Race and Mobility in U.S. Marriage Markets: Quantifying the Role of Segregation

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We investigate racial disparities in the marriage market and their consequences for intergenerational mobility by linking American Community Survey respondents to their parents' tax records. After controlling for parental income rank, we document large racial gaps in marriage rates. Counterfactual simulations find that these gaps explain the entire White-Hispanic and White-Black gap in upward family income mobility among women, and compress Asian women's advantage in upward personal income mobility by 2/5. The former results are driven by differential opportunities to match with a high-income partner, while the last result is driven by differential partnering rates conditional on opportunities. Similar but quantitatively smaller patterns are found for men. We then leverage observed cross-area variation to estimate selection-corrected treatment effects of racial segregation on upward mobility. Our estimates attribute 1/3 of the White-Black gap in upward mobility of partner and total family incomes to this channel. We argue that our approach identifies "short-run" effects and present simple calculations suggesting that our estimates double over a multigeneration horizon.

JEL: D31, J12, J15

Keywords: marriage market, intergenerational mobility, tax records, family income, racial inequality, racial segregation, race-gender intersection, selection correction

One hundred and sixty years after the end of slavery, large income differences between racial groups persist in the United States. The relative income of Black Americans improved with the inclusion of paid Black labor in the industrial economy, migration out of the South, greater

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schooling completion, and civil-rights-era programs,[1] but has largely stagnated since 1970 (Bound and Freeman 1992; Bayer and Charles 2018). The absence of meaningful convergence over this horizon has motivated recent research that analyzes racial disparities from an intergenerational perspective (e.g. Killewald and Bryan 2018; Akee et al. 2019; Chetty et al. 2020; Davis and Mazumder 2020; Collins and Wanamaker 2022; Derenoncourt 2022).

Research on race and intergenerational mobility has made important strides, but has not fully considered the distinction between personal income and family income concepts. During the period in which the racial gap in personal incomes stagnated, the United States experienced a massive decline in marriage rates that occurred unevenly across demographic groups (Tucker and Mitchell-Kernan 1995; McLanahan and Percheski 2008; Cherlin 2014; Raley et al. 2015; Lundberg et al. 2016; Binder and Bound 2019). An approach that considers the marriage-market as well as labor-market determinants of family income can shed richer light on racial inequality, its intersection with gender inequality, and their driving forces.

In this paper, we 1) estimate disparities in upward intergenerational mobility across race and gender; 2) decompose our family income estimates into personal income, partnership formation, and partner income margins; and 3) estimate treatment effects of childhood exposure to racial segregation on each margin. We accomplish these aims with a dataset of adult individuals' American Community Survey (ACS) responses merged to their parents' income tax forms. Our data contain information on childhood location, childhood family income (CFI), race-ethnicity, adulthood income, and marriage-market outcomes for more than one million adults born between 1979 and 1986 and observed in 2011-2019. We use ACS relationship identifiers to consider unmarried as well as married couples in our analysis. We also identify a subset of couples that are not identifiable in the ACS or in the tax filing statuses alone through our merge between the two.

First, we present descriptive statistics. Our concept of interest is *absolute* (or *upward*) mobility, which is a group's residual position in the adulthood outcome distribution after conditioning on CFI rank (e.g. Mazumder 2008; Chetty et al. 2014, 2020). Relative to White individuals of a given gender, we find that Asian women and men have more upwardly-mobile personal incomes;

[1] Myrdal (1944) first reported on the persistence of racial disparities in the U.S. economy. For research documenting and explaining partial convergence, see Smith and Welch (1989); Donohue and Heckman (1991); Aaronson and Mazumder (2011); Collins and Wanamaker (2014); Margo (1995, 2016); Derenoncourt and Montialoux (2021).

Hispanic women and men and Black women have similar personal income mobility; and (as previously emphasized by Chetty et al. 2020) Black men have lower personal income mobility. We then show that these groups experience less upward mobility via the formation of romantic partnerships—particularly Black women, who are *half* as likely to be partnered as White women after conditioning on CFI. Conditional on finding a partner, we document smaller differences in partner income mobility. In sum, while White individuals experience greater upward mobility in family income than they do in personal income, the opposite is the case for other groups.

Next, we quantify the roles of marriage-market allocations in upward family income mobility. We first show that randomly redistributing partner incomes (within gender and CFI rank) would eliminate the entire White-Black mobility gap and the entire White-Hispanic mobility gap among women, and would moderately shrink these gaps among men. It would also substantially increase Asian women's mobility advantage over White women. Further simulations reveal that most of the observed White-Black and White-Hispanic mobility gaps among women are generated by differential *opportunities* to match with a high-income partner, as opposed to differential *partnering rates* conditional on opportunities. Among men, the smaller mobility gaps we document are driven mostly by differential partnering rates. These findings suggest that policies targeting partnering rates alone would be insufficient to address the strong disadvantages in the marriage market faced by Black and by Hispanic women.

Lastly, we analyze the role of childhood exposure to racial segregation in generating the observed mobility disparities. We build on the approach pioneered by Chetty et al. (2014), who documented cross-area variation in mobility and correlations with various neighborhood characteristics (including segregation). While the cross-area approach provides important suggestive evidence, its estimates cannot be interpreted as formal treatment effects. One refinement to this approach leverages moves from "worse" to "better" areas to estimate the effect of exposure to a better area on upward mobility (e.g. Chetty et al. 2016; Chetty and Hendren 2018a,b; Chetty et al. 2018; Chyn 2018; Baran et al. 2023; Chyn et al. 2023). This refinement identifies the treatment effect of an *omnibus of exposures* associated with the move.

Our approach aims to identify the treatment effect of a *specific exposure*, which here is racial segregation, on upward mobility. We do so by modifying the cross-area regression with a control function generated by information on the area's CFI distribution. Together with methodology specified by Oster (2019), this modification adjusts for nonrandom selection into the exposure of

interest and generates an average treatment effect of this exposure under a set of assumptions. Essentially, observed correlations between local CFI distribution and segregation exposure partially adjust for selection into more versus less segregation exposure. The observed change in the segregation coefficient after applying these controls is extrapolated according to Oster's formula—parameterized according to assumptions whose robustness we test—that removes remaining unobserved confounders.

We estimate small positive (negative) selection-corrected effects of segregation exposure on White women's (men's) upward personal income mobility, and offsetting effects on partner income mobility, leading to precise null effects of segregation on family income mobility. In contrast, we estimate large (small) negative effects of segregation on Black men's (women's) personal income mobility, and large (even larger) negative effects on partner income mobility. These cumulate to large negative effects on family income mobility for both genders. We calculate that complete desegregation would remove 3.9 rank points (30.7%) of the mobility gap in women's family income, and 4.3 rank points (37.1%) of the gap for men. We argue that these represent effects of a desegregation policy implemented in the current generation of children, leaving prior generations' exposures constant: longer-run effects could be considerably larger.

Our paper advances two strands of literature. First, our focus on racial inequality and its interaction with gender furthers literature that applies an intergenerational perspective to the study of marriage markets (see also Mazumder 2008). Chadwick and Solon (2002) first analyzed the contribution of the marriage market to intergenerational family income mobility, focusing on women only. Others have studied assortative matching on parental income or wealth (Ermisch et al. 2006, Charles et al. 2013, Choi et al. 2020, Wagner et al. 2020, Fagereng et al. 2022). Our simulations are the first attempt to quantify the role of marriage market processes in generating racial gaps in family income mobility.

Second, our work extends literature that estimates effects of racial segregation on racial disparities. Most prior literature examines intragenerational outcomes (e.g. Cutler and Glaeser 1997; Card and Rothtstein 2007; Ananat 2011; De La Roca et al. 2018; Cox et al. 2022; Goldman et al. 2025), but two recent examples extend the investigation to an intergenerational context. Derenoncourt (2022) found that Great-Migration-induced shocks to Black population shares in Northern cities reduced Blacks' upward mobility, without much affecting Whites', through its effects on segregation and associated exposures. Chyn et al. (2024) estimated effects of segregation

on upward mobility by using cross-city variation in historical railroad placement as an instrument. Our study differs in two ways. First, in contrast to their focus on political-economy mechanisms, such as local public good provision and voter attitudes, our work focuses on marriage-market effects. Second, their study reports a larger effect of segregation on the Black-White family income mobility gap. As we discuss in Section V.D, our approach approximates the effects of a desegregation policy implemented in the current generation, while theirs plausibly identifies a longer-run effect.

The paper proceeds as follows. Section I describes our data. Section II reports our descriptive findings. Section III simulates counterfactual marriage-market allocations and their effects on family income mobility. Section IV discusses our treatment-effect estimation approach. Section V presents estimates of exposure to racial segregation on upward mobility and their implications for Black-White mobility gaps. Section VI concludes.

