Online Appendix for "Does Quebec's Subsidized Child Care Policy Give Boys and Girls an Equal Start?"

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Abstract

This is the online appendix for Does Quebec's Subsidized Child Care Policy Give Boys and Girls an Equal Start? Three sections are included, which provide further empirical results that are respectively suggestive evidence of policy effect heterogeneity, suggestive evidence that maternal labour supply is not the mechanism underlying the policy effects and assessing whether the policy impact estimates are robust to alternative assumptions on unobserved variables. We discuss the potential limitations of these analyses as well as issues related to identification.

1 Evidence for Heterogeneity in Policy Effects

Since it is possible that the decline in developmental outcomes captured by the ITT parameter (and reported in BGM) can reflect the compositional change in the treatment group, we next aim to additionally report what we term as a composition adjusted intent to treat parameter. This composition adjusted ITT captures the effect of access to childcare that does not operate through observed characteristics that influence the decision to attend child care.[1] This analysis would provide an additional indication that there may be unmodeled heterogeneity would be obtained following Solon et al. (2015) if weighted estimates differ substantially from the unweighted results. While this would suggest that there is heterogeneity (by sex) that previous research did not identify, it does potentially leave outstanding the source of unmodeled heterogeneity.

To recover this child care attendance composition adjusted ITT, requires us to reweight equation (1) to ensure that there are no differences in the observed characteristics of those who attend child care across groups, years and genders. The weights are obtained from estimates of the following equation with the full data using a series logit estimator

$$Ccare_{ipt} = \gamma_0 + \gamma_1' Policy_{pt} + \gamma_2' PROV_p + \gamma_3' YEAR_t + \gamma_4' X_{ipt} + u_{ipt} \tag{A1}$$

where Ccare is an indicator for being in child care.[2] Using the estimated coefficients from equation (A1), the predicted probabilities of receiving child-care ($\hat{p}(X_i)$) are first calculated for each individual. These predicted probabilities are then used to calculate estimated weights (W_i) as follows

$$W_i = \sqrt{\frac{Ccare_{ipt}}{\hat{p}(X_i)} + \frac{(1 - Ccare_{ipt})}{(1 - \hat{p}(X_i))}}. \tag{A2}$$

for each individual. By reweighting the regression specification in equation (1) we ensure that the covariates are balanced between those that attend and do not attend child care observations, and potentially come closer to understanding the average effect of child care attendance than the standard intent to treat parameter. In other words, this reflects the policy effect that does not arise from differences due to observables in considering who took up subsidized child care.[3] We stress that this parameter is neither an average treatment effect or average treatment effect for the treated and that the reweighted estimates of equation (1) simply recover the average effect of the child care policy that does not arise due to any changes in child care attendance. Following guidance in Solon et al. (2015), we report both composition weighted and unweighted ITT estimates since by contrasting them allows us to understand the degree to which the policy effects are driven by changes in the composition of child care users that were induced by the policy.

This strategy is similar in spirit to the semi-parametric differences-in-differences estimator of Abadie (2005) that uses reweighing to address potential imbalance of characteristics between treated and control groups. Abadie (2005) suggests by reweighting, the common trend assumption becomes more credible. Abadie (2005) differs since it would use an indicator for living in Quebec post-policy as the dependent variable in equation (A1). Unlike, the unweighted ITT estimates presented in the main text that only required a

[1]Related, prior work by Heckman et al. (2010) evaluating early childhood education programs clearly shows that conclusions may not be robust to balancing observed covariates between those who gained access to child care and those that did not.

[2]The series logit estimation incorporates all of the covariates used in BGM as well as their interactions. Note that the results presented in the next section are robust to using both parametric probit and logit estimators that do not include the set of covariate interactions.

