The Effect of Dating Markets on Maternal and Neonatal Health

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Abstract

This paper provides causal evidence that the sex composition of dating markets affects maternal and neonatal health. Using a novel instrument that leverages randomness in sex at birth to vary the availability of male partners, I find that a more favorable dating market for women decreases non-marital but increases marital fertility, lowers rates of chlamydia and hypertension among mothers, and decreases the incidence of low APGAR scores and a composite index of adverse birth outcomes. These effects appear to operate primarily through changes in relationship dynamics and selection into motherhood. Connecting this to inequalities, racial disparities in partner availability can explain 5–10% of the Black–White pregnancy health gap.

1 Introduction

Reproductive health outcomes depend not only on access to medical care but also on the social context in which families are formed and resources allocated. Economic theory emphasizes that partner availability—the local sex ratio—shapes marriage, fertility, and women's bargaining power [Becker, 1973, Chiappori, 1992]. These dynamics are especially salient in the United States, where there are only 89 Black men for every 100 Black women of childbearing age,

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compared with 102 White men per 100 White women, with much of the imbalance reflecting incarceration. Against this backdrop, Black infants are twice as likely to die as White infants, and Black mothers face markedly higher morbidity. Could these disparities be related? Namely, do imbalances in sex composition causally affect dating markets and spill over to maternal and infant health? Although economic theory links sex ratios to reproductive outcomes, and policies like incarceration, immigration, and fertility restrictions shift these ratios, credible causal evidence remains limited.

In this paper, I study how the relative availability of men influences fertility, maternal health, and neonatal outcomes. A longstanding challenge in this literature is the endogeneity of local sex ratios. To address this problem, I develop an instrumental variables strategy that exploits random fluctuations in male births. I instrument local, adult sex ratios with the cohort sex ratios at birth, exploiting the near-random 51% probability of being born male or female and low spatial mobility. In smaller dating markets, where random fluctuations are not averaged out, this randomness often creates imbalanced sex ratios, which persist into adulthood. To study the consequences of this variation, I link sex composition at birth to maternal and neonatal health outcomes using U.S. natality microdata, covering approximately 7 million births between 2011 and 2019.

I present evidence suggesting that the variation in sex ratios at birth is plausibly exogenous and likely satisfies the exclusion restriction required for identification. First, maternal health, socioeconomic status, and environmental conditions during pregnancy explain at most a negligible share of the observed variation in sex ratios at birth. There is also no indication that selective abortion, fertility stopping rules, or migration account for the patterns I document. Instead, the distribution of sex ratios at birth closely matches what would be expected if each birth were an independent Bernoulli draw[1]. Finally, a placebo test shows that sex ratios at birth predict adult sex ratios only within their own cohort, not across older or younger cohorts in the same county–race group. This indicates that the variation reflects

[1]The probability of male birth is approximately 51%.

cohort-specific random fluctuations rather than systematic local factors that would affect all cohorts alike. Beyond exogeneity, identification relies on the assumption that sex ratios at birth affect maternal and infant health only through their influence on the composition of the local dating market. I assess the exclusion restriction by examining a range of alternative channels and find no evidence contrary to this identifying assumption.

Sex composition has meaningful effects on birth rates, marriage, and health. A one-standard-deviation increase in the proportion of men leaves the overall birth rate unchanged but raises marital births by 9% and lowers nonmarital births by 11% relative to their means. It also shifts marriage-market outcomes: the share of married women rises by 2.85 p.p. in the general population and 2.7 p.p. among mothers, and the share of births with unknown fathers falls by 1.4 p.p., reflecting positive selection into motherhood. I find no evidence that women partner with men of higher observable quality relative to their own. Maternal health improves when women have a more favorable position in the dating market: the prevalence of chlamydia among women giving birth falls by 13% of the mean, and hypertension by 17%. Infants born in markets with a stronger female position are also healthier—the likelihood of a low APGAR score falls by 8.3% relative to its mean. Other infant outcomes—birth weight, gestational age, and the need for assisted ventilation—move in the expected direction but fall short of conventional statistical significance.

The instrumental variable results raise the question of what mechanisms link sex ratios at birth to maternal and infant health. The most direct hypothesis is that sex ratios shape the local dating market: when women face a more favorable balance of potential partners, they are more likely to secure stable relationships, reducing exposure to risky sexual behavior in the short run and improving partnership quality, and control over fertility and household resources in the long run. Other channels, though less directly tied to dating markets, are also possible. Peer gender composition may affect attitudes, violence, or schooling; skewed sex ratios may shift public investment in education or healthcare; and imbalances at birth may trigger selective migration or influence parental behaviors.

Turning to the data, the evidence is most consistent with the dating-market mechanism. In markets more favorable to women, marriage rates are higher, nonmarital births are less common, and sexually transmitted infections are less prevalent, consistent with greater stability of romantic relationships. I also find evidence of positive selection into motherhood: women who give birth in these markets are more likely to be more educated, to have more educated partners, and to exhibit better pre-pregnancy health. By contrast, I find no evidence for alternative channels. Balance checks show that sex ratios at birth are not associated with schooling resources, access to medical care, educational attainment, or arrest rates. Similarly, parental divorce and migration do not appear to play a significant role. Although other factors cannot be fully excluded, the dating market emerges as a salient mechanism linking sex ratios to maternal and infant health.

In the United States, Black and White women face markedly different sex ratios and reproductive health outcomes. Using my estimates and illustrative simulations that equalize sex ratios between Black and White women, I find that sex ratio differences can account for 5–10 percent of the Black–White disparity in maternal and infant health. More broadly, the findings indicate that policies which alter sex composition—whether fertility restrictions in China [Ebenstein, 2010], mass incarceration in the United States [Pouget, 2017], or immigration policies in Taiwan [Ahn, 2021]—can have unintended consequences for reproductive and early-life health. In the case of incarceration, this points to an overlooked social cost: the reduced outside options faced by women in the dating market, with implications for both maternal and infant well-being. This is especially important because conditions at birth and in early childhood shape life chances across generations[2], making sex ratios a policy-sensitive driver of long-run inequality.

The primary contribution of this paper is to provide causal evidence on how the sex composition of dating markets influences fertility and pregnancy health. Building on Becker's foundational insight that a scarcity of women shifts relationship gains in their favor [Becker,

[2]As shown in, for example: Almond and Mazumder [2011], Schwandt [2018], Giuntella et al. [2022]

1973] and subsequent models linking sex ratios to bargaining power and household decisions [Grossbard-Shechtman, 1984, Chiappori, 1992], this study moves beyond a large empirical literature that has documented correlations between bargaining power, women's well-being, and child outcomes.[3] I show that more favorable sex ratios for women cause improvements in pregnancy health, with the dating market appearing to play a central role. This evidence contributes to the bargaining-power literature and extends prior work by examining a high-income setting where policy partially shapes dating market composition. In doing so, it also connects to the broader literature on the family, social, and environmental determinants of pregnancy outcomes.[4] The results also relate to the literature on fertility and family formation. Consistent with evidence from historical sex-ratio shocks [Abramitzky et al., 2011, Brainerd, 2016], greater male availability shifts births from non-marital to marital contexts. The findings also complement the literature on "marriageable men." In that framework, improvements in men's economic prospects raise fertility through income and household specialization channels [Schaller, 2016, Kearney and Wilson, 2018, Autor et al., 2019]. In contrast, the variation studied here holds incomes constant and isolates changes in partner availability, leading to compositional shifts in births rather than changes in fertility levels. Finally, recent research [Kearney et al., 2022, Kearney and Levine, 2025] documents a secular, cohort-level decline in fertility in the United States and other high-income countries driven by shifts in norms. Because this paper studies period-specific variation and finds no effect on overall birth rates, it does not speak to that aggregate decline. Instead, it highlights how dating markets shape selection into motherhood and the circumstances under which children are born.

This paper also contributes by introducing a novel identification strategy for studying sex ratios. Most prior work has relied on cross-sectional variation in local sex ratios or on

[3]For example, prior work has linked sex ratios to household outcomes [Lundberg et al., 1997, Chiappori et al., 2002], women's health and safety [Li and Wu, 2011, Armand et al., 2020, Rao, 1997, Panda and Agarwal, 2005, Stevenson and Wolfers, 2006, Calvi, 2020], and child health [Beegle et al., 2001, Thomas et al., 1999, Maitra, 2004].

[4]See, for example, [Rossin, 2011, McCrary and Royer, 2011, Currie and Schwandt, 2013, Farré and González, 2019, González and Trommlerová, 2022, Kennedy-Moulton et al., 2022].

historical shocks to address endogeneity concerns[5]. In contrast, I exploit natural variation in cohort-level sex ratios at birth, which generate plausibly exogenous differences in adult sex composition. While a related concurrent study [Goldman et al., 2025] draws on a similar variation to analyze marital sorting, my analysis focuses on how changes in dating market dynamics shape partnering behavior, fertility, and ultimately maternal and infant health outcomes. More broadly, this identification strategy is readily applicable to study a wide range of behaviors shaped by local sex composition, including migration, household consumption, or investments in children.

The remainder of the paper describes the conceptual framework and data, presents the empirical design and results, discusses mechanisms and policy implications, and concludes.

2 Conceptual Framework

While sex composition can affect health through many channels, I focus here on a central one: the dating market. Economic theory shows that the relative supply of potential partners shapes both the matching process and the division of surplus within unions [Becker, 1973, Chiappori, 1992]. Appendix Section D.1 illustrates this mechanism in a simple model. When women are relatively scarce, men compete more aggressively for partners, facilitating union formation and increasing the stability of relationships for women. These shifts in the dating market carry into the household, influencing partnership quality, resource allocation, and women's opportunities for motherhood.

The first pathway operates through the stability and quality of partnerships. A larger pool of potential partners allows women to be more selective, as experimental evidence confirms [Fisman et al., 2006, Fong, 2020], increasing the likelihood of securing a more committed and better-resourced partner. By pooling income, increasing material security, and offering emotional support, stable partnerships and marriage contribute to better maternal and infant

[5] See for instance: Chiappori et al. [2002], Cornwell and Cunningham [2008], Adimora et al. [2002], Kang and Pongou [2020], Angrist [2002], Lafortune [2013], Abramitzky et al. [2011], Brainerd [2016], Liu [2020], Alix-Garcia et al. [2022], Battistin et al. [2022]

outcomes [Buckles and Price, 2013, Hoynes et al., 2015, DeKlyen et al., 2006, Kennedy-Moulton et al., 2022]. In markets more favorable to women, they may also secure partners with advantageous characteristics—higher education, income, or health—that spill over to enhance women's own health and economic well-being [Skalická and Kunst, 2008, Fletcher, 2009, Jeon and Pohl, 2017, Du and Zaremba, 2024, Guo et al., 2020].

A second pathway operates through women's bargaining position within relationships. When potential outside options improve, women gain leverage over household decisions. This bargaining power can translate into higher spending on nutrition [Li and Wu, 2011], greater access to medical care, and more leisure [Angrist, 2002], while also reducing domestic violence and stress, both of which are strongly linked to adverse pregnancy outcomes [Rao, 1997, Panda and Agarwal, 2005, Currie et al., 2018]. Stronger female bargaining positions are also associated with greater partner fidelity and ability to enforce protection, lowering exposure to sexually transmitted infections that harm pregnancy and infant health [Cornwell and Cunningham, 2008, Kang and Pongou, 2020].

A third pathway operates through childbearing decisions. When dating markets are more favorable, women gain greater control over whether and when to become mothers. This shift can generate positive selection into motherhood, improving pregnancy outcomes and, in turn, infant health.

These mechanisms can also have long-term consequences for women's health. Sustained exposure to favorable dating markets may generate cumulative effects, as stronger competition among men for partners shapes health behaviors, fertility choices, family formation, and the resources available to women. Over time, these forces can improve health across a broad spectrum, leaving women in better condition by the time they reach pregnancy.

Taken together, these conceptual mechanisms point to dating markets as an important channel through which sex ratios shape maternal and infant health. I also examine alternative mechanisms in Section 4.6.

3 Sample Construction and Data

3.1 Defining Dating Markets and Computing Sex Composition

I measure the extent to which dating markets favor women by quantifying deviations from a balanced sex composition within a woman's dating market. My main explanatory variable is the proportion of the dating market that is male (*proportion male*), computed from the 2010 Census exact population counts.[6]

I define dating markets as the set of potential partners of the same race, within the same five-year age cohort, and living in the same county.[7] This definition reflects well-documented rigidities in partner selection: romantic pairing is highly assortative by race and age and remains strongly localized geographically [Bruch and Newman, 2019].[8] While this definition does not capture the entire market, it encompasses a segment substantial enough to alter outside options and influence decision-making for most participants. In the natality data, 42% of couples fall within this definition.[9]

My definition of the market follows prior work [Chiappori et al., 2002, Charles and Luoh, 2010], but I restrict the geographic scope to the county. This choice reflects the highly local nature of partner search: 70% of couples lived in the same county five years before marriage [Goldman et al., 2025], and the likelihood of forming a relationship declines sharply with distance. Geography remains central even in the era of online dating: proximity is the strongest predictor of messaging patterns on dating platforms [Bruch and Newman, 2019], and similar clustering appears in friendships [Bailey et al., 2018] and workplaces [Bayer et al., 2008]. These patterns reflect opportunity: short distances increase chances to meet, lower the

[6]While I use the proportion rather than the sex ratio, the two measures are injective, and I use the terms sex ratio and sex composition interchangeably. All results are robust to using either measure (see Table A14).

[7]This includes married individuals, as they can divorce or engage in extramarital relationships, thereby affecting outside options. I restrict attention to heterosexual partners, for whom sex ratios are meaningful.

[8]Educational attainment is an important dimension of assortative matching. However, because access to highly educated partners depends on one's relative position in the dating market, education is endogenous and thus not used to define markets.

[9]Figures A.2 and A.9 illustrate the share of couples formed within each type of market. Allowing for relationships that cross these boundaries would render my estimates conservative (Appendix B.10).

costs of repeated in-person contact, and provide support through overlapping social networks. They also reflect preference: survey shows that two-thirds of dating-app users restrict their search radius to 30 miles or less [Kirkham, 2019]. Consistent with this definition, placebo tests using neighboring counties' sex compositions show no spillovers on local marriage rates (Appendix Table A16).

Age is the second criterion used to define dating markets. Individuals must belong to the same five-year age cohort (15–19, 20–24, 25–29, 30–34 in 2010), following Census Summary cell definitions. Consistent with this choice, ACS data show that 80% of first marriages occur between partners within five years of age, and in the natality data, 40–50% of women give birth with a partner in the same five-year cohort. While large age gaps in relationships are comparatively rare, men are on average approximately three years older than their partners. Given these systematic age differences, the cohort-based market definition is necessarily a simplification, but it nonetheless captures meaningful variation across a broad set of potential partners; Appendix Section A.3 provides detailed evidence on observed age gaps and their measurement implications. The age homophily reflects both opportunity and preferences. On the opportunity side, social networks are heavily age-graded through schooling, higher education, and workplaces, concentrating available partner pools within narrow age bands. On the preference side, age proximity improves compatibility by synchronizing fertility potential, career trajectories, and retirement horizons, lowering the likelihood of mismatched expectations about family formation and long-run planning. Observational evidence from dating platforms confirms this pattern: initiation and reply rates decline sharply with age gaps [Bruch and Newman, 2019], and platform design reinforces this pattern by requiring users to set narrow age ranges, typically chosen to center near their own age [Rudder, 2010].

The final criterion is racial homophily. I use four racial groups: White, Black, Asian, and Native Americans[10]. This criterion is motivated by the rigidity of racial boundaries in

[10]At this level of granularity, the Census distinguishes only Hispanic and non-Hispanic Whites. Hence, the White group excludes Hispanics, while other groups may contain people of Hispanic origin. To remain consistent, I exclude White Hispanic mothers from the health data. Because older natality data do not record Hispanic origin, the instrument necessarily includes Hispanics; Table A13 shows that the results are virtually

partner selection. Only 10% of marriages in the US are interracial [Koh, 2025]. The same pattern appears in the natality data: over 90% of Black and White women have children with partners of the same race (see figure A.2 in the appendix). The figures are somewhat lower for Asian and Native women, but same-race unions remain predominant.[11]

This rigidity of racial boundaries in partner choice reflects both availability and preferences. On the availability side, residential segregation increases exposure to same-race partners, with similar clustering in schools and workplaces. Yet even when availability is held constant—as in speed-dating experiments—same-race matches remain substantially more likely. Fisman et al. [2008] show that this preference is driven largely by women's choices, and same-race matches are disproportionately associated with self-reported "shared interests," which may reflect shared traditions, family expectations, and cultural background [Kalmijn, 1998]. Consistent with this evidence, Goldman et al. [2025] find little interracial substitution in marriage markets, and my analysis likewise shows no evidence that individuals substitute across race or age when local markets are unfavorable (Appendix Section C.4).

Given the definition of the dating market, I compute the sex composition as the number of non-institutionalized men in county c, of race r, and in cohort a over the overall non-institutionalized population in the same c, r, a cell.

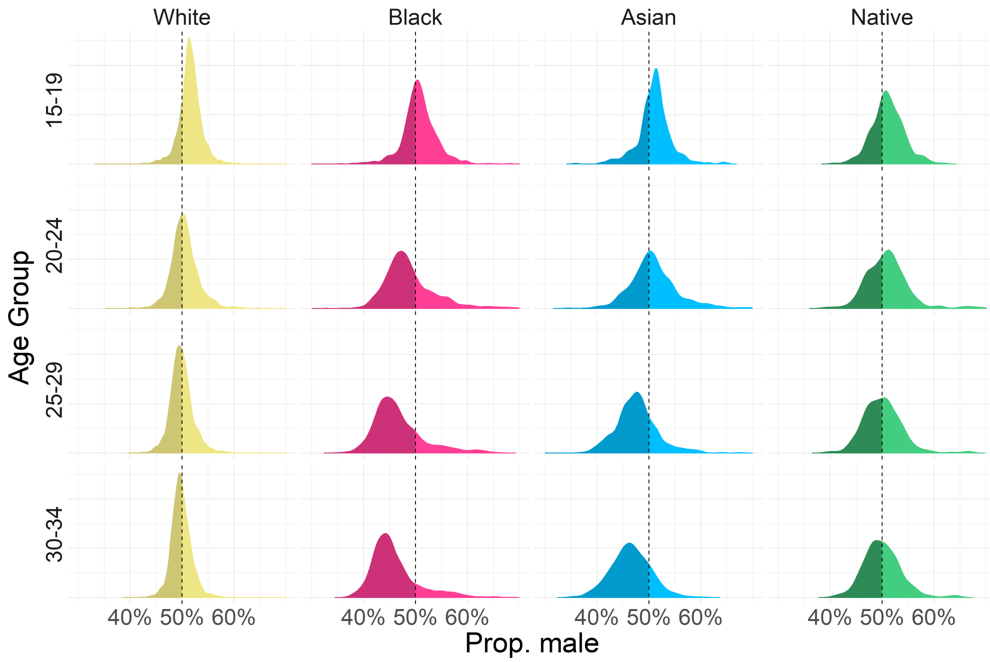
American women face markedly different partner availability depending on age, race, and location. Across markets, the proportion male varies substantially, with a standard deviation of 3.65 percentage points: the first quartile is 48.1% (92 men per 100 women) and the third quartile 51.7% (108 men per 100 women). A large share of this variation reflects racial differences. Figure I displays the distributions of proportion male across markets by race and cohort, revealing both cross-group disparities and within-group variation.

The most striking imbalance occurs among Black women: for those aged 30–34, men account for only 45% of the median dating market (82 men per 100 women), compared to

unchanged when Hispanics are included among Whites in outcomes and endogenous variable.

[11] The measure of dating market position captures more accurately the pool of potential partners in locations with fewer interracial unions. Consistently, the results are stronger in more racially segregated areas (Appendix Figure C.20).

Figure I: Density of Proportion Male in 2010 by Race and Cohort



Notes: Figure shows the empirical distribution of the sex composition. Each observation represents the proportion of men among agents on the dating market. The dashed line represents the balanced sex composition of 50%. Markets with fewer than 100 people are excluded. Hispanics are excluded due to the lack of relevant information in historical birth records used for the instrument.

near parity among White women of the same age. In a strictly monogamous setting, this implies that at least 18% of Black women in these markets would be unable to find a partner.

Incarceration alone accounts for nearly half of this imbalance. Around 10% of Black men in this age group are incarcerated, versus 2% of White men. Appendix A.4 presents a full decomposition of the racial gap in sex ratios, highlighting the role of incarceration alongside other contributing factors.

3.2 Health Outcomes

I measure fertility, maternal health, and neonatal outcomes using 2011–2019 U.S. natality microdata, which cover the universe of births and provide detailed information on parental characteristics, maternal health during pregnancy, and newborn health at delivery. Based on demographic and geographic identifiers, I assign each woman to a dating market and link her to the corresponding sex composition.[12]

[12] A limitation is that I use sex composition measured in the 2010 Census for births through 2019. However, local cohort sex ratios are highly persistent over time, as I show later.

The natality data show stark racial disparities in reproductive outcomes. Fertility is higher among Black women, yet their pregnancy outcomes are markedly worse: they are more likely to enter pregnancy with STIs, hypertension, or diabetes, and their infants are more often preterm, underweight, or low-APGAR, and twice as likely to die shortly after birth (Appendix Figures A.3a and A.3b). These disparities persist after accounting for socio-economic differences.

4 Sex Composition and Health: Empirical Framework

4.1 Basic approach

The empirical framework compares women exposed to dating markets with relatively many versus relatively few men. Under a strong assumption that variation in local sex ratios is exogenous conditional on covariates, one could estimate the treatment effect with:

$$y_{i,cra} = \beta PM_{cra} + \gamma X_i + \epsilon_i, \tag{1}$$

where $y_{i,cra}$ is an outcome for mother i residing in county c[13], of race r, and in cohort a. The key regressor PM_{cra} is the proportion male in her dating market, defined by county c, race r, and cohort a.

The large sample allows me to flexibly absorb confounding variation through covariates and fixed effects X_i. I control for cohort size in 2010 and include county-by-age-at-delivery, race-by-age-at-delivery, and race-by-individual-birth-year fixed effects.[14] These absorb county economic conditions, migration patterns, and broad racial differences that might otherwise generate spurious correlations between sex ratios and health. I further interact race with age and cohort to flexibly account for cross-racial differences in fertility timing and health. Because pregnancy outcomes vary non-linearly with maternal age, and cohort differences

[13] I use county of residence rather than county of birth occurrence, as residence more plausibly reflects the relevant dating market.

[14] "Age at delivery" refers to the mother's age at delivery; "individual birth year" captures each one-year maternal cohort, i.e. when mother was born.

may influence the timing of childbearing, I control for individual years of age.[15] Hence, I effectively compare women of the same race giving birth at the same age, while accounting for cohort-specific and county-specific factors.[16] After absorbing these controls, the residual variation in the proportion male is about 2 percentage points. Results are qualitatively unchanged when including alternative sets of fixed effects in the model.

4.2 Endogeneity Concerns

The parameter β captures the treatment effect only if, within race–age–county cells, variation in sex composition is orthogonal to other determinants of health. Even with rich controls, omitted variables may threaten validity. For instance, counties with high crime may both incarcerate more men—reducing the sex ratio—and expose women to violence, worsening pregnancy outcomes [Currie et al., 2018], which would bias β upward. Conversely, industries attracting male workers, such as mining, tend to be located in impoverished areas with poor health and high mortality [Cortes-Ramirez et al., 2018], biasing β downward. These factors could produce a correlation between sex composition and health outcomes even without the direct, causal impact of the sex ratio.

4.3 Instrumental Variables Approach

Hence, to isolate the exogenous variation in sex composition, I leverage randomness in the sex ratio at birth. The instrument for PM_{cra} is the proportion of male births of race r in county c in years when the cohort a was born. Denote it as PMB_{cra}.

As an illustration, consider White residents of Maricopa County, Arizona, aged 25–29 in 2010. Their instrument value is the of proportion male births among White births in Maricopa County between April 1980 and April 1985. I calculate the proportions using the natality microdata for 1975–1995. This dataset lets me calculate the number of boys and girls born in each county, race, and cohort. The standard deviation is 2.09 p.p., with first

[15] I do not control for variables such as parental education, since these are likely themselves influenced by sex ratios and would constitute bad controls.

[16] Mother's age at delivery is not itself affected by market imbalance (Table A29), and results remain stable when excluding it.

quartile 50.1% (100.4 boys per 100 girls) and third quartile 52.3% (109.6 boys per 100 girls). Appendix Figure A.4 shows the overall distribution, while Appendix Figure A.5 displays distributions by race and cohort.

Identification requires three conditions, each plausible in this setting: *relevance*—individuals tend to live near their childhood homes; *exogeneity*—sex at birth is largely random; and the *exclusion restriction*—sex ratios plausibly affect health primarily through the dating market.

4.4 Relevance

The first identifying assumption is supported by evidence on geographic persistence from *Opportunity Insights* data. Chetty et al. [2018] tracked cohorts born between 1978 and 1983 and documented where they lived in adulthood. Using commuting zones and census tracts as bounds, I estimate that 20–60% of adults remain in their childhood county (Appendix Figure A.6), with similar patterns across gender and race. Additionally, Sprung-Keyser et al. [2022] show that 60% of individuals aged 26 live within 10 miles of where they lived at the age of 16, and 80% live within 100 miles. Hence, one may expect a non-negligible amount of persistence in the sex composition of local cohorts over time.

4.5 Exogeneity

Although exogeneity cannot be directly verified, I aim to strengthen the credibility of this assumption through a series of simulations and empirical tests.

If sex at birth is truly random, each birth can be modeled as an independent Bernoulli trial with probability p of being male[17]. Exogeneity then requires that the empirical distribution of sex ratios at birth aligns with the distribution implied by this Bernoulli process. Simulations confirm this prediction: the empirical and simulated distributions are nearly identical, and their similarity cannot be rejected by a Kolmogorov-Smirnov test (Appendix section B.1). Moreover, sampling theory predicts that the variance of the sex ratio declines with cohort size, with a slope of -1 in the log(variance)–log(size) relationship. Appendix section B.2 shows that the observed data fit this prediction almost exactly. Together, these results indicate

[17] Empirically, this probability is approximately 0.51 among U.S. newborns.

that the empirical variance is consistent with sampling variation from a Bernoulli process, supporting the assumption of randomness at birth.

Figure II illustrates the empirical relationship between cohort size and variation in sex ratios at birth. In the largest markets the sex composition is close to balanced,[18] while smaller markets display much greater variation in the proportion of male births. This variation can be attributed to chance, where some cohorts happen to have more male or female births. Since these cohorts are small, they remain unbalanced. Ashlagi et al. [2017] show that even modest imbalances of this kind can meaningfully affect matching markets. The variation becomes negligible only in cohorts exceeding 5,000 births, which account for fewer than 20% of markets with more than 200 participants. I therefore exclude cohorts above this threshold from the main analysis,[19] as well as very small cohorts below 200 births, which tend to produce extreme values of the sex ratio and have few subsequent deliveries. Results are qualitatively robust to alternative thresholds (2,000 or 10,000; see Appendix Tables A11 and A12). These restrictions result in an IV analysis sample of roughly 7 million births residing in markets that satisfy the size restrictions.[20]

The IV sample covers 33% of U.S. births and 29% of individuals aged 15–34 during the study period (Table A2). Coverage varies across racial groups, ranging from 30% of births for Whites to 62% for Native Americans. The higher coverage for smaller groups reflects that these populations more often fall inside the IV cohort-size window even in larger counties. Restricting attention to county–race combinations with at least 200 individuals, the IV sample spans 84% of such combinations, with coverage exceeding 80% for all groups.[21]

Tables A3–A6 document how within-IV markets differ from the excluded sample on

[18] The sex ratio at birth is slightly skewed toward males.

[19] In over 80% of county–race pairs all four cohorts enter the sample. Raising the threshold reincorporates the excluded cohorts without altering results.

[20] The full universe consists of approximately 35 million U.S. births between 2011 and 2019. In most of the analysis, the sample is first restricted to non-Hispanic births, yielding approximately 23–24 million observations (the OLS sample). The market-size restrictions described here further reduce this sample to the main IV analysis sample of roughly 7 million births.

[21] Including all counties in the denominator mechanically lowers coverage, as many counties contain only a handful of individuals from smaller racial groups, for which a local partner market is not well-defined.

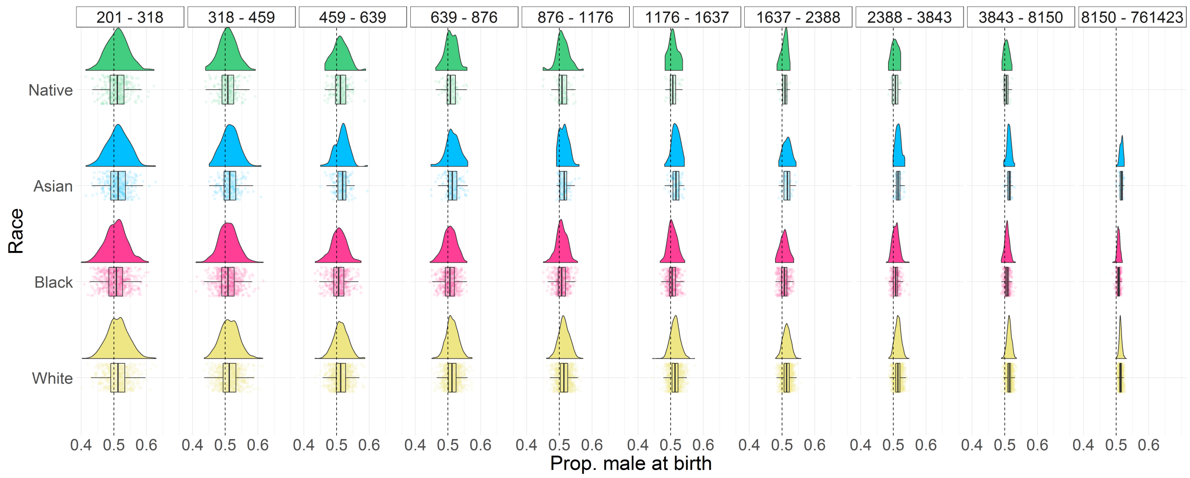


Figure II: Density of Proportion Male at Birth by Cohort Size

Notes: Figure shows the empirical distribution of the sex composition at birth. Distributions are divided by the deciles of the size of the cohort.

socio-demographic and birth characteristics. Relative to the outside-IV sample, the within-IV sample is systematically less urban (57% vs. 97%), lower-income (median household income $50,300 vs. $60,100), less educated (24% vs. 33% college share), and less served by healthcare providers (2.79 vs. 3.27 hospital beds per 1,000), with a smaller foreign-born share (7% vs. 15%). Births in the IV sample are to younger parents (mothers aged 28.2 vs. 29.1), with higher rates of maternal morbidity (e.g., hypertension 2.2% vs. 1.8%) and worse infant health outcomes (e.g., low APGAR 2.4% vs. 2.0%). Several of these characteristics are plausible moderators of the treatment relationship. For instance, higher levels of education may increase geographic mobility and reduce reliance on local partner markets, while variation in baseline morbidity may shape the scope for treatment effects. Importantly, the magnitude of these imbalances varies across groups. They are largest for Whites—whose observations in the IV sample are concentrated in smaller counties—and substantially smaller for minority populations, for whom a larger share of urban counties falls within the IV window.

These patterns carry direct implications for external validity. The design buys identification credibility at the cost of drawing identifying variation from a demographically and geographically specific subset of the U.S. population. The estimates are a LATE over compliers in the IV sample; they apply best to populations with characteristics similar to those in the sample: residents of small and mid-sized, less urban counties with lower income, lower educational attainment, and lower healthcare-provider density. In practice, this covers a substantial share of the non-urban White population, together with minority populations both in similar counties and in moderately large urban areas outside the largest metropolitan cores. By contrast, the estimates are less informative for populations that are underrepresented in the IV sample or less constrained by local sex composition, such as residents of the largest metropolitan areas, highly mobile individuals, and populations in semi-closed environments where partner availability is weakly tied to local demographic composition (e.g., college campuses or military bases).

