Troubled in School: Does Maternal Involvement Matter for Adolescents?

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Abstract

We estimate the causal effect of mother's involvement on the amount of trouble an adolescent experiences in school. We use multiple measures of school trouble and factor analysis to construct a composite and then link this composite with noncognitive skills. Our measure of mother's involvement encompasses discussing school-related matters and providing help with school projects. Using an instrumental variable constructed from a suitably chosen peer group, our main finding is that an increase in maternal involvement leads to a significant decrease in school trouble. We find this result to be robust across a large number of sensitivity tests designed to account for possible selection effects, shocks at the peer group level, and further potential violations of the exclusion restriction. Additionally, we present evidence suggesting that the effect of maternal involvement may operate through its effect on adolescents' college aspirations, mental health, and the perception of parental warmth.

Keywords: school trouble, noncognitive skills, maternal involvement, instrumental variables

JEL Codes: C26, I31, J13, J31

1 Introduction

We study the causal effect of maternal involvement on adolescents' trouble in school, using data from the National Longitudinal Study of Adolescent to Adult Health (Add Health).[1] Our focus on maternal involvement is motivated by existing evidence that parental efforts and investments during early childhood provide children with important and wide-ranging benefits (Carneiro et al. 2013; Heckman and Mosso 2014), including links between parental involvement and children's academic achievement (Jeynes 2007; Boonk et al. 2018). Yet, much less is known about the efficacy of parental investments during adolescence.

We use trouble in school as an outcome variable because it represents an important set of behaviors capturing both cognitive and non-cognitive skills that affect a wide range of long-term outcomes. In particular, the importance of non-cognitive skills, such as perseverance, impulse control, and empathy has recently been established in the literature (Heckman and Kautz 2014). Evidence from adolescence is sparser, but such skills appear to remain malleable during this period, reflect a measure of adolescent development (Heckman and Mosso 2014; Hoeschler et al. 2018), and yield labor market returns in adulthood that have been rising in the recent past, as more occupations shift toward team work and soft skills (Deming 2017).

Much of the research on the impact of parental investments for the development of cognitive and non-cognitive skills focuses on young children (Cunha and Heckman 2008; Cunha et al. 2010; Todd and Wolpin 2007; Aizer 2004; Welsch and Zimmer 2008; Kalb and Ours 2014). Establishing

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the causal impact of parental investments, however, remains difficult. In observational data, the correlation between parental action and children's outcomes often disappears after controlling for family background characteristics (Avvisati et al. 2010), and it is not clear whether this pattern reflects the absence of an effect or bias from unobserved factors. A limited number of experimental studies among younger age groups, however, suggest that the effects of involvement are strong once endogeneity is removed (e.g. Avvisati et al. 2014; Attanasio et al. 2020).

A large literature in education and developmental psychology has studied the association between parental involvement and children's academic achievement. These studies, however, have generally not been able to address endogeneity and their estimates are not necessarily causal effects (e.g., Jeynes 2007; Boonk et al. 2018). An exception is a set of studies around the family check-up (FCU) intervention, which engaged parents and students and aimed to improve family management practices (Dishion et al. 2002). It was evaluated in a randomized controlled trial (RCT), conducted at three public middle schools. Evaluations of the FCU have found that it lowered the propensity for substance use (Dishion et al. 2002; Connell et al. 2007; Stormshak and Dishion 2009), improved academic outcomes (Stormshak et al. 2009), reduced problem behavior (Connell et al. 2007; Van Ryzin et al. 2012), and improved mental health and school engagement (Stormshak et al. 2010). While the FCU provides important evidence about the potential effectiveness of family-based interventions, the relatively small number of students and schools involved in the study raises questions about generalizability to a larger population of adolescents.

Within the non-RCT literature, studies on the development of cognitive and noncognitive skills generally find parental investments are important for skill production early in life. These studies tend to either take structural (Cunha and Heckman 2008; Cunha et al. 2010; Todd and Wolpin 2007) or reduced form approaches (Aizer 2004; Kalb and Ours 2014; Price and Kalil

2019; Welsch and Zimmer 2008) and provide valuable information on the role of investments for skill production in early life.[2] Another strand of literature examines parental investment iteself such as differences across gender (Baker and Milligan 2016) and factors affecting the investment choice (Doepke et al. 2019). However, to our knowledge, there is no evidence available that leverages quasi-random natural variation to generate instruments with less restrictive assumptions and isolate the average treatment effects of involvement. There is also less evidence on the role of parental involvement for adolescents.

The main contribution of our paper is to provide new causal evidence about the effect of maternal involvement, specifically maternal schooling-related involvement, on adolescent trouble in school. We use data from the Add Health study, which is nationally representative and covers students across the full age range of adolescence. Our empirical analysis leverages a novel identification strategy that addresses potential bias due to unobserved heterogeneity and selection effects. We focus on maternal schooling-related involvement for several reasons. First, the importance of maternal investments for child development has been stressed elsewhere (Heckman and Mosso 2014; Carneiro et al. 2013), and schooling-related involvement may more directly relate to experiences in school of which the child has less choice or preference as compared to recreational activities with the parent. Second, we use data from the Add Health parental survey, which focused primarily on mothers because they were expected to be the most involved in their children's day-to-day lives. Third, survey data was missing for fathers

2. For example, using data among pre-adolescent children these studies have found that parental investments matter at very early ages for cognitive skills and remain effective for noncognitive skills at later ages (Cunha and Heckman 2008; Cunha et al. 2010; Todd and Wolpin 2007), that after school supervision is related to improving antisocial behaviors (Aizer 2004; Welsch and Zimmer 2008), and that parental reading investments with very young children improve child reading ability (Kalb and Ours 2014; Price and Kalil 2019).

much more often than for mothers.[3]

We construct our school trouble measure from a factor analysis aimed at capturing a wide array of skills with multiple observed indicators. We also use follow-up waves of the survey and explore the association between school trouble and subsequent education and wage outcomes. As we discuss in Section 2.2, our results are similar to the associations between non-cognitive skills and education and wages found elsewhere in the literature and suggest that trouble in school can have long-term consequences.

To address endogeneity in the relation between maternal involvement and school trouble, we propose an approach akin to that in Fruehwirth et al. (2019). They use variation within schools across an appropriately defined peer reference group to identify the effect of religiosity on mental health. In our study, we draw on evidence that parenting advice from social circles and families tends to be weighted more heavily than advice from experts (Kalil 2015). We expect that mothers are more likely to respond to a peer group of mothers who have similar education levels and children with the same exogenous characteristics (race, gender, school, and grade). This motivates our use of peer maternal involvement as an instrumental variable.

Our baseline estimates show that an increase in maternal involvement leads to a significant reduction in the adolescent's school trouble. This effect is obscured by a standard OLS regression, which yields a small effect estimate but one that may be biased toward zero by maternal responses to poor behavior (e.g., McNeal 2012). Our results provide new evidence that continued maternal involvement beyond early childhood remains important and, in particular, can support the development of non-cognitive skills during adolescence. We conduct a large number of sensitivity analyses—aimed at detecting possible violations of the exclusion restriction—and find that our baseline estimate remains robust.

3. In our sensitivity analysis, we consider a measure of involvement by fathers but find that our results on maternal involvement are highly robust.

Our study further relates to a wider literature examining the role of parental beliefs in changing parenting style or the level of parental investments. This literature has found that parents' subjective beliefs about the child's skill production function may be distorted and sensitive to environments outside of the home. This can lead to lower investments among those lacking information and resources (Attanasio et al. 2019; Attanasio 2015; Cunha 2015; Kiessling 2019; Han 2017). Our results show that maternal investments can have a substantial influence on adolescent skill development. Beliefs that change parental investments may therefore remain important throughout adolescence.

We additionally explore several mechanisms that may explain the impact of maternal involvement on school trouble. First, maternal involvement may change the adolescent's aspirations for future education. This is consistent with the theory that involvement is an effort to shift a child's choice set towards a more forward looking perspective (Doepke et al. 2019). Second, maternal involvement may affect the adolescent's mental health. Third, maternal involvement can affect the adolescent's perception of parenting style, which has been identified as an important factor determining child outcomes (Jeynes 2007; Doepke et al. 2019). To measure this, we use adolescent perceptions of parental warmth, control, and autonomy, which are three salient dimensions of parenting style (Steinberg et al. 1992; Marchant et al. 2001). We find that maternal involvement shifts adolescent aspirations, mental health, and, to a lesser extent, perceptions of warmth in the relationship with parents.

Finally, we conduct a descriptive analysis in which our measures of adolescent college aspirations, mental health, and parenting style are treated as potential mediators. Our findings suggest that these may act as significant mediators in the link between maternal involvement and school trouble. While our mediation analysis is descriptive, it provides further support to the importance of school trouble as an outcome measure and the role that maternal involvement continues to play during adolescence.

The remainder of this paper is organized as follows. Section 2 discusses the data and construction of the school trouble variable and maternal involvement. We outline our empirical strategy in Section 3 and present results in Section 4. We explore possible mechanisms in Section 5. Finally, Section 6 concludes.

2 Data and Variables

2.1 Data Description

For this study we use the National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health began in 1994 as a nationally representative sample of adolescents in the U.S. The study was split between an in-school survey and an in-home survey. The in-home survey is a subset of 20,745 adolescent students out of the 90,000 in-school participants. The in-home group has been followed through four waves, with the wave IV sample aged 26-32.

At wave I for the in-home sample, Add Health also conducted a parent survey. The mother was the targeted respondent. If the biological mother was not in the home, then the next mother figure was requested before the father. The expectation was that mothers would be more involved with the children's school and other activities and be able to provide more detail. We draw on this survey for several important measures on mothers.

The in-home sample provides rich information about the participants' home, social, and school life during the adolescent years. It also provides detailed information on young adult life outcomes. Key for our identification strategy is that, in wave I, we observe reference groups of "peer mothers." These are mothers who are similar along several dimensions and who have children with shared characteristics. For the analysis of mother's involvement and school trouble, we take advantage of random variation across groups of peer mothers to identify the effect of interest.

2.2 School Trouble and Skills

We conduct a factor analysis on observed school-trouble measures, with a single latent variable (factor) to capture the underlying skills these trouble measures proxy. Our observed measures of latent skills are all self-reported and consist of grade point average, the number of unauthorized missed school days, reports on a zero to four scale of trouble with teachers, trouble with other students, and trouble getting homework done, a measure for the frequency one gets into fights, and an indicator for being suspended at any point during the school year.[4][5] We take the negative of grade point average so that higher values imply greater trouble to be consistent with the rest of our measures; however, in a robustness check we omit GPA from the scale and find highly consistent, if marginally less efficient, results.

To create a single measure of skill, we estimate a basic latent factor structural equations model and predict the latent skill factor for each adolescent in the sample. For most observed measures, we use a linear measurement equation

$$M_j = \alpha_j \theta + \epsilon_j, \quad j = 1, \ldots k-1, \tag{2.1}$$

where M_j is the j-th indicator, α_j is the factor loading, θ is the latent skill factor, and ϵ_j is measurement error. Following standard practice, we set the scale of θ by constraining the factor loading for one of the observed measures to 1. For school suspension we use a probit measurement equation

$$M_k = \Phi(\alpha_k \theta) + \epsilon_k, \tag{2.2}$$

4. We drop students who missed more than 30 days of school. This reduces the sample by 236 observations.

5. Kautz and Zanoni (2014) have some overlapping measures with us in their analysis of the Chicago One Goal Program. They argue such measures are more likely observable for a school than personality measures.

where $\Phi(\cdot)$ is the CDF of the standard normal distribution.[6] We also drop missing observations in our measures to ensure that the measurement equations are estimated on the same sample. Summary statistics for the measures are available in the appendix, Table A.1. The estimated factor loadings are given in column 1 of Table A.2 in the appendix. Each measurement is strongly related to the latent skill variable θ. We standardized the scale to a mean of zero and a standard deviation of one. For ease of exposition, we often refer to the latent skill variable as the school-trouble scale.

To test against significant heterogeneity in the loadings, we also report them split across gender and grade-levels. Columns 2-3 in Table A.2 illustrate that the measures load onto our scale evenly across gender. Columns 4-9 illustrate the same by grade-level. The only exception is that days of skipping school loads more heavily at later grade-levels, otherwise the loadings are consistent. We think this is sensible because skipping school may be easier when one is older. However, in all specifications to come we will control for the grade-level effect in a non-linear manner.[7]

Finally, we explore the relation between our composite school-trouble scale and two future outcomes observed in wave IV: completed education level and wages. We report our results in the supplemental appendix, Section B.1. In terms of both completed education and wages, our school-trouble scale follows closely to the patterns reported by Heckman (2008) and Heckman et al. (2014) for noncognitive skills.[8] Likewise, the picture vocabulary test score closely matches the patterns found for cognitive skills.

Our aim is to broadly capture school trouble through the skills that determine it. While our scale is strongly related to noncognitive skills, cognitive skills may also contribute. However, the results in the supplemental ap-

6. We estimate the measurement system in (2.1) and (2.2) using the `gsem` command in Stata.

7. Also, see column 10 of Table A.2 for the loadings when we omit GPA.

8. These studies use different data from ours and identify separately the distribution of noncognitive skills and cognitive skills.

pendix, Section B.1 suggest that we have a reasonable proxy for noncognitive skills and that there are returns to these skills in the long run. Nevertheless, we focus on our scale as a broad measure of school trouble.

2.3 Mother's Involvement

The Add Health survey contains a number measures for maternal involvement. Our set of interest involves responses to a series of questions about whether the adolescent has done a particular activity with their mother in the last four weeks.[9] The full list with summary statistics is reported in the appendix, Table A.3.

We aggregate these using a principle component analysis (PCA) and reported rotated component loadings in the appendix, Table A.4.[10] The PCA returns three reasonable components that each explain a greater share of variance than a single item. The strongest of these explains nearly three times as much variance as a single item.[11] We then assign component interpretations based on loadings that are above 0.4. The first, and strongest, component loads on schooling-related involvement items. The second loads on activities – such as playing sports or going shopping. And, the third loads on communication items not directly related to school. We generate scales for each of the components based on the roated loadings.

The three items loading on the first component focus on mother's involvement in school-related matters. These are: (1) talking about school work or grades, (2) working together on a school project, and (3) talking about other things you are doing in school. Our hypothesis is that these are the most directly related to school-trouble, and it is these three items that drive the strongest component. Thus, we take the schooling-related involvement scale

9. Answers are no, yes (0,1).

10. Because of the binary nature of the involvement variables, we use the polychoric correlation matrix from the involvement variables for the PCA.

11. It has an eigenvalue of 2.94, while the remaining two components are 1.428 and 1.232. No other component is above 1.

as our preferred scale, though we do report results for the other two scales at the baseline.[12] We generally will refer to the schooling-related involvement scale simply as involvement and note where we use the alternative scales.

