0.528***	0.141	0.501***	0.664***	-0.006	0.158***	0.323**	0.319***	3.360***	3.887***	-0.560
	(5.05)	(0.87)	(3.40)	(6.46)	(-0.07)	(3.28)	(2.81)	(7.49)	(2.67)	(3.62)	(-0.09)
State-Year FE	N	Y	Y	Y	N	Y	Y	N	Y	Y	N
Demographics FE	Y	Y	Y	N	N	Y	N	N	Y	N	N
Demographics*Year FE	N	N	N	Y	Y	N	Y	Y	N	N	N
Demographics*State FE	N	N	N	Y	Y	N	Y	Y	N	Y	Y
Ln Population Density*Year	Y	N	N	Y	Y	N	Y	Y	N	Y	Y
Local Region*Year	N	N	N	N	Y	N	N	Y	N	N	Y
Number of Years	1	1	2	2	2	4	4	4	1	1	1
Race Share Level	State	State	Local	Local	Local	Local	Local	Local	Local	Local	Local
Clustering	State	State	Local	Local	Local	Local	Local	Local	Local	Local	Local
Observations	92,397	92,397	638,099	638,096	638,096	1,797,315	1,797,315	1,797,315	122,944	122,938	122,938
R-squared	0.439	0.440	0.286	0.299	0.305	0.293	0.301	0.302	0.340	0.349	0.353

Panel B - Central America												
Country	Costa Rica	Costa Rica	Costa Rica	Cuba	Cuba	Cuba	El Salvador	El Salvador	El Salvador	Jamaica	Jamaica	Jamaica
Race Share	-0.158	-0.212*	0.270*	0.012	-0.076*	0.064**	0.308***	0.319***	0.026	0.241	0.241	-0.045
	(-1.26)	(-1.77)	(2.00)	(0.80)	(-1.85)	(2.23)	(3.50)	(3.72)	(0.36)	(0.42)	(0.42)	(-0.08)
State-Year FE	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N
Demographics FE	Y	N	N	Y	N	N	Y	N	N	Y	N	N
Demographics*Year FE	N	Y	Y	N	Y	Y	N	N	N	N	Y	Y
Demographics*State FE	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Ln Population Density*Year	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Local Region*Year	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Number of Years	2	2	2	2	2	2	1	1	1	3	3	3
Race Share Level	Local	Local	Local	Local	Local	Local	Local	Local	Local	State	State	State
Clustering	Local	Local	Local	Local	Local	Local	Local	Local	Local	State	State	State
Observations	148,777	148,777	148,777	383,755	383,755	383,755	108,368	108,364	108,364	39,723	39,723	39,723
R-squared	0.432	0.444	0.446	0.268	0.273	0.275	0.405	0.414	0.417	0.301	0.301	0.303

Panel C - South America												
Country	Brazil	Brazil	Brazil	Colombia	Colombia	Colombia	Ecuador	Ecuador	Ecuador	Uruguay	Uruguay	Uruguay
Race Share	-0.038***	-0.056***	-0.015**	0.017	-0.027	0.012	0.200**	0.108*	0.016	-1.311***	-1.869***	1.056***
	(-3.70)	(-4.33)	(-2.05)	(0.28)	(-0.77)	(0.40)	(2.45)	(1.81)	(0.39)	(-5.08)	(-8.35)	(2.81)
State-Year FE	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N
Demographics FE	Y	N	N	Y	N	N	Y	N	N	Y	N	N
Demographics*Year FE	N	Y	Y	N	N	N	N	Y	Y	N	Y	N
Demographics*State FE	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Ln Population Density*Year	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Local Region*Year	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Number of Years	4	4	4	1	1	1	2	2	2	1	1	1
Race Share Level	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local
Clustering	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local	Local
Observations	16,032,064	16,032,064	16,032,064	678,567	678,566	678,566	492,708	492,706	492,706	52,118	52,103	52,103
R-squared	0.440	0.469	0.473	0.385	0.395	0.399	0.394	0.402	0.403	0.386	0.399	0.402

Table 7 – Racial Diversity, Marriage and Divorce

This table examines how local levels of racial diversity affect outcomes related to marriage and divorce. Observations are taken for women ages 18-40, in US census surveys from 1850 to 2021. The main independent variable is race share – the fraction of the local population that is the same racial/ethnic group as the woman. Controls are the same as those in Table 2. In Panel A, the dependent variable is a dummy equal to one if the woman is currently married, and zero otherwise. In Panel B, the dependent variable is a dummy equal to one if the woman ever married (that is, if she is either currently married, widowed, or divorced), and zero otherwise. In Panel C, the sample is limited to women who got married, and the dependent variable is a dummy equal to one if there are currently divorced. In Panel D, the same is limited to women who are currently married, and on their first marriage. The dependent variable is the age at which they got married. Coefficients are in the top row, and t-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - Currently Married							
Race Share	0.192***	0.046**	0.081***	0.112***	0.040***	0.042***	0.036***
	(4.57)	(2.25)	(4.02)	(4.39)	(3.44)	(3.67)	(2.95)
Effect of 1 σ change (unconditional)	0.062	0.015	0.026	0.036	0.013	0.014	0.012
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Local Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	7,118,413	7,118,412	5,967,596	5,967,587	5,967,587	5,967,587	5,967,587
R-squared	0.030	0.067	0.298	0.310	0.314	0.315	0.319

Panel B - Ever Married							
Race Share	0.191***	0.050**	0.074***	0.119***	0.041***	0.043***	0.036***
	(5.11)	(2.57)	(3.76)	(4.54)	(4.14)	(4.54)	(3.36)
Effect of 1 σ change (unconditional)	0.062	0.016	0.024	0.039	0.013	0.014	0.012
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Local Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	7,118,413	7,118,412	5,967,596	5,967,587	5,967,587	5,967,587	5,967,587
R-squared	0.023	0.068	0.376	0.387	0.392	0.392	0.396

