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 parents across cohorts within schools, we find that greater exposure to working mothers during adolescence substantially reduces the child penalty in employment later in life. Conversely, we find that greater exposure to working fathers increases the penalty. Our findings suggest that parental role models during adolescence are critical for shaping child-related gender gaps in the labor market.

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I Introduction

A recent literature shows that the remaining gender inequality in labor market outcomes can be attributed largely to child penalties—the negative effects of parenthood on women relative to men (Kleven et al. 2019a,b; 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 find strong effects of the culture in which girls grow up on their child penalties later in life (Boelmann et al. 2025; Kleven 2025).

While the prior evidence on social norms provides an important starting point, it does not reveal the specific channels through which childhood environment shapes preferences and ultimately child penalties. This limits our understanding of why gender inequality changes over time and how we might influence it through public policy. We contribute to this question by investigating if exposure to different parental role models during adolescence influences child penalties in adulthood. Our hypothesis is that adolescent girls who are socialized in an environment where most mothers work are more likely to develop a gender-role ideal that reconciles career and motherhood (Olivetti et al. 2020). These ideals and expectations, in turn, are likely to shape adult choices and outcomes.

To study this question, we leverage rich longitudinal data on US teenagers, linking them to their middle- and high-school peers and following them into adulthood as they eventually become parents. The data include labor market information on the parents of their classmates, allowing us to investigate if the family role models of their peers shape outcomes later in life. To overcome identification challenges, we exploit idiosyncratic variation in the employment outcomes of peers' parents across different cohorts of students within schools. This approach was first proposed by Hoxby (2000) to estimate the impact of classmates' gender and race, and has been widely used to study peer effects in education and labor market outcomes (Hoxby 2000; Hanushek et al. 2002; Angrist and Lang 2004; Friesen and Krauth 2007; Lavy and Schlosser 2011; Lavy et al. 2012; Olivetti et al.

2020). As far as we are aware, it has never been used to study child penalties, presumably because of the empirical challenges involved. Doing so requires data that allow for credibly estimating child penalties *and* linking those child penalty estimates to exogenous variation in childhood peer composition.

As in the existing literature, we estimate child penalties based on event studies of first child birth (Kleven et al. 2019b). We augment the standard approach to allow for heterogeneous treatment effects by exposure to working mothers (or fathers) among middle- and high-school peers. Our preferred specification interacts the event time dummies with quantile of exposure, controlling for school and cohort fixed effects. We provide balance tests showing that, conditional on the fixed effects, there are no statistically significant differences in the pre-child characteristics of individuals with high and low exposure to working parents.

We find that girls in the top tertile of exposure to working mothers have smaller child penalties in adulthood than girls in the bottom tertile. The effect is large and precisely estimated: the difference in employment penalties between the top and bottom tertiles is almost 11 percentage points. The effect is even larger when focusing on exposure to working mothers of same-race peers, a difference of almost 15pp between the top and bottom tertiles.[1] We also investigate the effect of exposure to working fathers. This effect goes in the opposite direction, with girls in the top tertile of exposure having larger child penalties than girls in the bottom tertile. The negative effect of working fathers on girls is weaker than the positive effect of working mothers (8pp vs 11pp) and less precisely estimated (p-value of 0.0596), making these results more suggestive. Overall, we interpret our findings as showing that gendered parental role models are intergenerationally transmitted and have strong effects on child penalties: adolescent girls respond positively to being exposed to working mothers and negatively to being exposed to working fathers.

As in any study of peer effects, isolating the exact mechanism(s) behind the reduced-

[1] A natural question is whether the effect of exposure to working mothers is driven only by employment status or if part of the effect is due to other characteristics correlated with employment. One such candidate is education: working mothers tend to be more educated than non-working mothers and this could be part of the role model package that affects adolescent girls. To address this question, we run the same specification focusing instead on the effect of exposure to *college-educated* mothers. We find no effect of such exposure on child penalties.

form effects is challenging. We are able to make some progress on this front by leveraging the richness of our data. Several pieces of evidence support our role model interpretation of the data: that the effects are not mediated by parental education, that the effects grow stronger with increased employment exposure, and that the effects cannot be explained by classmates' own employment choices in adulthood. These findings suggest that the effects come from the parents, not the classmates, and that employment rather than education is the key role model trait that shapes child penalties.

Our paper contributes to a burgeoning literature studying child penalties and their determinants (e.g., Kleven et al. 2019a,b, 2021, 2024b; Andresen and Nix 2022; Cortés and Pan 2023). By estimating peer effects on child penalties in employment, the paper also contributes to research on the effects of gender norms on female labor supply (e.g., Fernández et al. 2004; Fortin 2005; Fernández and Fogli 2009; Blau et al. 2011; Bertrand 2020; Olivetti et al. 2020). Our findings relate most directly to Boelmann et al. (2025) and Kleven (2025), who document strong relationships between child penalties for movers/migrants and the child penalty culture in place of birth. We add to this literature by leveraging quasi-random variation in exposure to gender norms within narrowly-defined places, namely schools. Our paper indirectly challenges the widespread notion that social norms are sticky and slow-moving, showing that very local variation in parental role models can have large long-term consequences for child penalties and gender gaps. Finally, we provide novel insights on the heterogeneous effects of male and female role models on child penalties. Our findings suggest that both role models are important, with working fathers and working mothers pulling in the opposite direction.

II Data and Methodology

II.A 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.[2] 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 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.[3] 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$\times$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 child birth. 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

[2]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.

[3]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.

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.

II.B Event Study Framework

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

Specifically, we estimate the following event study regression 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 is used to denote vectors. The first term includes dummies for each event time τ, omitting a base year before child birth. The omitted base year is $\tau = -2$, the year before pregnancy. The event time dummies are interacted with indicators for quantile of exposure to working mothers (i.e., the fraction of classroom peers with working mothers). We divide the distribution of classroom exposure into tertiles (low, medium, and high); finer granularity is possible but at a cost of statistical precision. The event time coefficients $\beta_{e\tau}^{g} \in \boldsymbol{\beta_e^g}$ measure the impact of child birth 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 the individuals, their parents, and their peers' parents. We also consider specifications with individual fixed effects. As we shall see, the estimates are very robust to these specification choices.

Estimating equation (1) gives the effects of child birth 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 present effects in percentage terms, the effects in absolute terms (percentage points) are qualitatively very similar.

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

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

A positive child penalty implies that parenthood increases the gender gap. To estimate the effect of classroom exposure to working mothers on child penalties, 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*.