I. Data

The foundation of our dataset consists of responses to the 2011-2019 American Community Survey (ACS), which contain information on the respondent's annual income, partnership status, and partner's annual income. We supplement the ACS with intergenerational information on economic background and contemporary information on marital status from IRS 1040 Individual Income Tax Returns. These forms contain the income of all people in the United States who file taxes,[2] along with information that allows us to identify parent-child and spousal relationships. We have access to 1040 forms for tax years 1994-1995 and 1998-2019. This ACS-IRS data linkage is made possible by the data infrastructure at the Census Bureau, which assigns harmonized personal identifiers called Protected Identification Keys (PIKs) as described in Wagner and Layne (2014).[3]

A. Main Sample

[2] While tax units with sufficiently low incomes are not required to file taxes, low-income individuals still face a strong incentive to file to receive government benefits such as the Earned Income Tax Credit and Child Tax Credit (Ramnath and Tong 2017).

[3] The PIK process is nearly comprehensive: in the 2010 ACS, about 93% of individuals were assigned a PIK (Bond et al. 2014), and nearly all 1040 records receive a PIK (Mulrow et al. 2011).

To define person *i*'s economic background, we take the average annual Adjusted Gross Income (AGI) of the adults who claimed person *i* as a dependent when *i* was aged 10 to 18.[4] We call this concept "childhood family income" (CFI). We require at least two potential data years of AGI observations before the child turns 18 to construct CFI, to smooth out transitory income fluctuations, and construct CFI ranks within birth cohort. We then merge CFI information onto the 2011-2019 ACS files by PIK. We consider the 1979-1986 birth cohorts—the oldest ones permitted by our data linkage. Given gender differences in age at union formation, and to avoid censoring the sample on age, we include men born between 1979 and 1983 (blue and purple cells shown in Appendix Figure A1) and women born between 1982 and 1986 (orange and purple cells). We end up observing men at ages 31-35 and women at ages 28-32, and do not make any restrictions on the ages of their partners.

Finally, we focus our analysis on U.S.-born respondents. Non-native-born respondents tend to have higher missing rates of PIK and childhood family income than do native-born respondents. Preliminary analyses suggested that this missingness was not random.[5] Our final sample amounts to roughly 1.1 million individuals.

B. Romantic Partnership Formation—A Novel Measure

The ACS indicates how each person in a household is related to the reference person. Thus, sample members who are the reference person, or the partner of the reference person, can easily be linked to their partners. This process is more challenging in more-complex households. For example, if a 28-year-old woman is living in her parents' home along with her partner, she would be listed as a child of the reference person and her partner would be listed as an "other nonrelative," but no information would exist about how the two individuals relate to each other.

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[4] AGI includes wages, dividends, capital gains, business income, and retirement income. We omit negative AGI values from this average because these are often transitory negative shocks to high income households (e.g., large capital losses) and do not represent the permanent income of parents who claim children on their tax forms. This matches the specification of Chetty et al. (2014), who provide further information.

[5] For example, we found a significantly different marriage rate between the full ACS sample and the ACS sample for which we could observe CFI. Moreover, this disparity remained when we reweighted the sample on observables to adjust for missing CFI, so it is more than just the result of observable sociodemographic differences. However, this problem disappeared when we imposed a US-born restriction.

We supplement ACS relationships with filing status information from the tax records. For each person sampled in ACS year *t*, we merge on the PIK of the first spouse with whom that individual filed taxes jointly in years *{t,t+1,…t+5}*. We link this person to their IRS spouse if neither they nor their IRS spouse is already linked to an ACS partner and their IRS spouse resides in the person's ACS household. We classify couples as unmarried if either partner responds as unmarried to the individual marital status question that is asked of all ACS adult respondents. This procedure identifies additional marriages in which neither member is the ACS reference person. It also captures additional cohabitations for non-ACS reference persons that will become marriages within five years—consistent with our desire of capturing marriage-like relationships.[6]

C. Racial-Ethnic Heterogeneity

We focus our analysis on four mutually exclusive and non-exhaustive groups: White non-Hispanic, Black non-Hispanic, Hispanic of any race, and Asian non-Hispanic.[7] Our sample size, given our aim to analyze mobility separately by gender, does not allow for reliable analysis of smaller groups. However, we retain all individuals in the sample when assigning income ranks, so that we make statements about a given racial group's average mobility in the full U.S.-born population. Appendix Figure A3 summarizes racial heterogeneity in CFI distribution.

II. Estimates of Absolute Intergenerational Mobility by Race and Gender

We begin by defining our main outcomes of interest. All income concepts used in this paper are converted to 2015 US dollars using the Bureau of Labor Statistics' Consumer Price Index deflator before ranks are assigned. Ranks are assigned within birth cohort and gender.

Personal Income Rank—self-explanatory.

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[6] Appendix Figure A2 shows the additional couples we identify with the IRS data. Approximately 48% of the main sample reports being married, and of them, we can link 91% to their spouses using just the ACS. Bringing in the IRS data raises this fraction to 95%. Panel A shows that we identify 6 percentage points more spouses in the bottom quintile of the childhood family income distribution and about 3 percentage points more spouses in the top quintile. Because the ACS does not ask all individuals about non-marital partnerships, we cannot perform the same benchmarking exercise for non-marital cohabitations. However, Panel B shows that bringing in the IRS data raises the proportion of individuals participating in a non-marital cohabitation by a full percentage-point (or roughly 9%).

[7] For ease of exposition, we drop the non-Hispanic label from each of the three non-Hispanic groups, and abbreviate the terms "racial-ethnic" and "race-ethnicity" as "racial" and "race".

Partnership Status—binary outcome taking the value 1 if the individual is a member of a couple, using the methodology described in Section I.B. (*extensive margin*)

Conditional Partner Income Rank—among those who are partnered, one's rank in the partner income distribution. Unpartnered individuals receive a missing value. (*intensive margin*)

Partner Income Rank—one's rank in the partner income distribution where unpartnered individuals are classified as having 0 partner income. This measure combines extensive and intensive margins into a summary marriage-market outcome.[8]

Family Income Rank—We define family income as the sum of personal and partner income, the latter being 0 if the individual is unpartnered (e.g. Chadwick and Solon 2002; Mazumder 2008; Chetty et al. 2020).

A. Graphical Analyses a la Chetty et al. (2020)

We refer the interested reader to Appendix Figures B.1-B.6, which cycle through the outcomes and plot the average value as a function of CFI rank, separately by gender and race.

B. Summary Measures of Absolute Mobility

We collapse the appendix figures into summary measures that we analyze throughout the rest of the paper. We consider:

$$(1) \qquad y_{i,r}^{g} = \alpha_r^g + f^g(Q(CFI_i)) + \varepsilon_i^g$$

where y is an adulthood outcome for individual i; α is a set of racial-ethnic fixed effects (with individuals belonging to racial-ethnic groups outside of the four considered in this paper assigned their own group); and f is a flexible function of CFI quantile.[9] For each racial-ethnic group r, the corresponding α coefficient describes that group's level of *absolute mobility*: i.e., how much higher in the adulthood outcome distribution that group tends to be after controlling for the relationship between the outcome and CFI rank. We estimate equation (1) separately by gender g.

[8] We reduce noise by assigning all unpartnered individuals of gender g a partner income rank of $x_g/2$, where x_g is the unpartnered share of gender g. An alternative way to specify partner income rank is to assign all unpartnered individuals a rank of 0. Because doing so pulls the median partner income rank below 0.5, we add back in a gender-specific constant to everyone's rank to restore stationarity to our measure. As shown in Figure 1, this specification tends to raise White individuals' absolute mobility, exert small negative effects on Hispanic and Asians' mobility, and substantively lower Black individuals' mobility.

[9] We use a quartic; results are similar if we use a lower-order polynomial.

Table 1 presents the estimates. Asian individuals have substantially higher absolute mobility of personal income than do other groups. On the other hand, White individuals have substantially higher absolute mobility of partnership formation and partner income rank. Hispanic personal income mobility resembles the population average, while partner and family income mobility are slightly below average. We estimate a Black-White mobility gap in personal income of -0.5 (11.4) rank points among women (men); in partnership formation of 31.5 (19.5) percentage points among women (men); in partner income of 16.5 (6.9) rank points among women (men); and in family income of 12.6 (11.4) rank points among women (men).

The gender difference in Black-White gaps in personal income mobility—no gap among women but a stark gap among men—resembles what Chetty et al. (2020) found. However, we document a tremendous gap in partner income mobility between Black and White women, compared with a much smaller gap between Black and White men. In sum, the Black-White gap in family income mobility is slightly larger among women than it is among men.

C. We Estimate a Smaller Black-White Disparity Compared to Chetty et al. (2020)

Averaged across gender, we find a partnering rate mobility gap of 25.5 percentage points and a family income mobility gap of 12.0 ranks. These estimates are lower than those of Chetty et al. (2020). Their Figure IV.A reports a marriage rate mobility gap of 33.2 percentage points, and their Figure II.A reports a family income mobility gap of 14.2 ranks.[10] We attribute these discrepancies to our more-inclusive definition of partnering, as theirs is based on legal tax filing statuses. On the other hand, our estimates of Black-White gaps in personal income mobility match theirs more closely. Their estimates, inferred from their Figure V, are 10.9 ranks for men and -1.2 for women (average: 4.9), while ours are 11.4 ranks for men and -0.5 ranks for women (average: 5.5).

