Child Penalties and Parental Role Models: Classroom Exposure Effects*

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Abstract

This paper investigates whether the effects of children on the labor market outcomes of women relative to men—child penalties—are shaped by the work behavior of peers' parents during adolescence. Leveraging quasi-random variation in the fraction of peers with working mothers across cohorts within schools, we find that greater exposure to working mothers reduces the child penalty in employment later in life. The effect is economically meaningful, precisely estimated, and robust across specifications. Evidence on mechanisms suggests that these effects are driven by exposure to maternal employment as a parental role model, shaping child-related gender gaps in the labor market.

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1 Introduction

A recent literature shows that much of the remaining gender inequality in labor market outcomes can be attributed to child penalties—the negative effects of parenthood on women relative to men (Kleven, Landais, and Søgaard 2019; Kleven *et al.* 2019; Cortés and Pan 2023). Research on the mechanisms that drive child penalties suggests that social norms and culture play an important role. For example, epidemiological studies of movers and migrants document strong effects of the cultural environment in which girls grow up on their child penalties later in life (Boelmann, Raute, and Schönberg 2025; Kleven 2025).

While prior studies provide an important starting point, they typically rely on broad, aggregate proxies for culture such as state-level attitudes or country of origin. This makes it difficult to separate the effects of cultural norms from the effects of other place-level characteristics, and it prevents an understanding of the precise setting and stage of life at which exposure to social norms is most critical. Building on the insight that the school environment of adolescent girls is important for female labor supply (Olivetti, Patacchini, and Zenou 2020), we examine whether school exposure to different parental role models influences child penalties in adulthood. Our hypothesis is that adolescent girls socialized in environments where mothers commonly work are more likely to develop gender-role ideals that reconcile career and motherhood, thereby resulting in smaller child penalties.

To study this question, we leverage rich longitudinal data on U.S. adolescents, linking them to their middle- and high-school peers and following them into adulthood as they become parents. The data include labor market information on classmates' parents, allowing us to examine whether exposure to different parental role models during adolescence shapes labor market outcomes later in life. To address identification concerns, we exploit idiosyncratic variation in the employment of peers' parents across cohorts within schools. This research design, first proposed by Hoxby (2000), has been widely used to estimate peer effects in education and labor market outcomes

(Hanushek, Kain, and Rivkin 2002; Angrist and Lang 2004; Friesen and Krauth 2007; Lavy and Schlosser 2011; Lavy, Paserman, and Schlosser 2012; Olivetti, Patacchini, and Zenou 2020). To our knowledge, it has not previously been applied to the study of child penalties, reflecting the demanding data requirements involved: such an analysis requires both credible identification of child penalties and their linkage to plausibly exogenous variation in peer composition during adolescence.

As in the existing literature, we estimate child penalties based on event studies of first childbirth (Kleven, Landais, and Søgaard 2019). We extend the standard approach by allowing treatment effects to vary with exposure to working mothers among middle- and high-school peers. Our baseline specification interacts event-time indicators with quantiles of exposure, controlling for school and cohort fixed effects. We present balance tests showing that, conditional on these fixed effects, adolescents with high and low exposure to working mothers are similar across a wide range of family background characteristics.

We find that girls in the top tertile of exposure to working mothers experience substantially smaller child penalties in adulthood than girls in the bottom tertile. The effect is sizable and precisely estimated: the difference in employment penalties between the top and bottom tertiles is roughly 11 percentage points. Estimates based on finer partitions of the exposure distribution—quintiles or deciles—yield similar patterns, with child penalties declining in exposure and substantial differences between the top and bottom of the distribution.

Our findings are highly robust across specifications, including models that allow for within-school time trends, as well as models that flexibly control for differential post-birth dynamics at the school and cohort level by interacting either school fixed effects or cohort fixed effects with post-birth indicators.[1] These specifications absorb school-specific or cohort-specific changes in outcomes around childbirth, addressing concerns about selection on preferences for family vs career across individuals from different schools or cohorts. The fact that our estimates hardly move when considering such rich

[1]We also estimate specifications that simultaneously interact school and cohort fixed effects with post-birth indicators. These models yield qualitatively similar results, but the estimates are under-powered due to limited within-cell variation.

specifications strengthens the credibility of our research design.[2]

We also examine an alternative exposure measure based on the work behavior of mothers relative to fathers among classroom peers. This analysis serves two purposes. First, it sheds light on whether adolescent girls respond primarily to maternal employment or to the gendered division of labor within peer-group families. Second, it provides an additional identification check by differencing out confounders that operate similarly for mothers and fathers. We obtain virtually identical estimates using this alternative exposure measure, further supporting the credibility of the empirical design.

As in any study of peer effects, isolating the precise mechanisms underlying the reduced-form estimates is challenging. We make progress on this front by exploiting the richness of our data. Two findings are worth highlighting. First, the exposure effects are stronger when focusing on same-race peers: the difference in employment penalties between the top and bottom tertiles is about 15 percentage points, compared to 11 percentage points in the baseline specification. This is consistent with the notion that adolescents form preferences and beliefs based on socially similar peers. Second, we examine whether the exposure effects operate through classmates' parents or through the classmates themselves. We find that the effects are driven by parental role models: conditional on adolescent exposure to working mothers, contemporaneous exposure to working women from the same school and grade has no effect on child penalties. Together, these findings support a parental role-model interpretation of the exposure effects.

Our paper contributes to a growing literature on child penalties and their determinants (e.g., Kleven, Landais, and Søgaard 2019; Kleven *et al.* 2019; Kleven, Landais, and Søgaard 2021; Moriconi and Rodríguez-Planas 2021; Andresen and Nix 2022; Cortés and Pan 2023; Kleven, Landais, and Leite-Mariante 2025). By estimating peer effects on child penalties in employment, it also contributes to research on the role of gender norms in shaping

[2] A natural question is whether the exposure effects are driven by employment status per se or by other characteristics correlated with employment. One such candidate is education: working mothers tend to be more educated than non-working mothers, and this may be part of the broader role-model package affecting adolescent girls. To address this concern, we estimate the same specification using exposure to college-educated mothers instead of exposure to working mothers. We find no effect of such exposure on child penalties.

female labor supply (e.g., Fernández, Fogli, and Olivetti 2004; Fortin 2005; Fernández and Fogli 2009; Blau, Kahn, and Papps 2011; Olivetti, Patacchini, and Zenou 2020; Bertrand 2020). Our findings relate most directly to Boelmann, Raute, and Schönberg (2025) and Kleven (2025), who document strong associations between child penalties for movers and migrants and the child penalty in their place of birth. We add to this literature by exploiting quasi-random variation in exposure to gender norms within narrowly defined settings—different cohorts within the same school—thereby isolating the influence of highly localized role models rather than broad place-level culture. Identifying the school environment as a setting in which preferences related to work and family roles are formed is important for understanding when and how gender inequality emerges. Taken together, our results challenge the notion that social norms are necessarily sticky and slow-moving, showing that local variation in parental role models can have long-term consequences for child penalties and gender gaps.

The paper proceeds as follows. Section 2 describes the data and empirical methodology. Section 3 presents the empirical findings. Section 4 concludes.

2 Data and Methodology

2.1 Data

The analysis is based on Add Health data (Harris 2018), a school-based longitudinal survey designed to be nationally representative of students in grades 7–12 in the United States.[3] This covers two years of middle school and all years of high school. Add Health includes a representative set of 144 schools, starting with the 1994-1995 school year (Wave I). Every student on the school roster was asked to complete an *In-School Questionnaire* which included basic questions about the student's demographics and the

[3]The National Longitudinal Study of Adolescent to Adult Health (Add Health) is funded by grant P01-HD31921 from the *Eunice Kennedy Shriver* National Institute of Child Health and Human Development (NICHD), with cooperative funding from 23 other federal agencies and foundations. Add Health is currently directed by Robert A. Hummer at the University of North Carolina at Chapel Hill. The study was designed by J. Richard Udry, Peter S. Bearman, and Kathleen Mullan Harris at the University of North Carolina at Chapel Hill. See Harris *et al.* (2019) for a detailed description of the survey design. Information on how to obtain access to the Add Health data files is available on the website www.cpc.unc.edu/addhealth.

characteristics of their parents, including educational attainment and employment. A randomly selected subsample of about 20,000 students were also interviewed at home, where in-depth questions about family, attitudes, and other sensitive topics were asked. This is labeled the *In-Home Questionnaire*. Only students selected for the Wave I in-home survey were re-interviewed in 1996 (Wave II), 2001-2002 (Wave III), 2007-2008 (Wave IV), and 2016-2019 (Wave V). In these follow-up interviews, individuals were asked extensive questions covering topics such as employment and fertility.

We link information from Wave I, III, IV, and V.[4] The longitudinal structure of the data allows us to track adolescents into adulthood and gather information on their childbearing and employment outcomes. Because our estimation strategy exploits school×cohort variation in student composition, we retrieve information on all students included in the in-school survey of Wave I. For each student, we construct a measure of exposure to working mothers/fathers, calculated as the fraction of peers from the same school and cohort with working mothers/fathers. Having a "working" parent is defined as the parent being employed in a wage-paying occupation at the time that the student attended school.

We retrieve fertility and employment histories by pooling together information from Waves III-V. In each wave, respondents were asked to provide the exact dates of each of their pregnancies, which we use to identify the date of first childbirth. We meticulously reconstruct longitudinal employment histories by collecting current and past employment statuses. Due to variation in employment questions across survey waves, we construct a harmonized labor supply indicator. This indicator equals 1 if the respondent reports working at least 20 hours per week for at least 3 months of a given year, and 0 otherwise.

Our estimation sample is an unbalanced panel of men and women who had their first child between the ages of 22 and 40, and who are observed between five years before and five years after the birth of their first child. This leaves us with a dataset of 22,452 individual×year observations.

[4] In Wave V, AddHealth switched from in-person interviews to mixed-method interviews. To keep technology constant, we only keep Wave V respondents who were administered an in-person interview.

