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Post-Divorce Placement Arrangements and Children's Test Scores

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INTRODUCTION

Both income and family structure have been linked to educational outcomes for children. The link between family economic well-being and school performance across a variety of metrics is well documented, with persistent gaps between children from higher and lower income households (Davis-Kean 2005; Noble et al. 2015; see Duncan, Magnuson, and Votruba-Drzal 2017 for a review). Likewise, children from single-parent households fare more poorly in terms of educational outcomes, a relationship that is partly though not entirely explained by income differences (Alexander, Entwisle, and Olson 2001; Carlson and Corcoran 2001; Ermisch and Francesconi 2001). Finding ways to counter persistent inequities among children from different backgrounds continues to be a high priority for schools, in Wisconsin and nationwide.

Shared placement, a growing arrangement for children following parental divorce, could potentially affect children's post-divorce school outcomes, either positively or negatively, through a variety of mechanisms. This report extends previous research around shared placement completed as part of the Child Support Policy Research Agreement by examining the relationship between shared placement and educational outcomes (specifically, standardized test scores) in Wisconsin, using the unique administrative dataset housed at the Institute for Research on Poverty.

BACKGROUND

With nearly half of marriages ending in divorce, marital dissolution remains common among American families (Hemez 2017; Kennedy and Ruggles 2014; Stevenson and Wolfers 2011). As a result, over one-quarter of children born to married parents are estimated to

experience parental separation by age 12 (Kennedy and Bumpass 2008). Compared to children from intact dual-parent families, those who experience parental divorce score lower in a variety of indicators, ranging from psychosocial well-being to behavioral conduct to academic achievement (Amato 2005; Amato 2010; Bernardi and Radl 2014; Carr and Springer 2010; Cavanagh and Huston 2006; Eriksen, Falgreen, Hvidtfeldt, and Bie Lilleør 2017; Garriga and Pennoni 2017; Gruber 2004; Kreidl, Stipkova, and Hubatková 2017; Strohschein 2005; Sun 2001; Weaver and Schofield 2015).

Marital Dissolution and Educational Outcomes

A substantial body of work suggests that marital dissolution is associated with negative academic outcomes for children during the kindergarten through high school years (Anthony, Diperna and Amato 2014; Aughinbaugh, Pierret, and Rothstein 2005; Carlson and Corcoran 2001; Frisco, Muller, and Frank 2007; Keller 2016; Potter 2010; Sun 2001; Sun and Li 2001; Sun and Li 2002). Most commonly, this body of work uses longitudinal datasets from the United States that track students from before to after their parents' divorce, while Keller (2016) uses Hungarian administrative data.

Whereas lower test scores among children with divorced versus married parents have been well documented, there are many different potential explanations for this disparity, including pre-divorce parental investment in children (Sun 2001; Sun and Li 2001; Sun and Li 2002), family income and mothers' characteristics (Carlson and Corcoran 2001), post-divorce children's psychosocial well-being (Potter 2010), and unobserved characteristics of children (Aughinbaugh, Pierret, and Rothstein 2005) and their parents (Keller 2016). It is unclear whether gender moderates the divorce-score correlation; divorce seems differentially associated with girls' scores more so than with boys' in some studies (Anthony, Diperna and Amato 2014) but

not in others (Sun 2001; Sun and Li 2001; Sun and Li 2002). In recent work that utilized fixed-effects models, in which children effectively serve as their own controls via the inclusion of pre-divorce scores, negative associations between divorce and test scores were found for math but not for reading; and notably, associations were smaller than typically found in cross-sectional research (Anthony, Diperna and Amato 2014).

Shared Placement and Educational Outcomes

The above work focuses on marital dissolution, but does not address the potential moderating impact of placement arrangements—an issue that may be particularly salient given placement trends in recent years. Research in Wisconsin has documented stark changes in children's post-divorce placement arrangements. Over 45 percent of Wisconsin divorces involving children result in shared placement, according to recent data, an increase from 14 percent of such cases in the early 1990s (Cancian, Meyer, Brown, and Cook, 2014). Placement outcomes differ widely by income: among the highest income couples, two thirds (67 percent) have shared placement for their children, as compared to 11 percent among the lowest-income parents (Brown and Cook, 2012).

Thus far, there has been little attention to how placement is associated with children's school-related outcomes—an important question, given the well documented link between divorce and poorer academic performance. A variety of mechanisms for such a relationship are possible, and both beneficial and harmful impacts of shared placement are plausible. Mothers who have shared placement arrangements experience larger average declines in economic well-being following divorce as compared to their counterparts with sole placement, stemming from losses in ex-partners' earnings that are only partially offset in the form of child support, even as their own earnings may increase (Bartfeld and Han 2014). Factoring in both mothers' and

fathers' household economic well-being, children in shared placement arrangements experience somewhat larger declines in well-being after divorce than children in sole placement (Bartfeld, Brown, and Ahn 2009). Given the link between economic well-being and school outcomes, it is possible this relative decline could have ramifications for school outcomes. At the same time, shared placement is heavily concentrated among parents who are better off economically prior to divorce, such that greater declines in economic well-being still result in absolute levels of well-being that exceed those of sole-placement children. Beyond income implications, shared placement may also make it more challenging for children to establish consistent routines supportive of strong school performance, although research has not examined this.

On the positive side, placement could also affect educational outcomes by virtue of stronger relationships with or higher parenting quality from both parents. Indirect evidence from research on sole-placement children with varying degrees of contact with both parents suggests more contact with fathers is associated with better child outcomes (including educational outcomes) when contact is in the context of protective factors such as competent parenting, low conflict, and availability of financial support (for review see Bartfeld 2011). And research from shared placement provides a large and growing array of evidence that shared placement is associated with better child adjustment across a wide range of well-being measures spanning family relationships, physical and mental health, and adolescent behaviors (see, e.g., Nielsen 2018a,b; Bauserman 2002 for reviews). At the same time, a comprehensive recent review of the literature on shared placement found less evidence for associations between placement arrangements and cognitive and school outcomes such as grades and school adjustment than for other kinds of outcomes (Nielsen 2018b). The relationship between placement and standardized test scores has not, thus far, been examined in the literature.

This project examines the extent to which shared placement is associated with children's performance on standardized tests, relative to the more traditional sole-mother placement. We leverage longitudinal data on children encompassing the pre-divorce and post-divorce periods to compare how patterns of pre-divorce and post-divorce scores differ for children in different placement arrangements.

