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Gendered beliefs about mathematics ability transmit across generations through children's peers

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In many societies, beliefs about differential intellectual ability by gender persist across generations. These societal beliefs can contribute to individual belief formation and thus lead to persistent gender inequality across multiple dimensions. We show evidence of intergenerational transmission of gender norms through peers and how this affects gender gaps in learning. We use nationally representative data from China and the random assignment of children to middle-school classrooms to estimate the effect of being assigned a peer group with a high proportion of parents who believe that boys are innately better than girls at learning mathematics. We find this increases a child's likelihood of holding the belief, with greater effects from peers of the same gender. It also affects the child's demonstrated mathematics ability, generating gains for boys and losses for girls. Our findings highlight how the informational environment in which children grow up can shape their beliefs and academic ability.

Some important societal beliefs transmit across generations despite widely available information that directly contradicts these beliefs[1,2]. One such belief is the notion that men are inherently better than women at learning mathematics. This belief persists in many countries despite women often performing as well or better than men in K-12 maths assessments[3–6].

These societal beliefs contribute to individual belief formation and this linkage can be harmful if it affects children's beliefs about themselves or their effort in school[3,7–9]. Prior work has shown that strong cultural beliefs about gender are correlated with differential effort, enthusiasm and performance in school by gender[7,10]. To understand and address the under-representation of women in science, technology, engineering and mathematics (STEM) fields, it is therefore imperative to better understand what drives the transmission and persistence of societal beliefs about gendered mathematics ability and the impacts of this transmission on related educational outcomes[11].

In this paper, we show how beliefs about differential maths ability by gender transmit across generations through children's peers and affect girls' demonstrated maths ability, relative to boys'. First, we show that gendered beliefs formed in one generation pass on to the next through a child's peer group. Second, we estimate how this influences their actual maths ability, as demonstrated in school-by-grade level midterm maths exams. We find that this improves boys' test scores and harms that of girls', thus amplifying the message behind the belief.

We focus on how beliefs transmit from one generation to the next because this is an important means through which such beliefs persist. Children's beliefs are more malleable than those of adults' for two reasons: (1) because children have less experience than adults from which to judge the reliability of new information; and (2) because recent evidence finds that that children process new information through a lens of broader exploration about how the world works than do adults[12–15]. This implies that adults' beliefs are probably themselves formed in childhood, at a time of greater incidence of societal beliefs that privilege males over females[16,17]. This means that traditional societal beliefs can be passed on through generations even when they are contradicted by reality, as they are in the context we study.

Studying this transmission is fraught by both practical and ethical concerns. Although recent work has shown evidence that parents may transfer beliefs to their children[7,18], it is impossible to randomize parental exposure. Furthermore, it is not ethically justifiable to randomly expose some children to greater levels of these beliefs for the purpose of studying them because of existing evidence linking greater levels of these beliefs to greater disadvantage for girls[7,19]. To get around these problems, in this paper we use methods from the rich literature on peer effects and exploit the random assignment of children to classrooms, to generate quasi-experimental evidence of the transmission of societal beliefs about gendered maths ability from one generation to the next. We use the fact that the random assignment of children to classrooms generates random variation in the number of a child's classmates (which we call 'peers') whose parents believe that boys are inherently better than girls at learning mathematics. We use the statistical tools developed in quasi-experimental causal inference research, and in the peer effects literature specifically, to isolate the effect of exposure to a greater number of peers whose parents hold this belief, from the wide range of other peer effects.

Our setting has three features that make it well-suited to our study. First, the period of life we study is one ripe for belief updating. In China, the difficulty of the maths curriculum in middle school increases substantially from that of primary school[20]. This sudden increase in difficulty is likely to cause students to consider revising their beliefs about their own maths ability and those of each gender[21]. Second, the widespread use of random assignment of children to classrooms within schools in this context allows us to generate quasi-experimental estimates of this transmission, a phenomenon that previously has been estimated primarily using only correlational analysis[7,18]. This random assignment generates random variation in various student traits which can be analysed in a manner akin to analysis of a randomized control trial, with some

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modifications[22,23]. This strategy has been used to study the impacts of being assigned to peer groups with various traits, such as groups of peers with higher academic aptitude[23], a greater proportion of female peers[24] and groups of peers whose parents have higher levels of education[25]. Third, the belief we study—that boys' natural ability in mathematics is greater than that of girls—is commonly held among middle-school children and parents in China, despite the fact that girls outperform boys in the maths assessment data we observe.

Our data are from the China Education Panel Survey (CEPS), a nationally representative survey of Chinese middle-school students from 112 schools. The survey team surveyed all students in two randomly selected seventh-grade classes and two randomly selected ninth-grade classes in each school. We use CEPS data from students, their teachers and their parents. These include administrative records of the child's academic performance in mathematics as well as each child's responses to a survey about their beliefs. The parent data include demographic data and similar responses to a survey on parent beliefs. We present summary statistics of our sample by child gender in Supplementary Table 1 and by parent beliefs in Supplementary Table 2. We report tests of the randomization of children to classrooms in Supplementary Table 3, which fail to reject the null that children were randomly assigned to classrooms within a school.

The CEPS asks a parent of each child whether or not they agree with the statement 'boys' natural ability in studying mathematics is greater than that of girls'. For shorthand we will refer to this as believing that $B_m > G_m$, where B_m represents boys' innate maths ability and G_m represents that of girls. We use this question to generate a child-level leave-one-out measure that captures, for each child, the proportion of randomly assigned peers in their classroom whose parents report believing that $B_m > G_m$. In our data, ~41% of parents agree with the statement, while the remaining 59% disagree; we do not observe whether this group sees boys' and girls' ability as equal, they think girls' ability is superior or do not know. Our measure of the proportion of a child's peers whose parents believe that $B_m > G_m$ ranges from zero to 83.3%, with a standard deviation of 11.2%. From here onward, we refer to this variable as our 'measure of peer parent beliefs'. Our use of this question builds upon theory and evidence from other work showing that this particular societal belief may influence individual children's beliefs, habit formation, career choice and decisions of effort and enthusiasm that often reinforce the message sent by the belief[8,19,26–28].

We also construct two additional, related measures: one measure for the beliefs of girl peers' parents and a separate measure for those of boy peers. We use this to test a prediction motivated by the notion of 'homophily', the tendency for individuals to associate with like others[29–31]. Homophily predicts that we should observe larger effects for exposure to own-gendered peers whose parents believe that $B_m > G_m$ than for other-gendered peers. This means that variation in the beliefs of boy peers should have a greater effect on a boy than on a girl and vice versa.

Given the random assignment of children to classes, we can use ordinary least squares regression analysis to recover how being exposed to more peers whose parents believe that $B_m > G_m$ affects a given child's beliefs and performance in mathematics[22]. Our empirical approach can be represented using the following equation:

$$Y_{icgs} = \beta_0 + \beta_1 P_{icgs} + \beta_2 P_{icgs} F_{icgs} + \beta_3 O_{icgs} + \beta_4 O_{icgs} F_{icgs} + \beta_5 \mathbf{C}_{icgs} + \eta_{gs} + \epsilon_{icgs}$$

In this equation, Y_{icgs} refers to the outcome of interest for child i in class c in grade g in school s. P_{icgs} is the proportion of child i's peers in their classroom whose parents believe that $B_m > G_m$, standardized to be mean 0, s.d. 1. As mentioned earlier, we use three different proportions: the proportion for all peers' parents, for those of boy peers only and for those of girl peers only. F_{icgs} is an indicator for the child being female. O_{icgs} is an indicator for whether the child's own parent believes that $B_m > G_m$, following other recent work studying peer parent traits which include the own child's parent's trait[10]. $\mathbf{C}_{icgs}$ is a vector of controls. This includes characteristics specific to the child: rural versus urban household residency (hukou) status, mother's years of education, father's years of education, household income (a 0/1 variable for being classified as 'poor' by the school), proxies for the child's academic ability before entering school ((1) results from a cognitive skills test taken outside of school at the beginning of the study; and (2) the child's maths performance in the final grade of primary school, before entering middle school), the child's ethnicity, their number of siblings and all these interacted with the child's gender. It also contains teacher characteristics: specifically, teacher gender; teacher gender interacted with child gender; years of teaching experience; type of degree; and receipt of various teaching awards. We also control for teacher beliefs. While we do not have data on teachers' beliefs about the innate ability of boys relative to girls, as we have for children and parents, we do observe a separate and useful variable. Specifically, we observe teachers' response to the question: 'what is the relationship between a student's innate ability and their grades?' There are three response options: 'basically no relationship', 'some relationship' and 'a very close relationship'. We create an indicator variable equal to one if the maths teacher answers 'a very close relationship' and zero otherwise and add this as an additional control. η_{gs} is a grade-by-school fixed effect (we observe two classrooms per grade per school; on average there are seven classrooms in a given grade in a given school) and ϵ_{icgs} is a standard error, clustered at the grade-by-school level.

