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Abstract

We study the transmission of beliefs from adults to children and how this contributes to the gender gap in mathematics. We exploit plausibly exogenous variation in the proportion of a child's middle school classmates whose parents believe boys are better than girls at learning mathematics. An increase in exposure to peers whose parents report this belief increases a child's likelihood of believing it, with similar effects for boys and girls and greater transmission from peers of the same gender. This exposure also affects children's perceived difficulty of math, aspirations, and academic performance, generating gains for boys and losses for girls.

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1 Introduction

Historically, average levels of education among men far exceeded those of women (Goldin et al., 2006). Over the past 30 years, however, this gender gap in educational attainment has closed and then reversed in a large number of countries (Asadullah and Chaudhury, 2009; Bailey and Dynarski, 2011; Rosenzweig and Zhang, 2013). Despite this reversal of the gender gap in educational attainment, in many countries the majority of children continue to believe that boys are better than girls at learning math (Beilock et al., 2010; Jayachandran, 2015; OECD, 2015). These beliefs can be harmful if they affect behavior and, in so doing, translate into real outcomes. As early as age 7, boys and girls begin to exhibit preferences and make decisions that reflect gender norms about appropriate hobbies and behavior (Bian et al., 2017). Prior work has shown that cultural norms about gender translate to differential effort, enthusiasm, and performance in school (Nollenberger et al., 2016; Rodríguez-Planas and Nollenberger, 2018). These, in turn, are associated with differences in labor market outcomes across the genders, both within countries and across nationalities of second generation immigrants from countries with stronger pro-male cultural norms (Antecol, 2000, 2001; Rodríguez-Planas et al., 2018).

In this paper, we use nationally representative data from Chinese middle schools to study how beliefs about the math ability of men and women transmit across generations and how this affects girls' academic performance relative to boys. In this context, girls outperform boys in all subjects, including math, yet over 40 percent of the parents in our data believe boys are better than girls at learning mathematics[1]. We exploit the random assignment of children to classrooms in these schools to generate plausibly exogenous variation in the proportion of peers' parents who hold this belief, henceforth, "biased" parents. We estimate how increases in exposure to peers with biased parents affect children's beliefs about the world, beliefs about themselves, aspirations, and math performance.

We find that a one standard deviation (SD), or roughly 11 percentage point, increase in the proportion of peers whose parents are biased increases the likelihood that a child holds the belief that boys are better than girls at learning mathematics by 4.6 percentage points, from a baseline of 52 percent. These effects are monotonic over the distribution of possible values for the proportion

[1] Using the framework in Bordalo et al. (2016), the stereotype that boys are better than girls in learning math is therefore not "representative" of the actual performance of the boys and girls in our sample.

of peers with biased parents, and are similar for boys and for girls. Moving a child from the least biased classroom in our sample (no peers whose parents express bias) to the most biased classroom in our sample (where more than 83 percent of peers' parents express bias) would generate an increase of 34.8 percentage points in the likelihood of the child holding this bias.

We know that children do not learn from all sources equally, however, and we test for homophily: the tendency of individuals to associate with and learn from similar others (Currarini et al., 2009). The similarity we study here is by gender, and we create separate measures of the proportion of peers with biased parents by the gender of a child's peers, generating one measure for the proportion of girl peers with biased parents bias and another for boy peers. We find that the impact of exposure to girl peers with biased parents on girls' beliefs is roughly twice as large as it for boys' beliefs, and vice versa for boys.

Our subsequent results show that this exposure affects children's beliefs about themselves as well as their performance in mathematics in ways that embody the message of the bias, improving outcomes for boys and worsening them for girls. We estimate that a one SD increase in exposure to peers with biased parents increases girls' likelihood of perceiving math to be difficult, relative to boys', by 1.9 percentage points (a 25 percent increase in the gap between girls' and boys' perceived difficulty), and worsens their relative performance on standardized math exams by 0.06 SD. These effects are also monotonic over the distribution of the peer parents' bias measure, suggesting larger effects for greater changes in peer parents' bias. The effects on test scores are also remarkably similar to the 0.07 SD (7.47 point) change in the gender test gap in PISA scores that both Nollenberger et al. (2016) and Rodríguez-Planas and Nollenberger (2018) find is associated with a one standard deviation change in a country's gender equality index. As with belief transmission, for our math performance estimates we see larger effects of exposure to own-gendered peers with biased parents than those that accrue with exposure to other-gendered peers.

We then conduct a series of analyses to interpret our results thus far. Our measure of the proportion of peers with biased parents captures the average levels of peers' parents' beliefs and any factors which have contributed to it, which may include a variety of other well-known sources of peer effects such as peer ability and parental education or occupation (c.f., Sacerdote et al. 2011; Feld and Zölitz 2017). We first attempt to distinguish between two possible explanations for our estimated effects. Explanation one is that peer parents' bias is merely a new measure for a broader latent variable, also captured by other sources of peer effects documented in prior studies (c.f. Sacerdote et al., 2011). For example, if peer parents' bias and peer ability were driven by the same source, our estimates would be capturing the effect of exposure to this source of peer effects, not exposure to more gender bias. Explanation two is that there is sufficient variation in our peer parents' bias measure, independent of other known sources of peer effects, to generate the patterns we observe. We conduct a series of horse race regressions to distinguish between the two, adding in controls for other well-known sources of peer effects - peer ability (Feld and Zölitz, 2017), peer parents' education (Fruehwirth, 2017), and peer gender composition (Hu, 2015). If our estimates of the effects of exposure to peers with biased parents were to attenuate, this would suggest the former explanation. In line with the latter explanation, however, we find that our coefficient estimates for beliefs and test performance are stable, both in magnitude and significance, to adding controls for these known sources of peer effects.

Second, we test for reverse causality and a specific flavor of the "reflection problem." In this context, there are two concerns. The first is that the beliefs of parents of children in a given classroom are affected by each other's beliefs (the reflection problem, as in Manski, 1993, and Angrist, 2014). The second is that the ability of a child's peers in her randomly assigned classroom affects her parent's beliefs about the relative math ability of boys and girls. In other words, observing gender differences in her child's peers' math performance causes a parent to update her beliefs, as opposed to children learning from the beliefs of their peers' parents (reverse causality). We find no evidence that parents' beliefs are affected by the beliefs of parents of other children in their child's classroom. Our test of reverse causality rejects even small effects of an increase in the underlying ability of a child's boy peers, relative to girls, on her parent's likelihood of believing that boys are better than girls at learning math[2].

We next study how these results vary across different lengths of exposure to peers. To do so, we compare children who have spent more than two years with their randomly assigned peers to those who have only spent three to six months with these peers. Consistent with a simple model

[2] The explanation we offer for why parents' beliefs do not respond is Bayesian: because children have lived for less time than adults, they have been exposed to less information and their beliefs (priors) are thus more prone to updating (flexible). Adults' beliefs were largely formed when they themselves were children and are now more difficult to update (firmer). This may also explain the source of the bias: when adults' beliefs were formed, the stereotype of boys' superiority in math was likely more "representative," as in Bordalo et al. (2016).

of Bayesian updating in which children update their priors only in response to new information, we find little evidence that more time spent with peers with biased parents increases the likelihood a child will herself hold that belief. Consistent with a model of small differences in enthusiasm or effort having compounding effects on performance over time (Cunha and Heckman, 2007), we find larger negative effects on girls' test scores, relative to boys', among children who have been exposed to peers with biased parents for a longer period of time.

Finally, we look more closely at how exposure to peers with biased parents affects girls. We find that this exposure is more harmful for girls whose own parents are themselves biased. We also find that girls who have no close friends in their randomly assigned classroom experience much greater harm in aspirations and test scores from increases in exposure to peers with biased parents, while girls whose five closest friends are all in the classroom appear to be immune to the negative aspirations and performance effects of this exposure (this result is in line with the findings of Lavy and Sand, Forthcoming, and Hahn et al., 2017). We also present evidence to try to distinguish between two distinct ways girls might respond to high levels of gender bias: one, that exposure to peers with biased parents would cause girls to reallocate effort away from math towards other subjects (e.g., language arts) or hobbies; and two, that exposure to peers with biased parents may cause girls to change their broader beliefs about their own ability. We show suggestive evidence in support of the second story, finding small reductions in performance on standardized Chinese and English tests, no effects on time spent on hobbies, and weak evidence of a drop in girls' confidence about their future.

We aim to contribute to ongoing research on the formation of beliefs, the causes and consequences of gender disparity, and peer effects. First, we add to a series of papers studying how beliefs form among children and the process of belief transmission from parent to child (e.g., González de San Román and de la Rica Goiricelaya, 2016; Olivetti et al., Forthcoming; Rodríguez-Planas and Nollenberger, 2018). The closest papers to ours are Dhar et al. (2018) and Rodríguez-Planas et al. (2018), who also study intergenerational transmission of gender bias. We further their work by exploiting a data set with universal coverage of classmates, random assignment of children to classrooms, and administrative performance data. Second, we add to the budding literature on gender gaps, both overall and in STEM fields (e.g., Dee, 2007; Niederle and Vesterlund, 2010; Ellison and Swanson, 2010; Jayachandran, 2015). Third, we contribute to the rich literature on peer effects (e.g., Sacerdote et al., 2011; Lavy and Schlosser, 2011; Feld and Zölitz, 2017), and, specifically, work on the impact of peers' parents on children's performance (e.g., Carrell and Hoekstra 2010; Bifulco et al. 2011; Fruehwirth 2017; Olivetti et al. Forthcoming).

The rest of the paper proceeds as follows. Section 2 describes the setting we study, our data, and empirical approach. Section 3 presents our main empirical results. Section 4 presents a series of analyses to guide interpretation of Section 3's estimates. Section 5 shows further analyses, and Section 6 concludes.

2 Setting, data, and empirical methodology

Our analysis takes place in a nationally representative sample of Chinese middle schools. This setting has two features which facilitate causal inference and the study of gender bias. The first feature is common but not universal presence of the belief that boys are better than girls at learning mathematics: 41 percent of parents and, among children, 58.4 percent of boys and 47.4 percent of girls in our sample of middle school students agree with the statement "boys are better than girls at learning math," despite the fact that, in our data, girls outperform boys in math. In the rest of the paper, we will refer to this belief as "bias." The second feature is the random assignment of children to classrooms within schools. Students are usually allotted to middle schools by their local educational authority based on geographic proximity to schools. China's compulsory education law requires that, within middle schools, students be randomly assigned to classes[3]. Several previous studies have used this policy and the random assignment it creates as a source of exogenous variation in classroom characteristics to study peer effects and the effects of teacher-student gender match on child performance (Hu, 2015; Eble and Hu, 2017; He et al., 2017; Gong et al., 2018).

2.1 Data

We use the first wave of the China Education Panel Survey (CEPS) for our empirical analysis. The CEPS is a nationally representative sample of Chinese middle school students, collecting a series of data from the students, their parents, their teachers, and their principals, planned to continue

[3] We discuss the potential for and extent of deviation from this rule below.

over several waves. The CEPS follows all students in two randomly selected seventh grade classes and two randomly selected ninth grade classes in each of 112 randomly selected schools[4]. These schools were selected using a nationally representative random sampling frame with selection probability proportional to size. The dataset comprises approximately 20,000 students and the first wave was collected in the 2013-2014 academic year. The second, latest available wave collects data only for a subset of children, and so we do not use it here.

The CEPS student data includes administrative data on the child's academic performance in mathematics, Chinese, and English, as well as the child's responses to a survey about her beliefs, hobbies, social life, and aspirations. The parent data include a variety of demographic data as well as parent beliefs. The teacher and administrator data include information on teacher behavior, training, school facilities, and the method used to assign children to classes. We use the same sample restriction that prior work using these data employs: using only within-grade classroom pairs that report using random assignment of children to classrooms[5] (Hu, 2015; Gong et al., 2018). This leaves us with 9,361 children in 215 classrooms spread across 86 schools, the estimation sample we use for our analysis. These children are assigned to a peer group at the start of seventh grade and will remain with them throughout middle school. The excluded grade-by-school classroom pairs report either using methods other than random assignment to place children in classes or re-sorting children to classrooms in the years after the initial random assignment. These are predominantly ninth grade classrooms, where re-sorting often occurs due to administrative concerns about placing children in good high schools[6]; and classrooms in rural

[4]Chinese middle schools typically span three grades: seven, eight, and nine. The median school in our dataset has six seventh grade classrooms and six ninth grade classrooms (mean: 7.3 and 6.9, respectively). There are not enough schools in our sample with only two classes per grade, i.e., where we would have all students in a grade, to study those as a separate subgroup.