2.2 Event Study Framework

We estimate child penalties based on event studies around the birth of the first child, following the approach developed by Kleven, Landais, and Søgaard (2019). Event time is denoted by $\tau = t - T_i$, where t is calendar year and T_i is the year of first childbirth. The key innovation relative to standard child penalty estimations is to allow treatment effects to vary with exposure to working mothers—or to working mothers relative to working fathers—during middle and high school, conditioning on school and cohort fixed effects.

Specifically, we estimate the following event study separately for men and women:

$$Y_{it}^g = \boldsymbol{\beta_e^g} \cdot \boldsymbol{D_{i\tau}} \cdot \mathbf{1}\left[E_i = e\right] + \gamma_e + \delta_s + \zeta_c + \eta_a + \theta_t + \nu_{it}^g, \tag{1}$$

where Y_{it}^g is the employment outcome for individual i of gender $g = w, m$ in year t. On the right-hand side, boldface denotes vectors. The first term includes indicators for each event time τ, omitting a base year prior to childbirth. The omitted base year is $\tau = -2$, the year before pregnancy. Event-time indicators are interacted with indicators for quantiles of exposure to working mothers, defined as the fraction of classroom peers with working mothers. We divide the distribution of classroom exposure into tertiles (low, medium, and high), but also consider finer partitions of the exposure distribution. The event time coefficients $\beta_{e\tau}^g \in \boldsymbol{\beta_e^g}$ measure the impact of childbirth on gender g at event time τ in tertile e of exposure to working mothers.

The specification includes fixed effects for quantile of exposure (γ_e), school (δ_s), student cohort (ζ_c), age (η_a), and year (θ_t). The school fixed effects control for all cohort-invariant differences in students, teachers, and resources across different schools. The cohort fixed effects control for all school-invariant differences across cohorts of students. The age and year fixed effects control for any lifecycle and time trends in employment. In an extensive set of robustness checks, we include additional controls for the demographic characteristics of individuals' parents, and their peers' parents. We also estimate specifications with individual fixed effects and linear time trends, as well as models that allow for school- and cohort-specific post-birth dynamics. As we shall see, the estimates are highly robust to these specification choices.

Estimating equation (1) gives the effects of childbirth in levels. We convert the level effects into percentage effects by calculating

$$P_{e\tau}^{g} = \frac{\hat{\beta}_{e\tau}^{g}}{\mathbb{E}\left[\tilde{Y}_{it}^{g} \mid e, \tau\right]}, \tag{2}$$

where $\tilde{Y}_{it}^{g}$ is the predicted outcome when omitting the contribution of the event time coefficients, i.e. the counterfactual outcome absent children. While we focus on effects in percentage terms, the effects in absolute terms (percentage points) are qualitatively similar.

Finally, we define the child penalty as the average effect of parenthood on women relative to men over five years following the first childbirth. The child penalty in exposure tertile e is given by

$$\text{Child Penalty}_{e} \equiv \mathbb{E}\left[P_{e\tau}^{m} - P_{e\tau}^{w} \mid e, \tau \geq 0\right]. \tag{3}$$

A positive child penalty implies that parenthood increases the gender gap. To estimate the effect of classroom exposure to working mothers, we compare child penalties in the top and bottom tertiles of exposure. Given the empirical design, this comparison is based on variation across individuals who were differentially exposed to working mothers *in different cohorts of the same school*.

2.3 Variation in Exposure Measures

To illustrate the identifying variation, Figure 1 plots the distributions of our two exposure measures: the share of working mothers (Panel A) and the share of working mothers relative to the share of working fathers (Panel B). Both measures are defined at the school×cohort level.

Two points are worth noting. First, both exposure measures exhibit substantial variation across school×cohort cells. For example, the share of working mothers ranges from roughly 0.50 to 1.00, with the mode around 0.8. This wide support is essential for our empirical strategy, as it ensures that students are exposed to meaningfully different parental employment environments.

Second, the vertical lines in each panel mark the bottom and top tertiles of exposure, corresponding to the exposure groups used in our baseline specification. For the share of working mothers (Panel A), mean exposure equals 0.74 in the bottom tertile and 0.89 in the top tertile—a gap of 15 percentage points.[5] A similarly wide gap appears for the relative exposure measure (Panel B), where the means in the bottom and top tertiles are 0.78 and 0.93, respectively. These differences indicate that the tertile split generates a clear separation between low- and high-exposure students. To obtain an even stronger separation, we also estimate child penalties by quintiles of classroom exposure.

2.4 Balance Tests

To validate the empirical design, Table 1 presents a set of balance tests. The table reports results from regressions of family background variables on our baseline exposure measure—indicators for having a low, medium, or high share of classroom peers with working mothers. We show results separately for girls (left) and boys (right). The following dependent variables are included: having a college-educated parent, living in a two-parent household, having a US-born parent, having a working mother, having a working father, parental income per child, number of siblings, and race/ethnicity (white, black, and Hispanic). Each regression controls for school and cohort fixed effects. The reported coefficients represent predicted values in the top and bottom tertiles, as well as their difference.[6] Corresponding results for our alternative exposure measure—the share of working mothers relative to the share of working fathers—are presented in Appendix Table A.1.

The balance tests provide strong support for our empirical design. Among the twenty differences reported in the table, none are statistically significant at the 10% level.[7] This mitigates concerns that students with different levels of peer exposure are

[5]For reference, this difference is comparable to the increase in labor force participation among U.S. mothers with children aged 6-17 between 1980 and 2000, which rose by roughly 15 percentage points according to the U.S. Bureau of Labor Statistics.

[6]Specifically, the coefficients are the predicted values (in the top and bottom tertiles, respectively) after adding back the average school and cohort fixed effects in the full sample. This is done to provide meaningful levels of the different variables.

[7]One might notice the coefficient on parental income, where the difference between tertiles is somewhat larger and negative (though not statistically significant). Two considerations temper this concern. First,

selected on characteristics that affect child penalties. While we cannot rule out selection on unobservables, the absence of selection on observables makes it less likely that unobserved confounders generate substantial bias (see, e.g., Altonji, Elder, and Taber 2005).

3 Empirical Results

3.1 Main Results

To establish a benchmark and assess whether our sample differs from those studied in the prior literature, we begin by estimating the average child penalty in the full sample. The results are presented in Figure 2, which shows event studies of first childbirth for men (black series) and women (red series) from five years before to five years after childbirth. Each dot represents the percentage effect on employment in event year τ (relative to the omitted base year $\tau = -2$), estimated using the specification in Kleven, Landais, and Søgaard (2019).[8] The year of pregnancy is marked by a vertical dashed line, and the year of childbirth is marked by a vertical solid line. The figure also reports the average child penalty across event times 0-5.

As in prior studies, our sample exhibits parallel trends for men and women before pregnancy, a modest divergence during pregnancy, and a sharp divergence after childbirth. Having a child is essentially a non-event for men, while it leads to an immediate and persistent drop in employment for women. The resulting child penalty

parental income is a relatively noisy measure in Add Health, as it suffers from higher rates of missingness and is self-reported. Second, and more importantly, when using our alternative exposure measure—the share of working mothers relative to working fathers—the corresponding coefficient is of similar magnitude but positive. The sign instability across exposure measures (and the fact that our results are almost identical when using either of them) suggests that parental income does not systematically predict exposure in a way that would bias our results.

[8] This is a simplified version of equation (1) in which we do not interact the event time indicators with quantile of classroom exposure and without fixed effects for quantile of exposure (γ_e), school (δ_s), and cohort (ζ_c). The event study regression is specified as follows:

$$Y_{it}^g = \boldsymbol{\beta^g} \cdot \boldsymbol{D_{i\tau}} + \eta_a + \theta_t + \nu_{it}^g. \tag{4}$$

We calculate percentage effects as $P_\tau^g = \hat{\beta}_\tau^g / \mathbb{E}\left[\tilde{Y}_{it}^g \mid \tau\right]$ for gender g at event time τ, where $\tilde{Y}_{it}^g$ is the predicted outcome absent the effect of childbirth.

equals 22.4% and is precisely estimated. This estimate is virtually identical to the child penalty in employment reported by Kleven (2025) using PSID and NLSY data. He finds a child penalty of 22 percent in annual employment—closest to our employment outcome. Hence, there is nothing unusual about our sample in terms of the impact of parenthood on male and female employment outcomes.

Having established a baseline, we turn to our main research question: the effect of classroom exposure to working mothers on child penalties later in life. Figure 3 presents event studies of first childbirth based on specification (1). The top row focuses on our baseline exposure measure—the share of classmates with working mothers. The panels show employment effects of parenthood for women and men, respectively, in the top and bottom tertiles of classroom exposure. The figure is otherwise constructed in the same way as the preceding one.

We find strong exposure effects for women. Relative to the underlying lifecycle and time trends, women in both the low- and high-exposure groups exhibit parallel trends prior to pregnancy and sharp divergence immediately thereafter. While the patterns are qualitatively similar, the magnitudes differ substantially. The average employment decline due to motherhood equals 29.8 percent in the low-exposure group and 17.6 percent in the high-exposure group. The difference between the two—the effect of greater exposure to working mothers—equals 12.2 percentage points and is strongly statistically significant. By contrast, we find no exposure effects for men: having a child is a non-event for them, regardless of their exposure to working mothers during middle and high school.

The bottom row of Figure 3 examines our alternative exposure measure—the share of working mothers relative to the share of working fathers among classroom peers. As discussed above, this measure serves two purposes. First, it helps distinguish whether adolescent girls respond primarily to maternal employment or to the gendered division of labor within peer-group families. Second, it provides a validation check by differencing out confounders that operate similarly for mothers and fathers. The results closely mirror those from the baseline specification: women with greater classroom exposure exhibit substantially smaller employment effects of childbirth, and the magnitude of the exposure effect is nearly identical to that obtained from the non-relative measure. This robustness

to the choice of exposure measure supports the credibility of the empirical design.

Table 2 reports results from our baseline specification and summarizes the visual evidence presented above. The first two rows show the effects of children on women and men, respectively, by classroom exposure to working mothers (left) and working mothers relative to working fathers (right). The third row shows the implied effect on the child penalty under each exposure measure. Standard errors are reported in parentheses. Greater exposure to working mothers during adolescence reduces the employment penalty by 10.8 percentage points, and the effect is strongly statistically significant. The alternative exposure measure yields a very similar estimate of 10.6 percentage points. These results suggest that the labor market outcomes of mothers are shaped by the parental role models they are exposed to during adolescence: when these role models include a greater share of working mothers ("modern families"), child penalties are smaller.