Because credible identification rests on exogeneity, it is important to carefully evaluate threats to this assumption. One possibility is that sex ratios at birth might respond to socioeconomic or health conditions, which could also shape maternal health in the next generation. This concern is most commonly motivated by the Trivers–Willard hypothesis (TWH), which posits that favorable maternal conditions increase the likelihood of male offspring. Several studies document associations consistent with this mechanism—for example, a slightly higher probability of male birth for children of more educated or married mothers [Almond and Edlund, 2007], or temporary declines in sex ratios following extreme environmental shocks [Fukuda et al., 1998, Song, 2012]. Other studies, however, find mixed or opposite effects [Lee and Orsini, 2017, 2018], and a recent review [Thouzeau et al., 2022] emphasizes that parental education and income are among the least reliable predictors of sex at birth, with detectable shifts arising mostly after very large disruptive events.

I test this directly in my data (Appendix B.3). Maternal education is weakly positively associated with the probability of male birth, but the magnitude is far too small to explain observed variation: a ten-percentage-point increase in the share of high-school-educated mothers raises the proportion male by only 0.00012 (0.6% of a cross-market SD), and the joint R^2 from regressing sex at birth on education, marital status, and age is 0.000008. Maternal health, unemployment, and pollution show no systematic effect, and a placebo test finds no evidence that cohorts born in male-skewed markets reproduce at higher sex ratios.

While some correlations are consistent with TWH in sign, their magnitudes are too small to materially affect identification. This aligns with the broader evidence [Thouzeau et al., 2022] that sex ratios shift only under very large disruptive shocks, absent in my sample.

Another potential concern is sex-selective abortion or parity-related stopping rules. While both mechanisms exist, the evidence indicates they are quantitatively negligible in the U.S. context and, if anything, would bias results against my findings. Sex-selective abortion is confined to small subgroups (Chinese and Indian mothers) and, even under extreme assumptions, could alter the sex ratio by less than 0.1 percentage point (about 3% of a standard deviation). Moreover, because girls tend to experience worse outcomes in communities with son preference, such selection would attenuate rather than generate my estimated effects. Stopping rules could also influence sex composition if parents continue childbearing until reaching a desired gender, but their impact is minimal. Explaining the observed variation would require implausibly large cross-market differences in parity, and I find no relationship between cohort size and the proportion male. Full robustness analyses of these issues are provided in Appendix B.4 and Appendix B.5.

4.6 Exclusion Restriction

Even conditional on exogeneity, it is important to consider the channels through which variation in sex composition at birth may affect maternal and neonatal outcomes. My empirical strategy requires that such variation operate primarily through the sex composition of local dating markets. Accordingly, the instrument should be orthogonal to other intermediate environments—such as school resources, educational inputs, or healthcare availability—that are determined prior to partnership formation or fertility decisions and that could independently affect health. I assess the relevance of these alternative channels using a series of balance tests based on multiple data sources; the main results are reported here, with additional analyses in Appendix B.

First, I test whether cohort sex composition predicts schooling environments that could affect health through human capital accumulation. I link the relevant birth cohorts to the years in which they attended high school using data from the Common Core of Data (CCD) and the Civil Rights Data Collection (CRDC), and estimate balance regressions of the form

$$W_{cra} = \alpha + \beta \, \text{PropMaleBirth}_{cra} + \delta X_{cra} + \varepsilon_{cra},$$

where $\text{PropMaleBirth}_{cra}$ denotes the proportion male at birth in county c, race r, and cohort a. The vector X_{cra} includes the log of cohort size at birth and in 2010, race fixed effects and age-group fixed effects. Across both data sources, I find no systematic association between sex composition at birth and various measures of school resources (see Panels A and C of Figure III). These results hold both in individual regressions and in pooled omnibus balance tests.[22] This lack of association does not purely reflect limited statistical power: as expected, the sex composition at birth precisely predicts realized sex composition in high school using both datasets (see Table A24).

Although sex composition at birth strongly predicts the gender composition of high schools, gender peer effects during schooling are unlikely to account for the results. Existing evidence indicates that female peer effects, when present, are modest and operate in directions inconsistent with the findings documented here.[23] For instance, Lavy and Schlosser [2011] implies that a one-standard-deviation increase in the share of female classmates would increase matriculation probabilities by only about 0.35 percentage points. Consistent with these small magnitudes, I find no evidence that cohort sex composition at birth affects long-run educational attainment. Using Opportunity Insights data and the specification equivalent to the one above, reduced-form estimates for college completion are small and statistically insignificant (see Panel D of Figure III). Additional discussion and bounding exercises are provided in Section B.7.

Second, I examine whether healthcare availability responds to sex composition at birth. Using county-level measures of general healthcare supply and services targeted to pregnant

[22] See Section B.6 for details about data and alternative specifications.

[23] See, for example, Hoxby [2000], Sacerdote [2011], Lu and Anderson [2015].

women and children from the Area Resource File, I estimate regressions analogous to those above. I find no evidence that cohort sex ratios affect healthcare availability. Intuitively, cohort-specific sex ratios are too small relative to county-level outcomes to shift healthcare resources, though large enough to alter dating markets (see Panel A of Figure III and Section B.6).

Third, I assess whether gender imbalances affect crime and antisocial behavior. Causal evidence from settings with extreme sex imbalances shows that male surplus can increase violence [Edlund et al., 2008, Amaral and Bhalotra, 2017]. Using Opportunity Insights data and the same specification as for education outcomes, I find no statistically significant effects of cohort sex composition at birth on incarceration. While coefficients are directionally consistent with the literature, they are small, statistically not significant, and would run counter to my main results (see Panel D of Figure III). Additional details, including alternative specifications and outcomes, are discussed in Section B.7.

I also examine other potential channels, including the formation of norms or social skills and parental divorce—since parents of daughters may be more likely to separate. Using Opportunity Insights Social Capital data and specifications analogous to those above, I find no statistically significant relationship between cohort sex composition at birth and measures of social interaction, including network clustering and volunteering rates (see Panel B of Figure III). Likewise, using ACS data, I find no evidence that sex composition at birth affects parental divorce rates. Consistent with these null findings, bounds based on estimates from the literature imply quantitatively small effects: for example, Dahl and Moretti [2008] suggests that a one-standard-deviation increase in the share of girls would raise divorce rates by only 0.077 percentage points. Additional details are provided in Sections B.8 and B.9.

These placebo tests provide reassurance that variation in sex composition at birth is orthogonal to the main potential confounding channels. Nevertheless, measurement limitations remain, and some alternative mechanisms cannot be fully ruled out. With this caveat in mind, the evidence presented in Mechanisms Section (6) indicates that dating markets offer

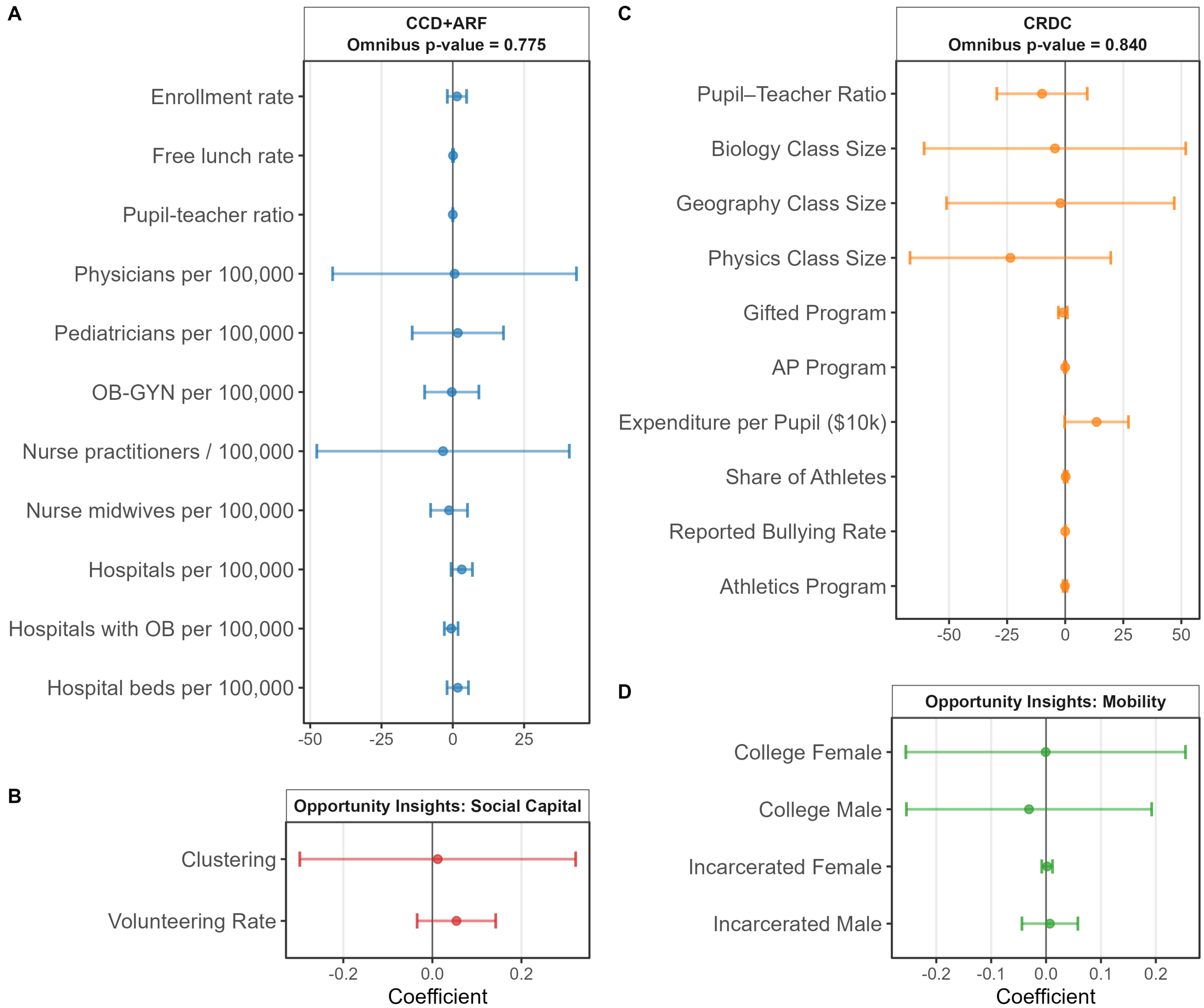


Figure III: Covariate balance and auxiliary reduced-form evidence

Notes: Each panel reports coefficients from regressions of market-level outcomes on the proportion male at birth (the instrument), with point estimates and 95% confidence intervals. **Panel A:** education outcomes from the Common Core of Data (CCD) and healthcare-availability measures from the Area Resource File (ARF, 2010). Observations at the county×race×age-group level; regressions control for log cohort size at birth and in 2010, include race and age-group fixed effects, weighted by 2010 market size; standard errors clustered at the county level; sample restricted to markets with complete covariates; omnibus F-test jointly tests orthogonality of the instrument to all covariates (see Table A22). **Panel B:** 2009–2010 Civil Rights Data Collection (CRDC). Observations at county×race level; controls for cohort size and race fixed effects; weighted by total high-school enrollment; SEs clustered at the county level; omnibus F-test on schooling covariates (see Table A23). **Panel C:** Opportunity Insights data for cohorts born 1978–1983, assigned to their childhood county. Observations at race×county×gender level; controls for cohort size and race fixed effects; heteroskedasticity-robust SEs. **Panel D:** Opportunity Insights: Social Capital for cohorts born 1986–1996, assigned to the county where individuals attended high school. Observations at county level; control for log cohort size at birth; heteroskedasticity-robust SEs.

an empirically robust and theoretically grounded explanation for the observed effects and are plausibly the primary channel through which they operate.

4.7 Specification and Outcomes

Under the above assumptions, the instrument eliminates the endogeneity issues present in the OLS. I estimate the effect of sex composition using a standard two-stage least squares framework:

$$\hat{PM}_{i,cra} = \zeta PMB_{i,cra} + \theta X_i \tag{2}$$

$$y_{i,cra} = \beta \hat{PM}_{cra} + \gamma X_i + \epsilon_i \tag{3}$$

In the first stage, I predict the value of the proportion male in 2010 given the proportion male at birth and the covariates and fixed effects X_i (the same as in equation 2, controlling additionally for cohort size at birth). I hence isolate the variation in adult sex composition that is only due to randomness in sex at birth. The second stage recovers the causal effect of market sex composition on outcomes.

I analyze four sets of outcomes, chosen to reflect the mechanisms implied by the conceptual framework. These outcomes span socio-demographic characteristics of mothers and their partners, as well as maternal and infant health indicators, that prior theory and evidence suggest should respond to shifts in dating market sex composition.

Fertility. Measured as the annual number of births per 1,000 women in each market-year. The denominator comes from Surveillance, Epidemiology, and End Results data (SEER), disaggregated by race-cohort-county, and year. Specifically, for each race–cohort–county cell, the numerator is the number of births to women in that cell in a given year, and the denominator is the population of women in the same race–cohort–county cell and year from SEER. This construction yields age-group specific birth rates at the market-year level. Each market-year is treated as a single observation. I regress this birth rate measure on the instrumented proportion of men, controlling for county-cohort, race-cohort and year fixed effects, as well as cohort sizes at birth and in 2010.

Marriage market dynamics. I examine whether the father is identified on the birth record[24], whether the mother is married, educational and age differences between partners.[25]

These outcomes serve both as a validity check—testing whether the instrument consistently shifts dating market outcomes in line with prior work [Angrist, 2002, Abramitzky et al., 2011]—and as evidence that changes in health are mediated by dynamics within the dating market.

Maternal health. Outcomes include diagnoses of chlamydia, gonorrhea, or syphilis during pregnancy, as well as pre-pregnancy diabetes and hypertension. These variables capture channels through which sex ratios may shape health both in the short run and cumulatively: sexual networks, household resources and their allocation, stress, and selection into child-bearing through greater control over fertility. To mitigate multiple-hypothesis concerns and increase statistical power, I also construct an Adverse Maternal Health Index, calculated as the simple average of standardized z-scores of above outcomes following Hoynes et al. [2016]. A lower index indicates fewer adverse health events.

Neonatal health. I assess newborn health using standard clinical indicators: preterm delivery (gestation < 37 weeks), low birth weight (< 2500g), APGAR score below 7, need for assisted ventilation, and death shortly after delivery. Analogous to maternal health, I also construct and utilize the Adverse Neonatal Health Index.

5 Sex Composition and Health: Results

The instrumental variable framework shows that a higher proportion of men in the dating market improves female marital prospects, maternal health, and neonatal outcomes.

The validity of the IV estimates depends on the strength of the relationship between the sex composition at birth and the proportion male in 2010. Table I demonstrates that this association is strong. The first stage coefficient is positive and highly significant, with the

[24] Following Spencer [2022], I classify the father as unknown if the birth certificate omits his age.

[25] These differences (following Charles and Luoh [2010], Abramitzky et al. [2011], Brainerd [2016]) proxy assortative matching on observable characteristics rather than relationship quality. Robustness checks using alternative measures are reported in Table A33

Kleibergen–Paap Wald statistic exceeding conventional thresholds.[26]

Table I: First Stage

Dependent Variable:	Prop. male 2010
Model:	(1)
Prop. male at birth	0.2329***
	(0.0236)
Fit statistics	
Within R^2	0.065
Wald Kleibergen-Paap (IV only)	97.3
Dependent variable mean	0.496
Observations	7,138,182

Notes: The regression contains controls for cohort size in 2010 and at birth, County×Age at birth, Race×Single age cohort, and Race×Age at birth fixed effects. The coefficient on *Prop. male at birth* correspond to β in equation 2. Each observation represents a single birth. Standard errors are clustered at the County-Race level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

To interpret the results, it is important to consider where the identifying variation comes from, focusing on three key aspects: (1) sample inclusion, (2) the strength of the first stage, and (3) the heterogeneous relevance of markets measured in this way.

First, the instrument relies on relatively small cohorts, so estimated effects are local to these populations. While small markets represent diverse contexts, they are not fully representative of the U.S. population. The instrument's variation is driven by the U.S.-born population, since foreign-born migrants are not included in U.S. natality data.[27]

Second, examining where the first stage is strongest is key to understanding the source of the identifying variation. Heterogeneity analyses show that the first stage is stronger among younger, less mobile cohorts, in Black and Native populations (Appendix Tables A1), and in rural counties (Table A15). By contrast, it weakens in large urban counties (Figure A.7), where migration likely plays a greater role in shaping adult sex ratios. In this context, the

[26] In the subsequent analysis, I combine the KP Wald statistic with the tF critical values developed by Lee et al. [2021] to perform valid t-ratio inference for the IV coefficients. The tF procedure adjusts the reference distribution of the usual t-statistic as a smooth function of the first-stage F-statistic, accounting for first-stage uncertainty. This yields confidence intervals with correct coverage even in the presence of weak instruments.

[27] The IV sample may underrepresent immigrant-heavy counties—the mean foreign-born share is 0.063 inside vs. 0.14 outside—though medians and upper quartiles are similar, and the difference is driven by a handful of large urban centers.

compliers are individuals who have remained in place rather than those who have moved away, implying that the estimated effects are most specific to this less mobile population.

Third, the strength of the instrument depends on how precisely the market definition (race–cohort–county) captures actual dating behavior. Relevance is greatest for cohorts over age 19 and for Black and White populations, who more often partner within their race (Figures A.9 and A.2). As the model in Appendix Section D.2 shows, market imbalance affects bargaining power broadly. It also affects those already in relationships, and at the high end of the market, as they experience the most significant changes in their outside options. If the dating market is indeed the primary channel, the impact should be strongest in areas where the definition of the market most closely reflects actual partnering behavior.

Placebo tests (Figure IV) show that the instrument predicts sex composition only within the same cohort, race, and county, but not across cohorts in the same location. This rules out county-level confounders and demonstrates that the identifying variation reflects cohort-specific fluctuations in sex ratios.

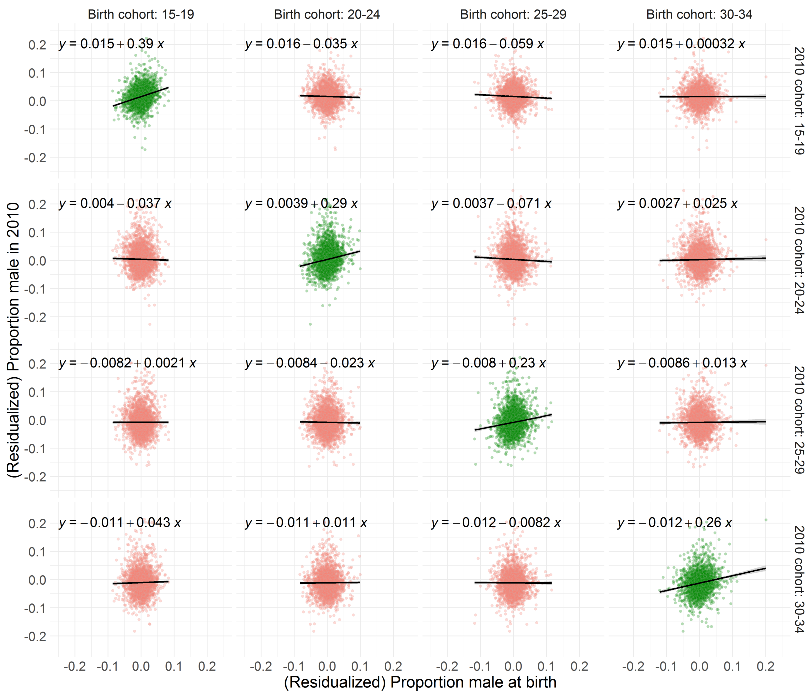
The results of the IV estimation are presented in Table II, while the OLS results for the full sample (Table A9) and the IV estimation sample (Table A10) are provided in the appendix[28].

First, I find that a higher proportion of men shifts fertility from nonmarital to marital births, with no effect on the overall birth rate. A one-standard-deviation increase in the proportion male raises the marital birth rate by 4.6 births per 1,000 women (9.7 percent of the mean of 47) and lowers the nonmarital birth rate by 3.9 births (11.2 percent of the mean of 34.4).[29] The decline in nonmarital births is concentrated in markets with easier access to abortion, suggesting that women are more empowered to avoid unintended pregnancies

[28] Because Tables A9 and A10 estimate the same OLS specification on different samples. While they are generally similar in sign, their magnitudes do not coincide in some cases. Table A10 provides the more relevant non-instrumented benchmark for the main IV results because it uses the same sample support. Differences across the two tables may reflect sample composition, suggesting either differential confounding or treatment heterogeneity across settings.

[29] Marital status-specific denominators are unavailable in SEER and the Census, so the rates are per 1,000 women overall. An alternative measure using ACS counts of married and unmarried women (Table A8) yields consistent though less precise estimates.

Figure IV: First Stage Placebo



Notes: Figure shows linear relationships between proportion male at birth and proportion male in 2010. The values are residualized with respect to the race. The diagonal panels represent the correlation between the prop. male at birth PMB_{cra} and the prop. male in 2010 PM_{cra} for the same cohort (and market). The off-diagonal panels plot a placebo relationship between sex composition at birth of one cohort PMB_{cra} and prop. male in 2010 of a different cohort but of the same race and county. The estimated coefficients are provided on each graph.

(Appendix Table A7).

Second, a higher proportion of men strengthens women's positions in the marriage market. Moving from the 25th to the 75th percentile of the proportion male reduces the likelihood of a birth without a father by 1.6 percentage points and increases the share of married mothers by 2.9 percentage points. Both coefficients are significant according to tF standard errors. These effects are not limited to childbearing women but extend to the broader female population, with women in more male-heavy markets more likely to be married by age 24 and remaining so as they age (Appendix Section C.3). The results regarding marital outcomes are overall consistent with empirical literature [Angrist, 2002, Charles and Luoh, 2010, Abramitzky et al., 2011, Brainerd, 2016] showing that the scarcity of women improves their marital prospects and decreases the rate of out-of-wedlock births. I do not find a clear response of relative partner education or age to changes in the sex ratio. In Appendix Table A33, I

Table II: IV Results

Fertility Outcomes Dependent Variables:	Birth Rate	Birth Rate (marital)	(non-marital)			
Prop. male 2010	11.19	119.7***	-100.1**			
	(59.17)	(42.57)	(39.18)			
Dep. var. mean	82.091	47.062	34.416			
Observations	142,064	142,064	142,064			
Sig. at 5% (Lee et al. 2022)	No	Yes	Yes			
Wald KP (1st stage)	40.512	40.512	40.512			

Marriage Market Outcomes Dependent Variables:	Unknown Father	Married	Diff. in Edu. (years)	Father Less Educated	Age Diff. (Father-Mother)	
Prop. male 2010	-0.4025***	0.7563***	0.0300	-0.0239	-0.3518	
	(0.1311)	(0.1840)	(0.5214)	(0.1200)	(1.208)	
Dependent variable mean	0.127	0.621	0.360	0.338	2.56	
Observations	7,166,343	7,478,536	6,105,173	6,105,173	6,259,559	
Sig. at 5% (Lee et al. 2022)	Yes	Yes	No	No	No	
Wald KP (1st stage)	96.1	98.0	79.7	79.7	76.9	

Maternal Health Outcomes Dependent Variables:	*Chlamydia*	*Gonorrhea*	*Syphilis*	*Diabetes*	*Hypertension*	*Adverse Maternal Health Index*
Prop. male 2010	-0.0676**	-0.0042	-0.0019	-0.0317*	-0.0955***	-0.3268***
	(0.0266)	(0.0093)	(0.0049)	(0.0169)	(0.0322)	(0.1105)
Dependent variable mean	0.019	0.003	0.0008	0.010	0.022	0
Observations	7,138,182	7,138,182	7,138,182	7,151,592	7,151,592	7,138,182
Sig. at 5% (Lee et al. 2022)	Yes	No	No	No	Yes	Yes
Wald KP (1st stage)	97.3	97.3	97.3	96.6	96.6	97.3

Infant Health Outcomes Dependent Variables:	*Preterm Birth*	*Low BW*	*Low APGAR*	*Assisted Ventilation*	*Death*	*Adverse Neonatal Health Index*
Prop. male 2010	-0.0798	-0.0644	-0.0512**	-0.0681*	-0.0013	-0.271**
	(0.0545)	(0.0461)	(0.0251)	(0.0413)	(0.0084)	(0.1105)
Dependent variable mean	0.121	0.087	0.024	0.046	0.003	0
Observations	7,540,450	7,539,221	7,515,076	7,149,031	7,155,905	7,116,816
Sig. at 5% (Lee et al. 2022)	No	No	Yes	No	No	Yes
Wald KP (1st stage)	97.2	97.5	97.0	96.5	96.0	95.8

Notes: Birth Rate is the yearly number of births given between 2011-2019 in a given dating market divided by the number women in that market (and year) and multiplied by 1000. Both marital and non-marital births use the same denominator, which comes from the SEER data. Fertility regressions are at the market level and contain controls for cohort size in 2010 and at birth, and interactions of county-cohort and race-cohort, and year. Negative *Diff. in Edu.* means that the father is more educated than the mother; *Father Less Educated* is an indicator for the father having strictly fewer years of education than the mother, and *Age Diff. (Father-Mother)* is the father's age minus the mother's age, in years. The proportion of men in 2010 is instrumented with proportion of men at birth of the cohort. Each regression at the individual level (Marriage Market, and Health Outcomes) contains controls for cohort size in 2010 and at birth, County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. The coefficient on *Prop. male 2010* correspond to β in equation 3. Sample of markets between 200-5000 people. Hispanics are excluded both in the outcomes and in the endogenous variable. Standard errors clustered at the County-Race level in all regressions. Wald statistic (Kleibergen-Paap) for the first stage is presented together with an information whether the coefficient is significant at 5% according to tF statistic (Lee et al. 2022). *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

examine additional measures of partner quality and find only weak evidence of a decline in the probability of large age gaps, consistent with Bitler and Schmidt [2011], Brainerd [2016]. While one might expect a more favorable sex ratio to improve partner quality, such an effect is not mechanical. Unlike Shenhav [2021], where higher women's relative wages improve observed partner quality by raising women's selectivity and screening out lower-surplus matches, sex-ratio variation affects partner quality only indirectly through the composition of available men. This distinction is especially relevant in my setting, where women are already slightly more educated than men on average, likely compressing the scope for sex-ratio improvements to appear on the education margin. Combined with the relatively modest variation in sex ratios in my data, this may help explain the limited response of observed partner education. More broadly, this contrasts with studies exploiting much larger sex-ratio shocks, such as those generated by historical wars, often in settings with markedly different male and female education distributions (e.g., Battistin et al. [2022]; Abramitzky et al. [2011]; Brainerd [2016]). At the same time, my findings are closer to those in Charles and Luoh [2010], who analyze a context more similar to mine and exploit sex-ratio variation generated by incarceration, also finding no clear evidence of improvements in observed partner quality.

The results on *Maternal Health Outcomes* show that women giving birth are healthier in markets with a higher proportion of men. Moving from the 25th to the 75th percentile of the proportion male reduces the share of mothers with chlamydia by 0.26 percentage points (13 percent of the mean) and with hypertension by 0.37 percentage points (17 percent of the mean). These magnitudes are about three times as large as the OLS estimates. Gonorrhea, Syphilis, and Diabetes, relatively rare conditions, have coefficients that are not statistically significant but align with the expected direction. The adverse health index coefficient is negative and strongly significant, indicating that a higher proportion of men decreases the occurrence of adverse outcomes.

Finally, a higher proportion of men in the dating market results in healthier newborns. IV results indicate that moving from the 25th to the 75th percentile of the proportion male reduces the likelihood of a low APGAR score (<7) by 0.2 percentage points, a sizable effect given the baseline rate of 2.4 percent. The APGAR score is particularly interesting because 11% of infants with a low APGAR score die within a year of birth (compared to 0.2% of infants with a normal score). For comparison, the EITC expansion studied by Hoynes et al. [2015] reduced low APGAR scores by 0.185 percentage points. The adverse neonatal health index results are negative and significant, consistent with fewer births experiencing adverse outcomes. Other outcomes align with the expected direction but are not statistically significant at conventional levels.

Heterogeneity analysis shows that the effects are stronger among racial minorities (Figure C.19), in urban markets[30] (Figure C.18), and in racially segregated markets (Figure C.20), where within-race imbalances are especially consequential for partner matching. Finally, women with longer exposure to local dating conditions—particularly older women (Figure C.21) —experience larger effects, consistent with cumulative advantages over time.

It is important to consider how dating market imbalances affect men as well as women, since understanding whether women's gains come at men's expense is central to assessing the broader welfare implications.

While my data do not allow me to observe men's outcomes directly, I examine two complementary sources of evidence. First, I study singlehood, which is often associated with adverse outcomes. Using American Community Survey, I find that higher male shares do not mechanically increase the prevalence of never-married men, consistent with Angrist [2002]. This suggests that improvements in women's bargaining position do not simply translate into more unpartnered men. Second, I draw on existing evidence to evaluate whether higher share male harms men. This evidence indicates a more nuanced picture: while some studies find adverse effects for men in certain contexts—such as higher depression rates in China [Zhang et al., 2021] or increased later-life mortality in Taiwan [Chang et al., 2024]—others document benefits, including lower incidence of sexually transmitted infections in the UK (Kang and Pongou, 2020) and higher marital satisfaction in Australia (Grosjean and Brooks, 2017). These results suggest that women's gains need not symmetrically imply men's losses, and in some domains improvements in women's standing may even benefit both sexes. A more detailed discussion is provided in Appendix Section C.6.

[30]Although the first stage is somewhat weaker in urban counties, it remains statistically strong (Kleibergen–Paap statistics above 20). Stronger effects in urban settings likely reflect higher minority shares and reduced search frictions in larger dating pools.

6 Mechanisms

The findings above indicate that sex composition affects maternal and neonatal health. Among the possible channels, changes in the dating market provide the most coherent and theoretically grounded explanation.

First, consistent with Section 2 and prior literature, sex ratios shape dating market dynamics. I find that imbalances in sex composition shift fertility from nonmarital to marital, altering women's marital status and partnership formation. These shifts are likely central mediators of the health effects. Married women display better health profiles and give birth to healthier children, whereas births to unmarried mothers—particularly when the father is unknown—are associated with worse outcomes [Currie and Moretti, 2003]. Descriptive OLS estimates underscore the magnitude of these associations: married mothers are 1.45 percentage points less likely to test positive for chlamydia (relative to a mean of 1.9pp), and their infants exhibit improved health indicators (Table A18).

Second, changes in sexual networks provide another pathway. Kang and Pongou [2020] show that higher male-to-female ratios reduce the number of partners, concurrent relationships, and unprotected sex. These adjustments are consistent with the declines in STIs observed in my IV estimates, reinforcing the dating market as a plausible channel.

Third, more favorable dating markets also influence the social profile of women who enter motherhood. Women giving birth in markets with more men are more likely to be highly educated and more often partnered with educated fathers (Appendix Table A29). These patterns suggest that women pursue pregnancies more selectively under favorable conditions, generating positive selection into motherhood with likely benefits for infant health. Importantly, maternal age does not vary systematically with sex composition, ruling out delayed childbearing as the relevant margin of adjustment.

To assess the contribution of such compositional changes, I conduct a decomposition that combines shifts in key maternal and partnership characteristics with their estimated associations with health outcomes (Appendix Section C.5). This exercise is necessarily illustrative rather than causal, but it provides insight into the extent to which observable social traits account for the results. Focusing on marital status, maternal education, and paternal education, Figure C.24 shows that these variables jointly explain 5–22 percent of the observed health effects, with marital status alone accounting for up to 16 percent.