[5]Across China, various methods are used for assignment of children to classes, including random number generators, alphabetical assignment based on surname, and the system described in He et al. (2017) wherein an alternating sequence assigns students to classrooms sequentially based on entrance exam scores in a way that preserves mean test score balance and avoids stratification across classrooms. The randomness of assignment of children to classrooms in Chinese middle schools and its appropriateness for causal inference has been probed in several recent papers, many of which use this same dataset (Hu, 2015; Eble and Hu, 2017; He et al., 2017; Gong et al., 2018).

[6]In Table A.2, we show summary statistics of schools in our estimation sample separately by whether or not they contain grade 9 classrooms that maintain the randomization initiated in grade 7 (in other words, schools that do not re-sort students by ability in subsequent years). They are balanced on most observable characteristics (size, number of teachers, percent of teachers with a BA, whether they are private or public). The only significant difference we observe is that schools whose ninth grade classrooms do not maintain randomization are slightly higher-ranked than schools whose ninth grade classrooms do maintain randomization. This pattern is consistent with the fact that re-sorting of children by ability is regarded as a way for middle schools to improve the likelihood of sending top children to higher-ranked high schools, and middle school ranking partly reflects this placement record.

areas, where enforcement of such rules is less strict overall.

Table 1 presents summary statistics for students, by gender, for those students randomly assigned to classrooms. The girls in our sample are slightly younger than the boys, and they are more likely to have wealthier, more educated parents. Girls also have more siblings, consistent with traditional norms and fertility responses to birth control policy in China which permits further parity, in some cases, if the first child is a girl (Chan et al., 2002). Finally, there is a "reverse gender gap" in all subjects, i.e., girls perform better than boys[7].

2.2 Empirical approach

In our empirical analysis, we focus on estimating two key relationships: the effect of exposure to peers with biased parents on a child's outcomes (e.g., own bias, perceived difficulty of mathematics, aspirations, and academic performance) and how this varies with the child's gender. Our identification strategy is to exploit random variation between classrooms in a given grade, within a given school, in the proportion of peers with biased parents. Our main estimating equation is as follows:

$$\begin{aligned} Y_{icgs} = \beta_0 + \beta_1 PPB_{icgs} + \beta_2 PPB_{icgs} * F_{icgs} + \beta_3 OPB_{icgs} + \beta_4 OPB_{icgs} * F_{icgs} \\ + \beta_5 F_{icgs} + \beta_6 SC_{icgs} + \beta_7 TC_{cgs} + \eta_{gs} + \varepsilon_{icgs} \end{aligned} \tag{1}$$

In this equation, Y_{icgs} refers to the outcome of interest for child i in class c in grade g in school s. PPB_{icgs} is the proportion of child i's peers in her or his classroom who have parents that believe boys are better than girls at learning math, henceforth "peer parents' bias." This is a leave-one-out measure: in calculating the peer parents' bias measure, we exclude the child's parent's response to the question about the relative math ability of boys and girls. In our data, this measure varies from zero to 0.833 (mean 0.410). We follow the example of Chetty et al. (2014) in standardizing the variable by subtracting the mean and dividing by the SD. This ensures that our coefficient estimates for β_1 and β_2 are easily interpretable as the effect of a one SD, or 11 percentage point, increase in exposure to peers with biased parents. This measure is more policy-relevant than the

[7] In the 2009 PISA results for China, boys significantly outperformed girls in math. In the 2015 PISA results, this difference was no longer significant. These data, however, apply only to a select group of children from urban areas: Shanghai (2009) or Beijing, Shanghai, Jiangsu, and Guangdong. Our data come from a nationally representative sample of middle schools across China and include both rural and urban areas.

Table 1: Summary statistics

	All	Girls	Boys	Girls - boys	P-value
Age	13.23	13.17	13.28	-0.11	0.00
Holds agricultural hukou	0.49	0.48	0.51	-0.03	0.04
Number of siblings	0.71	0.76	0.66	0.10	0.00
Household is poor	0.19	0.18	0.20	-0.02	0.01
Father's highest credential					
Middle school	0.41	0.41	0.42	-0.01	0.44
High school	0.26	0.25	0.26	-0.01	0.69
College	0.19	0.20	0.18	0.02	0.03
Mother's highest credential					
Middle school	0.38	0.39	0.37	0.02	0.01
High school	0.23	0.23	0.23	0.00	0.56
College	0.16	0.17	0.16	0.01	0.12
Ethnic minority	0.12	0.12	0.11	0.01	0.23
Math test score	70.1	70.9	69.4	1.50	0.00
English test score	70.1	73.0	67.4	5.60	0.00
Chinese test score	70.0	73.2	67.1	6.10	0.00
Number of observations	9,361	4,492	4,869	—	—

Note: this table presents summary statistics for observations in our estimation sample. The variables are all coded as 0 = No, 1 = Yes, except for age and number of siblings, which are self-explanatory, and test score (mean = 70, SD = 10). "Holds agricultural hukou" means the residence permit of the household was given in a rural, agricultural (as opposed to non-agricultural, urban) locality.

raw variable, which would capture the effect of moving from a classroom with no peers with biased parents to one with only peers with biased parents (the latter of which does not appear in our data).

F_{icgs} is an indicator for the child being female. OPB_{icgs} is an indicator for whether the child's own parent believes that boys are better than girls at learning mathematics. SC_{icgs} is a vector of characteristics specific to the student, including rural vs. urban household residency (hukou) status, parents' education, income level, these three interacted with the child's gender, the child's ethnicity, her number of siblings, and her perceived ability, proxied by her perceived difficulty of mathematics in the sixth grade. TC_{cgs} is a vector of teacher characteristics including gender and its interaction with child gender, the teacher's years of experience, type of degree, and her receipt of various teaching awards. η_{gs} is a grade-by-school fixed effect[8], and ε_{icgs} is a standard error, clustered at the grade-by-school level.

We include own parent's bias for three reasons: one, to follow recent work also studying peers' parents' impact on children, which includes the child's own parents' characteristic of interest in addition to those of peers (Bifulco et al., 2011; Fruehwirth, 2017; Olivetti et al., Forthcoming). Two, we include it because this correlation is an object of separate interest - it is the main focus of Dhar et al. (2018) - and helps benchmark the relative importance of peer parents' and own parent's bias. In the appendix we present a series of parallel tables for our main analyses which show the results generated by excluding the own parent's bias variables from the list of controls. Our findings are robust to choice of specification. Finally, we include own parent's bias because we will show later in the paper that the interaction between peer parents' bias and own parent bias shows greater harms for girls' exposure to peers with biased parents among girls whose own parents are biased. In our main specification, however, we wish to estimate the average effect of exposure to peers with biased parents, and including own parent's bias improves the precision of our estimate.

Our main coefficients of interest are β_1 (peer parents' bias) and β_2 (its interaction with the female child dummy). We further differentiate between two closely related but separate types of effect estimate for girls. The first type type is the effect of exposure to peers with biased parents on the gender gap, captured by β_2. This is the primary effect of interest. The second type is the

[8] We do not use classroom fixed effects because we wish to exploit the variation in peer parents' bias between classrooms within a grade within a school.

Table 2: Test for randomization / balance

	(1)	(2)
Age	0.124***	-0.010
	(0.050)	(0.007)
Holds agricultural hukou	-0.120	0.014
	(0.074)	(0.019)
Number of siblings	-0.081**	-0.006
	(0.041)	(0.008)
Household is poor	-0.064	0.044**
	(0.071)	(0.020)
Female	0.024	0.015
	(0.029)	(0.011)
Mother's highest credential		
Middle school	-0.060	0.013
	(0.070)	(0.016)
High/technical school	0.071	0.026
	(0.080)	(0.021)
College or above	0.108	0.023
	(0.082)	(0.025)
Father's highest credential		
Middle school	-0.023	0.004
	(0.046)	(0.017)
High/technical school	0.067	0.007
	(0.068)	(0.029)
College or above	0.060	0.018
	(0.081)	(0.032)
Ethnic minority	0.148	-0.009
	(0.250)	(0.023)
Number of observations	8,964	8,964
R-squared	0.05	0.69
Joint test F-statistic	2.19	1.13
[p-value]	[0.02]	[0.35]

Note: this table presents a balancing test, as in Antecol et al. (2015), which tests for our set of predetermined characteristics' joint ability to predict the peer parents' bias measure. Grade-by-school fixed effects are added to the estimating equation to generate the estimates in column 2. Variables are all coded as 0 = No, 1 = Yes, except for age and number of siblings, which are self-explanatory. The dependent variable, peer parents' bias, is standardized (mean = 0, SD = 1).

overall effect of exposure to peers with biased parents on girls' outcomes, captured by $\beta_1 + \beta_2$.

For causal interpretation of our estimates of β_1 and β_2, we need to establish that within a school, across classrooms within a grade, the peer parents' bias measure is uncorrelated with other determinants of our outcome variables. If this assumption holds, we can use OLS to estimate the effect of exposure to peers with biased parents on child outcomes. We evaluate this assumption by regressing our peer parents' bias measure on the (predetermined) characteristics in SC_{icgs}. This approach follows Antecol et al. (2015), Bruhn and McKenzie (2009), and Hansen and Bowers (2008). We present our results in Table 2: in column 1, we show the results for regressing peer parents' bias on the vector of predetermined characteristics without any fixed effects; in column 2, we present results from a similar regression, now including the grade-by-school fixed effects we use in our main empirical specification. At the bottom of the table, we report the F-statistic and p-value from a Wald Test of the joint significance of the regressors. In column 2, we fail to reject the null that the regressors do not significantly predict our measure of peer parents' bias. We find similar results if we conduct the test separately by the grade a student is in, reported in Table A.1.

To include own parent's bias on the right hand side of our regression, we need that it be predetermined with respect to peer parents' bias (in other words, that it does not suffer from the "reflection problem, as in Manski, 1993 and Angrist, 2014). To test this, we regress peer parents' bias on own parent's bias using our core specification. Note that regressing an individual's given characteristic on the leave-one-out average of this same characteristic in an individual's randomly assigned cluster yields a mechanical negative correlation. The intuition behind this is as follows: given the random assignment of students into classes, the law of large numbers predicts that, in a given class, the proportion of students with a certain characteristic (e.g., average parent bias or percent female) will be distributed normally. A student's characteristic is thus negatively correlated with the leave-one-out average because the proportion (including the student herself) is equivalent to the sum of the student's characteristic and this average.

To formalize this intuition, we conduct a permutation test, randomly assigning 1,000 random variables with the same potential values (0/1) and mean (0.410) as the parent bias variable. We standardize this and regress the student's random variable on the leave-one-out average of her peers' random variable values within her class, its interaction with the female dummy, and the controls given in Equation 1. This generates $\tilde{\gamma}$, the mean of our permutation test estimates. We find $\tilde{\gamma} = -0.107$ ($SE = 0.026$). Using the true data, we estimate $\hat{\gamma}$, the effect of a one SD increase in the proportion of peers with biased parents on a child's own parent's likelihood of holding bias, to be $\hat{\gamma} = -0.072$, well within the 95% confidence interval around $\tilde{\gamma}$ generated by the permutation test.

2.3 The peer parents' bias measure

In this subsection we describe our measure of the proportion of peers with biased parents (for short, the "peer parents' bias measure"), a key innovation of our paper, in greater detail. First we describe what characteristics are correlated with the belief that boys are better than girls at learning mathematics. We then describe the extent to which this belief reflects reality as opposed to bias. Finally, we show the dispersion of parents' bias between classrooms in each within-school, within-grade pair.

What characteristics are correlated with parent beliefs about the relative ability of boys and girls in mathematics? In Table 3, we show summary statistics of predetermined characteristics separately for parents who do and do not believe that boys are better than girls at learning mathematics. Though there are differences for some characteristics (household income, number of siblings), they are small. Our interpretation of the patterns in this table is that, overall, these two groups of parents are remarkably similar on most observable characteristics associated with the other traditional drivers of peer effects, suggesting that much of the variation in parent beliefs we measure is idiosyncratic to these.