Panel C - Age at First Marriage (Given Currently Married, Married Only Once)							
Race Share	-1.790***	-1.382***	-1.124***	-1.443***	-0.686***	-0.691***	-0.627***
	(-5.96)	(-7.61)	(-8.87)	(-6.98)	(-5.24)	(-5.34)	(-5.48)
Effect of 1 σ change (unconditional) in Months	-6.6	-5.1	-4.1	-5.3	-2.5	-2.5	-2.3
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Local Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	1,730,380	1,730,380	1,730,380	1,730,368	1,730,368	1,730,368	1,730,368
R-squared	0.026	0.054	0.234	0.243	0.247	0.248	0.254

Panel D - Divorced, Given Married							
Race Share	-0.060**	0.012	-0.032***	-0.019**	0.003	0.002	0.007
	(-2.39)	(0.98)	(-3.99)	(-2.64)	(0.48)	(0.34)	(1.00)
Effect of 1 σ change (unconditional)	-0.019	0.004	-0.010	-0.006	0.001	0.001	0.002
Race	Y	Y	N	N	N	N	N
State-Year FE	N	Y	Y	Y	Y	Y	N
Demographics FE	N	N	Y	N	N	N	N
Demographics*(State, Year) FE	N	N	N	Y	Y	Y	Y
Area Type * Population FE	N	N	N	Y	Y	N	N
Area Type * Population FE *Year	N	N	N	N	N	Y	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * (State, Year) FE	N	N	N	N	N	Y	N
Local Area * Year FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	3,883,186	3,883,186	3,061,482	3,061,468	3,061,468	3,061,468	3,061,468
R-squared	0.019	0.038	0.127	0.136	0.137	0.138	0.141

Table 8 – Effects of Interracial Marriage by Race and Sex

This table examines whether the effects of racial diversity on the number of children are impacted by measures of interracial marriage. In this table, I consider both men and women, ages 18-40, using the same survey data from 1850 to 2021, and take as the dependent variable the number of children assigned to that person. In columns 1-3, I interact the race share measure with a measure of abnormal levels of interracial marriage for that racial group and year. This is done by taking the set of all men and women aged 18-50 in that survey year, and computing the number from that race who are currently married to someone of a different race (using the previous definitions of race). Next, I randomize the races of all men and women in that sample who are currently married, and compute the number of interracial marriages I have under this random pairing. I compute 1000 such simulations, and use these to create a mean random rate of interracial marriage, and a standard deviation. The anormal interracial marriage measure is the actual rate minus the randomized mean, divided by the randomized standard deviation. This is interacted with race share, and included separately in column 1 (whereas in all other columns, the base variable is absorbed by the race-by-year fixed effect). In columns 5-8, I consider sex differences in the interracial marriage rate. For each race and year, I compute the number of women from that race who are married to someone of a different race, divided by the number of men from that race who are married to someone of a different race. This intermarriage sex ratio is then interacted with race share, and race share interacted with a dummy for the person being male. The other interaction terms (intermarriage sex ratio, where it is not omitted, and intermarriage sex ratio interacted with a male dummy) are included in the regression, but not reported. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Race Share	0.026	0.016	-0.021	0.121***	0.309**	0.304**	0.268***	0.206***
	(0.50)	(0.30)	(-0.47)	(2.97)	(2.82)	(2.74)	(3.77)	(3.30)
Race Share * Abnormal	-0.140***	-0.144***	-0.103***	-0.021				
Intermarriage Rate	(-5.34)	(-5.42)	(-4.33)	(-0.94)				
Race Share * InterMarriage Sex					-0.180	-0.119	-0.215**	-0.135
Ratio					(-1.38)	(-0.87)	(-2.33)	(-1.50)
Race Share * InterMarriage Sex					0.235***	0.115***	0.127***	0.141***
Ratio * Male					(5.78)	(4.77)	(5.61)	(5.77)
Sex*State-Year FE	Y	Y	Y	N	Y	Y	Y	N
Demographics*(State, Year) FE	Y	N	N	N	Y	N	N	N
Sex*Demographics*(State, Year) FE	N	Y	Y	Y	N	Y	Y	Y
Area Type * Population FE *Year	N	N	Y	N	N	N	Y	N
Sex*Area Traits * (State, Year) FE	N	N	Y	N	N	N	Y	N
Sex * Area * Year FE	N	N	N	Y	N	N	N	Y
Observations	11,853,697	11,853,691	11,853,691	11,853,691	11,853,697	11,853,691	11,853,691	11,853,691
R-squared	0.408	0.425	0.427	0.429	0.409	0.425	0.427	0.430

Table 9 – Trust and Birth Rates

This table examines how local measures of trust affect the relationship between racial isolation and the number of children a woman has. In Panel A, I consider the generalized state-level trust measures in 2006, from Putnam (2007). These are compared to state-level race share measures (in columns 1-6) and baseline local area measures (in columns 7-12). The years examined are either all years, only the years between 2001 and 2010, or 2006 only. In Panel B, I consider the county-level measures of social capital from Chetty et al. (2022), namely the volunteering rate, friendship clustering, and economic connectedness. These are compared with county-only measures of race share. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - Social Capital Survey Direct Trust Measure												
Race Share (State)	0.358***	0.282***	0.353***	0.267***	0.328***	0.243***						
	(7.42)	(7.11)	(6.50)	(5.85)	(5.81)	(4.79)						
State Level Trust		0.432***		0.465***		0.449***		0.326**		0.350***		0.359***
		(4.38)		(4.77)		(4.43)		(2.77)		(2.99)		(3.03)
Race Share (Base)							0.294***	0.274***	0.286***	0.264***	0.283***	0.262***
							(9.61)	(9.84)	(7.85)	(7.65)	(8.78)	(8.62)
Years	All	All	2001-2010	2001-2010	2006	2006	All	All	2001-2010	2001-2010	2006	2006
Demographics *Year FE	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y	N
Observations	9,179,897	9,179,897	3,154,144	3,154,144	414,474	414,474	5,967,596	5,914,400	1,956,940	1,945,083	322,868	321,148
R-squared	0.384	0.384	0.369	0.370	0.371	0.372	0.393	0.394	0.381	0.382	0.377	0.378