Our main coefficients of interest are β_1 and β_2. The coefficient β_1 captures the impact of a 1 s.d. (11.2 percentage point) increase in the proportion of peers whose parents believe that $B_m > G_m$ on boys' outcomes. The coefficient β_2 captures the impact of a 1 s.d. increase in this measure of peer parent beliefs on girls' outcomes, relative to boys; equivalently, this is the impact of peer parent beliefs on the 'gender gap' in the outcome being studied. The overall effect of exposure to peers whose parents believe that $B_m > G_m$ on girls' outcomes is captured by $\beta_1 + \beta_2$. We estimate β_1 and β_2 for the overall measure of peer parent beliefs, as well as for the measure of boy peers' parents' beliefs and girl peers' parents' beliefs, respectively.

We estimate this equation for two main outcome variables. The first is a child's likelihood of believing that $B_m > G_m$ based on a survey question similar to the one administrated to parents, referring to beliefs about the innate maths ability of each gender, not just the relative performance of boys and girls in the child's current school or class. The second is a child's midterm maths test score, which is taken from administrative data recording the child's performance on the maths exams given by all schools in the middle of the autumn semester as part of the regular assessment schedule in core subjects required of all middle schools. All students within a grade, within a school, take the same midterm test in each core subject (mathematics, Chinese, English), which is graded centrally at the school level on an absolute scale. These scores go into students' permanent academic records and capture the student's demonstrated maths ability as it is observed by their school and teachers.

We prefer these administrative test score data to data from a higher-stakes exam, such as the college entrance exam, for two reasons: (1) because there is substantial evidence to suggest that in more competitive or evaluative situations socially stigmatized groups may perform worse than their true ability[9,32]; and (2) that girls specifically may underperform relative to boys because boys have less aversion to competitive situations[33–35]. In the current study, we are interested in measuring children's acquisition of skill in mathematics during the normal course of their education, not their performance in highly competitive situations per se. While our estimates

Table 1 | Mapping of exposure to peers whose parents believe that $B_m > G_m$ onto child beliefs

Outcome variable—child believes boys are innately better than girls at learning mathematics			
	(1)	(2)	(3)
	All peer parents' beliefs	Boy peer parents' beliefs	Girl peer parents' beliefs
Peer parent beliefs	0.045	0.040	0.018
	(0.022, 0.068)	(0.018, 0.063)	(−0.011, 0.047)
	0.000	0.001	0.230
PPB × female	0.008	−0.007	0.025
	(−0.019, 0.035)	(−0.034, 0.021)	(−0.001, 0.051)
	0.551	0.640	0.057
Own parent beliefs (OPB)	0.275	0.275	0.271
	(0.245, 0.306)	(0.244, 0.305)	(0.240, 0.301)
	0.000	0.000	0.000
OPB × female	0.020	0.016	0.023
	(−0.020, 0.060)	(−0.023, 0.056)	(−0.018, 0.064)
	0.332	0.415	0.273
Female	−0.308	−0.302	−0.309
	(−0.374, −0.243)	(−0.367, −0.237)	(−0.376, −0.243)
	0.000	0.000	0.000
R^2	0.186	0.185	0.185
Observations	8,057	8,057	8,056

This table shows results for estimating the effects of exposure to peers whose parents believe that $B_m > G_m$ on the child's likelihood of holding the belief. We estimate this for three types of peer, as indicated in the column headings. The dependent variable is coded 0 for 'no' and 1 for 'yes'. For each estimate we present, we provide the coefficient estimate first, with the 95% CI underneath it in parentheses and the relevant P value underneath the CI.

look at differential response to exposure after controlling for gender, differential gender response to competitive situations[31,33] could also interact with the exposure we study, generating even greater effects in more competitive situations. We test for this in the Methods.

Results

We first show how being assigned to a greater number of classmates whose parents believe that $B_m > G_m$ affects whether or not a child holds the belief. We show these results in Table 1. Different columns pertain to different measures of peer parent beliefs as labelled in the column headings.

In the first column of Table 1, we estimate that a 1 s.d. increase in the peer parent belief measure is associated with a 4.5 percentage point increase in the likelihood that a child will believe that $B_m > G_m$ themselves (95% confidence interval (CI) 0.022 to 0.068; $P < 0.001$). The estimated relationship between own parent beliefs and child beliefs is also highly significant and has the same sign (positive) as the causal estimate of being exposed to more peers whose parents hold this belief.

In columns 2 and 3 of Table 1, we show how these patterns vary with exposure to peers of different genders. These results show that own-gendered peers have a greater impact on a child's likelihood of holding the belief than do other-gendered peers. In column 2, we see that the effect of exposure to boy peers whose parents believe that $B_m > G_m$ on boys is similar but the sign is negative for the point estimate for girls (peer parent beliefs × female). In column 3, we see two patterns: (1) the estimate for boys (peer parent beliefs) diminishes in magnitude by more than half; and (2) the interaction of peer parent beliefs × female is positive and on the margin of significance (CI −0.001 to 0.051; $P = 0.057$). The overall effect for girls, the sum of the coefficients on the peer parent beliefs (PPB) and PPB × female variables ($\beta_1 + \beta_1$), is 0.043 and highly statistically significant ($F_{1,123} = 9.36$, $P = 0.003$).

Overall, these patterns are consistent with a stronger relationship from own-gendered peers than from other-gendered peers, consistent with the predictions of homophily[31]. We show these three relationships graphically in Fig. 1, plotting a kernel-weighted local polynomial regression of child beliefs on the peer parent beliefs measure. The left column shows results for girls and the right column for boys. There are three rows in the figure: row 1 shows results for exposure to all peers (similar to column 1 in Table 1) and rows 2 and 3 show results for exposure to own-gendered peers and other-gendered peers, respectively. We see a positive gradient across all plots. The plots show visibly steeper gradients from exposure to own-gendered peers (second row) than from exposure to other-gendered peers (third row). This mirrors the patterns we see in our regression results.

Next, we show how this exposure affects children's demonstrated maths ability. In Table 2, we present regression results with children's midterm maths test scores as the dependent variable. The presentation of results mirrors that of Table 1: column 1 in Table 2 shows results for exposure to all peers whose parents believe that $B_m > G_m$; columns 2 and 3 show results for exposure to boy and girl peers whose parents hold the belief, respectively.

We estimate that a 1 s.d. increase in exposure to all peers whose parents hold the belief is associated with a non-significant decline in girls' demonstrated maths ability (as measured by the midterm test score) relative to boys', by 0.044 s.d. (95% CI −0.092 to 0.004; $P = 0.071$). The signs on our estimates are consistent with the idea that exposure to these peers raises boys' scores and lowers those of girls, further reinforcing the message behind the belief but these estimates are only marginally significant and therefore not informative for the hypotheses. The estimates in column 2 and 3 show even more clearly that exposure to own-gendered peers has a much larger effect on children's outcomes than exposure to other-gendered peers. A 1 s.d. increase in exposure to boy peers whose parents hold this belief is associated with a marginally significant increase in