[3]As additional motivation, there is unmodeled heterogeneity in the partial effects of the policy and as Solon et al. (2015) note the unweighted policy effect also captures the variation in characteristics of those who attend child care across regions and time.

mean independence assumption on the regressors in the estimating equation, this approach imposes full distributional independence. To understand why a comparison of effects is suggestive of heterogeneity, recall that the additive structure of the linear difference in differences estimating equation imposes the assumption that there is no systematic heterogeneity in treatment effects by characteristics. The reweighting similar to a matching strategy does not make any functional form assumptions but it should be emphasized that this methods cannot adjust for differences in unobserved characteristics.

Estimates of the policy effects that involves using the weights defined in equation (A2) to reweight the regression specification in equation (1) are presented in Online Appendix Table 2. On the one hand, these estimates do present a similar picture to the ITT estimates presented in the main text for all the child care and work decision outcomes with the exception of type of child care being attended, where the gender difference disappears and becomes statistically insignificant. This should not be a surprise since we are by design using the weights to account for changes in the composition of children attending child care. However, it is somewhat striking that once we reweight the observations in the data that statistically significant gender gaps emerge in the effects of access to subsidized child care on emotional anxiety, separation anxiety, hyperactivity and inattention. These gaps are driven exclusively by changes in the estimated policy effect for boys. These results reinforce that even when we do not change the characteristics of boys attending child care, that this policy is disproportionately shortchanging boys by increasing their scores on the behavioral indices related to these three negative behaviors. Similarly, after accounting for changes in composition of children attending child care, we observe that boys are significantly more likely to be in poor health after the policy while girls face a smaller significant decrease in having a nose / throat infection.[4]

Comparing the policy effects with and without weighting makes it clear that the changing composition of children entering child care in response to the policy is driving the negative effects of access on child anxiety and hyperactivity scores for girls.[5] The change in the statistical significance of the estimated policy effects after reweighting equation (1) is consistent with the Kottelenberg and Lehrer (2013) finding of a large negative causal effect of child care only for children who attended child care as a result of subsidization. This set of results suggests that the negative mean policy effects reported in BGM may be due to the changing composition of girls who attend subsidized child care. However, this is not the case for boys, indicating that the policy indeed leads to significant declines in child behavioral outcomes even among those who look similar to those who attended child care in the absence of the policy.

For completeness, we also reexamine how investments in home environments responded to the introduction of the Quebec Family Policy by reestimating equation (1) with the child care attendance weights. The results of this exercise for each of the outcomes summarized in the last panel of Table 2 are presented in Online Appendix Table 3. Comparing the ITT estimates in the main text to these weighted estimates presents several interesting differences. First, in the top panel that examines the impacts on measures of parental health and household environment, there are several striking differences since many of the ITT effects lose their statistical significance once the sample is reweighted. After reweighting, only boys experienced significant, at the 5% level, increases to family dysfunction and parent consistency once the policy was introduced. Second, in the final two panels that examine specific parental inputs for children aged 0-3 and 4 respectively, we find the statistically significant reduction in being read to daily from the policy reported in the main text disappears for girls but remains for boys. This continues to suggest that it is not solely the

[4] The unweighted estimates do not indicate any gender differences in the estimated policy effects on the child physical health measures, with the exception of parents of girls being less likely to claim their child is in excellent health.

[5] For girls, only the statistical significance of the estimated effect of access to subsidized child care on physical aggression scores is invariant to weighting Equation (1).

boys who are induced to attend child care by the policy that are shortchanged, but also among those who were attending child care in the absence of the policy. Among the subsample of 4 year old children, we find that the introduction of subsidized child care significantly reduces parental time spent focused on or doing activities with their child, but only in families with a daughter. In contrast, there was a larger significant reduction in the frequency with which parents of boys laughed with their children on a daily basis.