Beyond these social factors, improvements in maternal health at the onset of pregnancy appear central. The observed gains are not necessarily driven by changes at the time of pregnancy. For instance, access to healthcare during pregnancy does not appear to change (Appendix Table A17). Rather, the gains stem from women entering pregnancy in better health. Stronger positions in the dating market likely enhance women's bargaining power and long-run living conditions, reducing pre-pregnancy risks such as diabetes, hypertension, and overweight (Appendix Table A29). These long-term health gains are likely the product of prolonged exposure to more favorable dating market conditions.

Finally, I consider migration as a potential channel through which dating market imbalances may affect outcomes. Women are somewhat more likely to move away from markets with fewer men, consistent with relocation toward more favorable dating environments. However, this migration is not strongly selective along observable characteristics, making it unlikely to drive the main estimates (Section C.7).

Overall, the evidence is most consistent with dating markets playing a central role in shaping partnership formation, sexual behavior, selection into motherhood, and maternal health, with other channels appearing less central in the settings examined here.

7 Counterfactual scenarios

To assess the broader relevance of my findings, I use counterfactual simulations to quantify how much of the racial gap in pregnancy outcomes between Black and White mothers can be attributed to disparities in dating markets.

I focus on three main counterfactuals: (i) eliminating the racial gap in sex ratios, (ii) eliminating the gap in incarceration for non-violent offenses, and (iii) reducing incarceration rates nationwide to New York's level.[31]

For each scenario, I construct counterfactual sex ratios and use the significant IV estimates to predict the resulting health outcomes. I then assess how much these shifts would narrow racial gaps in maternal and infant health. The results are summarized in Figure V. The simulations are implemented using bootstrap resampling, with full details provided in Appendix C.8.

The first scenario equalizes sex ratios so that Black women face the same male availability as White women of the same age and county. Removing this disadvantage reduces the racial gap in non-marital births by 3.5%, in maternal chlamydia and hypertension by 5.4% and 10.5%, and in low Apgar scores by 9.2%. The gap in maternal and neonatal health indices declines by 5.9% and 5.5%, respectively. Overall, these results imply that disparities in sex composition might account for up to 5–10% of the racial health gap.

The second scenario examines a shift in sex ratios comparable to that generated by eliminating racial disparities in incarceration for non-violent offenses. Because Black men are disproportionately incarcerated, these disparities directly lower the proportion of men available in Black women's dating markets.[32] Under this scenario, the racial gap in non-marital births falls by 1.1%, in maternal chlamydia and hypertension by 2.2% and 4.2%, and in low Apgar by 2.8%. The maternal and neonatal health indices improve by 2.0% and 1.6%, respectively.

The third scenario reduces incarceration rates nationwide to New York's levels. This reduces the gap in marriage rates by 1.2%, in chlamydia and hypertension by 2.1% and 4.5%, and in low Apgar by 2.9%. The gap in maternal and neonatal health indices declines by 2.1% and 1.6%.

The counterfactuals are best read as a descriptive illustration of the magnitudes of the dating-market channel, calibrated by an empirically observed shift in sex composition, not as

[31] In the early 2000s, New York implemented reforms targeting non-violent offenders that plausibly led to a sharp decline in its prison population, with incarceration falling by about 26% between 1999 and 2012 (Raphael and Stoll [2014]).

[32] Details on the construction of counterfactual sex ratios are provided in Appendix A.5.

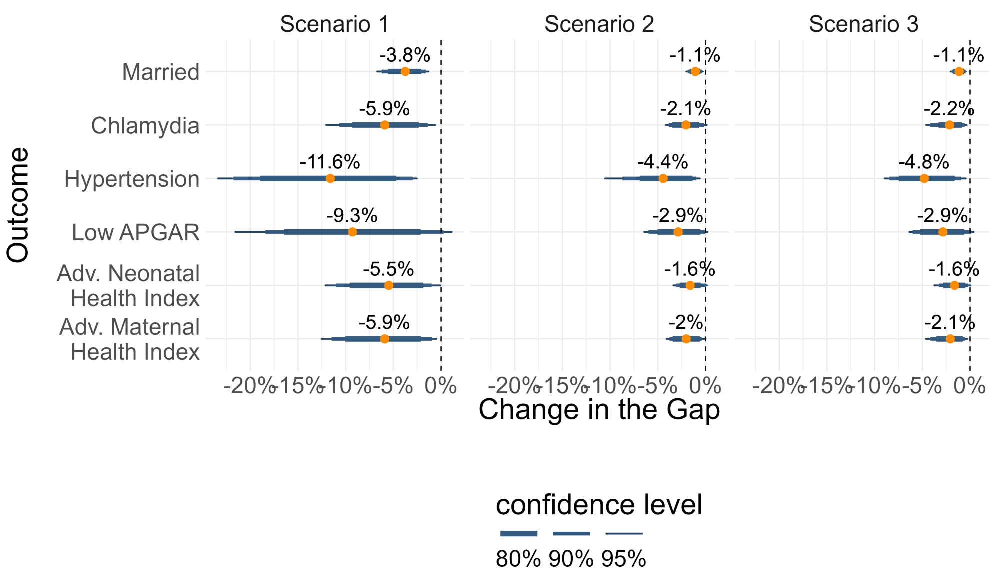
a causal estimate of any specific policy. Their validity rests on four assumptions:

Channel transportability. The marginal effect of local sex ratios through the dating market channels should be similar whether variation comes from birth cohorts (which identifies β) or from later-life shocks such as incarceration (which calibrates the shift). This is a real concern, because incarceration selectively removes men of plausibly lower quality. Three pieces of evidence bear on the question. Most directly, Charles and Luoh [2010] and Liu [2020], using incarceration-induced sex-ratio variation, recover marriage-market responses similar to my IV estimates. Second, a dating-market model with heterogeneous male quality (Appendix D.2) shows that lower-quality entrants still raise women's outside options, so bargaining gains propagate at magnitudes close to those implied by random reshuffling. Third, the scenario 2 releases only non-violent offenders, whose observables are closer to the baseline male pool. Effects may nonetheless differ across sources of variation, limiting the external validity of the estimates. A related concern is that incarceration may affect health through channels beyond the sex-ratio mechanism. Accordingly, the counterfactual is best interpreted as purely descriptive: incarceration is used only to calibrate a plausible magnitude, and the exercise does not identify the causal effect of any specific policy. Instead, it isolates the dating-market component of a shift in sex composition, abstracting from other channels through which an actual policy might operate.

External validity. The pooled IV coefficient should apply to the Black population the counterfactual is projected onto. The concern stems in part from differences in sample composition: the IV sample is 57% urban, compared to 90.3% for the Black population. Several patterns nonetheless support a cautious extrapolation. Effects are at least as large in urban markets (Figure C.18); subgroup IV estimates for urban minority and urban Black markets track the pooled coefficient in sign and magnitude, though noisier and not consistent for infant-health outcomes (Figure C.22). In addition, restricting the counterfactual to the IV sample—where the estimates are most directly applicable—yields similar implied effects, though in a narrower population (Figure C.23). While these patterns are reassuring, they do not prove that the coefficient transports with certainty, and caution in interpretation remains warranted.

Local linearity and no cross-market spillovers. The counterfactual shift is narrow (47.1% to 50.5% male) and lies within the IQR of fitted sex composition over which β is identified (48.5%–51.3%); approximate constancy of β across such a short in-sample window is plausible. Cross-market spillovers are likely limited given the near-complete same-race partnering and limited cross-race and cross-county substitution documented before. Nonetheless, the exercise is partial-equilibrium by construction.

Figure V: Simulations: Reduction in Racial Health Inequality



Notes: Reduction in the racial gap in health outcomes under the counterfactual scenarios. Scenario 1 equalizes sex ratios across races, Scenario 2 equalizes incarceration rates for non-violent offenses, and Scenario 3 reduces incarceration to New York levels. Horizontal lines show bootstrap confidence bands.

Finally, I examine whether relaxing search rigidities—through inter-racial partnering, cross-age partnering, or widening the geographic scope—could mitigate sex-ratio disparities and improve health outcomes.

These stylized counterfactuals point to modest but non-negligible gains (details in Appendix C.9). For example, pooling Black and White women into a common dating market raises the male share for Black women by 2.8 percentage points. Based on my IV estimates, this would prevent roughly 6,976 low-Apgar cases among Black newborns, offset by 5,454 additional cases among White newborns, for a net reduction of about 1,522 cases. Two caveats are important. First, this exercise extrapolates beyond the range of identifying variation. Second, interracial partnering may alter intra-household bargaining dynamics: sociological work on hypogamy suggests that racial differences within couples can shift relative power [Merton, 1941, Kalmijn, 1998]. Nonetheless, descriptive comparisons of Black mothers partnered with White versus Black fathers (Table A38) indicate that outcomes are at least as good—and often better—for those with White partners. While selection and endogeneity remain concerns, these patterns suggest that interracial partnering is not associated with worse maternal or infant outcomes.

By contrast, pooling across counties or age groups yields negligible benefits, since sex-ratio disparities are less systematic along those dimensions than across race.

8 Conclusion

In this study, I examine how sex composition in the dating market influences fertility and pregnancy outcomes, using a novel instrument based on cohort sex ratios at birth. I find that greater male availability changes fertility patterns and improves maternal and neonatal health. These improvements appear to arise from positive selection into motherhood and enhanced bargaining power within relationships, though other channels may also play a role.

Taken together, the findings suggest that policies or broader forces that strengthen women's outside options in the dating market may have downstream benefits for maternal and infant health. Identifying optimal individual responses lies beyond the present design. Nonetheless, understanding how women adjust to constrained markets—such as by prolonging search or accessing partners in alternative markets—remains an important avenue for future research.

Finally, the results highlight that dating-market conditions can affect outcomes as early as the first 24 hours of a child's life. These effects may persist and accumulate over time, and understanding their persistence could help shed light on the intergenerational transmission of health inequalities and inform efforts to improve outcomes for vulnerable populations.

Declaration of Generative AI and AI-Assisted Technologies in the Writing Process. During the preparation of this work, the author used ChatGPT to correct grammar and improve language. After using this tool, the author reviewed and edited the content as needed and takes full responsibility for the content of the published article.

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A Data and Sample Description

A.1 Additional Figures

Figure A.1: Racial Composition of Parents and Interracial Births

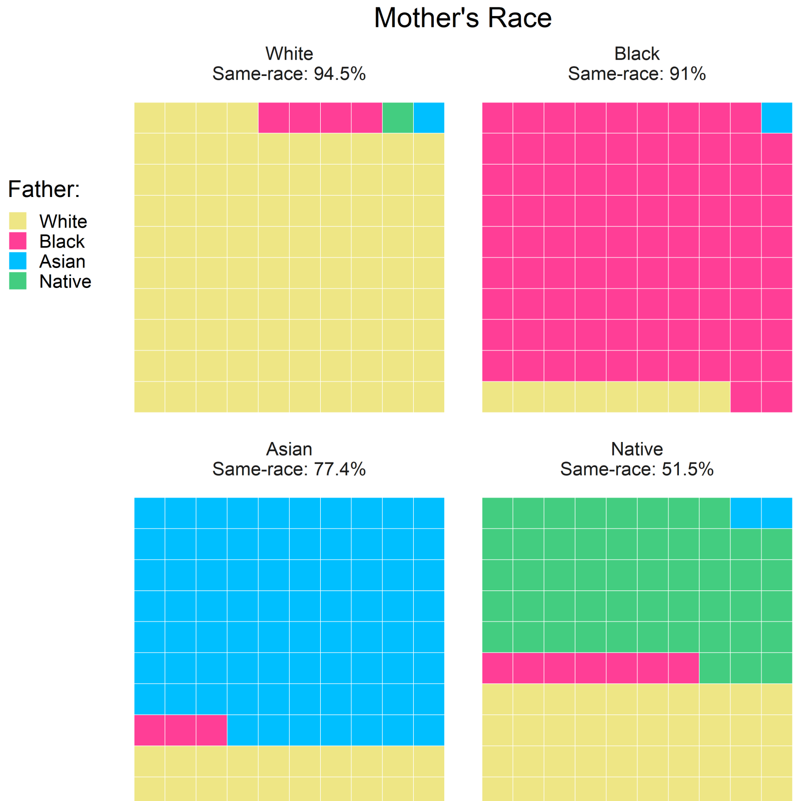
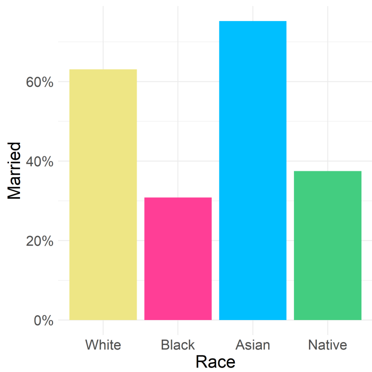


Figure A.2: Racial Composition of Parents

Notes: Plots show racial composition of fathers given mother's race. In each subplot, the number of colored boxes is proportional to the fathers of a given race. Source: Natality data 2011-2019

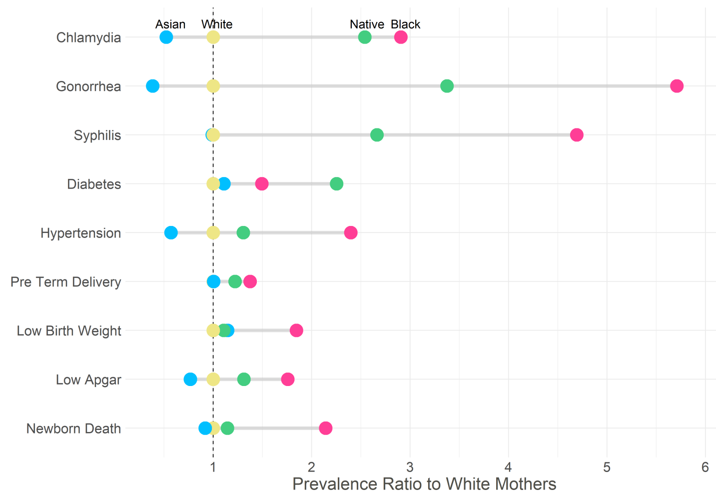
Figure A.3: Pregnancy Outcomes by Race



(a) Marital Rates by Race

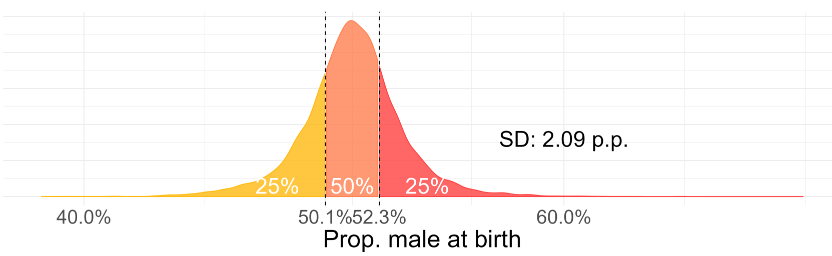
Notes: Each bar represents the share of married mothers by racial group. Source: Natality data 2011-2019

(b) Racial Disparities in Pregnancy Outcomes



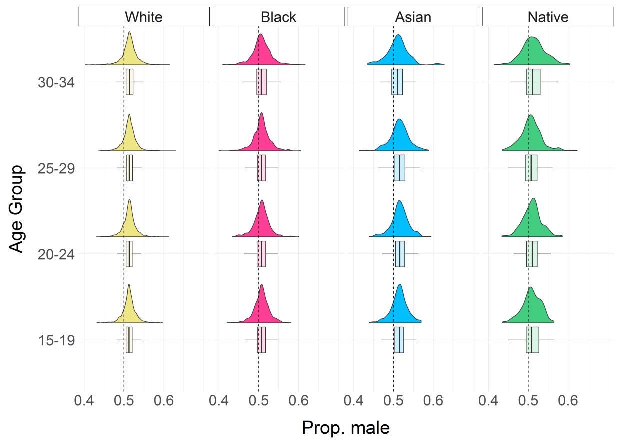
Notes: The light dots on the dashed line correspond to the baseline of the White mothers. Other dots represent the ratio of the average prevalence of a morbidity among a racial group to the average prevalence among White mothers. Source: Natality data 2011-2019

Figure A.4: Density of Proportion Male at Birth



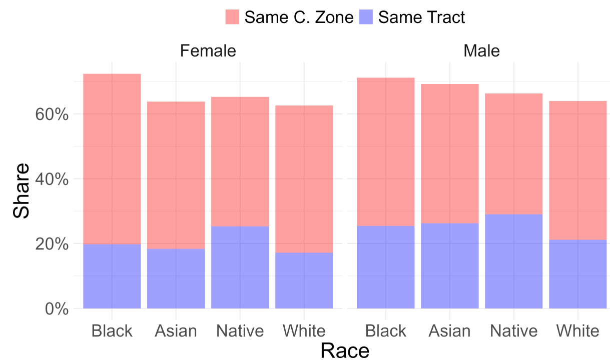
Notes: Figure shows the empirical distribution of the sex composition at birth. Each observation represents the proportion of male births among all births in the market. The two vertical lines show the first and the third quartile. Standard deviation is noted on the side. Markets with fewer than 200 and more than 5000 births are excluded. Each market has the same weight.

Figure A.5: Density of Proportion Male at Birth by Race and Cohort



Notes: Figure shows the empirical distribution of the sex composition. Each observation represents the proportion of male births in a dating market.

Figure A.6: Geographic Mobility since Childhood



Notes: Figure shows the proportion of people born between 1978 and 1983 who live in their childhood census tract or commuting zone as young adults. Source: Opportunity Insights data.

A.2 Additional Tables

Table A1: First Stage by Cohort and Racial Group

Dependent Variable:	Prop. male 2010						
	Full IV sample	15-19	20-24	25-29	30-34	White & Asian	Black & Native
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Prop. male at birth	0.2329***	0.4267***	0.3281***	0.0511	0.2040***	0.1950***	0.3269***
	(0.0236)	(0.0336)	(0.0489)	(0.0645)	(0.0655)	(0.0242)	(0.0459)
Wald Kleibergen-Paap (IV only)	97.3	160.9	45.0	0.628	9.70	64.8	50.7
Dependent variable mean	0.496	0.511	0.498	0.486	0.482	0.501	0.481
Observations	7,138,182	1,966,817	2,486,046	2,033,986	991,687	5,673,531	1,805,005

Notes: Controls include cohort size in 2010 and at birth, and fixed effects for county-age, race-age, and county-cohort. Standard errors are clustered at the county-race level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2: Sample coverage: fraction of U.S. births, population, markets, and counties represented in the IV analysis

	Population coverage		*Market coverage*		*Relevant counties*
Group	Births	Population	Markets	Counties	Counties with $\geq$ 200 pop.
All	0.330	0.293	0.377	0.283	0.843
White	0.295	0.267	0.765	0.727	0.810
Black	0.366	0.329	0.372	0.273	0.890
Asian	0.477	0.415	0.100	0.070	0.928
Native	0.621	0.539	0.099	0.061	0.983

Notes: A county–race–age cell is included in the IV sample if its birth-cohort size lies between 200 and 5,000 individuals. *Births* and *Population* are the shares of each group's national births (2011–2019) and population aged 15–34 that fall in included cells. *Markets* is the share of all county–race–age cells that are included. textitCounties is the share of all county–race groups that ever enter the IV sample. *Counties with* $\geq$ 200 *pop.* restricts the sample to county x race groups where the row's race has at least 200 individuals of some age group.

Table A3: Balance: outside vs. within IV sample – county characteristics, overall

	U.S. average	Outside IV	Within IV	Diff.
Hospital beds / 1,000	3.115 (2.229)	3.269 (1.944)	2.787 (2.711)	-0.482***
Incarceration rate	0.025 (0.029)	0.021 (0.025)	0.037 (0.036)	0.016***
Median income	56,940 (18,447)	60,062 (18,315)	50,314 (16,911)	-9,748***
PCPs / 1,000	0.727 (0.292)	0.789 (0.268)	0.595 (0.296)	-0.194***
College share	0.301 (0.131)	0.329 (0.120)	0.242 (0.134)	-0.088***
Foreign-born share	0.123 (0.107)	0.150 (0.112)	0.068 (0.070)	-0.082***
Married share	0.485 (0.099)	0.476 (0.093)	0.505 (0.107)	0.030***
Unemployment share	0.063 (0.016)	0.062 (0.015)	0.065 (0.017)	0.002**
Urban share	0.844 (0.363)	0.973 (0.163)	0.571 (0.495)	-0.401***

Notes: The unit of observation is the county–race group. Entries report population-weighted means for individuals aged 15–34 (weighted standard deviations in parentheses). "Within IV sample" refers to county–race-cohort groups that appear in the IV analysis for at least one age cohort, defined as having a birth-cohort size between 200 and 5,000 individuals; "Outside IV sample" is the complement. The difference column reports Within − Outside. Significance is based on a weighted difference-in-means test using Kish effective sample sizes. *Sources:* Hospital beds and PCPs from the 2010 Area Resource File; median income, college share, foreign-born, and married share from the 2015 ACS 5-year estimates; unemployment from FRED (2010–2019); urban from the OMB classification. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A4: Balance: outside vs. within IV sample − birth characteristics, overall

	U.S. average	Outside IV	Within IV	Diff.
Mother's age	28.80 (4.99)	29.10 (4.93)	28.16 (5.03)	-0.942***
Father's age	31.67 (6.20)	31.93 (6.10)	31.10 (6.35)	-0.830***
Edu. diff. (years)	0.308 (1.99)	0.284 (2.02)	0.360 (1.92)	0.076***
Unknown father	0.114 (0.318)	0.108 (0.310)	0.127 (0.332)	0.019***
Married	0.649 (0.477)	0.663 (0.473)	0.621 (0.485)	-0.042***
Chlamydia	0.0157 (0.124)	0.0142 (0.118)	0.0188 (0.136)	0.0046***
Gonorrhea	0.0027 (0.052)	0.0026 (0.051)	0.0030 (0.054)	0.0004*
Syphilis	0.0008 (0.029)	0.0009 (0.029)	0.0008 (0.028)	-0.0001
Diabetes	0.0081 (0.090)	0.0074 (0.086)	0.0095 (0.097)	0.0021***
Hypertension	0.0192 (0.137)	0.0180 (0.133)	0.0216 (0.145)	0.0037***
Preterm birth	0.113 (0.317)	0.109 (0.312)	0.121 (0.326)	0.012***
Low birth weight	0.081 (0.274)	0.079 (0.269)	0.087 (0.282)	0.008***
Low APGAR	0.0213 (0.144)	0.0199 (0.140)	0.0242 (0.154)	0.0043***
Assisted ventilation	0.0425 (0.202)	0.0410 (0.198)	0.0458 (0.209)	0.0048***
Death	0.0029 (0.053)	0.0027 (0.052)	0.0031 (0.056)	0.0004***

Notes: The unit of observation is the individual birth. Outside IV and Within IV are means for births in county–race–cohort cells with cohort size outside or within the IV sample range (200–5,000). Diff. is (Within IV − Outside IV) with significance from a two-sided test with standard errors clustered at the county–race level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A5: Balance: outside vs. within IV sample – county characteristics, by race

	U.S. average	Outside IV	Within IV	Diff.
Panel A. White				
Hospital beds / 1,000	3.024 (2.193)	3.168 (1.824)	2.671 (2.874)	-0.496***
Incarceration rate	0.017 (0.012)	0.014 (0.008)	0.027 (0.015)	0.012***
Median income	59,248 (16,202)	62,812 (16,287)	50,526 (12,188)	-12,286***
PCPs / 1,000	0.719 (0.295)	0.789 (0.272)	0.549 (0.278)	-0.240***
College share	0.308 (0.118)	0.343 (0.110)	0.223 (0.093)	-0.120***
Foreign-born share	0.115 (0.102)	0.142 (0.106)	0.048 (0.049)	-0.093***
Married share	0.515 (0.053)	0.502 (0.050)	0.545 (0.048)	0.042***
Unemployment share	0.062 (0.016)	0.062 (0.015)	0.064 (0.017)	0.002**
Urban share	0.828 (0.377)	0.977 (0.151)	0.464 (0.499)	-0.513***
Panel B. Black				
Hospital beds / 1,000	3.764 (2.535)	4.120 (2.465)	3.195 (2.542)	-0.925***
Incarceration rate	0.071 (0.049)	0.067 (0.044)	0.078 (0.057)	0.012
Median income	37,321 (12,992)	37,814 (12,192)	36,541 (14,129)	-1,273
PCPs / 1,000	0.740 (0.277)	0.793 (0.243)	0.654 (0.305)	-0.138***
College share	0.193 (0.074)	0.201 (0.060)	0.182 (0.092)	-0.019**
Foreign-born share	0.137 (0.113)	0.168 (0.123)	0.088 (0.074)	-0.081***
Married share	0.290 (0.060)	0.278 (0.056)	0.310 (0.061)	0.033***
Unemployment share	0.066 (0.014)	0.065 (0.011)	0.068 (0.018)	0.003
Urban share	0.903 (0.296)	0.969 (0.173)	0.797 (0.402)	-0.172***
Panel C. Asian				
Hospital beds / 1,000	2.825 (1.517)	2.701 (1.572)	2.935 (1.457)	0.234
Incarceration rate	–	–	–	–
Median income	76,478 (21,613)	75,877 (20,454)	76,995 (22,549)	1,117
PCPs / 1,000	0.831 (0.264)	0.815 (0.241)	0.844 (0.282)	0.029
College share	0.510 (0.134)	0.488 (0.123)	0.529 (0.141)	0.042
Foreign-born share	0.223 (0.114)	0.274 (0.125)	0.178 (0.081)	-0.096***
Married share	0.577 (0.078)	0.565 (0.079)	0.588 (0.075)	0.023
Unemployment share	0.063 (0.015)	0.065 (0.013)	0.061 (0.017)	-0.003
Urban share	0.972 (0.166)	0.947 (0.224)	0.993 (0.082)	0.046
Panel D. Native				
Hospital beds / 1,000	2.917 (2.197)	2.966 (2.771)	2.886 (1.739)	-0.080
Incarceration rate	0.031 (0.035)	0.032 (0.050)	0.031 (0.027)	-0.002
Median income	39,636 (13,607)	41,201 (18,665)	38,808 (9,836)	-2,392
PCPs / 1,000	0.657 (0.276)	0.632 (0.316)	0.673 (0.246)	0.041
College share	0.140 (0.083)	0.151 (0.114)	0.134 (0.053)	-0.017*
Foreign-born share	0.103 (0.099)	0.089 (0.101)	0.112 (0.097)	0.023*
Married share	0.371 (0.110)	0.400 (0.147)	0.353 (0.073)	-0.046***
Unemployment share	0.069 (0.024)	0.070 (0.028)	0.069 (0.021)	-0.002
Urban share	0.661 (0.473)	0.615 (0.487)	0.690 (0.463)	0.075

Notes: The unit of observation is the county–race group. Entries report population-weighted means for individuals aged 15–34 within each racial group; weighted standard deviations are in parentheses. "Within IV sample" refers to county–race groups that appear in the IV analysis for at least one age cohort, defined as having a birth-cohort size between 200 and 5,000 individuals; "Outside IV sample" is the complement. The difference column reports Within − Outside. Significance is based on a weighted difference-in-means test using Kish effective sample sizes. Incarceration rate for Asians is suppressed due to small counts. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A6: Balance: outside vs. within IV sample – birth characteristics, by race

	U.S. avg.	Outside IV	Within IV	Diff.
Panel A. White				
Mother's age	28.96 (4.86)	29.40 (4.78)	27.91 (4.91)	-1.487***
Father's age	31.53 (5.88)	31.92 (5.79)	30.57 (5.99)	-1.347***
Edu. diff. (years)	0.337 (2.00)	0.296 (2.04)	0.437 (1.88)	0.142***
Unknown father	0.075 (0.263)	0.068 (0.251)	0.093 (0.290)	0.025***
Married	0.726 (0.446)	0.743 (0.437)	0.685 (0.464)	-0.058***
Chlamydia	0.0102 (0.100)	0.0090 (0.094)	0.0130 (0.113)	0.0040***
Gonorrhea	0.0013 (0.036)	0.0013 (0.036)	0.0015 (0.038)	0.0002***
Syphilis	0.0004 (0.019)	0.0004 (0.019)	0.0004 (0.019)	0.0000
Diabetes	0.0070 (0.083)	0.0064 (0.080)	0.0083 (0.091)	0.0019***
Hypertension	0.0163 (0.126)	0.0152 (0.122)	0.0189 (0.136)	0.0037***
Preterm birth	0.101 (0.301)	0.098 (0.297)	0.109 (0.312)	0.012***
Low birth weight	0.068 (0.252)	0.066 (0.249)	0.073 (0.259)	0.006***
Low APGAR	0.0191 (0.137)	0.0174 (0.131)	0.0231 (0.150)	0.0057***
Assisted ventilation	0.0428 (0.203)	0.0410 (0.198)	0.0472 (0.212)	0.0061***
Death	0.0022 (0.047)	0.0021 (0.046)	0.0024 (0.049)	0.0003***
Panel B. Black				
Mother's age	27.45 (5.24)	27.34 (5.19)	27.60 (5.30)	0.257
Father's age	30.97 (7.27)	30.91 (7.27)	31.05 (7.26)	0.131
Edu. diff. (years)	0.374 (1.83)	0.355 (1.83)	0.408 (1.82)	0.053***
Unknown father	0.286 (0.452)	0.295 (0.456)	0.271 (0.445)	-0.024*
Married	0.313 (0.464)	0.302 (0.459)	0.331 (0.470)	0.029**
Chlamydia	0.0389 (0.193)	0.0378 (0.191)	0.0409 (0.198)	0.0031
Gonorrhea	0.0086 (0.092)	0.0086 (0.093)	0.0085 (0.092)	-0.0001
Syphilis	0.0026 (0.051)	0.0029 (0.054)	0.0021 (0.046)	-0.0008**
Diabetes	0.0114 (0.106)	0.0108 (0.103)	0.0125 (0.111)	0.0018***
Hypertension	0.0344 (0.182)	0.0330 (0.179)	0.0368 (0.188)	0.0038**
Preterm birth	0.163 (0.369)	0.160 (0.367)	0.167 (0.373)	0.007***
Low birth weight	0.130 (0.336)	0.129 (0.335)	0.132 (0.338)	0.003*
Low APGAR	0.0325 (0.177)	0.0321 (0.176)	0.0331 (0.179)	0.0009
Assisted ventilation	0.0470 (0.212)	0.0455 (0.208)	0.0496 (0.217)	0.0041
Death	0.0056 (0.074)	0.0056 (0.074)	0.0056 (0.075)	0.0001
Panel C. Asian				
Mother's age	30.72 (4.50)	30.82 (4.57)	30.60 (4.41)	-0.217
Father's age	34.07 (6.09)	34.18 (6.20)	33.92 (5.95)	-0.256
Edu. diff. (years)	-0.017 (2.12)	0.018 (2.09)	-0.057 (2.15)	-0.075**
Unknown father	0.035 (0.184)	0.036 (0.187)	0.034 (0.181)	-0.002
Married	0.839 (0.367)	0.818 (0.386)	0.858 (0.349)	0.040*
Chlamydia	0.0061 (0.078)	0.0056 (0.075)	0.0066 (0.081)	0.0010
Gonorrhea	0.0005 (0.023)	0.0005 (0.021)	0.0006 (0.024)	0.0001**
Syphilis	0.0005 (0.022)	0.0005 (0.022)	0.0005 (0.022)	0.0000
Diabetes	0.0083 (0.091)	0.0078 (0.088)	0.0088 (0.093)	0.0010
Hypertension	0.0089 (0.094)	0.0081 (0.090)	0.0097 (0.098)	0.0016**