Table 10 – Similarity In Other Variables and Number of Children

This table examines how diversity in other demographic variables is associated with different numbers of children. For each demographic variable, I take as the independent variable the fraction of residents in the local area who share the same value of the trait as the woman. Local area is taken as county, then city if county is missing, then detailed metro area if both city and county are missing. Panel A examines education, income and age. Education is a dummy variable for the highest level of schooling (e.g., high school, college, graduate degree). Income is deciles of income across the US in the year in question. Age is the fraction of the population that is between two years younger and ten years older than the woman. Panel B examines country of birth and citizenship. Country of birth is a dummy for whether the person was born in the US, and citizenship is a dummy for whether the person is a US citizen. Panel C includes these variables together, and also computes the marginal effect of a one-standard deviation unconditional change in each of the variables for each specification. All other control variables are defined in Table 2. Coefficients are in the top row, and t-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

	Panel A - Education, Income, Age											
Education Share	0.102*	0.176**	-0.010	-0.011								
	(1.97)	(2.72)	(-0.30)	(-0.33)								
Income Decile Share					0.659***	0.789***	0.733***	0.740***				
					(7.57)	(10.06)	(10.73)	(10.94)				
Age (-2,+10) Share									-0.771***	-0.513***	0.475***	0.504***
									(-4.98)	(-5.09)	(3.33)	(3.50)
State-Year FE	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y	N
Demographics FE	Y	N	N	N	Y	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Area Type * Population FE *Year	N	N	Y	N	N	N	Y	N	N	N	Y	N
Area Traits * Year	N	N	Y	N	N	N	Y	N	N	N	Y	N
Area * Year FE	N	N	N	Y	N	N	N	Y	N	N	N	Y
Observations	5,967,596	5,967,585	5,967,585	5,967,585	5,967,596	5,967,585	5,967,585	5,967,585	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.391	0.399	0.404	0.405	0.392	0.400	0.404	0.405	0.392	0.399	0.404	0.405

Panel B - Country of Birth, Citizenship								
US Born Share	0.152***	0.178***	-0.024	-0.025				
	(4.20)	(4.35)	(-1.17)	(-1.21)				
Citizenship Share					0.354***	0.405***	0.047	0.046
					(10.91)	(7.73)	(1.25)	(1.21)
State-Year FE	Y	Y	Y	N	Y	Y	Y	N
Demographics FE	Y	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	Y	Y	Y	N	Y	Y	Y
Area Type * Population FE *Year	N	N	Y	N	N	N	Y	N
Area Traits * (State, Year) FE	N	N	Y	N	N	N	Y	N
Area * Year FE	N	N	N	Y	N	N	N	Y
Observations	5,967,596	5,967,585	5,967,585	5,967,585	5,967,596	5,967,585	5,967,585	5,967,585
R-squared	0.391	0.399	0.404	0.405	0.392	0.400	0.404	0.405

Panel C - Other Variables in Combination								
	Coefficients				Effect of 1 s.d. unconditional change			
Race Share	0.194***	0.226***	0.188***	0.191***	0.063	0.073	0.061	0.062
	(9.98)	(7.30)	(5.96)	(5.99)				
Education Share	0.007	0.013	-0.090***	-0.092***	0.001	0.002	-0.013	-0.013
	(0.17)	(0.29)	(-3.09)	(-3.14)				
Income Decile Share	0.609***	0.678***	0.640***	0.645***	0.022	0.024	0.023	0.023
	(8.20)	(10.78)	(10.44)	(10.73)				
Age (within 10 years) Share	-0.605***	-0.296**	0.402***	0.429***	-0.032	-0.016	0.021	0.023
	(-3.39)	(-2.84)	(2.85)	(3.01)				
US Born Share	-0.401***	-0.345***	-0.166***	-0.167***	-0.100	-0.086	-0.041	-0.042
	(-8.48)	(-5.61)	(-5.60)	(-5.34)				
Citizenship Share	0.677***	0.625***	0.175***	0.177***	0.210	0.194	0.054	0.055
	(11.35)	(10.54)	(4.13)	(3.86)				
State-Year FE	Y	Y	Y	N	Y	Y	Y	N
Demographics FE	Y	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	Y	Y	Y	N	Y	Y	Y
Area Type * Population FE *Year	N	N	Y	N	N	N	Y	N
AreaTraits * (State, Year) FE	N	N	Y	N	N	N	Y	N
Area * Year FE	N	N	N	Y	N	N	N	Y
Observations	5,967,596	5,967,585	5,967,585	5,967,585				
R-squared	0.394	0.401	0.405	0.406				

Table 11 – Instrumenting Race Share with Immigration Shocks

This table examines the effect of instrumenting county-level race share with prior years' county-level immigration shocks, and the effect of this instrumented race share on birth rates. Immigration shocks are taken from Burchardi et al (2024), based on predicted ancestry in a given county in 1975, and national immigration from that ancestry group over a given five-year period. All other control variables are defined in Table 2. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable is number of children at time of survey						
County Race Share, Instrumented	0.206**	0.919**	2.417***	1.375***	1.381***	1.754**	0.243
With Immigration Shocks	(2.12)	(4.42)	(6.74)	(5.87)	(6.98)	(3.34)	(0.93)
Effect of 1 σ change (unconditional)	0.064	0.286	0.751	0.427	0.429	0.545	0.076
Effect of 1 σ change (conditional)	0.064	0.151	0.359	0.203	0.153	0.183	0.022
Race	N	Y	Y	N	N	N	N
State-Year FE	N	N	Y	Y	Y	Y	Y
Demographics FE	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	N	N	N	Y	Y	Y
Area Type	N	N	N	N	Y	N	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * Year FE	N	N	N	N	N	Y	Y
Area Type * Population FE *Year	N	N	N	N	N	Y	Y
Area FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	1,026,653	1,026,653	1,026,653	1,026,653	1,026,625	1,026,625	1,026,625
R-squared	-0.005	0.002	-0.054	-0.029	-0.016	-0.029	0.000