In this subsection, we propose using a child care composition adjusted ITT to shed additional light on whether is unmodeled heterogeneity in the policy effects. After all, many policies have effects that arise due to changes in the composition of those who take up the services newly subsidized by the policy. This take up reflects self-selection and one can simply not compare child care users to non-users to understand the impacts of child care policy. However, by comparing estimates of the policy effects reported in the main text to those that additionally adjust for changes in observables among the composition of those who attend subsidized child care allows us to conclude that for the subsample of girls, many of the negative impacts of access to child care are driven by changes in the composition of the sample who attends child care. Further, our results reinforce that even when the characteristics of boys attending child care are held constant, the policy is disproportionately shortchanging boys by increasing their scores on indices related to hyperactivity and inattention, emotional anxiety and separation anxiety.

2 Labour Supply as a Mechanism

The influence of maternal employment on parenting strategies and home environments is now indirectly considered in Online Appendix Table 3. We reestimate equation (1) on subsamples defined according to whether or not the mother is working. Since our aim is simply to provide suggestive evidence we are treating the labor supply decision as exogenous. However, the results are not substantially different if we allow for selection on unobservables and identify the equation by functional form assumptions and interaction terms.

Recall, in the main text we demonstrated that the policy had a large effect on maternal labour supply on the extensive margin for both genders. The top panel of Online Appendix Table 3 examines aggregated measures of parenting and indicates that there are larger statistically significant negative impacts on all four behaviors for girls than for boys. In families where mothers do not work, girls experience substantially more ineffective parenting, parent inconsistency, and negative interaction after the introduction of the policy. In contrast, the effects of access to child care on ineffective parenting and positive interactions are only statistically significant for boys in families where the mother works. Among families with working mothers, boys experience significantly more ineffective parenting and family dysfunction than girls. It is clear that there are significant differences relating to child gender in home environments on the basis of maternal employment.

The middle panel of Online Appendix Table 4 examines several specific child rearing practices for younger children aged 0-3. Notice, there are no differences in the magnitude, sign, or statistical significance of any of the estimated policy effects for daughters by maternal employment status. The sole exception is reading to the child, which experiences a larger decline among daughters of working mothers. In contrast, sons of working mothers do not experience a decline in having books read to them after the implementation of the policy, whereas sons of mothers who did not enter the labour force did experience a decline on this measure. Indeed, among sons of stay at home mothers there are also significant declines in spending focused time with the child and laughing with the child after the policy is introduced. The only parenting activity that declines for boys once their mother is working is the possibility of doing special activities which may be a result of parental time being crowded out.

In the bottom panel of Online Appendix Table 4, we observe that among stay at home mothers of boys aged 4 there is no change in the estimated policy effect for any of the parenting strategies. In contrast, for daughters aged 4 whose mother does not work, we observe a significant change in parenting practices. Parents are now more likely to give their daughters focused time daily but less likely to make more time intensive special activities as well as to laugh with their children. A similar increase in focused time for daughters of working mothers is also observed following the introduction of the policy, but now comes at the expense of taking their daughters to the library. Interestingly, the pattern and statistical significance of the effect of the policy on laughing with the child and doing special activities differs based on maternal labor supply between the genders. Boys face large and significant reductions in these activities only if their mother works. Yet, the inputs of focused time and being read to, an input which may have larger impacts in a human capital function, does not differ after the policy, continuing to reinforce that the quality of inputs does not change on the basis of maternal employment for boys.

In summary, irrespective of the assumption we place on the labour supply decision in generating the subgroups, we do not find significantly different estimates of the policy effect. These results are suggestive that labour supply is not the primary mechanism and in the main text we provide evidence that changes in parenting practices may explain why the Quebec subsidized child care policy reduced a multitude of individual and family outcomes related to health and behaviour.