D. A Closer Look at the Intensive Margin

Table 1 enables a comparison of racial disparities between the extensive and intensive margins. After conditioning on CFI rank, Black men are 19.5 percentage points *less* likely to be partnered than White men; however, partnered Black men are 3.7 rank points *higher* in the partner income distribution than partnered White men. Asian (Hispanic) men are also less likely to be partnered

[10] We calculate these numbers by averaging their mobility gap estimates at each of the 25th and 75th percentiles.

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but enjoy higher (similar) partner income mobility relative to White men. Similar patterns are observed among women.

Because White men have higher partnering rates than other men, it is plausible that their partners are a differently-selected group than are other men's. Figure 1 explores this point further by plotting partner CFI rank as a function of own CFI rank. In contrast to the partner *income rank* statistics shown in Table 1 (see also Figure B.3), the partner *CFI rank* plots show substantial White-Hispanic and White-Black disparities. Similarly, while Asian men have a considerable mobility advantage over White men in partner income rank, they have no advantage in partner CFI rank. This suggests that observed racial differences in partner income rank may not be informative about racial differences in the *potential partners* encountered by marriage-market participants. We investigate this in the next section.

III. The Role of Marriage Market Forces in Racial Mobility Gaps

In this section, we quantify the role of marriage-market allocations on family income mobility. We do so by conducting a series of simulations that randomly redistribute personal or partner incomes while leaving aggregate income unchanged.

Table 2 presents the results. We begin with a personal income simulation. This simulation reassigns everyone's personal income to that of a randomly selected individual with the same gender and CFI percentile.[11] For women, this simulation exerts almost no effect on White-Black and White-Hispanic family income mobility gaps, but reverses Asian women's advantage over White women. For men, it substantially reduces the observed White-Black and White-Hispanic family income mobility gaps, and eliminates Asian men's advantage over White men.

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The second ("baseline") simulation randomly reassigns partner incomes instead of personal incomes. This simulation eliminates almost the entire 12.6-rank mobility gap between White and Black women and eliminates the entire 4.0-rank mobility gap between White and Hispanic women. Moreover, it changes the 5.0-rank advantage for Asian women into an 8.9-rank advantage. Marriage-market dynamics thus play a crucial role in women's family income mobility. They also matter for men. The third row of Panel B shows that randomizing partner incomes removes 3.0

[11] To minimize the impact of sampling error on our estimates, we sample with replacement, and we repeat each simulation 100 times. The statistics reported in the table are averaged across these 100 counterfactual distributions.

rank points of the 11.4-rank mobility gap between White and Black men, and removes 1.2 rank points of the 3.1-rank mobility gap between White and Hispanic men. It also increases Asian men's mobility advantage over White men from 2.9 ranks to 4.4 ranks.

As emphasized in the previous section, partner incomes are determined by two related but distinct margins: extensive (match with a potential partner or remain single) and intensive (variation in potential partner income). The remaining simulations attempt to shed light on each margin. This quantification is difficult because we do not easily observe potential partner income for individuals who remained single.

We build up to our preferred simulation by presenting two illustrative ones. First, we repeat the partner income reassignment except that we fix the extensive margin: unpartnereds continue to receive 0 partner income, while partnereds have their partner incomes replaced with that of a random partnered individual with the same gender and CFI status. Among women, we see that this simulation removes only 1.4 ranks of the 12.6-rank White-Black mobility gap and 0.7 ranks of the 4.0-rank White-Hispanic mobility gap. Among men, the simulation exerts no effect on the White-Hispanic mobility gap and modestly increases the White-Black mobility gap. These results might suggest that racial disparities in the marriage market stem primarily from the extensive margin. However, this interpretation assumes that marginal unpartnered individuals have marriage-market opportunities equal to those of already-partnered individuals.

Alternatively, we invert the simulation by removing partnering rate differences while preserving the intensive margin. We iterate through each gender-by-CFI-percentile cell and eliminate racial disparities in partnering rates by bringing the rate of each group up to the level of the group with the highest rate. This requires assigning counterfactual partner incomes to the newly-partnered individuals. We leverage the fact that we can learn about marginal marriage-market offers by observing the incomes of unpartnered individuals of the opposite gender. (After all, these are the exact individuals still available to be partnered.) We thus assign each newly-partnered individual a partner income equal to the personal income of an opposite-gender, unpartnered individual belonging to the same racial group and personal income quintile. If the extensive margin is the dominant force behind marriage-market disparities, this simulation should close mobility gaps by as much as the baseline one. In contrast, we find this simulation eliminates only 5.3 ranks of the mobility gap between White and Black women and 1.8 ranks of the gap between White and Hispanic women.

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Moreover, this simulation *still* likely overstates extensive margin. For example, if Black women tend to have worse offers in the marriage market than White women with the same CFI percentile, then a counterfactual that equalizes Black and White partnering rates is one in which Black women are forced to be "less choosy" than White women. Our preferred simulation equalizes not partnering rates, but rather, *partner acceptance functions*—i.e., the probability of partnering *conditional on potential partner income*.[12] This preferred simulation eliminates only 2.6 rank points of the 12.6-rank mobility gap between White and Black women, while the full partner income reassignment eliminated 12.4 rank points. This implies that the extensive margin (i.e. differential partner acceptance rates) generates only 2.6 rank points of the family income mobility gap, while the intensive margin (i.e. differential partner income offers) generates 12.4-2.6=9.8 rank points. Similarly, the extensive margin generates only 0.9 rank points of the White-Hispanic mobility gap while the intensive margin generates 3.3 rank points. In contrast, the extensive margin lowers Asian women's mobility advantage over White women by 3.7 rank points, whereas the intensive margin affects this advantage by only -0.2 rank points.[13]

In preserving aggregate income in the economy, our simulations implicitly assume a zero earnings premium from entering a partnership. Recent estimates suggest that this may be an appropriate assumption for women, but the average man still experiences a moderate marital earnings premium (Juhn and McCue 2017). This average premium is likely too high for our data and simulation, which applies to hypothetical partnerships that did not actually form, and to non-marital as well as marital partnerships. The final simulation in the table re-estimates the preferred

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[12] We execute the simulation as follows: First, we randomly assign each unpartnered individual a different-sex partner from the same race group and personal income quintile. Second, for each combination of gender, CFI vigintile, and partner income vigintile, we compute the partner acceptance rate for each race group as the number of observed matches divided by the sum of observed and assigned matches. Finally, we randomly select unpartnered people to accept their assigned matches to bring the partnering rate of each race group up to the group with the highest rate.

[13] One may have noticed that the residual portions of the decomposition are similar to, although not always equal to, the portion of the mobility gap that is removed by the personal income simulation. This near equivalence is far from guaranteed, given that our simulations assume away endogeneity of personal incomes with respect to partner incomes, and do not fix the correlation between personal and partner incomes. The slight discrepancies between the residual effects and the personal income effects likely reflect slight differences in these parameters across racial-ethnic groups, but also suggest that our conclusions would be robust to reasonable assumptions about these parameters. These results are also consistent with the fact that cross-wage elasticities have become quite small in recent cohorts (e.g. McClelland and Mok 2012; Gonalons-Pons et al. 2021).

one assuming an 8% partnership earnings premium for newly partnered men—a premium slightly below Juhn and McCue's most recent estimate. The results are quite similar.

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These results suggest that policy efforts to improve marriage-market opportunities—as opposed to partnering rates—could have substantial payoff in closing White-Black and White-Hispanic mobility gaps among women. It is also interesting to note that the conditional partner income mobility statistics shown in Table 1 might suggest that Black men have slightly better marriage-market opportunities than White men, and that Asian individuals have much better opportunities than White individuals. Our simulations reveal that accounting for selection into partnership formation mostly or completely negates these apparent advantages.

IV. Quantifying Determinants of Absolute Mobility: A Selection-Correction Approach

We now turn our investigation to the mechanisms that determine absolute intergenerational mobility. In this section, we outline an approach that adjusts cross-area covariation between a mobility statistic and a treatment of interest to infer a specific treatment effect under certain assumptions. In Section V, we apply this approach to estimate effects of childhood exposure to racial segregation on absolute mobility. We focus our analysis on understanding how Black-White mobility gaps are shaped by segregation exposure, consistent with prior literature.