2.4 Sample Selection and Controls

We control for observable maternal characteristics, household characteristics, and adolescent individual characteristics drawn from the in-home wave I and the wave I parent survey. These include mother's education level indicators, mother's age, household income, the number of siblings in the home, an indicator for single parent homes, whether the adolescent is female, race and ethnicity, school-grade indicators, and school fixed effects. We also control for whether the interview took place during the summer months since some of the in-home surveys did not occur until this point.[13]

To construct our dependent variable, we dropped individuals who were not in school during wave I (395), who were older than 19 (85), who have missing values for any of the school-trouble scale measures (412), or who are extreme outliers in the number of skipped school days (236). The full sample, after constructing the dependent variable, consists of 19,617 observations. For our final selected sample, we drop observations with missing values for mother's involvement or peer mothers' involvement.[14] We also drop observations whose respondent to the parental survey is listed as male or as not the biological mother, when the biological mother, in fact, lives in the home. We do this because maternal education is taken from responses to the parental survey. This accounts for only a small percentage of obser-

12. In extended robustness checks, we also evaluate whether maternal schooling related involvement is affected by peer maternal invovlement defined on the other two scales. We find no evidence that it is the case.

13. Our results are also highly robust to interview month fixed effects (results not shown). Moreover, we obtain nearly identical estimates to our baseline on the sub-sample omitting summer interview observations though we lose some efficiency.

14. When one of the control variables is missing, we impute a value (the mean for a continuous variable and zero for a discrete variable) and add a missing indicator.

vations that are dropped (384 total).[15] Our final selected sample consists of 12,316 observations.[16]

We then check that the distribution of our key schooling-related maternal involvement variable is similar across the selected and unselected sample. The appendix, Figure A.1 shows the comparison. The distributions are very similar across samples and indicate considerable variation across the scale.[17]

In the appendix, Section A.2, we report summary statistics for the sample used to construct school-trouble and for the final selected sample. Table A.5 shows that the mean differences are in some cases statistically significant; however, in all cases the magnitudes of these differences are very small, indicating that the full sample and the selected sample are very similar. We also show, in Appendix, Figure A.2, that in our selected sample our instrument has a strong degree of variation to identify first stage effects even after removing school fixed effects.

3 Empirical Strategy

We use a standard linear regression model to estimate the causal effect of mother's involvement on school trouble:

$$Y_{is} = I_{is}\alpha_1 + X_{is}'\alpha_2 + \alpha_s + \varepsilon_{is}. \tag{3.1}$$

Here, Y_{is} is school trouble for adolescent i in school s; I_{is} is our measure of maternal involvement; X_{is} is a vector of covariates; α_s is a school fixed effect

15. The specific numbers of observations dropped at each stage of the sample selection process are given in Table A.5 in the appendix.

16. Our sample selection is not unlike other studies who have used Add Health for similar analysis with the in-home data. For example, see Fruehwirth et al. (2019) who use Add Health and a similar identification strategy to ours to explore the effect of religiosity on mental health and have a very similar selected sample size.

17. The variation clusters around four points which we might expect given that three items drive the scale. Nevertheless, the scale is approximately continous as all items contribute some extent of variation based on the rotated loadings in the appendix Table A.4.

and ε_{is} represents unobserved heterogeneity. An obvious concern is that I_{is} may be endogenous, for example due to reverse causality between Y_{is} and I_{is}.

Becker and Tomes (1976) suggest that parents' involvement with their children may follow either an "enhancement model" or a "response model." In the enhancement model parents become more involved when their children do better and experience less school trouble, resulting in a negative correlation between I_{is} and ε_{is}. Assuming that α_1 in equation (3.1) is negative, the OLS estimator $\hat{\alpha}_1$ will be biased away from zero and will overestimate the magnitude of the effect of involvement. Alternatively, in the response model parents increase their involvement in response to school trouble.[18] Consequently, I_{is} and ε_{is} will be positively correlated. In this case – assuming again that α_1 is negative – the OLS estimator $\hat{\alpha}_1$ will be biased towards zero and will underestimate the magnitude of the involvement effect.

To estimate the effect of mother's involvement on school trouble, we follow an identification strategy similar to the one proposed by Fruehwirth et al. (2019). They use peer religiosity as an instrument to estimate the effect of religiosity on mental health. In this paper, we use as an instrument the average of maternal involvement, excluding the individual, in a suitably chosen peer group.[19] For a given adolescent, the peer reference group is defined as adolescents in the same school, grade, race and gender group and whose mothers have the same education level.[20] Thus, our instrument is the leave-one-out mean involvement among peer mothers who share the same school-grade-race-gender-mother's education (SGRGE).

The rationale behind this instrument is the idea that mothers with similar education levels and whose children are similar (in terms of the characteristics listed above) are more likely to interact with and influence each other.

18. This is sometimes referred to as the "reactive hypothesis." See, for example, McNeal (2012).

19. This is known as the leave-one-out mean and is standard in the peer effects literature.

20. In our data, we categorize the mother's self-reported level of education as (1) no high school, (2) high school diploma, (3) some college, (4) college graduate and (5) post-college training.

This idea is not new: Earlier studies by Carbonaro (1998), Sheldon (2002), McNamara Horvat et al. (2003), and Mullis et al. (2003) have all found that parental networks can influence parents. Additionally, Kalil (2015) point out evidence suggesting parents, especially less educated parents, are more likely to take advice from their social circle than from experts.[21] Thus, by choosing a peer reference group at a level where the mothers are likely to interact, we expect the instrument to be relevant for mother's involvement.

If $g(i)$ denotes the peer reference group and $\bar{I}_{g(i)s}$ is the leave-one-out average level of maternal involvement in that group, the first-stage model can be written as

$$I_{is} = \bar{I}_{g(i)s}\beta_1 + X'_{is}\beta_2 + \beta_s + u_{is}, \tag{3.2}$$

where β_s is a school fixed effect and u_{is} a residual. Our identifying assumptions are (1) $\beta_1 \neq 0$ and (2) $E(\varepsilon_{is}|\bar{I}_{g(i)s}, X_{is}, S_i) = E(\varepsilon_{is}|X_{is}, S_i)$, where S_i is an indicator for the school of adolescent i. Assumption (1) is the instrument relevance condition. Assumption (2) and the exclusion of $\bar{I}_{g(i)s}$ from Equation (3.1) combine the exogeneity of the instrument and the exclusion restriction.

Our model accounts for selection at the school level. Selection implies that there are unobservables that are correlated with both the reference group and school trouble. An example is a case where more involved parents sort into schools that are better resourced, correlating peer group involvement with school resources, which may also determine school trouble. Our empirical strategy avoids such factors by isolating within school and between-cohort variation in maternal involvement conditional on school fixed effects. If parents select schools based on school-level characteristics, variation in maternal involvement between peer reference groups will be exogenous after controlling for school fixed effects.[22] An example that would violate this, would

21. Consistent with this point, in the supplementary appendix, Table B.2, we indeed find a pattern suggesting a stronger involvement response to peer mothers' involvement by mothers with less education.

22. This is a now well-known argument in the peer effects literature. See Sacerdote

occur if parents within SGRGE cells share information on available school resources and at the same time maternal involvement is associated with better information. While this seems unlikely, we will consider a number of sensitivity analyses in Section 4.2.1, aimed at detecting possible selection on unobservable factors. We also test against the presence of within-cohort common teacher effects that could similarly violate this assumption. In all instances, the estimates are very similar to the baseline results.

The presence of peer effects in school trouble could also lead to a violation of Assumption (2). If a relevant measure of peer school trouble is incorrectly omitted from Equation (3.1) but correlated with our instrument, the 2SLS estimator of α_1 will be biased. If, on the other hand, the instrument is uncorrelated with the peer effect, the 2SLS estimator remains unbiased. In this context, it is important to be specific about the notion of a peer effect, since different features of the peer school trouble distribution could affect an adolescent's own school trouble. A natural candidate is the (leave-on-out) average of peer school trouble. Alternatively, it could be that the tails of the peer distribution (i.e., the low-trouble or high-trouble peers) drive the peer effect.

As part of our identification strategy, we assume that a peer effect, if present, operates through exposure to peers in the tails of the school trouble distribution. While peer mothers' involvement is likely correlated with the average of peer school trouble, we assume that the average is correctly omitted from Equation (3.1) as a measure of a peer effect. In Section 4.2.2 we present several empirical analyses that support these assumptions. First, the estimated coefficient of the leave-one-out average of peer school trouble is very close to zero. This is consistent with a null or negligible peer effect in terms of the average.

In the case that peer average school trouble did have a positive effect, then including a peer effect measure that is also endogenous could lead to

(2014) for a comprehensive review.

bias in all estimated coefficients. In the Supplementary Appendix B.8, we examine this more formally and demonstrate clearly our necessary assumptions. We believe, however, that this possibility is unlikely for two main reasons. First, we find robust estimates of the effect of mother's involvement across all of our sensitivity checks for peer school trouble effects, and it is unlikely that all suffer from a more or less identical amount of bias. Second, we experiment with using an additional instrument, and in these cases, we fail to reject the null that the overidentifying restrictions are satisfied. This further suggests that the peer average is correctly omitted from Equation (3.1). Finally, we present results that suggest that the extremes of the school trouble distribution, rather than the average, affect the adolescent's school trouble. The estimated coefficients are large and statistically significant, but do not alter our estimated effect of mother's involvement. In addition, we present evidence that our instrument is uncorrelated with these relevant peer effect measures, thereby lending further credibility to our baseline results.

Flexible checks on peer effects in school trouble also help us test against threats from common shocks within schools. The idea here is that violations via shocks within the school would likely correlate school trouble across individuals in our refined reference group. Thus, our inclusion of flexible forms of peer effects in school trouble in Section 4.2.2 should capture these and lead to sensitivity in our results if they represent violations of the IV assumptions. We again find that estimates for the effect of maternal involvement on school trouble remain essentially unchanged.

Finally, in Sections 4.2.3 and 4.2.4, we explore a range of additional sensitivity checks including concerns around fathers' involvement and a machine learning approach for instrument and control variable selection. We continue to find evidence consistent with our baseline result, lending further credibility to the assumption of instrument exogeneity and the exclusion restriction. Subsequently, we examine heterogeneity in Section 4.3 and explore some potential mechanisms that can explain the effect of mother's involvement on school trouble in Section 5.

4 Results

4.1 Baseline Results

We report our baseline results in Table 1.[23] All specifications control for school fixed effects, our controls and, where applicable, missing indicators for the control variables. Standard errors are clustered at the school level. In the first row, we report estimates for the schooling-related involvement scale. The OLS estimate of mother's involvement in column 1 is negative and significant but may be either over- or underestimated. Under the response model, where mothers respond to poor behavior in school with more involvement, this estimate is biased toward zero.

Next, we turn to 2SLS. The first-stage estimate in column 2 shows that peer mothers' involvement is positively and significantly related to maternal involvement, suggesting that the instrument is indeed relevant. In column 3, we report the second-stage estimate. Based on this, a standard deviation increase on our scale of maternal (schooling-related) involvement translates into nearly half a standard deviation decrease in school trouble. This effect is larger in magnitude than the OLS estimate and suggests that endogeneity leads to a substantial attenuation bias.[24]

To provide some context for the magnitude of the 2SLS estimate, consider the difference in average involvement and school trouble between mothers with no high school degree and those with post-college training. From column (2) of Table B.3 in the supplementary appendix, the conditional mean difference in involvement is about 0.35 standard deviations. Our 2SLS estimate predicts that this leads to a difference of about $0.35 \times 0.47 \approx 0.16$ standard deviations on the school trouble scale. Given the between-group

23. A full table of results is available in the supplementary appendix, Table B.4.
24. Recent evidence on the impact of parental investments during early childhood also point to attenuation bias in OLS (Attanasio 2015; Attanasio et al. 2020).

difference in school trouble of about 0.43 standard deviations (see column (4) of Table B.3), our 2SLS estimate shows that about 39% of the difference in school trouble between mothers without a high school degree and mothers with post-college training can be explained by the difference in mother's involvement.[25]

The Kleibergen-Paap F statistic (K-P F) is 13.451, suggesting that the instrument is reasonably strong, yet weak instrument bias is a concern. We follow the advice of Andrews et al. (2018) and report the Anderson-Rubin (AR) weak instrument robust test for the null hypothesis that $\gamma = 0$.[26] The AR test rejects the null with a p-value of 1.5%. Thus, our IV estimate does not appear to be driven by weak instrument bias.

As demonstrated in the supplementary appendix, Table B.2, the school-trouble scale is strongly associated with future education and wages. Depending on the specification chosen from Table B.2 and based on a simple translation, a standard deviation increase in mother's involvement is associated with a 1.9%-6.3% increase in future wages. Together with the 2SLS estimate, this result implies that maternal involvement can have a long-lasting impact.

Our primary baseline result is the estimate for schooling-related maternal involvement; however, in columns 4 and 5 we replace this scale with the activities related scale (column 4) and the non-schooling-related communication scale (column 5). Our aim is to explore the relationships between school trouble and different available measures of involvement. For each measure, we define the instrument as the average of that measure in our reference group.[27]

25. Some of the involvement effect may be explained by other factors, for example early childhood parental investments. However, given our data and a single instrument, it is not feasible to decompose the "overall" involvement effect into a number of indirect effects.

26. In our single endogenous regressor just identified case, the AR test is both robust to weak instruments and efficient (Andrews et al. 2018).

27. In the supplementary appendix, Table B.5, we examine the first stage relationship between the peer average of our primary scale and each alternative scale. We show that

Table 1. School-Trouble and Maternal Involvement

	OLS	First-Stage	2SLS		
	(1)	(2)	(3)	(4)	(5)
Mother's Involvement (School)	-0.109***		-0.474**		
	(0.010)		(0.224)		
Peer Mothers' Involvement		0.069***			
		(0.019)			
Mother's Involvement (Act.)				-0.488**	
				(0.239)	
Mother's Involvement (Comm.)					-0.203
					(0.289)
School FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	12316	12316	12316	12316	12316
K-P F			13.461	8.174	7.094
AR Weak IV Robust p			0.015	0.019	0.431

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. All specifications include school fixed effects, our base set of controls, and missing indicators for missing observations in our control set. Mother's involvement (our preferred scale) is extracted from the PCA first component rotated loadings and is defined by the schooling-related items. Mother's involvement (Act.) is based on the second component which loaded on activities, and mother's involvement (Comm.) is based on the third component, which loaded on non-schooling related communication. All scales predicted based on the rotated loadings and are standardized to mean zero and standard deviation of one. Column 2 reports the first stage of average peer mother's involvement (schooling-related scale) at the school-grade-race-gender-mother education level on mother's involvement. The Anderson-Rubin (AR) weak IV robust p-values are reported at the 95% level and 250 gridpoints. These report a weak instrument robust test of the null that $\gamma = 0$.

We find a similar effect on the activities scale, suggesting there are benefits from wider types of involvement. However, the first stage is weaker (K-P F:

the average of peer mothers' schooling-related involvement is not related to the alternative scales.

8.174); while it passes the AR test, we pursue our robustness checks and further analysis around the school-related scale, which exhibits a stronger first stage and is more directly related to school. Next, on the communication scale, we find a point estimate that is not significant.

We do not claim that other measures of involvement are irrelevant; rather, the schooling-related measures seem particularly important. Thus, in the remainder of this paper we use our preferred measure of mother's involvement. Of course, the reliability of our baseline estimate rests on the validity of the exclusion restriction for the instrument. In the following sections, we explore several robustness checks aimed at detecting potential violations of that restriction.