II.C Balance Tests

To validate the empirical design, Table 1 provides an array of balance tests. The table presents results from regressions of family background variables on our peer exposure measure: dummies for having a low, medium, or high share of classroom peers with

working mothers or working fathers, respectively.[4] We show results for girls—our main sample of interest—while corresponding results for boys are provided in Appendix Table A.1. The following dependent variables are considered: having a college-educated parent, living in a two-parent household, having a US-born parent, parental income per child, number of siblings, and race/ethnicity (white, black, and Hispanic). Each regression controls for school and cohort fixed effects. The estimated coefficients represent predicted values in the low and the high tertiles, as well as their difference.[5]

The balance tests provide strong support for our empirical design. Among the sixteen differences shown in the table, none are statistically significant at the 10% level. This mitigates concerns that students with different peer exposure are selected on characteristics that impact child penalties. While we cannot rule out selection on unobservables, the absence of any selection on observables makes it much less likely that unobservable confounders create bias (see e.g., Altonji et al. 2005).

III Empirical Findings

III.A Main Results

To establish a benchmark and to see if our sample is different from those considered in the existing literature, we start by estimating the average child penalty in the full sample. The results are presented in Figure 1. The figure provides event studies of first child birth for men (black series) and women (red series) between five years before and five years after child birth. Each dot gives the percentage impact on employment in event year τ (relative to the omitted base year $\tau = -2$), estimated based on the specification in Kleven et al. (2019b).[6] The year of pregnancy is marked by a vertical dashed line and the year of child birth is marked by a vertical solid line. The figure shows the average child penalty over

[4] The average difference in the fraction of peers with working mothers (fathers) between the bottom and top tertiles is equal to 0.15 (0.08).

[5] Specifically, the coefficients are the predicted values (in the low and high 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.

[6] This is a simplified version of equation (1) where we do not interact the event time dummies with quantile of classroom exposure, and where we drop the fixed effects for quantile of exposure (γ_e), school

event times 0-5 implied by the event studies.

As in prior studies, our sample features parallel trends between men and women before pregnancy, marginal divergence during pregnancy, and sharp divergence after child birth. Having a child is a non-event for men, while it leads to an immediate and persistent drop in the employment rate for women. The resulting child penalty equals 22.4% and is precisely estimated. Interestingly, this estimate is virtually identical to the child penalty in employment estimated by Kleven (2025) using PSID and NLSY data. He finds a child penalty in annual employment (closest to our employment outcome) equal to 22%. 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. Figure 2 presents event studies of first child birth based on specification (1). It shows the employment impact of parenthood on women (Panel A) and men (Panel B), splitting each gender by tertile of exposure to working mothers during middle- and high-school. 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-exposure and high-exposure groups feature parallel trends before pregnancy/birth and sharp divergence immediately after. However, while the patterns are qualitatively similar, the magnitudes vary strongly. The average employment drop due to motherhood equals 29.8% in the low-exposure group and 17.6% 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. Conversely, 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.

Are women influenced only by exposure to working mothers or do fathers also play a

(δ_s), and cohort (ζ_c). The event study regression is specified as follows

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

corresponding to the specification proposed by Kleven et al. (2019b) and used in many studies. We present percentage effects calculated 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 child birth.

role? Figure 3 repeats the analysis focusing on the effect of exposure to working fathers. Here we find exposure effects going in the opposite direction, although the estimates are statistically weaker than in the previous figure.[7] Women with high exposure to working fathers during middle- and high-school experience larger employment drops after child birth than women with low exposure. The average employment drop equals 26.3% in the high-exposure group and 21.8% in the low-exposure group, corresponding to an exposure effect of 4.5pp. This is consistent with a story in which adolescent girls, when faced with greater peer exposure to working fathers, respond by following a more traditional family model with larger child penalties in their adulthood.

Table 2 summarizes the visual results presented above and provides robustness checks. Panel A considers our baseline specification: the first two rows show the effects of children on women and men, respectively, by exposure to working moms and dads—the estimates shown in the preceding figures—while the third row shows the implied child penalty by exposure to working moms and dads. For the child penalty estimates, we show standard errors in parentheses and p-values in brackets. Greater exposure to working moms during adolescence reduces the employment penalty by 10.8pp (with a p-value of 0.0056), while greater exposure to working dads increases the employment penalty by 8.1pp (with a p-value of 0.0596). This suggests that the labor market choices of mothers are heavily influenced by preferences shaped by the parental role models observed during adolescence. If these role models feature a greater share of working mothers ("modern families"), their child penalties are smaller. Conversely, if these role models feature a greater share of working fathers ("traditional families"), their child penalties are larger.

Panel B evaluates the robustness of our estimates to alternative specifications. The first row adds controls for the demographic characteristics of the individual, the second row adds controls for the demographic characteristics of their parents, while the third row adds controls for the demographic characteristics of their peers' parents.[8] The final

[7] This is unsurprising when considering that exposure to working fathers features less variation in the data than exposure to working mothers (see section II.C). Comparing the top and bottom tertiles, the mean difference in the fraction of peers with working fathers is about half as large as the difference in the fraction of peers with working mothers.

[8] The control variables are specified as follows. Individual controls: own education (college degree or not), own marital status, and own race. Parent controls: parents' employment, parents' education (college degree or not), parents' marital status, and parents' race. Peer-parent controls: fraction of peers with at

row considers a specification with individual fixed effects.[9] Our results are extremely robust: the point estimates hardly move across specifications, and if anything, adding controls improves statistical precision. For example, the effect of exposure to working fathers on child penalties—marginally insignificant in the baseline specification—is statistically significant when controlling for the demographic characteristics of the parents (with a p-value of 0.0407). It is also reassuring that the estimates are virtually identical when allowing for individual fixed effects.

III.B Mechanisms

To shed light on the underlying mechanisms, we focus on exposure to working mothers and investigate whether the observed effects are truly driven by maternal employment or by other correlated factors. The most obvious candidate is education: the share of peers with working mothers is correlated with maternal education levels among peers. One possibility, therefore, is that exposure to better educated mothers shapes career preferences later in life and ultimately drives our findings. To test for this possibility, Appendix Figure A.1 presents event studies of first child birth by exposure to college-educated mothers.[10] As the figure shows, the event studies are virtually identical across the two exposure groups, for both women and men. This lends support to our interpretation of the data: child penalties in employment are shaped by the gendered employment patterns among the mothers of adolescent peers, not by their education levels.