A. Constructing Area-by-Race Aggregates

To begin, we aggregate the main sample to cells defined by (birth geography (CBSA)[14], race, gender). This involves modifying equation (1) to include a full set of cell fixed effects:

$$(2) \qquad y_{i,r,c}^{g} = \alpha_{r,c}^{g} + f^{g}(Q(CFI_i)) + \varepsilon_i^{g}.$$

That is, α now includes a separate dummy for each birth CBSA *c* by race group *r* by gender *g* cell. As above, we retain all race groups and birth CBSAs in the sample to estimate (2), but then develop a dataset containing absolute mobility estimates only for White and Black men and women, and only for cells containing at least 20 unweighted observations to reduce noise. We end up with a

[14] A core-based statistical area (CBSA) is a grouping of one or more counties that share an urban center and are economically linked through commuting patterns. These generally coincide with commuting zones in small and medium-sized areas and are subsets of commuting zones in large areas. There are 925 CBSAs in the United States. We assign each member of the main sample to a local marriage market according to their birth CBSA.

dataset covering approximately 2,350 cells spanning 900 birth CBSAs. Within each cell, we generate absolute mobility estimates for each of the six outcomes shown in Table I.[15]

For each racial group, we compute a racial segregation index (Jahn et al. 1947) for each CBSA in our dataset, denoted $\sigma_{r,c}$. This measure ranges from 0 to 1 and describes the percentage of people with a given racial identity that would need to move to a different tract to equalize their representation across tracts.[16] Our preferred specification exposes White individuals to a White / non-White index and Black individuals to a Black / non-Black index. This is done to increase precision for White individuals in local areas with very few Black individuals.[17]

B. Local CFI Distribution: An Informative Control Function

Building on Altonji et al. (2005), Oster (2019) developed a method to adjust a bivariate relationship between an outcome and a treatment for nonrandom selection into the treatment. Intuitively, if the bivariate relationship is changed little by a set of *informative* control variables—i.e. controls that are substantially correlated with the outcome but not the treatment—then the average treatment effect is likely close in magnitude to the observed controlled relationship. Oster derived a treatment effect formula in terms of the bivariate relationship, the controlled relationship, the R^2s on these regressions, and two researcher-specified assumptions (to be discussed in Section V.B) that validate this intuition.

We apply Oster's method by leveraging cell-level information on the CFI distribution as a control function in a regression of absolute mobility on racial segregation. As has been shown in prior work, richer, poorer, more unequal, and less unequal areas exhibit systematically different mobility patterns.[18] Embedded in this heterogeneity may be family-level or neighborhood-level

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[15] We document substantial variation in absolute mobility across birth areas, which matches prior findings. For example, White men experience an absolute family income mobility coefficient of -2.0 ranks in the (population-weighted) 10th percentile CBSA but 4.9 ranks in the 90th percentile CBSA. Similarly, Black women experience absolute family income mobility of -13.9 ranks at the 10th percentile but -6.8 ranks at the 90th percentile.

[16] To increase precision, we construct this index with the full IRS universe of claimed dependents that match our birth cohort restrictions, rather than the ACS.

[17] Results for Black individuals are similar when we instead expose them to a Black / White index.

[18] Appendix Figure C1 illustrates correlations estimated in the literature between local absolute mobility and local CFI characteristics. Each graph plots the relationship between an adulthood outcome and CFI rank within multiple local areas (solid lines), along with the national average relationship (dotted line). The top graph compares a Rich and a Poor area. It is easy to see that the Rich area has a positive level of absolute mobility and the Poor area has a negative level. (Absolute mobility is the weighted-average vertical distance between the given area's line and the national dotted line across all CFI ranks, with weights equivalent to the given area's population share belonging to each rank.)

factors that are correlated with (and not directly caused by) segregation exposure. The method controls for these factors by controlling for this heterogeneity and then extrapolating according to the imposed assumptions.

We consider two specifications of local CFI information. Our first specification consists of cell-level percentages of individuals from the top and from the bottom quintiles of the CFI distribution. This captures both how rich the observed cell is as well as how unequal it is. The second specification contains the first specification plus additional information on CFI inequality: i) log mean CFI among those from the top 20% minus log mean CFI among those from the bottom 80%; and ii) log mean CFI among the top 80% minus log mean CFI among the bottom 20%.

Table 3 shows that our baseline control function explains roughly 20% of cross-area variation in most groups' personal income mobility, and 12% to 18% of cross-area variation in partner income mobility. Augmenting the baseline control function increases the R^2s slightly. As a benchmark, CFI and race effects jointly explain 10% to 12% of the variation in individual family income rank. Our control functions explain substantial variation even though individual CFI and race are controlled in constructing our cross-area dataset.[19]

C. Counterfactual

Our control functions hold fixed the local CFI distribution and factors correlated with this distribution. That is, the correlation between Black children's exposure to racial segregation and their exposure to poverty is controlled. Some of this correlation may reflect the confounded nature of segregation with other impoverishing circumstances, while some of it may reflect direct effects of historical segregation on Black poverty (e.g. Baker 2022). To the extent that *historical exposure* to segregation has compromised the local resources available to Black children, basing our inferences on controls for local CFI distribution can be interpreted as removing these effects and

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The middle graph illustrates a version of the "Great Gatsby" relationship by comparing an Egalitarian area to an Unequal one. The Unequal area exhibits negative absolute mobility, while the Egalitarian area exhibits positive absolute mobility. The bottom graph stylizes the patterns we observe in our dataset through a comparison among Rich, Poor, and Unequal areas.

[19] One exception is that the control functions do not explain much variation in Black men's intergenerational mobility. On the other hand, Appendix Table A1 shows that the control functions are correlated at 40% with White children's exposure to racial segregation but only 14%-20% with Black children's exposure. This works to raise the relative precision of estimates for Blacks. The bootstrapped standard error estimation accounts for the (lack of) informativeness of the control function and still returns reasonably tight confidence intervals for Black men

focusing on how exposure to segregation *freshly creates racial inequality in the current generation*. This "conditional" treatment effect gets at what would happen if a given area experienced an incremental shift in segregation, holding history fixed.

We note several processes that could generate variation in segregation exposure conditional on local CFI distribution. First, areas with similar historical disadvantage could experience different contemporary trends in segregation. Second, areas could experience persistently different levels of segregation, but migration between the areas could equalize local CFI distributions and thus the average levels of historical disadvantage experienced by the populations in each area. Third, non-persistent factors (i.e. "market luck"—Becker and Tomes 1979; 1986) could equalize CFI distributions in two areas with persistently different segregation levels. The idea is that controlling for local CFI distribution raises the extent to which these comparisons are made, and lowers the extent to which a less-segregated area is compared to a more-segregated area with other disadvantages. The selection-on-unobservables assumptions, discussed below, remove these remaining biased comparisons to recover formal treatment effects.

V. Estimating Treatment Effects of Racial Segregation on Absolute Mobility

A. OLS Regression Estimates

We build up to our selection-corrected estimates by presenting bivariate associations between segregation and mobility. We take the absolute mobility statistics from equation (2) and put them on the lefthand side, estimating

$$\alpha_{r,c}^{g} = \gamma_r^{g} + \beta_r^{g} \cdot \sigma_{r,c} + h_r^{g}\left(Q(CFI_{r,c})\right) + k_r^{g}\left(I(CFI_{r,c})\right) + \epsilon_{r,c}^{g} \quad (3)$$

separately for each race-gender combination. The function *h* contains our baseline control function measured in CFI quintile shares; the function *k* contains the additional inequality statistics measured in log CFI differentials.

Table 4 presents estimates of β_r for White women in Panel A and Black women in Panel B. The first column presents the bivariate association, the second column adds in control function *h*, and the third column adds in control function *k* as well.

Looking at the first column of Panel A, we see bivariate effects of all sizes and directions. The association between segregation and White women's personal income mobility is large and positive, while the association with partner income mobility is large and negative. In sum, the

association with White women's absolute family income mobility is a precise zero. Columns 2 and 3 reveal that none of these associations, except for the null association with family income mobility, is robust to our control functions. Although the personal and partner income rank results remain statistically significant, they are much smaller in magnitude, while the regression R^2 increases considerably.

In contrast, the first column of Panel B shows substantial negative associations between segregation and absolute mobility of Black women's partnering rate, partner income, and family income. These estimates are robust to inclusion of the control functions: for example, the family income mobility estimate changes by 0.8 rank points, or 11.9%, when the full set of controls is added, while the regression R^2 nearly doubles (from .071 to .126). We also estimate modest but insignificant negative effects of segregation exposure on Black women's personal income mobility, and zero effects on partner income mobility conditional on being partnered. These estimates are also robust to the control functions.

Table 5 presents analogous estimates for White men (Panel A) and Black men (Panel B). The findings tell a broadly similar story. For White men, the bivariate associations are null for personal income mobility, strongly negative for partnering rate mobility, strongly positive for conditional partner income mobility, and precisely null for family income mobility. The last result is robust to controls even as the R^2 increases by .182; on the other hand, the partnering rate and conditional partner income results change dramatically as controls are added. In contrast, for Black men, the bivariate associations are strongly negative for all outcomes except partner income mobility conditional on being partnered, and the negative associations are extremely robust to controls. Although the R^2s barely budge for personal income and family income mobility, they increase nontrivially for the marriage-market outcomes.

B. Selection-Corrected Estimates

We adjust the controlled estimates from Tables 4 and 5 for remaining selection on unobserved variables that determine both segregation and absolute mobility, according to Oster's (2019) formula and the assumptions it requires.