3 Assessing Potential Bias in the Analysis of Parental Inputs as a Mechanism

One potential concern related to the analysis undertaken in Table 6 is whether there is any unobserved heterogeneity in the impact of the policy on parenting practices and in the effectiveness of those parenting practices on facilitating child development that may be driving observed effects. To assess this assertion we apply an adaptation of Altonji, Elder and Taber (2005) outlined in Appendix B of Baker and Milligan (2016).[6] Consider the following two equations

$$Y_i = \beta_0 Policy_i + \beta_1 (SEX * Policy_i) + \beta_2 X_i + \beta_3 (SEX * X_i) + \beta_4 PAR_i + \beta_5 (SEX * PAR_i) + \nu_i \tag{A3}$$

$$PAR_i = \delta_0 Policy_i + \delta_1 (SEX * Policy_i) + \beta_4 X_i + \beta_4 (SEX * X_i) + \eta_i, \tag{A4}$$

where X_i includes both the covariates from the main body of the paper and for ease of this analysis also incorporates the province and cycles dummies. The analysis presented in Table 6 can be replicated through the estimates of equation (A3) and equation (A4) by formally acknowledging that the policy variable simultaneously affects both the developmental scores and parenting variables. The potential concern regarding the analysis in Table 6 is that one implicitly assumes that the unobserved components of Equation (A3) and (A4), ν_i and η_i, are uncorrelated when in fact they may not be. Thus, the analysis completed in Online Appendix Table 5 presents estimates of β_0 and β_1 in Equation (A3) by relaxing this assumption and allowing for the correlation between ν_i and η_i to vary.

[6] A similar robustness exercise was undertaken in Kottelenberg and Lehrer to explore the robustness of a set of policy impacts. The prior analysis did not consider parenting behaviors and also did not use the linear framework outlined in Baker and Milligan (2016).

The results presented in Online Appendix Table 5 consider an analysis for the mediating variable "Reading – Times a Month" suggest that the analysis is robust to relatively strong failures of this assumptions.[7] We observe that the estimated impacts of β_0 for children aged 4 would change its sign only when the correlation between ν_i and η_i is equal to -0.5. At lower levels of the correlation, the effects remain unchanged in sign. Similarly, the main policy effects for children aged 0–3 and the gendered policy effects for both age groups, display even less sensitivity to the failure of this assumption. In all, this analysis is suggestive that the conclusions stated in the main text are robust to reasonably sized failures of this assumption.

[7] The tractability of the methodology requires that we examine a single parenting input at a time. We examine each of the parenting variables and find that results are reflective of the analysis presented and discussed in this section.

Table A1: Testing the Common Trend Assumption

	In some type of care	Mother Works	PPVT Standardized Score	MSD Score
Girls	0.003	-0.031	-1.164	-0.157
	(0.807)	(0.000)***	(0.124)	(0.775)
Boys	-0.003	-0.043	0.077	-0.839
	(0.862)	(0.000)***	(0.888)	(0.006)***

— Note: For the outcome variable in each row we present the test of the common trend assumption as laid out in the following equation: $Y_{ipt} = \beta_0 + \beta_1 YEAR_t + \beta_2 YEAR_t * QUE + \varepsilon_{ipt}$. Using data in the period before the policy implementation we report the coefficents β_2 for each corresponding outcome and test these coefficients for statistical difference from zero using a two-tailed test. P-values are presented in parentheses corresponding to the estimate in the row above. ***, ** and * indicate significance at the 1%, 5% and 10% level respectively.

Table A2: Estimates of the Causal Effect of Access to Universal Child Care by Gender