DATA AND METHODS

Data and Sample

The primary data for the analysis is the Wisconsin Court Record Data (WCRD). We combine this with standardized test score data spanning third grade through eighth grade from the Wisconsin Department of Public Instruction (DPI), for the 2005–2006 through 2013–2014 academic years. Test scores are math and reading scores from the Wisconsin Knowledge and Concepts Exam, normalized statewide by grade-year with a mean of zero and a standard deviation of one. The DPI data also provide basic demographic information about students including gender, race and ethnicity, low-income status (denoting eligibility for free or reduced price school meals), English language learner status, and presence of disability. We also draw on wage records from Unemployment Insurance; these records are used to determine quarterly earnings for jobs in Wisconsin, which allow us to control for children's pre-divorce economic well-being.

The WCRD contains data for a sample of divorce cases in 21 Wisconsin counties. The data include weights to adjust for differences in sampling rate across counties and over time. Of particular relevance to this study, the data include information on the divorce petition date, the date of final divorce judgment, and the physical placement arrangements at the time of the final judgment. Our sample consists of children from divorce cases in Cohorts 27-30 of the WCRD,

which includes cases with petition dates between July 2006 and June 2010.[1] We then construct an analysis sample of children who have test scores available both in the year prior to their parents' divorce petition (p-1), as well as in at least one of the first three years after the petition year (p+1, p+2, p+3), limited to post-petition years in which the divorce has been finalized. Thus, reflecting the third through eighth grade testing period, these are children who were in fourth through seventh grade at the time their parents began divorce proceedings, and they each have from one to three post-divorce test observations available during the first three years following the divorce petition.[2] We refer to this as a pre-post sample, and for each child our analysis file includes observations during the pre-petition year as well as each available year after the divorce is finalized, yielding two to four observations per child. We define the outcome period based on the petition date (looking up to three years post-petition) since that denotes the formal start of household disruption, but we exclude observation for years that are at or after the petition but before the final judgment, as we do not have information about the child's living arrangement prior to the final judgment. This sampling scheme yields a sample of 2,575 observations for 827 children. Because scores are normalized statewide at the grade-year level, the underlying distribution of scores is the same for each grade, thus enabling us to pool observations over the third through eighth grade range.

[1]Cohort 27 is the earliest cohort for which we have access to test scores from the pre-petition year.

[2]Starting with children between the ages of 9 and 13 at time of their parents' divorce petition (the age range of children who potentially have both pre-petition and post-divorce outcome data available), the final sample includes 55 percent of those children age 9 at petition, 88-89 percent of those age 10 to 11 at petition, 76 percent of those age 12 at petition, and 22 percent of those age 13 at petition. The 9-year-olds who are excluded are largely those without pre-petition test scores (the pre-petition year is often second grade for these children), while the older children who are excluded largely lack post-divorce test scores as of eighth grade. (Post-divorce data requires the divorce being finalized as of a given test year.) At any age, children would be missing relevant test scores if they were not attending public school in Wisconsin in a given grade.

We classify children according to their placement arrangements at the final divorce judgment, defining shared placement in accordance with the thresholds used in child support guidelines. “Sole-mother placement” denotes children who live with the mother more than 75 percent of the time; “mother-primary-shared placement” denotes children who live with mothers from 51-75 percent of the time and with fathers at least 25 percent of the time; “equal-shared placement” denotes children who live half time with each parent. The “other placements” category includes groups that are not large enough to analyze on their own including sole-father placement (children who live with the father more than 75 percent of the time), father-primary placement (children who live with the father from 51-75 percent of the time), children not living with either parent, as well as other miscellaneous arrangements.

Methods

Our analyses examine within-child changes in normalized test scores from the pre-petition to post-divorce period, comparing children with different post-divorce placement arrangements. We use two approaches: difference-in-difference models with child fixed effects; and lagged dependent variable models. The former assumes that observed and unobserved time invariant child characteristics have constant impacts over time, and compares how children’s scores vary from pre- to post-divorce across placement groups. This approach reflects and addresses the assumption that unobserved time-invariant variables (such as ability) are correlated with both scores and placement. The latter approach assumes that the past score itself, rather than unobserved time-invariant factors that may influence that score, is correlated with placement. This could be the case, for instance, if family stress prior to the petition systematically affected the pre-petition score and also the placement outcome. Angrist and Pischke (2008) demonstrate that if the lagged dependent variable assumptions are correct, then fixed effects estimates tend to

be too high, whereas if fixed effects assumptions are correct then lagged dependent variable assumptions tend to be too low, such that the two estimates may broadly serve to bracket the true effect.

Difference-in-Difference Models with Child Fixed Effects

Our initial analytic approach is a difference-in-difference ordinary least squares regression with child fixed effects. Using pooled outcome data from the pre- and post-petition periods for the pre-post sample described above, we model test scores as a function of child fixed effects, a post-divorce dummy denoting observations after the final judgment, and interactions between post-divorce status and placement type. Because we include child fixed effects, no time invariant child or family background characteristics are included. This model essentially compares within-child changes in scores from the pre-petition to post-divorce period for children in shared as compared to sole-placement post-divorce arrangements. The coefficients on the interaction terms capture the differential change from pre- to post-divorce scores between children with shared versus sole placement. To the extent children in the different placement groups would have displayed similar trends to each other net of placement outcomes (often referred to as the parallel trends assumption), any differences captured by the coefficients on the post-placement interactions can be interpreted as causal.

Our base models do not include time-varying factors that might be affected by divorce or placement arrangements, as we are interested in associations between placement and test scores that encompass any intermediate differences that may arise from placement itself. We are, however, also interested in whether any observed differences are accounted for by current economic hardship or school characteristics. In our full models, we therefore add 2 time-varying measures: current economic disadvantage; and percent of economically disadvantaged students in current school (as well as percent squared to account for nonlinearities). These variables are

defined for each observation year, whether pre-petition or post-divorce. Economic disadvantage is based on the DPI data and denotes children categorized in their school as eligible for free or reduced price school meals by virtue of income below 185 percent of the federal poverty line or categorical eligibility through receipt of FoodShare or in connection with selected other public programs. Note that we do not know, in the instance of equal-shared placement children, which household's circumstances are reflected in the designation. In the models that exclude these factors, any observed associations between placement and test scores would encompass associations stemming from different pre-post trends by placement in the presence of economic disadvantage or in school income levels. All models are estimated with robust standard errors clustered by child.