Table 12 – Time Series Effects of Diversity on Fertility

This table examines how time series changes in the average local level of diversity (measured across the US) are associated with changes in US birth rates. The dependent variable is the average across all respondents of race share, either measured using combined geography (i.e., county, then city if county is unavailable, then detailed metro area if both city and county are unavailable), city only, county only, or state. In Panel A, the dependent variable is the total fertility rate in the year after the diversity measure, taken from the St Louis Fed FRED database. Additional controls are included for the level of inflation, unemployment, and GDP growth. The first five columns use OLS regressions with data back to 1971. The last five use Newey-West regressions with five lags, and data from 2006. "Full Sample Change" is the change in the independent variable (i.e., TFR) over the period in question. "Predicted Change" is the regression coefficient multiplied by the change in the independent variable from the first sample year to the last. "Fraction of Change Explained" is the ratio of these numbers. In Panel B, the dependent variable is the unadjusted average number of children for all women in the survey year.

Panel A - Total Fertility Rate and Economic Controls

Race Share (Base Combined)	1.440***	2.569***				3.969***	3.920***			
	(3.86)	(5.10)				(10.29)	(9.11)			
Race Share (City Only)			2.470					7.902***		
			(1.50)					(10.92)		
Race Share (County Only)				2.090**					6.808***	
				(2.73)					(12.50)	
Race Share (State)					2.570***					4.433***
					(4.69)					(8.49)
Inflation		-0.042**	-0.020	-0.024	-0.043**		0.001	0.008	0.010	0.002
		(-2.70)	(-0.80)	(-1.10)	(-2.56)		(0.03)	(0.55)	(0.70)	(0.13)
GDP Growth		-0.011	-0.011	-0.008	-0.005		0.005	0.008	0.019**	0.012
		(-0.74)	(-0.54)	(-0.44)	(-0.30)		(0.55)	(0.74)	(2.86)	(1.39)
Unemployment		0.329	0.152	0.094	0.748		0.598*	0.479	0.203	0.902**
		(0.22)	(0.07)	(0.04)	(0.48)		(1.93)	(1.11)	(0.51)	(2.90)
First Year	1971	1971	1981	1971	1971	2006	2006	2006	2006	2006
Method	OLS	OLS	OLS	OLS	OLS	NW	NW	NW	NW	NW
Full Sample Change	0.602	0.602	0.148	0.602	0.602	0.444	0.444	0.444	0.444	0.444
Predicted Change	0.393	0.702	0.364	0.513	0.704	0.426	0.421	0.462	0.512	0.424
Fraction of Change Explained	0.653	1.166	2.458	0.852	1.170	0.960	0.948	1.040	1.153	0.954
Observations	21	21	19	20	21	16	16	16	16	16
R-squared	0.439	0.643	0.159	0.381	0.605	0.886	0.892	0.865	0.924	0.880

Panel B - Number of Children, Long Sample								
Race Share (Base Combined)	0.853***				0.644**			
	(6.51)				(2.32)			
Race Share (City Only)		0.630***				0.478**		
		(6.25)				(2.62)		
Race Share (County Only)			2.021***				1.767**	
			(8.84)				(3.62)	
Race Share (State)				0.791***				0.597**
				(6.31)				(2.30)
First Year	1850	1850	1950	1850	1850	1850	1850	1850
Years Included	All	All	All	All	Decades	Decades	Decades	Decades
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Full Sample Change	0.496	0.496	0.018	0.496	0.517	0.517	0.039	0.517
Predicted Change	0.426	0.349	0.692	0.415	0.314	0.264	0.583	0.304
Fraction of Change Explained	0.858	0.704	38.916	0.836	0.607	0.511	15.053	0.588
Observations	32	30	22	32	16	15	7	16
R-squared	0.585	0.583	0.796	0.571	0.278	0.346	0.724	0.274

Table 13 – Race Share, Annual Birth Probabilities, and Implied Total Fertility Rate Changes