4.2 Robustness Checks

In this section, we present a number of robustness checks aimed at detecting potential violations of the exclusion restriction. We organize these checks around several different channels. First, we consider the possibility that there are unobserved effects that are not accounted for by school fixed effects and that correlate with both peer mothers' involvement and adolescent school trouble. An example of this is the presence of teacher effects that vary within the school. If a specific teacher has a significant impact on a student's school trouble and also encourages parental involvement, then the exclusion restriction would not hold. Second, we investigate the possibility of peer effects in school trouble. If average school trouble in the peer group affects the adolescent, then the average involvement of mothers in the peer group is an invalid instrument.[28] Third, we explore whether our instrument affects other forms of parental involvement, which in turn can affect school trouble. Fourth and final, we consider robustness with respect to the

28. A maybe less likely concern is that peers' parents directly affect the adolescent. If this is the case, we expect our estimates to be sensitive to the inclusion of a range of peer means of parental characteristics. We investigate this in Section 4.2.2.

choice of instruments, control variables and functional form by employing a lasso-based 2SLS estimator.

4.2.1 Robustness to Selection

We consider the inclusion of a variety of additional controls that would reasonably be associated with a selection mechanism, if one is present. Table 2 reports our results. In columns 1-3, we control for peer maternal involvement in different peer groups that get progressively closer to the group that defines our instrument. We control for peer maternal involvement at the same school and grade level in column 1, at the same school, grade and race level in column 2, and at the same school, grade, race and gender level in column 3. We expect that if unobservables are correlated with both our instrument and school trouble, then controlling for maternal involvement in different peer groups should result in sensitive estimates. For example, if the added control variables for mothers' involvement are correlated with an unobserved teacher effect, we expect estimates of our treatment effect to be sensitive to their inclusion.

We find that the estimated effect of mother's involvement remains robust and significant at the 5% level in all cases of controlling for peer maternal involvement at different reference groups (columns 1-3). For a common teacher effect to be completely missed here, this effect would have to be strictly demarcated along the exact definition of our peer reference group used for the instrument, i.e., school-grade-gender-race-mother's education. This seems unlikely, and the evidence here supports our identifying assumptions.

In column 4, we include the Add Health Peabody picture vocabulary test (AH PVT) score as a control for the adolescent's cognitive ability in case school fixed effects has not adequately captured selection on ability. We find this has little impact on the estimated effect of mother's involvement, nor does it affect the strength of the instrument.

Next, in columns 5-6 we include school trends. Our first approach is to

Table 2. Robustness to Selection: Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mother's Involvement	-0.386**	-0.401**	-0.546**	-0.448**	-0.643	-0.380*	-0.636*	-0.154	-0.572**	-0.466**
	(0.181)	(0.185)	(0.268)	(0.223)	(0.412)	(0.214)	(0.359)	(0.301)	(0.269)	(0.227)
SG Peer Mothers' Inv.	-0.107									
	(0.072)									
SGR Mother's Inv.		-0.046								
		(0.042)								
SGRG Mother's Inv.			0.020							
			(0.037)							
AH PVT				-0.112***						
				(0.015)						
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SG Trend	No	No	No	No	Yes	No	No	No	No	No
SG-Peer Mothers' Inv. Trend	No	No	No	No	No	Yes	No	No	No	No
Not a PTO Member	No	No	No	No	No	No	Yes	No	Yes	No
PTO Member	NA	NA	NA	NA	NA	NA	No	Yes	No	NA
Peer Mothers Not PTO	No	No	No	No	No	No	Yes	No	Yes	No
Block and County Controls	No	No	No	No	No	No	No	No	No	Yes
N	12316	12316	12316	12316	12316	12316	8226	4078	7651	12316
K-P F	18.390	17.132	9.579	13.085	4.755	14.017	8.121	5.973	14.251	13.269

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. All specifications include our baseline controls and school fixed effects. Inv. is involvement; SG is school-grade; SGR is school-grade-race; SGRG is school-grade-race-gender. Each of these refers to the definition of the peer group level used in controlling for the peer mean. AH PVT is the Add Health Peabody Picture Vocabulary test score administered to the adolescent. SG-peer trend includes an interaction between grade-level and each school indicator. SG-peer mothers' involvement trend includes an interaction between school-grade level average peer mother involvement and each school indicator. PTO is an indicator for whether the parent reports being apart of a parent-teacher organization. We report results restricting to those who are not (column 7) and then to those who are (column 8) a member. Block and country controls are as follows: the census block-level percent of children above the age of three in private school (elementary or high school), the census block-level percent of adults with a college degree, and the county total juvenile arrests per 100,000 of the population.

interact each school indicator with a grade-level variable (column 5), allowing across grade trends. The estimated coefficient for maternal involvement is similar to our baseline estimate, though it, and its standard error, slightly increase in magnitude. Our second approach is to interact each school indicator with the same school-grade peer average maternal involvement to control for school trends at the school-grade level in peer mothers' involvement. In column 6, controlling for differences in peer mothers' involvement between schools and grades, the estimate remains similar to our baseline result.

In columns 7 and 8, we again consider potential teacher effects. We use information from the parent survey about membership of a parent-teacher organization (PTO). In columns (7) and (8), we restrict the sample to observations where the parents were not members of a PTO, or were members of a PTO, respectively. If a common teacher effect drives our results, we expect our estimates to be mostly determined by parents who are more likely to interact with teachers, thereby correlating their involvement with less school trouble. We find no evidence for this; in fact, the results suggest the opposite.[29] The estimates in columns (7) and (8), while more noisy because of the smaller sample size, suggest that those who are not members of a PTO drive the effect of mother's involvement. This pattern is consistent with a story that parents who are less engaged with teachers rely more on other parents for advice. Relatedly, Kalil (2015) indicates that less educated parents rely more on their networks for parenting advice. Indeed, in our data, parents who are not members of a PTO have on average less education.[30]

In column 9, we extend our check against concerns over interactions with teachers. Here we restrict both to mothers who are not PTO members and restrict the peer reference group to only be those peer mothers who are also not in a PTO. The idea here is to remove those mothers from the peer reference group who are more likely to interact with teachers. Here the 2SLS point estimate for the effect of maternal involvement is significant and

29. We recognize that membership of a PTO could be a form of involvement and thus, may be endogenous. This sensitivity check is therefore given with caution. Nonetheless, these in combination with columns (1)-(3) still confirm the robustness of our baseline estimate.

30. We also show in the Supplementary Appendix Figure B.2b that, indeed, mothers with less education appear to respond more strongly to peer maternal involvement and in Supplementary Appendix Table B.9 that 2SLS effect appears stronger for less educated mothers. These findings are consistent with Kalil (2015) and our results here.

quite similar to the baseline. We see this as entirely consistent with our expectations given the previous discussion and suggestive that a correlation between our instrument and teachers is creating a violation of the exclusion restriction. Again, it appears to be those mothers less involved at the school are most affect each others involvement.

Finally, we include census controls aimed to capture features that may predict common shocks around involvement and school trouble. In doing so, we aim to capture neighborhood effects that may vary within school and correlate with our instrument and outcome. In column (10), we add controls for the census block-level percent of children above the age of three in private school (as a proxy for block-level parental investments), the block-level percent of adults with a college degree, and county-level juvenile arrests per 100,000 population. We again find that the effect of maternal involvement on school trouble remains highly robust.

Overall the results in Table 2 support our claim that selection into schools is largely based on factors fixed at the school level. These are accounted for by the school fixed effects. To test this further, we also explore balancing tests in Section B.3 and Table B.6 of the supplementary appendix. In these tests, we regress the observable controls that are not part of our peer group definition on our instrument. Moreover, we supplement this set with some additional characteristics: being the first born and birth weight. If selection effects are removed conditional on school fixed effects, then we do not expect much correlation to exist between these variables and our instrument. We find no evidence that our instrument is related to these controls, further suggesting that any selection effects have been removed.

4.2.2 Robustness to Peer Effects

In this section, we consider the potential impact of peer effects in school trouble on our estimates. First, we note that even if adolescent peer effects are present, these do not necessarily invalidate our instrument. A number of studies do not find evidence that the mean of outcomes—for example, ability—drives the relevant margins of peer effects in school based reference groups (Sacerdote 2014). The findings of Lavy et al. (2012) on ability peer effects suggest that cohort peer effects may be driven by "extreme" peers. In our case, this would be those on either the high or low end of the school trouble spectrum, rather than by peers near the average. Key for our study is that to the extent that peer mothers' involvement shifts school trouble within the peer group, it must not shift the relevant margin for peer effects in school trouble, as we discuss in more detail in the Supplementary Appendix Section B.8.

Regarding instrument validity, we first check the conditional correlation between our instrument and the percentage of peers (within the SGRGE reference group) in the bottom decile (high peer quality) and the top decile (low peer quality) of the school-grade, school trouble distribution. These results are reported in the Supplementary Appendix, Table B.6. We find no relation between our instrument and these measure of high and low school trouble among peers. Next, we test for sensitivity in our second-stage estimate for maternal involvement after introducing both average school trouble at our reference group level and the shares of high and low trouble among these peers.

In column 1 of Table 3, we add to our baseline controls only the leave-one-out average of school trouble in the peer group defined by the school, grade, race, gender and mother's education level, and in column 2, we add to this the leave-one-out average of peer ability (AH PVT scores) defined at the same level.[31] In column 3, we further add the percentage of high- and low-quality peers in terms of school trouble and ability in the SGRGE

31. We prefer to use this reference group over an adolescent peer group based on friendship nominations, because the latter is subject to selection effects. Interestingly, when we re-estimated the model in column (1) with peer measures based on friendship links, the estimated effect of mother's involvement remained similar (-0.45) and significant at the 5% level.

reference group, and in column 4, we further supplement the controls with means of peer characteristics. In all of these specifications, our effect of interest on maternal involvement remains remarkably consistent and very near the baseline estimate.

The estimated coefficient for average peer school trouble is small and negative in columns 1-2. This could be due to exclusion bias (Caeyers and Fafchamps 2020). Exclusion bias is mechanical and arises because individuals cannot be their own peer. If the leave-one-out average of peers' outcomes is high, the outcome for the individual is more likely to be low, and vice versa. Consequently, a regression with the leave-one-out average as a control variable yields a coefficient estimate that is confounded by negative correlation and contains a negative bias. In columns 3-4, where we add the high- and low-quality peer controls, the effect on the peer average is now positive but it is small in magnitude and does not change our estimate for the effect of maternal involvement. As we show through a simulation in the Supplementary Appendix, Figure B.4, our estimated treatment effect for maternal involvement can tolerate relatively sizable effects from average peer school trouble and our evidence here is consistent with that.

Finally, for peer quality, the estimates are intuitive: High-quality peers (very low school trouble) decrease school trouble, while low-quality peers increase it. Since we found no relation between the low-quality and high-quality peer measures and our instrument, as noted earlier, it is not surprising that including these peer measures here does not fundamentally change our baseline 2SLS estimate. Additionally, when we omit the leave-one-out peer averages in column 8, in order to focus on the low- and high-quality peer measures, we continue to find no real change in the estimated maternal involvement effect.

In column 5, we supplement the specification estimated in column 3 with a quadratic in the leave-one-out mean of school trouble to allow for nonlinearities at the peer mean. We find no evidence for nonlinearities on this margin

Table 3. Robustness to Peer Effects I

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's Involvement	-0.491**	-0.466*	-0.470*	-0.460*	-0.470*	-0.469**	-0.465*	-0.512**
	(0.250)	(0.250)	(0.257)	(0.254)	(0.257)	(0.232)	(0.255)	(0.258)
Peer School Trouble	-0.012	-0.012	0.055*	0.057**	0.057**		0.055*	
	(0.026)	(0.025)	(0.029)	(0.029)	(0.028)		(0.029)	
(Peer School Trouble)^2					-0.004			
					(0.013)			
Low Peer Quality (School Trouble)			-0.429***	-0.430***	-0.423***		-0.430***	-0.279**
			(0.112)	(0.112)	(0.114)		(0.111)	(0.113)
High Peer Quality (School Trouble)			0.223**	0.220**	0.230**		0.221**	0.125
			(0.088)	(0.087)	(0.089)		(0.087)	(0.081)
SG Peer School Trouble						0.015		
						(0.078)		
Peer Maternal Labor Supply							0.002	
							(0.038)	
SGRGE AH PVT	No	Yes	Yes	Yes	Yes	No	Yes	Yes
SGRGE Avg. Controls	No	No	No	Yes	No	No	No	No
Sch-Grade AH PVT	No	No	No	No	No	Yes	No	No
Sch-Grade Avg. Controls	No	No	No	No	No	Yes	No	No
N	12316	12316	12316	12316	12316	12316	12316	12316
K-P F	11.888	11.438	10.828	11.132	10.831	12.311	10.920	11.260

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. All specifications control for school fixed effects, our base set of controls, and missing indicators for our controls. Column 1 reports 2SLS estimates for our preferred schooling related involvement index after controlling for the SGRGE peer leave-one-out mean of school trouble and column 2 adds the SGRGE leave-one-out mean of AH PVT scores. Columns 3 is similar but adds high and low quality peers defined by the percent of SGRGE peers in the bottom (top) decile of the school-cohort school trouble distribution. We also include SGRGE peers' AH PVT scores, the percent of SGRGE peers in the bottom (top) decile of the school-cohort AH PVT distribution, and own AH PVT scores. In column 4 we further add SGRGE peer average characteristics: number of siblings, parental age, single parent homes, and household income. We cannot include peer controls on the variables used to define the peer reference group. Column 5 reports results supplementing the specification in column 3 with a quadratic in the leave-one-out mean of peer school trouble. Column 6 reports 2SLS estimates controlling for peer level averages at the school-grade level. Column 7 repeats the specification in 1 and adds mother's and peer (SGRGE) mothers' labor force participation. Column 8 repeats the specification in column 3 but omits the leave-one-out mean peer variables. Missing indicators and imputation are included throughout where needed.

and our effect estimate for maternal involvement remains unchanged. In column 6, we shift to the school-grade peer group level and again control for peer skills and peer averages in the control variables.[32] Again, our estimate of the effect of maternal involvement remains stable.

32. We do not repeat the high and low quality peer controls here because we defined those based on the school cohort distribution.

A related concern that would violate the exclusion restriction is that the mothers of an adolescent's peers may have some direct effect on the outcome. We believe this to be unlikely, particularly in high school and given that we are not using actual friend groups but exogenously defined peer groups; however, we consider this possibility in columns 4 and 6 by including controls for a wide range of peers' parental background characteristics. While these characteristics only serve as a proxy for peer mothers' involvement, we again find no sensitivity in the estimated effect of mother's involvement.

Next, in column 7, we add an indicator for whether the mother works outside of the home and control for the percent of mothers who work in our main peer reference group (SGRGE). Olivetti et al. (2020) find that exposure to the labor force participation of their peers' mothers increases participation among girls, potentially through identity formation. Thus, given that identity formation could plausibly influence school trouble here we check that our instrumenting strategy with peer mothers is not sensitive to this factor. Again, we find our results are robust.