These assumptions are twofold. We have already shown that the control function is related to segregation exposure (Appendix Table A1). The first assumption is a scalar δ that describes,

relative to this benchmark, how related the index of unobservables is to segregation exposure.[20] A δ of zero implies that remaining unobservables are unrelated to segregation. A δ of 1 implies that the control function and the unobserved index are equally related to segregation exposure. Oster argues that in typical empirical settings, [0,1] is a conservative set of bounds. Because researchers typically observe the most informative controls, and moreover, any unobserved determinant of treatment exposure that is correlated with an observed control will load onto it in the regression, a value of δ below 1 makes intuitive sense. Our setting is no exception. Individual CFI rank and race (and any unobservable correlated with one of these and segregation exposure) are controlled in the construction of area-by-race mobility outcomes. On top of this, controlling for local CFI distribution removes generalized relationships among poverty, inequality, and segregation exposure. It is plausible that remaining unobservables are either obliquely related to segregation exposure or could be considered a consequence of segregation exposure. Our preferred specification sets $\delta = 0.5$, and we demonstrate robustness to alternative δ values below.

Second, we must specify how much outcome variation would be explained if we were able to observe the unobserved variables that systematically determine absolute mobility. Oster argues that this "*Rmax*" value should be substantially below 1. First, there may be measurement error in the outcome. This seems plausible in our case given the existence of income mismeasurement in household surveys and the fact that we run into small sample sizes when constructing race-by-gender aggregates in small CBSAs. Second, there may be variation in the outcome that is not traceable to any systematic mechanism. The classic model of Becker and Tomes (1979, 1986) allows for a substantial role of luck in determining intergenerational outcomes. Oster also observes that an *Rmax* of 1 would improperly cover a range of well-published results from randomized control trials: an *Rmax* of 1.3 times the R^2 of the controlled regression achieves 90% coverage. We therefore use this value in our preferred specifications.

In sum, letting $\tilde{R}$ denote the R^2 of the controlled regression, our preferred set of $(\delta, Rmax)$ assumptions is $\left(0.5, 1.3\tilde{R}\right)$. We use Oster's *psacalc* Stata package to estimate treatment effects of

[20] That is, suppose we estimated equation (3), collapsed the estimated control function into one index, and then regressed segregation exposure on this index. The parameter δ is the inverse of the ratio of the coefficient on this index to the hypothetical coefficient on the index of unobservables. See Oster (2019), Section 3.1.

segregation exposure under these assumptions along with bootstrapped standard errors. The bootstrap approach is one way to conduct inference: we also examine a bounded set of estimates generated by an "aggressive" and a "conservative" assumption. The aggressive assumption is that δ is zero, while the conservative assumption involves setting δ to 1 and *Rmax* to $1.6\tilde{R}$. We record below each standard error whether the 90% bootstrapped confidence interval covers this bounded set. Reassuringly, it usually does. In the 9 (out of 48) cases where it does not, 6 of the point estimates are small and statistically insignificant according to the bootstrap anyway. The remaining 3 estimates are marginally significant according to the bootstrap, but the bounded sets of estimates overlap with zero, suggesting caution in assigning statistical significance to these results.

Table 6 presents selection-corrected estimates for women. The significantly positive association between segregation and White women's personal income mobility survives correcting for unobserved selection although the resulting treatment effects are around half the size. The partner income mobility treatment effects are dramatically smaller than the bivariate associations, and usually lose statistical significance, though remain modestly negative. The treatment effect of segregation exposure on White women's family income mobility is a precise zero. On the other hand, for Black women, the strong bivariate associations between segregation and absolute mobility of partnering rate, partner income, and family income are all changed little by adjusting for selection. Averaging across the two specifications, a 0.10 increase in segregation exposure reduces Black women's absolute family income mobility by 0.59 rank points, and this estimate is roughly 2.4 times as large as its standard error (and much larger than the range of the bounded set). Going from the 10^{th} to the 90^{th} percentile of Black segregation exposure induces a -1.85 rank-point response, which is roughly 26% of the 10^{th}-90^{th} cross-area differential in Black women's absolute family income mobility.

Table 7 presents selection-corrected estimates for men. For White men, we find that the large bivariate associations of segregation exposure with partnering rate mobility and with conditional partner income mobility lose most of their magnitude and significance upon correcting for selection. We end up with a modest negative and insignificant treatment effect for personal income mobility, and a modest positive and usually insignificant treatment effect for partner income mobility. These effects cumulate to a precise null treatment effect for family income mobility. For Black men, all the strong negative bivariate associations survive correcting for selection, and the

magnitudes of the effects change little. A 0.10 increase in segregation exposure reduces Black men's absolute family income mobility by 0.70 rank points, and this estimate is roughly 2.6 times as large as its standard error. Going from the 10^{th} to the 90^{th} percentile of Black segregation exposure induces a -2.20 rank-point response, which is roughly 31% of the 10^{th}-90^{th} differential in Black men's absolute family income mobility.

It is interesting to note that for both genders, segregation appears to cause a substantial decline in Black partnering rate mobility but no decline in conditional partner income mobility. This might suggest that segregation reduces upward mobility in the marriage market primarily by affecting partnering rates given a set of marriage-market opportunities. We have shown in Section 4 that this narrative is incomplete, especially so for Black women.

C. Implications for the Aggregate Mobility Gap

We provide simple illustrations in Figure 6 that quantify the aggregate effects of childhood exposure to racial segregation—conditional on the local distribution of family resources and the historical exposures that generated it—on Black-White absolute mobility gaps. We start with the observed mobility gaps from Table I, and plot them as the total bars shown in the figure (i.e. blue + magenta).[21] For each racial group, we compute a counterfactual level of absolute mobility based on reducing segregation exposure from its sample average level to zero. We subtract the White counterfactual from the Black counterfactual and compute a conservative confidence interval under the assumption of independence of the two sets of estimates. The counterfactual mobility gaps remove the magenta regions and equal the heights of the blue bars, with black whiskers denoting the 90% confidence intervals. We do this for both control-function specifications, such that there are two counterfactuals for each outcome-gender combination. In all cases we can exclude the original mobility gap from our counterfactual. Averaging across the two specifications, we estimate that current segregation exposure generates 4.8 rank points (20.4%) of the Black-White mobility gap in women's partner income, 5.5 rank points (50.0%) of the mobility gap in men's partner income, 3.9 rank points (30.7%) of the mobility gap in women's family income, and 4.3 rank points (37.1%) of the mobility gap in men's family income.

[21] The Black-White mobility gap for women's personal income is 0.5 rank points in favor of Black women, and increases to roughly 4 rank points when segregation is removed.

D. Robustness and External Validity

These effects are substantial. However, given the range of exposures that plausibly result from racial segregation (e.g. Wilson 1987), it is reasonable to wonder whether they are too small. One possibility is that our strategy "overcontrols," in that equalizing the local CFI distribution leads us to a compare less-segregated area to a more-segregated one that also has some unobserved *advantage* (as opposed to disadvantage). This implies *negative* selection on unobservables. We assess this by re-estimating our headline family income mobility regressions with δ values ranging from -0.1 to -1.0, with an *Rmax* assumption of $1.3\tilde{R}$. Recall that in 3 of 4 demographic groups, the bivariate association between segregation and upward mobility was robust to control functions which also substantially increased the regression R^2 (Tables 4-5). Accordingly, we find that δ perturbations exert little influence on the selection-corrected estimate. This is summarized by Appendix Table A3, which presents treatment effect estimates based on $\delta = -0.5$. The remaining group is Black men, where the low value of $\tilde{R}$ for the baseline control function results in modest sensitivity of the treatment effect to negative-selection assumptions.

Another possibility is that historical disadvantage takes multiple generations to overcome. Our effects are consistent with desegregation closing a sizable chunk of the Black-White mobility gap in the short run, but the full effects may not manifest immediately. In the spirit of our analysis, we can shed light on this by cleansing the baseline mobility gap of differences in local CFI distribution and other historical factors. The ratio of the segregation effect to this adjusted gap tells us what share of mobility differences, that *freshly* originate in the given generation, stem from *fresh* segregation exposure. This ratio is potentially informative about how *multiple* generations of segregation exposure shape cumulative mobility gaps over a *multigenerational* horizon.

To compute this adjusted ratio, we first use the estimates in Table 3 to compute upward family income mobility in a hypothetical area with CFI distribution equivalent to that of the nation. These estimates result in an adjusted Black-White gap of 8.3 ranks for women and 9.0 ranks for men (averaging across control function specifications). The simple estimator described in Section 3.2 of Oster (2019), with the assumption that δ=1 and $Rmax = 1.6$, dictates a selection-adjusted estimate of 1.6 times the adjusted gap minus 0.6 times the baseline gap. We can consider this to be an estimate of mobility gaps that result from unequal exposures experienced by the current generation of Black and White children, after controlling for past inequality. This results in a gap

of 5.72 ranks for women and 7.56 ranks for men, which gives selection-corrected desegregation effects of 68.2% of this gap for women and 56.9% for men. Though this is a crude back-of-the-envelope exercise, it supports the conclusion that the short-run effects of segregation identified by our quasi-experiment cumulate over a multigenerational horizon.

We compare these headline estimates to ones reported by Chyn et al. (2024). These authors estimated effects of segregation exposure on upward family income mobility by leveraging 19th-century railroad placement as an instrument. The effects identified by this strategy come from exposing several generations of families to different levels of railroad-induced segregation and its downstream implications. Table 8 of Chyn et al. (2024) suggests that full desegregation would remove at least 82.7% of the Black-White mobility gap, depending on initial CFI percentile. Our implied long-run effects are not this large, perhaps due to differences in sample and measurement of family income, although they tell the same qualitative story. The differences between our effects and theirs could also suggest that initial segregation of American cities created historical legacies that cannot be fully reversed by persistent desegregation policies implemented today.