Lagged Dependent Variable Models

Our second approach involves lagged dependent variable models. Here, we model post-divorce outcomes and control for the score from the year prior to petition. The sample consists of all of the post-divorce observations in our pre-post sample—one to three per child. Thus, these are lagged dependent variable models, in which the lag is not from the prior year, but from the year preceding the divorce petition. In addition to lagged scores, we include dummy variables for placement type, student and family demographic characteristics (gender, race and ethnicity, mothers' age at petition, child age, English-language-learner status, presence of disability, and mothers' and fathers' wages and wages squared prior to petition, and dummy variables denoting zero-earnings). These variables may be relevant to the extent they have associations with post-divorce scores beyond any association captured by the lagged score. (In the fixed-effects models, these and all other time invariant factors, measured and unmeasured, are effectively controlled by the fixed effects approach.) The inclusion of pre-petition earnings controls explicitly for economic well-being that precedes marital dissolution, and allows us to control for an important

difference likely correlated with both placement and educational outcomes.[3] By design, it does not explicitly capture post-divorce economic circumstances. The coefficients on the shared placement variables denote the differential association between placement and post-divorce test scores, among students with similar observed characteristics who also had comparable baseline performance prior to their parents' divorce. As with the fixed-effects models, we also estimate models in which we add the additional time-varying variables that may be correlated with placement, namely current economic disadvantage and extent of economic disadvantage in current school. As in the child fixed-effects models, standard errors are clustered by child.

RESULTS

Pre-Divorce Economic Circumstances and Placement Outcomes

We begin by looking at the distribution of placement outcomes among the children in our sample, as well as how pre-divorce characteristics vary by later placement status. Table 1 shows the placement breakdown of the children, using sample weights to adjust for differences in sampling rates across counties and cohorts. Overall, 37 percent of the children had sole-mother placement, 14 percent had shared placement with mother as the primary custodian, and 40 percent had equal-shared placement. The remaining 9 percent had either sole-father placement, shared-father-primary placement, or miscellaneous other arrangements including with nonparents, and are included together in the 'other' category.

Consistent with past work, we find substantial differences in pre-divorce circumstances across the various placement types. Overall, pre-divorce combined parental earnings (in 2016 dollars) are highest for couples that subsequently end up with either mother-primary or equal-

[3]Specifically, baseline earnings are constructed based on the four calendar quarters that end in September of the first pre-petition school year.

shared placement, with substantially lower earnings for those who end up with mother-sole placement.[4] Looking separately at mothers' and fathers' earnings, and focusing on the three main placement groups, both mothers' earnings and fathers' earnings are lowest for children who end up with mother placement, as compared to those in the shared placement categories.[5] Consistent with their lower parental earnings, children in subsequent sole-mother placement are more likely to have received FoodShare prior to the divorce petition than those in shared placement arrangements, while children with equal-shared placement are least likely. The mean child age at time of divorce petition is about 10.5, with little difference across placement groups; note that our sample is almost entirely made up of children between the ages of 9 and 13 at petition date (reflecting pre-petition and post-divorce test score availability), and is not representative of the full range of children in divorcing households. Girls make up half the sample and are slightly overrepresented among mother-sole and mother-primary groups.

Changing Circumstances from Pre- to Post-Divorce

Table 2 shows two characteristics that may be relevant to children's school-related outcomes, both of which may be influenced by marital dissolution in general, and potentially by placement arrangements. These are economic disadvantage as reported in DPI data (and thus linked to the child's current household), and share of economically disadvantaged students in

[4]These earnings are based on earnings reported to the state Unemployment Insurance system, and include zero earnings when none are reported. In some cases, parents may nonetheless have earnings from other sources such as self-employment or work not reported to the UI system including out-state or off-the-books.

[5]Note that these earnings are somewhat higher than reported in other work that looks at all divorcing households in Wisconsin, although the pattern across placement groups is consistent with past work. Our sample is based on children rather than households, and only includes children age 9 to 13 at petition; in addition, our incomes are reported in 2016 dollars whereas earlier work in Wisconsin uses earlier reference years for income.

current school. We show these outcomes for four time periods: the pre-petition year and each of the first three post-petition years, limited to years in which the divorce was final.

The first panel shows that the prevalence of economic disadvantage increases sharply in the post-divorce years, from around 22 percent prior to petition to 36-40 percent post-divorce. Baseline levels of economic disadvantage are highest for children who subsequently have sole-mother placement, and lowest for those who subsequently have equal-shared placement (consistent with the previously noted differences in parental earnings). The prevalence increases sharply after marital dissolution for all groups (though the change is not statistically significant in the case of mother-primary placement), with the largest increase for the equal placement group, although absolute rates remain highest for the sole placement group throughout the period. The second panel shows that children on average attend schools with higher levels of economic disadvantage after divorce as compared to pre-petition. Looking across groups, children in the equal-shared placement group on average attend economically better off schools prior to divorce than the other groups, a difference that generally persists in the post-divorce period.

Test Scores by Placement Type: Descriptive Results

Table 3 shows mean normalized test scores at each time point relative to petition, from one year pre-petition to three years post-petition. All children in our sample have pre-petition scores and one or more post-petition scores. Looking across groups, pre-petition scores are highest for the children who subsequently move into shared placement arrangements (equal-shared or mother-primary), and lower for those who subsequently move into sole-mother placement. While there appear to be some changes from the pre-petition to post-divorce period,

none of these differences over time are statistically significant, either overall or for any of the placement subgroups.

Difference in Difference Fixed-Effects Models

We estimate difference-in-difference models with child fixed effects, as described above, to estimate the relationship between placement and post-divorce scores. We begin with models that do not control for placement type, in order to obtain an aggregate estimate of the association between divorce and test scores among the range of placements observed in our sample. As seen in Table 4, math scores are lower after divorce as compared to pre-petition by 0.054 standard deviations ($p<.05$), while they are statistically indistinguishable pre- and post-divorce in reading. Looking separately by gender, the decline in math scores is only significant for boys, while reading scores continue to be statistically indistinguishable before and after divorce for both girls and boys. In the remainder of our analyses, we focus on whether these pre-post relationships mask differences by placement arrangements.