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

least one college-educated parent, fraction of peers with married parents, fractions from each race, and the fraction of peers with working parents of the "other gender" (working fathers when estimating the effect of exposure to working mothers, and vice versa).

[9] In a sample restricted to individuals who have children, it is not possible to separately identify individual fixed effects, year fixed effects, and event-time coefficients. The reason is that, conditional on individual and year, there is no variation in event time (Miller 2023). 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] While education and employment are strongly correlated at the individual level, the correlation between the share of classmates with working mothers and the share with college-educated mothers is relatively moderate in our data (a correlation coefficient of 0.57). This leaves sufficient independent variation to empirically distinguish the effect of exposure to working mothers from the effect of exposure to highly-educated mothers.

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—students 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 et al. 2001), making this a natural candidate to consider. Appendix Figure A.2 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.[11]

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.[12] We then estimate event studies of first child birth 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 Figure A.4 of the appendix. Conditional on adolescent exposure to working mothers, we find similar effects of child birth 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 strong support for our interpretation of the

[11] 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.3, restricting attention to same-gender peers does not yield a larger exposure effect.

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

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 key 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.

IV Conclusion

Recent research finds large child penalties in female labor market outcomes, showing that these penalties explain most of the remaining gender inequality in high-income countries (Kleven et al. 2019a,b, 2024a). In this paper, we investigate the underlying determinants of child penalties, focusing on the role of gender norms experienced during middle- and high-school. Using national longitudinal data on US adolescents, followed from their school years and into adulthood, we leverage quasi-random variation in the fraction of peers (across student cohorts within schools) with working mothers or fathers. This type of research design, initially proposed by Hoxby (2000), has been widely used to study peer effects, but not in the context of child penalties. Estimating child penalties based on event studies of first child birth, we show that greater exposure to working mothers during adolescence reduces the child penalty, while greater exposure to working fathers increases the child penalty. The effects are sizable and robust to specification. We provide evidence that the effects are in fact mediated by the *work* behavior of peers' parents, and not by correlated variables such as education levels.

We interpret our findings as showing that parental role models during adolescence have economically meaningful effects on child penalties. Adolescent girls socialized in an environment with a greater share of working mothers are more likely to develop a gender-role ideal that reconciles motherhood and career. In contrast, adolescent girls socialized in an environment with a greater share of working fathers are more likely to develop a

gender-role ideal conforming with the traditional homemaker-breadwinner model. Our findings are consistent with existing work on child penalties and gender norms, but offer additional empirical credibility and insights. Our measure of gender norms (based on peers within the same school and cohort) is more local and relevant than the state- and country-level proxies considered in recent epidemiological studies (Boelmann et al. 2025; Kleven 2025). Our approach provides a more nuanced understanding, allowing us to uncover the specific influence of parental role models during middle- and high-school. We provide novel insights on the effects of male vs female role models: their work behavior have opposing effects on child penalties later in life. Finally, our empirical design—leveraging granular variation in gender norms that is plausibly orthogonal to other determinants of child penalties—alleviates selection concerns that apply to other studies of gender norms and child penalties.

Our findings lend support to the idea that preference formation, social norms, and culture are crucial for shaping child penalties and therefore gender inequality. This suggests that public policies aimed at increasing maternal labor supply and reducing child penalties may be ineffective unless they are complemented by initiatives that shift societal preferences or enhance exposure to diverse role models. This may help explain recent findings that family policies such as parental leave schemes and childcare provision tend to have small effects on child penalties (see e.g., Kleven et al. 2024b).

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Table 1: Balance Tests for Girls

	Exposure to Working Moms			**Exposure to Working Dads**		
	Low	**High**	Δ	**Low**	**High**	Δ
College-Educated Parent	0.344	0.343	**-0.002**	0.440	0.412	**-0.027**
	(0.059)	(0.066)	**(0.043)**	(0.093)	(0.091)	**(0.049)**
Two-Parent Household	0.714	0.695	**-0.020**	0.923	0.950	**0.026**
	(0.051)	(0.057)	**(0.029)**	(0.027)	(0.025)	**(0.016)**
US-Born Parent	0.912	0.926	**0.014**	0.889	0.900	**0.011**
	(0.026)	(0.029)	**(0.015)**	(0.033)	(0.029)	**(0.025)**
Parental Income	23,344	20,992	**-2,352**	25,709	23,327	**-2,381**
	(1,987)	(2,405)	**(1,613)**	(3,071)	(3,029)	**(1,490)**
Number of Siblings	1.439	1.532	**0.093**	1.423	1.634	**0.211**
	(0.127)	(0.174)	**(0.128)**	(0.184)	(0.184)	**(0.118)**
White	0.642	0.618	**-0.025**	0.640	0.637	**-0.004**
	(0.044)	(0.049)	**(0.037)**	(0.059)	(0.053)	**(0.031)**
Black	0.197	0.195	**-0.002**	0.138	0.163	**0.025**
	(0.043)	(0.046)	**(0.029)**	(0.049)	(0.044)	**(0.022)**
Hispanic	0.091	0.107	**0.016**	0.076	0.098	**0.022**
	(0.021)	(0.037)	**(0.026)**	(0.036)	(0.033)	**(0.023)**
School FE	X	X	X	X	X	X
Cohort FE	X	X	X	X	X	X
No. Observations	733	792	1525	521	613	1134