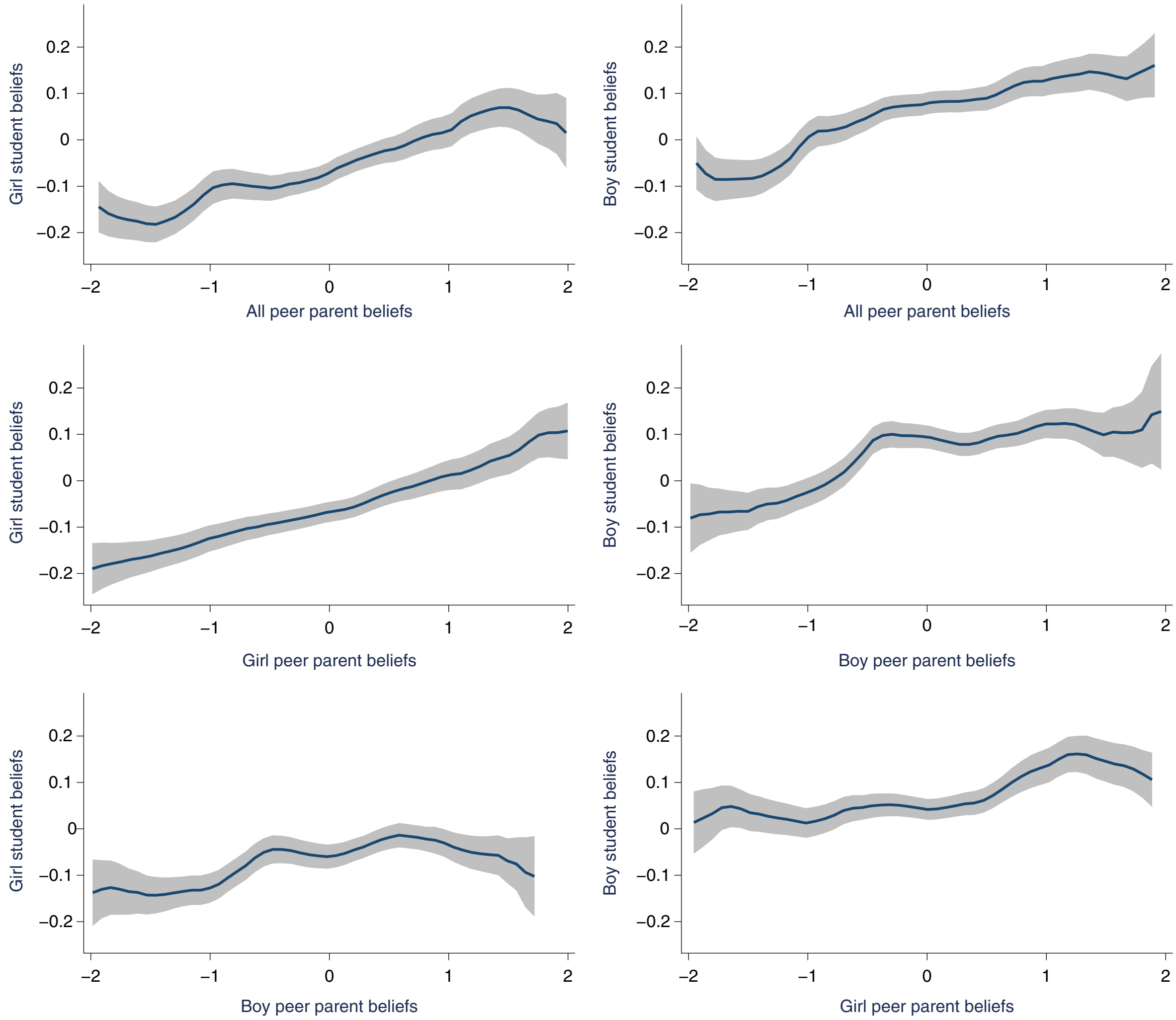

Fig. 1 | Mapping of exposure to peers whose parents believe that $B_m > G_m$ onto child beliefs. This shows a kernel-weighted local polynomial regression and its 95% CI of a child's likelihood of reporting that boys are better than girls at learning mathematics (0, no; 1, yes) on the proportion of peers whose parents hold that belief (the *x* axis variable), after removing grade-by-school fixed effects from the dependent (*y* axis) variable. The *x* axis variable is shown in s.d. terms; a 1 s.d. change means that 11.2% more of peer parents hold the belief; at 0 s.d., 41% of parents hold the belief. The six plots are divided by child gender (girls in the left column, boys in the right) and the gender of the peers used to create the peer parent beliefs measure (all peers in the first row, own-gendered peers in the second and other-gendered peers in the third. A one unit increase in the peer parent beliefs measure (the *x* axis variable) is equivalent to an 11 percentage point increase in the proportion of own- or other-gendered peers whose parents believe that $B_m > G_m$. We trim the sample to include only children whose value for peer parent beliefs falls in the range [−2,2]; this drops 317 observations from the original sample of 8,057 used to generate the corresponding results in Table 1.

boys' test scores by 0.064 s.d. (column 2; 95% CI −0.003 to 0.131; $P = 0.063$). Finally, we estimate that a 1 s.d. increase in exposure to girl peers whose parents hold this belief has a roughly symmetric, negative effect on girls: the sum of the coefficients on the PPB and PPB × female variables ($\beta_1 + \beta_2$) is −0.074 and this estimate is statistically significant ($F_{1,123} = 6.04$, $P = 0.015$). The estimates for exposure to other-gendered peers are substantially smaller for both genders. Again, the sign of the own parent beliefs estimates mirror those of the peer parent beliefs measures, giving further assurance that we are measuring similar phenomena.

In Fig. 2, we show kernel-weighted local polynomial regressions, similar to those in Fig. 1 but regressing child test scores on exposure to peers whose parents believe that $B_m > G_m$. The gradients in these plots reflect the regression results. For girls (in the left column) we see a negative relationship between exposure and test scores; for boys we see a positive relationship. We again see steeper gradients in the own-gendered plots (second row) than in the other-gendered plots (third row), further evidence in support of the predictions of homophily, that own-gendered peers are more influential than other-gendered peers in the transmission of these beliefs and their effects on demonstrated maths ability.

Our final analysis in this section addresses an important alternative explanation for these results: the estimates we present for our key independent variable, peer parent beliefs, are primarily driven by the broader, latent peer effects studied in other work[22,36] and not the effect of exposure to peers whose parents hold these specific beliefs per se.

To investigate this possibility, we estimate whether there is sufficient variation in the peer parent beliefs measure, independent of these other, well-known sources of peer effects, to generate the results we measure. The other sources of peer effects we study include traits of peers' parents, including education, income and family background[10,25]; the gender composition of the child's classroom[24]; and peers' cognitive ability[22,36].

Table 2 | Effects on demonstrated maths ability

Outcome variable—child's midterm maths score

	(1)	(2)	(3)
	All peer parents' beliefs	**Boy peer parents' beliefs**	**Girl peer parents' beliefs**
Peer parent beliefs	0.032	0.064	−0.053
	(−0.040, 0.103)	(−0.003, 0.131)	(−0.120, 0.015)
	0.382	0.063	0.127
PPB × female	−0.044	−0.029	−0.021
	(−0.092, 0.004)	(−0.073, 0.015)	(−0.066, 0.024)
	0.071	0.196	0.354
OPB	0.139	0.147	0.141
	(0.087, 0.190)	(0.096, 0.198)	(0.090, 0.191)
	0.000	0.000	0.000
OPB × female	−0.238	−0.251	−0.252
	(−0.316, −0.159)	(−0.329, −0.172)	(−0.333, −0.172)
	0.000	0.000	0.000
Female	0.274	0.272	0.277
	(0.136, −0.413)	(0.134, 0.411)	(0.140, 0.413)
	0.000	0.000	0.000
R^2	0.284	0.285	0.285
Observations	8,028	8,028	8,027

In all regressions, the dependent variable is the child's test score on a midterm maths test centrally administered by the child's school. The maths test score variable is continuous and standardized to be mean 0, s.d. 1. The observations in this sample reflect all children for whom we have a maths test score. Different columns pertain to different measures of peer parent beliefs as labelled in the column headings. For each estimate we present, we provide the coefficient estimate first, with the 95% CI underneath it in parentheses and the relevant *P* value underneath the CI.

We operationalize these tests with a series of 'horse race regressions' in which we add these controls one at a time and study how the magnitude and precision of our estimates of β_1 and β_2 vary. If the magnitude and precision of these estimates persist after adding controls for other known contributors of peer effects, this would indicate that variation in PPB, independent of these other known peer effect sources, is driving the results presented in Tables 1 and 2 and Figs. 1 and 2 (refs. [37,38]).