Fixed-effects models adding placement by post-divorce interactions are shown in Table 5. Looking first at the base math model (Panel A column 1), the uninteracted "post-petition years" coefficient indicates that post-divorce normalized scores, while negative, are not significantly different than corresponding pre-petition scores, after controlling for time invariant child characteristics via fixed effects. Because of the inclusion of the shared placement by post-divorce interaction terms, the interpretation on the uninteracted "post" coefficient is the average post-divorce versus pre-divorce difference for children in sole-mother placement. The coefficients for the interactions between post-divorce and each of the shared placement categories (mother-primary and equal placement) show the treatment effects (the 'difference-in-difference') of shared placement relative to sole-mother placement; both are small and

statistically indistinguishable from zero. In the case of reading scores (Panel B column 1), the post-petition coefficient again indicates that there is no statistically significant change from pre-petition to post-divorce. The coefficients on the post-divorce-by-placement interactions show a difference of -.108 standard deviation for children with equal placement relative to children with sole-mother placement ($p<.05$), and relative differences that are smaller and not statistically significant in the case of mother-primary placement.

The above models control for time-invariant characteristics via child fixed effects. We next add time-varying variables that could potentially be influenced both by divorce in general and by placement arrangements, including current economic disadvantage and percent of economically disadvantaged children in current school, as described earlier. None of these coefficients are statistically significant, and including these variables does not lead to substantive changes in the associations between placement and either math or reading scores (Column 2). Overall, these initial models show that math scores do not significantly differ after divorce in the event of sole-mother placement, and that there is no differential change from pre- to post-divorce among placement groups; that reading scores do not change in the aggregate but do show declines for children with equal-shared placement relative to those with sole placement; and that none of these relationships are explained by changes in economic disadvantage or the share of disadvantaged children in a school.

We also look at girls and boys separately to see if there are different placement associations with test scores by gender. Table 5 also shows these models. We find differing patterns by gender in our base models. For boys, math scores decline following divorce by 0.114 standard deviations ($p<.05$), while the decline is slightly smaller and not statistically significant for reading (Column 3). There are no significant differences in this decline among placement

groups, although the coefficients for shared placement are uniformly positive. These patterns are unchanged by the addition of the time-varying variables.[6] Unlike boys, girls do not show a significant post-divorce change in math scores (Column 5); and as with boys, there are no differential changes in post-divorce math scores by placement. In the case of reading scores, girls show a significant increase following divorce of 0.157 standard deviations ($p<.05$), though offset by larger declines of 0.195 standard deviations ($p<.05$) and 0.269 standard deviations ($p<.01$) in the case of mother-primary and equal-shared placement, respectively. All of these relationships persist and are if anything slightly larger after controlling for time-varying characteristics (Column 6). Overall, these results suggest that there is an overall post-divorce decline in math scores following divorce that is limited to boys, while the previously noted decline in reading scores for shared placement children relative to those with sole-mother placement is limited to girls.

We also examine whether the association (or lack of association) between placement and test scores is broadly evident over the three-year post-petition period. For this analysis, we include separate dummy variables for each post-petition year as well as corresponding interactions with placement type. Because of limited sample size, we combine equal shared and mother-primary placement into a single shared placement category, which is consistent with the similar coefficients on these variables in our earlier models. Table 6 shows the relevant coefficients for the base models, overall and for girls and boys. Shared placement is not linked to either math or reading scores in any post-petition years for the overall or boys sample. For girls, the placement*year interactions are also insignificant for all time periods in the math model; in

[6]We also estimated the models with a combined shared-placement group that combined mother-primary and equal-shared; the shared*post coefficient for boys was still positive but not statistically significant.

the girls' reading model, the associations between shared placement and test scores are negative and significant for both the first and third post-petition. These results suggest that the negative association between shared placement and girls' reading scores is fairly broadly evident in the early post-divorce years.

Finally, we assess whether our estimates are sensitive to the inclusion or exclusion of outliers, which we define as students for whom the range between low and high scores is greater than three standard deviations (equivalent to a range of 1.551 or higher in normalized scores) from the mean range of 0.635 for math and 0.682 for reading. We define outliers based on the within-student score range versus absolute score levels because the model is estimating, in effect, the association between placement and the observation-level differences from in-student means. Focusing on the girls' reading model—the only model in which we observed significant placement associations—dropping the outliers results in 7 fewer students, out of the original 408 (Table 7). With this exclusion, the equal-shared placement coefficient remains strongly significant ($p<.01$), while declining in magnitude from -.269 to -.192; the mother-primary placement coefficient declines in magnitude from -.195 to -.118 and is no longer significant; and the post-divorce coefficient declines from 0.157 to 0.08 and is no longer significant. Thus, the negative association between equal-shared placement and girls' reading scores seems fairly robust to the exclusion of outliers, while the negative association for mother-primary placement and the positive association for post-divorce are more sensitive to their exclusion. Excluding outliers had no substantive impact on either the math models or the boys' reading model (not shown). Note that excluding outliers allows us to assess the extent to which a small number of students with large changes may be influencing the results; it does not imply that there are problems per se with the outliers.

Lagged Dependent Variable Models

We next estimate lagged dependent variable models, predicting post-divorce scores as a function of pre-petition scores and a variety of fixed and time-varying child characteristics (Table 8). Here, the treatment effects are captured by uninteracted placement coefficients, as only post-divorce observations are included (with pre-divorce outcomes serving as lags). Looking first at the overall sample, results show no differences in post-divorce test scores among placement groups, in either the base or full models, net of pre-petition score and a range of child and family characteristics (Column 1). Pre-petition scores are strongly correlated with post-divorce scores. Very few other covariates are significant (results not shown), which likely reflects that their impact is already captured by the lagged score. When we look separately by gender, we continue to see no differences in either math or reading scores by placement for boys, though the shared placement coefficients are in all cases positive (Column 3). For girls, there are also no differences in post-divorce math scores across placement groups, net of baseline scores and characteristics, while reading scores are significantly lower for girls with equal-shared placement in both base and full models (by 0.162 and 0.167 standard deviations, $p<.05$) (Columns 5 and 6). The coefficient for mother-primary placement is also negative and similar in magnitude but only significant in the full model. Overall, these results are broadly consistent with the fixed-effects models, in that they suggest negative associations between shared placement and reading scores that are limited to girls, and no association between placement and math scores for either girls or boys.[7] We experimented, additionally, with the inclusion of a

[7]Because the boys' coefficients for both placement groups were positive in both math and reading models, we also estimated the lagged models with a combined shared placement groups to garner greater power; shared placement, while positive, did not approach significance for boys in either the math or reading models, and remained negative and significant in the girls' reading model (results not shown).

variable for the number of months between petition and final judgment, as a proxy for the degree of difficulty in reaching a divorce settlement, which could potentially be associated with differential placement or outcomes. This was not significant nor did it substantively affect the other coefficients (results not shown).