For a second set of sensitivity checks, we develop an additional instrument by redefining the peer group based on another potentially relevant dimension for mothers, namely religious denomination.[33] To sort denominations, we follow the same approach as Fruehwirth et al. (2019).[34] We list the categories in the supplementary appendix, Table B.7 and provide the frequency distribution.

In Table 4 we report the first- and second-stage, using as an instrument only the average of peer mothers' involvement from the new peer group definition. We first condition on observations that are non-missing in this variable. The first-stage (column 1) is similar to the baseline first-stage estimate, although the instrument is slightly weaker with a K-P F of 9.698. However,

33. We will refer to this as the school-grade-race-gender-mothers' religious denomination (SGRGR) peer group.

34. The only difference is that we use the mother's report of religious denomination, whereas Fruehwirth et al. (2019) use the adolescent's report.

the estimated effect of maternal involvement (column 2) is similar to our baseline estimate and significant at the 5% level.

In columns 3-4, we use both our new and original instrument, conditioning on the sample that is non-missing in either instrument ($N = 10,670$). In the first stage, each instrument remains significantly correlated with maternal involvement, and our second-stage estimate again remains stable and statistically significant. Moreover, we do not reject the null hypothesis that the overidentifying restrictions are valid.

Table 4. Robustness to Peer Effects II

	(1) 1st-Stage	(2) 2SLS	(3) 1st-Stage	(4) 2SLS	(5) 1st-Stage	(6) 2SLS
Mother's Involvement		-0.627**		-0.551**		-0.476***
		(0.272)		(0.229)		(0.178)
Peer Mothers' Involvement			0.056***		0.069***	
			(0.020)		(0.020)	
SGRGR Peer Mothers' Inv.	0.071***		0.051**		0.055**	
	(0.023)		(0.024)		(0.024)	
N	12117	12117	10670	10670	12316	12316
K-P F		9.698		7.915		11.271
Over-ID p				0.355		0.338
AR Weak IV Robust p		0.006		0.013		0.008

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. Inv. is involvement. All specifications control for school fixed effects, our base set of controls, missing indicators for our controls, and indicators for the mother's religious denomination. Columns 1 and 2 report the first and second stages from redefining the reference group to the same school-grade-race-gender-mother's religious denomination (SGRGR). We omit observations missing peer mother involvement at this reference group definition. Column 3 and 4 report the first and second stages using peer mother involvement at both our original reference group and redefined group as instruments. Column 5 and 6 report results after setting missings in peer mother involvement for the redefined level to the mean and controlling for a missing indicator in both stages. Again we have two instruments of peer mother involvement at two definitions of the reference group.

Finally, in columns 5-6, we return to our original selected sample by imputing missing observations in SGRGR peer mothers' involvement to the mean and including an indicator for missingness. We include the missing indicator in both stages but maintain our instrument set. The estimated effect of maternal involvement is about -0.48 and significant at the 1% level, even when using the weak-instrument robust AR test.

In summary, we checked for possible violations of the exclusion restriction that may run through the adolescent's peer group but find no evidence consistent with this concern. The estimates presented in Tables 3 and 4 are similar to the baseline estimate in Table 1. Next, we consider robustness with respect to using alternative forms of parental involvement, and to selection of instruments and control variables.

4.2.3 Robustness to Alternative Forms of Parental Involvement

Different types of parental involvement, for example focused on other activities or coming from fathers, might impact school trouble. If these are affected by the instrument, it would violate the exclusion restriction. We start by exploring the relationship between the instrument and alternative forms of mother's involvement (activities and communication). Based on the estimates reported in Section B.5 of the supplementary appendix, Table B.8, we find no evidence of such a relationship. This suggests that peer mothers' involvement in schooling-related matters is indeed strongly related to mother's involvement of the same type but not to other types of involvement. While we cannot precisely disentangle how mothers in the peer reference group interact and influence each other, these results further support the relevance of the instrument.

Next, we consider father's involvement as an additional form of parental involvement. If fathers respond to peer mothers' involvement, we would again have a potential violation of the exclusion restriction. We examine this in Table 5. We use the average of peer mothers' schooling-related involvement as the instrument and consider different ways of controlling for mother's and father's involvement in the school trouble equation. First, we form a combined involvement measure that is the sum of the mother's and father's schooling-related involvement. When data on the father is missing, which frequently occurs, we use mother's involvement instead. The estimate in column 1 is similar to our baseline result. The same is true for the first-stage K-P F statistic.

Second, the estimates in column 2 are based on instrumenting maternal involvement while controlling for the father's involvement. We impute missing fathers to the mean and use a missing indicator as a control variable. Column 2 shows that our estimate for mother's involvement is somewhat larger but still yields the same conclusions as our baseline model.

Finally, in columns 3-4 we report results from regressing father's involvement on maternal involvement, our instrument, and baseline control set. As long as fathers respond to the mother but not directly to peer mothers, there is no threat to the exclusion restriction. To maintain our selected sample, we maintain the imputation for missing fathers in column 3 and control for the missing father indicator. Father's and mother's involvement are highly correlated, as expected, but we find no significant correlation between peer maternal involvement and father's involvement. To ensure that this result is not driven by data imputation for missing fathers, we restrict the sample to non-missing fathers in column 4. Again, we find no correlation between our instrument and the father's involvement. While maternal involvement is endogenous in these regressions, this evidence is consistent with peer mothers' schooling-related involvement affecting the mother directly but not the father.

4.2.4 Selecting Instruments and Controls

Our choice of instrument is based on a homophily argument: mothers are more likely to interact with and be influenced by other mothers who have similar education levels and whose children have similar characteristics. This still allows for several different ways to define peer groups. Beforehand, it is not necessarily clear what the most relevant grouping will be. A second

Table 5. Father Involvement: Robustness Checks

	School Trouble		Father's Involvement	
	(1)	(2)	(3)	(4)
Parental Involvement	-0.555**			
	(0.256)			
Mother's Involvement		-0.570*	0.423***	0.595***
		(0.312)	(0.013)	(0.012)
Father's Involvement		0.212		
		(0.177)		
Missing Father's Involvement		0.317	-0.414**	
		(0.242)	(0.159)	
Peer Mothers' Involvement			0.002	0.013
			(0.009)	(0.013)
N	12316	12316	12316	8775
K-P F	13.582	10.207		

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. All specifications include the base set of controls, missing indicators for controls, and school fixed effects. Inv. is involvement. Column 1 uses a combined mother/father standardized scale of the sum of mother and father involvement (equal to mother if father missing and vice-versa). Column 2 instruments mother's involvement and controls for father involvement and missingness in father involvement. Columns 3 reports on a father involvement specification where we maintain our analytic sample via imputation to the mean and controlling for missingness in father involvement. Columns 4 reports on a specification removing imputation and dropping observations missing in father involvement.

issue is functional form: can we use a linear model for the relation between the instrument and maternal involvement, or should we account for possible nonlinearities (e.g., through polynomials or interactions)? If a large number of nonlinear transformations of the instrument are used, however, the instrument set overall may be weak and can lead to familiar bias problems.

To address these issues and assess the robustness of our baseline results further, we consider several approaches. First, we employ the lasso-based method of Belloni et al. (2012) to select the optimal instruments among a large set of candidate instruments. The selected instruments are then used to calculate the standard 2SLS estimator. Second, variable selection issues also affect the school trouble equation. Inclusion of too many controls reduces the efficiency of the estimator, therefore we apply the post-double-selection (PDS) lasso approach proposed by Belloni et al. (2014). In this approach, the lasso is used twice to select two sets of control variables: one that predicts school trouble and one that predicts mother's involvement. A third lasso step is employed for instrument selection, whereby the selected controls from the mother's involvement lasso are always included in the model. The final step calculates the 2SLS estimator with the union of selected controls from first two lasso steps and the selected instruments from the third step. The advantage is that we can reduce the dimensions of the control set and can also explore including many controls at once.

Results are reported in Table 6.[35] In column 1, we only allow selection of instruments. The instruments are the average of peer mothers' involvement at six different configurations of the peer group.[36] For each definition of the instrument, we also include second and third degree polynomials to capture possible nonlinearities. The total number of included instruments is 18. In column 2, we repeat the exercise but also allow the controls to be selected. There are a total of 21 variables in our original control set. In column 3, we add new reference groups based on a mother's religious denomination and again include a third degree polynomial.[37] Finally, in column 4, we maintain our baseline instrument but include all controls from our robustness check sections—including school-grade trends and school-grade mothers' involvement trends—and follow the PDS method for selection on controls. In this case we have 354 possible controls.[38]

In all cases, we find that the 2SLS estimate is close to our baseline es-

35. We used Stata and the *ivlasso* package of Ahrens et al. (2018) for estimation.

36. The reference group definitions are same school-grade, school-grade-race, school-grade-race-gender, school-grade-race-gender-mother education, school-grade-mother education, and school-grade-gender-mother education.

37. We add two new reference groups. These are same school-grade-race-gender-mother's religious denomination and school-grade-race-mother's religious denomination. As before,

Table 6. Machine Learning and 2SLS

	Select IVs			High-Dim. Controls
	(1)	(2)	(3)	(4)
Mother's Involvement	-0.474**	-0.465**	-0.517**	-0.535*
	(0.224)	(0.195)	(0.260)	(0.281)
Observations	12316	12316	10670	12316
# IVs Penalized	18	18	24	0
# IVs Selected	1	1	1	1
# Controls Penalized	0	22	27	354
# Controls Selected	0	8	7	40
Cluster-Robust IV F	13.460	17.631	13.006	9.419
SGRGR IVs Included	No	No	Yes	No
IVs Selected	SGRGE	SGRGE	SGRGE	NA
Reference Group				
Penalized Controls	No	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. The instrument set is always based on peer mother involvement. We include this at different versions of the peer reference group and include up to a third degree polynomial in peer mother involvement at each reference group level. SGRGE is the same school-grade-race-gender-mother's education reference group that we use in our baseline estimates. The SGRGE selected IV is the leave-one-out SGRGE average mothers' involvement. SGRGR is the same school-grade-race-gender-mother's religious denomination reference group used in section 4.2.2. In column 4, we include all base controls plus all controls from our robustness checks. The number of controls does not include the school fixed effects. These are always included as they are crucial for the identification assumption.

timate. In columns 1-3, the lasso always selects just one instrument, our original baseline instrument. This is consistent with our expectation that mothers with similar children and who share similar education levels will be the most likely to influence each other, and thus, the most relevant choice

use of religious denomination lowers our selected sample size.

38. The rows for number of instruments (controls) penalized inform how many of these were available to be selected from the lasso. The number selected informs how many were selected. Where the number penalized reads zero, then the variables here were not put through a lasso step.

for our peer reference group. In column 2 only 8 controls are selected. Not surprisingly, the first-stage relevance increases, as the value of the F-statistic increases. In column 3, the sample size falls because we include the religious denomination based reference groups, but our effect estimate remains similar. Finally, in column 4, we find that out of the 354 possible controls, 40 are selected by the PDS method. Again, the second-stage estimate is close to our baseline estimate and remains significant. Overall, the results in Table 6 show that our baseline estimate is not sensitive to different choices of instruments or control variables.

4.3 Heterogeneity

We explore heterogeneity across three dimensions. First, there is evidence in the literature that parental influence on skill development declines as a child ages (Doepke et al. 2019; Heckman and Mosso 2014). Our sample includes 7th and 8th graders, so we aim to test whether a mother's response to peer mothers' involvement and the effect of mother's involvement are driven by the youngest adolescents in our sample. Second, we investigate whether a mother's response to peer mothers' involvement and the efficacy of mother's involvement varies by education level. Third, we test for heterogeneity by gender since in the Add Health data, males generally experience more trouble in school.

Our ability to explore heterogeneity is limited. First, our sample size precludes many refined cuts of the data. Second, the instrument may not be strong enough to disentangle multiple layers of heterogeneity. Third, some of the heterogeneity questions may be substantive. How parents choose to invest and their subsequent influence along differing dimensions of socio-economic status, neighborhoods, and other characteristics may depend on a number of factors that are beyond the scope of this study and that deserve careful theoretical and empirical attention.[39] Thus, our analysis here is exploratory

39. See Doepke et al. (2019) for a theoretical model dealing with some of these issues

in nature and provides a direction for further work.

We report our analyses in the supplementary appendix, Section B.6. We find no evidence for heterogeneity by grade level. This applies to both the mother's response to peer mothers' involvement and to the efficacy of mother's involvement.

On maternal education level, we do find some evidence in the first stage that less educated mothers have the strongest response to peer maternal involvement. As noted in Section 3, this result is consistent with evidence that parents, especially less educated parents, put more weight on parenting advice from their social relationships, communities, and families (Kalil 2015).

Next, we find that the effect of mother's involvement on school trouble appears to be driven by mothers who did not complete college. Less educated parents have lower overall involvement, as we show in the Supplementary Appendix Table B.3, thus small increases in their involvement level may matter a lot. This is sensible if there are diminishing returns to involvement. Given that more highly educated parents tend to invest more in their children, interventions attempting to boost maternal involvement will likely be focused on those with lower levels of education (Heckman and Mosso 2014). Our evidence implies that such targeted interventions can indeed be beneficial.

Finally, school trouble exhibits substantial variation by gender. In the supplementary appendix, Figure B.3, we plot the estimated density of school trouble by gender. The distribution of school trouble for males is substantially shifted to the right, compared to females. This can be partly explained by the fact that male noncognitive development at early ages lags behind that of girls (Bertrand and Pan 2013). In our results, however, we do not find evidence that the effect of mother's involvement varies substantially by gender (Supplementary Appendix Table B.10).

along with a review of the literature.

5 Mechanisms

In this section, we discuss three potential mechanisms that we can explore empirically in the Add Health data. The first mechanism is the transfer of educational values and expectations from parents to children. Fan and Chen (2001), Hill and Tyson (2009), Jeynes (2007) and Castro et al. (2015) show that parental expectations and aspirations for their children's academic achievement are significant predictors of academic outcomes. If maternal involvement coincides with communicating and transferring values, expectations, and aspirations to adolescents, then this may be one channel through which maternal involvement reduces school trouble.

The second mechanism is adolescent mental health. Wang and Sheikh-Khalil (2014) present evidence that parental involvement reduces adolescent symptoms of depression. This may occur because involvement provides parents an opportunity to give emotional support to their children. Involvement may also foster a feeling of connectedness between parents and children that improves emotional and mental well-being. In turn, this can facilitate the transfer of values and aspirations between parents and adolescents and increase academic engagement in school (Wang and Sheikh-Khalil 2014).

The third mechanism we consider is parenting style. Parenting style reflects the relation between parents and children and is a strong predictor of academic achievement (Jeynes 2007). Steinberg et al. (1992) identifies three salient dimensions of style: parental warmth and responsiveness, behavioral supervision and strictness, and granting psychological autonomy. The empirical results of Dornbusch et al. (1987), Steinberg et al. (1992), Deslandes et al. (1997) and Marchant et al. (2001) show that an "authoritative" parenting style, characterized by high levels of emotional responsiveness and parental supervision but without being overly strict, is associated with higher academic achievement. Experimental studies of the family check-up (FCU) intervention have also pointed to parenting style as a mechanism that can explain its success. Dishion et al. (2003), Stormshak et al. (2010), and

Fosco et al. (2013) find evidence that parental style and monitoring are associated with greater adolescent self-regulation, which in turn leads to a range of better outcomes.