VI. Conclusion

We have linked American Community Survey responses with parental tax records to generate new estimates of racial disparities in the mobility of income across generations. Our linkage identifies a large set of unmarried couples, which expands on the legal tax filing statuses used in recent studies. We intersect race with gender, revealing a detailed portrait of gender-specific racial disparities. We show that the modest Asian advantage over Whites in family income mobility hides a disadvantage in partner income mobility for both genders. In addition, we match Chetty et al. (2020) in finding a small personal income mobility advantage for Black women but a strong disadvantage for Black men. However, while both genders experience a partner income mobility disadvantage, Black women's is so strong that it yields a larger Black-White family income mobility gap for women than for men.

The partner income mobility disparities between Whites and other individuals that we show, at first glance, appear to be driven mostly by the margin of partnership formation. This is because partnering rate disparities are large, but conditional on partnering, most groups achieve similar or higher partner income ranks than same-gender Whites. However, simulation analyses that account for selection into partnering reveal that Black and Hispanic women have substantially worse

opportunities in the marriage market than White women, while Asian individuals and Black men have similar opportunities to their White counterparts despite obtaining higher partner income ranks when partnered.

To shed light an important mechanism, we apply Oster's (2019) selection-correction methodology to a cross-area regression of upward mobility on neighborhood racial segregation. The intuition is that local CFI distribution contains information about separate mobility determinants that may be correlated with segregation exposure. Comparing a more- and less-segregated area with equivalent CFI distribution removes some of these factors. The change in the bivariate segregation effect (and regression R^2) upon controlling for local CFI distribution can then be extrapolated according to Oster's formula to remove remaining unobserved confounders. We demonstrate robustness of our estimates to a wide range of assumptions needed to parameterize the formula, and conduct conservative inference by taking the union of two different bounding approaches. We estimate precise null effects of segregation exposure on Whites' absolute family income mobility, and strong negative effects for Black women and Black men. Full desegregation would remove one third of the Black-White mobility gap for each gender, and around three-fifths of the gaps that remain after conditioning on our control function.

Our application of selection-correction methodology is general in scope: our approach can be applied to other mobility moments and childhood exposures, so long as those exposures are partially orthogonal to CFI distribution (e.g. one cannot study the effects of poverty exposure with our methodology). We leave these inquires to future research. We have also argued that using local CFI distribution as a control function effectively holds constant historical exposures, in addition to contemporaneous exposures not determined by the treatment of interest. As a result, our analysis identifies "short-run" effects of a shift in the given exposure experienced by the current generation, holding history constant. The literature would benefit from further estimates of how specific exposures shape economic opportunity, with careful consideration of demographic heterogeneity as well as the temporal horizons over which these estimates apply.

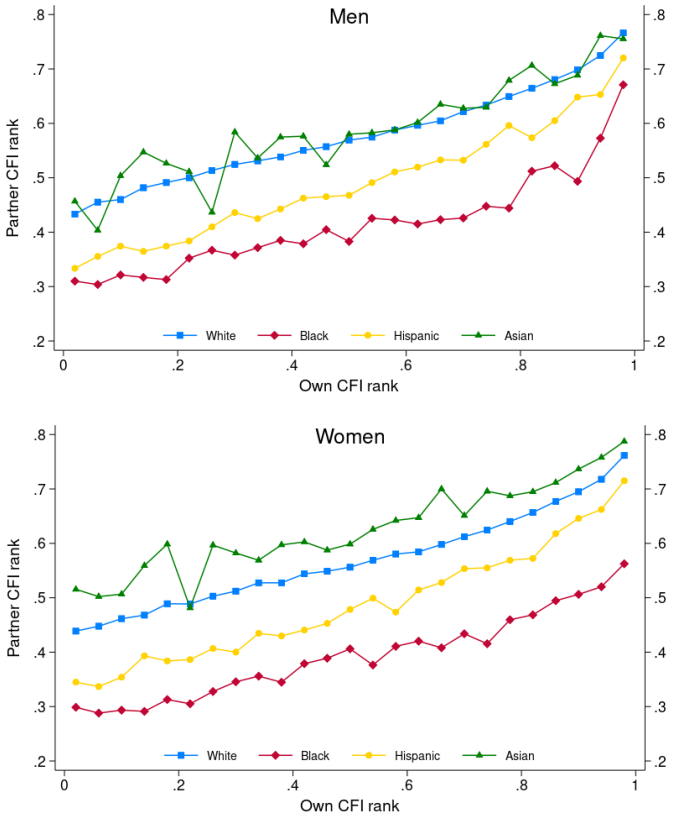


FIGURE 1. AMONG THOSE PARTNERED,
PARTNER CFI RANK BY OWN CFI RANK AND RACE-ETHNICITY

Note: See Section I.A for information on main sample construction and measurement of childhood family income (CFI) rank.
Source: 2011-2019 ACS linked to IRS 1040 Forms.

TABLE 1—ABSOLUTE INTERGENERATIONAL MOBILITY STATISTICS BY RACE-ETHNICITY AND GENDER

Panel A. Women

Absolute mobility relative to population	White	Black	Hispanic	Asian	B-W Difference
Personal income rank	-0.003	0.002	-0.001	0.124	0.005
	(.001)	(.001)	(.002)	(.004)	(.002)
Partnering rate	0.060	-0.255	-0.025	-0.081	-0.315
	(.001)	(.003)	(.003)	(.006)	(.003)
Partner income rank | partnered	0.007	-0.076	-0.015	0.091	-0.083
	(.001)	(.003)	(.002)	(.005)	(.003)
Partner income rank spec 1	0.032	-0.133	-0.022	-0.020	-0.165
	(.001)	(.001)	(.002)	(.004)	(.001)
Partner income rank spec 2	0.046	-0.190	-0.029	-0.038	-0.236
	(.001)	(.002)	(.002)	(.005)	(.002)
Family income rank	0.023	-0.103	-0.017	0.073	-0.126
	(.001)	(.001)	(.002)	(.004)	(.002)

Panel B. Men

Absolute mobility relative to population	White	Black	Hispanic	Asian	B-W Difference
Personal income rank	0.017	-0.097	-0.007	0.065	-0.114
	(.001)	(.002)	(.002)	(.003)	(.002)
Partnering rate	0.035	-0.160	-0.028	-0.127	-0.195
	(.001)	(.003)	(.003)	(.007)	(.003)
Partner income rank | partnered	-0.005	0.032	-0.005	0.107	0.037
	(.001)	(.003)	(.003)	(.005)	(.003)
Partner income rank spec 1	0.013	-0.056	-0.019	-0.025	-0.069
	(.001)	(.002)	(.002)	(.004)	(.002)
Partner income rank spec 2	0.021	-0.085	-0.029	-0.049	-0.106
	(.001)	(.002)	(.002)	(.005)	(.002)
Family income rank	0.018	-0.096	-0.013	0.047	-0.114
	(.001)	(.002)	(.002)	(.004)	(.002)

Note: See Section I.A for information on main sample construction and measurement of childhood family income (CFI) rank. Family income is defined as the sum of personal income and partner income (set to 0 if unpartnered). Nominal incomes are converted to 2015 US dollars before assigning ranks. See Section II and note 8 for information on the two specifications used to handle unpartnered individuals in the assignment of partner income ranks. Most estimates are given in rank points, as specified in equation (1), with robust standard errors appearing below in parentheses. Partnering rate mobility estimates are given in percentage points. All estimates at least .003 in absolute value are statistically significant at conventional levels.

Source: 2011-2019 ACS linked to IRS 1040 Forms.

TABLE 2—ABSOLUTE FAMILY INCOME MOBILITY STATISTICS UNDER VARIOUS COUNTERFACTUALS

	Panel A. Women				*Panel B. Men*			
Counterfactual	White	Black	Hispanic	Asian	White	Black	Hispanic	Asian
Observed	0.023	-0.103	-0.017	0.073	0.018	-0.096	-0.013	0.047
	(.001)	(.001)	(.002)	(.004)	(.001)	(.002)	(.002)	(.004)
Reassign personal income	0.024	-0.101	-0.016	-0.001	0.006	-0.028	-0.008	0.007
	(.000)	(.001)	(.001)	(.003)	(.000)	(.001)	(.002)	(.003)
Reassign partner income	-0.001	-0.003	0.000	0.088	0.012	-0.072	-0.007	0.056
	(.000)	(.001)	(.001)	(.003)	(.000)	(.001)	(.001)	(.004)
Reassign partner income, fix partnership	0.020	-0.092	-0.013	0.057	0.019	-0.101	-0.013	0.030
	(.000)	(.001)	(.001)	(.004)	(.000)	(.001)	(.001)	(.004)
Equalize partnering rates, match within race and own income quintile	0.012	-0.061	-0.010	0.097	0.012	-0.071	-0.006	0.075
	(.000)	(.001)	(.001)	(.003)	(.000)	(.001)	(.002)	(.004)
Equalize partner acceptance functions, match within race and own income	0.017	-0.083	-0.014	0.104	0.012	-0.074	-0.005	0.082
	(.000)	(.001)	(.001)	(.004)	(.000)	(.001)	(.001)	(.004)
Same as previous, impose an 8% male partnering premium	0.017	-0.082	-0.014	0.106	0.012	-0.073	-0.004	0.084
	(.000)	(.001)	(.001)	(.003)	(.000)	(.001)	(.001)	(.004)
Relative to White (Wo)men…								
Total mobility gap	--	-0.126	-0.040	0.050	--	-0.114	-0.031	0.029
Extensive margin	--	-0.026	-0.009	-0.037	--	-0.028	-0.014	-0.040
Intensive margin	--	-0.099	-0.033	-0.002	--	-0.002	0.003	0.026
Residual	--	-0.002	0.001	0.089	--	-0.085	-0.020	0.044