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	U.S. avg.	Outside IV	Within IV	Diff.
Preterm birth	0.098 (0.297)	0.095 (0.293)	0.101 (0.302)	0.006
Low birth weight	0.081 (0.273)	0.079 (0.269)	0.084 (0.278)	0.005***
Low APGAR	0.0140 (0.118)	0.0137 (0.116)	0.0143 (0.119)	0.0006
Assisted ventilation	0.0293 (0.169)	0.0273 (0.163)	0.0315 (0.175)	0.0042
Death	0.0022 (0.046)	0.0020 (0.045)	0.0023 (0.048)	0.0003**
Panel D. Native				
Mother's age	27.06 (5.11)	27.32 (5.23)	26.85 (5.02)	-0.471
Father's age	29.84 (6.65)	30.18 (6.69)	29.59 (6.59)	-0.590
Edu. diff. (years)	0.299 (1.72)	0.310 (1.76)	0.291 (1.70)	-0.018
Unknown father	0.205 (0.404)	0.188 (0.391)	0.216 (0.411)	0.027***
Married	0.362 (0.480)	0.396 (0.489)	0.337 (0.473)	-0.059**
Chlamydia	0.0362 (0.187)	0.0337 (0.180)	0.0380 (0.191)	0.0044
Gonorrhea	0.0053 (0.073)	0.0043 (0.065)	0.0060 (0.077)	0.0017***
Syphilis	0.0015 (0.039)	0.0013 (0.037)	0.0016 (0.040)	0.0003
Diabetes	0.0191 (0.137)	0.0201 (0.140)	0.0186 (0.135)	-0.0014
Hypertension	0.0217 (0.146)	0.0209 (0.143)	0.0221 (0.147)	0.0013
Preterm birth	0.134 (0.341)	0.131 (0.337)	0.136 (0.343)	0.006**
Low birth weight	0.077 (0.267)	0.077 (0.267)	0.077 (0.266)	-0.001
Low APGAR	0.0247 (0.155)	0.0222 (0.147)	0.0262 (0.160)	0.0040***
Assisted ventilation	0.0492 (0.216)	0.0503 (0.218)	0.0487 (0.215)	-0.0016
Death	0.0029 (0.054)	0.0029 (0.053)	0.0030 (0.055)	0.0001

Notes: U.S. avg. is the unweighted mean across all U.S. births in the natality file. Outside IV and Within IV are means for births in county–race–cohort cells with cohort size outside or within the IV sample range (200–5,000). Diff. is (Within IV − Outside IV) with significance from a two-sided test with standard errors clustered at the county–race level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Fertility Outcomes by Abortion Restrictions

	Marital birth rate		Non-marital birth rate	
	Low restrictions	High restrictions	Low restrictions	High restrictions
Proportion male 2010	103.1*	102.9***	-120.9**	-25.71
	(58.19)	(38.42)	(50.27)	(44.20)
Dependent variable mean	48.01	44.26	33.29	37.51
Observations	105,254	35,430	105,254	35,430

Notes: This table reports IV estimates of the effect of the sex ratio (Proportion male in 2010) on fertility outcomes, split by whether a state had abortion restrictions above or below the median. Abortion restrictions are measured using Gutmacher Institute's Hostile and Supportive State Abrotion Laws in 2010 data. Controls include log cohort size in 2010 and at birth. All regressions include *county* × *cohort*, *race* × *cohort*, and year fixed effects. Standard errors clustered at the county–race level are shown in parentheses. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: ACS Fertility Outcomes: Overall and by Marital Status

	OLS			IV		
	Overall births	Marital births	Non-marital births	Overall births	Marital births	Non-marital births
Proportion male 2010	16.44	126.9***	-37.43***	-19.75	28.16	-111.8
	(11.75)	(17.87)	(12.17)	(70.54)	(121.3)	(69.70)
Dependent variable mean	25.4	31.53	19.84	27.7	34.18	22.16
Wald (1st stage)				29.6	25.72	23.75
Observations	53,432	53,407	53,436	33,037	33,014	33,041
R^2	0.811	0.778	0.814	1.00	0.782	0.758

Notes: This table reports ACS-based estimates of the effect of the sex ratio (*Proportion male 2010*) on fertility outcomes. Columns 1 and 4 report estimates for the overall birth rate. Columns 2–3 and 5–6 report estimates separately for marital and non-marital births. The unit of observation is a county × race cell. Fertility rates are constructed using ACS population counts. For the overall birth rate, the denominator is the total number of women in the relevant county × race cell. For the marital-status-specific outcomes, the denominators are the numbers of married and unmarried women, respectively, from the ACS. The instrument is the county × race sex composition at birth, pooling all age groups together. The estimation sample is restricted to markets with 200–5,000 individuals. Differences relative to Table II reflect both the use of marital-status-specific denominators and the reliance on more aggregated county–race cells, without age-specific disaggregation. All regressions include county×cohort, race×cohort, and year fixed effects. Standard errors clustered at the county–race level are shown in parentheses. *Conventional significance levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: OLS Results in the Entire Sample

Fertility Outcomes (OLS)						
Dependent Variables:	Birth Rate	Birth Rate (marital)	Birth Rate (non-marital)			
Prop. male 2010	66.79***	61.17***	2.183			
	(6.920)	(5.986)	(4.519)			
Dependent variable mean	85.580	49.049	35.913			
Observations	344,486	344,486	344,486			
R^2	0.686	0.624	0.770			
Marriage Market Outcomes						
Dependent Variables:	Unknown Father	Married	Diff. in Edu. (years)	Father Less Educated	Age Diff. (Father-Mother)	
Prop. male 2010	-0.1912***	0.3055***	-0.5233***	-0.0988***	-0.5149***	
	(0.0208)	(0.0329)	(0.0722)	(0.0153)	(0.1603)	
Dependent variable mean	0.113	0.650	0.308	0.330	2.51	
Observations	23,299,377	23,818,474	19,718,794	19,718,794	20,661,599	
Maternal Health Outcomes						
Dependent Variables:	Chlamydia	Gonorrhea	Syphilis	Diabetes	Hypertension	Adverse Maternal Health Index
Prop. male 2010	-0.0189***	-0.0072***	-0.0022***	-0.0067***	-0.0326***	-0.137***
	(0.0033)	(0.0012)	(0.0007)	(0.0015)	(0.0045)	(0.019)
Dependent variable mean	0.015	0.003	0.0008	0.008	0.019	0
Observations	23,224,271	23,224,271	23,224,271	23,257,824	23,257,824	23,224,271
Infant Health Outcomes						
Dependent Variables:	Preterm Birth	Low BW	Low APGAR	Assisted Ventilation	Death	Adverse Neonatal Health Index
Prop. male 2010	-0.0472***	-0.0486***	-0.0085***	-0.0092*	-0.0022***	-0.093***
	(0.0053)	(0.0046)	(0.0021)	(0.0054)	(0.0007)	(0.011)
Dependent variable mean	0.113	0.082	0.021	0.041	0.003	0
Observations	24,467,061	24,461,432	24,385,422	23,246,802	23,266,090	23,142,465

Notes: Birth Rate is the yearly number of births given between 2011-2019 in a given dating market divided by the number women in that market (and year) and multiplied by 1000. Both marital and non-marital births use the same denominator, which comes from the SEER data. Fertility regressions are at the market level and contain controls for cohort size in 2010 and at birth, and interactions of county-cohort and race-cohort, and year. Negative *Diff. in Edu.* means that the father is more educated than the mother; *Father Less Educated* is an indicator for the father having strictly fewer years of education than the mother, and *Age Diff. (Father-Mother)* is the father's age minus the mother's age, in years. Each regression other than fertility contains controls for cohort size in 2010, County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. The coefficient on *Prop. male 2010* correspond to β in equation 3. Sample of markets between 200-5000 people. Standard errors clustered at the County-Race level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: OLS Results in IV sample

Fertility Outcomes Dependent Variables:	Birth Rate	Birth Rate (marital)	Birth Rate (non-marital)			
Prop. male 2010	64.26***	75.82***	-10.01			
	(13.60)	(9.775)	(9.367)			
Dependent variable mean	82.1	47.1	34.4			
Observations	142,064	142,064	142,064			
R^2	0.734	0.665	0.796			
Marriage Market Outcomes Dependent Variables:	Unknown Father	Married	Diff. in Edu. (years)	Father Less Educated	Age Diff. (Father-Mother)	
Prop. male 2010	-0.2126***	0.3976***	-0.7418***	-0.1311***	-0.1991	
	(0.0215)	(0.0407)	(0.1169)	(0.0217)	(0.2239)	
Dependent variable mean	0.127	0.621	0.360	0.338	2.56	
Observations	7,166,343	7,478,536	6,105,173	6,105,173	6,259,559	
Maternal Health Outcomes Dependent Variables:	Chlamydia	Gonorrhea	Syphilis	Diabetes	Hypertension	Adverse Maternal Health Index
Prop. male 2010	-0.0292***	-0.0092***	-0.0026***	-0.0110***	-0.0247***	-0.1617***
	(0.0051)	(0.0017)	(0.0010)	(0.0031)	(0.0052)	(0.0216)
Dependent variable mean	0.019	0.003	0.0008	0.010	0.022	0.014
Observations	7,138,182	7,138,182	7,138,182	7,151,592	7,151,592	7,138,182
Infant Health Outcomes Dependent Variables:	Preterm Birth	Low BW	Low APGAR	Assisted Ventilation	Death	Adverse Neonatal Health Index
Prop. male 2010	-0.0592***	-0.0603***	-0.0118***	-0.0255***	-0.0021	-0.1300***
	(0.0101)	(0.0085)	(0.0046)	(0.0095)	(0.0014)	(0.0217)
Dependent variable mean	0.121	0.087	0.024	0.046	0.003	0.019
Observations	7,540,450	7,539,221	7,515,076	7,149,031	7,155,905	7,138,182

Notes: Birth Rate is the yearly number of births given between 2011-2019 in a given dating market divided by the number women in that market (and year) and multiplied by 1000. Both marital and non-marital births use the same denominator, which comes from the SEER data. Fertility regressions are at the market level and contain controls for cohort size in 2010 and at birth, and interactions of county-cohort and race-cohort, and year. Negative *Diff. in Edu.* means that the father is more educated than the mother; *Father Less Educated* is an indicator for the father having strictly fewer years of education than the mother, and *Age Diff. (Father-Mother)* is the father's age minus the mother's age, in years. Each regression other than fertility contains controls for cohort size in 2010, County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. The coefficient on *Prop. male 2010* correspond to β in equation 3. Sample of markets between 200-5000 people. Standard errors clustered at the County-Race level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: IV Results, 200-2000 Markets

Fertility Outcomes Dependent Variables:	Birth Rate	Birth Rate (marital)	Birth Rate (non-marital)			
Prop. male 2010	60.91	166.5***	-112.0**			
	(62.32)	(51.73)	(54.79)			
Dep. var. mean	83.494	47.215	35.677			
Observations	110,154	110,154	110,154			
Sig. at 5% (Lee et al. 2022)	No	Yes	Yes			
Wald KP (1st stage)	14.809	14.809	14.809			
Marriage Market Outcomes Dependent Variables:	*Unknown Father*	*Married*	*Diff. in Edu. (years)*	*Father Less Educated*	*Age Diff. (Father-Mother)*	
Prop. male 2010	-0.4146**	0.7849***	0.6495	0.0939	1.727	
	(0.1720)	(0.2261)	(0.6442)	(0.1528)	(1.302)	
Dependent variable mean	0.132	0.618	0.342	0.333	2.69	
Observations	3,538,303	3,702,314	2,988,393	2,988,393	3,071,959	
Sig. at 5% (Lee et al. 2022)	Yes	Yes	No	No	No	
Wald KP (1st stage)	52.9	52.4	43.0	43.0	41.3	
Maternal Health Outcomes Dependent Variables:	*Chlamydia*	*Gonorrhea*	*Syphilis*	*Diabetes*	*Hypertension*	*Adverse Maternal Health Index*
Prop. male 2010	-0.0891***	-0.0142	-0.0030	-0.0465**	-0.1088**	-0.4782***
	(0.0345)	(0.0115)	(0.0062)	(0.0213)	(0.0428)	(0.1662)
Dependent variable mean	0.020	0.003	0.0009	0.010	0.022	0.018
Observations	3,522,378	3,522,378	3,522,378	3,529,591	3,529,591	3,522,378
Sig. at 5% (Lee et al. 2022)	Yes	No	No	Yes	Yes	Yes
Wald KP (1st stage)	53.5	53.5	53.5	53.1	53.1	53.5
Infant Health Outcomes Dependent Variables:	*Preterm Birth*	*Low BW*	*Low APGAR*	*Assisted Ventilation*	*Death*	*Adverse Neonatal Health Index*
Prop. male 2010	-0.0201	-0.0290	-0.0523	-0.0776	0.0008	-0.2252
	(0.0698)	(0.0592)	(0.0336)	(0.0527)	(0.0109)	(0.1488)
Dependent variable mean	0.125	0.091	0.025	0.046	0.003	0.023
Observations	3,727,677	3,727,204	3,713,742	3,528,853	3,532,839	3,522,378
Sig. at 5% (Lee et al. 2022)	No	No	No	No	No	No
Wald KP (1st stage)	53.3	53.5	53.1	53.1	52.8	53.5

Notes: Negative *Diff. in Edu.* means that the father is more educated than the mother; *Father Less Educated* is an indicator for the father having strictly fewer years of education than the mother, and *Age Diff. (Father-Mother)* is the father's age minus the mother's age, in years. Each regression contains controls for cohort size in 2010, County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. The coefficient on *Prop. male 2010* correspond to β in equation 3. Sample of markets between 200-2000 people. Standard errors clustered at the County-Race level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A12: IV Results, 200-10000 Markets

Fertility Outcomes			
Dependent Variables:	Birth Rate	Birth Rate (marital)	Birth Rate (non-marital)
Prop. male 2010	-16.93	84.72**	-89.47**
	(54.57)	(40.34)	(38.94)
Dep. var. mean	81.400	46.840	33.894
Observations	155,314	155,314	155,314
Sig. at 5% (Lee et al. 2022)	No	Yes	Yes
Wald KP (1st stage)	58.804	58.804	58.804

Marriage Market Outcomes					
Dependent Variables:	*Unknown Father*	*Married*	*Diff. in Edu. (years)*	*Father Less Educated*	*Age Diff. (Father-Mother)*
Prop. male 2010	-0.3685***	0.7678***	-0.5424	-0.1442	-0.0469
	(0.1313)	(0.1860)	(0.5043)	(0.1162)	(1.176)
Dependent variable mean	0.124	0.622	0.354	0.338	2.50
Observations	10,548,074	10,973,113	9,011,860	9,011,860	9,239,918
Sig. at 5% (Lee et al. 2022)	Yes	Yes	No	No	No
Wald KP (1st stage)	108.8	111.2	91.7	91.7	88.6

Maternal Health Outcomes						
Dependent Variables:	*Chlamydia*	*Gonorrhea*	*Syphilis*	*Diabetes*	*Hypertension*	*Adverse Maternal Health Index*
Prop. male 2010	-0.0765***	-0.0047	0.0010	-0.0287*	-0.0830**	-0.3174**
	(0.0274)	(0.0095)	(0.0047)	(0.0163)	(0.0338)	(0.1262)
Dependent variable mean	0.018	0.003	0.0008	0.009	0.021	0.010
Observations	10,508,996	10,508,996	10,508,996	10,527,013	10,527,013	10,508,996
Sig. at 5% (Lee et al. 2022)	Yes	No	No	No	Yes	Yes
Wald KP (1st stage)	109.9	109.9	109.9	109.1	109.1	109.9

Infant Health Outcomes						
Dependent Variables:	*Preterm Birth*	*Low BW*	*Low APGAR*	*Assisted Ventilation*	*Death*	*Adverse Neonatal Health Index*
Prop. male 2010	-0.0955*	-0.0778*	-0.0534**	-0.0662*	-0.0033	-0.2961***
	(0.0526)	(0.0455)	(0.0237)	(0.0400)	(0.0082)	(0.1120)
Dependent variable mean	0.119	0.085	0.024	0.045	0.003	0.012
Observations	11,109,756	11,108,124	11,074,088	10,521,957	10,532,750	10,508,996
Sig. at 5% (Lee et al. 2022)	No	No	Yes	No	No	Yes
Wald KP (1st stage)	110.5	110.8	110.4	109.1	108.6	109.9

Notes: Negative *Diff. in Edu.* means that the father is more educated than the mother; *Father Less Educated* is an indicator for the father having strictly fewer years of education than the mother, and *Age Diff. (Father-Mother)* is the father's age minus the mother's age, in years. Each regression contains controls for cohort size in 2010, County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. The coefficient on *Prop. male 2010* correspond to β in equation 3. Sample of markets between 200-10000 people. Standard errors clustered at the County-Race level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A13: IV Results with Hispanics

Fertility Outcomes Dependent Variables:	Birth Rate	Birth Rate (marital)	Birth Rate (non-marital)
Prop. male 2010	-7.404	102.6**	-108.1***
	(66.53)	(44.91)	(37.18)
Dep. var. mean	73.938	42.240	31.310
Observations	142,087	142,087	142,087
Sig. at 5% (Lee et al. 2022)	No	Yes	Yes
Wald KP (1st stage)	39.515	39.515	39.515

Marriage Market Outcomes Dependent Variables:	*Unknown Father*	*Married*	*Diff. in Edu. (years)*	*Father Less Educated*	*Age Diff. (Father-Mother)*
Prop. male 2010	-0.4168***	0.7967***	-0.0574	-0.0296	-0.1482
	(0.1342)	(0.1933)	(0.5132)	(0.1196)	(1.215)
Dependent variable mean	0.126	0.615	0.360	0.337	2.54
Observations	7,847,933	8,171,602	6,683,688	6,683,688	6,858,435
Sig. at 5% (Lee et al. 2022)	Yes	Yes	No	No	No
Wald KP (1st stage)	101.2	102.8	84.1	84.1	81.3

Maternal Health Outcomes Dependent Variables:	*Chlamydia*	*Gonorrhea*	*Syphilis*	*Diabetes*	*Hypertension*	*Adverse Maternal Health Index*
Prop. male 2010	-0.0678**	-0.0052	-0.0022	-0.0341**	-0.0918***	-0.3346***
	(0.0270)	(0.0093)	(0.0049)	(0.0174)	(0.0318)	(0.1221)
Dependent variable mean	0.019	0.003	0.0008	0.010	0.021	0.003
Observations	7,815,379	7,815,379	7,815,379	7,831,290	7,831,290	7,815,379
Sig. at 5% (Lee et al. 2022)	Yes	No	No	No	Yes	Yes
Wald KP (1st stage)	102.2	102.2	102.2	101.6	101.6	102.2

Infant Health Outcomes Dependent Variables:	*Preterm Birth*	*Low BW*	*Low APGAR*	*Assisted Ventilation*	*Death*	*Adverse Neonatal Health Index*
Prop. male 2010	-0.0950*	-0.0587	-0.0472*	-0.0670	0.0001	-0.2620**
	(0.0553)	(0.0463)	(0.0253)	(0.0413)	(0.0084)	(0.1127)
Dependent variable mean	0.121	0.086	0.024	0.045	0.003	0.004
Observations	8,239,166	8,238,117	8,211,720	7,828,888	7,836,187	7,815,379
Sig. at 5% (Lee et al. 2022)	No	No	No	No	No	Yes
Wald KP (1st stage)	102.0	102.2	101.8	101.6	101.0	102.2

Notes: Birth Rate is the total number of births given in each year in 2011-2019 in a given dating market divided by the number women in that market (and year) and multiplied by 1000. Both marital and non-marital births use the same denominator. Fertility regressions are at the market level and contain controls for cohort size in 2010 and at birth, and interactions of cohort and race and, county and cohort, and year. Negative *Diff. in Edu.* means that the father is more educated than the mother; *Father Less Educated* is an indicator for the father having strictly fewer years of education than the mother, and *Age Diff. (Father-Mother)* is the father's age minus the mother's age, in years. The proportion of men in 2010 is instrumented with proportion of men at birth of the cohort. Each regression at the individual level (Marriage Market, and Health Outcomes) contains controls for cohort size in 2010 and at birth, County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. The coefficient on *Prop. male 2010* correspond to β in equation 3. Sample of markets between 200-5000 people. Hispanics are included both in the outcomes and in the endogenous variable. Standard errors clustered at the County-Race level in all regressions. Wald statistic (Kleibergen-Paap) for the first stage is presented together with an information whether the coefficient is significant at 5% according to tF statistic (Lee et al. 2022). *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A14: IV Results: Sex Ratio

First Stage						
Dependent Variable:	Sex ratio in 2010					
Sex ratio at birth	0.2280***					
	(0.0228)					
Observations	7,138,182					
Wald Kleibergen-Paap	99.60					
Fertility Outcomes						
Dependent Variables:	Birth Rate	Birth Rate (marital)	Birth Rate (non-marital)			
Sex ratio in 2010	4.907	29.62***	-22.61**			
	(14.18)	(10.32)	(9.334)			
Dependent variable mean	82.1	47.1	34.4			
Observations	142,064	142,064	142,064			
Sig. at 5% (Lee et al. 2022)	No	Yes	Yes			
Wald KP (1st stage)	44.0	44.0	44.0			
Marriage Market Outcomes						
Dependent Variables:	Unknown Father	Married	Diff. in Edu. (years)	Father Less Educated	Age Diff. (Father-Mother)	
Sex ratio in 2010	-0.0930***	0.1808***	-0.0098	-0.0087	-0.0523	
	(0.0311)	(0.0438)	(0.1227)	(0.0283)	(0.2856)	
Dependent variable mean	0.127	0.621	0.360	0.338	2.56	
Observations	7,166,343	7,478,536	6,105,173	6,105,173	6,259,559	
Sig. at 5% (Lee et al. 2022)	Yes	Yes	No	No	No	
Wald KP (1st stage)	98.3	102.7	83.3	83.3	80.1	
Maternal Health Outcomes						
Dependent Variables:	Chlamydia	Gonorrhea	Syphilis	Diabetes	Hypertension	Adverse Maternal Health Index
Sex ratio in 2010	-0.0165***	-0.0008	-0.0005	-0.0077*	-0.0238***	-0.0850***
	(0.0064)	(0.0022)	(0.0012)	(0.0041)	(0.0077)	(0.0301)
Dependent variable mean	0.019	0.003	0.0008	0.010	0.022	0.014
Observations	7,138,182	7,138,182	7,138,182	7,151,592	7,151,592	7,138,182
Sig. at 5% (Lee et al. 2022)	Yes	No	No	No	Yes	Yes
Wald KP (1st stage)	99.6	99.6	99.6	98.8	98.8	99.6
Infant Health Outcomes						
Dependent Variables:	Preterm Birth	Low BW	Low APGAR	Assisted Ventilation	Death	Adverse Neonatal Health Index
Sex ratio in 2010	-0.0193	-0.0164	-0.0129**	-0.0168*	-0.0004	-0.0689**
	(0.0132)	(0.0112)	(0.0061)	(0.0100)	(0.0020)	(0.0280)
Dependent variable mean	0.121	0.087	0.024	0.046	0.003	0.019
Observations	7,540,450	7,539,221	7,515,076	7,149,031	7,155,905	7,138,182
Sig. at 5% (Lee et al. 2022)	No	No	Yes	No	No	Yes
Wald KP (1st stage)	102.1	102.3	101.9	98.8	98.1	99.6

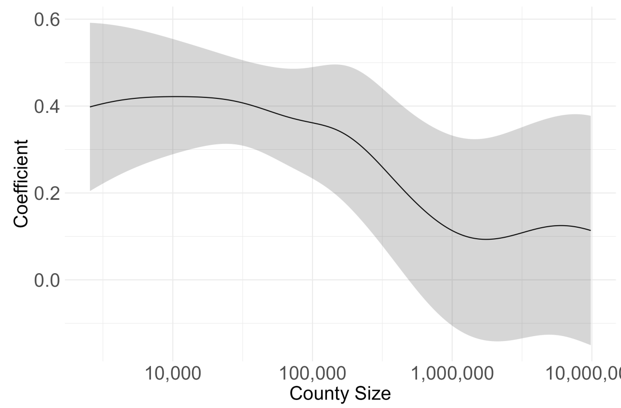
Notes: Negative *Diff. in Edu.* means that the father is more educated than the mother. The sex ratio in 2010 is instrumented with sex ratio at birth of the cohort. Each regression contains controls for cohort size in 2010 and at birth , County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. The coefficient on *Prop. male 2010* correspond to β in equation 3. Sample of markets between 200-5000 people. Standard errors are clustered at the County-Race level. Wald statistic (Kleibergen-Paap) for the first stage is presented at the bottom together with an information whether the coefficient is significant at 5% according to tF statistic (Lee et al. 2022). *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A15: First Stage by Rural-Urban Status

Dependent Variable:	Proportion	
Sample	Rural	Urban
Model:	(1)	(2)
Prop. male at birth	0.433***	0.164**
	(0.065)	(0.073)
R^2	0.91296	0.86783
Observations	8,695	5,514
Dependent variable mean	0.50098	0.49603

Notes: This market-level regression includes controls for cohort size at birth and in 2010, as well as fixed effects for county-cohort and race-cohort. Standard errors are clustered at the county-race level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A.7: First Stage by County Size



Notes: The graph presents coefficients from a local linear regression corresponding to the first stage along the log of the county size. County size refers to the total population of the county where the market is located. The regression is fitted using a Gaussian kernel. Standard errors are clustered at the county-race level, and only markets with population 200-5000 were used. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A16: Cross-County Marriage Effects

	By age							
	24		26		29		32	
	Female	Male	Female	Male	Female	Male	Female	Male
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prop. male at birth	0.037	0.038	0.029	0.006	0.051	0.071	0.076	0.063
	(0.070)	(0.049)	(0.070)	(0.056)	(0.068)	(0.060)	(0.066)	(0.060)
R^2	0.96128	0.94770	0.96689	0.95354	0.97140	0.96289	0.97197	0.96400
Observations	19,316	19,340	19,316	19,340	19,316	19,340	19,316	19,340
Dependent variable mean	0.31562	0.21905	0.38898	0.30209	0.45456	0.38693	0.48662	0.43618

Notes: The outcome variable is the proportion of men or women married at a given age. Population under consideration was born in 1978-1983 and is assigned to the county where they spent their childhood. Each observation represents a pair of counties *times* gender *times* race. *Prop. male at birth* measures the share of births during period 1978-1983 in the neighboring county and of the same race who were male. Each regression contains controls for log of the cohort size at birth in the outcome county, counties-pair and race fixed effects. Maximum effect, according to 95% confidence interval, of one standard deviation change in sex ratio at birth on neighboring county's marriage rate is in the range: (-0.001,0.0032). Standard errors are heteroskedacticity robust. Source: Opportunity Insights data Chetty et al. [2018]. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A17: Prenatal Care IV

Dependent Variables:	Month of Prenatal Care Start	Number of Visits
Model:	(1)	(2)
Prop. male 2010	0.1648	-1.129
	(0.4699)	(1.319)
Dependent variable mean	2.98	11.3
Observations	6,973,738	7,312,109
Sig. at 5% (Lee et al. 2022)	No	No
Wald KP (1st stage), Prop. male 2010	112.3	115.3

The proportion of men in 2010 is instrumented with the proportion of males at birth of the cohort. Controls include cohort size in 2010 and at birth, and fixed effects for county-age, race-age, and county-cohort. Counties are divided according to the 2013 Rural-Urban Continuum Codes. Non-metro areas are classified as rural. Standard errors are clustered at the county-race level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A18: Maternal and Neonatal health as function of parents Charactersitics

Variables	Married	Mother's Edu.	Father's Edu.	Overweight
Chlamydia	-0.0145***	-0.0002***	-0.0009***	-0.0012***
	(0.0002)	(3.45×10^{-5})	(3.93×10^{-5})	(0.0001)
Gonorrhea	-0.0018***	-0.0001***	-0.0001***	-0.0002***
	(6.34×10^{-5})	(1.25×10^{-5})	(1.22×10^{-5})	(3.83×10^{-5})
Syphilis	-0.0004***	-0.0001***	8.09×10^{-6}	2.85×10^{-5}
	(3.54×10^{-5})	(1.05×10^{-5})	(8.06×10^{-6})	(2.11×10^{-5})
Diabetes	-6.63×10^{-5}	-0.0008***	-0.0004***	0.0085***
	(0.0001)	(3.53×10^{-5})	(3.31×10^{-5})	(0.0001)
Hypertension	-0.0018***	-0.0002***	-0.0009***	0.0211***
	(0.0002)	(5.36×10^{-5})	(5.06×10^{-5})	(0.0002)
MH Index	-0.0333***	-0.0037***	-0.0037***	0.0441***
	(0.0007)	(0.0002)	(0.0002)	(0.0005)
Preterm Birth	-0.0157***	-0.0041***	-0.0020***	0.0048***
	(0.0005)	(0.0001)	(0.0001)	(0.0004)
Low Birthweight	-0.0165***	-0.0029***	-0.0012***	-0.0078***
	(0.0004)	(0.0001)	(0.0001)	(0.0004)
Low APGAR	-0.0018***	-0.0003***	-0.0004***	0.0044***
	(0.0002)	(5.99×10^{-5})	(5.96×10^{-5})	(0.0002)
Assist. Vent.	-0.0041***	-0.0005***	-0.0004***	0.0086***
	(0.0003)	(7.77×10^{-5})	(7.19×10^{-5})	(0.0002)
Death	8.95×10^{-5}	-0.0001***	$-5.1 \times 10^{-5***}$	0.0008***
	(6.24×10^{-5})	(1.63×10^{-5})	(1.6×10^{-5})	(4.64×10^{-5})
NH Index	-0.0272***	-0.0058***	-0.0031***	0.0141***
	(0.0008)	(0.0003)	(0.0002)	(0.0007)

This table presents estimates from ordinary least squares (OLS) regressions using data on births from 2011 to 2019 in the IV sample. Each row corresponds to a specific health outcome, and columns indicate different regressor. The regressions includes the same controls, fixed effects and clustering as the main specification. The variable "Overweight" refers specifically to the mother's overweight status. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.3 Age cohorts, partner age gaps, and implications for the market definition

Age is used as a defining dimension of the dating market using five-year age groups (15–19, 20–24, 25–29, 30–34), following the smallest consistently available cells in Census 2010 tabulations. This cohort-based approach likely does not coincide perfectly with the full set of partners individuals may consider in practice. However, the objective of this market definition is not to recover the entire partner search space, but to capture a segment of the partner pool that is sufficiently large to influence outside options and relative bargaining positions. Changes in the composition of even part of the relevant partner set can alter dating-market conditions and, in turn, behavior. Nonetheless, if the misalignment were substantial, it could affect the interpretation of the estimated effects. For this reason, I document in this section how observed age differences in partnerships relate to the empirical market definition used in the main analysis.

Evidence from dating applications

I begin by documenting evidence on age preferences using online dating data. These sources provide information on search behavior and stated preferences.

Evidence from the dating website OkCupid [Rudder, 2010], indicates that preferred partner ages are gendered and widen with age. For men, stated acceptable partner ranges are initially concentrated near their own age—around 18 to 21 at age 18—but expand over the life cycle, reaching roughly 19 to 28 by age 24 and approximately 24 to 38 by age 34. Women's stated ranges broadens less and remain centered on somewhat older partners, shifting from about 18 to 24 at age 18 to roughly 24 to 31 by age 24 and to around 30 to 40 by age 34.

Additional evidence from online dating behavior using a European mobile dating application [Šetinová and Topinková, 2021] documents similar gendered patterns. For instance, among men ages 18–23, roughly 50–70% of invitations target women of similar age, while by age 30 only about 25% do so. Among women ages 18–23, only around 30–40% of invitations target men of the same age group, with the remainder directed toward older partners.

These findings indicate that age preferences are asymmetric and gendered—men tend to target slightly younger women, while women more often target somewhat older men—but they also show that search behavior spans adjacent ages rather than forming sharply segmented markets.

Because stated preference data do not necessarily reflect the effective set of partners available in equilibrium, I next examine realized matches using ACS and natality data.