An authoritative style may also take greater effort to implement by the parent. For instance, it may require more involvement to habituate the child towards a more forward looking perspective (Doepke et al. 2019). Greater involvement, in turn, may alter an adolescent's perception of parenting, boosting their aspirations or providing protective emotional support. These predictions suggest we should find a link between involvement and some measure of parenting style.

We constructed several measures from the Add Health survey to explore these mechanisms. Details about the construction of each measure can be found in Appendix B.7. One measure represents college aspirations, three measures represent mental health (depression, self-esteem and suicidal ideation) and three measures reflect parenting style (warmth and responsiveness, behavioral supervision and strictness, and autonomy). For depression, we use the 19 item scale from the Center for Epidemiological Studies Depression (CES-D) scale (CES-D, Radloff 1977). This scale is available in Add Health and is a widely accepted screening tool for depressive symptoms used in psychiatric epidemiology.[40]

Table 7 reports the estimated impact of mother's involvement on each of our mechanism variables, where involvement is instrumented as before and we include our baseline set of controls and school fixed effects. An increase in mother's involvement leads to a statistically significant increase in the level of college aspirations and a decrease on the depression scale, while self-esteem and suicidal ideation do not appear to be affected. Turning to the parental style measures, mother's involvement is significantly related to the perceived warmth of the parents (column 5) but not to perceived parental control and

40. Our construction of the CES-D scale and the self-esteem scale is the same as in Fruehwirth et al. (2019).

Table 7. Mechanisms: Effect of Maternal Involvement

	(1) College Aspirations	(2) CES-D	(3) Self-Esteem	(4) Suicidal Ideation	(5) Warmth	(6) Control	(7) Autonomy
Mother's Involvement	0.560**	-0.462**	0.041	-0.084	0.476**	0.111	0.128
	(0.239)	(0.227)	(0.204)	(0.069)	(0.204)	(0.186)	(0.166)
N	12296	12311	12298	12276	12273	12265	12307
K-P F	13.241	13.560	13.750	13.397	13.546	13.246	13.430

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. Column headers indicate the dependent variable for the specification. In each specification, we drop additional observations that are missing in the column header. Sample sizes remain very similar to our baseline. All specifications include our baseline set of controls and school fixed effects.

autonomy (columns 6-7). The link with warmth is nevertheless consistent with the notion that involvement and parenting style are intertwined.

Next, we conduct an exploratory mediation analysis in Table 8. There are a number of intuitive reasons to suspect a link between mother's involvement, the mechanism variables, and school trouble. For instance, in a model where students hold beliefs over uncertain capabilities and future opportunities, maternal involvement may act to improve these beliefs, translating into improved aspirations and mental health. Involvement may also alter the adolescent's perception of the relationship with their mother, as proxied by our style measures such as warmth. Indeed, Doepke et al. (2019) think of involvement as effort to shift children toward a more forward looking perspective, while beliefs about opportunity represents an important mechanism for shifts in depressive symptoms (Quidt and Haushofer 2017). In turn, greater patience, more positive emotional symptoms, and improved parent-child relationships may translate into less trouble in school.

Our mediation analysis is not necessarily causal and should primarily be viewed as descriptive. It does, however, reveal the associations between school trouble and the mechanism variables, and we can combine this with the causal evidence from Table 7. To do this, we follow a decomposition approach from Gelbach (2016). We assess how much of our treatment effect from maternal involvement on school trouble runs through each of our mechanism variables, using the following specifications:

$$m_{isk} = \alpha_{1k}^{m} I_{is} + X_{is}' \alpha_2^m + \alpha_s^m + \epsilon_{is}^m \tag{5.1}$$

$$y_{is} = \alpha_1^y I_{is} + X_{is}' \alpha_2^y + \alpha_s^y + \epsilon_{is}^y \tag{5.2}$$

$$y_{is} = \alpha_1^{aux} I_{is} + \sum_{k=1}^{K} m_{isk} \gamma_k^{aux} + X_{is}' \alpha_2^{aux} + \alpha_s^{aux} + \epsilon_{is}^{aux}. \tag{5.3}$$

The first equation is estimated for each mediator variable $m_{isk}(k = 1, \ldots, K)$ with 2SLS and reported in Table 7. The second equation is our baseline model with school trouble as the outcome. The third and final equation is an auxiliary 2SLS regression where we instrument for maternal involvement (I_{is}) while controlling for the all mediators (m_{isk}). We then calculate the indirect effect of maternal involvement on school trouble through mediator k as $IE = \alpha_{1k}^m \times \gamma_k^{aux}$, and the fraction of the total effect that is mediated through m_{isk} as IE/α_1^y. For this mediation analysis to be causal, the mediators would have to be uncorrelated with the error term in the auxiliary regression. Since this is unlikely, we reiterate that this is mostly a descriptive exercise.

First, in column 1, we present the association between each of our mechanism variables and school trouble. Each coefficient estimate is from a regression of school trouble on the row variable plus our baseline controls and school fixed effects omitting maternal involvement.[41] While these are not causal estimates, they have the expected sign: college aspirations, self-esteem, parental warmth, and autonomy are all related to less school trouble, while depressive symptoms and suicidal ideation are related to more school trouble.

Next, in column 2, we present associations for the mediators with school

41. Here we are including the mediators one at a time to look at their simple association with school trouble conditional on the baseline controls.

trouble from a single regression corresponding to the our auxiliary specification. We then use these estimates along with the estimates in Table 7 to calculate the indirect effect reported in column 3. Finally, column 4 contains the share, dividing the indirect effect by our baseline "total effect" estimate for maternal involvement in Table 1.

The mediation estimates are consistent with our intuition and the results from Table 7. College aspirations and depressive symptoms (CES-D scores) each account for about 24% of the effect of mother's involvement on school trouble, while perceptions parental warmth accounts for a smaller 6% share.

Table 8. Descriptive Mediation

	D.V. = School Trouble			
	OLS	Auxiliary	IE	Share
College Aspirations	-0.30***	-0.20***	-0.11	24.16%
	(0.01)	(0.02)		
CES-D	0.35***	0.25	-0.11	24.19%
	(0.01)	(0.01)		
Self-Esteem	-0.23***	-0.02***	-0.00	0.20%
	(0.01)	(0.01)		
Suicidal Ideation	0.54***	0.16	-0.01	2.79%
	(0.03)	(0.03)		
Warmth	-0.24***	-0.06***	-0.03	6.33%
	(0.01)	(0.03)		
Control	-0.01	-0.03***	-0.00	0.65%
	(0.01)	(0.01)		
Autonomy	-0.10***	0.01**	0.00	-0.42%
	(0.01)	(0.02)		
Unexplained Share				42.09%

Notes: Column 1 contains coefficient estimates from regressing school trouble on each of the row variables plus our baseline set of controls and school fixed effects. Column 2 contains estimates from the auxiliary regression for the association of each mediator with school trouble when all included in one regression along with instrumenting maternal involvement and including the baseline controls as our auxiliary specification outlines. Column 3 reports the indirect effect estimates and column 4 reports these as shares of the estimated total effect from maternal involvement to school trouble. Standard errors are in parentheses and are clustered at the school level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In summary, the estimates presented here suggest that the beneficial effect of maternal involvement may operate through shifting beliefs and improving well-being. This, in turn, may protect the adolescent from experiencing trouble in school. Our evidence points to these as useful avenues for future work and further strengthens the case that maternal involvement during adolescence is linked to important features of adolescent development and well-being.

6 Conclusion

Over the past few decades parental involvement has been promoted by policy makers and educators as an important factor that can help drive student success. The No Child Left Behind Act of 2002 and the Every Student Succeeds Act of 2015 both required states to formulate strategies to promote parental involvement at home and in the school. Part of this policy focus has been driven by a large body of research, emanating from education and developmental psychology, that has pointed to a positive association between parental involvement and student outcomes.

Very few studies have been able to estimate the causal effect of parental involvement on academic achievement and noncognitive outcomes using observational data. Recent evidence has emerged about the causal link between parental investments and skill formation during early childhood but much less is known about the period of adolescence. The main contribution of this paper is to provide new evidence in this area. Specifically, we estimate the causal effect of maternal involvement on adolescent trouble in school.

We construct a measure of adolescent school trouble and link it with noncognitive skills. We identify the causal effect of maternal involvement on adolescent school trouble by using the average of mothers' involvement in an appropriately chosen peer group as an instrument. The peer group of mothers is not self-selected but rather defined as the group of mothers who have a number of exogenous characteristics in common (the child's race, gender, school and grade, and the mother's education level). We then leverage within-school, across-cohort variation as an approach to eliminate selection factors and satisfy instrument exogeneity. Our baseline estimates point to a statistically significant and substantial effect of mother's involvement: an increase of 1 standard deviation leads to a reduction in school trouble of about 0.5 standard deviations. The richness of the Add Health data allows us to conduct a wide range of robustness checks around the exclusion restriction. We find our result to be remarkably stable, lending further credibility to our baseline results.

Finally, we explore a number of mechanisms that may explain the causal effect of maternal involvement on school trouble. These include the impact of involvement on the adolescent's college aspirations, mental health and perceptions of parenting style. We find that an increase in involvement is associated with higher college aspirations, lower levels of depression, and a higher perceived level of warmth in the relationship with parents. What mothers do may shift how adolescents feels about themselves and their family, and this mechanism can operate as a protective device that prevents subsequent poor choices by the adolescent at school. A more thorough study of processes within the family remains a promising topic for future study.

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A Appendix

A.1 School-Trouble Scale Measures and Factor Loadings

Table A.1. Summary Statistics for Measures of School Trouble

	Mean	SD	Min	Max
GPA	2.761	0.766	1.000	4.000
School Skips	1.620	4.219	0.000	30.000
Trouble with Teachers	0.856	0.959	0.000	4.000
Trouble with Students	0.857	0.978	0.000	4.000
Trouble Getting Homework Done	1.187	1.074	0.000	4.000
Frequency of Fighting	0.455	0.716	0.000	2.000
Been Suspended from School	0.132	0.339	0.000	1.000
Observations	19617			

Table A.2. Factor Loadings for School Trouble Scale

	(1) Baseline	(2) Female	(3) Male	(4) Grade 7	(5) Grade 8	(6) Grade 9	(7) Grade 10	(8) Grade 11	(9) Grade 12	(10) Omit GPA
Negative GPA	0.746	0.772	0.729	0.718	0.737	0.796	0.872	0.764	0.659	
	(0.023)	(0.037)	(0.033)	(0.054)	(0.055)	(0.051)	(0.066)	(0.062)	(0.066)	
Days Skipping School	0.472	0.458	0.525	0.128	0.277	0.468	0.801	0.732	0.823	0.461
	(0.018)	(0.027)	(0.028)	(0.015)	(0.026)	(0.035)	(0.064)	(0.064)	(0.085)	(0.020)
Trouble with Teachers	0.800	0.826	0.832	0.843	0.914	0.774	0.769	0.737	0.800	0.984
	(0.026)	(0.041)	(0.038)	(0.065)	(0.069)	(0.053)	(0.060)	(0.062)	(0.080)	(0.033)
Trouble with Students	0.630	0.739	0.623	0.637	0.634	0.559	0.667	0.622	0.698	0.823
	(0.022)	(0.038)	(0.032)	(0.056)	(0.055)	(0.043)	(0.055)	(0.057)	(0.074)	(0.030)
Home Work Done	0.767	0.758	0.821	0.698	0.839	0.744	0.815	0.753	0.875	0.818
	(0.026)	(0.039)	(0.039)	(0.058)	(0.067)	(0.052)	(0.066)	(0.066)	(0.089)	(0.030)
Fighting	0.576	0.535	0.568	0.508	0.575	0.557	0.620	0.646	0.575	0.639
	(0.019)	(0.027)	(0.029)	(0.044)	(0.047)	(0.040)	(0.051)	(0.054)	(0.059)	(0.023)
Suspension	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Observations	19617	9952	9665	2667	2665	3480	3820	3686	3204	19617

Notes: Standard errors are in parentheses. Each coefficient represents the factor loading for the measurement equation given by the row variable. Column 1 is our primary scale for analysis. The following factor analyses (columns 2-9) are on sub-samples given by the column except in the last column (10) which is on the full sample but drops GPA from the scale.

A.2 Maternal Involvement and Descriptive Statistics

Table A.3. Summary Statistics for Maternal Involvement Items

	Mean	SD	Min	Max
gone shopping	0.728	0.445	0.000	1.000
played a sport	0.086	0.280	0.000	1.000
gone to a religious service	0.382	0.486	0.000	1.000
talked about dating or party you went to	0.470	0.499	0.000	1.000
gone to a movie, play, etc.	0.257	0.437	0.000	1.000
spoke about a personal problem you are having	0.390	0.488	0.000	1.000
had a serious argument about your behavior	0.333	0.471	0.000	1.000
talked about school work or grades	0.640	0.480	0.000	1.000
worked on a project for school	0.135	0.341	0.000	1.000
talked about other things you are doing in school	0.548	0.498	0.000	1.000
Observations	18511			

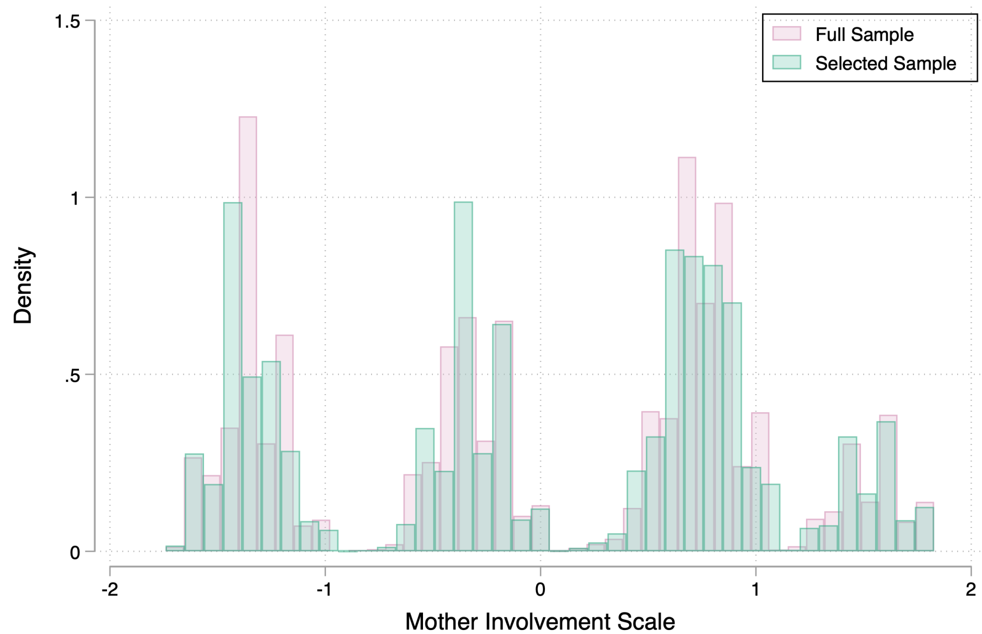
Notes: Each item is a binary yes/no and preceded by the following: "which of these things listed on this card have you done with your mother/adoptive mother/stepmother/foster mother/etc. in the past 4 weeks?"