This table estimates the relationship between annual chances of giving birth and the race share in the woman's local area. Woman-by-year observations are included for women aged 18-40, where the dependent variable is a dummy equal to 100 if the woman gave birth that year, and zero otherwise. These are created based on the number and ages of her children, if any, at the time of survey. Area-level information, including race share and area-level controls, are matched up to the year before the woman gave birth (approximating the level at the time of conception). Observations are only included if area information is known for that particular year. Other control variables are from levels at the (later) time of survey. Controls, including fixed effects are the same as in Table 2, except that "year" interactions in fixed effects are now for the year of birth, not the year of the survey. Several versions of marginal effects are reported. I estimate implied effects on Total Fertility Rate (TFR) by taking the regression coefficients (which measure annual birth probability), and multiplying them by 23 (to turn the annual probabilities for women age 18-40 into a total expected number at age 40), then multiply this by either the unconditional standard deviation of race share, or the conditional standard deviation (after having removed the control variables in the relevant regression). Next, I use the time series information from Table 11 to estimate how the large the implied change in time series TFR is from the specification. I use the coefficient in the column, multiply by 23, and multiply by the change in average national race share between 1971 and 2021. I report both the implied change in TFR over the period, as well as this value as a fraction of the actual TFR change over the period. Finally, I do the same exercise for changes between 2006 and 2021. Standard errors are double clustered by year and state. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable is 100x a dummy for if the woman gave birth that year										
Race Share	0.348	2.085***	1.893***	1.489***	0.894***	1.471***	0.965***	0.736***	0.758***	1.058***	1.054***
	(1.14)	(5.49)	(5.60)	(4.88)	(5.08)	(5.43)	(5.47)	(4.03)	(4.12)	(6.43)	(6.39)
Effect of 1 σ change (unconditional) on TFR	0.025	0.150	0.137	0.107	0.065	0.106	0.070	0.053	0.055	0.076	0.076
Effect of 1 σ change (conditional) on TFR	0.025	0.082	0.070	0.055	0.033	0.044	0.028	0.021	0.021	0.025	0.025
Implied change in TFR, 1971-2021	0.022	0.131	0.119	0.094	0.056	0.092	0.061	0.046	0.048	0.066	0.066
Fraction of actual TFR change, 1971-2021	0.036	0.218	0.198	0.155	0.093	0.154	0.101	0.077	0.079	0.110	0.110
Implied change in TFR, 2006-2021	0.009	0.052	0.047	0.037	0.022	0.036	0.024	0.018	0.019	0.026	0.026
Fraction of actual TFR change, 2006-2021	0.019	0.116	0.105	0.083	0.050	0.082	0.054	0.041	0.042	0.059	0.059
Race	N	Y	Y	N	N	N	N	N	N	N	N
State	N	N	Y	Y	N	N	N	N	N	N	N
Year	N	N	Y	Y	N	N	N	N	N	N	N
State-Year FE	N	N	N	N	Y	Y	Y	Y	Y	Y	N
Demographics FE	N	N	N	Y	Y	N	N	N	N	N	N
Demographics*(State, Year) FE	N	N	N	N	N	Y	Y	Y	Y	Y	Y
Area Type	N	N	N	N	N	N	Y	N	N	N	N
Area Traits	N	N	N	N	N	N	Y	Y	N	N	N
Area Traits * Year FE	N	N	N	N	N	N	N	N	Y	Y	N
Area Type * Population FE	N	N	N	N	N	N	N	Y	N	N	N
Area Type * Population FE *Year	N	N	N	N	N	N	N	N	Y	Y	N
Area FE	N	N	N	N	N	N	N	N	N	Y	N
Area * Year FE	N	N	N	N	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980	1980	1980	1980	1980
Observations	24,856,104	24,856,104	24,856,104	24,580,809	24,580,809	24,580,797	24,515,680	24,515,680	24,515,680	24,515,680	24,580,783
R-squared	0.000	0.002	0.003	0.027	0.047	0.051	0.051	0.051	0.051	0.051	0.052

Appendix

1. Data

I describe additional details of the data used in the sample

U.S. Data – Main Sample

U.S. census data sources are taken from the IPUMS default samples for each year, namely:

1% sample from 1850, 1860, 1870, 1880, 1900, 1910, 1920, 1930, 1940, 1950

(1890 and 1960 samples are missing due to lacking geographic information)

1% metro fm1 sample from 1970

1% metro sample from 1980 and 1990

1% sample from 2000

10% sample from 2010

ACS surveys from 2003-2021 (earlier ACS vintages lack necessary geographic information)

City information is available in 1850, 1860, 1870, 1880, 1900, 1910, 1920, 1930, 1940, 1950, 1980, 1990, 2000, and 2005-2021

County information is available in 1950, 1970, 1980, 1990, 2000, and 2005-2021

Detailed metro area information is available in 1850 1860 1870, 1880, 1900, 1910, 1920, 1930, 1940, 1950, 1970, 1980, 1990, 2000, 2003, and 2005-2011

Construction of Birth Panel for Table 12

The panel of birth-year observations is constructed similarly to Nickerson and Solomon (2024). I exclude group homes, households with inmates or children-in-law, and households with no adult woman present (as households with only an adult male and children will lack information on the mother's age). If there are multiple women over 18 years of age in a household, I split the women into separate observations, assigning children to the corresponding mother.

I use information from survey year *t* for the age of the woman and any children, and infer ages and birth events for years from *t*-1 back to when the woman was 18 years old. Like with the main data, I am only able to observe children still present in the household at the time of the survey, so risk undercounting births for older women whose children may have left the house.

For demographic variables, because I only observe these at the time of survey, these are merged to birth observations in all prior years. For area-level measures, including race share, I merge birth observations to the area information in the prior year (so as to approximately map to the values at the time of conception). In the main tests I only include observations for years with a matched value of area information, although in untabulated results, similar answers are obtained if all birth years are matched to the next closest year's value for area values, or if area values are matched to the year of birth itself (instead of the year before). Also like in the main panel, the implicit assumption is that the woman has not moved areas between the survey year and the year of the birth decision. If women are moving, this assumption will introduce measurement error and likely attenuate the results, biasing against finding an effect.

International Data

Table A1 shows data for the number of samples, observations, control variables and racial classifications for each of the international countries used in Table 6.

2. Alternative Specifications for Main Result

In this section, I explore a number of variations on the main specifications. Table A2 Panel A constructs versions of the *RaceShare* variable using alternative definitions of race. These are i) omitting the Hispanic/Latino category, ii) using detailed race (instead of broad race) and omitting Hispanic/Latino, iii) using broad race and including Hispanic/Latino as an interaction rather than a separate category, iv) using detailed race and including Hispanic/Latino as an interaction, v) using ancestry share, instead of any race classification, and vi) using the base definition over the whole population, including those under eighteen. For each variable, I include *Area*Year* and *Demographics*(State, Year)* controls (corresponding to Table 2 Panel A column 11). The effects are positive and significant at the 1% level in all cases.