We present these results in Table 3 where outcomes A and B follow the results in column 1 of Tables 1 and 2, respectively. Outcome A shows that our estimates of the effects of exposure to more peers whose parents believe that $B_m > G_m$ on a child's beliefs vary little with the inclusion of additional controls for various peer parent traits, the gender composition of the classroom and the average cognitive ability of peers. Outcome B shows similar results for the stability of our estimates for maths test scores. Together, these show that our main estimates in Tables 1 and 2 and Figs. 1 and 2 are driven by variation in PPB, independent of other common sources of peer effects.

Robustness of results. In this section, we address the potential for parent beliefs to be affected by the characteristics of the classroom their child was assigned to. A weakness of our data is that the parent survey was administered after the child was randomly assigned to a classroom. This means that, potentially, our estimates of β_1 and β_2 could suffer from reverse causality or omitted variables bias. We first discuss existing empirical and theoretical work on the formation of beliefs among children and adults, which suggests that children's beliefs are far more malleable than those of adults. We then directly test for three possible alternative explanations for our results that involve parents changing their beliefs. The first possibility is reverse causality: that randomly occurring differences between the maths ability of boys and girls in a given classroom affects parent beliefs. The second is a flavour of the 'reflection problem' in which parents' beliefs could be affected by the beliefs of other parents in their child's randomly assigned classroom. The third is that the gender of the maths teacher the child is randomly assigned to may also affect parent beliefs about the relative maths ability of boys and girls. We find no empirical evidence for any of these channels in our data.

Several pieces of recent research studying the formation of beliefs during the life course find that parent beliefs are more stable or less likely to change in response to new information than those of children[12–15]. This is due to two factors. First, parents have decades more of life experience than children do, during which they have gathered information to form their beliefs. Seen through the lens of Bayesian updating, this suggests that, in response to new information, any update to beliefs from a given packet of information is likely to be much smaller for parents, whose priors are 'firmer', than for children, whose priors are more diffuse[13,14]. Second, laboratory studies of how children and adults process information show that children take an approach that is more exploratory, with the goal of learning how the world works, as compared with adults, who take an approach that is more context-specific[12,15]. Together, this suggests that adults' beliefs are far less likely to update than children's. Furthermore, parents in our context also spend very little time with the peers of the child, particularly as compared to the child, who spends most waking hours in school with their classmates. This means that the child is also getting, in absolute terms, more 'information' from their random assignment to a classroom than the parent is; seen through the same lens of Bayesian updating, this too predicts that parents are less likely to change their beliefs in response to the composition of the child's classroom than is the child.

We can also test these hypotheses empirically. Using our main estimating equation, we implement two empirical tests to evaluate the extent to which parents' beliefs may respond to the relative maths ability of the other children in their child's randomly assigned classroom. In both tests, the dependent variable is an indicator variable for whether the parent believes that $B_m > G_m$. In the first test, our main explanatory variable is the difference between the maths

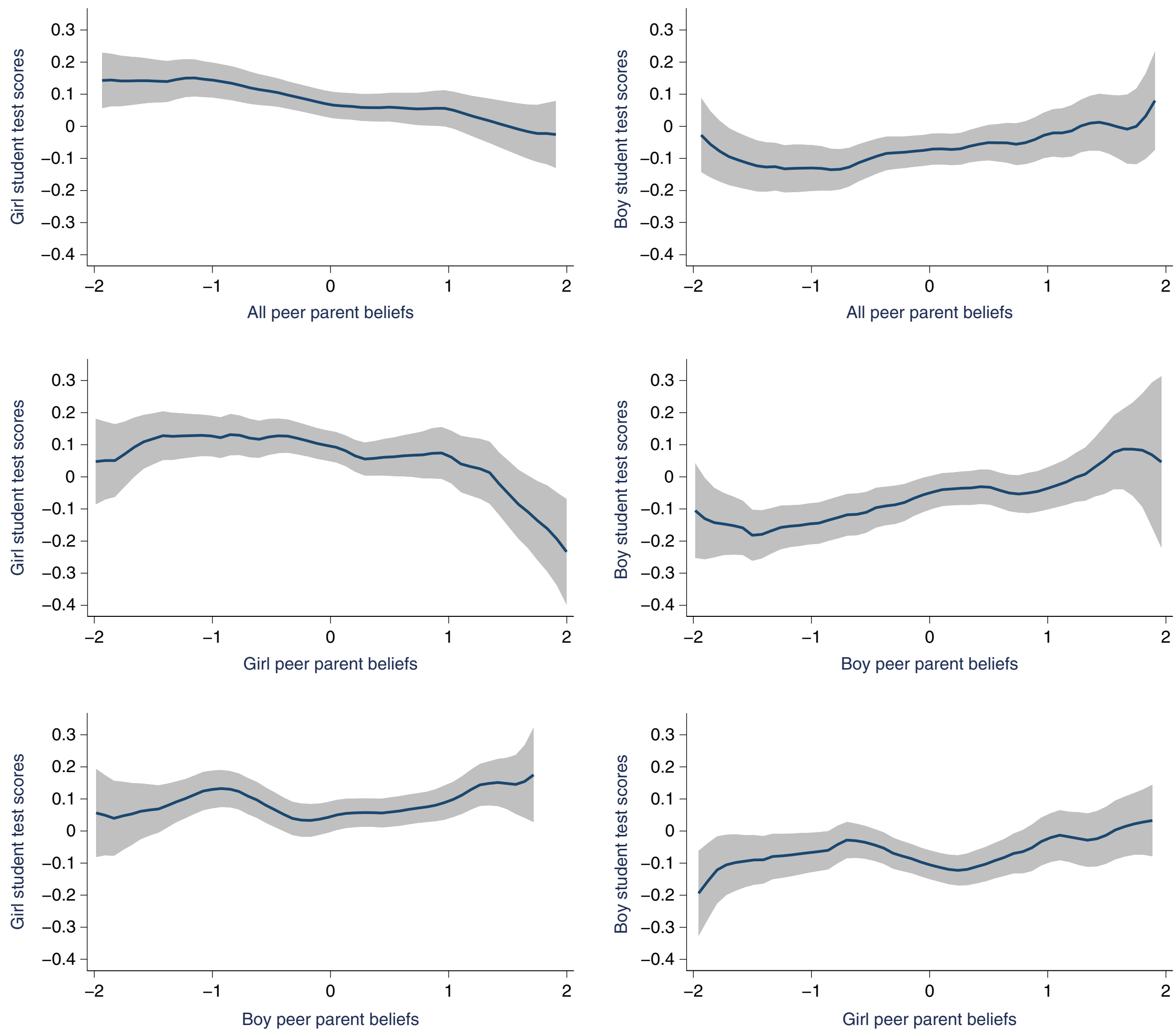

Fig. 2 | Mapping of exposure to peers whose parents believe that $B_m > G_m$ onto test scores. This is an analogue to Fig. 1; it shows a kernel-weighted local polynomial regression and its 95% CI of a child's (standardized) maths test score on the proportion of peers whose parents hold that belief (the *x* axis variable), after removing grade-by-school fixed effects from the dependent (*y* axis) variable. The *x* axis variable is shown in s.d. terms; a 1 s.d. change means that 11.2% more of peer parents hold the belief; at 0 s.d., 41% of parents hold the belief. The six plots are divided by child gender (boys in the left column, girls in the right) and the gender of the peers used to create the peer parent beliefs measure (all peers in the first row, own-gendered peers in the second and other-gendered peers in the third. A one unit increase in the peer parent beliefs measure (the *x* axis variable) is equivalent to an 11 percentage point increase in the proportion of own- or other-gendered peers whose parents believe that $B_m > G_m$.We trim the sample to include only children whose value for peer parent beliefs falls in the range [−2,2]; this drops 309 observations from the original sample of 8,028 used to generate the corresponding results in Table 2.

test scores of the child's peer boys and peer girls. This test estimates how an increase in the maths performance of boys (relative to girls) among the children in a child's classroom affects the beliefs of the child's own parent. In the second test, we create an indicator variable equal to one if the highest performing child in the class is male. This tests for the informational salience that comes with the recognition that top performers are often given and the potential for this to have asymmetric effects by gender.