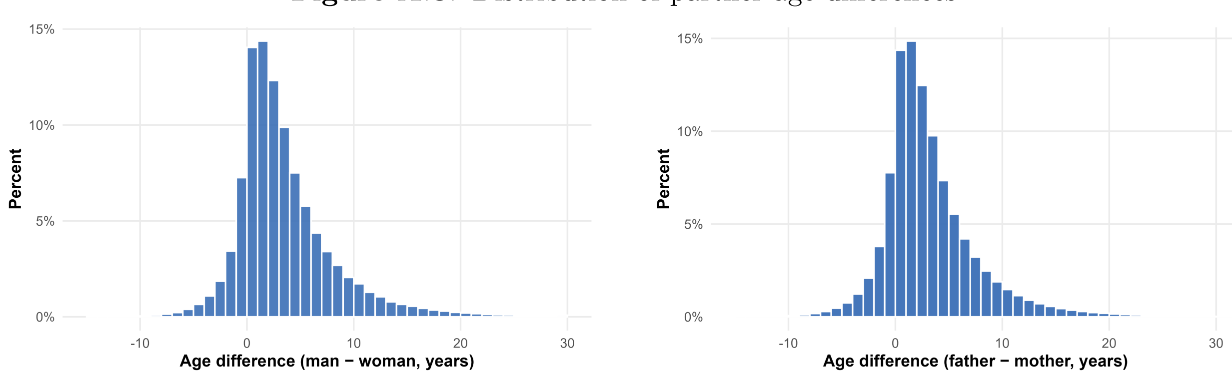
Evidence from cohabitation, marriage and births

Using ACS 2015 five-year data for women ages 15–34, the average age difference between partners (pooling married and cohabiting couples) is approximately three years, with men older. Across age groups, roughly 75–80% of couples form within five years of age, and approximately 40–50% fall within the same five-year cohort used in the empirical market definition.

Natality data (for women 15-34) show very similar patterns. Fathers are typically 2.7 years older than mothers. Approximately 80% of births occur to parents whose ages differ by at most five years, and about 48% of couples fall within the same five-year cohort.

Figure A.8 illustrate these patterns. The distributions are concentrated near small positive age gaps, and large differences are comparatively rare.

Figure A.8: Distribution of partner age differences



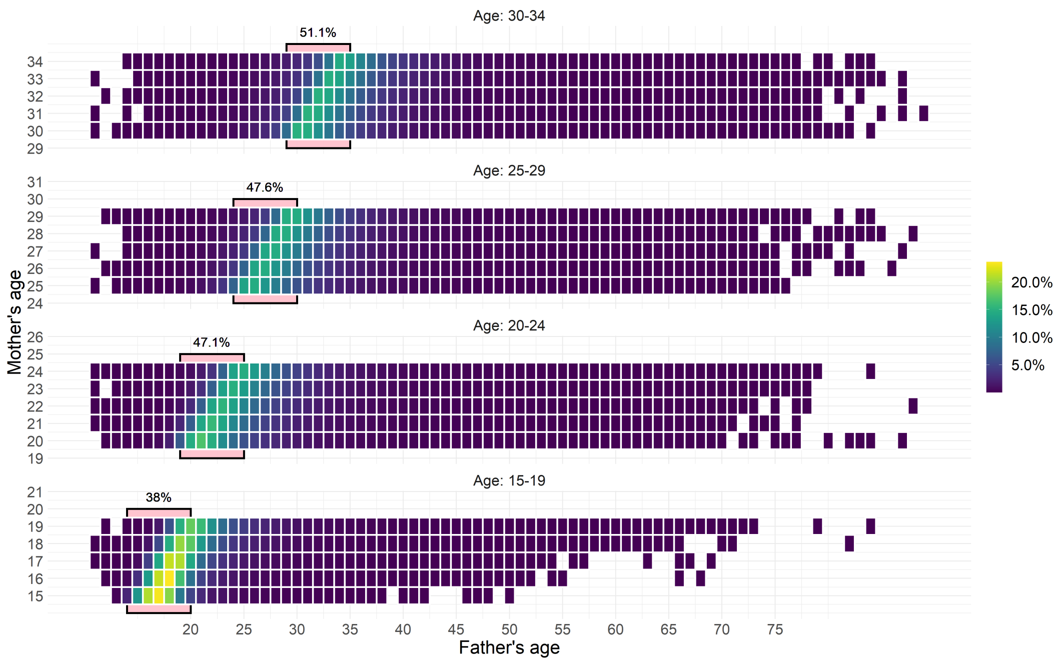
(a) ACS couples

(b) Natality data

Notes: Histograms show the distribution of age differences (man minus woman). The American Community Survey sample (2015 5-year) includes married and cohabiting couples with women ages 15–34. The natality sample corresponds to the full birth records used in the analysis.

More detailed evidence is provided in Figure A.9, which plots the joint distribution of mothers' and fathers' ages. The heatmap shows a strong concentration along the diagonal, indicating that most partnerships occur between individuals of similar age, with fathers typically only slightly older.

Figure A.9: Age Composition of Parents



Notes: Small rectangles show couples by father's age a_f and mother's age a_m. Colors indicate the share of mothers aged a_m with fathers aged a_f, with light colors on the diagonal showing most women have children with men of similar age. Larger boxes represent 5-year age groups, and the number above indicates the share of mothers in that age group with similarly aged fathers. Source: Natality data 2011-2019

The realized data confirm that men are typically older than their partners, which implies that some partnerships necessarily span adjacent cohorts. At the same time, the fact that roughly 40–50% of couples fall within the same five-year cohort indicates that the empirical definition still captures a substantial portion of the relevant partner pool.

Measurement Implications

Because some partnerships span adjacent age cohorts, the empirical market definition necessarily understates the true set of potential partners. This induces measurement error in the constructed sex-composition variable: the empirical measure captures only a subset of the relevant partner pool, while effective outside options also depend on nearby cohorts. As a result, changes in measured sex composition reflect shifts in only part of the relevant market, so the effective variation in partner availability is diluted, implying attenuation of the estimated coefficients toward zero (see Appendix B.10 for a formal derivation).

Second, even within the defined cohort, the effective supply of potential partners is unlikely to be uniform across all ages. If individuals place greater weight on certain segments of the cohort—for example, if women are more likely to consider slightly older men as viable partners—then variation in sex composition arising from less relevant ages will be only weakly related to effective outside options. This introduces additional noise into the market measure, lowers the signal-to-noise ratio, and again biases the estimated effects toward zero rather than generating spurious relationships.

A.4 Details on the decomposition of racial differences in the sex composition

To quantify how various factors contribute to racial disparities in the sex ratio, I analyze how the disparity changes when racial differences in each factor are eliminated. I construct a simple model where the number of available mates depends on parameters like incarceration rates, mortality rates, and immigration rates. By comparing the proportion of men in the actual situation to a hypothetical scenario where a chosen parameter equals that of White people (the reference group), I assess the impact of that specific factor.

Below are the derivations for racial differences at the national level. This method can be further disaggregated, and I also present race- and cohort-specific results (figure A.11). This decomposition was inspired by Hall [2000]'s analysis of changes in the Black sex ratio in the late 20th century.

Consider incarceration as an example of a factor affecting Black-White sex composition differences. The number of Black men and women in dating markets is calculated by multiplying the total number of Black men and women by the complement of gender-specific incarceration rates[33]. To assess the impact, I replace Black incarceration rates with White ones while holding other factors constant, then calculate the proportion of Black men under these

[33] i.e. $N_{bm}(1 - i_{bm})$, where N_{bm} is the number of Black men and i_{bm} is the race- and gender-specific incarceration rate

conditions. Comparing the actual and counterfactual sex compositions reveals incarceration's contribution to the Black-White sex composition gap Formally, let N_{rs} be the number of people of race r and sex s. This number can be decomposed in the part born in the US (B_{rs}) and foreign born (IM_{rs}): $N_{rs} = \underbrace{B_{rs}}_{\text{US Born}} + \underbrace{IM_{rs}}_{\text{Foreign Born}}$.

I model the number of domestically born individuals of race r and sex s on the dating market in 2010 as follows. First, I multiply a hypothetical base population by the share born in the US $(1 - w_r)$, where w_r is the share of race r born abroad. This gives all domestic births for race r. Then, I multiply it by the probability pb_{rs} that a birth is of sex s, yielding the total domestic births of sex s and race r. Next, I apply the survival rate $(1 - m_{rs})$, where m_{rs} is the mortality rate, to get those still alive in 2010. Finally, I multiply by the probability they are not incarcerated $(1 - i_{rs})$, where i_{rs} is the incarceration rate for race r and sex s. Incarceration and mortality can also be disaggregated by cause or offense.

$$B_{rs} = \underbrace{BP_r}_{\substack{\text{Base}\\\text{Population}}} \underbrace{(1 - w_r)}_{\substack{\text{Proportion}\\\text{local born}}} \underbrace{pb_{rs}}_{\substack{\text{Probability that}\\\text{birth is of sex } s}} (1 - \underbrace{m_{rs}}_{\substack{\text{Mortality}\\\text{rate}}})(1 - \underbrace{i_{rs}}_{\substack{\text{Incarceration}\\\text{rate}}})$$

The term for the foreign-born population is similar, with two modifications. The product $BP_r * w_r$ represents the baseline immigrant population of race r arriving before 2010. This is then multiplied by pi_{rs}, the proportion of sex s among immigrants of race r. Thus, Im_{rs} represents all foreign-born individuals of race r and sex s who are alive and not incarcerated in 2010.

$$Im_{rs} = \underbrace{BP_r}_{\substack{\text{Base}\\\text{Population}}} \underbrace{w_r}_{\substack{\text{Proportion}\\\text{foreign born}}} \underbrace{pi_{rs}}_{\substack{\text{Probability that}\\\text{immigrant is of sex } s}} (1 - \underbrace{m_{rs}}_{\substack{\text{Mortality}\\\text{rate}}})(1 - \underbrace{i_{rs}}_{\substack{\text{Incarceration}\\\text{rate}}})$$

Parameters N_{rs},w_r, pi_{rs}, pb_{rs}, m_{rs}, and i_{rs} were computed from administrative data sources such as census and vital statistics. The details of the parameters computation and additional assumption are below. In the next step, I calculate the value of the residual X_r which represents all unaccounted factors affecting sex composition. It is fitted through equating the empirical sex composition Pm_r to the predicted sex composition $\frac{N_{rm}}{N_{rm}+N_{rf}}$ multiplied by X_r: $Pm_r = \frac{N_{rm}}{N_{rm}+N_{rf}} * X_r$. Note that this last fraction is a function of parameters and can be used to predict counterfactual sex compositions under different parameter values. I substitute the male and female parameters for race r with those of White males and females in the same age group to obtain a counterfactual sex composition without racial differences in that parameter. For example, to calculate the impact of incarceration on Black-White sex composition differences: (1) Replace Black incarceration rates with those of Whites; (2) Compute the counterfactual proportion of Black males using the new incarceration rates while keeping other parameters fixed; (3) Calculate the counterfactual sex composition disparity. The difference between the counterfactual and empirical results gives the contribution of incarceration.

Parameters computations The incarceration rate i_{rs} is calculated from the 2010 Census as the ratio of the population aged 15-34 of race r and sex s living on prison census blocks to the total population of the same group. Offense-specific rates are obtained by multiplying i_{rs} by the share of prisoners of race r and sex s sentenced for specific offenses, sourced from the BJS CSAT tool[34]. The mortality rate m_{rs} is derived from vital statistics by counting deaths of individuals of race r and sex s born between 1976 and 1996, up to 2009. I calculate death rates by dividing total deaths by the sum of those alive in 2010 and deaths. Cause-specific mortality (natural, violent, external) is classified per ICD9/ICD10. The probability of a birth being sex s in race r is derived from 1976-1996 natality data as the ratio of births of sex s to total births of race r. The share of the foreign-born population is calculated from 2010 census microdata, as is the probability that an immigrant of race r is of sex s.

Parameters computations Several simplifying assumptions are necessary due to data limitations. First, death and incarceration rates are assumed the same for both local-born and immigrant populations, as I cannot differentiate them in mortality data. Second, Hispanics

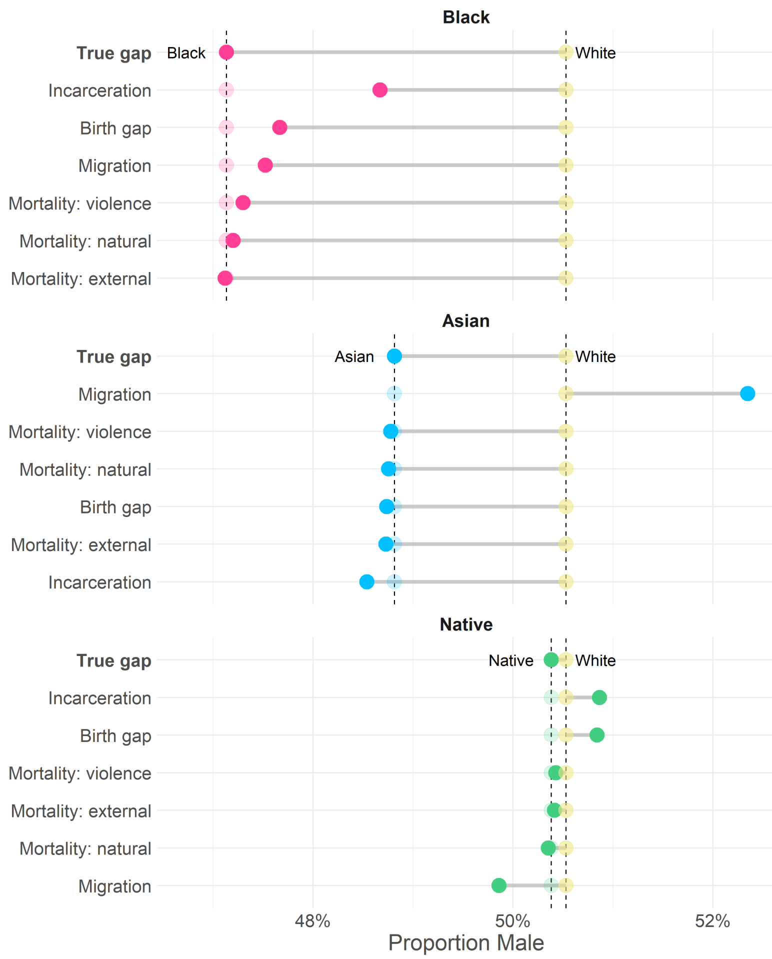
[34] https://csat.bjs.ojp.gov/advanced-query

are included for all races, since I can only distinguish them in the mortality dataset starting from 1989, while my first cohort was born in 1976. Third, the proportion of foreign-born is based on 2010 data, which already reflects mortality, though I assume it does not. The same applies to the male proportion among immigrants, but I adjust using mortality data. Fourth, I do not manipulate the relative shares of US-born and foreign-born populations. Lastly, I do not account for interactions between multiple parameters.

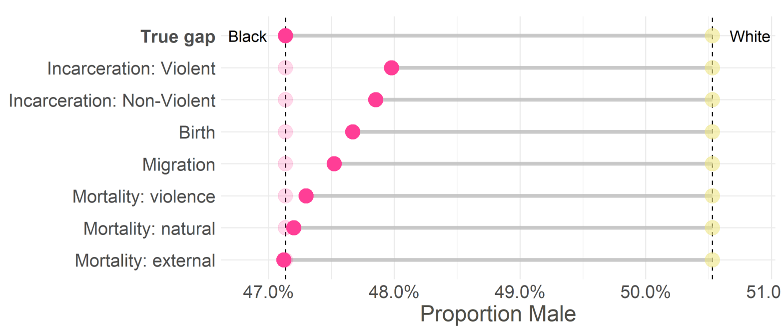
Main results Figure A.10a demonstrates the primary factors driving the racial differences in sex composition. The x axis represents the proportion of men under each scenario, and y axis shows the parameters, ordered by their importance for each race. The first row in each panel illustrates the actual values of the sex compositions.

For Black people, incarceration is the largest factor, explaining 45% of the gap. A 15% contribution comes from the lower proportion of male births among Black people globally. Violent deaths account for 5%. Among Asians, migration is the key driver of male scarcity. Differences between Native and White Americans are mostly negligible. The desegregation by cohort shows that these gaps mostly arise above the age 20 for Black people and above the age 26 for Asian people.

Figure A.10: Counterfactual Gaps in the Sex Composition



(a) All Racial Groups



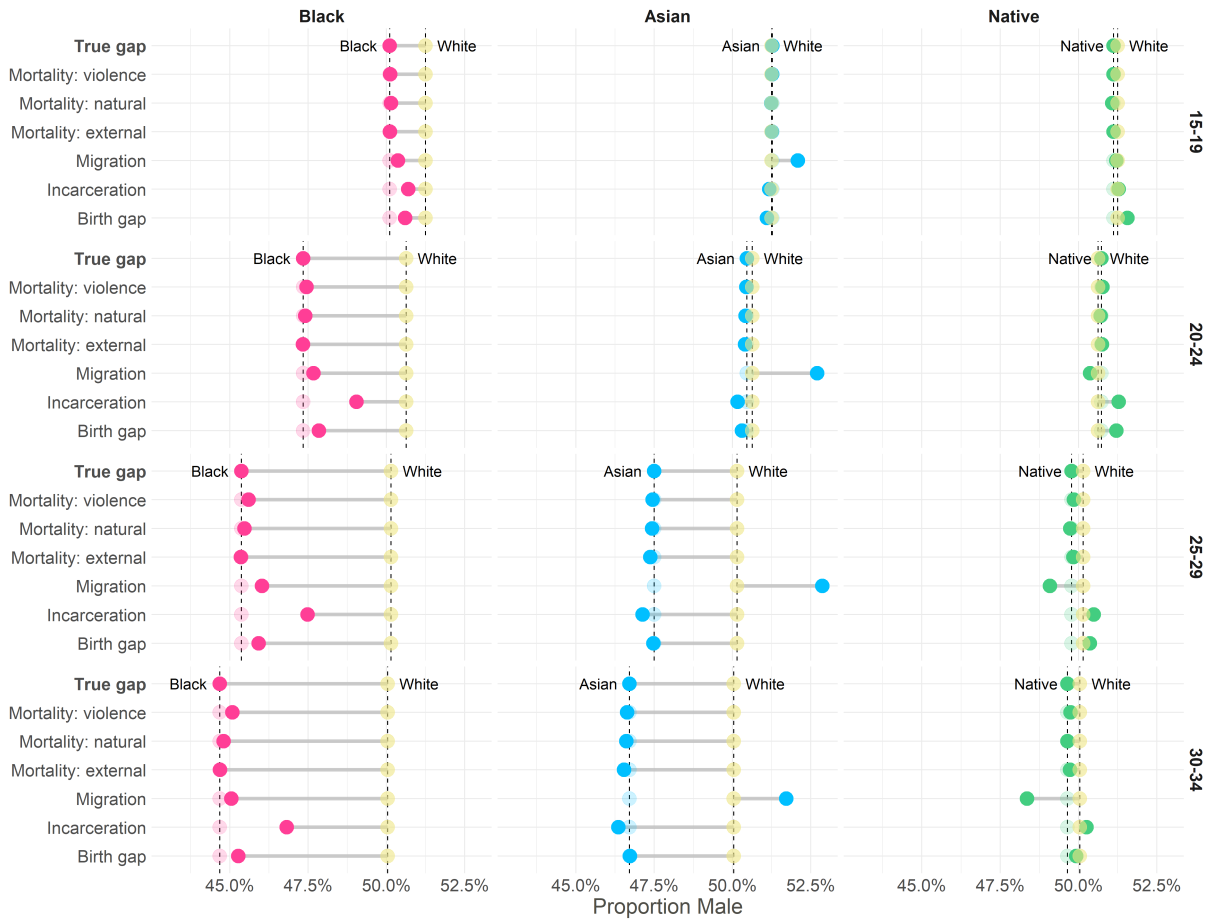
(b) Desegregated by Crime Type

Notes: Lines show the counterfactual gap (for the cohort 15-34 in 2010) that would arise if rates for a given factor were equalized to the value of White people. The dashed lines represent the true sex compositions.

Differences in sex ratios between Black and White people are policy sensitive. They come from biases interacting with legislation which prescribe harsher sentences for habitual offenders or particular drugs[35]. An example in case was 100:1 sentencing disparity between crack cocaine, used disproportionately by Black people, and powder cocaine, consumed by White people[36]. Even race-neutral reforms, like changes to probation violations, can disproportionately impact minorities (Rose [2021]). Policies that reduce over-reliance on incarceration and address biases in the justice system could narrow the incarceration gap. Raphael and Stoll [2014] suggest eliminating mandatory minimums and reducing "truth in sentencing" laws to lower incarceration rates without risking public safety. Other reforms include expanding bail and sentencing options, increasing diversity in the legal profession, diverting drug offenders to treatment, and mandating racial impact analyses of legislation (Ghandnoosh [2015]).

[35]See for instance: Gelman et al. [2007], Rehavi and Starr [2014], Arnold et al. [2018]
[36]Reduced to 18:1 by The Fair Sentencing Act of 2010

Figure A.11: Counterfactual Gaps in the Sex Composition: by Cohort



Notes: Each line shows the counterfactual gap (for the cohort 15-34 in 2010) that would arise if rates for a given factor were equalized to the value of White people. The dashed lines represent the true sex compositions.

A.5 Assigning Incarcerated Individuals to Their Communities

I first use census data on prison blocks to calculate the number of incarcerated individuals by state, race (Black and White), gender, and age group, denoted Inc_census_{srga}. I assume all individuals are incarcerated in their state of residence, though this may not hold for federal prisons, which house a small share of inmates. Next, I use Vera [2022] data, which provides inmate counts by year, race, and county of commitment. Let Inc_vera_{cr} represent the number of inmates of race r from county c. I average counts from 2008-2012 to address missing data and compute the share of inmates from each race contributing to the state's inmate population as $Share^s_{cr} = \frac{Inc\_vera_{cr}}{\sum_{c \in s} Inc\_vera_{cr}}$. I will use this to redistribute them to counties, assuming that count of inmates from county c, race r, age group a and gender g is $Inc\_census_{crga} = Share^{s}_{cr} * Inc\_census_{srga}$.

The simulation equates incarceration rates for non-violent offenses between Black and White people. Since detailed geographic data isn't available, I use national race- and gender-specific shares of non-violent inmates ($Share_N V_{rg}$) from BJS CSAT. I calculate the number of non-violent inmates as $Inc\_census_N V_{crga} = Share_N V_{rg} * Inc\_census_{crga}$, which allows me to estimate the share of dating market participants incarcerated for non-violent crimes.

B Identification: Exogeneity and Exclusion Restriction

B.1 Simulated Distribution of Sex Composition at Birth

I conduct an exercise showing that empirical and simulated distributions of sex compositions at birth are identical, as visualized in Figure B.12. First, I calculate the mean proportion of male births (p_r) for each race, assuming randomness in sex ratio conditional on race. Then, for each market, I simulate n_{cra} "coin tosses" with probability p_r, where n_{cra} is the number of births, repeating the process 100 times. If sex at birth is truly a random "coin toss", the empirical and simulated distribution should be similar[37]. The resulting simulated distribution mirrors the empirical one, as sex at birth seems to follow a Bernoulli process. Both distributions are visually identical, and Kolmogorov-Smirnov tests confirm no significant differences (table A19), with p-values above traditional significance levels, though close to 0.13-0.15 for Black and White populations.

[37] Note that they mechanically have the same mean

Figure B.12: Actual vs Simulated Density of Proportion of Male Births

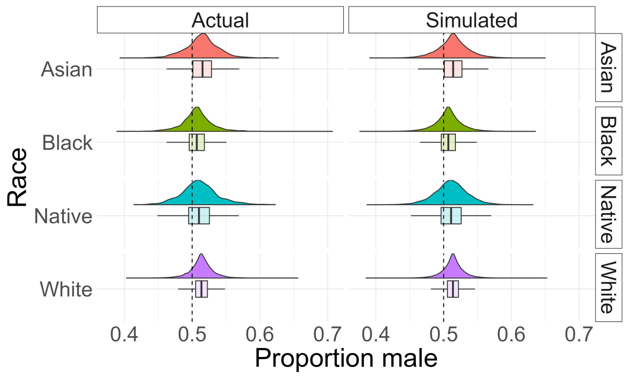


Table A19: Kolmogorov-Smirnov Test

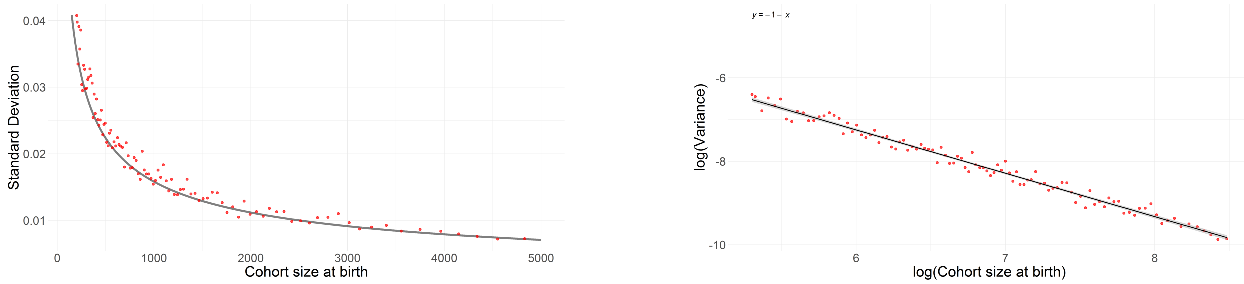
Race	P-value
Asian	0.728
Black	0.155
White	0.1303
Native	0.921

Notes: In the left figure, the left panel shows the empirical distribution of sex compositions where each observation represents the proportion of male births at a dating market when the cohort was born. The right panel shows the simulated distribution. Simulations are draws from the binomial distribution with parameters p_r and n_{cra}, and divided by n_{cra}. The table on the right shows p-values from Kolmogorov-Smirnov tests for the hypothesis that the empirical and simulated distributions in the figure B.12 are equal.

B.2 Relationship between Cohort Size and Variation in Sex Composition

Deviations from a balanced sex ratio are expected to be small in large cohorts but substantial in small ones, and this pattern holds in the data. Panel a of Figure B.13 shows the theoretical standard deviation by cohort size (n) using $p = 0.5$ and $\sqrt{\frac{p(1-p)}{n}}$, alongside empirical values. Markets are divided by birth cohort size percentiles, and the standard deviation of proportion male is plotted against average size. The close match between theoretical and empirical values suggests that observed variation is largely due to randomness in sex at birth. I also test this relationship formally. Taking logs of the theoretical variance gives $log(var) = log(p(1-p)) - log(n)$, so regressing the log of empirical variance on log cohort size should yield a coefficient of -1, as shown in panel b. A bootstrap regression with 10,000 samples produces a mean coefficient of -1.025 with a 95% confidence interval of (-1.052, -0.998).

Figure B.13: Variation in the Sex Composition



(a) Theoretical and Empirical Standard deviation in Sex Ratio at Birth

(b) Log of Theoretical and Empirical Standard deviation in Sex Ratio at Birth

Notes: The curve in figure B.13a shows the theoretical standard deviation by sample size n using p=0.5 and $\sqrt{\frac{p(1-p)}{n}}$. The dots represent the standard deviation in the data. Specifically, markets were divided by the percentiles of the size of the birth cohort. Each dot represents a group of markets in a percentile. Standard deviation and average size are calculated in each percentile. Figure B.13b shows the relationship between log of the variance in the centiles of data and the average cohort size of the centile, and a fitted regression line.

B.3 Effects of socio-economic, health, and environmental factors on sex at birth

My main results rely on the assumption that no third variable drives both a cohort's sex composition at birth and its pregnancy outcomes about twenty years later. A natural concern is that socioeconomic, health, or environmental conditions might shape sex ratios at birth and also affect subsequent maternal health, thereby threatening identification. According to the fragile-male hypothesis, adverse conditions in utero reduce the probability of male births, and if such conditions were correlated with later-life outcomes, this could generate spurious relationships. To address this concern, I systematically examine socioeconomic factors (maternal education, marital status, and local unemployment), maternal health conditions, and environmental exposures (air pollution), and I also test for intergenerational persistence in sex ratios. Across all of these dimensions, the evidence consistently points to effects that are statistically weak and economically negligible.

First, using my primary analysis sample of the Natality Data, I analyze the relationship between a mother's education or marital status and her newborn's sex. In each case, I regress the likelihood of a male newborn on these variables and fixed effects (Table A20) and include all measures in a combined regression. Finally, I also add as a regressor the proportion of male births.

Table A20: Mother's characteristics and Sex at Birth

Dependent Variable:	Male birth								
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
High School	0.0012*								0.0012
	(0.0007)								(0.0007)
Between HS and C	0.0011								0.0012*
	(0.0007)								(0.0007)
College or more	0.0013*								0.0014*
	(0.0007)								(0.0008)
Married		-0.0003							-0.0004
		(0.0005)							(0.0005)
Chlamydia			-0.0005						-0.0006
			(0.0014)						(0.0015)
Gonorrhea				0.0011					0.0013
				(0.0035)					(0.0035)
Syphilis					-0.0066				-0.0057
					(0.0066)				(0.0066)
Diabetes						0.0026			0.0024
						(0.0019)			(0.0019)
Hypertension							0.0005		0.0004
							(0.0013)		(0.0013)
Prop. male at birth								0.0117	0.0138
								(0.0158)	(0.0161)
Fit statistics									
Dependent variable mean	0.512	0.512	0.512	0.512	0.512	0.512	0.512	0.512	0.512
Observations	7,138,182	7,070,315	7,138,182	7,138,182	7,138,182	7,138,182	7,138,182	7,138,182	7,070,315

Notes: Outcome variable is a dummy equal to one if a male is born. The controls are the same as in the main specification (controls for cohort size in 2010 and at birth, County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects). Mother's education can have 4 levels: (excluded) less than high school, high school, between high school and college, and college or more. Standard errors are clustered at the County-Race level. Analysis is done for the same sample as the main specification. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Data source: Natality Data.

I do find some patterns in Table A28 that are in line with the Trivers–Willard hypothesis and consistent with for instance Almond and Edlund [2007]. Mothers with high school or college education are marginally more likely to give birth to boys relative to mothers with less than high school (coefficients of 0.0012–0.0013, significant at the 10% level)[38]. This is the expected direction: education proxies for socioeconomic status, and higher-status mothers are hypothesized to produce more sons. However, the magnitudes are economically trivial. For example, the effect of high school completion implies a 95% confidence interval of [0.00017, 0.00257]. Translating this into market-level terms, even a large 10 percentage point increase

[38] Magnitudes are virtually identical in the full natality sample of over 23 million births, though estimated with greater precision.

in the share of high-school educated mothers would shift the sex composition at birth by only 0.00012, or 0.00026 at the upper bound. Compared to the observed cross-market standard deviation in sex composition, this amounts to just 0.6–1.2% of actual dispersion. Moreover, even under extreme assumptions, the amount of variation in maternal education across markets is insufficient to be a significant factor. For example, the cross-market standard deviation in years of schooling is only 0.91.

I also analyze the relationship between maternal health conditions and the sex of the child by regressing the probability of giving birth to a male child on indicators for maternal diseases available in the dataset: Chlamydia, Syphilis, Gonorrhea, Hepatitis B, pre-existing Diabetes, and pre-existing Hypertension. The results reveal no statistically significant associations, and the estimated coefficients exhibit inconsistent signs. A 95% confidence interval for these estimates rules out any impact greater than 0.5 percentage points in favor of male birth. While other health measures not captured here might influence male birth, the lack of consistent effects even for severe diseases like syphilis, which result in high mortality, challenges the hypothesis that male fetuses are more sensitive to maternal health and that this drives sex ratio patterns.