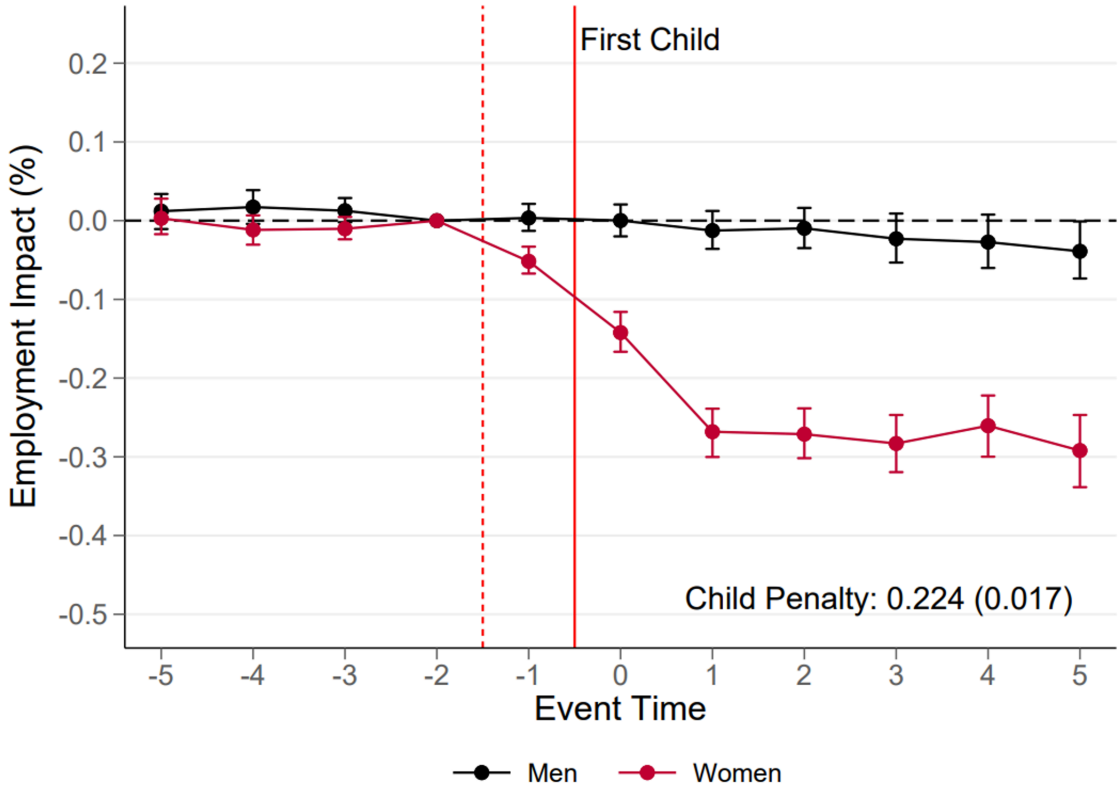
Notes: This table presents balance tests for girls with respect to our peer exposure variable: the fraction of classmates with working mothers (left) and working fathers (right), respectively. The table provides results from regressions of family background variables on tertiles of peer exposure (low, 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, 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 Dads		
	Low	**High**	**Δ**	**Low**	**High**	**Δ**
PANEL A: Baseline Specification						
Effect of Children on Women	-0.298	-0.176	**0.122**	-0.218	-0.263	**-0.046**
	(0.021)	(0.021)	(0.029)	(0.025)	(0.025)	(0.035)
Effect of Children on Men	-0.034	-0.020	**0.014**	-0.020	0.015	**0.035**
	(0.019)	(0.022)	(0.027)	(0.022)	(0.020)	(0.027)
Child Penalty (Men - Women)	0.265	0.157	**-0.108**	0.198	0.279	**0.081**
	(0.029)	(0.029)	(0.039)	(0.033)	(0.032)	(0.043)
			[0.0056]			[0.0596]
PANEL B: Child Penalties when Adding Controls						
Individual Demographics	0.261	0.150	**-0.111**	0.190	0.271	**0.081**
	(0.029)	(0.029)	(0.039)	(0.033)	(0.032)	(0.043)
			[0.0044]			[0.0596]
Parents Demographics	0.277	0.162	**-0.115**	0.194	0.282	**0.088**
	(0.029)	(0.029)	(0.039)	(0.033)	(0.032)	(0.043)
			[0.0032]			[0.0407]
Peer-Parents Demographics	0.264	0.155	**-0.109**	0.198	0.278	**0.080**
	(0.029)	(0.029)	(0.039)	(0.033)	(0.032)	(0.043)
			[0.0052]			[0.0628]
Individual Fixed Effects	0.285	0.171	**-0.114**	0.195	0.276	**0.080**
	(0.026)	(0.027)	(0.037)	(0.032)	(0.030)	(0.043)
			[0.0021]			[0.0628]

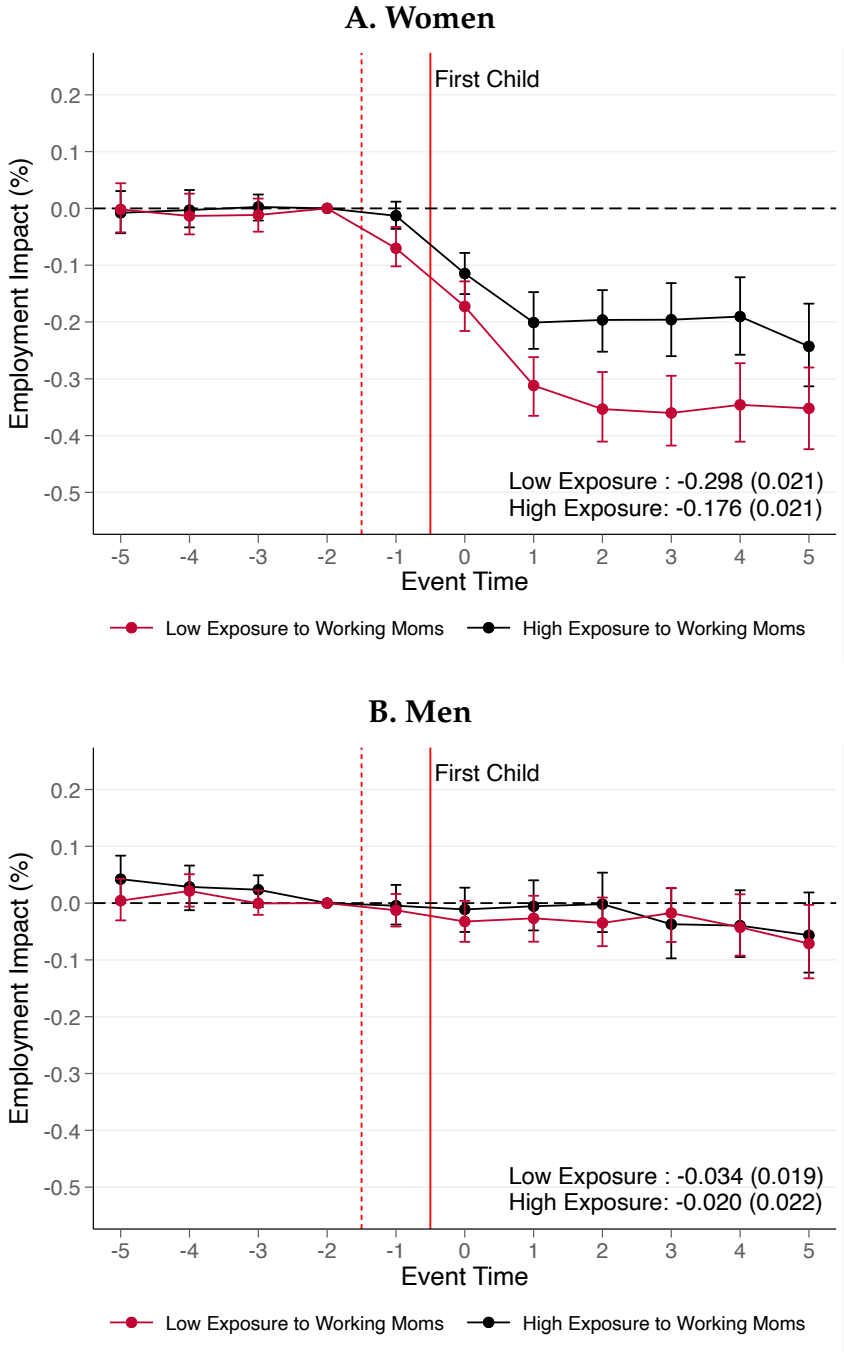
Notes: This table provides estimates of child penalties in employment by exposure to working mothers (left columns) and working fathers (right columns) during middle- and high-school. The distribution of exposure is divided into tertiles (low, medium, and high). The estimates are based on event studies of first child birth using equation (1). Panel A considers our baseline specification, while Panel B considers specifications with richer sets of controls. The table shows that greater exposure to working mothers reduces the child penalty, while greater exposure to working fathers increases it. The results are very robust to specification. Standard errors (in parentheses) and p-values (in brackets) are bootstrapped with 500 repetitions.