What if we are not measuring "bias" per se; in other words, to what extent do these beliefs reflect boys' actual superiority in math? Two pieces of evidence indicate the belief we call bias is indeed erroneous in this setting. The first, as mentioned in the introduction and explained in footnote 1, is that girls outperform boys in mathematics in these settings and so the belief that boys are better than girls at learning math persists in contradiction of the experience of parents and children in these middle schools. This stereotype is therefore not a "representative" heuristic, as in Bordalo et al., 2016. The second piece of evidence is that the wording of the question about bias refers to the underlying suitability of each gender in learning mathematics, not to children's actual performance. In Section 4, we address the extent to which variation in parent bias is affected

Table 3: Characteristics of children, by whether parents report gender bias

	(1) Full sample	(2) Does not express bias	(3) Does express bias	(4) Difference (column 2 - column 3)	(5) T-test p-value
Ethnic minority	0.12	0.11	0.12	-0.01	0.12
Agricultural hukou	0.49	0.50	0.48	0.02	0.12
Number of siblings	0.71	0.69	0.66	0.03	0.08
Low income household	0.19	0.19	0.17	0.02	0.02
Mother's years of schooling	9.88	9.95	10.04	-0.09	0.27
Father's years of schooling	10.61	10.66	10.76	-0.10	0.20
Number of observations	9,361	5,294	3,675	—	—

Note: this table gives summary statistics for children in our estimation sample, separately for those whose parents do (column 2) and do not (column 3) claim to believe that boys are better than girls at learning math. The variables are all coded as 0 = No, 1 = Yes, except for age, number of siblings, and parental years of schooling.

by actual gender gaps in the classroom, i.e., that our estimates suffer from reverse causality. Our test for this rejects even small effects on a given parent's beliefs from having her child assigned to a classroom where boys happen to outperform girls. This result is consistent with the idea that adults' beliefs about the relative math ability of the genders were largely formed when they themselves were children (decades prior to our measurement of them) and, perhaps, when the stereotype was more accurate.

Next, we describe variation in the peer parents' bias measure between classrooms. At the child level, the maximum value of the peer parents' bias measure is 0.833, the minimum is 0, and the mean is 0.410 (at three significant figures, the mean is the same for girls and boys). Once standardized, the variable ranges from -3.69 SD to 3.64 SD. The average extent of parent bias at the classroom level mirrors the distribution for children.

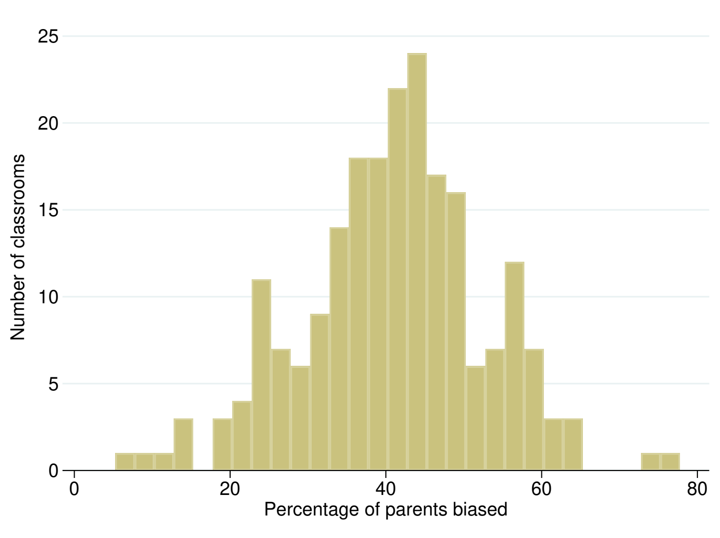
We next characterize the contribution of two sources of variation - differences between schools and differences between classes, within schools (the latter being our level of comparison) - to the overall variation we see in our peer parents' bias measure. If our variation came predominantly from between-school differences then our comparison between classes, within a grade within each school, could precisely estimate the impact of small changes in bias but would have little to say about larger changes, as they would necessitate out-of-sample predictions.

In Figure 1, we show two plots describing this variation. Panel A shows, for each classroom, the proportion of all parents who agree with the statement that boys are better than girls in learning math. This shows a roughly symmetric distribution around 41%, the mean, with a range from 7% to almost 80% (the peer parents' bias measure extends this range slightly, as it is a leave-one-out average). Panel B shows how parent bias varies within the 86 within-school, within-grade pairs of classrooms in our data. We plot each pair as a point, with the standardized class-average parent bias for class 1 shown on the x-axis, and that for class 2 on the y-axis. We overlay the 45-degree line onto this figure; each point's distance from the line shows the within-school, within-grade, between-classroom difference in parent bias for each pair. This shows substantial differences in parent bias between classrooms in these pairs, and a decomposition of variance finds that between-school variation explains less than a third of the overall variation between classes in the parent bias measure. A separate way to capture the differences between classrooms within grades, within schools, is to calculate the absolute value of the difference in parent bias between classroom 1 and classroom 2. We calculate this value for every grade-by-school pair of classrooms; its value varies between 0.1 and 4.35 SD, with a mean of 1 SD, and we plot the distribution in Figure A.1.

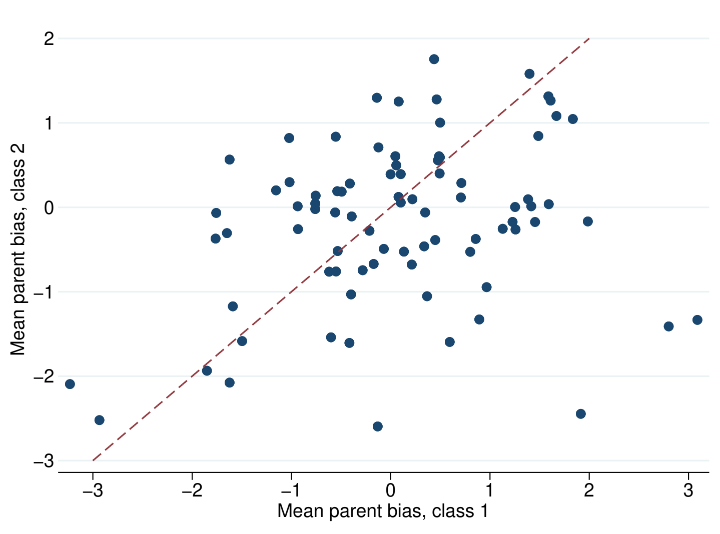
3 Estimated effects of exposure to peers with biased parents

In this section, we present our main estimates of the effect of being exposed to peers whose parents believe that boys are better than girls at learning math. The dependent variables we study in this section are children's beliefs about the world, their beliefs about themselves, and their performance on standardized mathematics examinations.

Figure 1: Dispersion of parent bias across classrooms



Panel A: Dispersion of parent bias across classrooms, raw data



Panel B: Standardized parent bias by within-school, within-grade classroom pair

Note: Panel A of this figure shows the classroom average of the raw parent bias data across all classrooms in our estimation sample. Panel B shows the average (standardized) parent bias in classroom 1 and classroom 2 in each of our 86 within-grade, within-school pairs.

3.1 Beliefs

First, we estimate the relationship between exposure to peers with biased parents and children's beliefs. In this analysis, we study three latent variables related to children's beliefs about the world and themselves. The first latent variable is the extent to which the child herself holds bias. As with parents, we proxy for child bias with the child's response to the prompt: "do you agree that boys are better than girls at learning mathematics?" The second latent variable is the child's perceived ability in mathematics, for which we use the child's response to the prompt "how difficult do you find your current math class?" The possible responses are very difficult, somewhat difficult, a little difficult, and not difficult at all. We code this as a 0/1 variable, equal to one for those children who respond that the current math class is very or somewhat difficult. The third latent variable is the child's aspirations for the future. To measure this, we use the child's response to a prompt asking for her ideal level of completed schooling (e.g., high school, BA, master's, PhD), coding the variable as one for those who aspire to complete a BA or higher, and zero otherwise.

We present these results in Table 4. This table follows the convention that we will use for most of our main result tables: we present coefficient estimates for exposure to peers with biased parents (β_1) and its interaction with whether the child is female (β_2). We also present the coefficients for own parent's bias (β_3) and its interaction with the female dummy (β_4). Finally, we present the coefficient on the child's own gender (β_5). At the bottom of our result tables we show the sample mean of the dependent variable and the number of observations used for estimation. Unless otherwise noted, variation in the number of observations comes from variation in the number of missing values across dependent variables. Our results are robust to restricting the sample to only those observations who have non-missing values for all dependent variables.

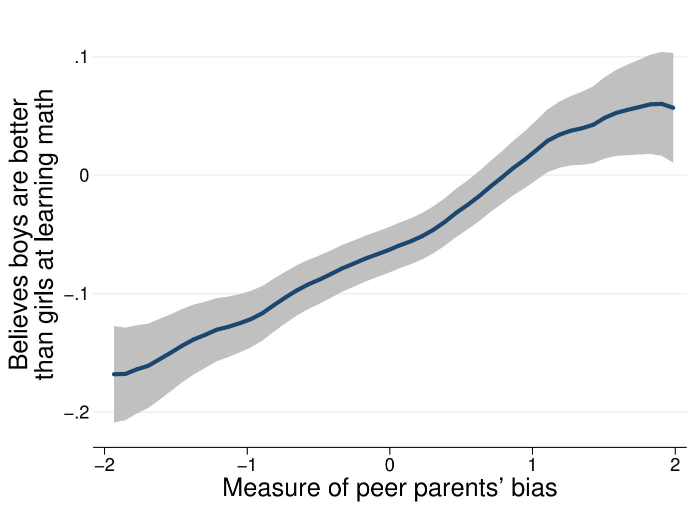
In the first column, we present our estimates for the intergenerational transmission of beliefs. We estimate that a one standard deviation increase in exposure to peers with biased parents is associated with a 4.6 percentage point (8.7%) increase in the likelihood that a child will hold the bias (β_1). Our estimate of the effect of exposure to peers with biased parents is similar for girls and boys; that is, β_2 is not statistically distinguishable from zero. In Figure 2, we plot this relationship non-parametrically. To isolate variation at our level of comparison, we remove grade-by-school fixed effects from the beliefs measure. We then show a kernel-weighted local polynomial

Table 4: Effects on beliefs

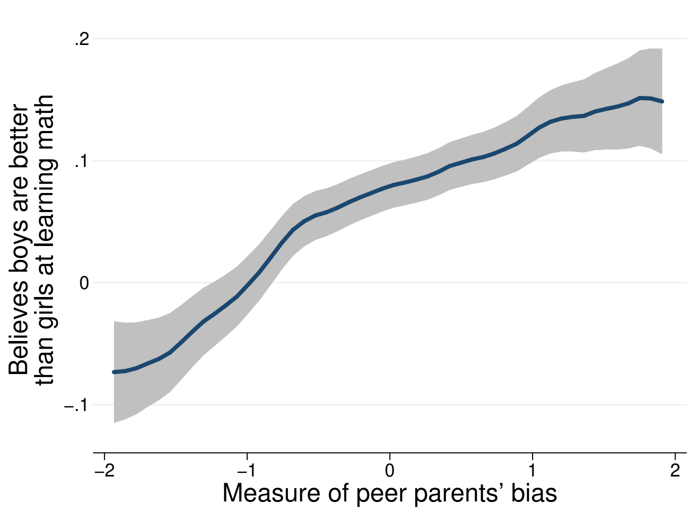
	(1) Believes boys are better than girls at learning math	(2) Perceives current math class to be difficult	(3) Aspires to complete at least a BA
Peers' parents' gender bias (PPB)	0.046***	-0.011	0.002
	(0.012)	(0.016)	(0.017)
PPB x female	0.002	0.019**	0.008
	(0.014)	(0.009)	(0.010)
Own parent's gender bias (OPB)	0.286***	-0.065***	0.018
	(0.016)	(0.016)	(0.014)
OPB x female	0.030	0.152***	-0.013
	(0.020)	(0.023)	(0.020)
Female	-0.129***	-0.013	0.101***
	(0.032)	(0.028)	(0.023)
Mean in sample	0.526	0.570	0.658
Number of observations	8,057	8,212	8,173

Note: this table shows results from estimating equation 1 using the dependent variable named in the column heading and described in the text. Variation in the number of observations across columns stems from differences in missing values for the dependent variables. The dependent variables are coded as 0 = No, 1 = Yes. In Table A.3, we show the analog to these results generated without own parent's bias on the right hand side.