Finally, for such a mechanism to matter, effects would need to persist across generations. If healthier mothers were indeed more likely to give birth to sons, and if these health advantages were transmitted to the next generation, then cohorts born in markets with unusually high sex ratios should themselves go on to reproduce at higher sex ratios. This provides a natural placebo test of the Trivers–Willard channel. In practice, when I regress the sex of the child on the proportion male in the mother's own birth cohort, the estimated coefficient is essentially zero, with a 95% confidence interval of [0.0004, 0.0009]. In other words, high-sex-ratio cohorts do not translate into higher sex ratios when those cohorts reproduce. This indicates that the intergenerational link, if it exists, is far too weak to generate persistent effects.

Next, I demonstrate that economic conditions, proxied by unemployment during pregnancy, do not affect sex composition. I regress the sex composition of births on unemployment levels at the time of delivery and throughout pregnancy, allowing for differential effects based on exposure timing. Monthly county-level sex composition (2003-2020) comes from the Natality data, with unemployment data sourced from FRED. I estimate both an OLS and IV model using a Bartik-type instrument. The OLS follows this equation:

$$Prop.Male_{c,t} = \sum_{lag:0}^{10} \beta^{lag} Unemployment_{c,t-lag} + \gamma_c + \delta_t + \epsilon_{c,t} \tag{4}$$

The outcome variable is the proportion of male births in county c during month-year t. The main independent variable, $Unemployment_{c,t-lag}$, represents the (lagged) unemployment level in county c at time $t - lag$. A lag of 0 refers to unemployment during the delivery month, while a lag of 10 refers to ten months prior. I include county γ_c and time fixed effects δ_t.

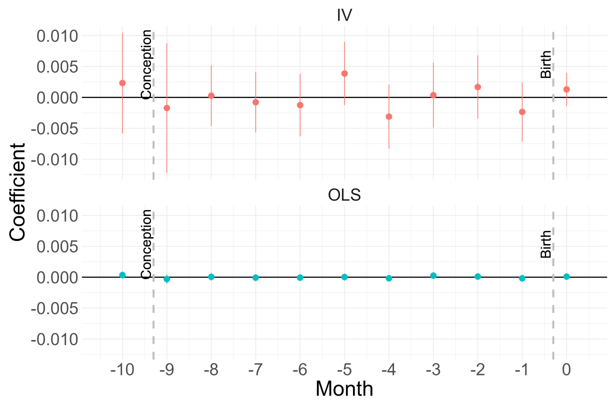
The IV framework uses a shift-share instrument to capture exogenous variation in unemployment based on county industry shares[39] and national industry-level monthly unemployment rates. Figure B.14 shows the results.

The regressions find no evidence that unemployment during pregnancy affects sex at birth. OLS coefficients are near zero with tight confidence intervals, and the IV results are similar. The instruments are strong, with Kleinberg-Paap Wald statistics between 24 and 44. Coefficients on all unemployment lags are insignificant. Hence, I conclude that the exposure to booms and recessions, as proxied by unemployment during pregnancy, does not influence sex at birth. Considering 95% confidence, the largest positive and negative impact (among all lags) of 1% change in unemployment on the probability of male birth would be {-0.0011, 0.0009}.

Similarly, one might argue that the relationship between pollution and the sex ratio at birth could be a confounder, as pollution is associated with a variety of other outcomes. However, I find no association between pollution levels during pregnancy and the sex ratio at birth in my sample. I regress the proportion of male births on the lagged Air Quality Index (AQI) for Ozone and PM2.5, following the specification in Equation 4:

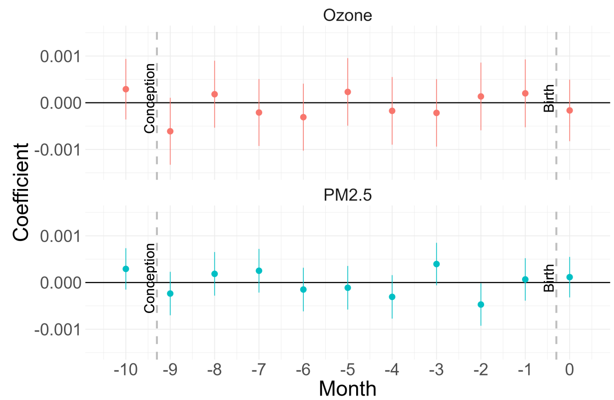
[39]Industry shares from 2000 Census summary file P049

Figure B.14: Unemployment and Sex at Birth



Notes: Each plot depicts coefficients from the regression 4. The IV is based on the Bartik-type instrument. Errors are clustered at the county level.

Figure B.15: Pollution and Sex at Birth



Notes: Each plot depicts coefficients from the regression 5. Estimated on the sample between years 2003 and 2020. Errors are clustered at the county level.

$$Prop.Male_{c,t} = \sum_{lag:0}^{10} \beta_P^{lag} AQI_{c,t-lag} + \gamma_c + \delta_t + \epsilon_{c,t} \tag{5}$$

Where β_P^{lag} captures the effect of AQI changes in the lag months before birth on the probability of a male birth. As shown in Figure B.15, there is no significant relationship between pollution and the sex ratio at birth. Considering 95% confidence interval, the largest positive and negative impact (among all lags) of 1 unit change in the AQI on the probability of male birth would be { -0.00133,0.00095}. These findings suggest that sex at birth in the US is not influenced by maternal economic status, aggregate economic fluctuations (proxied by unemployment), or pollution (proxied by AQI).

B.4 Sex-selective abortion

A potential concern is that sex-selective abortion could bias the instrument if son preference alters the sex ratio at birth and also influences maternal health in the next generation. Evidence, however, suggests that the magnitude of this channel in the U.S. is negligible. Abrevaya [2009] documents sex-selective abortion only among Chinese and Indian mothers, estimating that around 2000 female births were missing between 1992 and 2004—equivalent to just 0.04% of Asian births. Even if this rate applied during my study period, it would shift the sex ratio in the Asian category by only 0.09 percentage points, or 3% of a standard deviation. Moreover, the direction of bias would likely run against my hypothesis: girls born in son-preference communities experience worse outcomes (Borooah [2004], Bharadwaj and Lakdawala [2013], Barcellos et al. [2014], Almond and Cheng [2020], Blau et al. [2020]), implying that any health effects of sex-selective abortion would tend to weaken, not strengthen, my results. Therefore, sex-selective abortion is both too small in magnitude and likely signed in the opposite direction to threaten my identification strategy.

B.5 Stopping rules

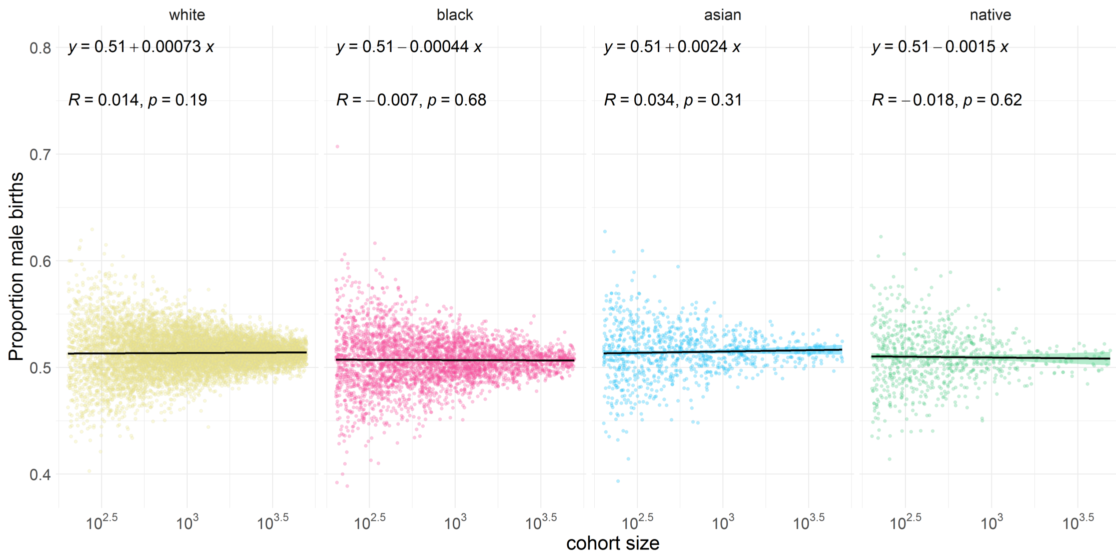
A potential concern is that stopping rules—where parents continue having children until reaching a desired gender—could bias the instrument if son preference is present. In this case, smaller cohorts might contain disproportionately more boys, and because smaller cohorts could also receive greater parental investments, such a mechanism could confound my estimates.

Empirically, however, there is no evidence for this concern. Figure B.16 shows that across all racial groups, the proportion of male births is unrelated to cohort size, ruling out systematic patterns consistent with stopping rules.

This conclusion is reinforced by prior work. While parity has been shown to have a modest association with sex ratios at birth [Almond and Edlund, 2008], the magnitude is far too small to explain the observed variation in my data. To generate the cross-county differences I exploit, parity would need to vary implausibly across markets—on the order of 33 children per mother—given that each additional birth reduces the probability of a male birth by only 0.06 percentage points (Appendix Table A21).

Taken together, both the absence of empirical patterns in the data and the implausibility of the required parity variation imply that stopping rules do not play a meaningful role in shaping sex ratios at birth in my sample.

Figure B.16: Birth Cohort Size vs Proportion Male at Birth



Notes: Each dot on the figure represents a dating market. It plots birth cohort size vs proportion of male births. A regression line is fitted and its coefficients and p value are shown on top.

Table A21: Parity

Dependent Variable:	Male child				
Race	Pooled sample	White	Black	Asian	Native
Model:	(1)	(2)	(3)	(4)	(5)
Order	-0.0006***	-0.0004***	-0.0006***	0.0009**	-0.0002
	(8.09×10^{-5})	(9.94×10^{-5})	(0.0001)	(0.0004)	(0.0006)
R^2	2.72×10^{-6}	1.26×10^{-6}	3.83×10^{-6}	3.73×10^{-6}	3.08×10^{-7}
Observations	23,861,242	16,919,170	4,514,031	2,147,662	280,379
Dependent variable mean	0.512	0.513	0.508	0.516	0.510

Notes: The outcome variable is the dummy for male birth. Each regression controls for race. The sample includes all births between 2011 and 2019. First column presents the results for the pooled sample of non-Hispanic births, following columns are for race specific sub samples. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Source: Natality Data

B.6 Additional Balance Tests for Exclusion Restriction

A central identifying assumption in our empirical strategy is that variation in sex ratios at birth influences maternal and neonatal outcomes solely through its effect on the sex composition of local dating markets. For the exclusion restriction to hold, the instrument must be orthogonal to other intermediate environments—such as school resources, educational services, or healthcare availability—that could independently shape maternal or neonatal health.

To assess this condition, I construct a series of balance tests drawing on multiple data sources. Educational covariates come from the *Common Core of Data* (CCD), which reports school-level information on enrollments by gender, race, and grade, eligibility for free lunch, and pupil–teacher ratios for the universe of public schools. I aggregate these data to the county $\times$ race $\times$ school year level, weighting by the number of students in each cell. I focus on the 1999/2000, 2004/2005, and 2009/2010 school years, when high schools were fully populated by the cohorts aged 25–29, 20–24, and 15–19 in 2010, respectively.[40] These variables align closely with the market definition and the instrument used in the main analysis. Additional educational covariates are drawn from the 2009/2010 *Civil Rights Data Collection* (CRDC), the earliest digitized wave. From the CRDC, I construct county-level averages of subject-specific class sizes (Biology, Geography, Physics), the availability of gifted and AP programs, per-pupil expenditures, student participation in athletics, reported bullying incidence, and the presence of athletics programs, weighting by the number of students in high school. These variables capture the broader school environment, including both in-class resources and extracurricular services. Because the 2009/2010 CRDC corresponds to the period when nearly the entire 15–19 cohort in 2010 was in high school, I restrict this analysis to that cohort.

Healthcare covariates come from the *Area Resource File* (ARF, 2010 edition). To capture the availability of medical care, I focus on measures related to both general healthcare and services specific to pregnancy and children: the per-capita availability of physicians, pediatricians, obstetricians/gynecologists, nurse practitioners, and nurse midwives, as well as the number of hospitals, hospitals with obstetric services, and hospital beds. These variables, measured as rates per 100,000, vary only at the county level.

I estimate balance regressions that parallel the empirical strategy in the main analysis. Specifically, for each county–race–age group cell I observe a covariate W (which may or may

[40]The 1994/1995 wave does not provide data at a sufficiently disaggregated level, so the 30–34 cohort is excluded.

not vary along all of these dimensions) and estimate:

$$W_{cra} = \alpha + \beta\,\text{PropMaleBirth}_{cra} + \delta X_{cra} + \varepsilon_{cra}, \tag{6}$$

where $\text{PropMaleBirth}_{cra}$ is the proportion male at birth in county c, race r, and cohort a. The vector X_{cra} includes the following controls controls: log cohort size at birth and in 2010, fixed effects for race, and, in some specifications, age group and county fixed effects. Standard errors are clustered at the county level to account for spatial correlation in the error term. All regressions are weighted by the size of the cohort in 2010 (or, for CRDC outcomes, by the number of students in the sample) so that coefficients can be interpreted as average effects per individual. This weighting mirrors the main regressions, which—because they are estimated at the mother level—are effectively weighted by cohort size.

For variables observed only at the county level (e.g., physicians per capita), I repeat values across race–cohort cells within a county.[41] One might instead consider aggregating the instrument to the county level and regressing county outcomes directly on county sex ratios. I deliberately retain the county–race–cohort level of variation for two reasons. First, this mirrors the main analysis and corresponds to the variation that identifies the main results—the relevant unit of observation is the market, defined as a county–race–cohort. These markets generate independent deviations in sex composition that are small relative to the county as a whole and tend to offset one another. As a result, the instrument is unlikely to affect county-level outcomes such as the supply of healthcare providers, which would require systematic shifts at the county level. The null findings on these outcomes therefore reinforce the strength of the design: the instrument generates deviations that are too small to alter aggregate county factors, but large enough to shape local dating markets. Second, once aggregated to the county level, the variation in sex ratios is mechanically reduced by the law of large numbers, leaving little scope to explain meaningful differences in outcomes.

[41] Clustering at the county level ensures that inference is not biased by this replication, and weighting ensures that coefficients are interpretable as per-person effects in the relevant population.

Tables A22 and A23 report the balance test results. Each table presents the baseline specification, in which I regress each outcome on the market-level sex composition. Because outcomes differ in their availability, sample sizes vary across specifications, as indicated in the tables. I also report the omnibus F-test, which regresses the instrument on the full set of covariates jointly. This test is necessarily estimated on the restricted sample with complete information, and for comparability I additionally report individual regressions for that same restricted sample. For outcomes measured at the county level, I include race and age-group fixed effects; for school outcomes from the CCD, I also present a specification with county fixed effects. The outcomes in Table A23 vary only at the county level and are available for a single cohort, so I include only race fixed effects. All tables report coefficients with clustered standard errors in parentheses, alongside p-values and the corresponding number of observations.

Across a wide range of covariates, I find no systematic association between the instrument and either educational or healthcare environments. Coefficients are small, inconsistent in sign, and far from statistical significance. Because the regression coefficient reflects the effect of a change in the proportion male from 0 to 1, the magnitudes may appear large; however, the standard deviation of the instrument is only 0.02, so economically relevant shifts are minor. For example, in Table A22, the instrument does not predict enrollment rates, the share of students receiving free lunches, pupil–teacher ratios, or per-capita healthcare availability. The coefficient on the number of hospitals with obstetric services is marginally significant ($p = 0.053$), but it is in the opposite direction of the hypothesized effect, disappears in alternative specifications, and is consistent with the appearance of occasional low p-values by chance when testing multiple outcomes. Most importantly, the omnibus F-test yields a high p-value, providing no evidence of joint imbalance across the set of covariates.

Table A22: Covariate Balance: Education and Healthcare

	Baseline		Restricted Sample		With County FE	
Variable	Coef.	P-val.	Coef.	P-val.	Coef.	P-val.
Enrollment rate	0.919	0.587	1.466	0.395	0.760	0.443
	(1.690)	(10723)	(1.724)	(9678)	(0.990)	(10723)
Free lunch rate	0.088	0.380	0.090	0.366	-0.036	0.482
	(0.100)	(9684)	(0.100)	(9678)	(0.052)	(9684)
Pupil-teacher ratio	0.008	0.398	0.008	0.368	0.003	0.510
	(0.009)	(10251)	(0.009)	(9678)	(0.004)	(10251)
Physicians per 100,000	5.270	0.814	0.617	0.977		
	(22.385)	(14209)	(21.807)	(9678)		
Pediatricians per 100,000	6.522	0.464	1.771	0.828		
	(8.907)	(14209)	(8.167)	(9678)		
OB-GYN per 100,000	1.737	0.740	-0.364	0.940		
	(5.227)	(14209)	(4.854)	(9678)		
Nurse practitioners / 100,000.	2.366	0.912	-3.411	0.880		
	(21.500)	(14209)	(22.586)	(9678)		
Nurse midwives per 100,000	0.898	0.758	-1.314	0.690		
	(2.911)	(14209)	(3.297)	(9678)		
Hospitals per 100,000	-1.786	0.314	3.141	0.100		
	(1.772)	(14209)	(1.906)	(9678)		
Hospitals with OB per 100,000	-1.842*	0.053	-0.555	0.651		
	(0.951)	(14209)	(1.226)	(9678)		
Hospital beds per 100,000	0.501	0.789	1.743	0.363		
	(1.871)	(14209)	(1.914)	(9678)		
Omnibus F-test p-value		0.775		0.775		0.637

Notes: Each cell reports the coefficient on the instrument (proportion male at birth) from a regression of the listed covariate on the instrument, controlling for the log of cohort size at birth and in 2010, symmetrically to the main regression. Baseline and Restricted Sample specifications include race and age-group fixed effects, while the County FE specification additionally includes county fixed effects. Standard errors clustered at the county level are shown in parentheses below coefficients. P-values are reported above, with the number of observations in parentheses below. Baseline sample are all the markets used as an instrument without the cohort 30-34, for which data do not exist. The Restricted Sample is limited to markets with complete data on all outcomes. The omnibus F-test reports the joint test of the null hypothesis that all coefficients are zero, estimated by regressing the instrument on the full set of covariates with the same controls and fixed effects. The F-test uses restricted sample in each case as it needs or characteristics present. All regressions are weighted by 2010 market size. Data on schooling comes from common core of data and represents information for all public schools in years 2009/2010, 2004/2005, and 1999/2000. Enrollment rate is total enrollment within given race x county x school year, divided by corresponding birth cohort. Free lunch ratio is the average share of students within school receiving free lunches. Data on healthcare come from area resource file (ARF) and reports 2010 quantities. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In Table A23, subject-specific class sizes, the availability of AP and gifted programs, per-pupil expenditures, and indicators of athletics or bullying are likewise uncorrelated with sex ratios at birth. The omnibus F-test again yields a high p-value, confirming the absence of systematic imbalance across these outcomes. One coefficient—per-pupil expenditures in the restricted sample—appears marginally significant, but this isolated result is not robust and does not alter the joint conclusion of no detectable relationship.

Table A23: Balance Tests: Detailed Education

	Baseline Sample		Restricted Sample	
Variable	Coef.	P-val.	Coef.	P-val.
Pupil–Teacher Ratio	-9.986	0.281	-9.977	0.315
	(9.265)	(3049)	(9.933)	(2409)
Biology Class Size	-5.075	0.858	-4.421	0.878
	(28.413)	(2613)	(28.725)	(2409)
Geography Class Size	-2.743	0.912	-2.061	0.934
	(24.695)	(2601)	(25.003)	(2409)
Physics Class Size	-23.620	0.284	-23.597	0.285
	(22.037)	(2422)	(22.053)	(2409)
Gifted Program	-1.076	0.247	-0.988	0.319
	(0.929)	(3055)	(0.990)	(2409)
AP Program	0.055	0.755	-0.014	0.941
	(0.178)	(3054)	(0.187)	(2409)
Expenditure per Pupil	91809.135	0.230	134909.128*	0.054
	(76459.651)	(3055)	(70098.627)	(2409)
Share of Athletes	0.142	0.654	0.201	0.552
	(0.317)	(3055)	(0.338)	(2409)
Reported Bullying Rate	0.009	0.225	0.002	0.466
	(0.008)	(3055)	(0.003)	(2409)
Athletics Program	-0.084	0.825	-0.095	0.816
	(0.382)	(3055)	(0.410)	(2409)
Omnibus F-test p-value		0.840		0.840

Notes: Each cell reports the coefficient on the instrument (proportion male at birth) from a regression of the listed covariate on the instrument, controlling for cohort size and race fixed effects. The sample is drawn from the 2009–2010 Civil Rights Data Collection, the earliest digitized wave, corresponding primarily to individuals aged 15–19 in 2010. The instrument is subset to this cohort and varies by race and county. Covariates are county-level weighted averages, and all regressions are weighted by the total number of high school students in each county. Standard errors clustered at the county level are shown in parentheses below coefficients. P-values are reported above, with the number of observations in parentheses below. The restricted sample includes only observations with complete covariate data. The omnibus F-test reports the joint test of the null hypothesis that all coefficients are zero, estimated by regressing the instrument on the full set of covariates using the restricted sample. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

While the balance tests show that the instrument does not predict educational or healthcare environments, the absence of correlations is not merely due to limited statistical power. To demonstrate this, I conduct an additional exercise where an effect should clearly be present if the dating market is the relevant channel: I regress the realized sex composition in high schools in both datasets on the sex composition at birth of the corresponding cohorts. Using the Common Core of Data and CRDC, I compute the average proportion male in high school[42] at the county × race × cohort level for Common Core of Data and at the county × race for CRDC data. Then I regress these values on the corresponding birth-cohort sex ratios, weighting by cohort size. The results (Table A24) show a strong and highly significant relationship with small standard errors. This confirms that the instrument, together with the data used in the balance tests, is not mere noise.

Table A24: Proportion Male at Birth and Proportion Male at High School

	Proportion Male in High School	
	Core Data	CRDC Data
Prop. Male at Birth	0.2695***	0.326***
	(0.021)	(0.08)
Fixed effects		
County	Yes	
Race	Yes	Yes
Age group	Yes	
Observations	10,257	2,602
R^2	0.440	0.0097
Mean of dep. var.	0.511	0.516

Notes: Each column reports coefficients from regressions of proportion male in high school on the proportion male at birth. Observations are at the county-race-age group level in Core data and county-race in CRDC data. All regressions are weighted by market size and include the fixed effects listed. Standard errors clustered by county are shown in parentheses. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Overall, the instrument does not shift outcomes such as school resources or healthcare availability, but it does affect the composition of local dating markets, with high schools serving as an important environment for couple formation.

[42] The weighted averages are constructed over the proportion male of a given race in each high school–year cell, using the corresponding number of students in the cell as weights.

B.7 Peer Effects in Education and Incarceration

One may be concerned that growing up in a location with unbalanced sex composition may impact behaviors through channels unrelated to the dating market.

A natural concern is whether peer effects in schools could generate an alternative pathway. A body of work suggests that the gender composition of social environments, particularly in school settings, can influence educational outcomes. I do find that sex composition at birth predicts the gender composition of high schools, which also serve as a major dating market (see Section B.6). Hoxby [2002] leverages variation in classroom gender ratios to show that student performance improves when there are more female classmates. Consistent with this, related studies find that a higher proportion of female peers positively affects academic achievement [Lavy and Schlosser, 2011, Sacerdote, 2011, Lu and Anderson, 2015]. However, the magnitude of these effects tends to be modest.Lavy and Schlosser [2011], for example, finds that a 10 percentage point increase in the share of female peers raises matriculation rates by about 1 percentage point, but does not affect dropout rates at any level. In the context of my study, where the standard deviation of the sex ratio shift is around 3.65 p.p., this would translate into an estimated change in matriculation probability of merely 0.35 percentage points. Anelli and Peri [2019] examine the impact of female peer share in high school on college completion and find that male students are slightly more likely to graduate from college when the proportion of female classmates exceeds 90 percent. However, no effect is observed when the female share is around 80 percent or below, and the study finds no significant impact on subsequent labor market outcomes. In my setting, the sex composition never reaches 90%.

A related mechanism—often discussed alongside peer effects—is the influence of gender composition on antisocial behavior and violence. Since females are generally less prone to violence, one might expect that a higher proportion of men in a cohort could raise the violent behaviors. Additionally, strong competition among men for female partners might result in violence. In line with this intuition, studies from high-imbalance settings using credible identification find that male surpluses raise crime [e.g., Edlund et al., 2008, Amaral and Bhalotra, 2017], whereas U.S. evidence is largely descriptive and shows a negative correlation between the male share and certain offenses [Schacht and Kramer, 2016].

To further assess these channel in my context, I conduct robustness checks using Opportunity Insights data and Uniform Crime Reporting data. First, I examine adult incarceration and four-year college completion. Second, I add a test using arrest rates, where any violence channel should show up as well.

I begin with county-by-race-by-gender stocks and estimate

$$y_{crg} = \beta^g \text{Prop. male at birth}_{cr} + \gamma^g X_{cr} + \delta_r^g + \epsilon_{crg} \tag{7}$$

Where y_{crg} represents either the share of incarcerated or college educated in county c, race r, and gender g. Estimation results (in table A25) show no relationship between outcomes unrelated to the dating market and the sex composition at birth. Columns (1)–(2) indicate no significant effect on adult incarceration; the 95% confidence intervals exclude changes larger than 0.02 percentage points for women and 0.10 percentage points for men. The coefficient for men is directionally consistent with the literature and a small increase in crime in the US context cannot be fully ruled out. Nonetheless, economically meaningful magnitudes are unlikely. Furthermore, there is no evidence that educational achievements are shaped by the sex composition at birth, as estimates of β in columns (3) and (4) are not statistically significant. With 95% confidence, it is possible to discount any effects of a one standard deviation shift in the sex ratio on college completion rates that are larger than 0.5 percentage points for both genders.

Table A25: RF: Education and Incarceration

Dependent Variables:	Incarcerated		College	
	Female	Male	Female	Male
Model:	(1)	(2)	(3)	(4)
Prop. male at birth	0.002	0.007	-0.0007	-0.031
	(0.005)	(0.026)	(0.130)	(0.114)
Dependent variable mean	0.00402	0.03937	0.35074	0.25554
R^2	0.12758	0.71730	0.39477	0.44805
Observations	3,558	3,555	3,017	2,993

Notes: Regressions of education and incarceration outcomes on the proportion of males at birth and covariates. The sample includes individuals born in 1978-1983, assigned to their childhood county. Each observation represents race × county × gender. *Prop. male at birth* measures the male share of births (1978-1983). Incarcerated refers to the share of the cohort incarcerated (columns 1 and 2), and College refers to the share with a college degree by age 25+ (columns 3 and 4). Controls: cohort size, race fixed effects. Standard errors are heteroskedasticity-robust. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Data: Opportunity Insights.

Moreover, even if peer effects in education were relevant, the direction of influence would go against the main findings of this paper. The education literature generally finds that a higher share of females improves outcomes for both genders in educational settings. In contrast, my results show that a higher female-to-male ratio at birth worsens women's health outcomes later in life. Thus, while shifts in sex composition could shape peer dynamics, this mechanism are unlikely to drive the main results.

To probe the crime mechanism using an alternative dataset, I analyze county×age arrest rates in 2010, grouping offenses into three bundles—Violent/Sex, Financial, and Drug/Liquor—and estimating both OLS and IV on an identical sample within each bundle (Table A26). The 2010 cross-section allows me to align age groups in the arrests data with the birth cohorts used in the instrument. Because the ASR do not jointly disaggregate by race and age, I restrict the sample to racially homogeneous counties (the largest racial group $\geq 90\%$). The OLS specification relates arrest rates to the observed adult male share in the county×age cell and includes county and age-group fixed effects, population weights, and county-clustered standard errors. The IV specification instruments the adult male share with the corresponding cohort sex ratio at birth measured at the county-cohort level. Consistent with U.S. evidence on adult sex ratios Schacht and Smith [2017], the OLS estimates show some negative associations between the male share and arrest rates. In contrast, the IV estimates are statistically indistinguishable from zero across categories, with a strong first stage (KP Wald $F \approx 81$–96). Magnitudes are also relatively small: a one–standard–deviation increase in the adult male share (0.035) implies changes on the order of 4–7% of the category mean. Some confidence intervals admit modest positive effects—qualitatively consistent with the incarceration results and with evidence from highly imbalanced settings (e.g., Edlund et al. 2008, Amaral and Bhalotra 2017). Moreover, an positive effect of proportion male on crime would operate against the paper's main mechanism (improved women's health when the local male share is higher).

Table A26: Adult Sex Composition and Arrest Rates

	Violent/Sex	**Financial**	**Drug/Liquor**
	(1)	(2)	(3)
Dependent variable: arrests per 1,000 residents			
OLS			
Proportion Male (OLS)	2.081	-2.184^{*}	-72.76^{***}
	(1.869)	(0.9555)	(17.74)
IV			
Instrumented Proportion Male (IV)	9.620	−2.566	30.38
	(9.834)	(5.492)	(70.38)
First-stage Wald F (IV)	93.720	81.027	95.772
Observations	4,691	4,115	4,754
Dep. var. mean	5.8001	1.3111	25.607

Arrest rates (arrests per 1,000 residents) in county×age cells are constructed from the FBI UCR *Arrests by Age, Sex, and Race (ASR)* and restricted to calendar year 2010 to align cohorts with the instrument. Offenses are grouped into three bundles: Violent/Sex (e.g., homicide, rape), Financial (e.g., fraud, forgery), and Drug/Liquor (e.g., drug abuse violations, DUI). The OLS rows use the observed adult proportion male in each county×age cell; the IV rows instrument this proportion with the corresponding cohort proportion at birth. All models include county and age-group fixed effects; standard errors are clustered by county level. Population denominators (and regression weights) are county×age adult populations. The estimation sample is restricted to racially homogeneous counties (at least 90% of residents of the same race) because the data are not jointly disaggregated by age and race. *Conventional significance levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B.8 Effects on Social Networks

There may also be broader, harder-to-measure channels through which sex composition at birth influences outcomes, particularly in areas with limited prior research. One such possibility is the development of soft skills or pro-social attitudes. Growing up in a sex-imbalanced cohort could affect interpersonal dynamics and shape social competencies. To explain the observed effects through this mechanism, one would need to assume that a higher presence of boys promotes the development of traits beneficial for women's health. Although these traits are difficult to observe directly, they may manifest in social behaviors such as network structure or participation in pro-social activities.

While soft skills are difficult to measure, they may be reflected in social behaviors like forming social networks or engaging in volunteer activities. Using data from Opportunity Insights on social capital (based on Facebook), I examine the reduced-form impact of the sex ratio at birth on social behavior later in life. I focus on two measures: (1) the clustering of high school friendships, which reflects the cohesion of social networks, and (2) civic engagement, which indicates the likelihood of participating in civic organizations or volunteering. I estimate:

$$y_c = \alpha + \beta \text{Prop. male at birth}_c + \gamma^g X_{cr} + \epsilon_{crg} \tag{8}$$

where y_c represents one of the social behavior outcomes in county c and β captures the effect of an imbalanced sex ratio at birth on these outcomes. As shown in the table A27, there is no significant relationship between the sex ratio at birth and these social behavior measures. The maximum impact of a standard deviation change in the sex ratio, according to the 95% confidence interval, would range from -0.0033 to 0.0036 for social networks clustering and from -0.00038 to 0.0016 for social networks volunteering—both negligible relative to their means. These findings suggest that an unbalanced sex ratio at birth does not significantly affect social network formation or civic engagement later in life.

Still, other dimensions of soft skill development may not be captured by these measures.