In Supplementary Table 4A, we present our results for the effect of an increase in boys' performance, relative to girls, on a parent's beliefs. We find no evidence that a parent is more likely to believe that $B_m > G_m$ when their child is assigned to a classroom where boys outperform girls in mathematics. Our estimated coefficients are small and not distinguishable from zero but also precise: the CIs we generate can reject that a 1 s.d. 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 that boys are better than girls at learning mathematics, from a baseline of 41% (coefficient = 0.0041; 95% CI −0.0013 to 0.0094; $P = 0.13$). In columns 2 and 3 of Supplementary Table 4, we estimate these effects separately for parents of seventh graders and ninth graders. Recall that the seventh-grade children of these parents have been with their peers for 3–6 months when the parent is interviewed and the ninth-grade children have been with their same peer group for 2 years and 3–6 months. This analysis tests for the possibility that, as the amount of time parents are exposed to their child's peers increases, so will the likelihood that they update their beliefs. Our coefficient estimates provide no evidence of this phenomenon either. In Supplementary Table 4B, we present analogue results using an indicator variable for the top-scoring student on the mid-term maths test being male as the main explanatory variable. We see no evidence of parents' beliefs changing in response to the gender of the top performer. Our results suggest that neither (1) a large change in the performance of boys in mathematics, relative to girls,

Table 3 | Stability of estimates when controlling for other well-known sources of peer effects

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome A: believes boys are better than girls at learning mathematics						
PPB	0.045	0.044	0.044	0.043	0.042	0.040
	(0.022, 0.068)	(0.021, 0.067)	(0.022, 0.066)	(0.020, 0.065)	(0.020, 0.064)	(0.017, 0.063)
	0.000	0.000	0.000	0.000	0.000	0.001
PPB × female	0.008	0.008	0.009	0.010	0.009	0.013
	(−0.019, 0.035)	(−0.019, 0.035)	(−0.017, 0.035)	(−0.017, 0.036)	(−0.017, 0.036)	(-0.015, 0.041)
	0.551	0.560	0.500	0.465	0.484	0.345
R^2	0.186	0.186	0.187	0.187	0.187	0.188
Observations	8,057	8,057	8,057	8,057	8,057	8,057
Outcome B: midterm maths test score						
PPB	0.032	0.024	0.029	0.028	0.028	0.028
	(−0.040, 0.103)	(−0.044, 0.092)	(−0.038, 0.097)	(−0.040, 0.095)	(−0.039, 0.095)	(−0.038, 0.094)
	0.382	0.487	0.394	0.416	0.412	0.399
PPB × female	−0.044	−0.041	−0.041	−0.041	−0.041	−0.041
	(−0.092, 0.004)	(−0.088, 0.007)	(−0.089, 0.007)	(−0.090, 0.008)	(−0.090, 0.008)	(−0.092, 0.009)
	0.071	0.091	0.093	0.098	0.097	0.106
R^2	0.284	0.286	0.286	0.286	0.286	0.287
Observations	8,028	8,028	8,028	8,028	8,028	8,028
Baseline controls	X	X	X	X	X	X
Peers' parents' education		X	X	X	X	X
Peers' parents' income			X	X	X	X
Peers' parents' hukou status				X	X	X
Proportion of peers female					X	X
Peers' cognitive ability scores						X

The dependent variable is given in the titles for outcomes A and B. The X at the bottom of the table indicate that the results shown in the column above are generated with the indicated controls added to our estimating equation. For peer parents' education, we add variables capturing the average number of years that the child's classmates' mothers and fathers spent in school, respectively. For income, we add the proportion of peers who fall in the 'low income' category according to the school. For family background status, we use the proportion of peers with a rural residence permit or hukou. The proportion of girl peers in the child's classroom is self-explanatory. The cognitive ability measure is from a proprietary test using items similar to those in a Raven's Matrices test, standardized to be mean 0, s.d. 1. For each estimate we present, we provide the coefficient estimate first, with the 95% CI underneath it in parentheses and the relevant *P* value underneath the CI.

among a child's peers, nor (2) a change in the gender of the top performer in mathematics, is likely to generate more than a very small change in parent beliefs. Analogue results using the proportion of the top three students in the class who are male, not shown here for brevity but available on request, yield similar patterns.

Next, we test whether parent beliefs adjust in response to exposure to other parent beliefs; this is a specific flavour of the 'reflection problem'[36,39]. To test for this, we regress PPB on own parent beliefs using our core specification. Note that regressing an individual's given characteristic (own parent's beliefs) on the leave-one-out average of this same characteristic (PPB) 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 (for example, average parent beliefs or per cent female) will be distributed normally. A student's characteristic is thus negatively correlated with the leave-one-out average because the overall proportion is equivalent to the sum of the student's characteristic and this average.

To formalize this intuition, we conduct a permutation test, randomly assigning to each child 1,000 'placebo own parent belief' random variables with the same potential values (0/1) and expected value (0.410) as the true parent belief variable. We then generate 1,000 new 'placebo PPB' measures, using the 1,000 placebo belief draws for the parents of each student's peers in their classroom. We standardize these and then run one regression for each of the 1,000 draws, regressing the random variable of each student's own parent's placebo beliefs on the placebo PPB measure, its interaction with the female indicator variable and the other controls as given in our estimating equation. This generates $\tilde{\gamma}$, the mean of our permutation test estimates. We find $\tilde{\gamma} = -0.127$ (95% CI −0.066 to −0.188; $P < 0.001$). Using the true data, we estimate $\hat{\gamma}$, the effect of a 1 s.d. increase in the proportion of peers whose parents believe that $B_m > G_m$ on a child's own parent's likelihood of holding the belief, to be $\hat{\gamma} = -0.079$. Because our estimate of $\hat{\gamma}$ falls well within the 95% CI around $\tilde{\gamma}$ generated by the permutation test, we conclude that there is no evidence for this reflection problem.

We also test whether the gender of the child's randomly assigned maths teacher affects a parent's likelihood of believing that $B_m > G_m$. Here again we can simply regress the parent's response to the belief question on the gender of the maths teacher, controlling for grade-by-school fixed effects. We find no evidence of this potential channel either. The estimated coefficient of the impact on the likelihood that a parent believes that $B_m > G_m$ of their child being assigned a female maths teacher is positive, small and statistically insignificant (treatment effect, using our estimating equation: $\beta_1 = 0.0257$; 95% CI −0.140 to 0.654; $P = 0.202$). We also find no evidence of differential effects for parents of seventh- and ninth-grade students.

We next study whether there is variation in PPB independent of the variation in peer beliefs. If peer beliefs entirely captured the effects we measure and attribute to peer parents' beliefs, then adding them would diminish the magnitude and significance of the peer parent variable. On the other hand, if the effect of PPB were not entirely captured by the inclusion of peer beliefs, then we would have greater reason to believe that the patterns we observe are driven by this intergenerational transmission of beliefs from parents to a child and on to the child's peers, rather than merely by idiosyncratic variation in child characteristics. In Supplementary Tables 5 and 6, we show analogues to Tables 1 and 2, respectively. These tables include a measure of peer beliefs (and their interaction with child gender), constructed as a leave-one-out measure of the child beliefs in a classroom, excluding the child, as we do for PPB. We see in both tables that the sign, magnitude and significance of our estimates are largely consistent with prior results, in some cases even larger. While peer beliefs do also clearly matter, this analysis, like that of Table 3, shows that there is variation in PPB independent of peer beliefs that leads to the effects we measure.

Finally, we comment on the extent to which parents may update their beliefs in response to their own child's gender and ability. We cannot empirically evaluate the extent to which a child's own ability affects their parents' beliefs about the relative ability of boys and girls in mathematics. This is because we lack panel data on the child's ability and their parents' beliefs from earlier in the child's life. We assume, however, that the primary interaction between parents and their child is parents' beliefs and actions shaping those of their children and not child ability shaping parent beliefs. This assumption is based on three arguments: (1) the vast child development literature documenting the great extent to which parents influence their children's development[21,40,41]; (2) the prior work and evidence presented earlier in this paper indicating that parents' beliefs are relatively harder to manipulate than children's[12–15]; and (3) the fact that even if child ability were to affect parent beliefs, we show in Table 3 that controlling for peer cognitive ability does not substantially change our belief transmission results or the estimated effects on test scores.