Table A.4. PCA Rotated Loadings for Involvement Items

	Schooling Related	Activities Related	Communication
gone shopping	-0.000	**0.464**	0.087
played a sport	-0.066	**0.548**	-0.014
gone to a religious service	0.106	0.278	-0.144
talked about dating or party you went to	0.038	-0.007	**0.600**
gone to a movie, play, etc.	-0.024	**0.592**	0.017
spoke about a personal problem you are having	0.085	0.058	**0.569**
had a serious argument about your behavior	-0.113	-0.016	**0.526**
talked about school work or grades	**0.601**	-0.076	0.056
worked on a project for school	**0.463**	0.209	-0.087
talked about other things you are doing in school	**0.623**	-0.059	0.016
Eigenvalue	2.940	1.428	1.232

Notes: Three components returned an eigenvalue above 1 (prior to rotation). We use standard orthogonal varimax rotation returning component loadings such that the components are orthogonal to each other. The PCA is conducted using the polychoric correlation matrix for involvement items because of their binary nature. We assign interpretations to the scales (column headers) based on measures with loadings above 0.4 (in bold). We form indexes of maternal involvement by extracting the predicted components based on the rotated loadings of each component.

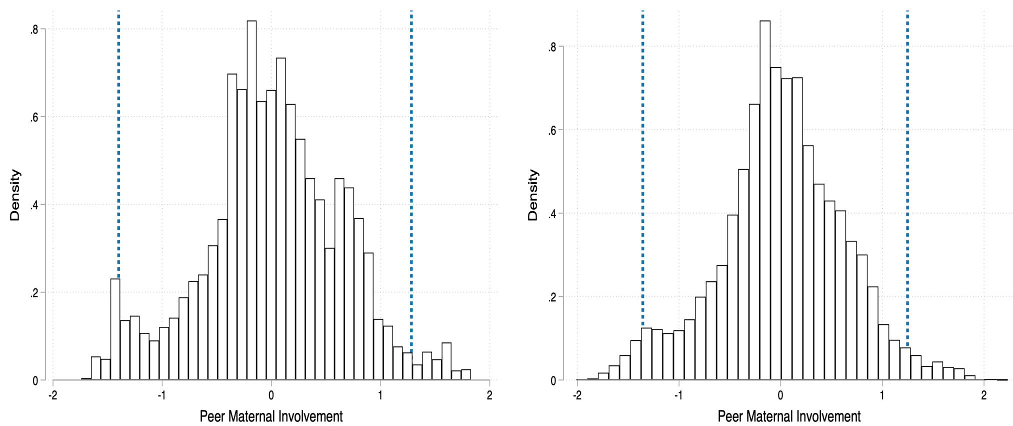
Figure A.1. Histogram of Mother's Schooling-Related Involvement Scale



Notes: This histogram is based on the school-related scale extracted from the first component of the PCA. The mother involvement scale has been standardized to a mean of zero and a standard deviation of one.

Figure A.2. Distribution of the Instrument: Peer Maternal Involvement

(a) Raw Variation **(b)** Variation Removing School Fixed Effects



Notes: This figure presents a histogram of the standardized leave-one-out mean for the same school-grade-race-gender-mother's education peer maternal involvement. Panel (a) reports the raw variation in the sample, and panel (b) reports this variation after removal of school fixed effects with the sample mean added back to place it on the same scale as panel (a). Vertical lines denote the 2.5 and 97.5 percentiles.

Table A.5. Summary Statistics for Primary Covariates

	Full Sample	Selected Sample	*p-value*
School-Trouble	0.00	-0.02	0.00
Mother Involvement	0.00	0.01	0.02
Peer Mother Involvement	0.01	0.01	0.36
Mother's Characteristics			
No HS Diploma	0.17	0.16	0.00
HS Diploma	0.29	0.32	0.00
Some College	0.30	0.32	0.00
College Graduate	0.14	0.13	0.00
Post-College Training	0.09	0.07	0.70
Mother's Age	41.93	41.76	0.00
Household Characteristics			
Household Income	46.42	46.70	0.24
Number of Siblings in H.H.	1.46	1.47	0.00
Single Parent Home	0.32	0.29	0.00
Individual Characteristics			
Female	0.51	0.51	0.00
Hispanic	0.17	0.15	0.00
Black	0.22	0.21	0.39
Other	0.09	0.05	0.00
White	0.53	0.60	0.00
Grade-Level 7	0.13	0.14	0.00
Grade-Level 8	0.14	0.14	0.00
Grade-Level 9	0.18	0.18	0.00
Grade-Level 10	0.20	0.21	0.00
Grade-Level 11	0.19	0.19	0.00
Grade-Level 12	0.16	0.14	0.00
Summer Interview	0.33	0.34	0.06

Notes: This Table reports summary statistics for the Add Health In-home wave I survey on the key variables and controls used for the primary analysis. The original wave I in-home sample has 20,745 observations. In creating our dependent variable, we dropped those not in school (395), those aged greater than 19 (85), missing in the school trouble scale measures (412), and outliers in our measure of skipped school days (236). Column 1 as full sample references the sample post-construction of the dependent variable. Thus, there are no missing observations in the school-trouble scale. The selected sample in column 2 drops missing observations in mother's involvement (1,106), school-grade-race-gender-mother's education peer mothers' involvement (5,811), parental survey respondent listed as male (324), and parental survey respondent listed as not the biological mother when the biological mother lives in the home (60).

B Supplementary Appendix

B.1 School Trouble and Links to Education and Labor Market Outcomes

We test whether our school trouble scale links to later life outcomes. Primarily, we are interested in establishing that the patterns in our scale and in the picture vocabulary test scores match the patterns found in the literature for noncognitive and cognitive skills. Additionally, we are interested in testing for evidence that our scale has long-term implications. Table B.1 provides summary statistics for variables used this analysis. It also provides a list of the controls we incorporate in addition to school fixed effects.

Table B.1. Summary Statistics for Variables in Logged Income Analysis

	Mean	SD	Min	Max
Logged Income	10.184	1.027	0.693	13.816
School-Trouble	-0.025	0.987	-1.633	5.101
AH PVT	0.082	0.947	-5.766	2.040
HS Drop Out	0.058	0.233	0.000	1.000
GED or Certificate Holder	0.036	0.185	0.000	1.000
HS Diploma	0.233	0.423	0.000	1.000
Some College	0.344	0.475	0.000	1.000
College Graduate	0.249	0.432	0.000	1.000
Master's Degree or Better	0.080	0.272	0.000	1.000
Age at Wave IV	28.439	1.753	24.000	34.000
Labor Market Experience	8.074	3.572	0.000	17.000
Any Health Limitations	0.089	0.285	0.000	1.000
Census Tract Unemployment Rate	0.079	0.050	0.000	0.615
Urban Living	0.820	0.385	0.000	1.000
Female	0.535	0.499	0.000	1.000
Hispanic	0.152	0.359	0.000	1.000
Black	0.217	0.412	0.000	1.000
Other	0.076	0.265	0.000	1.000
North East Region	0.119	0.324	0.000	1.000
South Region	0.415	0.493	0.000	1.000
West Region	0.237	0.425	0.000	1.000
Midwest Region	0.229	0.420	0.000	1.000
Ever Married	0.500	0.500	0.000	1.000
Number of Children	0.923	1.138	0.000	7.000
Observations	13746			

Figure B.1. Density Plots by Education Level and Gender of School-Trouble and Test Scores

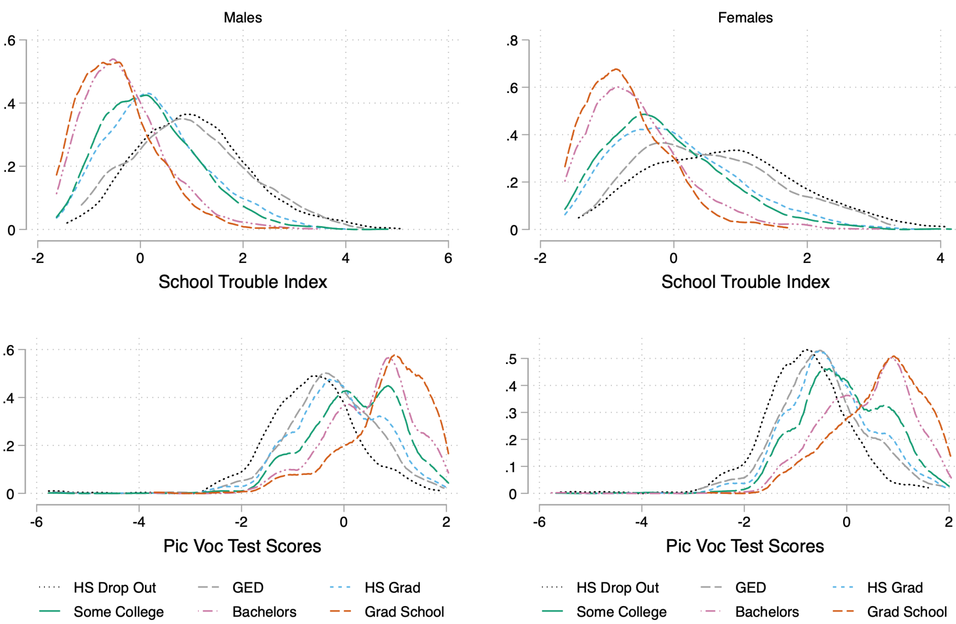


Figure B.1 displays kernel density plots for school trouble (top panels) and PVT scores (bottom panels), stratified by sex and completed education level. For both males and females, the distributions of school trouble among those who dropped out of high school or received the GED are almost identical. Both groups tend to have higher school-trouble scores than individuals with a high school diploma or higher levels of education. For both males and females, obtaining a bachelor's or graduate degree is associated with the lowest school trouble scores. These results are highly consistent with the distribution of noncognitive skills by education level reported in Heckman et al. (2006) and Heckman et al. (2014).

The bottom panel of Figure B.1 shows that these patterns are reversed for the picture vocabulary test (PVT) scores. The PVT score distributions are similar for GED holders and high school graduates, and both groups tend to have slightly higher scores than high school dropouts. Individuals with a bachelor's or graduate degree tend to have the highest PVT scores. Heckman et al. (2006), estimating the distribution of a cognitive skill factor with different data, find similar patterns.

In Table B.2, we report estimates from a regression of log wages in wave IV on the school trouble measure, PVT scores and a set of controls. All specifications are estimated using wave IV survey weights stratified by region. The specifications in columns 1-5 differ in the sets of covariates included (e.g., with or without school fixed effects). Columns 6 and 7 contain estimates based on a Heckman selection model for log wages. Across specifications the relation between school trouble and wages is consistently negative and highly significant. The estimates omitting the level of education – columns 1 through 3 – indicate that a standard deviation increase in school trouble is associated with a wage reduction of 14 to 15 percentage points. Including indicators for completed education level at wave IV (in columns 4 and 5), the negative impact is around 8 percentage points. Finally, the estimate from the selection model in column 6 is slightly smaller in magnitude, but still highly significant.

Heckman et al. (2006) estimate the effect of noncognitive and cognitive skills on wages. Our estimates for school trouble and the picture vocabulary test score are similar in magnitude, suggesting that these two variables are reasonable proxies for noncognitive and cognitive skills.[42]

42. The cognitive factor in Heckman et al. (2006) does appear to account for more wage variation than the test score here, which is to be expected because we only use a single test score.

Table B.2. School-Trouble and Wave IV Income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
School-Trouble	-0.137***	-0.126***	-0.123***	-0.059***	-0.060***	-0.041***	-0.043***
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)	(0.013)	(0.013)
AH PVT		0.116***	0.098***	0.041***	0.041***	0.022	0.018
		(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
GED or Certificate Holder				0.005	0.007	-0.028	-0.028
				(0.112)	(0.114)	(0.115)	(0.119)
HS Diploma				0.286***	0.286***	0.155**	0.156**
				(0.067)	(0.068)	(0.064)	(0.067)
Some College				0.399***	0.394***	0.213***	0.207***
				(0.076)	(0.077)	(0.075)	(0.077)
College Graduate				0.771***	0.747***	0.520***	0.493***
				(0.076)	(0.076)	(0.076)	(0.076)
Master's Degree or Better				0.931***	0.897***	0.668***	0.622***
				(0.089)	(0.087)	(0.089)	(0.087)
N	11775	11775	11775	11775	11775	13250	13250
R^2	0.116	0.124	0.161	0.165	0.195		
School FE	No	No	Yes	No	Yes	No	Yes

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors are in parentheses. Survey weight gswgt4_2 and strata region are used from the Add Health wave IV weight file. All specifications include controls for gender, ethnicity, age at wave IV, labor market experience, health limitations, the unemployment rate at the tract level from the 2000 census, an indicator for living in an urban area, and indicators for residence in northeast, south, or west of the US. Columns 6 and 7 contain estimates from a Heckman selection model with ever married and number of children excluded from the main equation. 5,491 observations are lost from sample attrition. We condition the sample on those with non-missing observations in all covariates. These are 2 from years of education, 681 from AH PVT, 222 from missing a school indicator, 37 from Hispanic, 18 from black, 15 from other, 26 from labor market experience, 1 from limitations, 6 from unemployment rate, 13 from ever married, and 1 from number of children. Also, we drop 89 observations whose school indicators contained at least less than 15 observations because these proved problematic for the estimation of the selection models with survey weights.

B.2 Additional Baseline Results

Table B.3. Baseline Gaps: Characteristics

	Maternal Involvement		School Trouble	
	(1)	(2)	(3)	(4)
Reference Category EDU: No High School	Ref.	Ref.	Ref.	Ref.
HS Diploma	0.122***	0.105***	-0.134***	-0.137***
	(0.030)	(0.029)	(0.044)	(0.048)
Some College	0.214***	0.194***	-0.171***	-0.184***
	(0.031)	(0.033)	(0.041)	(0.039)
College Graduate	0.296***	0.274***	-0.358***	-0.343***
	(0.037)	(0.039)	(0.054)	(0.054)
Post-College Training	0.365***	0.352***	-0.462***	-0.428***
	(0.040)	(0.045)	(0.054)	(0.056)
Mother's Age	0.001	0.000	-0.003**	-0.003**
	(0.002)	(0.002)	(0.002)	(0.002)
Number of Siblings in H.H.	-0.004	-0.004	-0.014	-0.021**
	(0.009)	(0.009)	(0.009)	(0.008)
Household Income	0.001***	0.000*	-0.000*	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)
Single Parent Home	0.078***	0.079***	0.238***	0.220***
	(0.021)	(0.021)	(0.023)	(0.023)
Female	0.114***	0.114***	-0.410***	-0.409***
	(0.016)	(0.017)	(0.019)	(0.017)
Reference Category Race: White	Ref.	Ref.	Ref.	Ref.
Hispanic	0.060*	-0.020	-0.004	0.046
	(0.031)	(0.040)	(0.057)	(0.051)
Black	0.081***	0.048	0.159***	0.128***
	(0.027)	(0.043)	(0.046)	(0.043)
Other	0.016	-0.041	-0.004	-0.084**
	(0.034)	(0.042)	(0.046)	(0.042)
Reference Category Grade: 7th	Ref.	Ref.	Ref.	Ref.
Grade-Level 8	0.012	-0.002	0.030	0.046
	(0.042)	(0.042)	(0.041)	(0.042)
Grade-Level 9	0.015	-0.023	0.119**	0.073
	(0.036)	(0.043)	(0.048)	(0.055)
Grade-Level 10	0.050	0.009	0.024	-0.036
	(0.037)	(0.042)	(0.047)	(0.055)
Grade-Level 11	0.077**	0.038	0.008	-0.047
	(0.036)	(0.041)	(0.051)	(0.056)
Grade-Level 12	0.000	-0.036	-0.093**	-0.149**
	(0.039)	(0.044)	(0.047)	(0.060)
Summer	-0.656***	-0.652***	0.014	0.015
	(0.053)	(0.056)	(0.019)	(0.018)
School FE	No	Yes	No	Yes
N	12316	12316	12316	12316

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and missing indicators are included for variables and observations where we have imputed. We do not include the instrument in the maternal involvement regressions, and we do not include maternal involvement in the school trouble regressions. This table reports the gaps for categorical variables in maternal involvement and school trouble by our baseline control set from OLS regressions. For continuous variables or those we treat this way, i.e., mother's age, number of siblings, and household income, we report the slope coefficients. Note that the reference group for mother's education level is no high school and the reference group for own-race/ethnicity is white.