Figure 2: Non-parametric relationships between exposure to peers with biased parents and child beliefs, by gender



Panel A: Girls



Panel B: Boys

Note: this figure shows the correspondence between the individual-level measure of exposure to peers with biased parents and the likelihood the child herself holds gender bias (1 = Yes, 0 = No) after removing grade-by-school fixed effects from the dependent (y-axis) variable. Panel A presents this correspondence for girls in our sample, and Panel B presents it for boys. A one unit increase in the peer parents' bias measure (the x-axis variable) corresponds to an 11 percentage point increase in the proportion of peers with biased parents.

regression of this residual beliefs measure on the peer parents' bias measure. The figure shows that the relationship between exposure to peers with biased parents and children's own likelihood of reporting bias is monotonic over the support of our peer parents' bias measure. Going from a classroom in which roughly 25 percent of peers' parents are biased to one where 75 percent of peers' parents are biased generates a 20.6 percentage point (39%) change in the likelihood that a child will also hold that bias[9]. The patterns in the figure also mirror the regression results showing that this relationship is similar for girls and for boys.

Referring back to the results in column 1 of Table 4, we note that the coefficients on own parent's bias and its interaction with gender are large in magnitude: children whose parents believe boys are better than girls at learning math are 29 percentage points (56%) more likely to also hold that belief and, again, the relationship holds similarly for boys and for girls[10]. These coefficients are comparable in magnitude to going from the least biased classroom (no peers whose parents believe that boys are better at learning math than girls) to the most biased classroom (where 83.3% of parents hold this bias), and this pattern also holds for our later estimates. The coefficient on the female gender dummy shows a pattern that we also see in the raw data: overall, girls are less likely than boys to espouse the belief that boys are better than girls at learning math.

In the next column, we show results for perceived difficulty of math. Here the signs of the estimates diverge for boys and girls, and we observe a significant effect of exposure to peers with biased parents on the gender gap in perceived difficulty. This pattern is the first in a series of evidence we present that exposure to peers with biased parents affects children's beliefs about themselves in ways that embody the message of the bias, i.e., that boys are better than girls at learning math. We estimate that a one SD increase in exposure to peers with biased parents increases the gender gap (β_2) in girls' perceived difficulty of math, relative to boys', by 1.9 percentage points, or an increase of 25 percent in the (7.7 percentage point) gap between girls and boys seen in the raw data. This pattern also holds for own parent's bias, and the estimates are again large: our estimated coefficient of own parent's bias on the gender gap is a 15.2 percentage point increase in the likelihood that a girl perceives math to be difficult, relative to the likelihood for boys. For boys, own parent's bias is associated with a 6.5 percentage point decrease in the

[9]Going from 25% of peer parents with bias to 75% comprises a 4.48 SD change in peer parents' bias.

[10]This estimate is substantially larger than the 11 percentage point increase found among Indian secondary school children in Dhar et al. (2018).

likelihood the child will perceive math to be difficult. Finally, we find no evidence that exposure to peers with biased parents affects aspirations on average, though in Section 5 we show that these results mask important heterogeneity among girls.

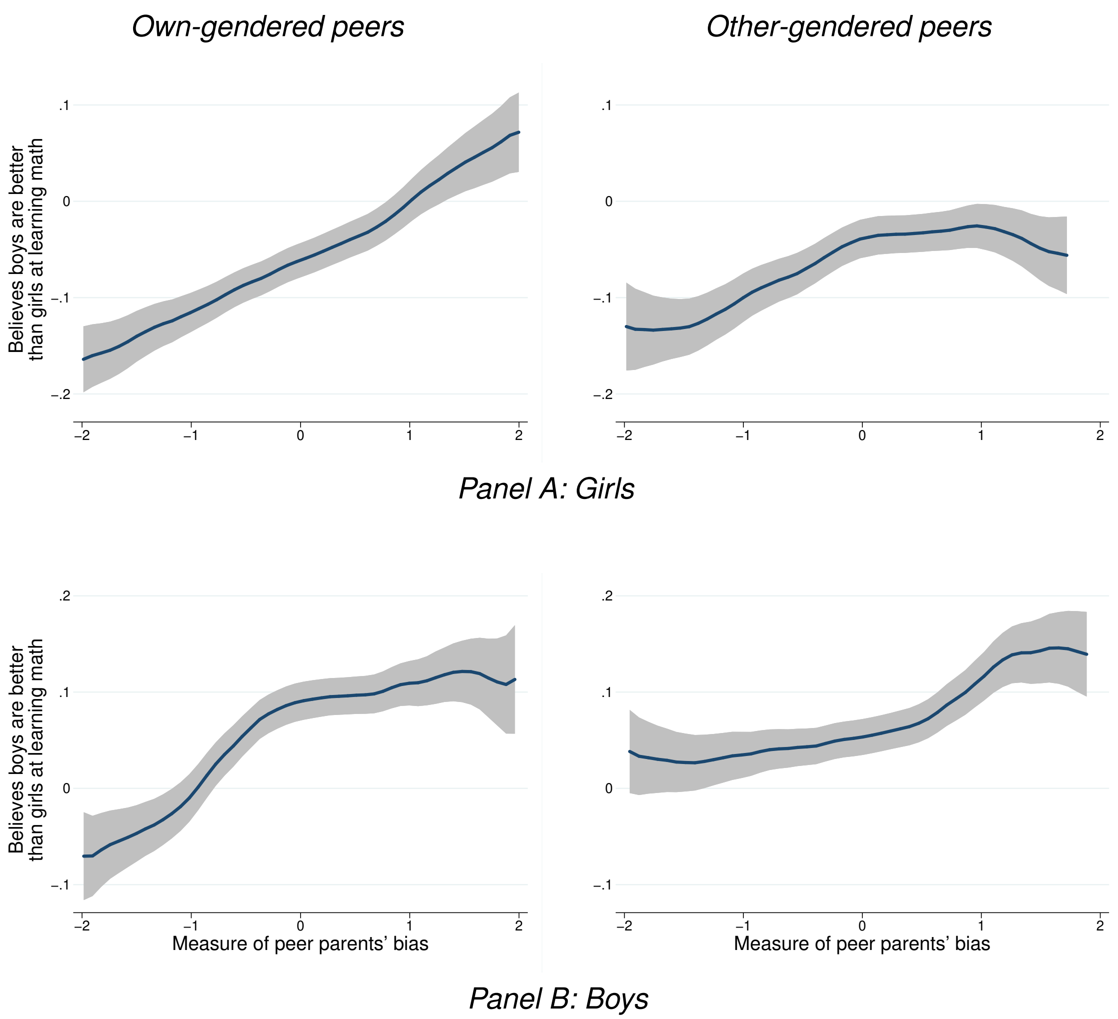
We next study how beliefs transmit across peers of different genders. For each child, we compute two class-specific measures of the proportion of parents reporting bias, one each for girl peers' and boy peers' parents, respectively. This is a test for homophily, the idea that children who share an identity (e.g., gender) are more likely to interact or serve as credible sources of information, and thus are more "influential" in the transmission of beliefs than children outside the identity group (Currarini et al., 2009). We present these results in Table 5. Our estimates confirm the predictions of homophily, i.e., that a child's beliefs are more affected by exposure to peers of the same gender whose parents are biased than by exposure to peers of the opposite gender whose parents are biased. While these results are slightly less precise than in Table 4, we show in Table A.4 that taking out the control for own parent's bias generates more precise homophily estimates whose magnitudes are similar to those shown in Table 5.

3.2 Performance on math examinations

Next, we study the effect of exposure to peers with biased parents on a child's performance in mathematics. Recall that the same midterm math test is administered across all classes within a grade, within a school. We standardize these test scores to be mean 0, SD 1, and, in Table 6, we present our results of estimating equation 1 with these math test scores as the dependent variable. Column 1 shows results for exposure to all peers with biased parents; columns 2 and 3 show homophily results for exposure to boy and girl peers with biased parents, respectively, akin to the specifications in Table 5.

As with perceived difficulty, our estimates of the effect of exposure to peers with biased parents on test scores differ in sign for girls and for boys. We estimate that a one SD increase in exposure to peers with biased parents worsens girls' performance, relative to boys', by 0.057 SD. In Figure 4, we show the analog to Figure 2 but for math test scores, plotting a kernel-weighted local polynomial regression of math test scores, after removing grade-by-school fixed effects from the score variable, on the proportion of peers with biased parents. This shows a similar monotonic relation-

Figure 3: Homophily in the effects of exposure to peers with biased parents on beliefs



Note: this figure shows the correspondence between exposure to peers with biased parents and children's own likelihood of reporting that boys are better than girls at learning math (1 = Yes, 0 = No), after removing grade-by-school fixed effects from the dependent (y-axis) variable (as in Figure 2). Here the four plots are divided by child gender (girls in the first panel, boys in the second) and the gender of the peers used to create the peer parents' bias measure (parents of own-gendered peers in the left column, and those of other-gendered peers in the right). A one unit increase in the peer parents' bias measure (the x-axis variable) corresponds to an 11 percentage point increase in the proportion of peers with biased parents.

Table 5: Evidence of homophily among peers

	Girl peers' parents' bias		*Boy peers' parents' bias*	
	(1)	(2)	(3)	(4)
	Believes boys are better than girls at learning math	Perceives math to be difficult	Believes boys are better than girls at learning math	Perceives math to be difficult
Gender-specific peers' parents' gender bias (PPB)	0.017	-0.005	0.043***	-0.006
	(0.014)	(0.015)	(0.011)	(0.012)
Gender-specific PPB x female	0.025*	0.020*	-0.015	0.005
	(0.014)	(0.011)	(0.014)	(0.008)
Own parent's gender bias (OPB)	0.282***	-0.066***	0.285***	-0.067***
	(0.016)	(0.016)	(0.016)	(0.016)
OPB x female	0.031	0.154***	0.027	0.156***
	(0.021)	(0.023)	(0.020)	(0.024)
Female	-0.131***	-0.013	-0.126***	-0.013
	(0.032)	(0.028)	(0.032)	(0.028)
Mean in sample	0.526	0.570	0.526	0.570
Number of observations	8,056	8,211	8,057	8,212

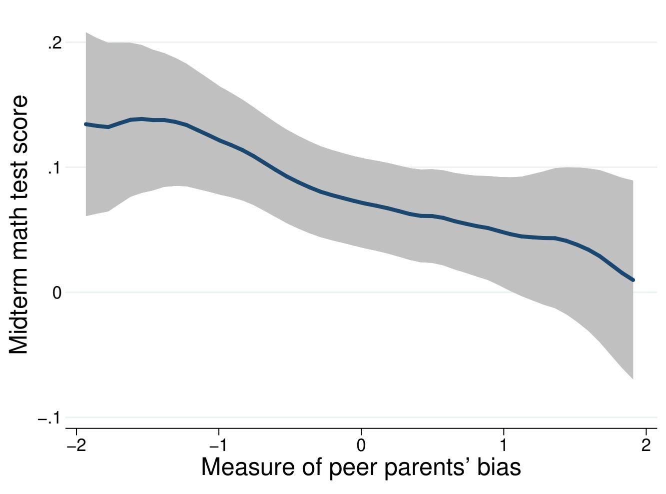
Note: this table shows results for estimating the effects of exposure to girl and boy peers with biased parents separately, as indicated in the two table headings. The dependent variables are coded 0 = No, 1 = Yes. In Table A.4, we show the analog to these results generated without own parent's bias on the right hand side.

Table 6: Effects on performance

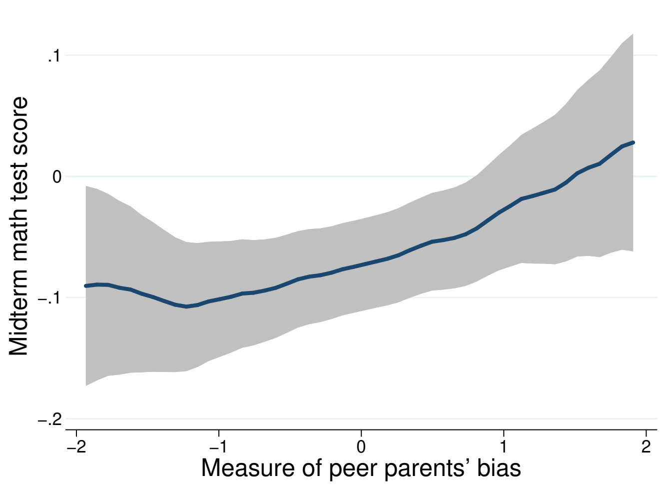
	(1) All peers' parents' bias	(2) Boy peers' parents' bias	(3) Girl peers' parents' bias
Peers' parents' gender bias (PPB)	0.039	0.081**	-0.054
	(0.039)	(0.037)	(0.037)
PPB x female	-0.057**	-0.045*	-0.023
	(0.025)	(0.023)	(0.025)
Own parent's gender bias (OPB)	0.169***	0.178***	0.172***
	(0.027)	(0.027)	(0.026)
OPB x female	-0.291***	-0.305***	-0.308***
	(0.041)	(0.042)	(0.043)
Female	0.355***	0.358***	0.357***
	(0.056)	(0.056)	(0.055)
Number of observations		8,028	

Note: in all regressions, the dependent variable is the student's test score on a midterm math test. The math test score variable is continuous and standardized to be mean 0, SD 1. The observations in this sample reflect all students for whom we have a math test score. Different columns pertain to different measures of peer parents' bias as labeled in the column headings. In Table A.5, we show the analog to these results generated without own parent's bias on the right hand side.