	Intention to Treat		Composition Adjusted	
	Girls (P-Value)	Boys (P-Value)	Girls (P-Value)	Boys (P-Value)
Child Care and Work Decisions				
In some type of care	0.186	0.205	0.135	0.134
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Care in Another's Home	**-0.007**	**-0.055**	-0.002	-0.017
	(0.788)	**(0.001)*** **	(0.841)	(0.059)*
Care in own home	-0.028	-0.007	0.003	0.006
	(0.031)**	(0.617)	(0.453)	(0.23)
Care in institutional setting	**0.224**	**0.263**	0.134	0.144
	(0.000)* **	**(0.000)*** **	(0.000)***	(0.000)***
Hours in all Child Care Arrangements	8.136	8.874	4.809	5.504
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
In full time care - More than 20 hours	0.221	0.222	0.13	0.137
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Mother Works	0.095	0.122	0.074	0.074
	(0.000)***	(0.000)***	(0.003)***	(0.002)***
Mother Works / Uses Childcare	0.143	0.165	0.108	0.109
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Mother Works / Does not use Childcare	-0.046	-0.049	-0.034	-0.035
	(0.000)***	(0.000)***	(0.174)	(0.145)
Mother does not Work / Uses Childcare	0.046	0.041	0.029	0.025
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Mother does not Work / Does not use Childcare	-0.143	-0.157	-0.103	-0.099
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Child Development, Behavior, and Health Outcomes				
MSD Score	-1.56	-1.74	-0.598	-0.551
	(0.198)	(0.004)***	(0.814)	(0.594)
PPVT Standardized Score	-0.912	-0.033	0.551	-0.391
	(0.449)	(0.977)	(0.925)	(0.901)
Hyperactivity and Inattention Score	0.136	0.511	**0.035**	**0.829**
	(0.449)	(0.000)***	**(0.944)**	**(0.000)*** **
Emotional Anxiety Score	0.333	0.1	0.193	0.344
	(0.002)***	(0.506)	(0.312)	(0.007)***
Physical Aggression Score	0.718	0.527	0.627	0.525
	(0.000)***	(0.000)***	(0.099)*	(0.038)**
Separation Anxiety Score	0.21	0.13	**-0.057**	**0.612**
	(0.079)*	(0.323)	**(0.925)**	**(0.000)*** **
Child in Excellent Health	-0.071	-0.029	**-0.032**	**-0.085**
	(0.000)***	(0.416)	**(0.451)**	**(0.000)*** **
Never had a Nose/Throat Infection	-0.153	-0.139	-0.113	-0.161
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Never had an Ear Infection	-0.066	-0.072	-0.015	-0.066
	(0.000)***	(0.000)***	(0.886)	(0.015)**

— Note: For the outcome variable in each row we present the estimates of the policy coefficient δ as specified in Equation (1) first without weighting (Intention to Treat) and second with the inverse propensity weights specified in Equation (2) (Composition Adjusted). For each specification we split the sample by gender as denoted in the column header. These regressions also include a set of dummies derived from the covariates listed in Table 1 as well as province and cycle indicators. We test the reported coefficients for statistical difference from zero using a two-tailed test and report adjusted p-values (presented in parentheses) corresponding to the estimate in the row above. These p-values make use of a Simes p-value adjustment procedure to account for testing effects on multiple related outcomes. The standard errors underlying the hypothesis tests are also corrected at the province-year level. ***, ** and * indicate significance at the 1%, 5% and 10% level respectively. Statistically different estimates between girls and boys are presented in bold face.

Table A3: Estimates of the Causal Effect of Access to Universal Child Care on Parenting / Family Outcomes