Table B.4. School Trouble and Mother's Involvement: Full Results

	OLS	First-Stage	2SLS		
	(1)	(2)	(3)	(4)	(5)
Mother's Involvement	-0.109***		-0.474**		
	(0.010)		(0.224)		
Peer Mothers' Involvement		0.069***			
		(0.019)			
Mother's Involvement (Act.)				-0.488**	
				(0.239)	
Mother's Involvement (Comm.)					-0.203
					(0.289)
HS Diploma	-0.126***	0.099***	-0.087*	-0.071	-0.142***
	(0.047)	(0.028)	(0.052)	(0.061)	(0.048)
Some College	-0.163***	0.183***	-0.092	-0.075	-0.179***
	(0.038)	(0.031)	(0.059)	(0.072)	(0.040)
College Graduate	-0.313***	0.256***	-0.213***	-0.179*	-0.342***
	(0.052)	(0.037)	(0.080)	(0.097)	(0.054)
Post-College Training	-0.390***	0.330***	-0.261***	-0.243**	-0.425***
	(0.053)	(0.042)	(0.093)	(0.111)	(0.056)
Mother's Age	-0.003**	0.000	-0.003**	-0.006***	-0.006
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
Number of Siblings in H.H.	-0.021**	-0.004	-0.023**	-0.018**	-0.029**
	(0.008)	(0.009)	(0.009)	(0.009)	(0.014)
Household Income	-0.001**	0.000*	-0.000	-0.000	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Single Parent Home	0.229***	0.080***	0.258***	0.210***	0.262***
	(0.023)	(0.021)	(0.030)	(0.024)	(0.064)
Female	-0.396***	0.106***	-0.355***	-0.311***	-0.320***
	(0.018)	(0.016)	(0.029)	(0.051)	(0.124)
Hispanic	0.044	-0.018	0.037	0.038	0.018
	(0.050)	(0.038)	(0.048)	(0.055)	(0.064)
Black	0.134***	0.041	0.151***	0.103**	0.091
	(0.044)	(0.040)	(0.054)	(0.046)	(0.067)
Other	-0.089**	-0.036	-0.104***	-0.131***	-0.159
	(0.041)	(0.039)	(0.037)	(0.047)	(0.109)
Grade-Level 8	0.046	-0.001	0.045	-0.006	0.077
	(0.041)	(0.039)	(0.040)	(0.044)	(0.064)
Grade-Level 9	0.071	-0.019	0.063	-0.066	0.135
	(0.055)	(0.041)	(0.053)	(0.085)	(0.109)
Grade-Level 10	-0.035	0.011	-0.032	-0.245**	0.044
	(0.054)	(0.039)	(0.054)	(0.116)	(0.137)
Grade-Level 11	-0.043	0.038	-0.029	-0.306**	0.064
	(0.055)	(0.039)	(0.056)	(0.138)	(0.180)
Grade-Level 12	-0.153**	-0.031	-0.166***	-0.428***	-0.027
	(0.059)	(0.041)	(0.056)	(0.146)	(0.197)
summer	-0.056**	-0.650***	-0.294**	0.057**	0.012
	(0.023)	(0.056)	(0.150)	(0.029)	(0.020)
miss_page	0.019	0.213**	0.098	0.016	0.023
	(0.111)	(0.102)	(0.124)	(0.111)	(0.126)
miss_phhinc	-0.017	-0.007	-0.020	0.010	-0.029
	(0.022)	(0.027)	(0.025)	(0.026)	(0.029)
miss_hispanic	-0.112	-0.029	-0.130	-0.213	-0.101
	(0.160)	(0.210)	(0.166)	(0.184)	(0.174)
miss_other	-0.024	-0.218	-0.110	0.147	-0.085
	(0.279)	(0.568)	(0.429)	(0.399)	(0.264)
N	12316	12316	12316	12316	12316
K-P F			13.461	8.174	7.094
AR Weak IV Robust p			0.015	0.019	0.431

Table B.5. School-Trouble (no GPA) and Maternal Involvement

	(1)	(2)	(3)
Mother's Involvement (School)	-0.461**		
	(0.225)		
Mother's Involvement (Act.)		-0.425*	
		(0.234)	
Mother's Involvement (Comm.)			-0.217
			(0.302)
School FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	12316	12316	12316
K-P F	13.461	8.174	7.094
AR Weak IV Robust p	0.020	0.042	0.417

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. All specifications include school fixed effects, our base set of controls, and missing indicators for missing observations in our control set. This table reports 2SLS results corresponding to the results in Table 1 but omitting GPA from the school-trouble scale.

B.3 Balancing Tests for Selection Checks

In Table B.6, we further check against selection effects via balancing tests on our observable controls that are not part of the peer reference group definition. Under an assumption of no selection effects conditional on school fixed effects we expect peer mothers' involvement to be uncorrelated with these controls. To properly conduct the test, it is important that we control for both the school fixed effects and the variables used in defining the reference group. For example, mother's education is likely correlated with these variables and by definition is correlated with our peer reference group.

Table B.6. Selection Robustness Checks: Balancing Tests

	(1) Single Parent Home	(2) Number of Siblings in H.H.	(3) Log H.H. Income	(4) Mother's Age	(5) AH PVT	(6) First Born	(7) Birth Weight	(8) Peers Low Trouble	(9) Peers High Trouble
Peer Mothers' Involvement	-0.016	0.001	0.000	0.001	0.011	-0.001	-0.000	0.001	-0.022
	(0.013)	(0.005)	(0.000)	(0.001)	(0.007)	(0.012)	(0.000)	(0.080)	(0.098)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ref. Group Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	12316	12316	12316	12316	12316	12316	12316	12316	12316
Mean	0.287	1.475	46.702	41.756	0.074	0.492	117.294	0.086	0.067

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. We regress peer mother's involvement on each column header. We impute where this variable is missing and control for a missing indicator. All specifications include school fixed effects and controls that define the peer reference group grade-level indicators, race indicators, gender, and mother's education indicators. These are necessary as otherwise they may induce mechanical correlation. Also, for the share of SGRGE peers with low trouble and the share of SGRGE peers with high trouble we also control for the leave-one-out mean of peer school trouble as omitting it may induce correlation between our IV and these shares that is actually between the IV and the mean. The row for with means reports the means of the variable in the column header.

We run our balancing tests over each of the column headers in Table B.6. We regress the instrument, peer mothers' involvement at our specified reference group, on each of these variables, the reference group controls, and school fixed effects.[43] In each case, we find peer mothers' involvement to be insignificant and near zero consistent with the our instrument being as good

43. To maintain our baseline sample, we impute the column header and control for a missing indicator where needed.

as randomly assigned conditional on school fixed effects.

B.4 Mother's Religious Denomination Category Definitions

We draw these categorizations from Fruehwirth et al. (2019) whose primary reference group for defining their instrument is at the same school-grade-race-gender-denomination level. One key difference is that we use the mother's report of her religious denomination since our focus is on mother involvement. In section 4.2.2, we use mother's religious denomination to redefine our peer reference group at the same school-grade-race-gender-mother's denomination as a robustness check.

Table B.7. Mother's Religious Denomination Category Definitions

	Included Religions	Percent Full Sample
None		6.47%
Catholic	Catholic	30.76%
Liberal Protestant	Episcopal, Friends/Quakers, Methodist, Presbyterian, Unitarian	12.36%
Moderate Protestant	Christian Church (Disciples of Christ), Lutheran, other Protestant	13.91%
Conservative Christian	Adventist, AME, AME Zion, CME, Assemblies of God, Christian Science, Jehovah's Witness, Congregational, Holiness, Latter Day Saints (Mormons), Pentecostal, Baptist	36.50%
Set to missing if	Buddhist, Eastern Orthodox, other religion, Hindu, Islam, Moslem, Muslim, Jewish	3.60%

B.5 Alternative Forms of Mother's Involvement

We aim to examine whether peer mothers' schooling-related involvement affects alternative forms of maternal involvement. If this is the case, the exclusion restriction may be violated. In Table B.8, we report the first-stage estimates from regressing alternative forms of mother's involvement on the peer mothers' average schooling-related involvement. Column 1 repeats the baseline first-stage. Column 2 reports estimates from regressing the activities scale on peer mothers' schooling-related involvement and our baseline controls, and column 3 repeats this using the communication scale. We find no evidence that peer mothers' involvement at our selected SGRGE reference group and based on primary schooling-related scale is related to either of the two additional scales.

Table B.8. First-Stage: Schooling-Related IV and Alternative Scales

	Schooling-Related Scale	Activities Scale	Comm. Scale
Peer Mothers' Involvement	0.069***	0.014	0.007
	(0.019)	(0.014)	(0.016)
N	12316	12316	12316

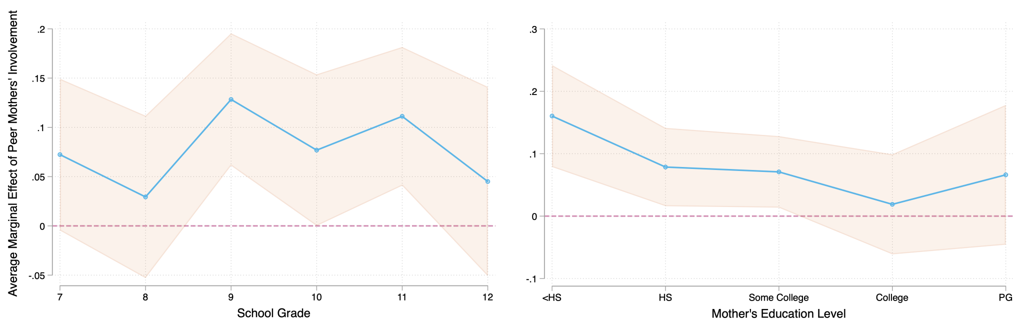
Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors are in parentheses and are clustered at the school level. Peer mother involvement is held at the average of schooling-related scale amongst the same school-grade-race-gender-mother's education reference group. All specifications include school fixed effects, our base set of controls, and missing indicators for missingness in control variables. The schooling-related scale corresponds to the first component of the PCA based on the rotated loadings – our preferred involvement scale we focus on throughout. The activities scale corresponds to that used in column 5 of table 1 and the communication (comm.) scale to that used in column 6 of table 1.

B.6 Heterogeneity Results

In the left panel of Figure B.2, we report the average marginal effect of peer mothers' involvement on a mother's involvement at each grade-level. The confidence intervals are quite wide because the sample sizes by grade-level are relatively small. Nevertheless, we see no clear heterogeneity across grades. In the right panel we report similar results stratified by the mother's education level. The pattern provides no evidence that the baseline first-stage estimate is driven by mothers with greater education levels. If anything, the point estimates suggest that mothers with less education respond more to peer mothers' involvement.

Figure B.2. Heterogeneity: Mother's and Peer Mother's Involvement

(a) By Grade **(b)** By Maternal Education



In Table B.9, we explore heterogeneity in the effect of mother's involvement across grade level and mother's education. In column 1, we interact mother's involvement with a grade-level variable – where grade 7 is normalized to 1 and so on – and instrument this interaction with the interaction between our main instrument and grade level. The interaction effect is not significant. In this specification, however, the instruments are weaker: the K-P F statistic is nearly 8, although we do pass the AR weak instrument robust test that the effects of mother's involvement and its interaction are jointly equal to zero. To probe this question further, we restrict the sample by dropping middle schoolers. In column 2, we find that the effect of mother's involvement is similar to the baseline result. Thus, our results are at least not driven by the 7th and 8th graders in the data.

In columns 3 and 4, we turn to test for heterogeneity by mother's education-level. In column 3, we interact mother's involvement with mother's education and again instrument it with the interaction of our instrument and mother's education. The results here suggest a strong effect of involvement that declines as mother's education increases. In other words, a substantial part of

Table B.9. Heterogeneity by Grade Level and Mother's Education

	(1)	(2)	(3)	(4)
Mother's Involvement	-0.320	-0.417**	-0.679***	-0.501**
	(0.413)	(0.199)	(0.213)	(0.222)
Mother's Involvement × Grade	-0.028			
	(0.097)			
Mother's Involvement × Education			0.208*	
			(0.111)	
N	12316	8866	12316	9810
K-P F	7.994	17.542	2.821	12.631
AR Weak IV Test	0.045	0.027	0.002	0.006

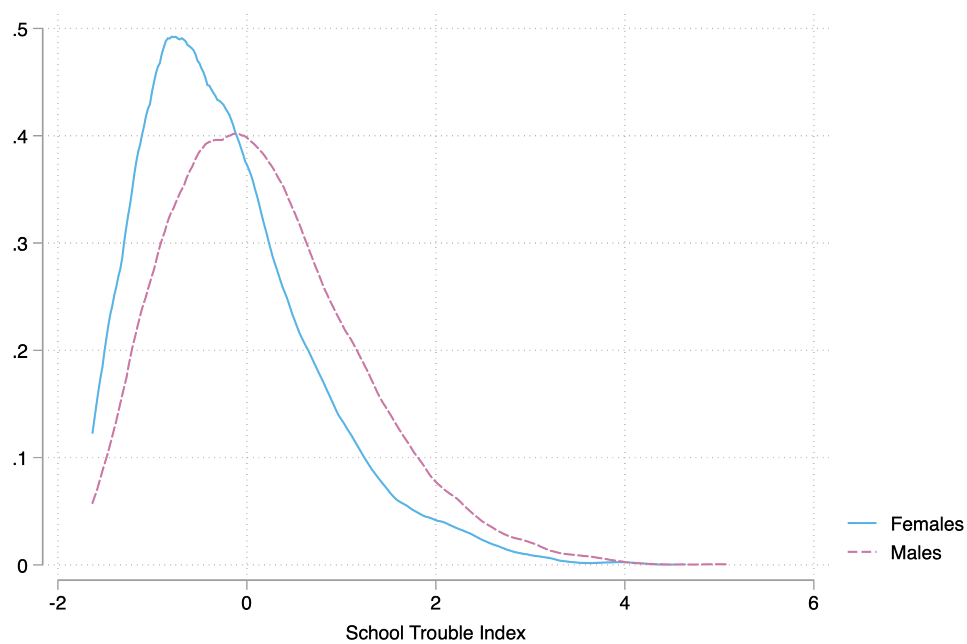
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. All specifications include the full set of controls and school fixed effects. In column 1, the instruments are peer mother involvement and its interaction with grade-level. Grade-level here is shifted such that grades 7-12 are represented by values of 1-6. We instrument both mother involvement and its interaction with grade-level. In column 3, we follow a similar approach for mother's education level. In column 2, we restrict the sample to those in 9th grade or above (in high school). In column 4, we restrict the sample to observations with mother's who have less than a college degree.

our baseline estimate may be driven by mothers with less than a completed college education. Weak instruments, however, may again be a problem and we caution against drawing strong conclusions. Nevertheless, we pass the AR weak IV test.