Table 3 evaluates the robustness of our estimates to richer specifications. The first row includes individual fixed effects, which absorb all time-invariant characteristics of individuals.[9] The next two rows consider specifications that control for the demographic characteristics of individuals' parents and their peers' parents, respectively.[10] The fourth row allows for within-school linear time trends. Our estimates are highly robust to these alternative specifications.

This robustness is not particularly surprising: these specifications control for level effects (or linear time trends), while our child penalty estimates rely on sharp changes around first childbirth. Given our empirical design, the larger threat to identification is selection on post-birth dynamics itself—for example, due to differences in the underlying preferences for family vs career across individuals from different cohorts within schools. To address such selection concerns, we would ideally add school×cohort fixed effects

[9] In a sample restricted to individuals who have children, it is challenging to separately estimate individual fixed effects, year fixed effects, and event-time coefficients (Miller 2023; Kleven 2025). To identify all three sets of coefficients, we expand the estimation sample to include individuals who never have children (for whom event time is undefined).

[10] The control variables are specified as follows. Parent controls: mother's employment, parents' education (college degree or not), parents' marital status, and parents' race. Peer-parent controls: fraction of peers with at least one college-educated parent, fraction of peers with married parents, and fractions from each race.

interacted with post-birth indicators, effectively allowing each school-cohort cell to have its own child penalty. Such a specification is infeasible because it would absorb all of the variation we exploit for estimating the effects of classroom exposure. Therefore, we adopt a more parsimonious approach that allows for heterogeneity in post-birth dynamics across schools and cohorts separately, but not across the interaction between the two. These specifications absorb differences in post-birth dynamics across institutional and cohort environments, while preserving the within-school-by-cohort variation used for identification. As shown in the table, the estimates are robust to cohort$\times$post-birth and school$\times$post-birth fixed effects, especially when using the relative measure of classroom exposure. However, adding both sets of fixed effects simultaneously reduces statistical precision as such a saturated specification soaks up much of the identifying variation across school-cohort cells. Still, the estimated effects remain economically meaningful and are qualitatively consistent with our baseline results.

In the last row of Table 3, we conduct a placebo test in which we reassign exposure using peers from an adjacent cohort within the same school (specifically, cohort $c-1$). This approach preserves the broader school environment while breaking the link between the individual and the actual peer group to which they were exposed. If our estimates were driven by cohort trends in unobserved preferences within schools, then exposure measured based on cohort $c-1$ would yield roughly similar effects as exposure to cohort c. By contrast, if the mechanism operates through direct exposure to students' own classroom peers, only exposure measured based on the actual classroom should matter. Indeed, we find that the placebo estimates are close to zero and statistically insignificant.

As a related check, Appendix Figure A.1 presents results from a Monte Carlo simulation in which we randomly reassign students to different school$\times$cohort cells. The figure shows the distribution of placebo estimates based on 1,000 draws for each of our two exposure measures. The placebo estimates are symmetrically distributed around zero. These results imply that our estimates—demarcated by vertical lines—are highly unlikely to arise by chance. The randomization-based p-values equal 0.001 and 0.002, respectively.

Finally, we consider a finer partition of the exposure distribution. Figure 4 shows child

penalties by quintiles of classroom exposure. The estimates are based on a modified version of equation (1) in which event-time indicators are interacted with quintiles of exposure. We focus on the relative exposure measure—the share of working mothers relative to working fathers—because it is empirically smoother and better suited for a more granular breakdown. The figure reveals a strong and nearly monotonic decline in child penalties as exposure increases: moving from the bottom to the top quintile reduces the child penalty by about 15 percentage points. This declining relationship demonstrates that the results are not driven by the choice of exposure bins and reinforces a causal interpretation of the relationship between exposure and child penalties.

3.2 Additional Results

3.2.1 Effect of Peers' Mothers vs Own Mother

To assess the magnitude of the classroom exposure effects we estimate, it is useful to compare the effect of peers' mothers to the effect of own mothers. Specifically, we implement an augmented version of equation (1) in which the event-time×exposure indicators are interacted with an indicator for whether the individual's own mother worked during middle and high school. The effect of peers' mothers is obtained by comparing coefficients in the bottom and top tertiles of exposure, averaging across individuals with and without a working mother. The effect of own mother is obtained from the coefficient on own maternal employment, averaging across different exposure tertiles.

Figure 5 presents results for each exposure measure: the classroom share of working mothers (left panel) and the classroom share of working mothers relative to working fathers (right panel). The effects of peer and family exposure to working mothers are similar in magnitude. Each form of exposure reduces the child penalty by 10-12 percentage points. Statistically, we cannot reject that the effects are the same. Hence, our findings suggest that school environment during adolescence may be as important for subsequent child penalties as one's own family.

3.2.2 Heterogeneity and Mechanisms

Are the effects of classroom exposure to working mothers truly driven by their *work* behavior or by correlated characteristics of those mothers? Even if the empirical design is valid and the exposure effects therefore causal, it is conceivable that the effects operate through role-model features other than employment per se. The most obvious candidate is education: maternal employment and education levels are positively correlated across school-cohort cells. It is therefore possible that exposure to lower- or higher-educated mothers is what shapes career preferences later in life and ultimately drives our findings. To test for this possibility, Appendix Figure A.2 presents event studies of first childbirth by exposure to college-educated mothers.[11] As the figure shows, the event studies are very similar in the two exposure groups, for both women and men. This supports our interpretation of the data: child penalties in employment are shaped by the employment patterns among mothers of adolescent peers, not by the education patterns.

We next consider alternative definitions of the relevant peer group. Our baseline specification includes all students from the same school and cohort, consistent with previous work using the same empirical design (e.g., Hoxby 2000). However, teenagers may form preferences and beliefs based on a narrower set of socially similar peers. Because individuals tend to interact more with those who resemble them (and are more likely to identify with their norms), they may be more susceptible to the role models observed within a subgroup of similar classmates. Existing research suggests that homophily is particularly strong in the race dimension (e.g., McPherson, Smith-Lovin, and Cook 2001), making this a natural candidate to consider. Appendix Figure A.3 repeats our analysis when restricting attention to same-race peers. We do find that the exposure effects are larger when focusing on this peer group. For women, the employment drop due to parenthood equals 30% in the low-exposure group and 15.2% in the high-exposure group, corresponding to an exposure effect of 14.8pp.[12]

[11] The correlation coefficient between the share of classmates with working mothers and the share with college-educated mothers equals 0.57, which leaves sufficient independent variation to distinguish the effects of exposure to working mothers and highly-educated mothers.

[12] We may alternatively focus on same-gender peers, although it is less clear what to expect in this dimension. Even if school friendships are sorted on gender, it seems plausible that girls will look to both girls and boys when forming expectations about gender roles. As we show in Appendix Figure A.4,

Lastly, we examine whether the observed effect of exposure to working mothers among peers might reflect the long-run employment outcomes of the peers themselves. Adolescents exposed to a greater share of working mothers in school may be surrounded by different peers in adulthood. For example, they may retain their school peers into adulthood or they may acquire similar peers over time. To disentangle these two channels (role models through peers' mothers versus the peers themselves), we split each tertile of exposure to working mothers into low vs high contemporaneous exposure to working female peers. We measure peer employment exposure using the average post-child employment rate of women who attended the same school and grade.[13] We then estimate event studies of first childbirth by low vs high exposure to peer employment (below vs above the median), within each tertile of adolescent exposure to working mothers. The results are presented in Appendix Figure A.5. Conditional on adolescent exposure to working mothers, we find similar effects of childbirth across women with low and high adult exposure to working peers. As shown in the figure, the differences are small and statistically insignificant. These findings provide additional support for our interpretation of the data: they indicate that our estimates are not driven by the labor market attachment of contemporaneous peers, but rather by the normative signals provided by the adult role models experienced during adolescence.

To summarize, the results in this section provide three insights. First, that the classroom exposure effects are specifically tied to maternal employment, as opposed to correlated variables like education. Second, that the role model mechanism is stronger for socially similar peers, proxied by race. And third, that the mechanism operates through classmates' mothers (adult role models) rather than through the classmates themselves.

4 Conclusion

Recent research finds sizable child penalties in female labor market outcomes and shows that these penalties account for much of the remaining gender inequality in high-income

restricting attention to same-gender peers does not yield a larger exposure effect.

[13] This measure includes all female peers who appear in the follow-up samples and have children of their own.

countries (Kleven, Landais, and Søgaard 2019; Kleven *et al.* 2019; Kleven, Landais, and Leite-Mariante 2025). This paper examines the determinants of child penalties, focusing on exposure to gender norms during middle and high school. Using national longitudinal data on U.S. adolescents—followed from their school years into adulthood—we exploit quasi-random variation in the fraction of peers with working mothers across student cohorts within schools. This empirical design, first proposed by Hoxby (2000), has been widely used to study peer effects, but not in the context of child penalties. We find that greater exposure to working mothers during adolescence substantially reduces child penalties. The estimates are highly robust across specifications. We provide evidence that these effects are indeed driven by the *work* behavior of peers' parents, rather than by correlated characteristics such as parental education.

We interpret our findings as suggesting that parental role models during adolescence have economically meaningful effects on child penalties. Adolescent girls socialized in environments with a greater share of working mothers appear to internalize norms that reconcile motherhood and career. Our findings are consistent with existing work on child penalties and gender norms, while offering additional empirical credibility and new insights. In particular, our measure of gender norms is more local and directly relevant than the state- and country-level proxies used in recent epidemiological studies (Boelmann, Raute, and Schönberg 2025; Kleven 2025). By relying on variation across cohorts within schools, our estimates are arguably less susceptible to conflating gender norms with other features of places, such as economic conditions, demographic composition, or policy environments. Overall, our study highlights adolescence—and the school setting in particular—as a critical period in which preferences are shaped and the seeds of subsequent child penalties are sown.