Panel B constructs the *RaceShare* variable at different geographical levels. As geographic information varies across census years, limiting the analysis to only one type of geography alters the range of years included. To ensure that the controls remain comparable, in columns 1-4 I include *State*Year* and *Demographics*(State, Year)* (as other controls require area level information, which is not available for all specifications). Recall that in the base case, levels are constructed sequentially based on availability, so that (for instance) county is only used if city information is missing. For the first three measures, I use i) city, ii) county, and iii) detailed metro area, for all observations in each respective category. Next, iv) I reverse the priority order of city and county, so using county first, then city, and finally detailed metro area. In columns 1-4, all the relationships are positive and significant. I also v) use state level measures in columns 5 and 6, thus including all observations from the state, even those with missing information on any finer geography. For these two specifications, I omit *State*Year* controls, as these would map to within-area versions of the variable (and thus not be comparable to the earlier columns). State level measures are only significant at the 10% level, however, in column 5. This suggests that it is more local geography that drives these effects. Consistent with this notion, in column 6 I add both the baseline *RaceShare* variable and the state level version in the same regression. The baseline local version is highly significant while the state level metric exhibits a somewhat negative effect.

Panel C explores different levels of weighting. The baseline regressions weight every response equally, which necessarily draws more observations both from larger population areas, and from recent years. In column 1, I weight every *Area*Year* combination equally, regardless of the number of respondents. In column 2, I weight every year equally. In column 3, I weight each year equally, but also weight each observation in that year according to the census household weights. In columns 4-6, I apply census household weights within the area when constructing the *RaceShare* variable, and apply the same observation-level weighting choices as before. The results are positive and highly significant in all specifications, which include *Demographics*(State,Year)* and *Area*Year*.

Panel D explores the effect of women's ages. Different concerns can arise about whether data limitations can drive the result. The first is children leaving home. Because the census only counts children living in the same household, if a woman has children at age 18, and is 40 at the time of the survey, her children may have left home, and thus not be counted. While age, age-by-year and age-by-state fixed effects control for average levels of this effect, race share may be correlated with such data omissions in other ways. To test this, in columns 1, 2 and 3, I limit the sample to women 35 and under, 30 and under, and 25 and under, where the chances of such missing children becomes small. 35 is sufficient to exclude most such cases, as a woman who gave birth at 18 would have a child who was still only 17 at the time of survey, and thus probably still living at home. The coefficient here is 0.147 with a *t*-statistic of 5.52, compared to the equivalent in Table 2 Panel A of 0.190 with a *t*-statistic of 5.77. This suggests that even in a sample with few missing children, the effect is similar, but slightly smaller. Importantly, for interpreting this decline, it is notable that the coefficient continues to decline in the 30 and under sample, and the 25 and under sample (though the coefficient is positive and significant throughout). This suggests that the declines are not primarily due to children leaving home, because in these latter samples this is already not a significant issue. Rather, the more likely interpretation is that at young ages, women in general have fewer children living at home overall, so almost mechanically *RaceShare* cannot reduce their number of children by as much, as many women will already be at zero children.

The second possibility is that the method of measuring children at survey time may conflate delayed childbearing with having fewer children. If a woman has children later, there will be fewer observations for that child to count, even if the total number of children is the same. To test this possibility, I analyze a subset of women aged 36 to 40, who are likely to have completed their childbearing. Running separate regressions for each age in this range yields consistently large and significant coefficients (between 0.314 and 0.337). This suggests that in a sample where women have likely had most of their children, the effects are even more pronounced. Additionally, I control for potential delayed births by converting the surveys into a panel of birth-year outcomes, as shown in Table 13. These panel data analyses yield similar results, further supporting my findings.

3. Effect of Correlations Between Race and *RaceShare*

One possible concern is that the higher coefficient on *RaceShare* after controlling for race (in Table 2 Panel A columns 1 and 2) is due to the correlation between *RaceShare* and race itself. Because race is a categorical variable, I report correlations of *RaceShare* with individual race dummies. The correlations are 0.829 with white, -0.382 with black, -0.102 with American Indian, -0.198 with Chinese, -0.088 with Japanese, -0.318 with other Asian or Pacific Islander, -0.083 with other, -0.212 with two races, -0.076 with three or more races, and -0.378 with Hispanic.

It is possible that these correlations may be driving the high significance when both variables are included in the same regression, such as due to multicollinearity. To test this possibility, I create a placebo version of *RaceShare* that preserves the same distributional properties and correlation with race, but which lacks the actual effects on the woman in question. If the correlation structure is driving the results, then keeping the correlation structure but severing the relationship between *RaceShare* and the particular woman should show the same effects. If the placebo version lacks these effects, it suggests that the important variation comes from the specific matching of *RaceShare* to each woman, and not the correlation structure.

To test this, I randomize the values of *RaceShare* within each race category. This produces a placebo version of *RaceShare*, where the particular value assigned to a given woman is another draw (without replacement) from the pool of women of her race. In other words, if the correlation structure between race and *RaceShare* is producing the result (or indeed any mechanical aspect of the *RaceShare* distribution), these placebo versions have exactly the same properties and should produce the same results as Table 2. I run 1000 simulations, and perform the same regressions as Table 2 Panel A columns 1 and 2.

In the simulated versions of column 1, all 1000 simulations show negative effects of *RaceShare*, with a mean coefficient of -0.0455, the smallest value being -0.0478 and the largest being -0.0430. This confirms that, absent a true economic effect of *RaceShare* (because it is randomized within race), the mechanical effect is negative due to the correlation with race (that is, whites have a high *RaceShare* generally, and a low birth rate generally) By contrast, the true value in Table 2 Panel A column 1 is 0.156 and significant, consistent with a positive true effect in addition to the negative mechanical effect of the correlation with race.

More importantly, in the simulated version of column 2 where both race and *RaceShare* are included together (with no other controls), none of the simulations remotely produce the results of Table 2. The mean simulated coefficient is extremely close to zero, at -0.0000571. This shows very tightly that, on average, the correlation of race with simulated versions of *RaceShare* does not induce an average effect for simulated *RaceShare* once race itself is controlled for. Second, the range of the simulated coefficients is tiny compared with the true value. Out of 1000 simulations, the largest coefficient is 0.00889, and the smallest is -0.00790. This is compared with the true value of 0.707 in Table 2 Panel A column 2.