Figure 4: Non-parametric relationships between exposure to peers with biased parents and test scores, by gender



Panel A: Girls



Panel B: Boys

Note: this figure shows the correspondence between the individual-level measure of exposure to peers with biased parents and the child's performance on her midterm math test (mean 0, SD 1), again with grade-by-school fixed effects removed from the y-axis variable. Panel A presents this correspondence for girls in our sample, and Panel B presents it for boys. A one unit increase in the peer parents' bias measure (the x-axis variable) corresponds to an 11 percentage point increase in the proportion of peers with biased parents.

ship between exposure to peers with biased parents and children's performance in mathematics, with gains for boys and losses for girls.

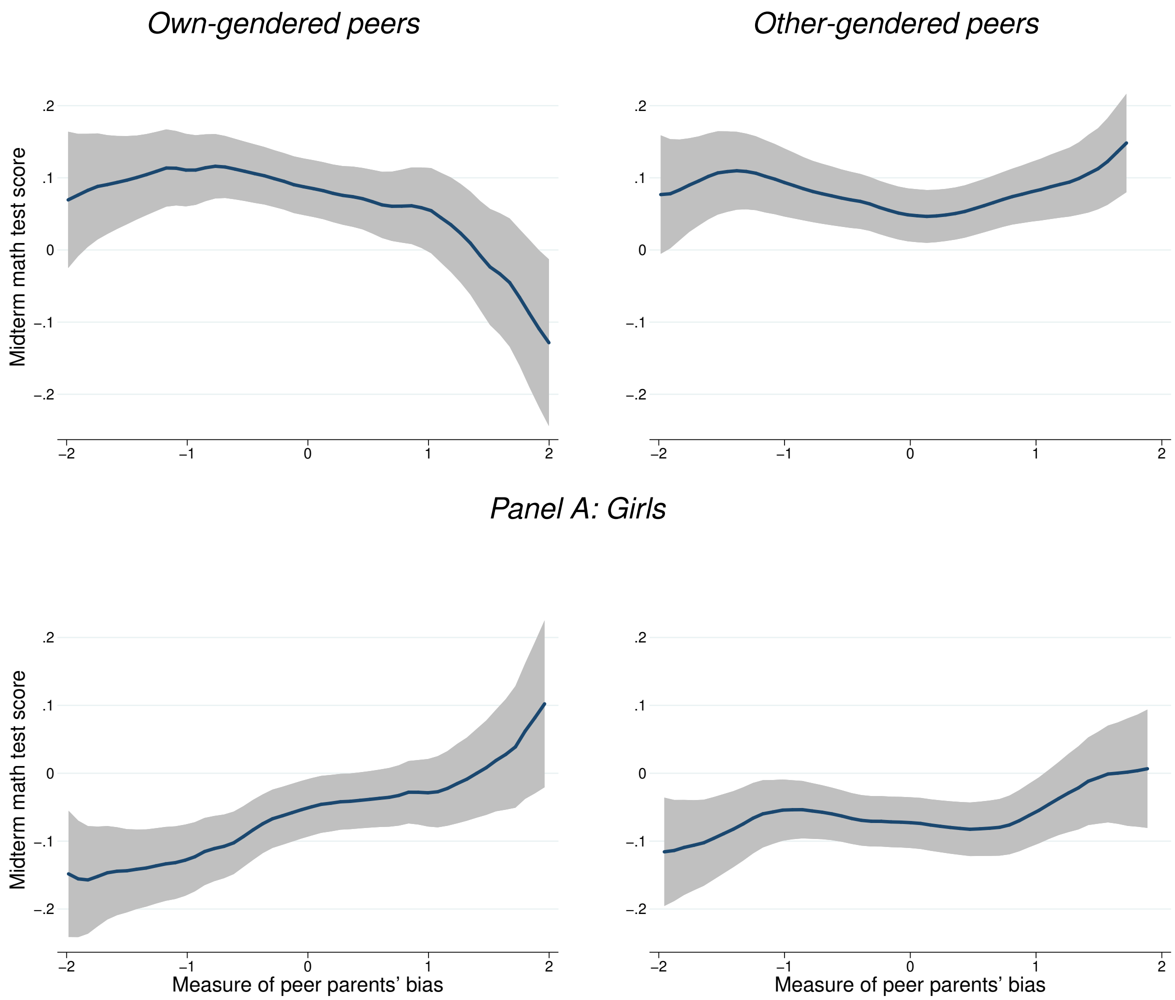
In columns 2 and 3 of Table 6, we see larger harms for girls from exposure to girl peers with biased parents than we do from exposure to boy peers, and larger gains for boys from exposure to boy peers with biased parents than from exposure to girl peers with biased parents. This result is similar to the relationships we find for belief transmission. In Figure 5, we show that this homophily also appears in the non-parametric analysis.

In these results we again see a large and significant correlation between own parent's bias and performance - the scores of boys whose parents believe that boys are better than girls at learning math are 0.17 SD higher than for boys whose parents do not believe this, and for girls, having a parent who holds this belief pushes the child's test score down, relative to boys' scores, by 0.29 SD. Finally, we see clear evidence of the "reverse" gender gap in math performance in spite of the prevalence of the belief that boys are better than girls at learning math: after conditioning on our set of controls and removing grade-by-school fixed effects, on average girls score 0.35 SD better than boys on these midterm math tests.

4 Interpreting our coefficient estimates

In this section, we address two potential issues in order to better understand the coefficient estimates presented in the previous section. First, in Section 4.1, we attempt to estimate the extent to which exposure to peers with biased parents is separate from, or coincident with, exposure to the other sources of peer effects studied in prior work (c.f., Sacerdote et al., 2011; Feld and Zölitz, 2017). Second, in Section 4.2, we explain the design and results of a test for reverse causality. The intuition for this test is that while we are interested in studying how children learn from their peers' parents, it may be also that parents learn about the relative math ability of girls and boys from observing their child's peers. Together, the results of these tests help guide our interpretation of the coefficients presented in Section 3.

Figure 5: Homophily in the effects of exposure to peers with biased parents on math test performance



Panel B: Boys

Note: this figure shows the correspondence between exposure to peers with biased parents and children's performance on midterm math examinations, again with grade-by-school fixed effects removed from the y-axis variable (which is in SD units). Here the four plots are divided by child gender (girls in the first panel, boys in the second) and the gender of the peers used to create the peer parents' bias measure (parents of own-gendered peers in the left column, and those of other-gendered peers in the right). A one unit increase in the peer parents' bias measure (the x-axis variable) corresponds to an 11 percentage point increase in the proportion of peers with biased parents.

4.1 Peer parents' bias vs. other peer effects

In this subsection, we attempt to disentangle two competing explanations for the results presented in Section 3. Explanation one is that a latent peer effect variable, as studied in many other analyses of peer effects (for example, peer ability or peer parent education; c.f. Sacerdote et al. 2011 and Feld and Zölitz, 2017), is driving our results and peer parents' bias is merely a good proxy for (or symptom of) it. Explanation two is that there is a direct channel of belief transmission from peers' parents to the child - either through a child's direct interaction with peers' parents (as in Olivetti et al., Forthcoming), through the child's interaction with peers, or both - which causes the differences in beliefs and math performance we measure in the previous section. This second explanation does not claim that other factors make no separate contribution to the transmission of beliefs or the gender gap in math performance, but rather that variation in the peer parents' bias measure, independent of the other known sources of peer effects and observable characteristics of parents who do and do not believe that boys are better than girls at learning math, generates the patterns we observe.

To disentangle these possible mechanisms, we conduct a series of horse race regressions where we add controls for other variables known to generate peer effects in prior work and study how our coefficient estimates change. These other sources of peer effects include peers' parents' education (Fruehwirth, 2017; Chung, 2018), peers' performance (as a proxy for their ability, c.f. Sacerdote et al., 2011; Feld and Zölitz, 2017), and the gender composition of the child's classroom (Hu, 2015). We add these controls, one at a time, to see how their inclusion affects our estimates of the impact of exposure to peers with biased parents. In this test, we study whether our estimates of the effects of exposure to peers with biased parents attenuate, which serves as a coarse measure of the extent to which the first explanation - that peer parents' bias is a proxy for a broader, latent peer effect measure - explains our results.

We present the results of this test in Table 7. Panel A shows that our estimates of the effects of exposure to more peers with biased parents on a child's beliefs vary little with the inclusion of additional controls for peer parent education, peer performance, and gender composition of the classroom. These results are consistent with the explanation that our estimates for the transmission of beliefs flow largely through the second channel - beliefs transmitting from peer parents

Table 7: Disentangling the effects of exposure to peers with biased parents and other sources of peer effects

	(1)	(2)	(3)	(4)
Panel A outcome: Believes boys are better than girls at learning math				
Peers' parents' gender bias (PPB)	0.046***	0.043***	0.041***	0.041***
	(0.012)	(0.012)	(0.012)	(0.012)
PPB x female	0.002	0.005	0.009	0.008
	(0.014)	(0.013)	(0.013)	(0.013)
R-squared	0.167	0.168	0.168	0.169
Panel B outcome: Midterm math test score				
Peers' parents' gender bias (PPB)	0.039	0.027	0.018	0.019
	(0.039)	(0.036)	(0.031)	(0.031)
PPB x female	-0.057**	-0.049**	-0.048**	-0.049**
	(0.025)	(0.024)	(0.024)	(0.024)
R-squared	0.193	0.197	0.199	0.199
Specification				
Baseline controls	X	X	X	X
Peers' parents' education		X	X	X
Peers' midterm math scores			X	X
Proportion of peers female				X

Note: this table shows a series of horse-race regressions, including additional independent variables as listed in the "specification" legend at the bottom of the table, to assess the relative importance of peer parents' bias and other determinants of peer effects in generating our estimates from Tables 4 and 6. The dependent variable in Panel A is coded as 0 = No, 1 = Yes (mean 0.526), and, in Panel B, the test score variable is in standardized SD units. There are 8,057 observations in the Panel A regressions and 8,028 in those of Panel B. In Table A.6, we show the analog to these results generated without own parent's bias on the right hand side.

to the child (either through peers and/or directly). Panel B shows more nuanced results. Adding the full battery of controls more than halves our estimates of the effect of exposure to peers with biased parents on boys' math performance. This is consistent with the many other papers showing that peer ability, peer parent education, and other factors matter for children's performance (e.g, Feld and Zölitz, 2017 and Fruehwirth, 2017). Our estimate of the gender gap (β_2), however, remains statistically significant and of similar magnitude. This suggests that exposure to peers with biased parents, and the changes in beliefs this generates, reduces girls' performance, relative to boys', independent of other peer effect mechanisms.

In Tables A.6-A.10 we provide several alternative versions of this table. Table A.6 shows these estimates when own parent's bias is removed from the estimating equation. Table A.7 shows these estimates for girl peer parents' beliefs (Table A.8 shows the girl peer parents' beliefs estimates when the own parent's bias variables are removed from the right hand side). Tables A.9 and A.10 show similar results for boys. The patterns they display are similar to those in Tables 3 and A.4: removing own parent's bias dampens the estimates of the effect of exposure to all peers with biased parents on child beliefs, but amplifies them for the homophily results, particularly for girls.

4.2 Addressing concerns of reverse causality

In this subsection, we study the extent to which a parent's beliefs vary with the relative performance of their child's boy and girl peers. The parental survey in the CEPS is administered after children are assigned to classrooms. As a result, parents will have observed the relative ability of boys and girls in the child's classroom when they are asked the question about girls' and boys' relative ability in math used to generate our peer parents' bias measure. If parents' beliefs adjust in response to their observation of the relative ability of boys and girls in their child's classroom, our estimates in the previous section would suffer from reverse causality.