Note: Absolute mobility is defined in equation (1) and related discussion. The first two simulations reassign everyone's specified income source with that from a randomly selected individual with the same gender and childhood family income (CFI) percentile. The third simulation repeats the second while preserving partnership status: only partnered individuals' partner incomes are randomly reassigned. The fourth simulation equalizes *partnering rates* across racial-ethnic groups, conditional on gender and CFI percentile, by bringing the partnering rate of each group up to the level of the group with the highest rate. Newly partnered individuals receive a random partner income draw from the observed distribution of opposite-sex unpartnereds' incomes, conditional on the individual's race and personal income quintile. The fifth simulation equalizes *partner acceptance rates* across racial-ethnic groups, conditional on gender and CFI vigintile, in a manner described in the text. Potential partner incomes are assigned to the observed unpartnereds in the same way as in the fourth simulation. The final simulation repeats the fifth one but increases each newly partnered man's income by 8% after counterfactual matches are made. Each simulation is repeated 100 times: reported mobility statistics are averaged across these counterfactual distributions.
Source: 2011-2019 American Community Survey linked to 1040 Forms.

TABLE 3—VALIDATING THE EXAMPLES: CROSS-AREA REGRESSIONS OF ABSOLUTE MOBILITY ON CFI DISTRIBUTION MEASURES FOR SELECTED DEMOGRAPHIC GROUPS

		Women		Men	
		White	Black	White	Black
Personal Income Rank	Log share top quintile	0.015	-.006	-.012*	-.007
		(.010)	(.005)	(.007)	(.005)
	Log share bottom quintile	-.037***	-.073***	-.042***	-.015
		(.011)	(.015)	(.004)	(.017)
	p-val: Poor = Rich	0.036	0.009	0.024	0.780
	p-val: Egalitarian = Unequal	0.489	0.019	0.002	0.561
	R-squared	0.199	0.206	0.178	0.011
	R-squared augmented spec	0.221	0.219	0.190	0.018
Partner Income Rank spec 1	Log share top quintile	-.034***	-.007**	-.026***	-.007
		(.004)	(.003)	(.004)	(.004)
	Log share bottom quintile	-.020***	0.026**	-.031***	0.014
		(.004)	(.011)	(.006)	(.011)
	p-val: Poor = Rich	0.139	0.073	0.677	0.271
	p-val: Egalitarian = Unequal	0.000	0.435	0.000	0.779
	R-squared	0.179	0.145	0.117	0.055
	R-squared augmented spec	0.213	0.162	0.123	0.089
Family Income Rank	Log share top quintile	-.014**	-.010**	-.015**	-.009*
		(.005)	(.005)	(.006)	(.005)
	Log share bottom quintile	-.035***	-.042***	-.043***	-.010
		(.004)	(.014)	(.005)	(.014)
	p-val: Poor = Rich	0.049	0.186	0.032	0.936
	p-val: Egalitarian = Unequal	0.000	0.102	0.001	0.550
	R-squared	0.135	0.059	0.178	0.018
	R-squared augmented spec	0.136	0.082	0.185	0.020

Notes: See Sections IV.A-IV.B for information on cross-CBSA sample construction and local income distribution variables. Family income is defined as the sum of personal income and partner income (set to 0 if unpartnered). Nominal incomes are converted to 2015 US dollars before assigning ranks. Upward mobility for a given demographic group - local area cell is the average outcome after controlling for a quartic in CFI (as specified in Equation 2). For each of the three outcomes listed, upward mobility measures are then regressed on features of the local CFI distribution. The White (Black) samples contain 885 (281) different CBSAs. Robust standard errors appear below coefficient estimates in parentheses. Standard statistical significance legend used. The reported p-values are from statistical significance tests of two different hypothetical comparisons: Poor versus Rich, and Egalitarian versus Unequal. In the hypothetical Poor (Rich) area, 30% (10%) of children belong to the bottom CFI quintile and 10% (30%) of children belong to the top; in the hypothetical Egalitarian (Unequal) area, 12.5% (27.5%) of children belong to each of the bottom and the top quintiles. See Section IV.B for information on the augmented specification.
Source: 2011-2019 ACS linked to IRS 1040 Forms.

TABLE 4—OLS ESTIMATES OF THE EFFECTS OF RACIAL SEGREGATION ON WOMEN'S ABSOLUTE MOBILITY

		Panel A. White Women			*Panel B. Black Women*		
Absolute mobility outcome		(1)	(2)	(3)	(1)	(2)	(3)
Personal income rank	Racial segregation	0.095***	0.060***	.052**	-.025	-.024	-.021
		(.025)	(.022)	(.021)	(.027)	(.025)	(.024)
	R-squared	0.113	0.227	0.240	0.008	0.212	0.223
Partnering rate	Racial segregation	-.173***	-.070***	-.050***	-.155***	-.155***	-.147***
		(.024)	(.018)	(.016)	(.028)	(.029)	(.029)
	R-squared	0.252	0.394	0.438	0.110	0.189	0.195
Partner income rank | partnered	Racial segregation	0.074***	0.022	0.012	-.014	-.009	-.007
		(.022)	(.015)	(.013)	(.033)	(.034)	(.032)
	R-squared	0.107	0.230	0.257	0.001	0.038	0.058
Partner income rank spec 1	Racial segregation	-.058***	-.026***	-.018**	-.066***	-.061***	-.058***
		(.007)	(.008)	(.008)	(.012)	(.012)	(.013)
	R-squared	0.119	0.194	0.220	0.093	0.213	0.217
Partner income rank spec 2	Racial segregation	-.096***	-.040***	-.027***	-.100***	-.094***	-.088***
		(.012)	(.011)	(.011)	(.018)	(.018)	(.019)
	R-squared	0.161	0.256	0.290	0.101	0.222	0.225
Family income rank	Racial segregation	0.008	0.003	0.003	-.067***	-.063***	-.059***
		(.012)	(.011)	(.010)	(.020)	(.020)	(.019)
	R-squared	0.002	0.136	0.136	0.071	0.113	0.126
Controls							
Top and bottom quintile shares			X	X		X	X
Inequality at top and at bottom quintiles				X			X
N		900	900	900	300	300	300

Note: See Sections IV.A-IV.B for information on cross-CBSA sample construction and local income distribution and segregation variables. Family income is defined as the sum of personal income and partner income (set to 0 if unpartnered). Nominal incomes are converted to 2015 US dollars before assigning ranks. See Section II and note 8 for information on the two specifications used to handle unpartnered individuals in the assignment of partner income ranks. Coefficients are given in percentage points for the partnering rate and rank points for all other outcomes. Robust standard errors appear below coefficient estimates in parentheses. Standard statistical significance legend used.
Source: 2011-2019 ACS linked to IRS 1040 Forms.

TABLE 5—OLS ESTIMATES OF THE EFFECTS OF RACIAL SEGREGATION ON MEN'S ABSOLUTE MOBILITY

		Panel A. White Men			*Panel B. Black Men*		
Absolute mobility outcome		(1)	(2)	(3)	(1)	(2)	(3)
Personal income rank	Racial segregation	0.009	-.008	-.013	-.071***	-.070***	-.076***
		(.018)	(.013)	(.011)	(.019)	(.021)	(.021)
	R-squared	0.002	0.179	0.193	0.069	0.070	0.080
Partnering rate	Racial segregation	-.140***	-.060***	-.044***	-.122***	-.110***	-.119***
		(.018)	(.015)	(.014)	(.033)	(.035)	(.035)
	R-squared	0.215	0.336	0.371	0.055	0.151	0.194
Partner income rank | partnered	Racial segregation	0.111***	0.079***	0.070***	0.009	-.003	0.005
		(.025)	(.024)	(.023)	(.027)	(.029)	(.028)
	R-squared	0.133	0.209	0.223	0.001	0.112	0.116
Partner income rank spec 1	Racial segregation	-.011	0.014	0.019*	-.053***	-.048***	-.048***
		(.009)	(.011)	(.011)	(.016)	(.017)	(.017)
	R-squared	0.004	0.121	0.130	0.044	0.085	0.118
Partner income rank spec 2	Racial segregation	-.034**	0.011	0.020	-.078***	-.069***	-.071***
		(.012)	(.015)	(.015)	(.024)	(.025)	(.026)
	R-squared	0.020	0.151	0.166	0.043	0.098	0.135
Family income rank	Racial segregation	0.010	0.001	-.002	-.077***	-.074***	-.078***
		(.016)	(.013)	(.012)	(.017)	(.019)	(.019)
	R-squared	0.003	0.178	0.185	0.080	0.081	0.084
Controls							
Top and bottom quintile shares			X	X		X	X
Inequality at top and at bottom quintiles				X			X
N		900	900	900	300	300	300

Note: See Sections IV.A-IV.B for information on cross-CBSA sample construction and local income distribution and segregation variables. Family income is defined as the sum of personal income and partner income (set to 0 if unpartnered). Nominal incomes are converted to 2015 US dollars before assigning ranks. See Section II and note 8 for information on the two specifications used to handle unpartnered individuals in the assignment of partner income ranks. Coefficients are given in percentage points for the partnering rate and rank points for all other outcomes. Robust standard errors appear below coefficient estimates in parentheses. Standard statistical significance legend used.
Source: 2011-2019 ACS linked to IRS 1040 Forms.