Taken together, our findings lend support to the idea that preference formation, social norms, and culture play a central role in shaping child penalties and, in turn, gender inequality. This suggests that public policies aimed at increasing maternal labor supply and reducing child penalties may have limited effects unless they are complemented by initiatives that shift societal preferences or promote exposure to diverse role models. This perspective may help explain recent findings that family policies such as parental leave

schemes and childcare provision tend to have relatively small effects on child penalties (see, e.g., Kleven *et al.* 2024).

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TABLE 1: BALANCE TESTS BY EXPOSURE TO WORKING MOTHERS

	Girls' Exposure to Working Moms			Boys' Exposure to Working Moms		
	Low	**High**	**Δ**	**Low**	**High**	**Δ**
College-Educated Parent	0.344	0.343	**-0.002**	0.351	0.356	**0.004**
	(0.059)	(0.066)	**(0.043)**	(0.083)	(0.099)	**(0.062)**
Two-Parent Household	0.714	0.695	**-0.020**	0.705	0.768	**0.063**
	(0.051)	(0.057)	**(0.029)**	(0.059)	(0.083)	**(0.056)**
US-Born Parent	0.912	0.926	**0.014**	0.910	0.906	**-0.004**
	(0.026)	(0.029)	**(0.015)**	(0.046)	(0.054)	**(0.020)**
Own Mom Works	0.792	0.823	**0.031**	0.693	0.755	**0.062**
	(0.047)	(0.063)	**(0.040)**	(0.071)	(0.102)	**(0.053)**
Own Dad Works	0.931	0.924	**-0.007**	0.911	0.951	**0.041**
	(0.029)	(0.046)	**(0.033)**	(0.039)	(0.058)	**(0.033)**
Parental Income	23,344	20,992	**-2,352**	23,354	21,564	**-1,790**
	(1,987)	(2,405)	**(1,613)**	(2,261)	(3,025)	**(2,037)**
Number of Siblings	1.439	1.532	**0.093**	1.398	1.513	**0.114**
	(0.127)	(0.174)	**(0.128)**	(0.133)	(0.204)	**(0.148)**
White	0.642	0.618	**-0.025**	0.582	0.619	**0.037**
	(0.044)	(0.049)	**(0.037)**	(0.048)	(0.066)	**(0.046)**
Black	0.197	0.195	**-0.002**	0.213	0.189	**-0.024**
	(0.043)	(0.046)	**(0.029)**	(0.026)	(0.045)	**(0.033)**
Hispanic	0.091	0.107	**0.016**	0.113	0.137	**0.024**
	(0.021)	(0.037)	**(0.026)**	(0.023)	(0.034)	**(0.027)**
School FE	X	X	X	X	X	X
Cohort FE	X	X	X	X	X	X
No. Observations	733	792	1525	521	613	1134

Notes: This table presents balance tests for girls (left) and boys (right) with respect to our main peer exposure variable: the fraction of classmates with working mothers. The table provides results from regressions of family background variables on tertiles of peer exposure (low, medium, and high). The following dependent variables are considered: having a college-educated parent, living in a two-parent household, having a US-born parent, having a working mother, having a working father, parental income (per child), number of siblings, and race/ethnicity. Each regression controls for school and cohort fixed effects. The coefficients shown are the predicted values in the low and the high tertiles, as well as their difference. None of the differences are statistically significant at the 10% level, suggesting that the variation in peer exposure used for identification is quasi-random. Standard errors are clustered at the school level.

TABLE 2: EFFECTS OF CLASSROOM EXPOSURE ON CHILD PENALTIES

	Exposure to Working Moms			Exposure to Working Moms Relative to Working Dads		
	Low	**High**	Δ	**Low**	**High**	Δ
Effect of Children on Women	-0.298	-0.176	**0.122**	-0.287	-0.175	**0.113**
	(0.021)	(0.021)	(0.029)	(0.023)	(0.021)	(0.031)
Effect of Children on Men	-0.034	-0.020	**0.014**	-0.029	-0.022	**0.006**
	(0.019)	(0.022)	(0.027)	(0.017)	(0.022)	(0.027)
Child Penalty (Men - Women)	0.265	0.157	**-0.108**	0.259	0.152	**-0.106**
	(0.029)	(0.029)	(0.039)	(0.029)	(0.031)	(0.042)

Notes: This table provides estimates of child penalties in employment by exposure to working mothers (left columns) and working mothers relative to working fathers (right columns) during middle and high school. The distribution of exposure is divided into tertiles (low, medium, and high). The estimates are based on event studies of first childbirth using equation (1). The table shows that greater exposure to working mothers reduces the child penalty, and that the effect sizes are virtually identical under the two exposure measures. Standard errors (in parentheses) are bootstrapped with 500 repetitions.

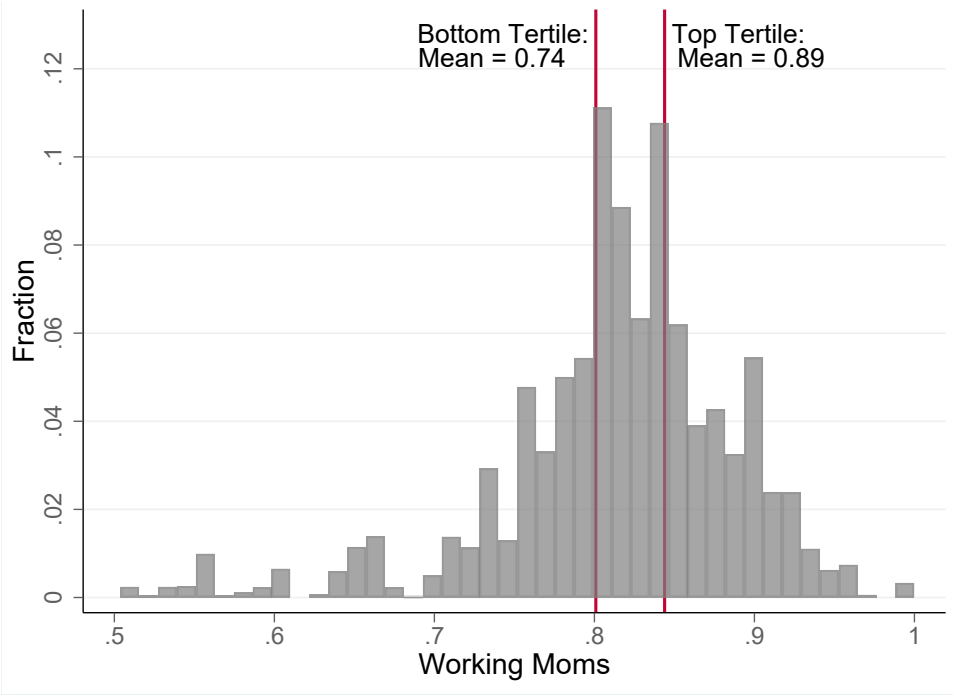
TABLE 3: ROBUSTNESS OF CLASSROOM EXPOSURE EFFECTS ON CHILD PENALTIES

	Exposure to Working Moms			**Exposure to Working Moms Relative to Working Dads**		
	Low	**High**	Δ	**Low**	**High**	Δ
Individual Fixed Effects	0.285	0.171	**-0.114**	0.280	0.176	**-0.104**
	(0.026)	(0.027)	(0.037)	(0.025)	(0.028)	(0.036)
Parents Demographics	0.275	0.161	**-0.114**	0.268	0.158	**-0.111**
	(0.029)	(0.029)	(0.039)	(0.030)	(0.031)	(0.043)
Peer-Parents Demographics	0.264	0.155	**-0.109**	0.258	0.151	**-0.107**
	(0.029)	(0.029)	(0.039)	(0.029)	(0.031)	(0.042)
Linear School Trends	0.265	0.172	**-0.093**	0.259	0.166	**-0.093**
	(0.029)	(0.029)	(0.039)	(0.029)	(0.031)	(0.042)
Own Mom×Post-Birth	0.259	0.153	**-0.106**	0.253	0.148	**-0.105**
	(0.030)	(0.030)	(0.039)	(0.029)	(0.032)	(0.043)
Cohort×Post-Birth	0.254	0.146	**-0.108**	0.252	0.147	**-0.105**
	(0.036)	(0.035)	(0.040)	(0.039)	(0.038)	(0.043)
School×Post-Birth	0.437	0.356	**-0.081**	0.429	0.329	**-0.100**
	(0.098)	(0.119)	(0.052)	(0.098)	(0.117)	(0.049)
School & Cohort×Post-Birth	0.480	0.416	**-0.064**	0.474	0.383	**-0.091**
	(0.128)	(0.151)	(0.059)	(0.125)	(0.146)	(0.054)
Placebo Exposure	0.240	0.247	**0.007**	0.265	0.289	**0.024**
	(0.052)	(0.063)	(0.074)	(0.078)	(0.058)	(0.087)

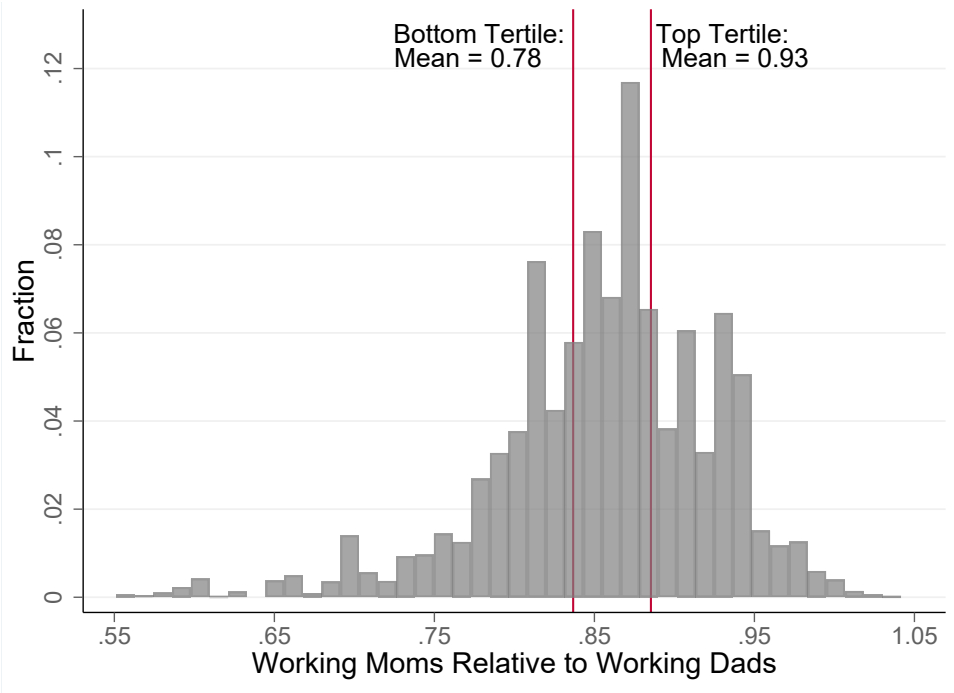
Notes: This table examines the robustness of our estimates of classroom exposure effects on child penalties. We provide estimates for each exposure measure across a range of specifications with richer controls than the baseline specification (1) considered in Table 2. Dividing the distribution of exposure into tertiles (low, medium, and high), we compare child penalties in the lowest and highest tertiles. The table shows that the estimates are highly robust across specifications. Standard errors (in parentheses) are bootstrapped with 500 repetitions.