We design an empirical test for this using our main estimating equation. We generate a leave-one-out average of the difference between the math test scores of the child's peer boys and peer girls, i.e., the gender gap among peers. We replace the peer parents' bias measure with the peer gender gap measure, and use each parent's response to the prompt, “do you believe that boys are better than girls at learning math,” as the dependent variable. This estimates how an increase

Table 8: Estimating the extent to which parents' beliefs change in response to the gender gap in performance among their child's peers

	Parent believes boys are better than girls at learning math
Gender gap in child's peers' test scores	0.004 (0.003)
Gender gap in child's peers' test scores x own child is female	-0.002 (0.003)
Own child is female	-0.035 (0.029)
Mean in sample	0.410
Number of observations	8,028

Note: this table presents a test for the possibility that a parent's beliefs are affected by the gender gap among her child's (randomly assigned) peers. Here, we generate a leave-one-out measure of the difference between boys' and girls' test scores in the midterm math exam for each child's peers in her randomly assigned classroom. Using the specification in Equation 1, we regress the child's parent's likelihood of believing boys are better than girls at learning math on this measure. Our results can reject that a 0.1 SD increase in boys' performance, relative to girls', generates anything larger than a 0.8 percentage point increase in a parent's likelihood of holding the belief.

in the peer gender gap in math performance in a child's classroom affects her parent's beliefs.

We present our results in Table 8. We find no evidence that a parent is more likely to report gender bias when her child is assigned to a classroom where boys outperform girls in mathematics. Our estimated coefficients are small and not distinguishable from zero, but precise: we can reject that a 0.1 SD increase in peer boys' performance, relative to peer girls, generates anything larger than a 0.8 percentage point change in the likelihood that a parent believes boys are better than girls at learning math (from a baseline of 41%). Taking these coefficients literally, our results suggest that even a large change in the relative gender performance in a child's peers' test scores does not generate more than a minute change in parent beliefs.

The model we use to interpret this fact - that children update their beliefs in response to exposure to peers whose parents hold a given belief, but parents don't appear to update their beliefs in response to the relative performance of their child's boy and girl peers - is one of simple Bayesian updating over time. At first, children's priors are very ripe for updating; they come into the world knowing little, and update their beliefs in response to the stimuli with which they come in contact. As they go through life, their priors are updated and become firmer. The period we study - middle school - is one where student priors about math ability are particularly likely to be updated, as the difficulty of math increases substantially. The priors of parents, on the other hand, have had 30 or so more years' worth of information with which to update. As a result, we would anticipate that a given dose of information is much less likely to affect parent beliefs than child beliefs, consistent with what we see in Table 8. These results, and the results of the test we report in Section 2 that a parent's beliefs are not affected by the beliefs of the other parents in her child's classroom, suggest that our coefficient estimates from Section 3 are unlikely to be meaningfully driven by reverse causality.

We interpret this section's results as evidence in favor of one interpretation - that plausibly exogenous variation in the proportion of peers whose parents believe that boys are better than girls at learning math has an impact on boys' and girls beliefs about the world and themselves and on their performance in mathematics. The evidence we present suggests that this effect is independent of other well-known sources of peer effects identified in the literature. Furthermore, we find no evidence that gender gaps in peer performance affect parental beliefs, i.e., reverse causality. As a whole, this suggests that, in this setting, the transmission of beliefs from peers' parents to children plays an important role in perpetuating, across generations, two phenomena: the persistent belief that boys are better than girls at learning math, and the gender gap in mathematics performance.

5 Heterogeneity and further impacts on girls

In this section, we conduct a series of analyses to investigate potential heterogeneity in the effects of exposure to peers with biased parents and provide a richer characterization of how this exposure affects girls' beliefs, aspirations, and performance in school.

5.1 Heterogeneity analyses

In this subsection we show results from three tests for heterogeneity and discuss the results of several others. First, we estimate the relationship between duration of exposure to peers and effect size. Next, we evaluate whether the effect of exposure to peers with biased parents varies by a child's own parent's reported beliefs. We then study whether having more friends in the child's randomly assigned classroom increases or dampens the effects of exposure to peers with biased parents. Finally, we discuss the results of tests for heterogeneity by income, parental education, and rural/urban residence.

How does the duration of time that a child is exposed to her peers condition our effect estimates? We have two sets of children in our data - seventh graders, who have been with their peers for three to six months when interviewed, and ninth graders, who have been with their peers for two more years than the seventh graders (recall that the children in our sample are assigned to a peer group in seventh grade and remain with them throughout middle school). Economic intuition generates disparate predictions for how increases in the duration of exposure to peers may condition our beliefs and performance estimates.

For beliefs, there are two predictions which point in opposite directions. Prediction one is consistent with a simple model of Bayesian updating. A child's priors are particularly ripe for updating when she enters seventh grade and encounters a large increase in the difficulty of the math curriculum. She updates her priors based on the new information she is confronted with - the curriculum, the overall middle school environment, and the information conveyed to her by her peers. Once that information is incorporated into her posterior, it is no longer "new" to her, and so we would expect only small subsequent changes in beliefs as exposure lengthens. Prediction two is that learning from peers may be a slow or iterative process, and so we would expect to see larger belief changes among the ninth graders than among the seventh graders.

For test scores, the predictions are clearer. Our intuition here comes from the fact that learning builds on itself. Initially, exposure to the message of the bias is likely to cause girls (boys) to exert marginally less (more) effort or enthusiasm for math, which leads to marginally worse (better) performance. This performance signal then provides information about the returns to subsequent effort, which can affect future enthusiasm/effort allocation decisions and lead to a cycle of effects compounding over time. This predicts larger performance effect estimates for ninth graders than for seventh graders.

In Table 9, we present coefficient estimates for three outcomes - believing that boys are better than girls at learning math, perceiving the current math class to be difficult, and the child's standardized midterm math test score - estimated separately by the grade the student is in[11]. For the beliefs estimates, our results are indeterminate: none of the coefficient estimates for seventh grade children are statistically distinguishable from those for ninth grade children, but their magnitude suggests the effects for girls may grow somewhat over time. As predicted, we estimate larger effects of exposure to peers with biased parents on both math test scores and perceived difficulty of math for those in grade nine than for those in grade seven.

Do the effects of exposure to peers with biased parents vary by the beliefs the child is exposed to in her own home? To answer this question, we add two variables to our estimating equation: the interaction of peer parents' bias and own parent's bias, and this variable's interaction with the child's gender. We present our results in Table 10. We find that increased exposure to peers with biased parents appears to generate greater harms for girls whose parents also believe that boys are better than girls at learning math, both in terms of the likelihood of the child holding that belief and her performance in mathematics. The standard errors in this analysis are large, however, suggesting that adding further interaction terms to our main specification pushes the limits of what we can precisely estimate using this estimation strategy and a dataset with this sample size.

We also test for the possibility that other predetermined factors might condition our results. For the sake of brevity, we describe the results but do not include the tables in the paper. We consider family income, parents' education, and whether the family lives in a rural or urban area (each variable and its interaction with child gender are already controls in our estimating equation). For the first two, we interact a dummy for either low income or low parental education, respectively, with peer parents' bias (and its interaction with child gender) to test for potential heterogeneity. Our main coefficient estimates do not differ substantially in magnitude, and the interaction coefficients are indistinguishable from zero. For the rural/urban comparison, we estimate results separately for urban and rural schools, as we do for seventh and ninth graders in Table 9. For this comparison

[11] As discussed in Footnote 6 and shown in Table A.2, the schools in our estimation sample with and without grade 9 classrooms that maintain the randomization initiated in grade 7 are balanced on most observable characteristics.

Table 9: Effect size by duration of exposure to peers

	Believes boys are better than girls at learning math		*Perceived difficulty of current math class*		*Math test score*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Grade 7	Grade 9	Grade 7	Grade 9	Grade 7	Grade 9
Peers' parents' gender bias (PPB)	0.038**	0.047***	0.008	-0.032*	0.040	0.070*
	(0.019)	(0.015)	(0.024)	(0.018)	(0.067)	(0.041)
PPB x female	-0.013	0.013	0.009	0.033***	-0.033	-0.080**
	(0.020)	(0.021)	(0.012)	(0.014)	(0.030)	(0.036)
Own parent's gender bias (OPB)	0.276***	0.303***	-0.057***	-0.077***	0.185***	0.141***
	(0.020)	(0.027)	(0.020)	(0.028)	(0.033)	(0.052)
OPB x female	0.023	0.030	0.135***	0.187***	-0.287***	-0.289***
	(0.026)	(0.032)	(0.028)	(0.040)	(0.053)	(0.065)
Female	-0.152***	-0.097*	-0.044	0.050	0.425***	0.192**
	(0.038)	(0.056)	(0.038)	(0.035)	(0.068)	(0.097)
Mean in sample	0.504	0.572	0.546	0.619	-	-
Number of observations	5,414	2,643	5,538	2,674	5,428	2,600

Note: This table presents results for the effect of exposure to peers with biased parents on children's beliefs and performance, estimated separately for those in grade seven and those in grade nine. The dependent variable in columns 1-2 are coded as 0 = No, 1 = Yes. In columns 3-4, the dependent variable is coded as 0 for low perceived difficulty and 1 for high perceived difficulty. In columns 5 and 6, the dependent variable is continuous with SD = 1. In Table A.11, we show the analog to these results generated without own parent's bias on the right hand side.

Table 10: The interaction of own and peer parents' biases

	(1) Believes boys are better than girls at learning math	(2) Midterm math test score
Peers' parents' gender bias (PPB)	0.055***	0.037
	(0.015)	(0.043)
PPB x female	-0.014	-0.038
	(0.016)	(0.028)
Own parent's gender bias (OPB) x PPB	-0.023	0.008
	(0.017)	(0.026)
OPB x PPB x female	0.042**	-0.053
	(0.020)	(0.034)
Own parent's gender bias (OPB)	0.285***	0.170***
	(0.016)	(0.027)
OPB x female	0.031	-0.292***
	(0.020)	(0.041)
Female	-0.130***	0.356***
	(0.032)	(0.056)
Mean in sample	0.526	-
Number of observations	8,057	8,028

Note: This table presents estimates of the effect of exposure to peers with biased parents on children's beliefs and performance, using our main specification with additional interaction terms one, between own parent's bias and our measure of peer parents' bias, and two, this interacted with child gender. The dependent variable in column 1 is coded as 0 = No, 1 = Yes. In column 2, the dependent variable is continuous with SD = 1.

as well, we find no evidence of meaningful heterogeneity in the effect estimates for either beliefs or performance in math.

5.2 How do girls respond to bias exposure?

We have shown that girls exposed to more peers with biased parents, ceteris paribus, are more likely to believe that boys are better at math than girls, are more likely to perceive math as difficult, and score worse on math exams. In this subsection, we try to distinguish between two potential responses girls may have to this information. The first response would be for girls to reallocate effort away from mathematics and towards other subjects or hobbies in which the bias suggests they have a comparative advantage. The second, alternative response is that increased exposure to the message that boys are better than girls at learning math may lead to a broader decrease in the student's beliefs about her own ability and performance. The clearest test for this is to look at the child's performance in other subjects; we show these results, estimating our main equation with children's Chinese and English test scores, in Table 11. We then discuss estimates for other variables, such as time use and self-confidence .

Our results can reject even small gains in girls' test scores for Chinese and English, and we conduct a series of other empirical tests to look for evidence of a reduction in enthusiasm or effort for girls. We find a small drop in girls' expressed confidence in their own future[12], but no evidence of any impact on time use (hours spent studying, in cram school, and on hobbies) or beliefs that math, English, or Chinese are helpful for their future. Together, our results suggest the second response - an overall downshift in enthusiasm or expectations among affected girls - may be more likely than the first. These results also suggest that the reversal in the gender gap in educational attainment, to say nothing of the reversal of the gender gap in math performance in Chinese middle schools, is all the more remarkable as it appears to have happened in spite of the downward pressure that exposure to peers with biased parents exerts on girls' academic performance, both in mathematics and other subjects.

[12]For the dependent variable in this analysis, we use a child's response to the question "how confident are you in your own future," with responses coded on a four point scale ranging for "not at all confident" to "very confident." Our estimate of β_2 is 0.06, with a p-value 0.087.