We can also measure how parent beliefs about gender differences in maths ability vary with the gender of their child(ren). The revelation of child gender is a large information shock associated with a change in parent beliefs at the time of revelation[42–44]. This has also been shown to be the case in China, where son preference often prevails[45]. We report analyses of this in our data. More than half of the families in our sample have multiple children. On average, among parents who have only girls, 38.0% believe that $B_m > G_m$; among parents who have both a girl and a boy, 40.8% hold the belief; among parents with only boys, 43.7% do.

The correlation between child gender and parent beliefs is small compared to the idiosyncratic variation in parent beliefs across classrooms, within a grade, within a school; we show in Supplementary Fig. 2 that the percentage of children whose parents hold this belief regularly varies by >15 percentage points. Furthermore, several previous studies show that after responding to the revelation of the child's gender, parent beliefs are unlikely to further change[42–44]. As a whole, we interpret the patterns we see in our data and existing evidence on parent response to child gender as further supporting our claim that beliefs about gender differences in maths ability are much more likely to shift in childhood than in adulthood.

Discussion

Our study's core contribution is to advance understanding of an important channel through which beliefs about gender differences in maths ability transmit across generations and how this affects children's beliefs about themselves and their academic performance. We overcome the logistical and ethical barriers to estimating this relationship experimentally by applying a quasi-experimental research design to a setting with substantial, randomly assigned variation in the proportion of a child's peers whose parents hold this belief. We show positive evidence of the intergenerational transmission of these beliefs via peers, an important channel of belief transmission and how this leads to self-reinforcing outcomes that confirm the once-erroneous message of the belief and which may contribute to under-representation of women in STEM fields[8].

Two aspects of this study make it likely to underestimate the full impacts of this exposure. First, we study only one round of outcomes. As a result, we are unable to track the longer-term academic and career effects of exposure to these beliefs, which are likely to be larger if these effects compound over time. We also work in a setting in which girls outperform boys in mathematics. Even in such a setting, we find harms of exposure to peers whose parents hold this belief.

There are a few key limitations of our approach. The first limitation is that the process we study—the intergenerational transmission of beliefs—is a 'black box'. Prior work has established that parent-to-child transmission of this belief occurs[7,17,18]. Our analysis establishes that transmission of this belief also occurs via a separate channel: children who learn the belief from their parents transmit it to their peers. In the Methods, we present results showing that for the effects we study, the most likely channel is from peer parent to the peer child and then peer child to child, rather than from peer parent directly to the child. Our research design, however, does not allow us to analyse how the beliefs are transferred from parent to child. Many likely channels exist, including direct discussion of the issue, the messages imparted by the gifts and encouragement parents give to children and even simple observation of the parents' conduct, tone of voice and values. The second limitation is that this is not a randomized controlled trial; instead, we use quasi-experimental variation in the proportion of peers whose parents hold this belief. It is therefore possible that there is some external, omitted factor that drives both PPB and child outcomes. While we are unable to rule out the existence of any such factor, we are able to show that the results we generate are independent of variation in observable factors related to all of the main contributors (parent education, income, peer aptitude and others) that have been identified in 20 years of prior work on peer effects. This gives us confidence that such an omitted factor is unlikely to play a meaningful role in driving the results we attribute to the intergenerational transmission of this specific belief via peers. The third limitation is that we rely on a single measure of parent beliefs, which is ultimately a proxy for parents' broader beliefs about gender and how they teach their child about it. Our main claims are: (1) that we provide evidence of the impact of this latent effect, using parent beliefs, a measure that we show can capture the impacts of this effect; and (2) we demonstrate that a large component of this effect is orthogonal to the other sources of peer effects, both broad and specific, that have been studied in the prior literature. Finally, while we have shown evidence that these effects exist, their magnitude will probably vary with the amount of exposure children have to their peers, which differs across contexts.

Our work highlights the importance of the informational environment that children grow up in and the role it plays in propagating societal beliefs across generations. Childhood exposure to such societal beliefs can come from myriad sources; our study shows that this exposure can perpetuate these beliefs and, in so doing, contribute to gender inequality. Future work should aim to address and reverse the harms that the transmission of these widely held beliefs about differential ability can cause.

Methods

Quasi-experimental analysis of the transmission of beliefs. We are interested in studying how an influential type of societal belief—that boys are inherently better than girls in certain dimensions—transmits across individuals and across generations. Because greater levels of this belief at the societal level have been shown to be correlated with lower relative educational performance[7,19], it is unethical to study the effects of greater exposure to this belief with a randomized controlled trial. Given this limitation, our approach is to study this transmission using a quasi-experimental approach[46,47]. Specifically, we exploit the random assignment of students to classmates in (Chinese) middle schools, a frequently used method in the study of peer effects[22,23], which generates random variation in the proportion of peers in a child's classroom with parents who hold this belief.

Our research design follows that of other quasi-experimental analyses which study the impact of being assigned to a peer group with different peer or peer parental characteristics, exploiting random classroom assignment[22,23,48]. While this approach does not experimentally vary only one characteristic while holding others constant, it is able to study the randomly assigned variation from a given characteristic that is independent of variation in other observable characteristics. These methods have been used to study the impact of being assigned to peers with different levels of gender composition[24,49], higher levels of academic ability[23,48] and more educated parents[25,50], among others.

When using such designs, researchers must establish that there is no omitted variable driving both the belief and the outcome. We present this analysis in Table 3, which shows that, for the most important previously identified sources of peer effects studied in this literature (peer gender, peer aptitude, parent education and parent income) there is variation in our main explanatory variable independent of each of these sources of peer effect. As described earlier in the paper, while this analysis does not rule out any such possible source, it does rule out all of the likely sources identified in previous work on peer effects.

Background information on our context and data. We use the first wave of the 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 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. 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). Schools in this dataset were selected using a nationally representative random sampling frame with selection probability proportional to size. Geographically, the sampling frame includes all counties and city districts of the 31 provinces, municipalities and autonomous regions of China, excluding Hong Kong, Macau and Taiwan. All further geographic information about observations is suppressed for users of these data to protect the anonymity of participants. The dataset comprises ~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. We do not use it here because of its smaller sample size and because it does not contain key data such as parent or child beliefs.

The CEPS student data include administrative data on the child's academic performance in mathematics, Chinese and English, as well as the child's responses to a survey about their beliefs and aspirations. The parent data include a variety of demographic data as well as survey responses to a series of questions about the parent's beliefs. The teacher and administrator data include information on teacher characteristics and the method used to assign children to classes.

According to a law passed in 2006, Chinese middle schools are normally required to randomly assign children to classrooms. Under this system, children are assigned to a classroom at the start of seventh grade and remain with the same peers in this randomly assigned classroom throughout middle school. In practice, some schools may deviate from this rule, for example, to sort children on ability. We follow the same sample restriction used in prior work using CEPS data: we analyse data from only those within-grade classroom pairs where principals and teachers report that random assignment was used to place children in classrooms[6,24,51]. This generates a sample of 8,057 children in 215 classrooms spread across 86 schools and this is the estimation sample we use throughout our analysis in this paper. 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. This latter group comprises predominantly ninth-grade classrooms, where re-sorting often occurs due to administrative concerns about placing children in good high schools. No statistical methods were used to predetermine sample sizes but our sample sizes are larger than those reported in other recent studies of peer effects[10,25,50].

Across China, various methods are used for assignment of children to classes, including random number generators, alphabetical assignment based on surname or an alternating assignment sequence based on entrance exam scores 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[6,24,51].

Supplementary Table 1 presents summary statistics for students, by gender, for students in our estimation sample. 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[52]. Finally, in all subjects, girls perform better than boys on average. This differs somewhat from Programme for International Student Assessment (PISA) results: in the 2009 PISA results for China, boys significantly outperformed girls in mathematics but in the 2015 PISA results, this difference was no longer significant. PISA data, however, apply only to a select group of children from urban areas: Shanghai (2009) or Beijing, Shanghai, Jiangsu and Guangdong (2015). Our data come from a nationally representative sample of middle schools across China and include both rural and urban areas. In Supplementary Fig. 1, we show the distribution of maths test scores by gender. The distribution for girls first-order stochastically dominates that for boys ($n = 8,028$; Kolmogorov–Smirnov two-sample equality-of-distributions test $P < 0.001$), with the largest difference in the left tail of the distribution.