FIGURE 1: DISTRIBUTION OF EXPOSURE VARIABLES

A. Share of Working Moms

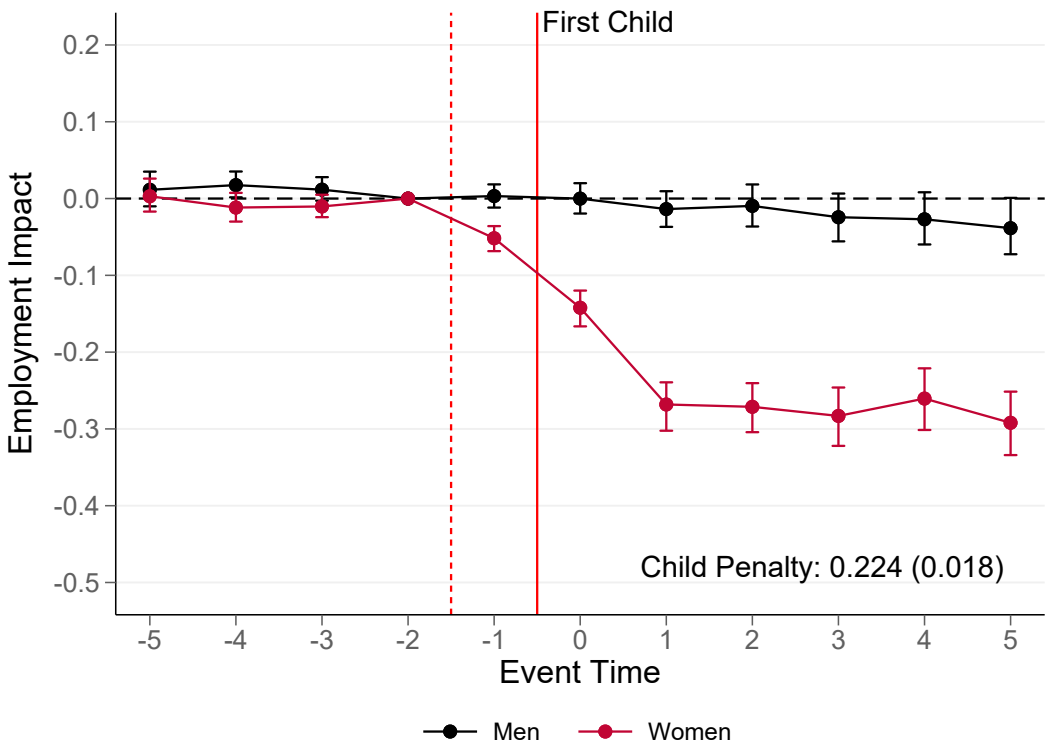


B. Share of Working Moms Relative to Working Dads



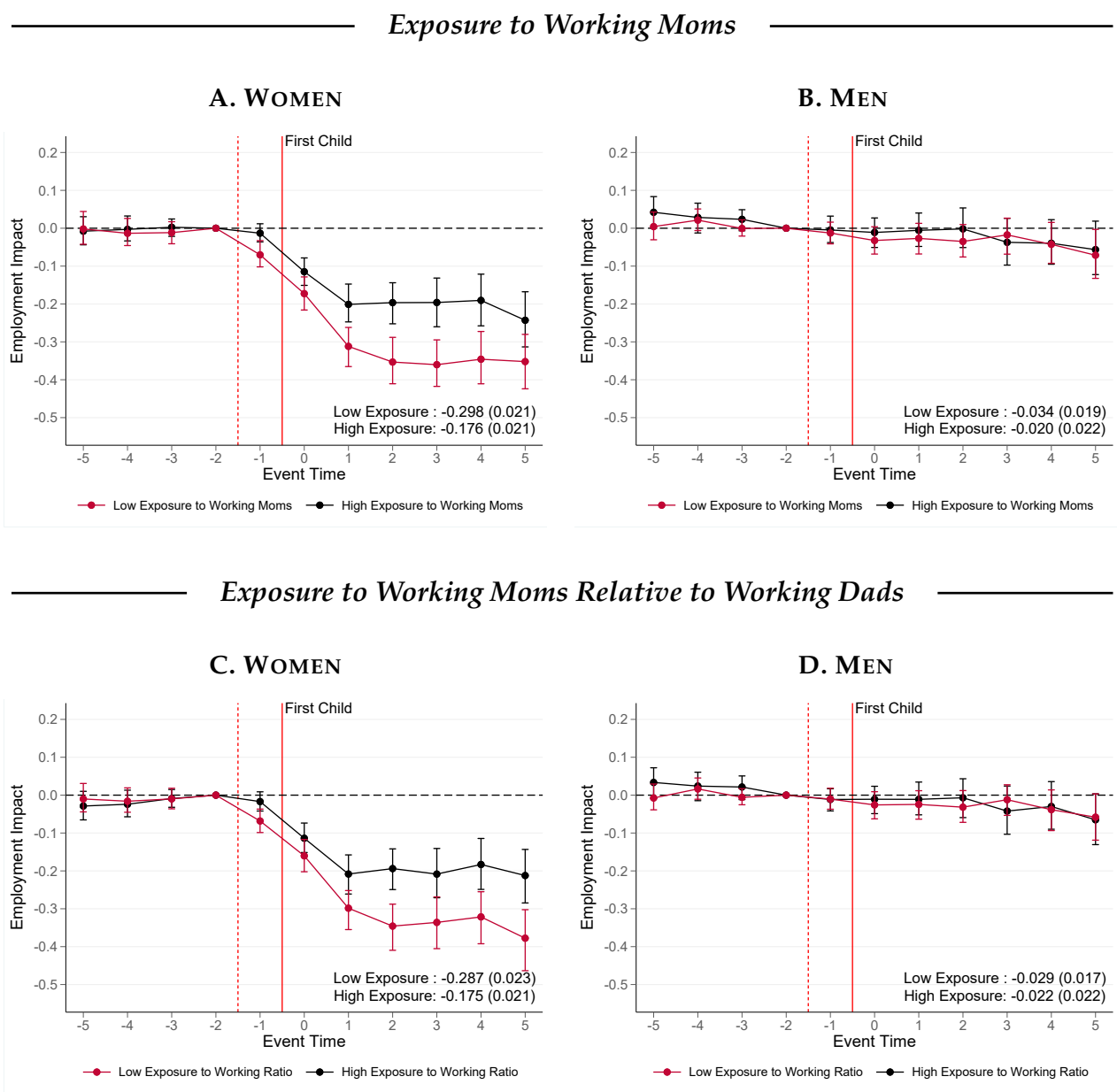
Notes: This figure presents the distributions of our two alternative exposure measures. The top panel shows the share of working mothers among classroom peers, while the bottom panel shows the share of working mothers relative to the share of working fathers among classroom peers. The vertical lines mark the cutoff points for the bottom and top tertiles.

FIGURE 2: EVENT STUDY OF FIRST CHILDBIRTH IN THE FULL SAMPLE



Notes: This figure presents event studies of first childbirth in the full sample of men (black series) and women (red series) observed between five years before and five years after childbirth. Each dot gives the percentage effect on employment in event year τ (relative to the omitted base year $\tau = -2$), estimated based on equation (4). The year of pregnancy is marked by the vertical dashed line, and the year of childbirth is marked by the vertical solid line. The figure also reports an estimate of the average child penalty—the effect on women relative to men across event times 0-5. Standard errors are bootstrapped with 500 repetitions.

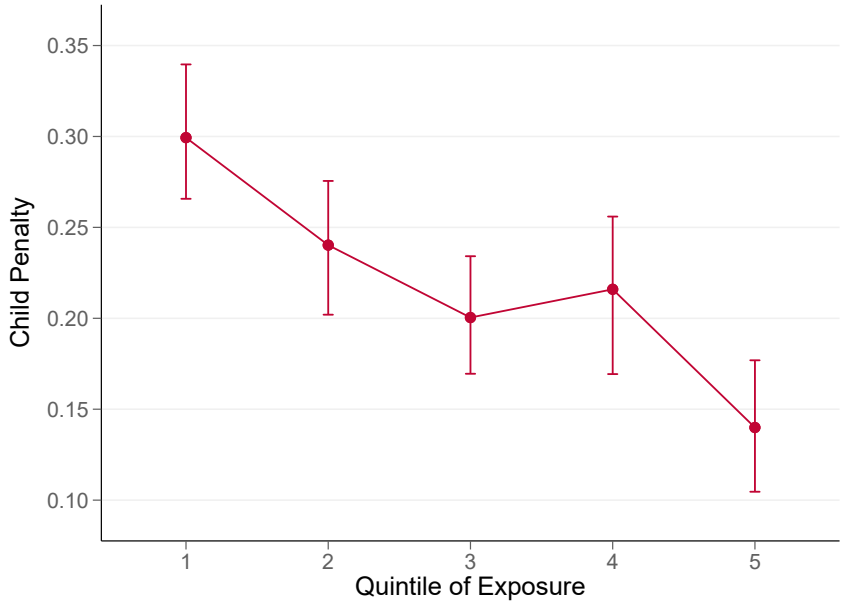
FIGURE 3: EVENT STUDIES OF FIRST CHILDBIRTH BY CLASSROOM EXPOSURE TO WORKING MOTHERS



Notes: This figure presents event studies of first childbirth for women (left panels) and men (right panels) by exposure to working mothers during middle and high school. The top panels measure exposure as the share of working mothers among classroom peers, while the bottom panels measure exposure as the share of working mothers relative to the share of working fathers among classroom peers. The event studies are estimated using specification (1), dividing the distribution of exposure into tertiles (low, medium, and high). Each dot gives the percentage effect on employment in event year τ (relative to the omitted base year $\tau = -2$) in a given exposure tertile. The year of pregnancy is marked by the vertical dashed line, and the year of childbirth is marked by the vertical solid line. Each panel reports estimates of the average employment effect of childbirth (over event times 0-5) in the low- and high-exposure groups. We find strong exposure effects for women (with higher exposure resulting in smaller employment drops) and no exposure effects for men. Standard errors are bootstrapped with 500 repetitions.