These results show strongly that the correlation structure between race and *RaceShare* is not an important driver of the results in Table 2, nor are any other mechanical aspects of the *RaceShare* distribution. When the correlation structure is preserved, but the true diversity effect is severed by randomization within race, the placebo effect of *RaceShare* has an average effect of zero, and a very small range of values, with the true value being almost 100 times larger than the largest simulated value from 1000 trials. This suggests strongly that the results are not due to multicollinearity, or any other mechanical distributional aspects of the relationship between race and *RaceShare.*

4. Instrumental Variables First Stage and Diagnostic Tests

Finally, in Table A3 I report the results of the first stage regressions for the instrumental variables regressions. I regress county-level race share on immigration shocks, keeping the same fixed effects and controls as in the main instrumental variables regressions. I also report weak instruments tests, using both the Cragg-Donald and Kleibergen-Paap Wald F-statistics, along with the Stock and Yogo critical values.

These results show a statistically significant and economically important effect of immigration shocks on race share. These are seen in the large *t*-statistics when clustered by state and year (between -4.05 and -16.38), and in the fact that the weak instruments tests exceed the critical values in all specifications. These results support the validity of the basic assumption behind the IV specification – that immigration shocks are a strong predictor of racial diversity.

Table A1 – Summary Statistics and Control Availability for International Data

Country	# Obs.	Mean # Children	# Samples	# Course Geography Units	# Fine Geography Units	Control Variables	Races	Number	Pct
Brazil	22,877,029	1.646	6	25	2,040	Age, Education,	White	10,232,595	54.92
						Employment,	Black	1,202,607	6.46
						Marital Status,	Indigenous	40,650	0.22
						Race, Urban	Asian	126,174	0.68
							Brown	7,028,211	37.72
Colombia	2,215,042	1.542	4	22	438	Age, Education,	White	561,681	82.77
						Employment,	Black	74,113	10.92
						Marital Status,	Indigenous	41,884	6.17
						Race, Urban	Other	889	0.13
Costa Rica	231,878	1.499	4	7	55	Age, Education,	White	138,357	93
						Employment,	Black	2,232	1.5
						Marital Status,	Indigenous	1,108	0.74
						Race, Urban	Asian	156	0.1
							Chinese	154	0.1
							Mulatto	6,015	4.04
							Other	755	0.51
Cuba	383,755	0.970	2	14	137	Age, Education,	White	242,150	63.1
						Employment,	Black	35,451	9.24
						Marital Status,	Mixed Race	106,154	27.66
						Race			
Ecuador	909,119	1.601	5	14	79	Age, Education,	White	39,427	8
						Employment,	Black	7,513	1.52
						Marital Status,	Afro-Ecuadorian	12,233	2.48
						Race, Urban	Indigenous	30,938	6.28
							Mestizo	370,966	75.29
							Mulatto	11,909	2.42
							Other	1,753	0.36
							Montubio	17,969	3.65
El Salvador	201,637	1.499	2	14	103	Age, Education,	White	14,437	13.32
						Employment,	Black	117	0.11
						Marital Status,	Indigenous	261	0.24
						Race, Urban	Mestizo	92,930	85.75
							Other	623	0.57
Jamaica	121,582	1.404	3	14	N/A	Age, Education,	White	229	0.2
						Employment,	Black	103,095	88.49
						Marital Status,	Chinese	213	0.18
						Race, Urban	Indian	1,559	1.34
							Other Asian	13	0.01
							Mixed Race	11,304	9.7
							Other	98	0.08

Country	# Obs.	Mean # Children	# Samples	# Course Geography Units	# Fine Geography Units	Control Variables	Races	Number	Pct
Mozambique	651,821	1.932	2	11	143	Age, Education,	White	457	0.07
						Employment,	Black	633,924	99.35
						Marital Status,	Indian	489	0.08
						Race, Urban	Pakistani	57	0.01
							Mixed Race	3,037	0.48
							Other	135	0.02
South Africa	3,141,423	1.042	5	5	19	Age, Education,	White	155,624	6.39
						Employment,	Black african	2,014,603	82.71
						Marital Status,	Asian	53,757	2.21
						Race, Urban	Coloured	208,378	8.56
							Other	3,288	0.13
United Kingdom	92,397	1.020	1	11	N/A	Age,	White	86,347	93.45
						Employment,	Black African	499	0.54
						Marital Status,	Black Caribbean	1,084	1.17
						Race	Other Black	350	0.38
							Chinese	388	0.42
							Indian	1,673	1.81
							Pakistani	820	0.89
							Bangladeshi	197	0.21
							Other Asian	504	0.55
							Other	535	0.58
Uruguay	282,446	1.229	6	19	67	Age, Education,	White	80,648	88.89
						Employment,	Black	3,433	3.78
						Marital Status,	Indigenous	1,529	1.69
						Race, Urban	Asian	183	0.2
							Mestizo	917	1.01
							Two or More Races	3,940	4.34
							Other	82	0.09
Zimbabwe	123,039	1.400	1	10	88	Age, Education,	White	145	0.12
						Employment,	Black	122,537	99.67
						Marital Status,	Asian	84	0.07
						Race, Urban	Mixed Race	170	0.14
							Other	8	0.01