One important feature of the context we study is that middle-school children are much more exposed to their classmates than in other contexts, such as those in Europe or the United States. Specifically, children are assigned to a class and then follow their classmates to each class, so that the same group of students takes mathematics together, Chinese together, English together and so on. The school day is longer in middle school than in primary school and often includes an 'evening session' for additional study and these are often grouped by home-room class[53]. As a result, the students we study have far greater exposure to each other than they would in other contexts, which means that there is greater scope for the transmission of beliefs via the various channels (discussion, demonstration and passive observation) that we outline above. A key advantage of this feature is that it provides an ideal setting for detecting the existence of these effects. A potential disadvantage is that, should there be a significant positive correlation between per-day hours of exposure and effect size, the magnitude of effect will vary by context-specific variation in the proportion of hours children spend with their school peers each day.

Our measure of peer parents' beliefs. Here, we describe how we construct our measure of the proportion of peers whose parents believe that boys are innately better than girls at learning math, that is, who believe that $B_m > G_m$. We also describe the properties of this measure.

We wish to measure a latent variable: each parent's beliefs about the innate ability of boys and girls in mathematics. We use a datapoint that the CEPS collects from the interviewed parent of each child to proxy for this. The CEPS asks the parent whether or not they agree with the statement 'boys' natural ability in studying mathematics is greater than that of girls'. Approximately 41% of parents agree with the statement, with the remaining 59% disagreeing. This belief is surprisingly common given the fact that, in China, girls generally outperform boys in mathematics at this level of schooling[51]. In the introduction, we argue that this pattern is at least partly the result of the fact that parent beliefs were most probably formed when the parent was a child, given that (1) beliefs are generally more malleable at younger ages and (2) the parents of the children in our sample were themselves children in the 1970s and 1980s, at a time in which son preference in China was stronger than it is today[54].

As described in the introduction, we use this question to generate a child-level variable summarizing the beliefs of the parents of the child's peers in their randomly assigned classroom. Specifically, we create a leave-one-out measure which captures, for each child, the proportion of peers in their classroom whose parents report believing that $B_m > G_m$, a variable that could potentially range from 0 to 1. In our data, the actual values of the variable range from 0 to 0.833, with a mean of 0.410 and a standard deviation of 0.112. At three significant figures, the mean of this variable is the same for girls and boys. Once standardized, the variable ranges from −3.597 to 3.739 s.d.

In Supplementary Table 2, we summarize the characteristics of parents who do and do not believe that $B_m > G_m$, respectively. We see differences for some characteristics (household income, number of siblings, parent's age, gender of the child) but these differences are small in magnitude. Our interpretation of the patterns in this table is that, overall, these two groups of parents are similar on most observable characteristics associated with the other traditional drivers of peer effects.

In the last part of this section, we briefly discuss how parent beliefs may be formed. We are unable to use CEPS data to study how parents' beliefs were formed but, as we describe elsewhere in this paper, we argue that parent beliefs are likely to be formed primarily when the parents themselves are children. The formation of beliefs in childhood is likely to be the result of diverse inputs—one's own parents[18], their peers[23], television and other national media[55] and their community. The contribution of the community is important because if the parent stays in the community when they raise their own children, the community may also contribute directly to the beliefs of the parent's child. China's compulsory education law requires that children go to school near their homes when possible, meaning that in most cases, all children within a school come from a concentrated local area. In our regressions, we control for grade-by-school fixed effects. This means that we are estimating the effect of random variation in exposure to these messages within the community the school draws from and our estimates exclude variation in these messages across communities. Furthermore, the research we cite earlier showing that child beliefs are more malleable than parent beliefs means that parents' beliefs are most likely to have been influenced by their community when they themselves were children, rather than at the time of measurement, when they are themselves the parents of adolescents.

Sources of variation in our PPB measure. Our study exploits the fact that across our units of observation—pairs of classrooms, within a grade, within a school—there is wide dispersion in the main explanatory variable, the proportion of children's parents who hold this belief. Here, we characterize that dispersion. There are two sources of variation in the classroom average proportion of parents who believe that $B_m > G_m$: (1) differences between schools and (2) differences between classes, within schools (the latter being our level of comparison). We can decompose overall variation to characterize the contribution of each source. Were our variation to come 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 PPB. This comparison, however, would have little to say about larger changes, as they would necessitate out-of-sample predictions.

In Supplementary Fig. 2, we show two plots describing the variation between classrooms in the proportion of parents who believe that $B_m > G_m$. Supplementary Fig. 2A shows, for each classroom, the proportion of parents who agree with the statement that boys are better than girls in learning mathematics. This shows a roughly symmetric distribution around 41%, the mean, with a range from 7% to almost 80%. Supplementary Fig. 2B shows how our measure of parent beliefs varies within each of 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 beliefs for class 1 shown on the x axis, and that for class 2 on the y axis. We see large differences in parent beliefs between classrooms in these pairs. A simple decomposition of variance finds that between-school variation explains less than one-third of the overall variation in classroom-level parent beliefs and within-school, between classroom variation explains the remaining (more than two-thirds of) overall variation. This allows us to estimate the impact of many different 'treatment intensities' while controlling for school fixed effects, thus removing the influence of unobservable traits that may vary from school to school.

A separate way to capture the differences in this variable between classrooms, within a grade, within each school, is to calculate the absolute value of the difference in the (standardized) parent beliefs measure 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 s.d., with a mean of 1 s.d. We show the distribution of these values in Supplementary Fig. 3.

Assessing random assignment. In this section, we evaluate our claim that children are, in fact, randomly assigned to their classrooms. First, we note that we follow the sample restriction of several previous papers that have used these data. These papers show that using this sample restriction yields a dataset of children who are randomly assigned to classrooms within a school, as evidenced by the results they present in their tests of randomization/balance[24,51,56].

We further probe the claim of random assignment of children to classrooms by regressing our PPB measure on the child-level characteristics in $\mathbf{C}_{icgs}$ from our estimating equation, all of which are predetermined relative to classroom assignment. This approach follows recent studies analysing classroom-level randomization in other contexts[57–59]. We present our results in Supplementary Table 3. In column 1 of the table, we show the results for regressing PPB on the vector of predetermined characteristics without any fixed effects; in column 2, we add controls for the grade-by-school fixed effects used in our main estimating equation. Our results in column 2 fail to reject the null that the regressors do not significantly predict variation in PPB within schools, within grades, between classrooms.

The random assignment of children to classes prevents our estimates of β_1 and β_2 from being confounded by potential non-random sorting of children to classrooms, either by ability or parent preference. Were such sorting to exist, our estimates of the effect of exposure to more peers whose parents believe that $B_m > G_m$ would also comprise the other effects of such sorting. For example, since girls outperform boys in mathematics in our sample (Supplementary Fig. 1), placing more able students together would lead to more girls in higher performing classrooms. This would lead to two differences: (1) more girl-to-girl (boy-to-boy) exposure in high-performing (low-performing) classrooms; (2) in high-performing classrooms, greater salutary effects of having more girl peers[24]. Random assignment also prevents confounding from two other potential sources: (1) that schools re-allocate teacher and classroom resources towards (or away from) higher-ability classrooms; (2) that parents with certain characteristics request their children to be among the children of similar parents.

Channels of transmission. Here, we study the relative importance of two potential channels of peer parent belief transmission to children: (1) from peers' parents directly to the child herself; (2) from the peers' parents to the peer and then from the peer to the child. Prior research finds that the direct channel is an important factor in shaping US girls' beliefs about their place in the labour market[10]. We present two analyses to study this. The first uses two new measures of PPB—peer mother beliefs and peer father beliefs. These are constructed in a way that is similar to the way that peer girl and peer boy parent beliefs are constructed, only in this case we use the identity of the parent whose response the survey captures to generate the variable. On average, mothers in China spend a larger portion of their time interacting with children than do fathers[60]. If the main channel of transmission were from peer parents to the child (as opposed to from parents to the child and then from the child to their peers), we should see larger effects for peer mothers than peer fathers. In Supplementary Table 7, we present our results; we find that the estimate for peer mothers' beliefs and peer fathers' beliefs have similar magnitudes and we cannot exclude that they are identical.