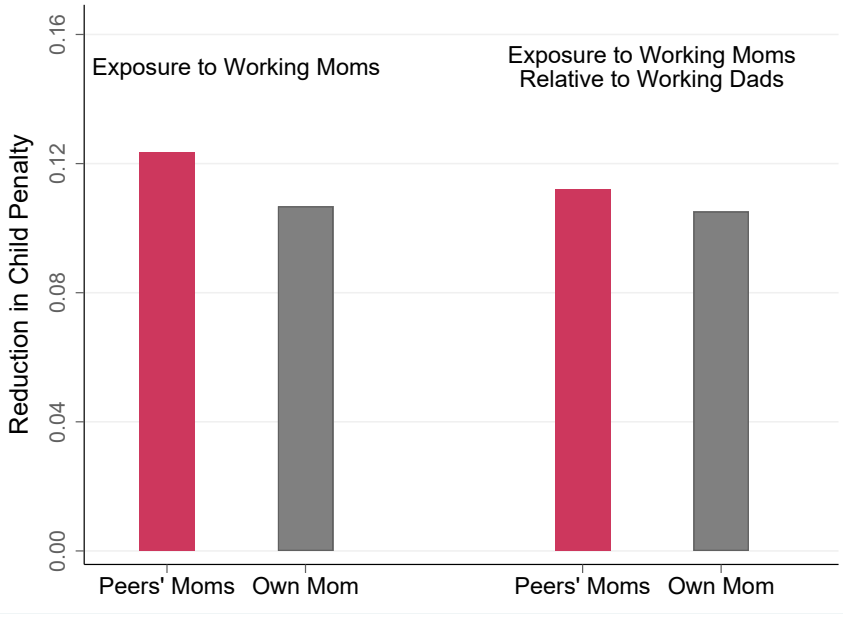
FIGURE 4: CHILD PENALTY BY QUINTILES OF CLASSROOM EXPOSURE

SHARE OF WORKING MOMS RELATIVE TO SHARE OF WORKING DADS



Notes: This figure presents estimates of child penalties in employment by quintiles of classroom exposure to working mothers. The estimates are based on a modified version of equation (1) in which event-time indicators are interacted with exposure quintiles rather than tertiles. We focus on the relative exposure measure—the share of working mothers relative to working fathers among classroom peers. The figure shows a strong and nearly monotonic decline in child penalties as exposure increases: moving from the bottom to the top quintile reduces the child penalty by about 15 percentage points. Standard errors are bootstrapped with 500 repetitions.

FIGURE 5: EFFECTS OF PEERS' MOTHERS VS OWN MOTHER ON CHILD PENALTIES



Notes: This figure compares the effect of peers' mothers to the effect of own mothers. The estimates are based on an augmented version of equation (1) in which the event-time×exposure indicators are interacted with an indicator for whether the individual's own mother worked during middle and high school. The effect of peers' mothers is obtained by comparing coefficients in the bottom and top tertiles of exposure, averaging across individuals with and without a working mother. The effect of own mother is obtained from the coefficient on own maternal employment, averaging across different exposure tertiles. Results are shown for each of our two exposure measures: the share of working mothers (left) and the share of working mothers relative to working fathers (right). The effects of peers' mothers and own mother are similar in magnitude. Their difference is about 1-2 percentage points and statistically insignificant.

Online Appendix:

"Child Penalties and Parental Role Models: Classroom Exposure Effects"

Henrik Kleven

Giulia Olivero

Eleonora Patacchini

May 2026

A Supplementary Exhibits

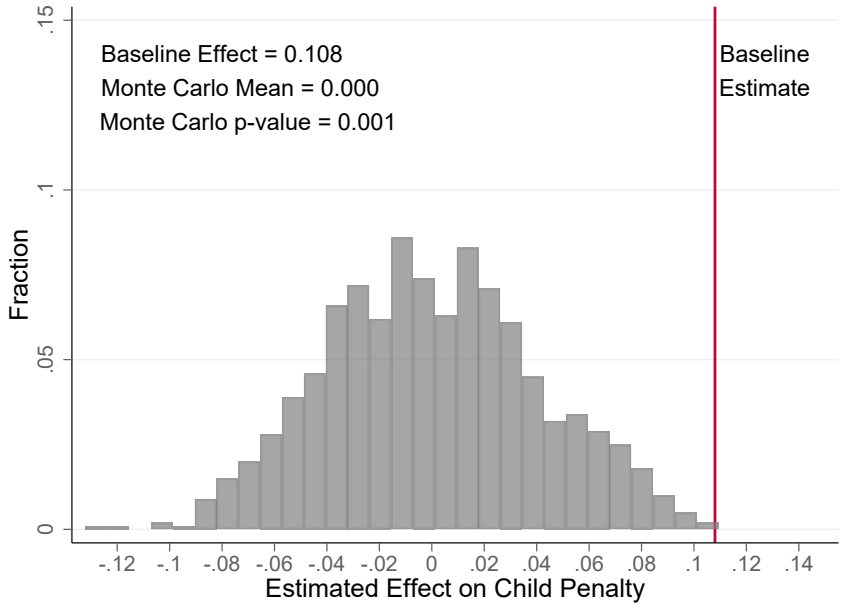
TABLE A.1: BALANCE TESTS BY EXPOSURE TO WORKING MOTHERS RELATIVE TO WORKING FATHERS

	Girls' Exposure to Working Moms Relative to Working Dads			Boys' Exposure to Working Moms Relative to Working Dads		
	Low	**High**	**Δ**	**Low**	**High**	**Δ**
College-Educated Parent	0.359	0.380	**0.021**	0.353	0.393	**0.040**
	(0.059)	(0.069)	**(0.034)**	(0.084)	(0.079)	**(0.044)**
Two-Parent Household	0.702	0.703	**0.001**	0.697	0.766	**0.069**
	(0.050)	(0.061)	**(0.037)**	(0.058)	(0.080)	**(0.052)**
US-Born Parent	0.925	0.936	**0.010**	0.901	0.937	**0.036**
	(0.026)	(0.027)	**(0.026)**	(0.045)	(0.047)	**(0.018)**
Own Mom Works	0.814	0.809	**-0.005**	0.702	0.790	**0.088**
	(0.047)	(0.055)	**(0.038)**	(0.070)	(0.097)	**(0.047)**
Own Dad Works	0.939	0.908	**-0.031**	0.927	0.927	**0.000**
	(0.030)	(0.039)	**(0.024)**	(0.039)	(0.058)	**(0.039)**
Parental Income	22,126	23,160	**1,035**	21,935	22,734	**0,799**
	(1,994)	(2,481)	**(1,404)**	(2,263)	(2,589)	**(1,499)**
Number of Siblings	1.498	1.453	**-0.045**	1.486	1.451	**-0.035**
	(0.127)	(0.164)	**(0.110)**	(0.134)	(0.198)	**(0.136)**
White	0.640	0.640	**0.001**	0.561	0.633	**0.072**
	(0.046)	(0.051)	**(0.032)**	(0.047)	(0.059)	**(0.038)**
Black	0.191	0.198	**0.007**	0.215	0.204	**-0.011**
	(0.043)	(0.048)	**(0.028)**	(0.025)	(0.039)	**(0.030)**
Hispanic	0.097	0.074	**-0.022**	0.119	0.124	**0.006**
	(0.021)	(0.027)	**(0.023)**	(0.024)	(0.030)	**(0.019)**
School FE	X	X	X	X	X	X
Cohort FE	X	X	X	X	X	X
No. Observations	726	763	1489	584	554	1138

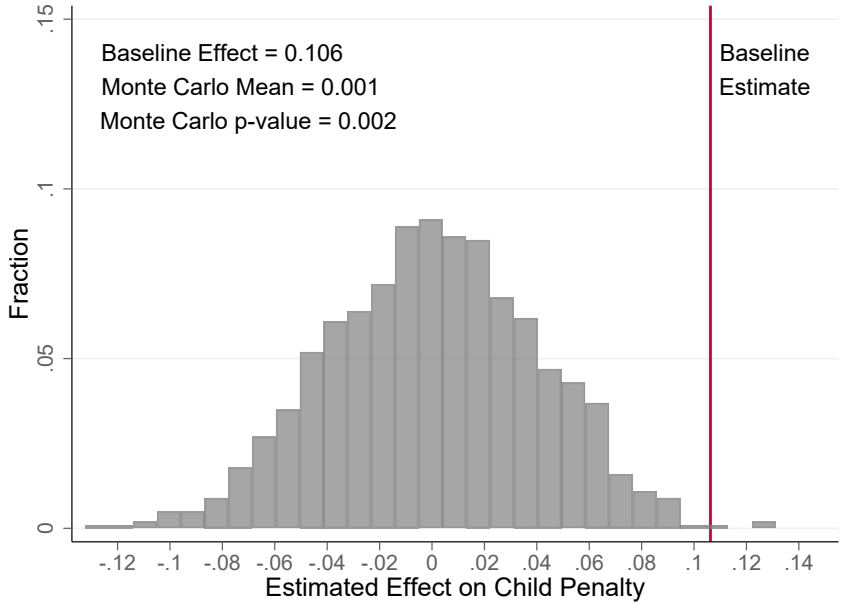
Notes: This table presents balance tests for girls (left) and boys (right) with respect to our alternative peer exposure variable: the share of working moms relative to working dads in the classroom. The table provides results from regressions of family background variables on tertiles of peer exposure (low, medium, and high). The following dependent variables are considered: having a college-educated parent, living in a two-parent household, having a US-born parent, having a working mother, having a working father, parental income (per child), number of siblings, and race/ethnicity. Each regression controls for school and cohort fixed effects. The coefficients shown are the predicted values in the low and the high tertiles, as well as their difference. One coefficient (US-born parent) is significant at the 5% significance level for boys which is within the range expected by chance given the number of variables tested and does not indicate systematic imbalance. This is especially inconsequential given that we find no effects of exposure on boys' employment. Standard errors are clustered at the school level.