To illustrate the importance of controlling for lagged scores, such that placement impacts are capturing differences net of baseline differences, we reestimated our models without lagged scores (Table 9). Here, the placement coefficients represent the difference in post-divorce scores controlling for observable characteristics of children and families, but not for unobserved differences captured in the baseline scores. This is akin to the kind of analysis that could be done if we were limited to cross-sectional post-divorce data. Both the full lagged model and the same model excluding the lag are shown. Most notably, the coefficients on both shared placement coefficients in the girls' reading model are smaller and not significant in the absence of the lagged (pre-petition) score, while the shared placement coefficients in all the other models (all the math models, and the reading models for full sample and boys) remain nonsignificant. More of the child and family control variables are significant in the models without the lag, with specifics varying across outcome and sample; this is as expected, as in the lagged models the associations between these characteristics and test scores are already reflected in the baseline score. Overall, this comparison highlights the importance to our analysis of controlling not only for observable child and family characteristics, but also for unobserved factors that are either captured in the pre-petition scores, or that are controlled via the fixed effects in our earlier models.

SUMMARY, CONCLUSIONS, AND POLICY IMPLICATIONS

This report has examined the association between post-divorce placement arrangements and children's standardized test scores, focusing on differences between children with shared- versus sole-mother placement arrangements. Our analysis has considered, specifically, the association between post-divorce placement and test scores among third through eighth grade students who were in fourth through seventh grades when their parents began divorce proceedings, and we examine test outcomes up to three years following the divorce petition. A critical feature of this work is our focus on within-child changes from the pre-to-post-divorce period, thus strengthening our ability to make causal inferences. Consistent with past work, we document substantial pre-divorce differences in economic circumstances of children who subsequently have different placement arrangements, and we further find that children with different placement arrangements have very different pre-divorce scores; these differences highlight the importance of controlling carefully for underlying differences among children in different placement groups, which we do via our focus on within-child changes using fixed effects and lagged dependent variable models. We demonstrate, further, that explicitly controlling for pre-divorce scores leads to different conclusions about the role of placement than simply including extensive controls in a cross-sectional analysis.

Our analyses provide some evidence that post-divorce placement arrangements are associated with modest differences in girls' standardized reading scores during the initial years following marital dissolution, with this difference most robust when comparing equal-shared placement to sole-mother placement. Specifically, we find that post-divorce reading scores are roughly 0.16 to 0.27 standard deviations lower among girls with equal-shared placement arrangements, relative to those with sole-mother placement, net of baseline scores and other

characteristics, with the magnitude depending on the model structure and whether outliers are included or excluded. We find no evidence that these associations operate through differences among placement groups in post-divorce economic disadvantage or school characteristics (specifically, the share of low-income students). The point estimates are larger in the fixed effects versus lagged dependent variable models. Consistent with Angrist and Pischke (2008), these estimates may bracket the true association. While we find some evidence of negative associations for girls in mother-primary-shared placement arrangements as well, this association is somewhat less robust across models and samples. Notably, the negative association for any of the shared placement groups is limited to girls' reading scores; we find no association between placement and math scores, nor between placement and either score for boys. We also note that the shared placement coefficients are consistently positive for boys for both math and reading scores, albeit not statistically significant.

An important caution is that we have only assessed the role of shared placement among the kinds of cases in which it is currently used. To the extent that any impacts vary across children, the effect of extending shared placement to children different from those who currently have such arrangements could potentially differ. Further, we have focused solely on a narrow subset of children—those in fourth through seventh grades at the start of their parents' divorce proceedings. While this affords traction for the analysis in allowing us to focus on within-child changes in scores, it also limits the generalizability of our findings. Furthermore, our analytic strategies are not foolproof even for this age range. In particular, we are unable to control for unobserved factors that might differentially affect children's adjustment to divorce. Thus, those who have similar baseline scores might nonetheless have very different vulnerabilities that impact their post-divorce performance, and such differences could potentially influence parents'

or courts' decisions about optimal placement arrangements. We note also that our sample sizes are relatively small, which makes it difficult to hone in on specific groups of children for whom different placement arrangements may be more or less beneficial. With a larger sample we could look more closely within the girls' and boys' samples, for instance, to assess whether there were observable factors that differentiate outcomes in different placement arrangements.

Our data are also limited to 21 counties in one state, and thus are not necessarily more broadly generalizable to other places. While we are not aware of other studies that have examined placement and test scores, our results are nonetheless broadly consistent with recent work that uses fixed effects approaches to estimate divorce impacts on test scores using national survey data, albeit with a broader age range of children. In that work divorce is associated with a decline in math scores, and no change in reading scores (Anthony, DiPerna and Amato 2014). We find similar patterns when we consider divorce independent of placement. This provides at least some evidence of external validity of our sample, albeit not speaking directly to whether the placement findings would be generalizable. On the other hand, Anthony and colleagues (2014) find that the negative math impacts of divorce are stronger for girls than for boys, which we do not observe in our sample.

Taken as a whole, and subject to the limitations as noted, our results provide no firm evidence of placement impacts on test scores for boys, or on math scores for either girls or boys, while they provide some evidence that shared placement, particularly equal-shared, may be associated with moderately worse reading performance for girls. It is important to consider these results in the context of the broader literature on shared placement, where a large and growing body of evidence points to substantial benefits of shared placement ranging from family relationships to physical and mental health to positive adolescent behaviors (see Nielsen 2018b

for comprehensive review). That literature has also tended to show that children have similar outcomes in the educational and cognitive realm regardless of placement—that is, the benefits associated with shared placement do not, by and large, appear to extend to the educational and cognitive realm, based on the bulk of the evidence to date. Because our study looks at an outcome that has not to our knowledge been examined in the shared placement literature (namely standardized test scores)—and because we use rigorous methods that explicitly control for baseline differences—our findings add important new information to the discussion. At the same time, it is critical to remember that standardized scores are but one of many outcomes, and that our analyses do not speak to the mechanisms underlying the observed relationships. An important direction for future work in this area is to go beyond quantitative comparisons of scores and to explore the extent to which outcomes are linked not only to placement or other objective circumstances but to important dimensions of family dynamics. The possibility of differences in attendance patterns with different placement types also warrants attention. Further, it is important for future work to further examine gender differences in placement outcomes, given the consistently positive, albeit not significant, coefficients for shared placement that we found in the boys' models. Surveys of parents in shared placement arrangements could provide more insight to help understand factors associated with children's academic and other outcomes. Overall, this study highlights the importance of more in-depth inquiry into the circumstances in which all children can thrive in the range of post-divorce parenting arrangements.