Another possibility is that unbalanced sex ratios influence social norms, which in turn shape behavior. For this to explain the observed outcomes, male-skewed cohorts would need to develop norms that benefit women. While this is a plausible mechanism, it is worth noting that sex ratios vary independently across cohorts. Hence, any such norm shifts due to market imbalance would likely be confined to the cohort itself and unlikely to affect older individuals—such as parents, teachers, or healthcare providers—who play a significant role in shaping women's health outcomes. It remains reasonable that prolonged exposure to an unbalanced dating market from early childhood could shape attitudes, relationships, or behavioral patterns in ways that influence health. To the extent such long-run adaptations exist, they could represent an additional pathway through which the sex composition exerts influence. While they might be considered part of a longer-term response to dating market conditions, their precise role remains difficult to isolate.

Table A27: Reduced Form: Social Networks

Dependent Variables:	Clustering	Volunteering Rate
Model:	(1)	(2)
Prop. male at birth	0.012	0.054
	(0.158)	(0.045)
R^2	0.03590	0.00032
Observations	4,124	4,113
Dependent variable mean	0.59015	0.05815

Notes: Regressions of social network and civic engagement outcomes on the proportion of males at birth within the cohort, controlling for covariates. The population under consideration was born between 1986 and 1996 and is assigned to the county where they attended high school. Each observation represents a county. Controls include the log of the cohort size at birth. Standard errors are heteroskedasticity-robust. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Data source: Opportunity Insights.

B.9 Effects on Divorce in Instrument's Parents Generation

A potential concern is whether the sex ratio at birth influences marital stability in the parents' generation, as Dahl and Moretti [2008], Kabátek and Ribar [2021] found that parents of first-born daughters are more likely to divorce, which could impact children's health. [Dahl and Moretti, 2008], for instance, find that parents with a first-born daughter are 2.2 percentage points more likely to divorce. According to this magnitude, and conservatively assuming that each additional girl causally increases the probability of divorce, a one standard deviation increase in the proportion of girls would imply an increase in the divorce rate of just 0.077 percentage points. This needs to be multiplied by the effect of divorce on potential birth outcomes. For example, Frimmel et al. [2024] document that parental divorce raises the probability of giving birth before age 20 by approximately 0.7 percentage points. Multiplying this by the 0.077 figure yields an effect of roughly 0.05 percentage points—a magnitude too small to plausibly drive the main results. To further assess the empirical relevance of this mechanism in my setting, I examine whether divorce rates are higher in areas with a greater proportion of female births. I analyze the reduced-form effect of the sex ratio at birth on divorce probability in the older generation using county-level data from the 2010 American Community Survey (ACS).

The analysis covers cohorts aged 35-54 in 2010 to capture variations in divorce timing among potential parents of generations aged 15-34. The variable *Prop. male at birth* measures the proportion of male births in a given county and cohort. The outcome variable indicates share of divorced respondents at the time of the survey. I estimate the following regression model:

$$y_{c,a} = \sum_{a'} \beta^{a'} \text{Prop. male at birth}_{c,a'} + \gamma X_{c,a} + \epsilon_{c,a} \tag{9}$$

where $y_{c,a}$ represents the divorce probability in county c, cohort a, and $\beta^{a'}$ captures the effect of the sex ratio at birth in cohort a' on the likelihood of divorce in an older cohort.

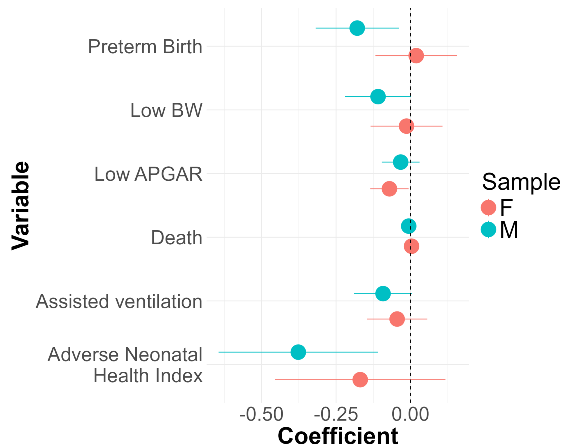
The results (table A28) indicate that there is no significant relationship between the sex ratio at birth and the probability of divorce in parental generations. The maximum impact of a one standard deviation change in the sex ratio, according to the 95% confidence interval, ranges from -0.0037 to 0.0044. These findings imply that an unbalanced sex ratio at birth does not have a substantial impact on the likelihood of divorce among parents.

Table A28: RF: Parent's Divorce

Model:	Age in 2010 35-39 (1)	40-44 (2)	45-49 (3)	50-54 (4)
Prop. birth 15-19	0.012	-0.009	0.104	0.049
	(0.083)	(0.087)	(0.074)	(0.077)
Prop. birth 20-24		0.118	0.065	0.030
		(0.082)	(0.081)	(0.074)
Prop. birth 25-29			0.011	0.080
			(0.085)	(0.078)
Prop. birth 30-34				-0.072
				(0.074)
R^2	0.00945	0.00246	0.00216	0.00754
Observations	5,584	5,578	5,576	5,558
Dependent variable mean	0.13797	0.16062	0.17352	0.17484

Notes: Regressions of a divorce probability in a cohort on the proportion of males at birth in a younger cohort. Each observation represents a county *times* cohort combination. The variable *Prop. male at birth* measures the share of births in a given younger cohort and county who were male. Controls include log of cohort size at birth. Standard errors are clustered at the county level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Data source: ACS 2010.

Figure B.17: Heterogeneity by Sex of the Child



Notes: Each plot depicts coefficients on the variable 'proportion male' from the primary instrumental variable (IV) framework estimated on two distinct subsamples. One subsample pertains to female children (F), while the other corresponds to male children (M).

B.10 Impact of the Market Misdefinition on the Coefficients

Call the proportion of men at the true market PM^T and assume that a woman from a county c search partners across multiple counties which belong to a set C (similar argument can be made about market expanding to other races or age groups). Let n_{cra} be population of age a, race r, and from county c and let n^m_{cra} be the number of men in this group. Let $\alpha_{c,c'}$ measure how often women from county c link with men from county c' and let it sum up to one across C. Assume that the proportions of men across markets are independent. Then, the relationship between the true market and the market limited to own county can be expressed as:

$$\underbrace{PM^T_{cra}}_{\substack{\text{Proportion Male}\\ \text{At the True Market}}} = \frac{\sum_{c'\in C}\alpha_{c,c'}n^m_{c'ra}}{\sum_{c'\in C}\alpha_{c,c'}n_{c'ra}} == \sum_{c'\in C}\underbrace{\frac{\alpha_{c,c'}n_{c'ra}}{\sum_{c'\in C}\alpha_{c,c'}n_{c'ra}}}_{\gamma_{c'}}\frac{n^m_{c'ra}}{n_{c'ra}} =$$
$$= \gamma_c\frac{n^m_{cra}}{n_{cra}} + \sum_{c'\neq c}\gamma_{c'}\frac{n^m_{c'ra}}{n_{c'ra}} = \underbrace{\gamma_c}_{\gamma_c<1}\frac{n^m_{cra}}{n_{cra}} + e_{cra} = \gamma_c \underbrace{PM_{cra}}_{\substack{\text{Proportion Male}\\ \text{At the Limited Market}}} + e_{cra} \tag{10}$$

Now assume that health outcomes Y_{cra} are a function of the proportion male at the true market, with true coefficient β. Regressing Y_{cra} on the proportion male at the limited market will give conservative estimate of the true effect:

$$Y_{cra} = \beta PM^T_{cra} + \epsilon_{cra} = \beta\gamma_c PM_{cra} + \beta e_{cra} + \epsilon_{cra} = \hat{\beta} PM_{cra} + v_{cra} \tag{11}$$

Since γ_c is lower than one, $\hat{\beta}$ is lower than β. Note that IV strategy does not eliminate this bias. Now assume conversely that the measured market is too large. In this case a classical measurement error arises, which is eliminated by the IV.

C Additional Results and Mechanisms

C.1 Change in Composition of Mothers

Conditions in the dating market may affect maternal health through the changes in the composition of mothers. Building on the changes in fertility, I next examine whether the birth rate effect is linked to changes in mothers' characteristics. Table A29 shows that women in more favorable markets tend to be healthier and more educated, with less likelihood of being overweight and having more educated partners. This suggests that empowered women pursue pregnancy only when household resources are sufficient. In contrast, women with less bargaining power may agree to childbearing as a concession to their partner. This also indicates that the average quality of couples having children does not decline as the supply of men increases.

C.2 Heterogeneity analysis

To evaluate the impact of market definition and sample selection, I examine heterogeneity in bargaining effects. The strongest effects are seen in urban markets and among racial minorities, suggesting the findings are generalizable to the excluded parts of the sample.

The impact of sex composition on maternal and neonatal outcomes is primarily driven by

Table A29: Effect on Composition

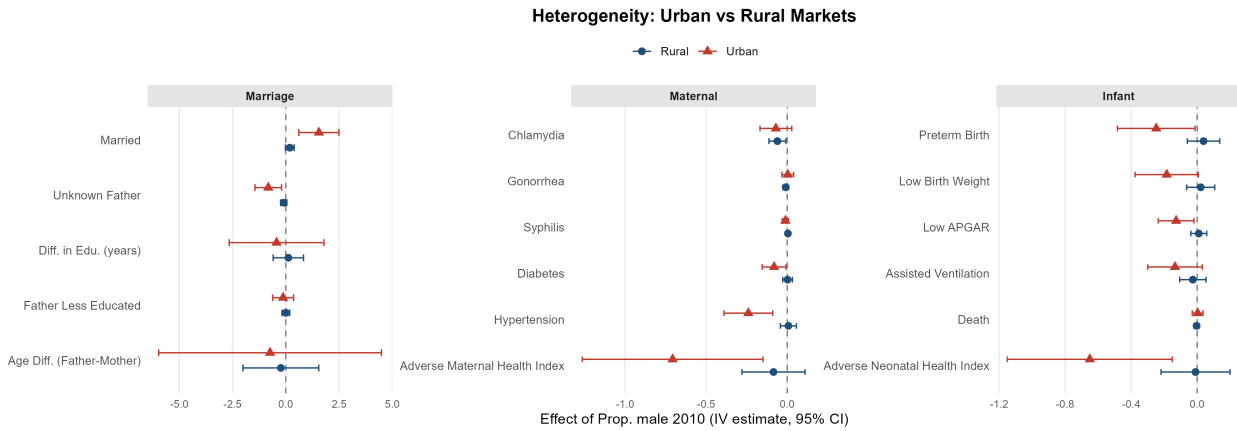
Dep. Var.:	Overweight	Age at Delivery	Mother's Edu.	Fathers's Edu.
Model:	(1)	(2)	(3)	(4)
Prop. male 2010	-0.2729**	-0.1546	3.246**	3.585**
	(0.1109)	(0.7672)	(1.286)	(1.464)
Dep. var. mean	0.544	28.3	13.9	13.7
Observations	6,973,738	6,973,738	7,119,580	6,116,977
Wald KP (1st stage)	112.3	113.5	99.7	78.9

Notes The table shows IV regressions with mother's and father's characteristics on instrumented proportion of men on the dating market and covariates. Covariates include County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

urban markets[43] , as shown in Figure C.18. The effects on unknown father status and marital status are significantly stronger in urban areas. Maternal health outcomes, like diabetes and hypertension, are also more influenced by bargaining in urban settings, while chlamydia shows similar coefficients in both. Neonatal health outcomes, such as preterm birth, low birth-weight, and low APGAR scores, exhibit larger negative coefficients in urban areas, though not statistically significant at 5% due to smaller sample size.

[43]Counties are classified according to the 2013 Rural-Urban Continuum Codes. Non-metro areas (codes larger than 3) are classifed as rural

Figure C.18: Heterogeneity: Urban vs Rural Markets

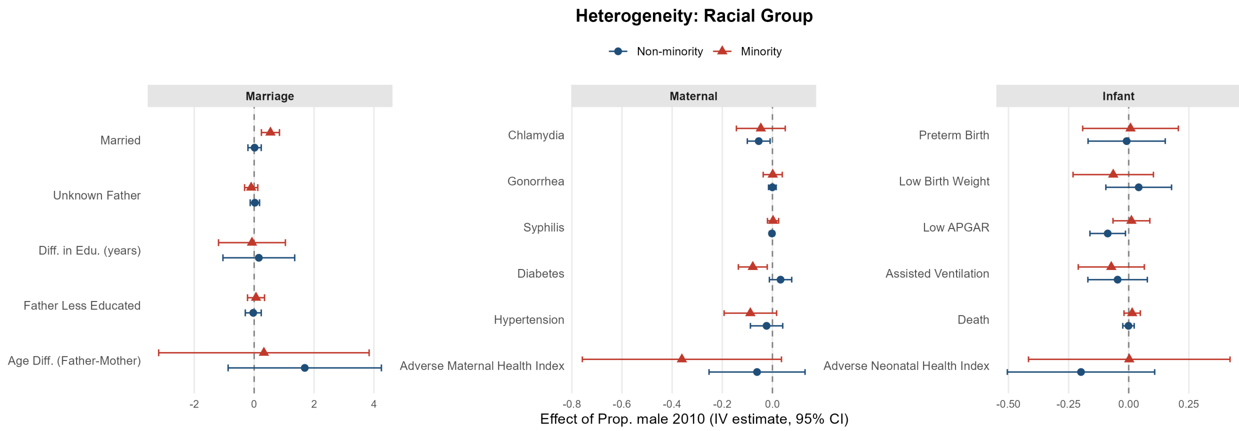


Notes: IV coefficients on proportion male 2010 with 95% confidence intervals, estimated separately on rural and urban markets. Counties are divided according to the 2013 Rural-Urban Continuum Codes; non-metro areas are classified as rural. Marriage-market outcomes include *Age Diff. (Father-Mother)*, rescaled by its sample mean (2.56 years) to put it on a scale comparable to the binary outcomes, and *Father Less Educated* (indicator for the father having strictly fewer years of education than the mother).

When split by race (Figure C.19), the dating market effects are more pronounced for racial minorities, with larger absolute coefficients in most outcomes. In the below analysis I divide the sample into two groups: (1) Black and Native, (2) White and Asian[44]. Larger effects for minorities may be due to minorities' higher poverty rates or differences in healthcare treatment, making them more sensitive to bargaining-related factors like domestic violence. Future research could explore how poverty and discrimination mediate the impact of household bargaining.

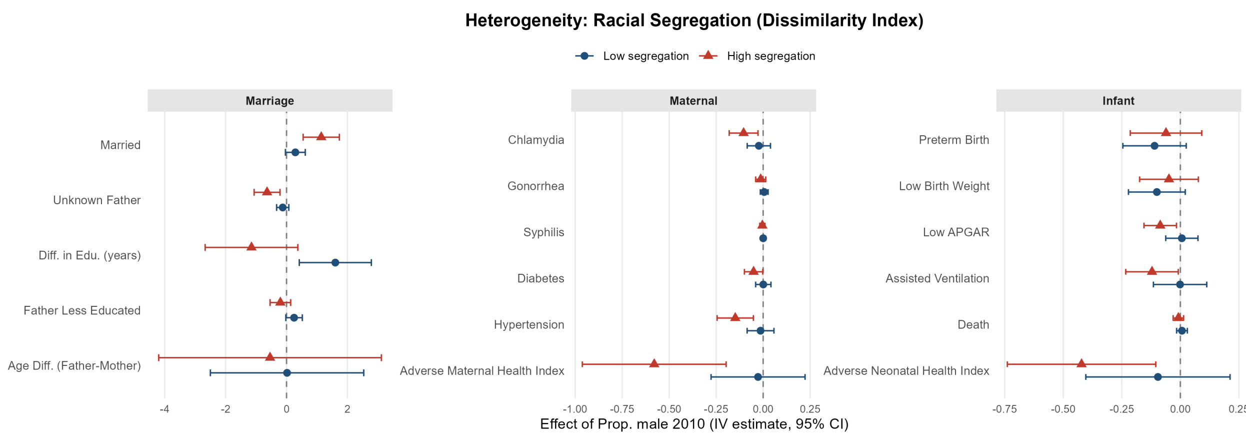
[44] Splitting by a single racial group leads to power issues

Figure C.19: Heterogeneity: Racial Group



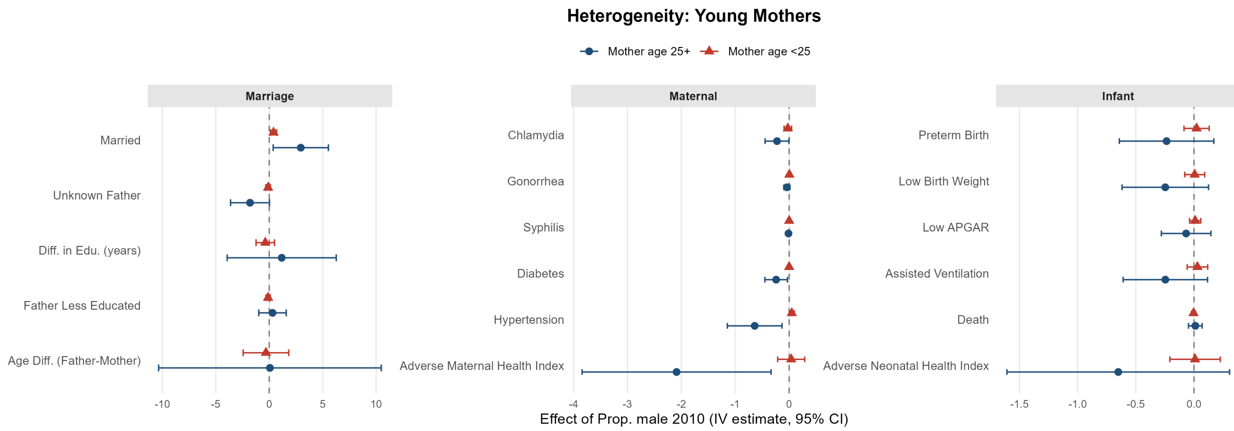
Notes: IV coefficients on proportion male 2010 with 95% confidence intervals, estimated separately for non-minority (White and Asian) and minority (Black and Native) samples. Marriage-market outcomes include *Age Diff. (Father-Mother)*, rescaled by its sample mean (2.56 years) to put it on a scale comparable to the binary outcomes, and *Father Less Educated* (indicator for the father having strictly fewer years of education than the mother).

Figure C.20: Heterogeneity by Racial Segregation: Above Median Dissimilarity Index



Notes: IV coefficients on proportion male 2010 with 95% confidence intervals, estimated separately for counties with above-median (High segregation) and below-median (Low segregation) dissimilarity index. Segregation is measured by the dissimilarity index (from FRED), which quantifies the share of the non-Hispanic White population that would need to relocate for equal racial distribution within a county. Marriage-market outcomes include *Age Diff. (Father-Mother)*, rescaled by its sample mean (2.56 years), and *Father Less Educated*.

Figure C.21: Heterogeneity: Young Mothers



Notes: IV coefficients on proportion male 2010 with 95% confidence intervals, estimated separately on mothers below age 25 (Young) and aged 25 and above (25+). Marriage-market outcomes include *Age Diff. (Father-Mother)*, rescaled by its sample mean (2.56 years), and *Father Less Educated*.

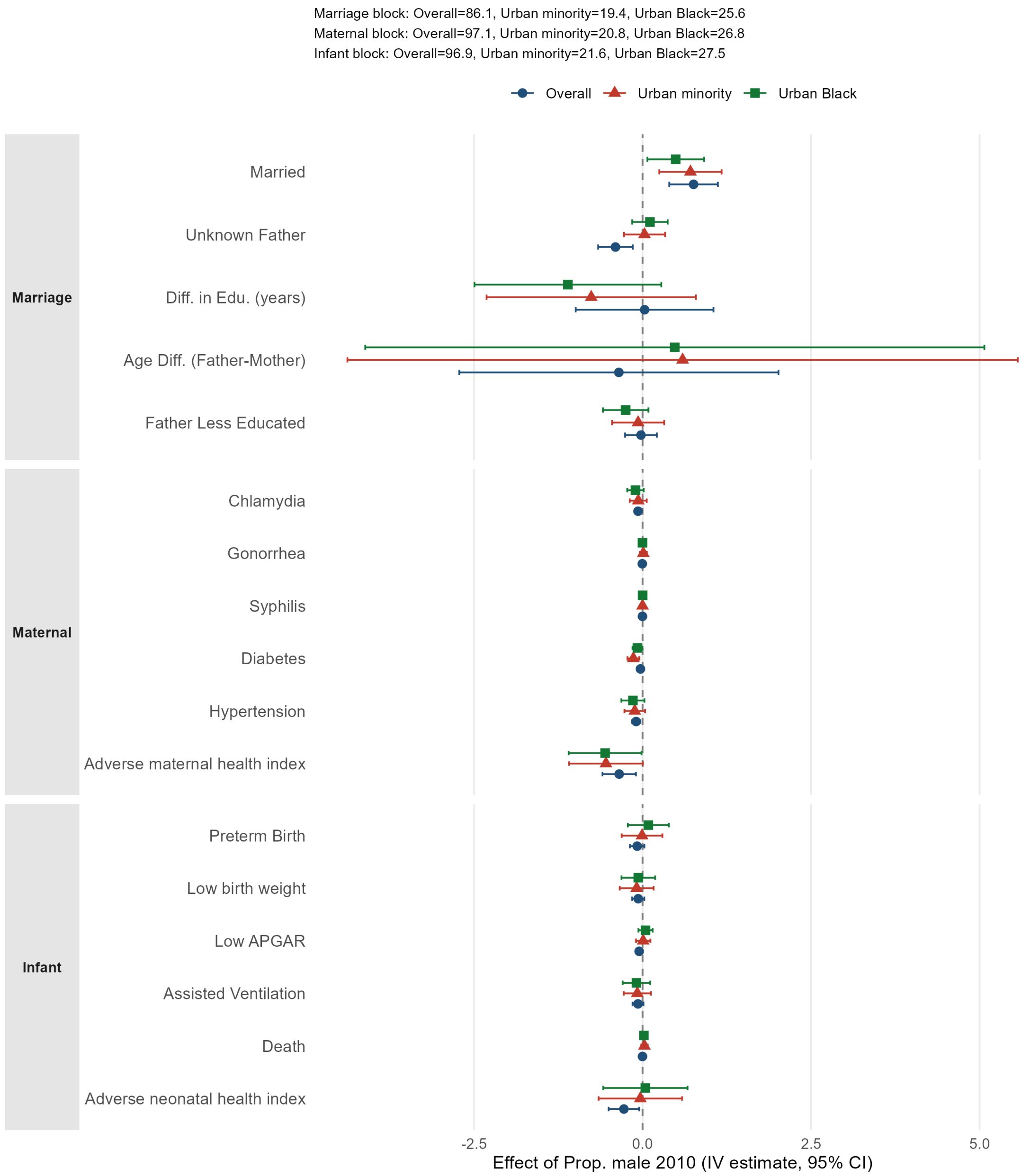


Figure C.22: Comparison of overall, urban minority, and urban Black estimates. The figure shows IV point estimates and 95% confidence intervals for the effect of the 2010 male share on the main marriage market, maternal health, and infant health outcomes. Urban minority and urban Black estimates broadly track the overall pattern, although they are less precisely estimated. First-stage Wald F statistics are noted on the top of the figure.

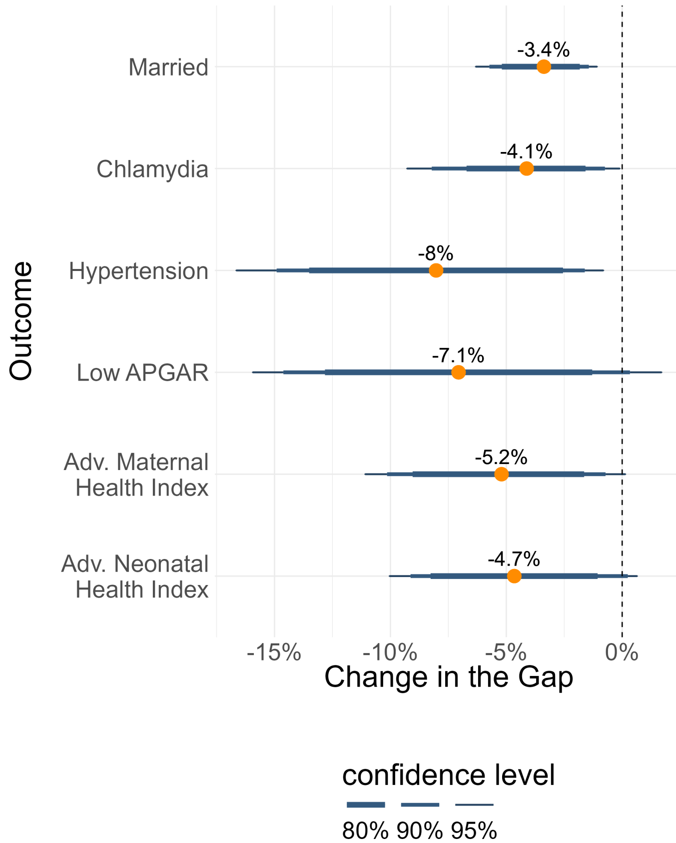


Figure C.23: Counterfactual Reduction in the Black–White Gap: Equalizing Local Sex Ratios

Notes: The figure plots the bootstrap distribution of the share of the Black–White gap in each outcome that would be closed if the share of Black men in a market were raised to match the White share, with the exercise restricted to the IV estimation sample. Points denote bootstrap means across 500 iterations; horizontal bars indicate the 80%, 90%, and 95% bootstrap confidence intervals.

C.3 Effect on population marriage rates

Dating market favorable to women increases the marriage rate in the female population. Table A30, based on the Opportunity Insights data (Chetty et al. [2018]), demonstrates this finding. I adapt my framework to this data by constructing a variant of the instrument: the proportion of male births in 1978-1983 in each county and race. Next, I estimate the following reduced form equation:

$$Married^a_{crg} = \beta^{ag}\text{Prop. male at birth}_{cr} + \gamma^{ag}X_{cr} + \lambda^{ag}_c + \delta^{ag}_r + \epsilon^a_{crg} \qquad (12)$$

Where $Married^a_{crg}$ is the share of people married at age a in county c, race r, and of

Table A30: RF: Marriage in the General Population

	Married at the age							
	24		26		29		32	
	Female	Male	Female	Male	Female	Male	Female	Male
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prop. male at birth	0.197*	0.090	0.180*	0.083	0.189*	0.072	0.236**	0.019
	(0.105)	(0.090)	(0.105)	(0.108)	(0.100)	(0.108)	(0.104)	(0.107)
Observations	3,945	3,947	3,945	3,947	3,945	3,947	3,945	3,947
R^2	0.96513	0.95193	0.97390	0.96073	0.97854	0.97017	0.98036	0.97232

Notes: The outcome variable is the proportion of men or women married at a given age. Population under consideration was born in 1978-1983 and is assigned to the county where they spent their childhood. Each observation represents race *times* county *times* gender. *Prop. male at birth* measures the share of births during period 1978-1983 in each county and race who were male. Each regression contains controls for cohort size in 2010 and at birth, County and Race fixed effects. Standard errors are heteroskedacticity robust. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Source: Opportunity Insights data Chetty et al. [2018]

gender g. The main independent variable is the proportion of male births, which varies across counties and races. Note that there is only one cohort in the outcome data. Controls include cohort sizes and fixed effects for race and county. The parameter β^{ag} identifies to what extent the sex composition at birth affects the marriage rates at age a in the general population of gender g. I perform only the reduced form regression as the years of births do not correspond to a well defined age cohort in 2010 census.

Results (columns 1,3,5,7) show women are more likely to be married when the proportion of men is high, with the largest effect at age 32 (significant at 5%). Women in the 75th percentile of male proportion are 3.6 p.p. more likely to be married than those in the 25th percentile. This suggests the increase in married mothers (Table II) is not solely due to fertility selection. For men (columns 2,4,6,8), the effect is smaller and consistent with Angrist [2002], who found a positive effect for women but no significant effect for men.

C.4 Effect on partners' characteristics

While my framework assumes people date within a *race* × *county* × *cohort* cell, women might explore other markets if their own has an unfavorable sex ratio. If this occurs, my estimates would be biased toward zero since the market I analyze is only a part of the actual market women face (see Section B.10 for derivation). To investigate, I use the IV framework from equations 2 and 3 to estimate the impact of *Proportion male* on two variables: the age difference between parents and whether the parents are of different races (Table A31). Column (1) shows that the age difference between parents does not change with sex composition. The coefficient on *Prop. male 2010* is small and insignificant—one standard deviation in the male proportion changes the age difference by just 0.06 years, suggesting women do not seek partners in different age groups when faced with an unfavorable sex ratio. Column (2) shows an increase in interracial partnerships—approximately 1.5%[45]—in response to a one standard deviation increase in the share of men, although the estimate is not statistically significant at the 5% level. This result would contradict the expectation that women would seek partners outside their race when men of their race are scarce and may instead reflect men's greater tendency to look outside their race in competitive markets. However, due to the imprecision of the estimate and the large standard error, I interpret this result with caution.

Table A31: Effect on Market

Model:	abs(Difference in age) (1)	Diff. Race Parents (2)
Prop. male 2010	-1.615	0.4240*
	(1.087)	(0.2381)
Dependent variable mean	3.5818	0.08616
Wald (1st stage)	76.913	51.041
Observations	6,259,559	6,300,696
Sig. at 5% (Lee et al. 2022)	No	No

Notes: The first outcome is the absolute value of the difference between parents' ages. The second outcome is a dummy for whether parents are of the same race. Each regression contains County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. The coefficient on *Prop. male 2010* corresponds to β in equation 3. Standard errors are clustered at the County-Race level. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A32: RF: Income Rank of Stayers

Model:	Female (1)	Male (2)
Prop. male at birth	0.184	0.106
	(0.116)	(0.113)
Dependent variable mean	0.45266	0.43570
Observations	3,493	3,503

Notes: Each observation correspond to race × county for people born in 1978-1983. The outcome is measured as the average income rank of those who still leave in the commuting zone of their childhood. The rank is relative to all children in their cohort. Controls include cohort size at birth and in the sample, and county and race fixed effects. Standard errors are Heteroskedasticity-robust. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Data source: Opportunity Insights

[45] Calculated as $1\text{SD} \times 0.424 = 0.0365 \times 0.424 = 0.01533$

Table A33: Alternative Measures of Partner's Quality

Panel A: Age measures

Dependent Variables:	Father older ≥5	Father younger ≥5	Age diff. ≥5	Teenage Father
Prop. male 2010	-0.1376	-0.0818**	-0.2194*	-0.0193*
	(0.1141)	(0.0406)	(0.1206)	(0.0104)
Dependent variable mean	0.241	0.030	0.271	0.012
Observations	6,095,693	6,095,693	6,095,693	6,095,693
Sig. at 5% (Lee et al. 2022)	No	No	No	No
Wald KP (1st stage)	79.7	79.7	79.7	79.7

Panel B: Education measures

Dependent Variables:	Same Edu. Level	Father More Educated	Father < HS
Prop. male 2010	0.0800	-0.0190	-0.0425
	(0.1093)	(0.0903)	(0.0577)
Dependent variable mean	0.419	0.215	0.025
Observations	6,105,173	6,105,173	6,116,977
Sig. at 5% (Lee et al. 2022)	No	No	No
Wald KP (1st stage)	79.7	79.7	78.9

Notes: Each column reports IV estimates from regressions where the endogenous variable is the proportion of men in 2010, instrumented with the proportion of men at birth of the cohort. Panel A reports outcomes capturing partner age differences; Panel B reports outcomes capturing educational sorting. *Same Edu. Level* is an indicator for father and mother having the same years of education, *Father More Educated* an indicator for the father having strictly more years than the mother, and *Father < HS* is an indicator for the father not having completed high school (i.e., fewer than 12 years of schooling, capturing the bottom of the paternal education distribution). Each regression includes controls for cohort size in 2010 and at birth, County×Age at delivery, Race×Mother's Birth Year, and Race×Age at delivery fixed effects. The sample is restricted to dating markets with population between 200 and 5,000. Hispanics are excluded. Standard errors are clustered at the County–Race level. Wald statistic (Kleibergen–Paap) for the first stage is reported together with an indicator for significance at the 5% level according to the tF statistic (Lee et al. 2022). *Conventional significance levels*: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

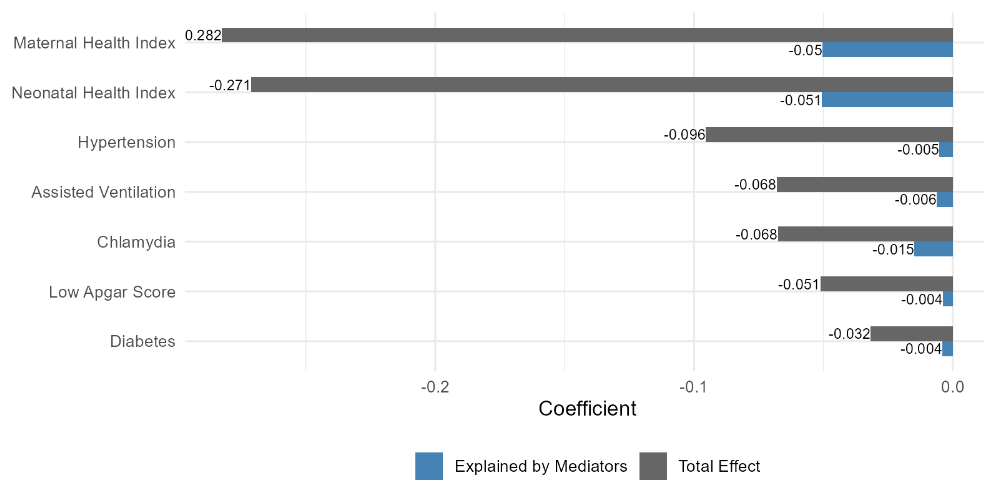
C.5 Mediator Analysis

To address the concern that part of the estimated effect may reflect selection, I conduct a decomposition exercise that investigates how much of the observed effect can plausibly be explained by compositional changes in maternal and partnership characteristics. Specifically, I focus on three socio-demographic variables that significantly shift in response to sex composition: marital status, maternal education, and paternal education.