Table A2 – Alternative Constructions of Diversity Variable

This table presents alternative versions of the main regressions in Table 2. Panel A considers alternative definitions of race. This includes i) omitting the Hispanic/Latino category ii) detailed race measures, but omitting the Hispanic/Latino category iii) race interacted with Hispanic/Latino, iv) detailed race measures interacted with Hispanic/Latino v) using ancestry instead of race and ethnicity, and vi) using the baseline measure (broad race plus a Hispanic category) for the whole population, including children under 18. Panel B varies the geographic region race share is measured at, including i) city only, ii) county only, iii) metro area only, iv) State, and v) a combined measure in the different order, namely city first, then county, then metro area. Panel C explores different weighting schemes, including i) weighting each area by year equally, ii) weighting each year equally, iii) using census household weights, and iv) using census household weights when constructing the race share variable. Panel D examines the effect when limiting the sample to different subsets of women's ages, namely 35 and under, 30 and under, 25 and under, and each year separately from 36 to 40. Standard errors are double clustered by year and state. Coefficients are in the top row, and t-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

Panel A - Different Race Definitions						
Race Share (No Hispanic Category)	0.174***					
	(7.29)					
Race Share (Detailed Race, No Hispanic Category)		0.179***				
		(7.73)				
Race Share (Hispanic Interaction)			0.201***			
			(6.02)			
Race Share (Detailed Race, Hispanic Interaction)				0.215***		
				(6.67)		
Ancestry Share					0.303***	
					(5.92)	
Race Share (Including Under 18 Population)						0.190***
						(5.77)
Demographics*(State, Year)	Y	Y	Y	Y	Y	Y
Area * Year	Y	Y	Y	Y	Y	Y
Observations	5,925,628	5,924,422	5,925,610	5,920,475	5,130,778	5,925,627
R-squared	0.405	0.407	0.408	0.411	0.417	0.407

Panel B - Different Region Measures						
Race Share (City)	0.205***					
	(5.99)					
Race Share (County)		0.302***				
		(7.66)				
Race Share (Metro Area)			0.175***			
			(3.28)			
Race Share (City, then County, then Metro)				0.286***		
				(6.54)		
Race Share (State)					0.151*	-0.236**
					(1.86)	(-2.42)
Race Share (Baseline - County, then City, then Metro)						0.215***
						(6.43)
State*Year	Y	Y	Y	Y	N	N
Demographics*(State, Year)	Y	Y	Y	Y	Y	Y
Observations	1,531,597	5,008,007	3,127,919	5,967,585	9,267,466	1,531,597
R-squared	0.393	0.401	0.394	0.400	0.392	0.392

Panel C - Weighting						
Race Share (Baseline - Unweighted)	0.210***	0.197***	0.190***			
	(6.47)	(5.87)	(6.22)			
Race Share (HH Weighted)				0.208***	0.199***	0.193***
				(6.88)	(6.17)	(6.55)
Sample Weighting	Area*Year (Indiv)	Year (Indiv)	Year (HH Weight)	Area*Year (Indiv)	Year (Indiv)	Year (HH Weight)
Demographics*(State, Year) FE	Y	Y	Y	Y	Y	Y
Area * Year FE	Y	Y	Y	Y	Y	Y
Observations	5,967,585	5,967,585	5,959,683	5,967,585	5,967,585	5,959,683
R-squared	0.402	0.406	0.393	0.402	0.406	0.393

Panel D - Female Age								
	Dependent variable is number of children at time of survey							
Female Age	<=35	<=30	<=25	40	39	38	37	36
Race Share	0.147***	0.090***	0.056***	0.322***	0.327***	0.337***	0.336***	0.314***
	(5.52)	(5.65)	(5.66)	(4.85)	(4.74)	(4.99)	(5.52)	(5.59)
Demographics* (State, Year) FE	Y	Y	Y	Y	Y	Y	Y	Y
Area * Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Earliest Year	1980	1980	1980	1980	1980	1980	1980	1980
Observations	4,616,993	3,325,686	2,040,585	272,930	258,290	258,868	258,197	259,752
R-squared	0.411	0.377	0.315	0.233	0.245	0.253	0.262	0.272

Table A3 – First Stage Regressions for Instrumental Variables

This table presents the first stage regressions for the instrumental variable regressions. The dependent variable is county-level race share. The independent variable is immigration shocks, which are taken from Burchardi et al (2024), based on predicted ancestry in a given county in 1975, and national immigration from that ancestry group over a given five-year period. All other control variables are defined in Table 2. Weak identification tests and critical values are reported underneath. Coefficients are in the top row, and *t*-statistics are below in parentheses, with *, ** and *** indicating statistical significance at the 10%, 5% and 1% level respectively.

	Dependent variable is County Level Race Share						
Immigration Shocks	-13.078***	-6.541***	-5.203***	-4.908***	-4.455***	-2.473**	-2.445**
	(-8.47)	(-16.38)	(-4.75)	(-4.85)	(-5.93)	(-4.05)	(-4.14)
Weak Identification - Cragg-Donald Wald F-statistic	6.5*10^4	5.6*10^4	2.2*10^4	2.0*10^4	2.5*10^4	6393	1950
Weak Identification - Kleibergen-Paap Wald F-statistic	71.81	268.17	22.52	23.56	40.14	16.41	17.10
Stock-Yogo weak ID test critical values: 10% maximal IV size	16.38	16.38	16.38	16.38	16.38	16.38	16.38
Race	N	Y	Y	N	N	N	N
State-Year FE	N	N	Y	Y	Y	Y	Y
Demographics FE	N	N	N	Y	N	N	N
Demographics*(State, Year) FE	N	N	N	N	Y	Y	Y
Area Type	N	N	N	N	Y	N	N
Area Traits	N	N	N	N	Y	N	N
Area Traits * Year FE	N	N	N	N	N	Y	Y
Area Type * Population FE *Year	N	N	N	N	N	Y	Y
Area FE	N	N	N	N	N	N	Y
Earliest Year	1850	1850	1850	1980	1980	1980	1980
Observations	1,026,653	1,026,653	1,026,653	1,026,653	1,026,625	1,026,625	1,026,625
R-squared	0.060	0.736	0.776	0.778	0.877	0.888	0.915