Table 11: The impact of exposure to peers with biased parents on Chinese and English test scores

	Midterm Chinese test score	Midterm English test score
Peers' parents' gender bias (PPB)	0.068	0.046
	(0.044)	(0.048)
PPB x female	-0.042	-0.037
	(0.027)	(0.026)
Own parent's gender bias (OPB)	0.024	0.054*
	(0.034)	(0.029)
OPB x female	-0.021	-0.054
	(0.045)	(0.040)
Female	0.549***	0.438***
	(0.085)	(0.090)
Number of observations	7,713	7,713

Note: this table shows an analog to column 1 of Table 6 for performance on standardized (mean = 0, SD = 1) Chinese and English midterm test scores. Note that we have slightly more missing scores for these tests than for mathematics.

5.3 The role of friends in mitigating harms from exposure to peers with biased parents

Last, we conduct an analysis building on the work of Lavy and Sand (Forthcoming) and Hahn et al. (2017), who show that proximity to friends, either in class or in a study group, has positive impacts on girls' academic performance. Our data collect information from the child on whether her five closest friends are in the same randomly assigned class as the child[13]. We add the number of friends in the class, its interactions with the child's own gender and our measure of peer parents' bias, and the triple interaction, as additional independent variables in our estimating equation. We study the estimated coefficients on these new explanatory variables to determine whether having friends in class affects beliefs or performance in our context. Note that this set of results is only suggestive, as our survey data on the number of friends inside or outside of the class are collected during the school year. As a result, who the child regards as one of her five closest friends is potentially endogenous to other factors, such as a child's overall experience in the school and the classroom, which may also affect, or be determined by, our outcome variables.

We present our results in Table 12, using the following four dependent variables: holding biased beliefs, perceived difficulty of math, aspirations to finish at least a BA, and midterm math test score. This analysis reveals substantial heterogeneity in the effect of exposure to peers with biased parents on girls' aspirations and performance. We find a much stronger negative effect on aspirations to complete college and on performance in mathematics (our estimates of β_2 in columns 3 and 4). These negative effects, however, capture the effect for children with no friends in their classroom. The [PPB x female x friends in Class] coefficient estimates show that the harms of exposure decrease as the number of friends the child has in her class increases. A child with five close friends in her class appears to be entirely immune to the negative effects of exposure to peers with biased parents on aspirations and math performance[14]. We see no evidence of heterogeneity by the number of friends in class in our estimates for the child's likelihood of holding bias or the perceived difficulty of math for girls, nor for any of the outcomes for boys.

[13]Unfortunately, we do not have access to friends' names or links to their identifiers and so cannot link a child's list of friends to other children in our dataset.

[14]To arrive at this conclusion, we take the [peer parents' bias x female] coefficient and add to it the [peer parents' bias x female x number of friends in class] coefficient multiplied by five, to capture the impact of all five friends being in the class.

Table 12: The moderating role of friends in class

	(1) Believes boys are better than girls at learning math	(2) Perceived difficulty	(3) Aspires to BA or higher	(4) Math test score
Peers' parents' gender bias (PPB)	0.034*	0.008	0.003	-0.009
	(0.018)	(0.019)	(0.023)	(0.054)
PPB x female	0.018	0.009	-0.037*	-0.142**
	(0.022)	(0.022)	(0.021)	(0.061)
PPB x number of friends in class (FIC)	0.004	-0.006	-0.000	0.016
	(0.004)	(0.005)	(0.004)	(0.010)
PPB x female x FIC	-0.005	0.003	0.015***	0.028*
	(0.007)	(0.006)	(0.006)	(0.015)
FIC	0.011**	-0.009*	0.014***	0.011
	(0.005)	(0.005)	(0.004)	(0.010)
FIC x female	-0.006	-0.004	-0.010*	0.006
	(0.007)	(0.006)	(0.006)	(0.013)
Own parent's gender bias (OPB)	0.289***	-0.068***	0.022	0.167***
	(0.016)	(0.016)	(0.014)	(0.027)
OPB x female	0.025	0.153***	-0.018	-0.288***
	(0.021)	(0.024)	(0.021)	(0.042)
Female	-0.108***	-0.004	0.131***	0.324***
	(0.040)	(0.035)	(0.029)	(0.074)
Mean in sample	0.526	0.570	0.658	-
Number of observations	7,890	8,024	8,006	7,850

Note: this table shows results from estimating equation 1 with the addition of four variables: number of close friends in the child's class (FIC), FIC x child gender, FIC x peer parents' bias, and FIC x peer parents' bias x child gender. The estimate on peer parents' bias now shows pertains to a child with no friends in her randomly assigned class. The FIC coefficients show the estimated effect of one additional friend being in the class. The dependent variables in columns 1 and 3 are coded as 0 = No, 1 = Yes. In column 2, the dependent variable is coded as 0 for low perceived difficulty and 1 for high perceived difficulty. In column 4, the dependent variable is continuous with SD = 1. In Table A.12, we show the analog to these results generated without own parent's bias on the right hand side.

Related work from sociology and psychology (Wentzel, 1998; Roseth et al., 2008) suggests a possible explanation for this pattern of results: friendship may increase children's resilience in the face of stressors such as being told that girls like you are worse than boys at learning math. This finding may provide (weak) evidence of the potential for outreach to vulnerable children, particularly girls, in minimizing the harm caused by the intergenerational transmission of gender bias. The greater problem to resolve, of course, is how to prevent this transmission altogether. This question is beyond the scope of the current research.

6 Conclusion

Gender bias persists across generations despite clear and growing evidence in contradiction of its main messages. Understanding how such bias is transmitted, the impacts of this transmission, and what can be done to prevent it, are issues of central importance to policymakers in both developed and developing countries. Our results document that the intergenerational transmission of biased beliefs occurs both within and across families and shed light on how these biases reinforce their messages by affecting children's beliefs about themselves and their performance in mathematics.

We know much less about how to address this phenomenon. We find that the negative effects of exposure to peers with biased parents on girls' performance and aspirations are ameliorated by the number of close friends the child has in her class, in line with prior work finding similar benefits to assigning girls to classes or study groups with their friends. In other work, we have shown that positive role models such as female math teachers can counter the message of gender bias for particularly vulnerable girls (Eble and Hu, 2017). The larger problem of how to prevent the transmission of beliefs such as the one we study, and the harmful effects this transmission can have on child development, remains unresolved.

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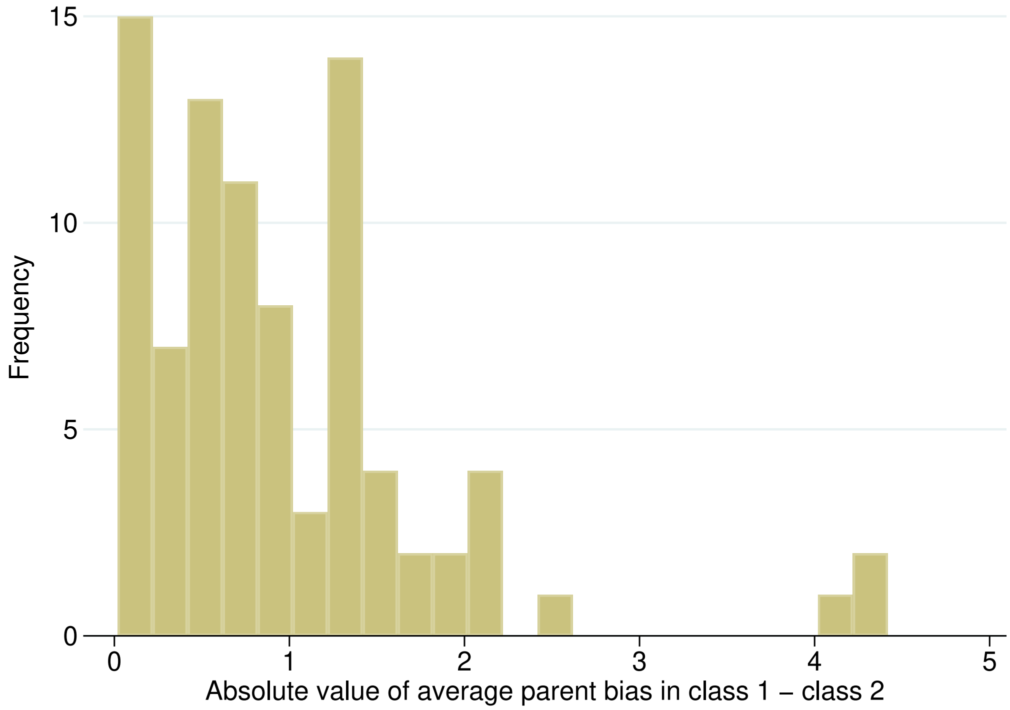
Appendix - for online publication only

Appendix figures

Figure A.1: Distribution of difference between average parent bias in within-school, within-grade class pairings



Note: this shows the distribution of the value $\| Bias_{c_1} - Bias_{c_2} \|$ across our 86 within-school, within-grade class pairs, where $Bias_{c_x}$ is the mean of all parents' responses to the question "do you believe that boys are better than girls at learning math" in class x.

Appendix tables

Table A.1: Balancing test conducted separately by grade

	(1) Grade 7	(2) Grade 9
Age	-0.075***	-0.006
	(0.029)	(0.013)
Holds agricultural hukou	-0.075	-0.017
	(0.066)	(0.036)
Number of siblings	-0.087**	-0.001
	(0.040)	(0.020)
Household is poor	-0.019	0.047
	(0.064)	(0.042)
Female	0.021	0.023
	(0.027)	(0.024)
Mother's highest credential		
Middle school	0.006	0.029
	(0.020)	(0.025)
High/technical school	0.008	0.068
	(0.022)	(0.054)
College or above	0.026	0.017
	(0.028)	(0.046)
Father's highest credential		
Middle school	-0.005	0.021
	(0.024)	(0.025)
High/technical school	0.000	0.021
	(0.036)	(0.058)
College or above	-0.008	0.076
	(0.036)	(0.075)
Ethnic minority	0.015	-0.063
	(0.023)	(0.050)
Number of observations	6,040	2,924
R-squared	0.73	0.60
Joint test F-statistic	0.68	1.22
[p-value]	[0.77]	[0.30]

Note: this table presents a balancing test, as in Antecol et al. (2015), which tests for our set of predetermined characteristics' joint ability to predict the peer parents' bias measure. Column 1 presents the results for seventh graders and Column 2 presents those for ninth graders. Both regressions include grade-by-school fixed effects. The variables are all coded as 0 = No, 1 = Yes, except for age and number of siblings. The dependent variable, peer parents' bias, is standardized to be mean 0, SD 1.

Table A.2: Characteristics of schools with and without randomized ninth grade classrooms

	(1) Full sample	(2) Does not have ninth grade classroom	(3) Has ninth grade classroom	(4) Difference (column 1 - column 2)	(5) p-value
Private school	0.09	0.10	0.09	0.01	0.89
Total number of students in a school	1024.84	1118.09	947.14	170.95	0.29
School ranking	3.95	4.12	3.80	0.32	0.05
Proportion of teachers with BA	0.81	0.76	0.86	-0.10	0.27
Number of teachers	87.38	88.85	86.04	2.81	0.79
Number of observations	86	41	45	—	—

Note: This table gives summary statistics of schools with and without ninth grade classrooms that maintain the randomization established in seventh grade. The only significant difference is that schools without ninth grade classrooms that maintain randomization are slightly higher ranked than schools who do have such classrooms. This pattern is consistent with the pattern that re-sorting of children by ability is regarded as a way for middle schools to improve the likelihood of sending top children to higher-ranked high schools, and school ranking partly reflects this placement record.

Table A.3: Analog to Table 4 - effects on beliefs

	(1) Believes boys are better than girls at learning math	(2) Perceives current math class to be difficult	(3) Aspires to complete at least a BA
Peers' parents' gender bias (PPB)	0.021	-0.018	0.002
	(0.016)	(0.014)	(0.017)
PPB x female	0.003	0.028***	0.004
	(0.015)	(0.009)	(0.010)
Female	-0.130***	0.025**	0.128***
	(0.018)	(0.011)	(0.011)
Mean in sample	0.526	0.570	0.658
Number of observations	8,709	8,885	8,845

Note: this table is the analog to Table 4 but excluding the own parent's bias variable and its interaction with student gender from the right hand side of the estimating equation. It shows results from estimating equation 1 using the dependent variable named in the column heading and described in the text. Variation in the number of observations across columns stems from differences in missing values for the dependent variables. The dependent variables are coded as 0 = No, 1 = Yes.