In column 4, we restrict the sample to mothers with less than a completed college education.Here the K-P F is near 10 and the effect of mother's involvement remains very close to our baseline estimate. Overall, these results suggest that for mothers with less education, schooling-related involvement can indeed be effective. One potential explanation is that adolescents from less educated households are more likely to experience trouble in school and thus represent the individuals likely to receive the most benefit from intervention by the mother.

Finally, we examine heterogeneity across gender. Figure B.3 shows that males in general exhibit much more school trouble. To test for heterogeneity by gender, we interact gender with mother's involvement and instrument the interaction with an interaction between our instrument and gender. One concern is that the interaction instrument may be too correlated with peer mothers' involvement itself to effectively identify the gender-specific effects of involvement on school trouble. Also, because our instrument is not very strong, splitting the sample by gender may reduce the sample size too much. Thus, we explore the interaction of mother's involvement with a female indicator for different constructions of the peer reference group. First, we keep our original reference group definition. Second, we drop gender, defining the reference group by school, grade, race (SGR) and mother's education. Third, we refine the SGR peer group further, by matching on the mother's religious denomination. This further reduces the sample size ($N = 11,299$). And, fourth, we use the SGR and mother's religious denomination reference group and the instrument at our original definition to obtain multiple instruments and overidentification.

Figure B.3. School-Trouble Empirical Density Plots by Gender



In Table B.10, we report the results. In column 1, using our instrument and its interaction with a female indicator, we find no evidence for a differential effect. In column 2 and 3, we redefine the peer reference group and find similar results. The estimated effects of involvement are similar in magnitude but less precise. The interactions are not significant and the K-P F statistics remain small. In column 4, we use the SGR-mother's religious denomination reference group and its interaction with female as instruments, in addition to our baseline instrument (and its interaction with gender). The estimates are again similar to the baseline results.

With multiple instruments, the K-P F increases but only slightly. We also report a range of weak instrument robust tests and find that in general we can reject the null that mother's involvement and its interaction with female are jointly equal to zero. Thus, overall the evidence here consistently points to a lack of heterogeneity by gender in the effect of involvement.

Table B.10. Heterogeneity by Gender

	(1) Original IV	(2) SGR-Mother's EDU IV	(3) SGR-Mother's RD	(4) Multiple IVs
Mother's Involvement	-0.438**	-0.569*	-0.631*	-0.566**
	(0.174)	(0.315)	(0.344)	(0.231)
Mother's Involvement × Female	0.093	0.130	0.155	0.145
	(0.227)	(0.204)	(0.215)	(0.207)
Female	-0.369***	-0.357***	-0.345***	-0.352***
	(0.025)	(0.035)	(0.041)	(0.028)
N	12316	12316	11299	11299
K-P F	2.747	3.281	4.071	6.059
AR Weak IV Test	0.020	0.061	0.128	0.065
CLR Weak IV Test				0.017
Lagrange K Weak IV Test				0.027
Over-ID p-value				0.775

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered at the school level. All specifications include the full set of controls and school fixed effects. Each specification includes two endogenous variables: mother's involvement and its interaction with female. Column headers indicate the instrument employed. In each case, the instrument set is the main IV and its interaction with female.Original IV is the average of mother's involvement at our primary reference group level: the same school-grade-race-gender-mother's education. SGR-Mother's EDU cuts gender from the reference group definition and is the same school-grade-race-mother's education level. SGR-Mother's RD defines the reference group at the same school-grade-race-mother's religious denomination. Some additional observations are lost using this reference group. Multiple IVs employs the SGR-Mother's RD, its interaction with female, and our original reference group definition to obtain overidentification. Weak IV robust tests are tests of that the effect of mother's involvement and its interaction with female are jointly equal to zero. CLR is the conditional likelihood ratio test. Lagrange K is the Lagrange Multiplier test.

B.7 Variable Definitions for Mechanism Section

Table B.11. Variable Definitions for Aspirations and Mental Health

Variable definitions for college attitudes and mental health

College Attitudes Construction: Normalized sum of scales

Scale: (1-5) higher is better.
1. How much do you want to go to college?
2. How likely is it that you will go to college?

CES-D Construction: Normalized sum of scales

How often was each of the following things true during the past week?
Scale: (0-3) Higher is more often. Positive feelings recoded to keep scale consistent
1. You were bothered by things that usually don't bother you.
2. You didn't feel like eating, your appetite was poor.
3. You felt that you could not shake off the blues, even with help from your family and your friends.
4. You felt that you were just as good as other people.
5. You had trouble keeping your mind on what you were doing.
6. You felt depressed.
7. You felt that you were too tired to do things.
8. You felt hopeful about the future.
9. You thought your life had been a failure.
10. You felt fearful.
11. You were happy.
12. You talked less than usual.
13. You felt lonely.
14. People were unfriendly to you.
15. You enjoyed life.
16. You felt sad.
17. You felt that people disliked you.
18. It was hard to get started doing things.
19. You felt life was not worth living.

Self-Esteem Construction: Normalized sum of scales

Four item scale (1-6 each variable). Higher values indicate higher esteem.
1. You have a lot of good qualities.
2. You have a lot to be proud of.
3. You like yourself just the way you are.
4. You feel like you are doing everything just about right.

Suicidal Ideation Binary (Yes, No)

During the past 12 months, did you ever seriously think about committing suicide?

Table B.12. Variable Definitions for Parenting Style Variables

Family Warmth	Construction: Normalized sum of scales
Scale: (1-5) higher is better. 1. How much do you feel that your parents care about you? 2. How much do you feel that you and your family have fun together? 3. How much do you feel that your family pays attention to you?	
Control	Sum of Yes, No questions then normalized
Scale: flipped ordering so that =1 implies more control 1. Do your parents let you make your own decisions about the time you must be home on weekend nights? 2. Do your parents let you make your own decisions about the people you hang around with? 3. Do your parents let you make your own decisions about what you wear? 4. Do your parents let you make your own decisions about how much television you watch? 5. Do your parents let you make your own decisions about which television programs you watch? 6. Do your parents let you make your own decisions about what time you go to bed on week nights? 7. Do your parents let you make your own decisions about what you eat?	
Autonomy Granting	Scale: 1-5 (5 is higher) and standardized
1. Your mother encourages you to be independent	

B.8 Peer Effects and Bias

In this section, we elaborate on the possibility of bias in the IV estimates of Table 1 and Table 3 when a peer effect in school trouble is present. Consider the following model, where, for simplicity, we ignore the presence of other covariates:

$$Y_i = aD_i + bX_i + e_i. \tag{B.1}$$

Here, Y_i is school trouble, D_i is mother's involvement and X_i is peer average school trouble. We assume that Z_i is an instrument for D_i that satisfies the exogeneity condition $E(Z_i e_i) = 0$. In particular, since Z_i is peer mothers' involvement, it is reasonable to expect that Z_i and X_i are correlated, so that $E(Z_i X_i) \neq 0$. We now consider IV estimates of a from two approaches: (1) Regress Y_i on D_i, using instrument Z_i; and (2) Regress Y_i on D_i and X_i, using Z_i as instrument for D_i. These approaches roughly correspond to the estimates in Tables 1 and 3, respectively. If there is a peer effect in school trouble, then $b \neq 0$, and approach (1) will suffer from omitted variable bias. Approach (2) will also result in biased estimates of a and b if X_i is endogenous.

The estimator of a under approach (1) is $\hat{\alpha} = [\sum_{i=1}^{n} Z_i D_i]^{-1} \sum_{i=1}^{n} Z_i Y_i$. Asymptotically, under standard assumptions, it follows that

$$\hat{\alpha} \xrightarrow{p} a + \frac{E(Z_i X_i) b}{E(Z_i D_i)}. \tag{B.2}$$

If X_i does not measure a relevant margin of peer effects, then $b = 0$ and $\hat{\alpha}$ is (asymptotically) unbiased. This is one of our identifying assumptions discussed in Section 3. If, on the other hand, X_i is a relevant margin of a peer effect, then presumably $b > 0$. From the first-stage, $E(Z_i D_i) > 0$. It is reasonable to expect (and this can be checked empirically) that increased involvement among peer mothers leads to less school trouble among the adolescent's peers, so that $E(Z_i X_i) < 0$. It follows from (B.2) that the bias in $\hat{\alpha}$ is negative. We demonstrate this in Figure B.4, where we report

$\hat{\alpha}$ fixing the value of b over a grid and estimating a 2SLS regression at each grid point using our specification from Table 3, column (3). Indeed, a true positive effect from peer mean school trouble would lead to smaller estimates of the effect of maternal involvement. Thus, for our baseline estimates to be consistent, we require that either $E(Z_iX_i) = 0$, which is not true, or that the effect from the peer mean of school trouble is null. Note that our simulated results do suggest that our conclusion about the effect of maternal involvement on school trouble is qualitatively robust even if there is a fairly large and positive true effect from peer mean school trouble. Our evidence from Table 3, where we include different measures of peer school trouble, strongly suggests that the effect of the peer mean is null or small in size.

Now, we consider approach (2). Let $(\tilde{a}, \tilde{b})$ be the estimator of (a, b) under approach (2). The estimator can be written as

$$\begin{pmatrix} \tilde{a} \\ \tilde{b} \end{pmatrix} = \left[\sum_{i=1}^{n} \begin{pmatrix} Z_iD_i & Z_iX_i \\ X_iD_i & X_i^2 \end{pmatrix} \right]^{-1} \sum_{i=1}^{n} \begin{pmatrix} Z_iY_i \\ X_iY_i \end{pmatrix}.$$

If (a^*, b^*) is the probability limit of $(\tilde{a}, \tilde{b})$, then

$$\begin{pmatrix} a^* \\ b^* \end{pmatrix} = \begin{pmatrix} a \\ b \end{pmatrix} + \begin{bmatrix} E(Z_iD_i) & E(Z_iX_i) \\ E(X_iD_i) & E(X_i^2) \end{bmatrix}^{-1} \begin{bmatrix} E(Z_ie_i) \\ E(X_ie_i) \end{bmatrix}. \tag{B.3}$$

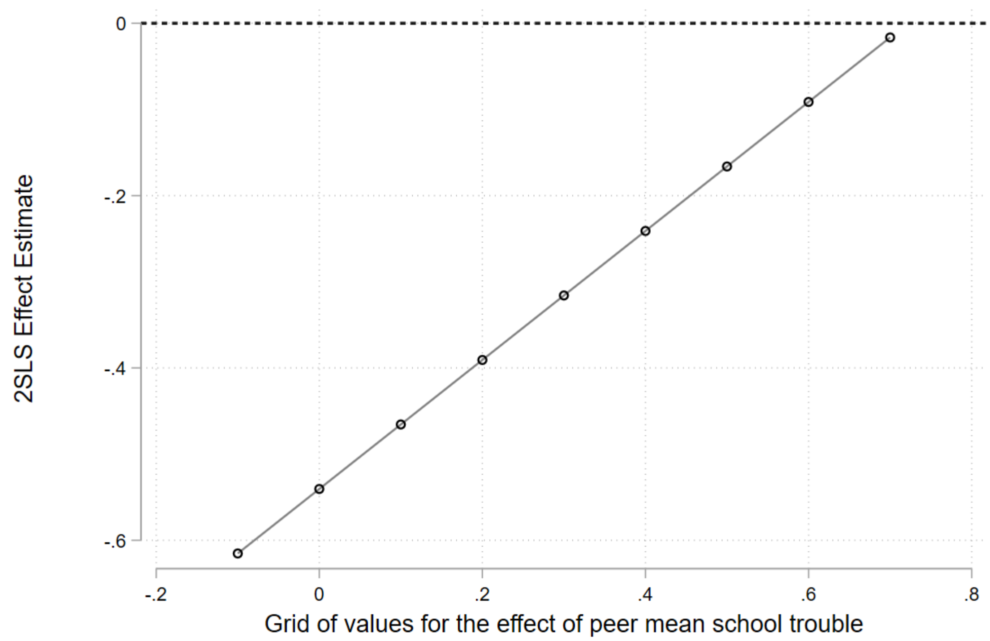
Assuming, as before, that $E(Z_ie_i) = 0$, it now follows that

$$\tilde{a} \xrightarrow{p} a^* = a - \frac{E(Z_iX_i)E(X_ie_i)}{E(Z_iD_i)E(X_i^2) - E(Z_iX_i)E(X_iD_i)}. \tag{B.4}$$

Equation (B.4) shows that $\tilde{a}$ is biased due to the correlation between Z_i and X_i and the endogeneity of X_i, regardless of whether b is zero or not. If $b = 0$, then $\hat{a}$ in (B.2) is unbiased, whereas $\tilde{a}$ in (B.4) remains biased.

Considering the standard errors of $\hat{a}$ and $\tilde{a}$ in Tables 1 and 3, both estimates are essentially the same. If $\hat{a}$ and $\tilde{a}$ are close to their probability limits,

Figure B.4. Simulated 2SLS Results Fixing b



Notes: Plotted y-axis values are 2SLS estimates for the effect of maternal involvement on school trouble fixing the value of b for the effect of peer mean school trouble to a value given by the x-axis.

then the bias terms in equations (B.2) and (B.4) are roughly the same. This implies that b satisfies

$$b \approx -\frac{E(Z_iD_i)E(X_ie_i)}{E(Z_iD_i)E(X_i^2) - E(Z_iX_i)E(X_iD_i)}.$$

In other words, if one is concerned that our baseline estimates are biased, then all peer effect robustness checks corresponding to Table 3 yield more or less the same amount of bias. Our stylized example here shows that this only happens if the peer effect (b) has a very specific magnitude, which seems unlikely. Instead, the estimates of b in Table 3 suggest that the effect of peer average school trouble is very close to zero, mitigating the concern that the baseline estimate of a in Table 1 is biased. Moreover, the robustness of the estimates of a in Table 3 combined with Equation B.4 suggest that the endogeneity of X_i, as measured by $E(X_i e_i)$, is limited in terms of its potential to induce bias.