Figure 1: Event Study of First Child Birth in the Full Sample



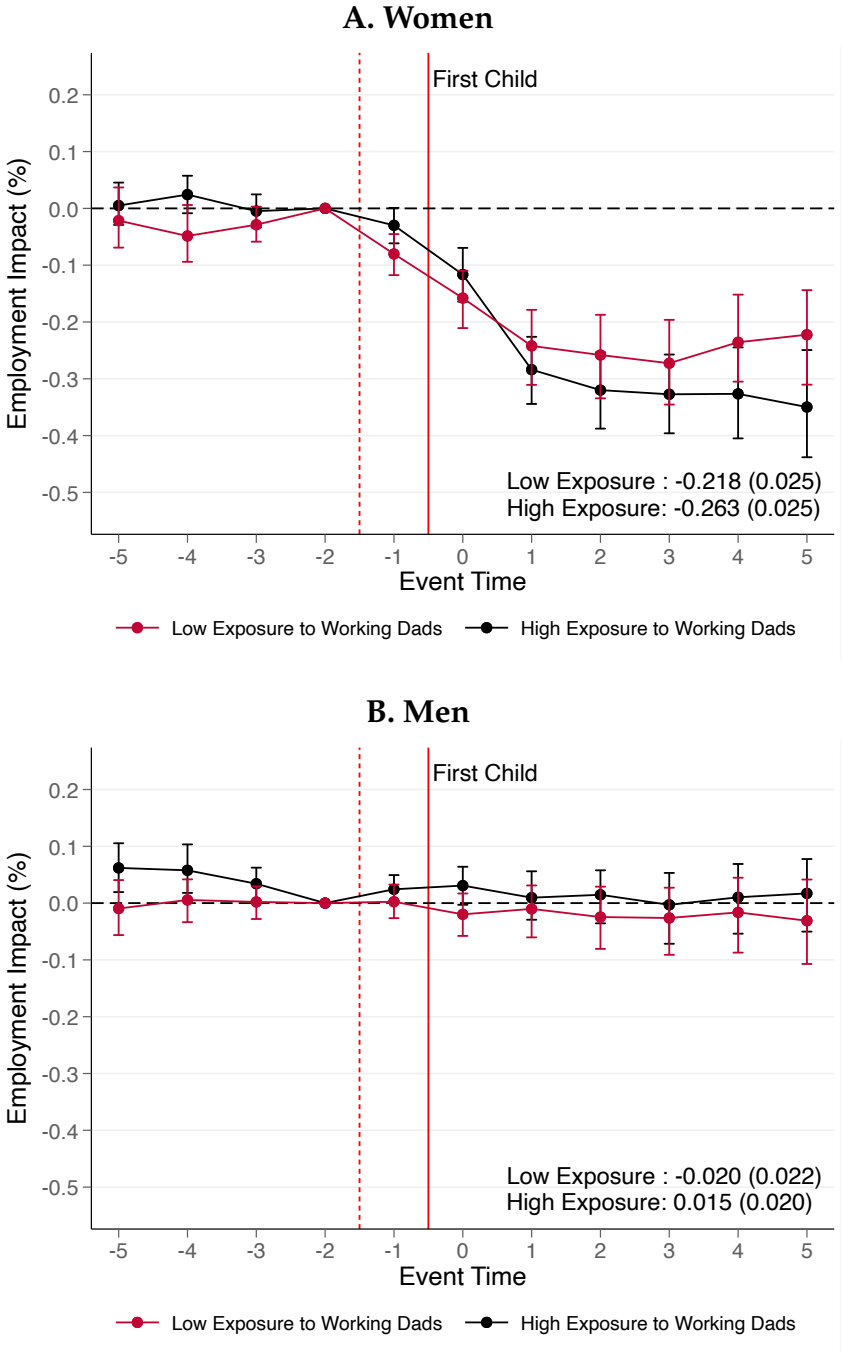
Notes: This figure presents event studies of first child birth in the full sample of men (black series) and women (red series) observed between five years before and five years after child birth. Each dot gives the percentage impact on employment in event year τ (relative to the omitted base year $\tau = -2$), estimated based on equation (4). The year of pregnancy is marked by the vertical dashed line and the year of child birth is marked by the vertical solid line. The figure also provides 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 2: Event Studies of First Child Birth by Classroom Exposure to Working Moms



Notes: This figure presents event studies of first child birth for women (top panel) and men (bottom panel) by exposure to working 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 impact on employment in event year τ (relative to the omitted base year $\tau = -2$). The year of pregnancy is marked by the vertical dashed line and the year of child birth is marked by the vertical solid line. The figure also provides estimates of the average employment impact of child birth (over event times 0-5) on men and women in the low- and high-exposure groups. We find strong exposure effects for women (higher exposure results in much smaller employment drops) and no exposure effects for men. Standard errors are bootstrapped with 500 repetitions.