FIGURE A.1: MONTE CARLO SIMULATION

A. Share of Working Moms

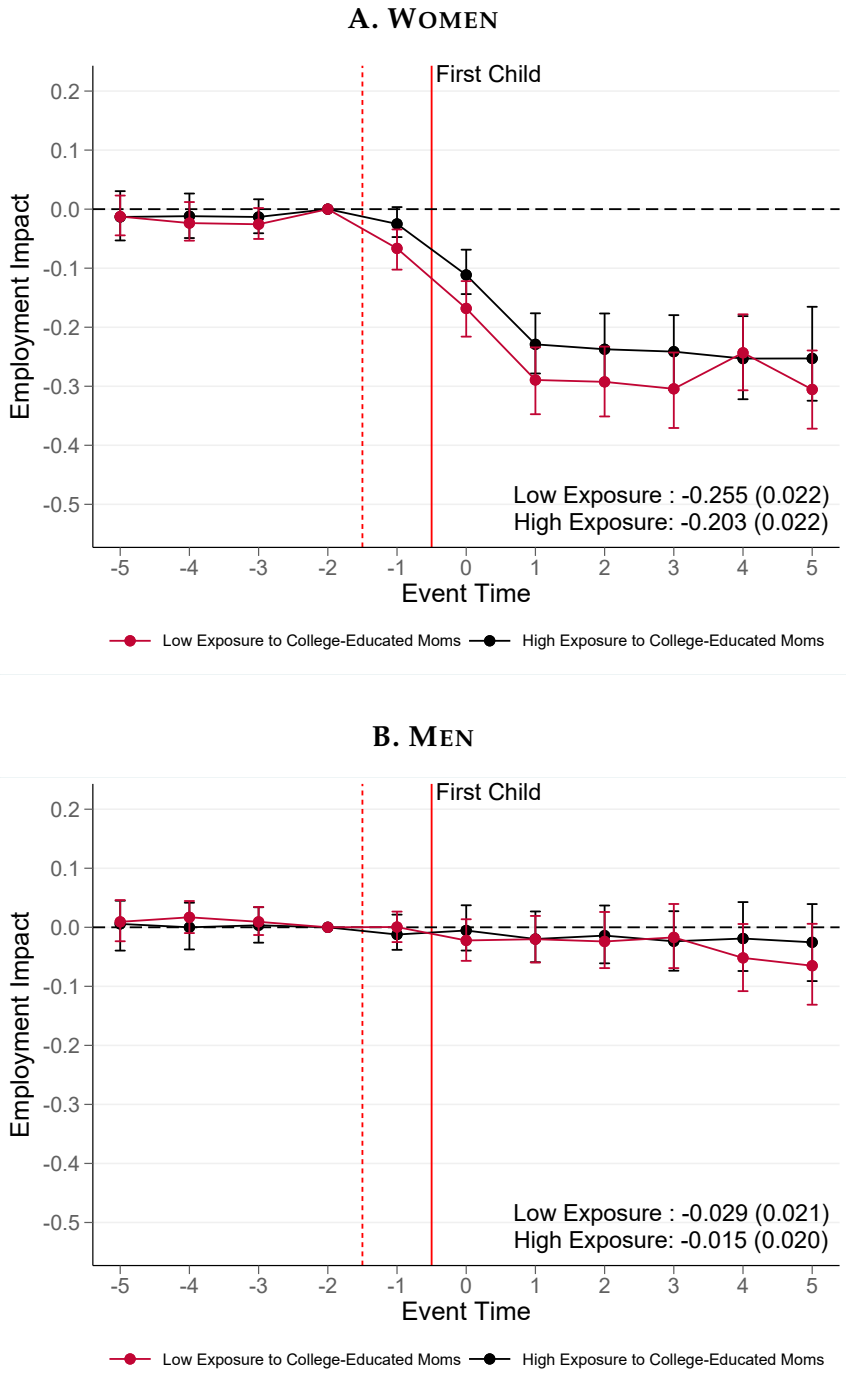


B. Share of Working Moms Relative to Working Dads



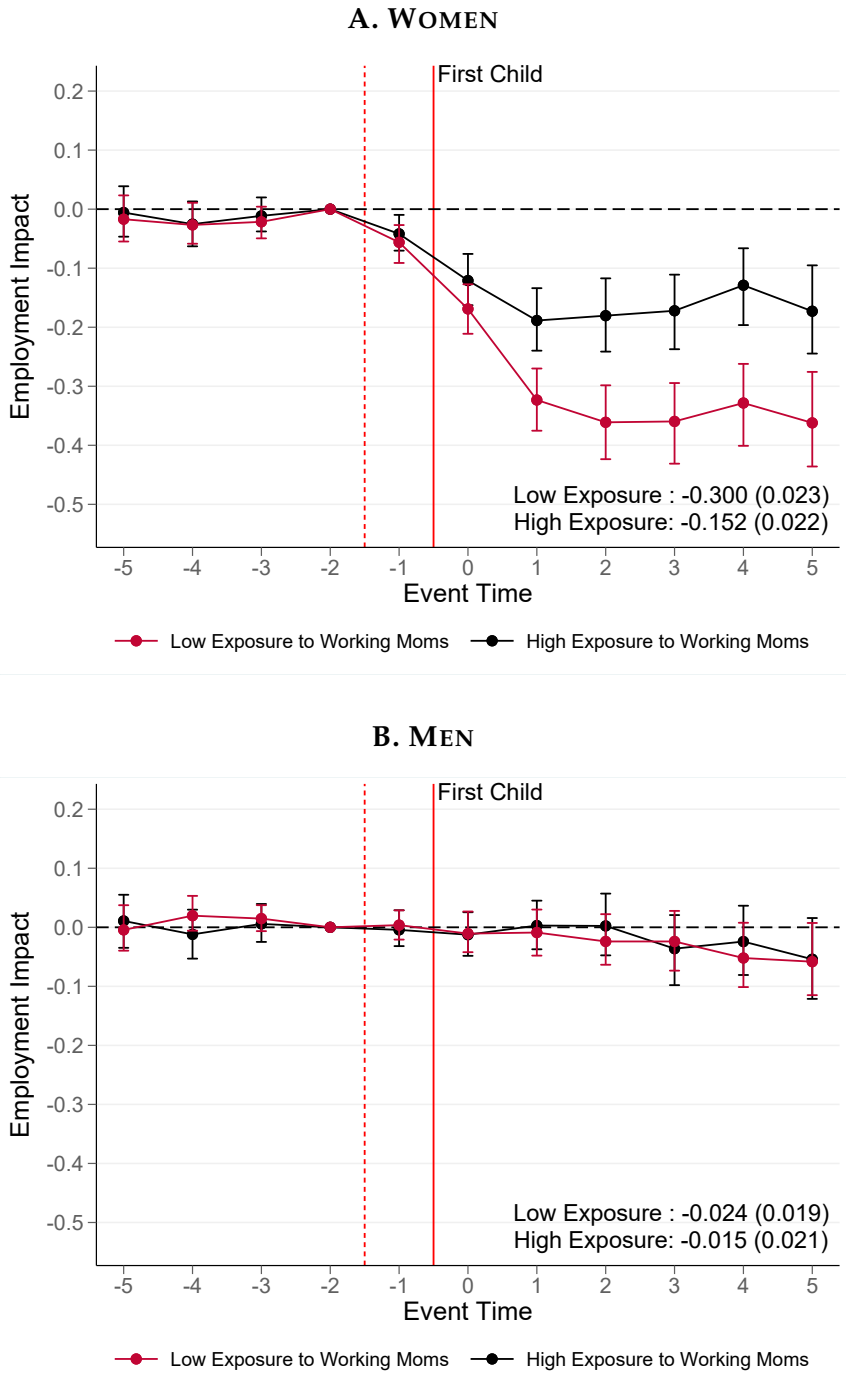
Notes: This figure presents results from a Monte Carlo simulation in which we randomly reassign students to different school×cohort cells. After reassigning students, we estimate the effect of classroom exposure to working mothers using our baseline specification (1) for each of our two exposure measures. The figure shows the resulting distributions of placebo estimates based on 1,000 draws, with our baseline estimates demarcated by vertical lines. The placebo estimates are symmetrically distributed around zero: their means are 0.000 and 0.001, respectively. The baseline estimates obtained from actual classroom assignment are extreme outliers. The implied p-values—the share of placebo estimates that are at least as large as the actual estimate—equal 0.001 and 0.002, respectively.