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Table 1. Characteristics of children with pre-petition and post-divorce test scores, overall and by placement

	Mother Sole	Mother Primary	Equal Shared	Others	Total
N students	308	116	330	73	827
Percent total	37%	14%	40%	9%	100%
Mother's earnings pre-petition year	25,091.6	31,363.6	32,209.9	24,174.7	28,710.8
Father's earnings pre-petition year	40,354.6	57,715.9	64,042.9	52,022.7	53,130.4
Parents' combined earnings pre-petition year	65,446.2	89,079.5	96,252.8	76,197.4	81,841.2
Any FoodShare pre-petition year	17.7%	7.9%	5.0%	15.3%	11.1%
Student's age at petition	10.7	10.4	10.6	10.9	10.6
Mother's age at petition	38.2	39.1	39.1	36.8	38.6
Female	53.0%	55.3%	45.5%	47.3%	49.9%

Note: Weighted to adjust for differing sample rates across cohorts and counties.

Table 2. Household and school economic characteristics by placement types and years since petition

Variables	Placement types	Year relative to petition			
		P-1	P+1	P+2	P+3
Economically disadvantaged (household)	Total				
	%	0.218	0.361***	0.387***	0.398***
	SE	*0.014*	*0.019*	*0.019*	*0.023*
	Mother Sole				
	%	0.339	0.455***	0.489***	0.490***
	SE	*0.027*	*0.032*	*0.033*	*0.039*
	Mother Primary				
	%	0.173	0.281	0.243	0.293
	SE	*0.035*	*0.054*	*0.044*	*0.052*
	Equal Shared				
	%	0.114	0.270***	0.319***	0.343***
	SE	*0.018*	*0.027*	*0.029*	*0.035*
Percent disadvantaged (school)	Total				
	%	0.269	0.296**	0.316***	0.345***
	SE	*0.008*	*0.008*	*0.008*	*0.010*
	Mother Sole				
	%	0.302	0.342	0.349**	0.386***
	SE	*0.014*	*0.015*	*0.015*	*0.020*
	Mother Primary				
	%	0.282	0.275	0.313	0.331
	SE	*0.021*	*0.023*	*0.022*	*0.023*
	Equal Shared				
	%	0.222	0.240	0.269***	0.297***
	SE	*0.010*	*0.010*	*0.010*	*0.012*

Notes: There are N=827, 631, 650, 467 total observations at years p-1, p+1, p+2, and p+3, respectively; the corresponding N's for "mother sole" are 308, 238, 237, 167; for "mother primary" are 116, 70, 98, 77; for "equal shared" are 330, 271, 259, 190. Percents weighted to reflect sampling rates by county and cohort. Asterisks denote significantly different from pre-petition (p-1) year (**=p<.05, ***=p<.01).

Table 3. Weighted mean test scores by year-relative-to-petition and placement type

Panel A: Math

	Total			Mother Sole			Mother Primary			Equal Shared		
Year	N	Mean	SE	N	Mean	SE	N	Mean	SE	N	Mean	SE
P-1	827	0.122	0.032	308	0.006	0.052	116	0.235	0.080	330	0.282	0.047
P+1	631	0.073	0.036	238	-0.087	0.056	70	0.216	0.099	271	0.261	0.055
P+2	650	0.078	0.037	237	-0.040	0.064	98	0.221	0.077	259	0.250	0.056
P+3	467	0.074	0.040	167	-0.032	0.071	77	0.137	0.083	190	0.218	0.060
Total	2575	0.090	0.018	950	-0.036	0.030	361	0.207	0.042	1050	0.257	0.027

Panel B: Reading

	Total			Mother Sole			Mother Primary			Equal Shared		
Year	N	Mean	SE	N	Mean	SE	N	Mean	SE	N	Mean	SE
P-1	827	0.060	0.033	308	-0.092	0.059	116	0.236	0.077	330	0.217	0.047
P+1	631	0.048	0.035	238	-0.090	0.061	70	0.317	0.102	271	0.164	0.050
P+2	650	0.043	0.037	237	-0.077	0.064	98	0.253	0.090	259	0.166	0.053
P+3	467	0.072	0.043	167	-0.016	0.074	77	0.097	0.092	190	0.210	0.066
Total	2575	0.055	0.018	950	-0.075	0.032	361	0.227	0.045	1050	0.190	0.026

Note: There is no significant difference between the mean at any post-petition year and the mean at pre-petition (p-1) year.

Table 4. Weighted fixed-effect regression models of normalized test scores, without placement controls

	Math			Reading		
	Overall	Boys	Girls	Overall	Boys	Girls
Post-petition	-0.054**	-0.063**	-0.045	-0.019	-0.055	0.017
	(0.021)	(0.030)	(0.029)	(0.023)	(0.031)	(0.034)
Observations	2,575	1,298	1,277	2,575	1,298	1,277
Number of students	827	419	408	827	419	408
R-squared	0.005	0.007	0.004	0.001	0.005	0.000

Note: The models only control for post-petition dummy and individual fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05.

Table 5. Weighted fixed-effect regression models of normalized test scores

Panel A: Math						
	Overall		Boys		Girls	
	Base	Full	Base	Full	Base	Full
Post-petition year	-0.063	-0.064	-0.114**	-0.120**	-0.017	-0.003
	(0.032)	(0.035)	(0.045)	(0.049)	(0.046)	(0.050)
Mother Primary*Post	0.005	-0.009	0.058	0.059	-0.041	-0.078
	(0.057)	(0.058)	(0.090)	(0.094)	(0.073)	(0.071)
Equal Shared*Post	0.017	0.029	0.054	0.054	-0.012	0.002
	(0.047)	(0.049)	(0.066)	(0.069)	(0.068)	(0.070)
person-level disadvantage		-0.046		-0.056		-0.039
		(0.043)		(0.067)		(0.051)
% disadvantaged in school		0.006		0.630		-0.739
		(0.374)		(0.510)		(0.508)
(% disadvantaged) squared		0.163		-0.566		0.884
		(0.409)		(0.601)		(0.511)
Observations	2,575	2,497	1,298	1,253	1,277	1,244
Number of students	827	821	419	416	408	405
R-squared	0.005	0.008	0.014	0.018	0.010	0.017