Figure 3: Event Studies of First Child Birth by Classroom Exposure to Working Dads



Notes: This figure presents event studies of first child birth for women (top panel) and men (bottom panel) by exposure to working fathers 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 impact on employment in event year τ (relative to the omitted base year $\tau = -2$). The year of pregnancy is marked by the vertical dashed line and the year of child birth is marked by the vertical solid line. The figure also provides estimates of the average employment impact of child birth (over event times 0-5) on men and women in the low- and high-exposure groups. We find marginal exposure effects for women (higher exposure results in weakly larger employment drops) and no exposure effects for men. Standard errors are bootstrapped with 500 repetitions.

Online Appendix

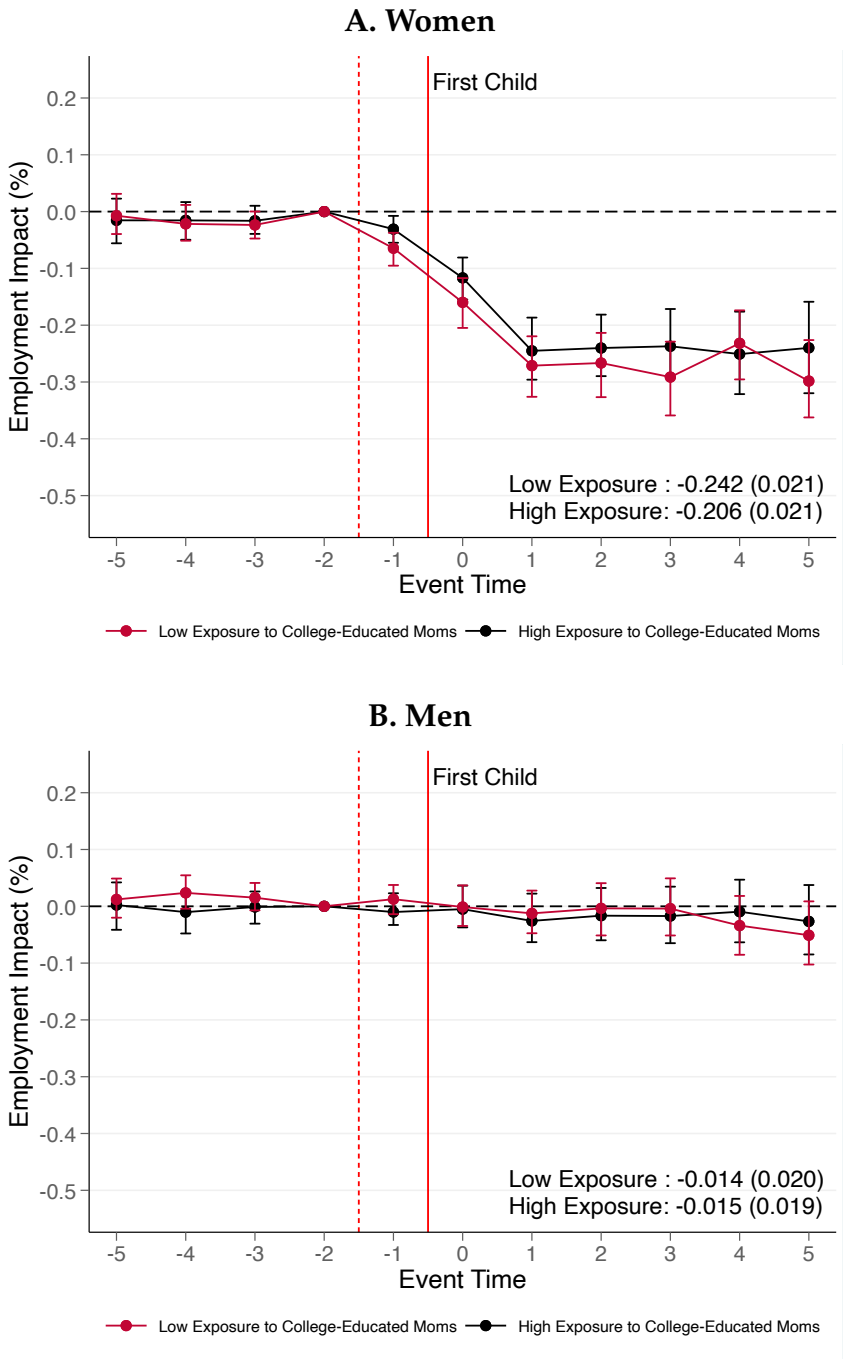
A Supplementary Tables and Figures

Table A.1: Balance Tests for Boys

	Exposure to Working Moms			**Exposure to Working Dads**		
	Low	**High**	Δ	**Low**	**High**	Δ
College-Educated Parent	0.351	0.356	**0.004**	0.379	0.441	**0.062**
	(0.083)	(0.099)	**(0.062)**	(0.109)	(0.103)	**(0.053)**
Married Parent	0.705	0.768	**0.063**	0.929	0.960	**0.031**
	(0.059)	(0.083)	**(0.056)**	(0.054)	(0.046)	**(0.027)**
US-born Parent	0.910	0.906	**-0.004**	0.904	0.874	**-0.030**
	(0.046)	(0.054)	**(0.020)**	(0.053)	(0.046)	**(0.021)**
Parental Income	23,354	21,564	**-1,790**	25,016	21,075	**-3,941**
	(2,261)	(3,025)	**(2,037)**	(2,818)	(2,345)	**(1,829)**
Number of Siblings	1.398	1.513	**0.114**	1.489	1.775	**0.286**
	(0.133)	(0.204)	**(0.148)**	(0.190)	(0.168)	**(0.125)**
White	0.582	0.619	**0.037**	0.631	0.656	**0.025**
	(0.048)	(0.066)	**(0.046)**	(0.064)	(0.058)	**(0.032)**
Black	0.213	0.189	**-0.024**	0.152	0.130	**-0.022**
	(0.026)	(0.045)	**(0.033)**	(0.040)	(0.034)	**(0.030)**
Hispanic	0.113	0.137	**0.024**	0.120	0.131	**0.011**
	(0.023)	(0.034)	**(0.027)**	(0.040)	(0.036)	**(0.022)**
School FE	X	X	X	X	X	X
Cohort FE	X	X	X	X	X	X
No. Observations	576	560	1136	449	467	916