Table A.4: Analog to Table 5 - homophily in the transmission of beliefs

	Girl peers' parents' bias		*Boy peers' parents' bias*	
	(1)	(2)	(3)	(4)
	Believes boys are better than girls at learning math	Perceives math to be difficult	Believes boys are better than girls at learning math	Perceives math to be difficult
Gender-specific peers' parents' gender bias (PPB)	-0.007	-0.010	0.036***	-0.008
	(0.017)	(0.014)	(0.014)	(0.012)
Gender-specific PPB x female	0.033***	0.027***	-0.022	0.012
	(0.014)	(0.011)	(0.014)	(0.008)
Female	-0.135***	0.052**	-0.129***	0.053**
	(0.030)	(0.025)	(0.030)	(0.025)
Mean in sample	0.526	0.570	0.526	0.570
Number of observations	8,364	8,534	8,365	8,535

Note: this table is the analog to Table 5 generated by excluding the own parent's bias variable and its interaction with student gender from the right hand side of the estimating equation. It shows results from estimating equation 1 using the gender-specific peer parents' bias measure named in the first heading and the dependent variable named in the column heading (and described in the text). Variation in the number of observations across columns stems from differences in missing values for the dependent variables. The dependent variables are coded as 0 = No, 1 = Yes.

Table A.5: Analog to Table 6 - effects on performance

	(1) All peers' parents' bias	(2) Boy peers' parents' bias	(3) Girl peers' parents' bias
Peers' parents' gender bias (PPB)	0.045	0.074**	-0.040
	(0.040)	(0.037)	(0.038)
PPB x female	-0.080***	-0.053**	-0.049*
	(0.026)	(0.026)	(0.027)
Female	0.241***	0.235***	0.235***
	(0.052)	(0.051)	(0.050)
Number of observations		8,334	

Note: this table is the analog to Table 6 generated by excluding the own parent's bias variable and its interaction with student gender from the right hand side of the estimating equation. In all regressions, the dependent variable is the student's test score on a midterm math test (standardized to be mean 0 SD 1).

Table A.6: Analog to Table 7 - horse race regressions after removing own parent's bias

	(1)	(2)	(3)	(4)
Panel A outcome: Believes boys are better than girls at learning math				
Peers' parents' gender bias (PPB)	0.023	0.020	0.017	0.016
	(0.016)	(0.016)	(0.017)	(0.017)
PPB x female	0.001	0.004	0.007	0.006
	(0.014)	(0.014)	(0.014)	(0.014)
R-squared	0.082	0.084	0.083	0.084
Panel B outcome: Midterm math test score				
Peers' parents' gender bias (PPB)	0.447	0.313	0.224	0.225
	(0.396)	(0.358)	(0.306)	(0.306)
PPB x female	-0.801***	-0.714***	-0.695***	-0.709***
	(0.265)	(0.260)	(0.255)	(0.256)
R-squared	0.183	0.188	0.189	0.190
Specification				
Baseline controls	X	X	X	X
Peers' parents' education		X	X	X
Peers' midterm math scores			X	X
Proportion of peers female				X

Note: this table is the analog to Table 7 generated by excluding the own parent's bias variable and its interaction with student gender from the right hand side of the estimating equation. It shows a series of horse-race regressions, including additional independent variables as listed in the "specification" legend at the bottom of the table, to assess the relative importance of peer parents' bias and other determinants of peer effects.

Table A.7: Analog to Table 7 - horse race regressions using the girl peers' parents' bias measure

	(1)	(2)	(3)	(4)
Panel A outcome: Believes boys are better than girls at learning math				
Girl peers' parents' gender bias (PPB)	0.017	0.015	0.014	0.013
	(0.014)	(0.014)	(0.014)	(0.014)
Girl PPB x female	0.025*	0.028**	0.033***	0.033***
	(0.014)	(0.014)	(0.014)	(0.014)
R-squared	0.166	0.168	0.168	0.168
Panel B outcome: Midterm math test score				
Girl peers' parents' gender bias (PPB)	-0.544	-0.605	-0.536	-0.544*
	(0.374)	(0.373)	(0.327)	(0.323)
Girl PPB x female	-0.232	-0.136	-0.150	-0.156
	(0.247)	(0.251)	(0.248)	(0.246)
R-squared	0.194	0.198	0.200	0.200
Specification				
Baseline controls	X	X	X	X
Peers' parents' education		X	X	X
Peers' midterm math scores			X	X
Proportion of peers female				X

Note: this table is the analog to Table 7, but using the index for girl peers' parents' bias instead of that for all peers' parents. It shows a series of horse-race regressions, including additional independent variables as listed in the "specification" legend at the bottom of the table, to assess the relative importance of peer parents' bias and other determinants of peer effects.

Table A.8: Analog to Table 7 - horse race regressions using the girl peers' parents' bias measure and removing own parent's bias

	(1)	(2)	(3)	(4)
Panel A outcome: Believes boys are better than girls at learning math				
Girl peers' parents' gender bias (PPB)	-0.007	-0.008	-0.008	-0.008
	(0.017)	(0.017)	(0.017)	(0.016)
Girl PPB x female	0.033***	0.035***	0.039***	0.038***
	(0.014)	(0.015)	(0.014)	(0.015)
R-squared	0.082	0.084	0.084	0.085
Panel B outcome: Midterm math test score				
Girl peers' parents' gender bias (PPB)	-0.397	-0.462	-0.389	-0.394
	(0.376)	(0.374)	(0.326)	(0.322)
Girl PPB x female	-0.491*	-0.378	-0.390	-0.399
	(0.265)	(0.271)	(0.266)	(0.263)
R-squared	0.183	0.188	0.190	0.190
Specification				
Baseline controls	X	X	X	X
Peers' parents' education		X	X	X
Peers' midterm math scores			X	X
Proportion of peers female				X

Note: this table is the analog to Table 7, but using the index for girl peers' parents' bias instead of that for all peers' parents, and excluding the own parent's bias variable and its interaction with student gender from the right hand side of the estimating equation. It shows a series of horse-race regressions, including additional independent variables as listed in the "specification" legend at the bottom of the table, to assess the relative importance of peer parents' bias and other determinants of peer effects.

Table A.9: Analog to Table 7 - horse race regressions using the boy peers' parents' bias measure

	(1)	(2)	(3)	(4)
Panel A outcome: Believes boys are better than girls at learning math				
Boy peers' parents' gender bias (PPB)	0.043 ***	0.040***	0.039***	0.038***
	(0.011)	(0.011)	(0.011)	(0.011)
Boy PPB x female	-0.015	-0.012	-0.010	-0.010
	(0.014)	(0.014)	(0.014)	(0.014)
R-squared	0.166	0.168	0.168	0.168
Panel B outcome: Midterm math test score				
Boy peers' parents' gender bias (PPB)	0.808**	0.662*	0.545*	0.529*
	(0.374)	(0.351)	(0.298)	(0.298)
Boy PPB x female	-0.449*	-0.416*	-0.405*	-0.391*
	(0.232)	(0.229)	(0.227)	(0.230)
R-squared	0.194	0.198	0.199	0.199
Specification				
Baseline controls	X	X	X	X
Peers' parents' education		X	X	X
Peers' midterm math scores			X	X
Proportion of peers female				X

Note: this table is the analog to Table 7, but using the index for boy peers' parents' bias instead of that for all peers' parents. It shows a series of horse-race regressions, including additional independent variables as listed in the "specification" legend at the bottom of the table, to assess the relative importance of peer parents' bias and other determinants of peer effects.

Table A.10: Analog to Table 7 - horse race regressions, using the boy peers' parents' bias measure and removing own parent's bias

	(1)	(2)	(3)	(4)
Panel A outcome: Believes boys are better than girls at learning math				
Boy peers' parents' gender bias (PPB)	0.036 ***	0.032***	0.028**	0.026*
	(0.014)	(0.014)	(0.014)	(0.014)
Boy PPB x female	-0.022	-0.018	-0.016	-0.016
	(0.014)	(0.014)	(0.015)	(0.015)
R-squared	0.082	0.084	0.083	0.084
Panel B outcome: Midterm math test score				
Boy peers' parents' gender bias (PPB)	0.743**	0.587*	0.460	0.441
	(0.373)	(0.345)	(0.288)	(0.288)
Boy PPB x female	-0.534**	-0.496*	-0.482*	-0.468*
	(0.260)	(0.257)	(0.255)	(0.256)
R-squared	0.183	0.188	0.189	0.189
Specification				
Baseline controls	X	X	X	X
Peers' parents' education		X	X	X
Peers' midterm math scores			X	X
Proportion of peers female				X

Note: this table is the analog to Table 7, but using the index for boy peers' parents' bias instead of that for all peers' parents, and excluding the own parent's bias variable and its interaction with student gender from the right hand side of the estimating equation. It shows a series of horse-race regressions, including additional independent variables as listed in the "specification" legend at the bottom of the table, to assess the relative importance of peer parents' bias and other determinants of peer effects.

Table A.11: Analog to Table 9 - effects by duration of exposure

	Believes boys are better than girls at learning math		*Perceived difficulty of current math class*		*Math test score*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Grade 7	Grade 9	Grade 7	Grade 9	Grade 7	Grade 9
Peers' parents' gender bias (PPB)	0.004	0.028	0.004	-0.030**	0.391	0.858*
	(0.028)	(0.018)	(0.023)	(0.014)	(0.679)	(0.445)
PPB x female	-0.020	0.017	0.018	0.038***	-0.485	-1.229***
	(0.020)	(0.018)	(0.012)	(0.013)	(0.324)	(0.457)
Female	-0.163***	-0.083*	0.010	0.129***	3.150***	0.779
	(0.038)	(0.048)	(0.033)	(0.033)	(0.608)	(0.935)
Mean in sample	0.504	0.572	0.546	0.619	-	-
Number of observations	5,571	2,794	5,706	2,829	5,585	2,749

Note: this table is the analog to Table 9 but excluding the own parent's bias variable and its interaction with student gender from the right hand side of the estimating equation. It presents results for the effect of exposure to peers with biased parents on children's beliefs and performance, estimated separately for those in grade seven and those in grade nine. Those in grade nine have been exposed to their peers for two years longer than those in grade seven. The dependent variable in columns 1-2 are coded as 0 = No, 1 = Yes. In columns 3-4, the dependent variable is coded as 0 for low perceived difficulty and 1 for high perceived difficulty. In columns 5 and 6, the dependent variable is continuous with SD = 1.

Table A.12: Analog to Table 12 - the moderating role of friends in class

	(1) Believes boys are better than girls at learning math	(2) Perceived difficulty	(3) Aspires to BA or higher	(4) Math test score
Peers' parents' gender bias (PPB)	0.015	0.002	-0.002	-0.023
	(0.022)	(0.018)	(0.023)	(0.529)
PPB x female	0.018	0.016	-0.033	-1.588***
	(0.024)	(0.022)	(0.021)	(0.634)
PPB x number of friends in class (FIC)	0.002	-0.005	0.001	0.161
	(0.004)	(0.004)	(0.004)	(0.101)
PPB x female x FIC	-0.005	0.003	0.013**	0.267*
	(0.007)	(0.006)	(0.006)	(0.152)
FIC	0.012***	-0.009**	0.014***	0.087
	(0.005)	(0.004)	(0.004)	(0.097)
FIC x female	-0.009	-0.004	-0.010*	0.098
	(0.007)	(0.006)	(0.006)	(0.134)
Female	-0.106***	0.061*	0.122***	1.975***
	(0.040)	(0.032)	(0.027)	(0.719)
Mean in sample	0.526	0.570	0.658	-
Number of observations	8,182	8,329	8,311	8,137

Note: this table is the analog to Table 12 but excluding the own parent's bias variable and its interaction with student gender from the right hand side of the estimating equation. It shows results from estimating equation 1 with the addition of four variables: number of close friends in the child's class (FIC), FIC interacted with child gender, FIC interacted with peer parents' bias, and FIC interacted with peer parents' bias and child gender. Note that in this new specification, the estimate on peer parents' bias now shows the effect for a child with no friends in her randomly assigned class, and the FIC coefficients show the estimated effect of one additional friend being in the class or the interaction of this with other variables, as specified. The dependent variables in columns 1 and 3 are coded as 0 = No, 1 = Yes. In column 2, the dependent variable is coded as 0 for low perceived difficulty and 1 for high perceived difficulty. In column 4, the dependent variable is continuous with SD = 1.