The approach proceeds in two steps. First, I estimate the associations between these characteristics and maternal and neonatal health outcomes using OLS regressions (Table A18). While these estimates are not causal, they likely capture the combined influence of causal effects and positive selection, and therefore provide an upper bound on the potential impact of these characteristics. Second, I multiply the observed shifts in each characteristic induced by sex composition with the corresponding OLS associations, and then sum the implied effects across all mediators. This provides an estimate of the total effect that can be attributed to selection on observables.

Figure C.24 reports the results. Across health outcomes, these mediators explain between 5 percent (hypertension) and 22 percent (chlamydia) of the total effect. Marital status is the single largest contributor, accounting for 16 percent of the chlamydia effect and 8–9 percent of the maternal and neonatal health indices. The remaining share of the effect—roughly four-fifths—cannot be accounted for by selection on these observables.

Figure C.24: Coefficients explained by composition



Notes: This figure decomposes the total effect of the proportion of males in the market on key outcomes. Gray bars represent the total estimated effect from the baseline specification. Blue bars show the portion of this effect accounted for by observed mediators: maternal marital status, father's education, mother's education, and overweight status. The contribution of mediators is calculated as their effect on outcomes multiplied by the how much they are impacted by the change in proportion male.

This exercise should be interpreted with caution: the decomposition is illustrative rather than causal, and the OLS associations likely overstate the true effects of the mediators because they also capture selection into these characteristics. It's worth noting that the compositional shifts themselves—such as the movement of fertility toward marriage—are plausibly a direct consequence of more favorable conditions in the dating market, reinforcing the interpretation that sex ratios shape health outcomes through partnership dynamics. Nonetheless, the results indicate that while selection accounts for part of the relationship between sex ratios and health, a substantial share remains unexplained. This residual component is consistent with other channels, such as improved within-relationship bargaining power for women.

C.6 Implications of Dating Market Imbalances for Men

While the main analyses in this paper focus on women—reflecting the absence of comparable male measures in natality microdata—it is equally important to assess how dating market imbalances affect men. This section presents two forms of evidence: (i) analysis of singlehood in the American Community Survey (ACS), and (ii) a review of the broader literature on men's outcomes.

A common concern is that increases in the male share mechanically raise the prevalence of unpartnered men, which is often associated with negative health and socioeconomic outcomes. To test this, I replicate the analysis of Angrist [2002] using the 2015 ACS and my instrument. Although the ACS does not permit perfect alignment with my instrument, I use county-level variation by age group, focusing on racially homogeneous counties and a fuzzy age-group match. Consistent with prior work, OLS estimates show a significant decline in the never-married rate among women in more male-heavy markets, but no detectable effect on men. While "never married" is only a proxy for unpartnered status—and may miss non-marital partnerships—these findings suggest that higher male shares do not necessarily come at the cost of large increases in unpartnered men.

Beyond singlehood, the literature documents heterogeneous effects of sex ratios on male outcomes. Health effects for men are mixed and appear context-specific. In the UK, Kang and Pongou [2020] find that a one–SD increase in the male share reduces the probability of chlamydia among men by 0.42 percentage points,[46] consistent with more stable relationships and reduced risky sexual behavior. In contrast, Zhang et al. [2021] document that higher male shares in China increase depressive symptoms among men. Mortality findings are similarly

[46] Original estimates in sex ratios were converted by the author to male-share units and scaled using the U.S. standard deviation.

Table A34: Sex Ratios and Share Never Married

	OLS		2SLS	
	Female	**Male**	**Female**	**Male**
Dependent variable: Share never married				
Proportion male (2010)	−0.2343**	0.0065	−0.3984	0.1008
	(0.0753)	(0.0707)	(0.3380)	(0.3457)
SEs clustered by County	Yes	Yes	Yes	Yes
Dependent var. mean	0.354	0.479	0.354	0.479
Observations	5,186	5,186	5,186	5,186
First-stage F	–	–	112.8	112.8

Notes: Each column reports coefficients from regressions at the county × age-group cell level, estimated separately for women and men. The dependent variable is the share never married in the cell, calculated from ACS 5-year estimates. Because the ACS does not report age by marital status by race jointly, the race dimension is omitted; instead, the sample is restricted to racially homogeneous counties, defined as those where at least 90% of the population is White. Age bins in the ACS do not perfectly align with the 2010 birth-cohort structure of the instrument, so the closest available mapping is used: 2010 ages 15–19, 20–24, 25–29, and 30–34 are matched to ACS 2015 ages 20–24, 25–29, 30–34, and 35–39. The OLS columns estimate the association between the 2010 male share and the 2015 never-married rate, while the 2SLS columns instrument the endogenous male share using cohort-by-county sex composition at birth.
Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

inconclusive: Chang et al. [2024] link higher male shares in the early thirties to elevated later-life mortality for both sexes in Taiwan, whereas Barclay [2013] find no mortality effect in Swedish registry data.

Evidence on life satisfaction and relationship quality is more favorable. Using historical Australian data, Grosjean and Brooks [2017] find that higher male shares increase both life and marital satisfaction for men and women, potentially reflecting the benefits of more stable partnerships for those who marry. These positive effects for partnered men may offset, at least partially, any disadvantages experienced by men who remain single.

Overall, the evidence does not support a simple zero-sum view. Strengthening women's dating-market position may improve some male outcomes (e.g., lower STI risk, higher relationship satisfaction) while worsening others (e.g., mental health in some contexts). The net welfare effect depends on how these channels balance, particularly for unpartnered men. The substantial gains for women documented here should be weighed against the more modest but meaningful potential effects on men, noting that the present analysis necessarily focuses on one side.

C.7 Migratory Response to Unfavorable Dating Market

Another potential mechanism is migration. An unfavorable sex ratio at birth may induce individuals to relocate to areas with a more favorable dating market. In this sense, migration can be viewed as an endogenous response to dating market imbalances and thus an extension of that channel. If this migration is not selective, it poses no threat to identification beyond potentially weakening the first-stage relationship. However, if migration is selective—for instance, if higher-quality individuals are more likely to move—it could affect the estimated effects.

Using census data on migration flows (2011-2015), I show that women are more likely to leave counties with a scarcity of men and move to places where men are relatively plentiful. I construct yearly arrival and departure rates for both genders and regress them on the proportion of male births in two cohorts (ages 15-24 and 25-34 in 2010). The data are not desegregated by race or age, so I restrict the sample to racially homogeneous counties. I estimate the following equation:

$$y_c^g = \alpha + \beta^g_{15-24}\text{Prop. male at birth: 15-24}_c + \beta^g_{25-34}\text{Prop. male at birth: 25-34}_c + \gamma^g X_i + \epsilon_c \tag{13}$$

Where y_c^g is the arrival or departure rate for gender g in county c. Rates are defined as the count of departing or arriving individuals divided by the county population. I control for the cohort size in 2010. The parameters β^g_{cohort} identify the migratory response to the instrument for gender g. Tables A36 and A35 presents the estimation results.

Table A36 indicates that women are less likely to leave counties with a favorable sex composition in cohort 25-34 (column 2), while the effect for men is smaller and not significant (column 1). Table A35 shows that female arrival rates increase with the proportion of men in cohort 15-24 (column 2), with no significant effect for men (column 1).

Table A35: In Migration

Dependent Variables:	Male arrival rate	Female arrival rate
Model:	(1)	(2)
Prop. male birth: 15-24	0.0714	0.1167***
	(0.0633)	(0.0442)
Prop. male birth: 25-34	-0.0112	0.0072
	(0.0410)	(0.0313)
Dependent variable mean	0.06990	0.05890
Observations	1,727	1,727

Table A36: Out Migration

Dependent Variables:	Male departure rate	Female departure rate
Model:	(1)	(2)
Prop. male birth: 15-24	-0.0158	0.0295
	(0.0498)	(0.0425)
Prop. male birth: 25-34	-0.0540	-0.0739***
	(0.0365)	(0.0361)
Dependent variable mean	0.06889	0.06248
Observations	1,735	1,735

Notes: The outcome variable is the count of yearly (male or female) in (and out)-migration to (from) a county (in years 2011-2015) divided by the population size (of men or women). Two independent variables measure the proportion of male births in this county in cohorts 15-24 and 25-34. The sample includes counties where 80% of individuals are of the same race. Regressions are weighted by the population. Controls include the log of cohort size. Standard errors are heteroskedasticity-robust. *Conventional Significance Levels:* $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

This migratory response helps explain why the first-stage magnitude in Table I is less than one: women leave counties with unfavorable sex ratios, evening out the sex composition. Xiong [2022] observes a similar pattern in China.

Nonetheless, additional evidence suggests that migration is not strongly selective, at least along observable dimensions such as income.[47] Although direct comparison data is unavailable, I use income rank data from the Opportunity Insights dataset to assess this. If women with better outcomes (proxied by income rank) are more likely to leave when the sex ratio is unfavorable, the average income rank of women who stay should decrease as the sex ratio decreases. To test this, I run the following regression:

$$rank.stayed_{crg} = \beta^g \text{Prop. male at birth}_{cr} + \gamma^g X_{cr} + \lambda_c^g + \delta_r^g + \epsilon_{crg} \tag{14}$$

Where $rank.stayed_{crg}$ represents the average income rank of stayers in county c, race r, and of gender g. The main independent variable is the proportion of male births in county c and race r at the time of cohort's birth. Controls include cohort size and race/county fixed

[47] I assume that individuals do not "overshoot" in their migratory response—i.e., those originating from areas with highly skewed sex ratios do not relocate to such an extent that these areas end up with more balanced or even reversed sex ratios compared to those with initially less skewed ratios. This assumption is consistent with migration acting to equilibrate supply and demand in the dating market and is necessary for the monotonicity condition underlying the instrumental variable strategy.

effects. Table A32 shows that the income rank of stayers is not related to the instrument. The coefficients β for both men and women are small and statistically insignificant. A one standard deviation change in the proportion of male births has an effect of 0.0067 for women and 0.0038 for men. This suggests that migration in response to the dating market does not alter the composition of stayers. While the evidence is somewhat reassuring, it does not fully rule out selective migration as a potential channel. However, any selection appears modest and likely reflects a broader behavioral response to dating market conditions, rather than a distinct mechanism.

C.8 Bootstrap

The IV estimation sample comes from the original IV sample, representing all mothers from the markets between 200 and 5000 people. The comparison sample comprises Black and White mothers from all the markets such that there were at least 200 people on the market, and both groups were present in the same county and age group. Each bootstrap iteration proceeds in two steps. In the first step, I draw with replacement the same number of clusters ($county \times race$) as in the original sample. Next, I run the IV regression on this sample and save the estimates. In the second step, I draw with replacement the same number of counties as in the entire comparison sample and calculate the empirical gap in health outcomes. Then, using the estimates from the first step and the counterfactual sex compositions, I predict the counterfactual health outcomes for all mothers. Finally, I compute the counterfactual racial gap in health. I repeat the bootstrapping for 1000 iterations.

C.9 Relaxing Market Rigidities

A natural margin to mitigate sex-imbalance harms is wider interracial partnering. I quantify this with a pooled counterfactual in which Black and White women draw from the same pool of men (i.e., race no longer segments the market, Table A37). The pooled male share is 0.498, implying that Black women's male proportion rises from 0.4705 to 0.498 (+2.75 pp) while

White women's falls from 0.504 to 0.498 (–0.63 pp).[48] I map sex-ratio shifts into predicted outcome changes using the paper's IV coefficients:

$$\text{Effect}_g \;=\; \Delta\text{Prop}_g \times N_{\text{women},g} \times \beta,$$

where $N_{\text{women},g}$ is group size and β is the outcome-specific coefficient. Using total female populations, the implied effects are 166,772 β for Black women and $-$149,781 β for White women, yielding a net gain of 16,991 β. Because $\beta < 0$ for the adverse outcomes studied, the net effect is an unambiguous reduction in adverse outcomes. Rescaling N to births gives a concrete magnitude for low Apgar: with 2011–2019 births (Black: 4.94m; White: 16.92m) and $\beta = -0.0512$, pooling implies about 6,976 fewer low-Apgar cases among Black newborns and 5,454 more among White newborns, for a net decline of 1,522 cases.

While the counterfactuals suggest that interracial partnering could mitigate adverse sex-ratio effects, the reality is more complex because simulations assume Black and White men are fully substitutable. Sociological research highlights that race functions as a status characteristic, so partnering with a White man may shift intra-household bargaining power (Merton [1941], Kalmijn [1998]). Moreover, White men are on average less disadvantaged economically, which could improve the situation of Black women in such relationships.

To provide context, I analyze natality data comparing outcomes for Black mothers with Black versus White partners (Table A38). Black mothers partnered with White fathers experience better maternal and infant outcomes. These differences reflect selection and endogeneity, so they are not causal, but they are informative: such partnerships are not associated with worse outcomes and may plausibly alleviate some of the negative consequences of skewed sex ratios.

Equivalently, one can relax other common search rigidities—age and geography. Relaxing

[48] In altering market rigidities, I isolate the dimension under study (here, race) while aggregating over age and geography in both the baseline and counterfactual. Performing the aggregation in the opposite order—first calculating changes within other dimensions and then aggregating—produces very similar results. The exercise is mechanical and does not account for behavioral responses or general-equilibrium feedback.

age segmentation for Black women (pooling ages 15–34 into a single market) yields mixed implications. The pooled male share is 0.4713, which is below baseline for younger women (15–19: -2.98 pp; 20–24: -0.22 pp) and above baseline for older women (25–29: $+1.76$ pp; 30–34: $+2.45$ pp). Aggregating with female–population weights delivers a small improvement of 5,316 β. In contrast, aggregating with births—heavily concentrated among the younger ages where $\Delta < 0$—produces $-15{,}759\,\beta$, which maps (with $\beta = -0.0512$) to an increase of about 807 low-Apgar cases. Intuitively, even though age pooling raises male supply at older ages, the birth-weighted margin moves in the opposite direction because many Black births occur among teenage girls, when proportion male is relatively balanced.

Next, I relax within-state geographic segmentation by allowing Black women to draw partners from the entire state (pooling markets across counties). This mechanically replaces each county's male share with the state-pooled share, generating heterogeneous Δ's across space. While negative Δ's are common, the positive Δ's tend to occur in counties with large numbers of women, so population weighting shifts the aggregate to a modest improvement of 9,919 β (Panel C) when weighted by population and 1,568 β when weighted by births. This would lead to a small reduction in 80 low Apgar Scores.

It needs to be acknowledged that these counterfactuals are also stylized and illustrative, not causal. They suggest aggregate direction and order of magnitude only. They are mechanical and assume substitutability across ages/counties—thus abstracting from mobility costs, selective sorting, and potential shifts in intra-household bargaining power, match quality, or resources.

Table A37: Counterfactuals Relaxing Market Rigidities Across Race, Age, and Geography

	Baseline	Pooled/CF	Δ	Female pop.	Effect (pop.)	Births 2011–2019	Effect (births)	Δ Apgar
Panel A: Race — Merge Black and White dating markets into one pool								
Black women	0.4705	0.498	**+0.0275**	6,054,861	166,772β	4,944,863	136,242β	–6,976
White women	0.5043	0.498	**–0.0063**	23,792,228	–149,781β	16,919,170	–106,522β	5,454
Net / total	–	0.498	**+0.0006**	29,847,089	**16,991**β	21,864,033	**29,720**β	**–1,522**
Panel B: Age — Pool Black dating market ages 15–34 into a single market								
15–19	0.5011	0.4713	**–0.0298**	1,687,257	–50,231β	1,581,689	–47,082β	2,411
20–24	0.4735	0.4713	**–0.0022**	1,560,194	–3,360β	1,618,571	–3,485β	178
25–29	0.4537	0.4713	**+0.0176**	1,435,390	25,270β	1,151,327	20,269β	–1,038
30–34	0.4468	0.4713	**+0.0245**	1,372,020	33,637β	593,276	14,545β	–745
Total	0.4705	0.4713	**+0.0009**	6,054,861	**5,316**β	4,944,863	**–15,759**β	**807**
Panel C: Geography — Pool Black dating markets across counties within the state								
Total effect (all counties)	0.4683	0.4699	**+0.0016**	6,054,861	**9,919**β	4,944,863	**1,568**β	**–80**

Notes: Each panel reports a mechanical counterfactual that relaxes one rigidity while aggregating over other dimensions (both in baseline and counterfactual). *Baseline* is the observed male proportion; *Pooled/CF* is the counterfactual proportion after pooling; Δ is the change in proportion male. *Effect (pop.)* and *Effect (births)* multiply Δ by the number of women or births and the coefficient β from the IV regressions; here β for low Apgar is -0.0512. Δ Apgar converts births-weighted changes into predicted changes in occurences of low Apgar scores: $\Delta\text{Apgar} = \text{Births} \times \Delta \times \beta$. Panel C widens the dating market to the states; In the panels B and C only Black populations are considered. The change in sex composition is calculated after aggregating over county and age group (Panel A and C) or county (Panel B).

Table A38: Black Mothers: Maternal and Infant Outcomes by Father's Race

Outcome	**Father Black (N = 3,155,849)**	**Father White (N = 203,696)**	**Diff. (pp)**	***p*-value**
Married	41.02	56.92	−15.91	< 0.001
Chlamydia	3.33	1.75	1.58	< 0.001
Gonorrhea	0.65	0.22	0.43	< 0.001
Syphilis	0.23	0.10	0.13	< 0.001
Diabetes	1.16	1.00	0.16	< 0.001
Hypertension	3.51	2.82	0.69	< 0.001
Low birthweight	12.69	10.23	2.46	< 0.001
Preterm birth	15.80	13.57	2.23	< 0.001
Apgar < 7	3.09	2.69	0.39	< 0.001
Assisted ventilation	4.45	4.46	−0.01	0.012
Infant death	0.47	0.42	0.05	0.001

Notes: Each entry reports the percentage of births to non-Hispanic Black mothers, within the specified father's race category, for which the outcome occurs. "Diff. (pp)" is the difference in these percentages, expressed in percentage points. *p*-values are from two-sample *t*-tests. The sample includes all births to Black mothers between 2011 and 2019 for which both parents' race is reported. Consistent with the analysis, Hispanic parents are excluded.

D Theoretical Framework

D.1 Dating market model

I solve a dating market model which demonstrates the effect of sex ratio on the equilibrium female welfare. Suppose that there is a population of men and women. Each person i has a utility function composed of a private good q and a public good Q and it has the form: $u_i(q_i, Q) = q_i Q$. Price of the private good is normalized to 1 and price of public good is p. Income (which can be conceived also as quality or human capital) of an individual is drawn from a uniform distribution $y_g \sim U(1, 2)$, where g is gender and $g \in \{m, f\}$. Mass of women is normalized to 1 and mass of men is equal to S which reflects the sex ratio. Without loss of generality, let's assume that $S < 1$, i.e. there is surplus of women on the dating market. Men and women can form couples in which case they maximize joint utility $(q_m + q_f)Q$. The main benefit of being in a couple stems from sharing the public good Q. However, the allocation of resources toward private goods, and hence the final utility, is a result of matching and bargaining in equilibrium. The goal of each woman (man) is to find a partner who maximizes her utility. The natural constraint is that partners must accept each other. These two forces, together with the distribution of partners, drive the equilibrium outcomes. With this model, I aim to show how changes in the sex composition affect female utility in equilibrium. The equilibrium of the dating market is defined as the matching and resource allocation such that no man or woman would prefer a partner different than their match. To solve for the equilibrium I proceed in three steps:

1. **Within couple maximization** Couples maximize their joint surplus S subject to the budget constraint:

$$S(y_f, y_m) = \max_{q_f, q_m, Q} (q_f + q_m) Q \qquad \text{s.t. } H_f + H_m + PQ = y_m + y_f$$

For this particular form $S = \frac{(y_m+y_f)^2}{4P}$, that is, surplus is supermodular in incomes. Mathematically, it translates to second derivative being positive: $\frac{\partial^2 S}{\partial y_m \partial y_f} > 0$. Intuitively, it means that an increase in surplus from additional income of a woman (man) is higher if their partner has high income as well.

2. **Matching** As the surplus is supermodular, it is a well known property of the matching models that matching will be assortative in incomes. That is, the highest income man matches with the highest income woman, and so on. Let the match of woman y_f be $\theta(y_f)$. Given the uniform distribution of income, assortativity requires that the mass of women with income above y_f must equal the mass of men with income above $\theta(y_f)$. Hence, the match of women is $y_m = \theta(y_f) = 2 - \frac{2-y_f}{s}$. This equation shows the first channel through which the sex composition affects female outcomes. The higher relative abundance of men, the better partner a woman can secure.

3. **Individual utility allocation** To solve for the allocation of resources toward private goods within the couple, I use two conditions that need to be satisfied in the equilibrium.

(a) Marriage participation constraint: $U_m^m(y_m) + U_f^m(y_f) \geq S(y_m, y_f) \forall y_m, y_f$. For any pair of man and woman, their individual equilibrium utilities must be higher or equal to the surplus they would create as a couple. The inequality is strict for any couple not matched in the equilibrium, and it is an equality for couples matched in the equilibrium. This condition is related to the stability of matching: switching partners could never generate enough of surplus to make the new couple better off.

(b) No surplus for last woman in a relationship

Since there is more women than men, some women at the bottom of the income distribution remain single. This condition states that last married women is indifferent between being single and being in a relationship.

The above conditions pin down female utility in equilibrium. In particular it is equal to:

$$U(y_f) = \frac{1}{2P}\left((\frac{x^2}{2} - \frac{(2-s)^2}{2})\frac{s+1}{s} + (x-2+s)\frac{2(s-1)}{s} \right) + \frac{(2-s)^2}{4P}$$

Importantly, it can be shown that: $\frac{\partial U(y_f)}{\partial S} > 0$ That is, female utility in equilibrium increases with the sex ratio. There are two channels leading to this result. The first one is matching. As there are more men available on the market, woman can secure a higher quality partner. The second one is the resource allocation. As there is more competition among men, they need to provide women with higher private consumption to sustain the partnership.

D.2 Dating market model: restrictions on men available

The previous model assumes men are added across the entire income distribution when increasing the sex ratio. But would the effect differ if the increase comes only from the lower end of the income distribution, such as releasing incarcerated individuals, who likely have lower potential income? To explore this, I adapt the model to assume that the sex ratio adjustment occurs only among men with income below a threshold t. Solving for equilibrium utilities, I show that all women's utilities increase, even if only low-income men are added to the dating pool. In fact, women at the top of the income distribution benefit the most, regardless of the quality of men added.

The new assumption about male distribution is illustrated in Figure D.25a. The mass of men with income $y_m > t$ (blue rectangle) remains unchanged. Any change in S comes from adding or removing men with income $y_m < t$. The green rectangle represents men with income below t on the market, while the red rectangle shows the "missing" men. Increasing S reduces the red rectangle and expands the green one. Adjusting t allows us to model the potential income of men, such as incarcerated individuals. A lower t reflects lower potential income for men entering the market. When $t = 2$, we return to the baseline scenario from subsection D.1. Solving for equilibrium female utility using this new distribution, I obtain:

$$U(y_f) = \frac{1}{2P}\left((\frac{x^2}{2} - \frac{(2-s)^2}{2})\frac{s-3+2t}{s-2+t} + (x-2+s)\frac{t(s-1)}{s-2+t} \right) + \frac{(2-s)^2}{4P}$$

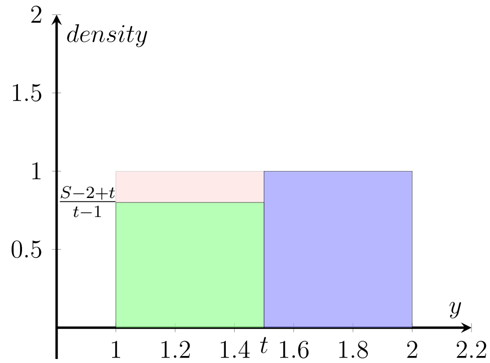
I now use this framework to explore the impact of changing the sex ratio under different assumptions about the men added to the dating pool. Specifically, I examine increasing the sex ratio from 0.9 to 0.95 (or from 47% to 49% male) under two values of $t \in 1.2, 1.8$[49]. The first, $t = 1.2$, represents adding only low-income men, while $t = 1.8$ includes both high- and low-income men.

For each t, I calculate the change in individual female utility (*individual treatment effect*) from the sex ratio increase. Figures D.25c and D.25d display the results.

[49] I set the price of the public good $P = 1$

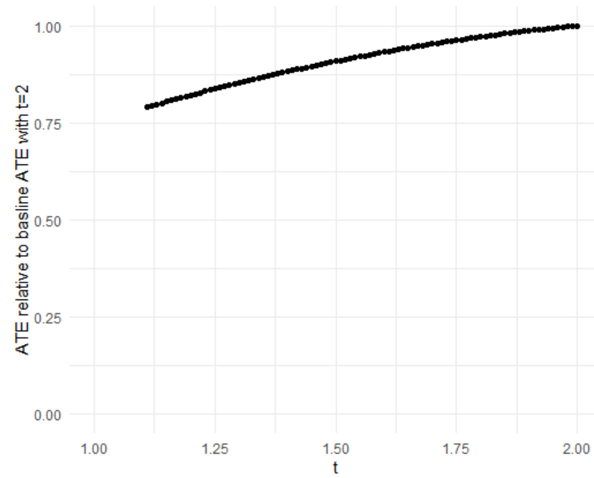
Figure D.25: Model

(a) Distribution of Men



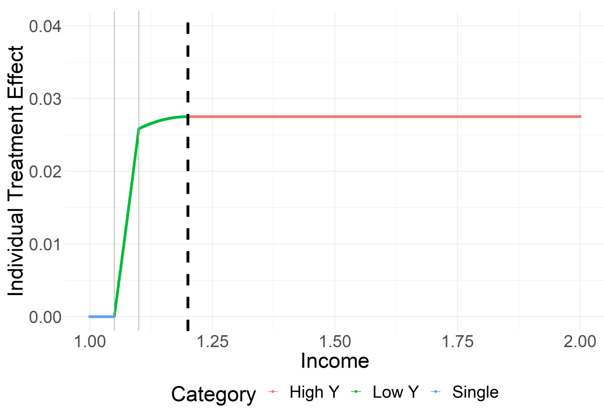
Notes: Plot shows the distribution of men on the dating market. The blue rectangle shows men with income above t, with mass equal to $2-t$. The green rectangle represents men with income below t on the market. The mass of men with $y_m < t$ is equal to $\frac{S-2+t}{t-1} * (t-1)$. The red rectangle corresponds to the "missing" men.

(b) ATE(t) relative to ATE(2)

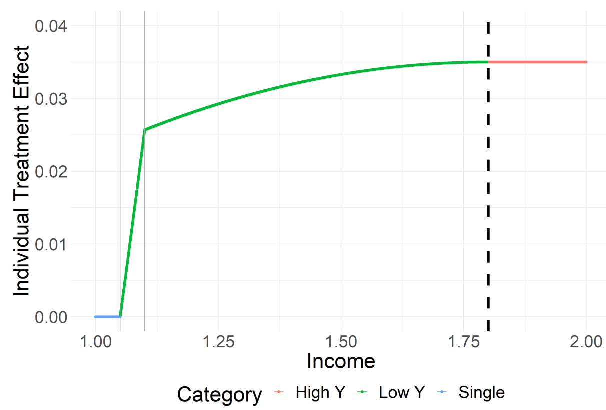


Notes: Plot shows the ratio of average treatment effect (ATE) for a given level of t compared to ATE at $t = 2$. The average treatment effect is the average change in the female utility across the income distribution for a change in the sex ratio from 0.9 to 0.95.

(c) Change in Female Utilities for t=1.2



(d) Change in Female Utilities for t=1.8



Notes: Plots show the changes in female utility as the result of an increase in the sex ratio from 0.9 to 0.95 when $t = 1.2$ and $t = 1.8$. The dashed line shows the value of t. The colors represent three groups of women. Blue shows women who were previously single and remain single. Green represents women below income t who have a partner. Women between two grey lines did not have a partner before and now have a partner. Red represents women who have income above t.

The impact of changing the sex ratio can be divided into four groups. First, women who were single and remain single (blue line), located at the bottom of the income distribution, experience no change in utility. Second, women with income below t who were previously single but now have a partner (between the grey lines) see an increase in utility from being in a relationship. Third, women with income below t who already had a partner benefit from both a slightly better partner and an improved outside option, enhancing their bargaining position and allowing them to negotiate more favorable resource allocations. Previously, the outside option of the last woman in this group was to be single. Now, her outside option is to be married to the man just below her current partner (previously such man was not on the market). As a result, her current partner needs to provide her with higher utility (more private good) to prevent her from switching to the outside option. Intuitively, her bargaining position improved and she can negotiate a more favorable allocation of resources. Lastly, women with income above t (red line) have the greatest utility increase, even though their partner doesn't change. Their improvement comes entirely from a better outside option. Women with $y_f > t$ can threaten their current partner to leave and date a man just below who now provides a higher utility to their partner. Hence, current partners of women with $y_f > t$ need to allocate more resources to female private good to maintain the relationship. Thus, increasing the pool of available men always benefits women at the top of the distribution.

Comparing the subplots D.25c and D.25d, the changes in utilities are not drastically different. The main utility increase comes from women who were single but are now married, improving the outside option for all subsequent women. In subplot D.25d, more women switch to higher-quality partners, but this contributes less to the utility gain than the effect of switching from singlehood to marriage.

I calculate the average increase in utility across the female income distribution (*Average Treatment Effect, ATE*) and compare them for different values of t. Figure D.25b shows the ratio of ATE for each t relative to the ATE at $t = 2$. Even at the lowest t, where only low-income men are added, the ATE is still more than 75% of the baseline ATE when men across the whole distribution are added to the pool.

Therefore, I conclude that the quality of men added to the dating pool has relatively little effect on the magnitude of increase in the utility and always affect women with high incomes.