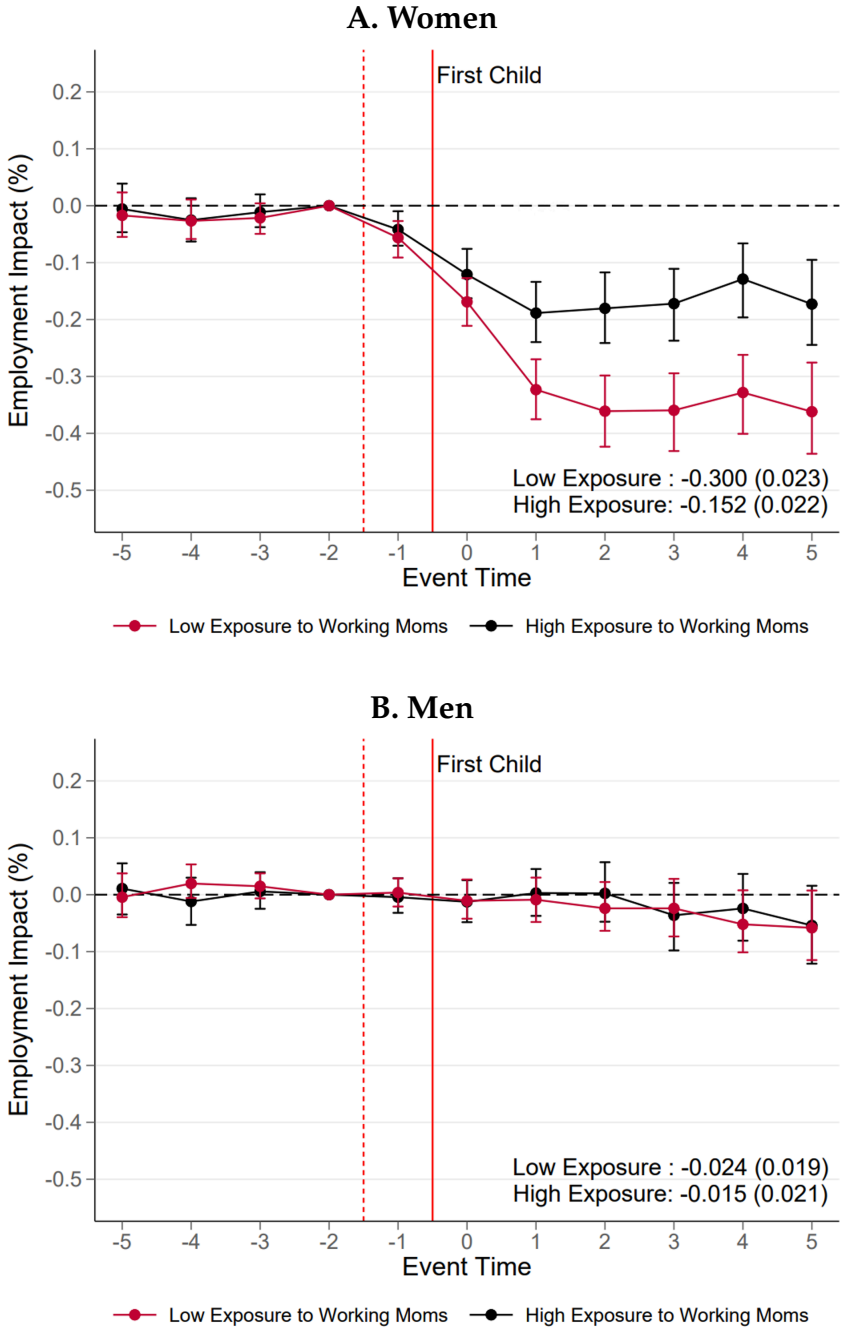
Notes: This table presents balance tests for boys with respect to our peer exposure variable: the fraction of classmates with working mothers (left) and working fathers (right), respectively. The table provides results from regressions of family background variables on tertiles of peer exposure (low, 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, 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. Standard errors are clustered at the school level.

Figure A.1: Event Studies of First Child Birth by Classroom Exposure to College-Educated Moms



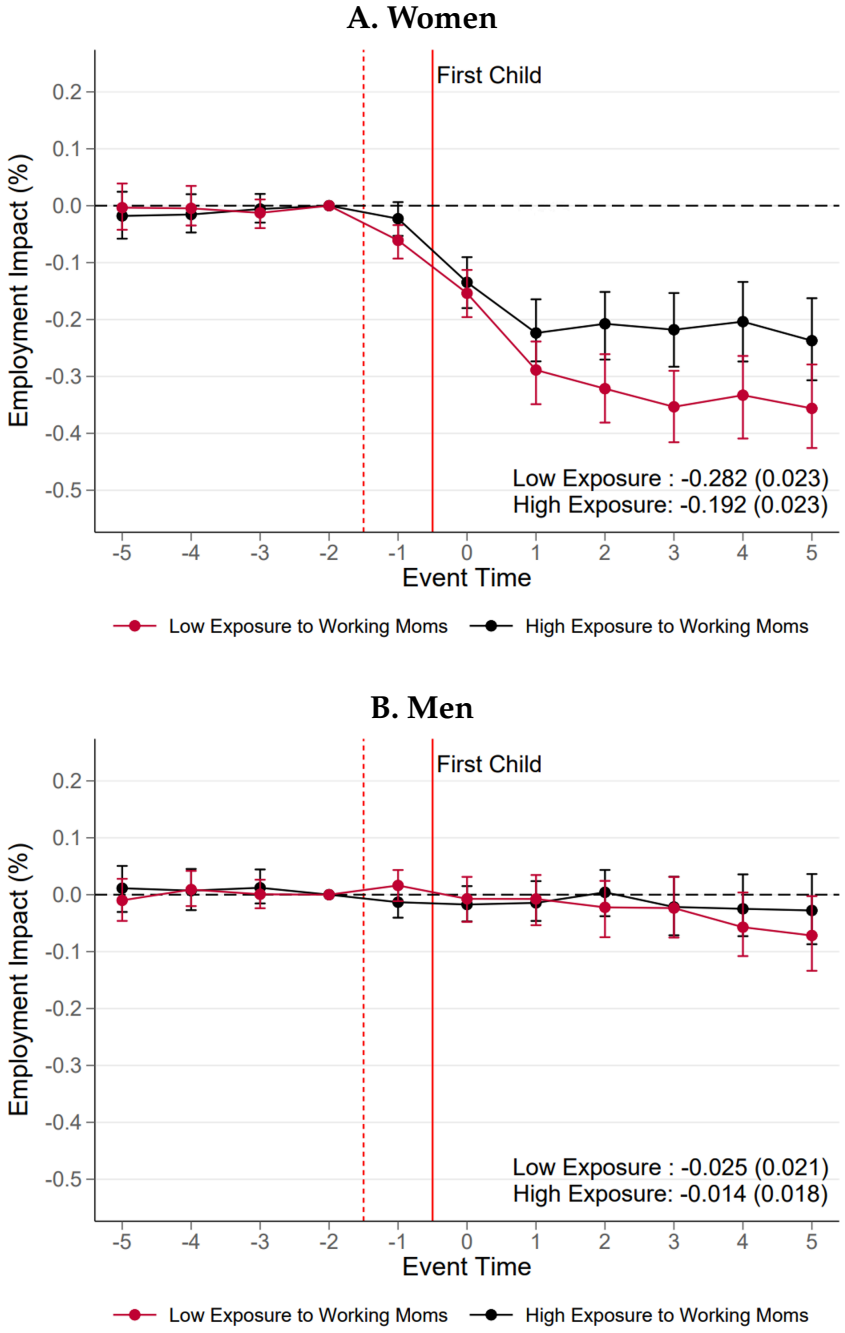
Notes: This figure presents event studies of first child birth for women (top panel) and men (bottom panel) by exposure to college-educated mothers during middle- and high-school. The event studies are estimated using specification (1), dividing the distribution of exposure into tertiles (low, medium,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