FIGURE A.2: EVENT STUDIES OF FIRST CHILDBIRTH BY CLASSROOM EXPOSURE TO COLLEGE-EDUCATED MOTHERS



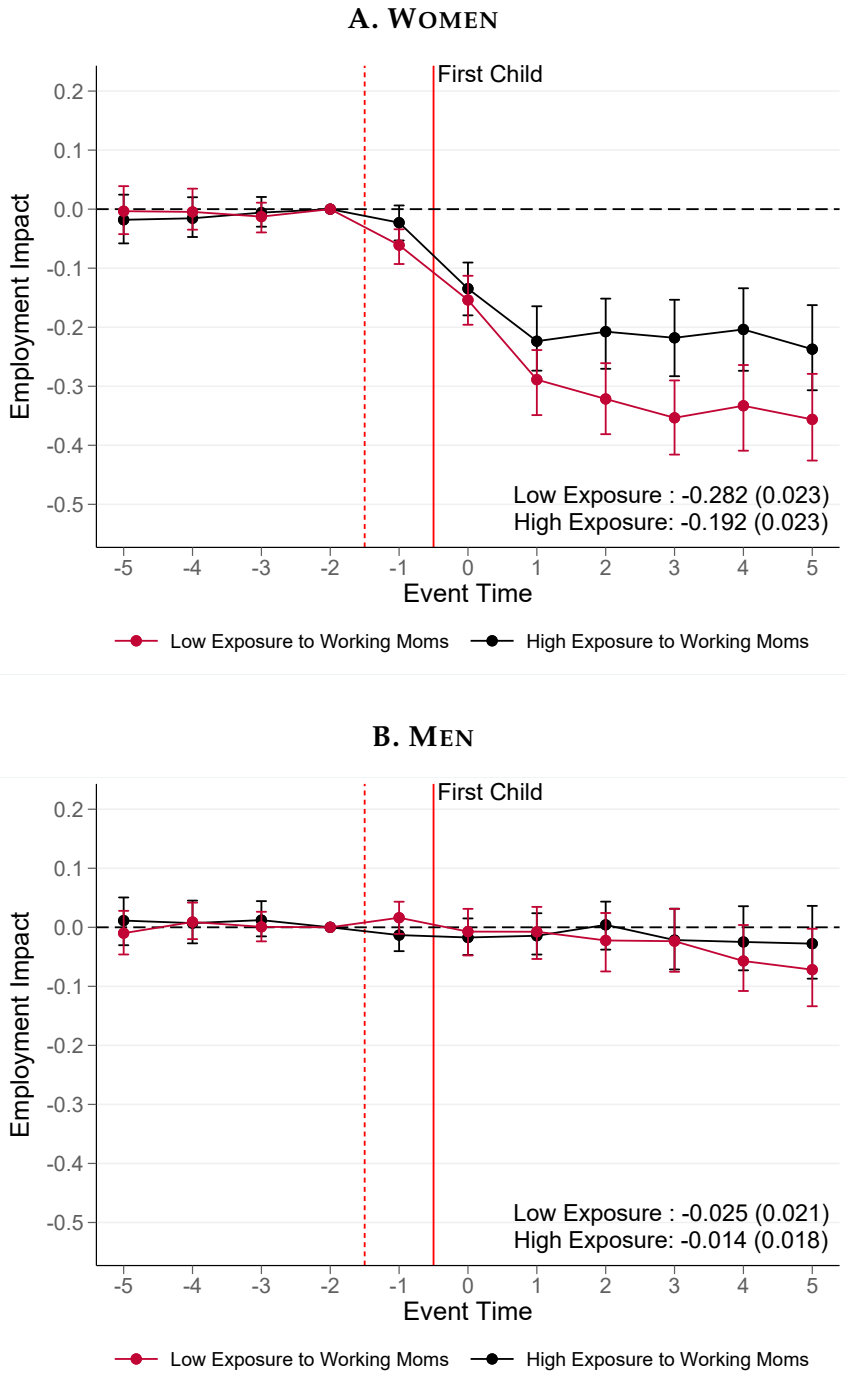
Notes: This figure presents event studies of first childbirth for women (top panel) and men (bottom panel) by exposure to college-educated mothers during middle and high school. The event studies are estimated using specification (1), dividing the distribution of exposure into tertiles (low, medium, and high). Each dot gives the percentage effect on employment in event year τ (relative to the omitted base year $\tau = -2$) in a given exposure tertile. The year of pregnancy is marked by the vertical dashed line, and the year of childbirth is marked by the vertical solid line. Each panel reports estimates of the average employment effect of childbirth (over event times 0-5) in the low- and high-exposure groups. We find no significant effects of exposure to college-educated mothers on either women or men. Standard errors are bootstrapped with 500 repetitions.

FIGURE A.3: EVENT STUDIES OF FIRST CHILDBIRTH BY CLASSROOM EXPOSURE TO WORKING MOTHERS OF SAME-RACE PEERS



Notes: This figure presents event studies of first childbirth for women (top panel) and men (bottom panel) by exposure to working mothers among *same-race* peers in middle and high school. The event studies are estimated using specification (1), dividing the distribution of exposure into tertiles (low, medium, and high). Each dot gives the percentage effect on employment in event year τ (relative to the omitted base year $\tau = -2$) in a given exposure tertile. The year of pregnancy is marked by the vertical dashed line, and the year of childbirth is marked by the vertical solid line. Each panel reports estimates of the average employment effect of childbirth (over event times 0-5) in the low- and high-exposure groups. We find even stronger exposure effects for women when focusing on same-race peers and still no exposure effects for men. Standard errors are bootstrapped with 500 repetitions.

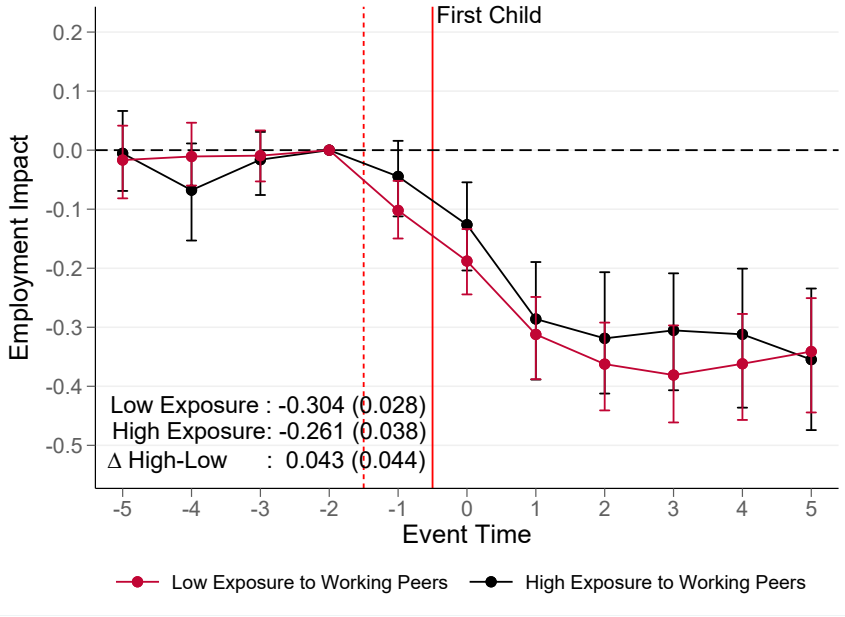
FIGURE A.4: EVENT STUDIES OF FIRST CHILDBIRTH BY CLASSROOM EXPOSURE TO WORKING MOTHERS OF SAME-GENDER PEERS



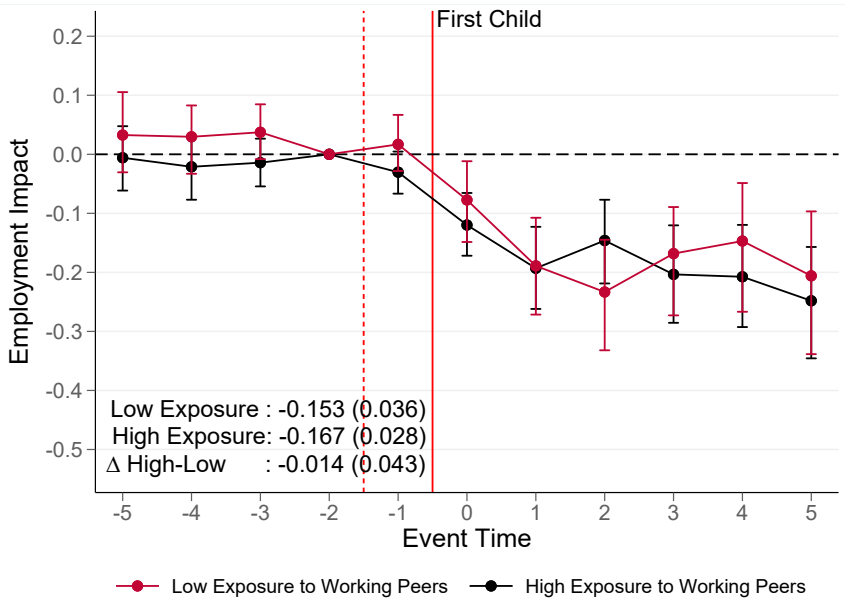
Notes: This figure presents event studies of first childbirth for women (top panel) and men (bottom panel) by exposure to working mothers among *same-gender* peers in middle and high school. The event studies are estimated using specification (1), dividing the distribution of exposure into tertiles (low, medium, and high). Each dot gives the percentage effect on employment in event year τ (relative to the omitted base year $\tau = -2$) in a given exposure tertile. The year of pregnancy is marked by the vertical dashed line, and the year of childbirth is marked by the vertical solid line. Each panel reports estimates of the average employment effect of childbirth (over event times 0-5) in the low- and high-exposure groups. Restricting attention to same-gender peers does not increase the exposure effects on women, unlike the results for race. Standard errors are bootstrapped with 500 repetitions.

FIGURE A.5: EVENT STUDIES OF FIRST CHILDBIRTH BY CLASSROOM EXPOSURE TO WORKING MOTHERS VS WORKING PEERS

A. Women with Low Exposure to Working Moms, by Exposure to Working Peers



B. Women with High Exposure to Working Moms, by Exposure to Working Peers



Notes: This figure presents event studies of first childbirth for women with low adolescent exposure to working mothers (Panel A) and high adolescent exposure to working mothers (Panel B) by contemporaneous exposure to working female peers. The event studies are estimated using an augmented version of equation (1), interacting each tertile of adolescent exposure to working mothers by an indicator for below- vs above-median contemporaneous exposure to working female peers. Each dot gives the percentage effect on employment in event year τ (relative to the omitted base year $\tau = -2$) in a given exposure bin. The year of pregnancy is marked by the vertical dashed line, and the year of childbirth is marked by the vertical solid line. Each panel reports estimates of the average employment effect of childbirth (over event times 0-5) on women with low and high exposure to working peers. Standard errors are bootstrapped with 500 repetitions.