TABLE 6—SELECTION-CORRECTED EFFECTS OF RACIAL SEGREGATION ON WOMEN'S ABSOLUTE MOBILITY

		Panel A. White Women		*Panel B. Black Women*	
Absolute mobility outcome	Inference for racial segregation	(2)	(3)	(2)	(3)
Personal income rank	Treatment effect	0.052**	0.040*	-.024	-.020
		(.021)	(.022)	(.026)	(.024)
	CI contains TE bounds?	Yes	Yes	Yes	Yes
Partnering rate	Treatment effect	-.043*	-.015	-.155***	-.144***
		(.025)	(.022)	(.033)	(.036)
	CI contains TE bounds?	No*	No	Yes	Yes
Partner income rank | partnered	Treatment effect	0.012	-.002	-.008	-.006
		(.017)	(.017)	(.040)	(.033)
	CI contains TE bounds?	Yes	No	Yes	Yes
Partner income rank spec 1	Treatment effect	-.017*	-.006	-.060***	-.055***
		(.010)	(.010)	(.014)	(.015)
	CI contains TE bounds?	No*	No	Yes	Yes
Partner income rank spec 2	Treatment effect	-.025	-.008	-.092***	-.085***
		(.015)	(.015)	(.021)	(.023)
	CI contains TE bounds?	No	No	Yes	Yes
Family income rank	Treatment effect	0.003	0.002	-.061**	-.057**
		(.010)	(.009)	(.026)	(.023)
	CI contains TE bounds?	Yes	Yes	Yes	Yes
Controls					
Top and bottom quintile shares		X	X	X	X
Inequality at top and at bottom quintiles			X		X
Assumptions					
Rmax		$1.3\tilde{R}$	$1.3\tilde{R}$	$1.3\tilde{R}$	$1.3\tilde{R}$
Delta		0.5	0.5	0.5	0.5

Note: See Sections IV.A-IV.B for information on cross-CBSA sample construction and local income distribution and segregation variables. Family income is defined as the sum of personal income and partner income (set to 0 if unpartnered). Nominal incomes are converted to 2015 US dollars before assigning ranks. See Section II and note 8 for information on the two specifications used to handle unpartnered individuals in the assignment of partner income ranks. Coefficients are given in percentage points for the partnering rate and rank points for all other outcomes. Treatment effect estimates and bootstrapped standard errors are computed by the *psacalc* statistical package provided by Oster (2019). Oster recommends a set of treatment effect bounds given by a conservative and an aggressive assumption regarding selection on unobservables: it is reported here whether the bootstrapped confidence interval, based on the "middle-ground" assumption stated in the table, contains Oster's recommended bounds. Standard statistical significance legend used.
Source: 2011-2019 ACS linked to IRS 1040 Forms.

TABLE 7—SELECTION-CORRECTED EFFECTS OF RACIAL SEGREGATION ON MEN'S ABSOLUTE MOBILITY

		Panel A. White Men		*Panel B. Black Men*	
Absolute mobility outcome	Inference for racial segregation	(2)	(3)	(2)	(3)
Personal income rank	Treatment effect	-.011	-.017	-.060**	-.079***
		(.012)	(.011)	(.029)	(.021)
	CI contains TE bounds?	Yes	Yes	Yes	Yes
Partnering rate	Treatment effect	-.038*	-.015	-.107***	-.118***
		(.020)	(.020)	(.041)	(.037)
	CI contains TE bounds?	No*	No	Yes	Yes
Partner income rank | partnered	Treatment effect	0.069***	.055**	-.004	0.004
		(.025)	(.025)	(.030)	(.031)
	CI contains TE bounds?	Yes	Yes	Yes	Yes
Partner income rank spec 1	Treatment effect	0.017	0.023*	-.046**	-.047***
		(.012)	(.013)	(.020)	(.018)
	CI contains TE bounds?	Yes	Yes	Yes	Yes
Partner income rank spec 2	Treatment effect	0.017	0.029	-.067**	-.070**
		(.017)	(.017)	(.029)	(.029)
	CI contains TE bounds?	Yes	Yes	Yes	Yes
Family income rank	Treatment effect	-.001	-.005	-.059**	-.081***
		(.013)	(.012)	(.031)	(.022)
	CI contains TE bounds?	Yes	Yes	Yes	Yes
Controls					
Top and bottom quintile shares		X	X	X	X
Inequality at top and at bottom quintiles			X		X
Assumptions					
Rmax		$1.3\tilde{R}$	$1.3\tilde{R}$	$1.3\tilde{R}$	$1.3\tilde{R}$
Delta		0.5	0.5	0.5	0.5

Note: See Sections IV.A-IV.B for information on cross-CBSA sample construction and local income distribution and segregation variables. Family income is defined as the sum of personal income and partner income (set to 0 if unpartnered). Nominal incomes are converted to 2015 US dollars before assigning ranks. See Section II and note 8 for information on the two specifications used to handle unpartnered individuals in the assignment of partner income ranks. Coefficients are given in percentage points for the partnering rate and rank points for all other outcomes. Treatment effect estimates and bootstrapped standard errors are computed by the *psacalc* statistical package provided by Oster (2019). Oster recommends a set of treatment effect bounds given by a conservative and an aggressive assumption regarding selection on unobservables: it is reported here whether the bootstrapped confidence interval, based on the "middle-ground" assumption stated in the table, contains Oster's recommended bounds. Standard statistical significance legend used.
Source: 2011-2019 ACS linked to IRS 1040 Forms.

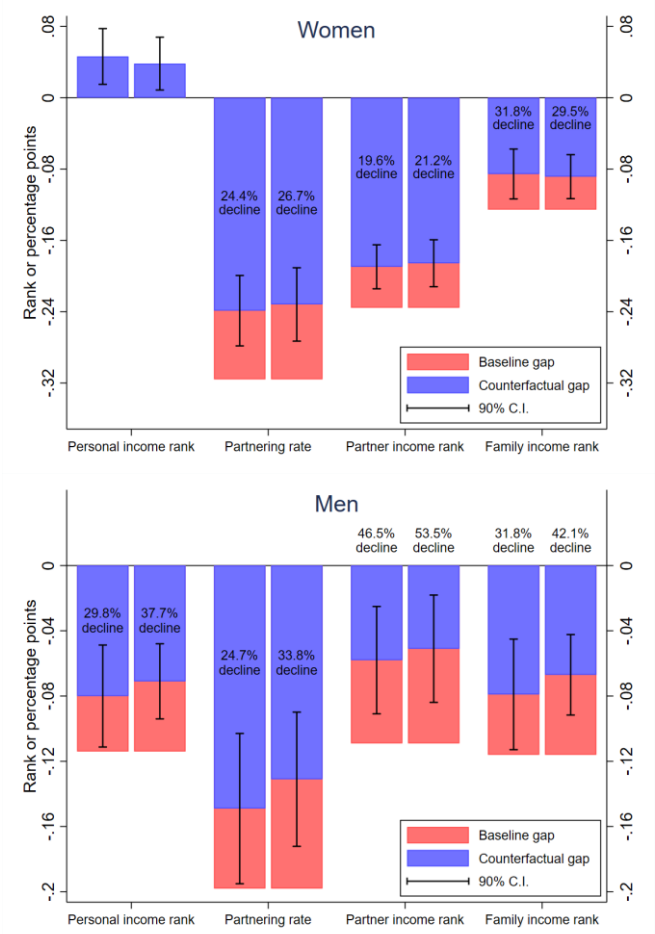


FIGURE 2. Contribution of Childhood Racial Segregation to Black-White Mobility Gaps

Note: Baseline mobility gap estimates (reported in the last column of Table 1) are plotted as the total height of each bar (i.e. blue + pink). Counterfactual gaps in blue are the predicted mobility gaps if childhood racial segregation were reduced to 0. See Section 6.4 for detail on the computation of these counterfactuals. Two counterfactuals are modeled for each outcome, one for each of the selection-correction specifications presented in Tables 6 and 7. The baseline personal income mobility gap between Black and White women is .005.
Source: 2011-2019 ACS linked to IRS 1040 Forms.

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