	Intention to Treat		Composition Adjusted	
	Girls (P-Value)	Boys (P-Value)	Girls (P-Value)	Boys (P-Value)
Mother in Excellent Health	-0.015	-0.013	0.037	-0.005
	(0.578)	(0.536)	(0.25)	(0.893)
Father in Excellent Health	0.007	-0.01	0.002	0.007
	(0.761)	(0.694)	(0.936)	(0.893)
Mother's Depression Score	0.846	0.483	0.114	0.451
	(0.002)***	(0.049)**	(0.797)	(0.081)*
Family Dysfunction Index	**-0.207**	**0.465**	**0.02**	**1.136**
	(0.578)	**(0.012)****	**(0.936)**	**(0.000)*****
Ineffective Parenting	0.943	0.602	0.203	0.201
	(0.000)***	(0.000)***	(0.25)	(0.236)
Parent Consistency	**-0.509**	**0.041**	0.578	0.878
	(0.012)**	**(0.799)**	(0.063)*	(0.000)***
Positive Interaction	**-0.821**	**-0.474**	-0.291	0.026
	(0.000)***	**(0.014)****	(0.285)	(0.893)
Ages 0-3				
Spends 5 minutes of focused time	**-0.119**	**-0.069**	-0.084	-0.04
- many times a day	**(0.017)*****	**(0.029)****	(0.022)***	(0.022)*
Laughs with child - many times a day	**-0.079**	**-0.039**	-0.065	-0.038
	(0.014)***	**(0.012)*****	(0.017)***	(0.018)**
Does a special activity that the child enjoys	-0.061	-0.056	-0.057	-0.062
- Once or twice a day or more	(0.017)***	(0.011)***	(0.025)**	(0.025)**
Plays a sport, game, or hobby with child	0.006	0.018	0.015	0.03
- Once or twice a day or more	-0.018	(0.010)*	-0.021	-0.02
Reads to child - daily	**-0.085**	**-0.032**	-0.005	-0.011
	(0.022)***	**-0.024**	-0.025	-0.025
Estimated Days Read to a Month	-1.56	-1.014	0.012	-0.02
	(0.448)***	(0.587)*	-0.572	-0.588
Age 4				
Spends 5 minutes of focused time	**0.084**	**-0.078**	**0.076**	**-0.071**
- many times a day	**(0.025)*****	**(0.042)***	**-0.058**	**-0.055**
Laughs with child - many times a day	**-0.029**	**-0.165**	-0.029	-0.116
	-0.022	**(0.036)*****	-0.046	(0.049)**
Does a special activity that the child enjoys	-0.097	-0.066	-0.077	-0.064
- Once or twice a day or more	(0.041)**	-0.045	-0.058	-0.057
Reads to child - daily	0.016	-0.05	**0.085**	**-0.054**
	-0.031	-0.053	**-0.06**	**-0.055**
Estimated Days Read to a Month	**-1.005**	**-2.479**	0.636	-2.015
	-0.77	**(1.097)****	-1.142	(1.143)*

— Note: For the outcome variable in each row we present the estimates of the policy coefficient δ as specified in Equation (1) first without weighting (Intention to Treat) and second with the inverse propensity weights specified in Equation (2) (Composition Adjusted). For each specification we split the sample by gender as denoted in the column header. These regressions also include a set of dummies derived from the covariates listed in Table 1 as well as province and cycle indicators. We test the reported coefficients for statistical difference from zero using a two-tailed test and report adjusted p-values (presented in parentheses) corresponding to the estimate in the row above. These p-values make use of a Simes p-value adjustment procedure to account for testing effects on multiple related outcomes. The standard errors underlying the hypothesis tests are also corrected at the province-year level. ***, ** and * indicate significance at the 1%, 5% and 10% level respectively. Statistically different estimates between girls and boys are presented in bold face.

Table A4: Estimates of the Causal Effect of Access to Universal Child Care on Parenting with Sub-Samples defined on Basis of Gender and Maternal Labour Supply