Panel B: Reading						
	Overall		Boys		Girls	
	Base	Full	Base	Full	Base	Full
Post-petition year	0.040	0.044	-0.092	-0.087	0.157**	0.165**
	(0.044)	(0.046)	(0.055)	(0.057)	(0.064)	(0.069)
Mother Primary#Post	-0.072	-0.090	0.067	0.042	-0.195**	-0.211**
	(0.063)	(0.065)	(0.093)	(0.095)	(0.084)	(0.087)
Equal Shared*Post	-0.108**	-0.110**	0.061	0.046	-0.269***	-0.261***
	(0.055)	(0.056)	(0.073)	(0.073)	(0.079)	(0.080)
person-level disadvantage		-0.048		0.002		-0.100
		(0.051)		(0.085)		(0.054)
% disadvantaged in school		0.439		0.672		0.116
		(0.440)		(0.696)		(0.527)
(% disadvantaged) squared		-0.398		-0.634		-0.087
		(0.496)		(0.819)		(0.596)
Observations	2,575	2,497	1,298	1,253	1,277	1,244
Number of students	827	821	419	416	408	405
R-squared	0.004	0.006	0.006	0.007	0.023	0.026

Note: The models also control for the interaction of "other placements" with the post-petition year dummy. Robust standard errors in parentheses. *** p<0.01, ** p<0.05.

Table 6. Weighted fixed-effect regression models of normalized test scores, including post-petition years

	Overall		Boys		Girls	
Variables	Math	Reading	Math	Reading	Math	Reading
Post-petition year 1	-0.052	0.070	-0.081	-0.036	-0.028	0.163***
	(0.036)	(0.048)	(0.052)	(0.073)	(0.050)	(0.062)
Post-petition year 2	-0.067	-0.007	-0.126**	-0.101	-0.013	0.078
	(0.040)	(0.049)	(0.061)	(0.057)	(0.053)	(0.077)
Post-petition year 3	-0.074	0.060	-0.157**	-0.181**	-0.006	0.257***
	(0.044)	(0.064)	(0.063)	(0.090)	(0.060)	(0.085)
Mother Primary & Equal Shared*Post-year-1	0.021	-0.106	0.033	0.042	0.015	-0.244***
	(0.052)	(0.058)	(0.075)	(0.087)	(0.072)	(0.076)
Mother Primary & Equal Shared*Post-year-2	0.009	-0.067	0.026	0.024	-0.001	-0.148
	(0.053)	(0.059)	(0.077)	(0.075)	(0.071)	(0.089)
Mother Primary & Equal Shared*Post-year-3	0.012	-0.133	0.14	0.166	-0.099	-0.387***
	(0.057)	(0.074)	(0.081)	(0.105)	(0.079)	(0.097)
Observations	2,575	2,575	1,298	1,298	1,277	1,277
Number of students	827	827	419	419	408	408
R-squared	0.007	0.008	0.021	0.015	0.016	0.034

Note: The models also control for the interactions of "other placements" with the three post-petition years. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05

Table 7. Weighted fixed-effect regression models of girls' reading scores, with and without outliers

outliers excluded?	no	yes
Post-petition years	0.157**	0.08
	(0.064)	(0.048)
Mother Primary#Post	-0.195**	-0.118
	(0.084)	(0.073)
Equal Shared#Post	-0.269***	-0.192***
	(0.079)	(0.066)
Observations	1,277	1,253
Number of students	408	401
R-squared	0.023	0.016

Note: Outliers are students whose maximum score range over the observation period is more than 3 standard deviations over the mean range.The models also control for the interaction between other placements and post-petition. Robust standard errors in parentheses. *** p<0.01, ** p<0.05

Table 8. Weighted lagged regression models of normalized test scores

Panel A: Math						
	Overall		Boys		Girls	
	Base	Full	Base	Full	Base	Full
Mother Primary	0.008	0.019	0.072	0.105	-0.045	-0.046
	(0.054)	(0.055)	(0.081)	(0.084)	(0.070)	(0.073)
Equal Shared	0.011	0.012	0.033	0.032	-0.018	-0.008
	(0.046)	(0.047)	(0.069)	(0.071)	(0.060)	(0.060)
Lagged math score	0.686***	0.677***	0.690***	0.673***	0.685***	0.679***
	(0.033)	(0.033)	(0.040)	(0.040)	(0.050)	(0.050)
person-level disadvantage		0.021		0.030		-0.012
		(0.053)		(0.075)		(0.066)
% disadvantaged		-0.291		-0.625		-0.025
		(0.314)		(0.493)		(0.398)
(% disadvantaged) squared		0.208		0.383		0.005
		(0.359)		(0.575)		(0.470)
Observations	1,738	1,704	874	849	864	855
R-squared	0.627	0.625	0.592	0.584	0.688	0.688

Panel B: Reading						
	Overall		Boys		Girls	
	Base	Full	Base	Full	Base	Full
Mother Primary	-0.018	-0.032	0.128	0.129	-0.141	-0.175**
	(0.065)	(0.066)	(0.092)	(0.096)	(0.083)	(0.087)
Equal Shared	-0.041	-0.055	0.067	0.046	-0.162**	-0.167**
	(0.055)	(0.055)	(0.073)	(0.074)	(0.076)	(0.075)
Lagged reading score	0.670***	0.662***	0.692***	0.682***	0.650***	0.647***
	(0.042)	(0.042)	(0.049)	(0.049)	(0.066)	(0.064)
person-level disadvantage		-0.084		0.024		-0.170**
		(0.048)		(0.068)		(0.068)
% disadvantaged		-0.311		0.168		-0.519
		(0.378)		(0.573)		(0.461)
(% disadvantaged) squared		0.303		-0.576		0.674
		(0.455)		(0.732)		(0.539)
Observations	1,738	1,704	874	849	864	855
R-squared	0.564	0.567	0.571	0.574	0.590	0.598

Note: The models also control for "other placements", its interaction with the post-petition years dummy, and other socioeconomic covariates such as race-ethnicity, disability status, English-language learner status, age, cohorts etc. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9. Weighted regression models of normalized test scores, with and without lagged score