Figure A.2: Event Studies of First Child Birth by Classroom Exposure to Working Moms of Same-Race Peers



Notes: This figure presents event studies of first child birth 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 impact on employment in event year τ (relative to the omitted base year $\tau = -2$). The year of pregnancy is marked by the vertical dashed line and the year of child birth is marked by the vertical solid line. The figure also provides estimates of the average employment impact of child birth (over event times 0-5) on men and women 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.

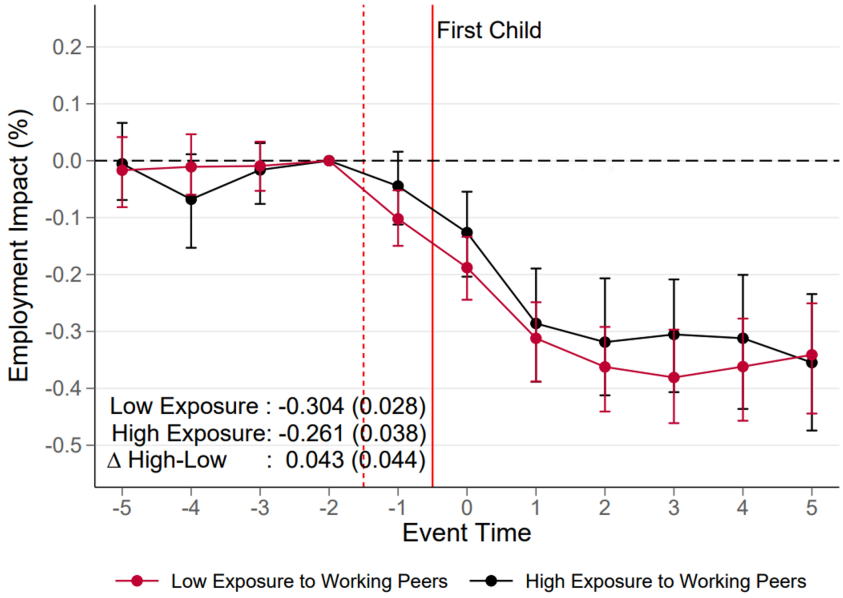
Figure A.3: Event Studies of First Child Birth by Classroom Exposure to Working Moms of Same-Gender Peers



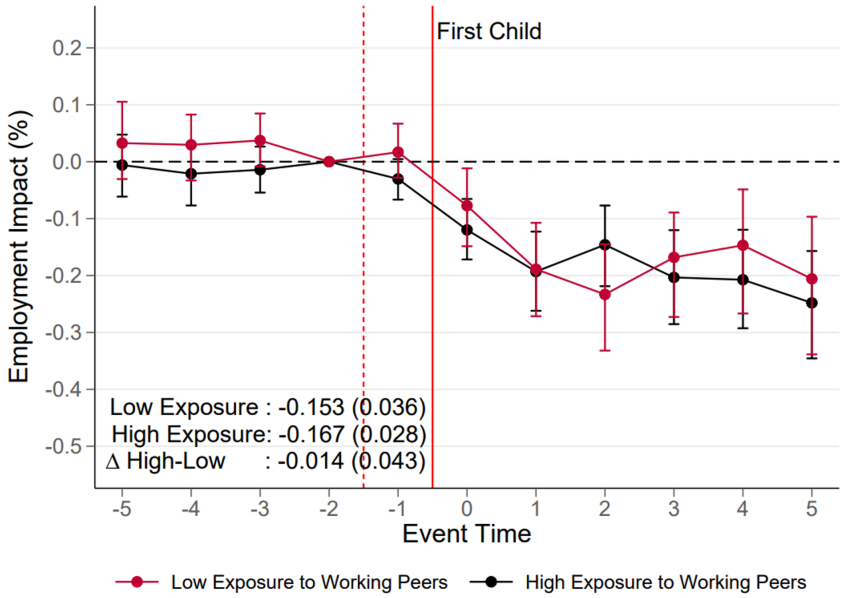
Notes: This figure presents event studies of first child birth for women (top panel) and men (bottom panel) by exposure to working mothers among *same-gender* peers in middle- and high-school. The event studies are estimated using specification (1), dividing the distribution of exposure into tertiles (low, medium, and high). Each dot gives the percentage impact on employment in event year τ (relative to the omitted base year $\tau = -2$). The year of pregnancy is marked by the vertical dashed line and the year of child birth is marked by the vertical solid line. The figure also provides estimates of the average employment impact of child birth (over event times 0-5) on men and women 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.4: Event Studies of First Child Birth by Classroom Exposure to Working Moms and Exposure to 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 child birth 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), splitting each tertile of exposure to working mothers into below-median and above-median contemporaneous exposure to working female peers. Each dot gives the percentage impact on employment in event year τ (relative to the omitted base year $\tau = -2$). The year of pregnancy is marked by the vertical dashed line and the year of child birth is marked by the vertical solid line. The figure also provides estimates of the average employment impact of child birth (over event times 0-5) on women with low and high exposure to working mothers, each split by low and high exposure to working peers. Standard errors are bootstrapped with 500 repetitions.