	Mother Doesn't Work		Mother Works	
	Girls (P-Value)	Boys (P-Value)	Girls (P-Value)	Boys (P-Value)
Family Dysfunction Index	0.447	0.591	**-0.541**	**0.451**
	(0.041)**	(0.042)**	**(0.184)**	**(0.003)*** **
Ineffective Parenting	**2.07**	**0.575**	0.34	0.648
	(0.000)* **	**(0.19)**	(0.056)*	(0.032)**
Parent Consistency	**-0.773**	**-0.108**	**-0.396**	**0.136**
	(0.018) **	**(0.63)**	**(0.03)** **	**(0.299)**
Positive Interaction	**-1.168**	**-0.166**	-0.649	-0.578
	(0.000)* **	**(0.326)**	(0.001)***	(0.015)**
Ages 0-3				
Spends 5 minutes of focused time - many times a day	-0.112	-0.056	**-0.111**	**-0.06**
	(0.000)***	(0.077)*	**(0.000)*** **	**(0.255)**
Laughs with child - many times a day	-0.061	-0.059	**-0.089**	**-0.027**
	(0.03)**	(0.015)**	**(0.000)*** **	**(0.095)* **
Does a special activity that the child enjoys - Once or twice a day or more	-0.062	-0.022	-0.049	-0.052
	(0.017)**	(0.339)	(0.276)	(0.01)***
Plays a sport, game, or hobby with child - Once or twice a day or more	-0.004	0.03	0.021	0.024
	(0.897)	(0.288)	(0.294)	(0.316)
Reads to child - daily	-0.126	-0.11	**-0.172**	**-0.048**
	(0.000)***	(0.063)*	**(0.000)*** **	**(0.347)**
Age 4				
Spends 5 minutes of focused time - many times a day	**0.103**	**-0.125**	**0.089**	**-0.051**
	(0.035) **	**(0.5)**	**(0.048)** **	**(0.281)**
Laughs with child - many times a day	-0.152	-0.052	**0.035**	**-0.233**
	(0.035)**	(0.734)	**(0.546)**	**(0.000)*** **
Does a special activity that the child enjoys - Once or twice a day or more	-0.136	0.038	-0.087	-0.106
	(0.07)*	(0.734)	(0.159)	(0.045)**
Reads to child - daily	-0.039	0.007	**0.072**	**-0.099**
	(0.496)	(0.899)	**(0.159)**	**(0.193)**

— Note: For the outcome variable in each row we present the estimates of the policy coefficient δ as specified in Equation (1) (Intention to Treat). We split the sample by mothers who work and those who do not and then also by gender as denoted in the column header. These regressions also include a set of dummies derived from the covariates listed in Table 1 as well as province and cycle indicators. We test the reported coefficients for statistical difference from zero using a two-tailed test and report adjusted p-values (presented in parentheses) corresponding to the estimate in the row above. These p-values make use of a Simes p-value adjustment procedure to account for testing effects on multiple related outcomes. The standard errors underlying the hypothesis tests are also corrected at the province-year level. ***, ** and * indicate significance at the 1%, 5% and 10% level respectively. Statistically different estimates between girls and boys are presented in bold face.

Table 5: Assessment of Potential Bias in the Analysis of Parental Inputs as a Mechanism

	Y_i = MSD Score		Y_i = PPVT Score	
	Policy (β_0)	Interaction (β_1)	Policy (β_0)	Interaction (β_1)
Reading - Times a Month	-1.230	-0.732	-0.809	1.186
	(1.015)	(1.389)	(0.963)	(1.948)
Corr(ν_i,η_i)				
-0.5	-0.186	-0.742	0.161	1.177
-0.4	-0.386	-0.740	-0.022	1.179
-0.3	-0.591	-0.738	-0.211	1.180
-0.2	-0.801	-0.736	-0.406	1.182
-0.1	-1.014	-0.734	-0.605	1.184
0	-1.230	-0.732	-0.809	1.186
0.1	-1.450	-0.730	-1.016	1.188
0.2	-1.674	-0.728	-1.228	1.190
0.3	-1.900	-0.725	-1.443	1.192
0.4	-2.129	-0.723	-1.662	1.194
0.5	-2.361	-0.721	-1.883	1.196

— Note: Applying the adaptation of Altonji, Elder and Taber (2005) outlined in Appendix B of Baker and Milligan (2016) we assess the role of unobserved bias in driving the policy impacts while controlling for parenting inputs. The row labeled "Reading - Times a Month" provides estimates from equation (3A) (standard errors in brackets). The remaining rows estimate the coefficients in equation (3A) while varying the correlation between ν_i and η_i. We present evidence of both age groups using the outcome variables MSD score and PPVT Score as is denoted in the column headers.