	Overall				Boys				Girls			
	Math		Reading		Math		Reading		Math		Reading	
with lagged score?	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no
Lagged math score	0.686***				0.690***				0.685***			
	(0.033)				(0.040)				(0.050)			
Lagged reading score			0.670***				0.692***				0.650***	
			(0.042)				(0.049)				(0.066)	
Mother Primary	0.008	-0.033	-0.018	0.034	0.072	0.069	0.128	0.134	-0.045	-0.128	-0.141	-0.080
	(0.054)	(0.094)	(0.065)	(0.101)	(0.081)	(0.132)	(0.092)	(0.144)	(0.070)	(0.125)	(0.083)	(0.133)
Equal Shared	0.011	0.008	-0.041	0.034	0.033	-0.017	0.067	0.067	-0.018	0.001	-0.162**	-0.032
	(0.046)	(0.070)	(0.055)	(0.074)	(0.069)	(0.102)	(0.073)	(0.099)	(0.060)	(0.094)	(0.076)	(0.100)
Female	-0.035	-0.151**	0.062	0.055								
	(0.040)	(0.061)	(0.045)	(0.062)								
Asian	0.292**	0.444**	0.108	0.239	0.020	0.120	-0.003	0.181	0.576***	0.833***	0.241	0.354
	(0.135)	(0.211)	(0.116)	(0.157)	(0.164)	(0.281)	(0.149)	(0.238)	(0.182)	(0.307)	(0.201)	(0.222)
Black	-0.135	-0.356***	-0.061	-0.297**	-0.244**	-0.482**	-0.249	-0.451**	-0.051	-0.282**	0.084	-0.205
	(0.071)	(0.119)	(0.092)	(0.132)	(0.110)	(0.206)	(0.138)	(0.213)	(0.082)	(0.133)	(0.110)	(0.148)
Hispanic	-0.023	-0.206	0.049	-0.053	-0.078	-0.219	0.024	-0.033	0.039	-0.197	0.046	-0.115
	(0.063)	(0.106)	(0.076)	(0.093)	(0.100)	(0.168)	(0.128)	(0.155)	(0.079)	(0.137)	(0.098)	(0.125)
Native American	0.046	0.004	0.197	0.370	0.212	0.369**	0.178	0.785	-0.040	-0.295	0.089	-0.070
	(0.136)	(0.224)	(0.111)	(0.337)	(0.109)	(0.149)	(0.175)	(0.410)	(0.215)	(0.364)	(0.122)	(0.404)
Disability	-0.222***	-0.776***	-0.159	-0.937***	-0.121	-0.599***	-0.009	-0.674***	-0.397***	-1.063***	-0.334	-1.295***
	(0.078)	(0.133)	(0.107)	(0.124)	(0.094)	(0.136)	(0.107)	(0.139)	(0.130)	(0.238)	(0.187)	(0.204)
English language learner	-0.444***	-0.658**	-0.213	-0.735***	-0.302	-0.296	-0.368	-0.604***	-0.598***	-1.090***	-0.224	-0.995***
	(0.161)	(0.268)	(0.136)	(0.185)	(0.224)	(0.218)	(0.212)	(0.217)	(0.220)	(0.368)	(0.177)	(0.206)
10 years old	0.015	-0.067	0.026	-0.077	-0.001	-0.145	0.021	-0.188	0.029	-0.012	0.037	0.024
	(0.052)	(0.083)	(0.059)	(0.083)	(0.079)	(0.123)	(0.089)	(0.118)	(0.067)	(0.104)	(0.076)	(0.104)
11 years old	-0.042	-0.062	0.032	-0.103	-0.114	-0.221	-0.088	-0.337***	0.026	0.086	0.168	0.132
	(0.058)	(0.087)	(0.063)	(0.087)	(0.087)	(0.127)	(0.091)	(0.116)	(0.073)	(0.118)	(0.090)	(0.127)
12 years old	-0.036	-0.163	0.049	-0.152	0.002	-0.233	0.069	-0.338***	-0.060	-0.081	0.060	0.041
	(0.057)	(0.090)	(0.065)	(0.091)	(0.090)	(0.140)	(0.097)	(0.127)	(0.071)	(0.114)	(0.087)	(0.122)
13 years old	-0.023	-0.022	-0.037	-0.153	-0.029	-0.126	-0.156	-0.565***	-0.042	0.077	0.036	0.209
	(0.091)	(0.122)	(0.083)	(0.145)	(0.108)	(0.157)	(0.106)	(0.188)	(0.146)	(0.188)	(0.118)	(0.189)
Mother's age at petition	0.005	0.013**	0.006	0.019***	0.007	0.016	0.007	0.026***	0.002	0.009	0.002	0.011
	(0.004)	(0.005)	(0.004)	(0.006)	(0.005)	(0.008)	(0.006)	(0.008)	(0.005)	(0.007)	(0.006)	(0.007)
Mother's earnings at P-1	0.010	0.047**	0.020	0.034	0.027	0.033	-0.024	-0.021	0.019	0.055**	0.042***	0.059**
	(0.012)	(0.018)	(0.013)	(0.019)	(0.025)	(0.038)	(0.025)	(0.036)	(0.015)	(0.024)	(0.016)	(0.026)
Mother's earnings squared at P-1	0.001	-0.000	-0.001	-0.001	-0.001	0.001	0.002	0.002	0.001	-0.001	-0.001**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Father's earnings at P-1	0.009	0.051***	0.009	0.031**	-0.007	0.033	-0.007	0.032	0.017	0.065***	0.012	0.036**
	(0.010)	(0.014)	(0.010)	(0.013)	(0.019)	(0.029)	(0.020)	(0.026)	(0.012)	(0.019)	(0.012)	(0.017)
Father's earnings squared at P-1	-0.000	-0.001**	-0.000	-0.001**	0.001	-0.001	0.001	-0.001	-0.000	-0.001***	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)
Constant	-0.242	-0.574***	-0.372**	-0.756***	-0.312	-0.505	-0.234	-0.724**	-0.164	-0.745***	-0.255	-0.639**
	(0.147)	(0.217)	(0.178)	(0.228)	(0.235)	(0.345)	(0.256)	(0.329)	(0.175)	(0.266)	(0.223)	(0.287)
Observations	1,738	1,738	1,738	1,738	874	874	874	874	864	864	864	864
R-squared	0.627	0.257	0.564	0.237	0.592	0.220	0.571	0.246	0.688	0.342	0.590	0.299

Note: The models also control for cohorts, other placement types, and missing or zero earnings indicators for mothers and fathers. Robust standard errors in parentheses. *** p<0.01, ** p<0.05.