While there may be additional variation across parents who have more or less interaction with the child, the only related datapoint collected in the CEPS is each parent's response to the following yes/no question: 'do you know the friends that your child often spends time with?' A high proportion (87.8% of parents) respond 'yes' to this question. We find that using this interaction to estimate whether peer parents who know their children's friends have greater transmission of beliefs has no more descriptive power than using a random sample of an equal proportion of parents (who may or may not interact with their children's friends). Given the flaws of this particular measure, we argue that this test is inconclusive. Overall, these tests suggest that the main channel is probably from the parent to the child and then from the child to their peers, rather than directly from peer parents to the child.

Exploration of heterogeneity. We conduct additional analysis of three dimensions of potential heterogeneity in the effects we measure. They are: (1) whether there are differential effects by the child's pretest maths ability; (2) whether there are differential effects by the child's mother's highest level of education; and (3) whether there are differential effects by whether the child's mother has a higher level of education than the father. We also test for interaction effects between own parent beliefs and PPB. Finally, we examine whether there are greater test score effects among children at highly competitive schools than at other schools.

For investigation of heterogeneity by pretest maths ability, mother's highest level of education and parental education gaps, we create binary variables to capture each concept. The pretest maths ability is split by low (0) versus high (1) performance in the final year of primary school mathematics (mean = 0.651); maternal education is split by low (0) versus high (1) maternal education (in our case, whether the mother attended high school; mean = 0.390); and the parental education gap is split by whether the mother has a higher level of education than the father (mean = 0.134). In each analysis, we add the binary variable in question, along with its interaction with PPB and gender, respectively, as well as the triple interaction (variable × PPB × gender), to the estimating equation. We report coefficients for all seven coefficients, for each of our two main outcome variables: the child's likelihood of believing that $B_m > G_m$ and the child's maths test score.

To study the interaction between own parent beliefs and PPB, we use a similar specification, adding the triple interaction to the estimating equation (the variable and its interaction are already present in our prior analyses). To study heterogeneity in whether the effects for test scores vary with whether the child attends a highly competitive school, we split the sample by whether the school is reported to be 'among the best' schools in its prefecture, a rough proxy for how competitive it is (true for 25.1% of the schools in our sample)[61].

We find no evidence of statistically significant interaction between exposure to peers whose parents believe that $B_m > G_m$ and pretest maths ability in terms of the transmission of beliefs; we show these results in Supplementary Table 8. We do find a statistically significant interaction between exposure to girl peers whose parents believe that $B_m > G_m$ and high baseline maths ability (an additional effect of −0.084; CI −0.150 to −0.018; $P = 0.013$); we show these results in Supplementary Table 9. We find no evidence of a statistically significant relationship between maternal education and either belief transmission or its effects on test scores (results in Supplementary Tables 10 and 11). For the parental education gap, the pattern is somewhat more complicated. In Supplementary Table 12, we show estimates for child beliefs as the outcome variable. We estimate a statistically significant interaction between the gap (the child's mother having more education than the father) and the beliefs of girl peers' parents, with a positive and significant effect overall (effect estimate: 0.043; CI 0.004 to 0.082; $P = 0.033$) and a negative and significant effect for the interaction with the child being female (effect estimate −0.076; CI −0.131 to −0.021; $P = 0.007$). We show relevant results for maths test scores in Supplementary Table 13. Here, the interaction between the gap and all peer parents' beliefs is also positive and significant (effect estimate: 0.101; CI 0.020 to 0.182; $P = 0.015$). We find no evidence of a statistically significant interaction between own parent beliefs and PPB for either the transmission of beliefs (Supplementary Table 14) or maths test scores (Supplementary Table 15). Finally, splitting the sample by high-performing schools also reduces our statistical power to detect effects: while the magnitude of the point estimates for the effect of exposure to peers whose parents believe that $B_m > G_m$ on maths performance are roughly twice as large in highly competitive schools as in less competitive schools, these differences are not statistically significant (Supplementary Tables 16 and 17).

Other statistical methods. We used a two-tailed α of 0.05 for all statistical tests and, when estimating our models, assume normally distributed errors; we do not formally test this because we are unable to directly observe properties of the error term. Because we use observational data, where data collection and randomization were conducted by (separate) third parties, our analysis did not require any additional human subjects ethical review. For the same reason, our sample sizes were determined by data availability rather than prospective sample size or power calculations. All of our analyses were conducted using Stata MP v.16.0. Our code is available at https://github.com/alexeble/NHBGenderedbeliefs2022/.

Reporting Summary. Further information on research design is available in the Nature Research Reporting Summary linked to this article.

Data availability
Our data are publicly available at the CEPS website, hosted by Renmin University of China, from which we accessed them: http://ceps.ruc.edu.cn/English/Overview/Overview.htm. This repository contains the entire 'minimum dataset' necessary to interpret, verify and extend the research in the article.

Code availability
Custom code that supports the findings of this study is available on the GitHub public repository at https://github.com/alexeble/NHBGenderedbeliefs2022/.

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Author contributions

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Competing interests

The authors declare no competing interests.

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Our data are publicly available at the website for the China Education Panel Survey, hosted by Renmin University of China, from which we accessed them: http://ceps.ruc.edu.cn/English/Overview/Overview.htm. This repository contains the entire “minimum dataset” necessary to interpret, verify and extend the research in the article.

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Study description	This is a quantitative, quasi-experimental analysis. Our analysis exploits the random assignment of students to classrooms in (Chinese) middle schools, a frequently used method in the study of peer effects which, in our case, generates random variation in the proportion of peers in a child's classroom with parents who hold this belief. We use this to estimate the effect of being assigned to a peer group with a high proportion of parents who believe boys are innately better than girls at learning math on a child's beliefs and demonstrated math ability.
Research sample	The China Education Panel Survey (CEPS) is a large-scale, nationally representative, longitudinal survey of two cohorts – the 7th and 9th graders in the 2013-2014 academic year. The website which provides the original data (http://ceps.ruc.edu.cn/English/Overview/Overview.htm) says this about the data: "The CEPS applies a stratified, multistage sampling design with probability proportional to size (PPS), randomly selecting a school-based, nationally representative sample of approximately 20,000 students in 438 classrooms of 112 schools in 28 county-level units in mainland China. The baseline survey of CEPS was completed in the 2013-2014 academic year, conducted by National Survey Research Center (NSRC) at Renmin University of China." We restrict this sample to only school-grade combinations which randomly assign students to classrooms, a restriction used in prior research using these data (Wang, H., Cheng, Z. & Smyth, R. Do migrant students affect local students' academic achievements in urban China? 2018. Economics of Education Review 63, 64–77 ; Gong, J., Lu, Y. & Song, H. The effect of teacher gender on students' academic and noncognitive outcomes, 2018. Journal of Labor Economics 36, 743–778; and Eble, A. and Hu, F., 2020. Child beliefs, societal beliefs, and teacher-student identity match. Economics of Education Review, 77, 101994.). In our final sample, the average age is 13.2, and 49.2 percent of the observations are female. This is similar to these characteristics in the original data.
Sampling strategy	No statistical methods were used to pre-determine sample sizes; we used all data which fit our inclusion criteria (described under "data exclusions"). Our sample sizes are larger than those reported in other recent studies of peer effects (Olivetti, C., Patacchini, E. & Zenou, Y., 2020. Mothers, peers, and gender-role identity. Journal of the European Economic Association 18, 266–301; Fruehwirth, J. C. & Gagete-Miranda, J., 2019. Your peers' parents: Spillovers from parental education. Economics of Education Review 73, 101910; and Chung, B. W. 2020 Peers' Parents and Educational Attainment: The Exposure Effect. Labour Economics 101812.)
Data collection	Not applicable; we did not collect our own data.
Timing	Not applicable; we did not collect our own data.
Data exclusions	We use the selected sample, as also used in other research (e.g., citations 15 and 23 in our manuscript), focusing only students in schools where there is random assignment of children to classrooms in a given grade. These criteria were pre-established relative to our analysis of the data.
Non-participation	No human participants were involved in our study.
Randomization	Across China, various methods are used for assignment of children to classes, including random number generators, alphabetical assignment based on surname, or an alternating assignment sequence based on entrance exam scores 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 (see citations 6 and 15 in the manuscript). In Table S3 we provide additional analysis of randomization fidelity. As is the case in the analysis of this data in the other aforementioned citations, our analyses also support the claim that there was random assignment of children to classes in this sample.

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Methods

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