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Associations Between Parenting Styles, Teaching Styles, and Inhibitory Control in Children and Adolescents

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Abstract. This exploratory study examined associations between recorded parenting and teaching-style scores and Go/No-Go performance in a workbook containing records labelled as children and adolescents aged 6–16 years. Of 70 records, 58 were retained according to the existing inclusion list. Original data collection, measurement instruments, and ethics and consent documentation could not be independently verified. Ordinary least squares regression with Heteroskedasticity-Consistent Covariance Matrix Estimator 3(HC3) robust standard errors included four environmental dimensions, age, and a mean-centered Parental Control × Teacher Control interaction. The mean recorded No-Go commission error rate was 29.7% (SD = 6.1 percentage points), and internal consistency ranged from α = .563 to .637. Model fit was R^2 = .661 (adjusted R^2 = .621). At the mean of the other control variables, parental control and teacher control had positive conditional associations with commission errors (B = 4.02 and 2.20 percentage points, respectively); parental warmth had a negative adjusted association (B = −2.59). The teacher-support coefficient was nonsignificant (p = .762), as was the interaction (B = −0.92, 95% CI [−2.60, 0.77], p = .279). Exploratory checks using unweighted item means and all 70 summary records yielded similar patterns. These results describe associations within the supplied workbook, rather than independently verified findings about children. The analysis provides a reproducible analytic workflow and identifies documentation and measurement requirements for a prospective study.

Keywords: parenting styles, teaching styles, inhibitory control, Go/No-Go task, exploratory analysis

1. Introduction

1.1. Inhibitory control: concept and significance

In this report, inhibitory control is defined narrowly as a phenomenon in the task context, and not as a broad synonym for self-control. It is the capacity to inhibit a prepotent automatic response, ignore irrelevant stimuli and maintain the application of task rules in goal-directed behaviour. In addition to working memory and cognitive flexibility, Diamond included inhibitory control as one of the underlying processes of executive function (EF) [1]. It is one of the other executive functions that

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slowly emerges throughout the course of childhood and adolescence. In classrooms, it manifests itself in the daily routine of waiting one's turn to speak and in withholding an impulsive response, but also in task switching and delayed gratification, which involve other cognitive processes and situational supports. Such behavioral tasks provide insight into the development of regulation; however, performance on such tasks is not a direct reflection of maturation of the prefrontal cortex (PFC).

A Go/No-Go task is a task that develops a powerful response habit in the go trials, and then requires the participant to inhibit that response during the no-go trials. Measurement properties change with age and with how the task is set up [2] and failure to withhold is manifested as commission errors, but commission errors can also be a result of other factors such as attention, understanding of the task and response strategy. Zhou et al. combined behavioural assessment with functional near infrared spectroscopy in studying inhibitory control in children [3]. The main outcome measure for the recorded-score analysis reported here was the commission error rate of the No-Go; mean reaction time was used as an index of response speed and d′ provided a secondary measure of discrimination sensitivity.

1.2. Parenting style and inhibitory control

Young children are still practising skills to manage their own behaviour. The emotional responsiveness, behavioral guidance and daily boundaries set by family caregivers are therefore significant external supports for regulation. In a prospective study, Bernier, Carlson, and Whipple studied 80 children and measured maternal sensitivity, mind-mindedness, and autonomy support at 12–15 months and executive function at 18 and 26 months [4]. In all three cases, parenting was linked to executive function, and autonomy support continued to predict executive function when controlling for general cognitive ability and maternal education [4]. These findings give an indication that warm responsiveness and granting the necessary autonomy can act in concert to support children in developing self-regulation.

Valcan, Davis, and Pino-Pasternak integrated the findings of various studies and gave them a quantitative context [5]. In 42 studies, parenting behaviors were categorized as positive, negative or cognitively supportive [5]. Their mean correlations with overall executive function were $r = .25$, $r = -.22$, and $r = .20$. The p-values were all less than .001 [5]. Although these small to moderate effects suggest family factors are important to consider, they are not enough to account for individual differences in children's EF alone. Nor do these correlations provide evidence that parenting behaviors are causal of the differences in executive function.

Wu, Zhang, and Wang used matched father-mother-adolescent data from 699 families in China [6]. There were 4 profiles of helicopter-parents identified for the boy families and 3 for the girl families, and affiliation with the profiles was also different for boys and girls in the case of decisional procrastination [6]. They did not show inhibitory performance on Go/No-Go task [6]. These findings provide cultural context for the potential of parental support or warmth to coexist with control for the purpose of the current analysis. Co-occurrence within a family offers structural context but does not replace a statistical test of the product term between parental control and the teacher environment, nor does it offer direct evidence for the behavioral outcome investigated here.

Constructs must be defined and scales must be adapted and documented for cross-cultural measurement. Psychological control, behavioral structure, and supportive involvement should not be confused, in particular. The current analysis treats parental control and parental warmth as separate dimensions, and the range of documentation of these measures is indicated in the Method section.

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1.3. Teaching style and inhibitory control

In the classroom, children are continuously being called upon to stick to the rules, get along with others, and keep their attention. Autonomy-supportive contexts can be distinguished from controlling contexts because the former can nurture internalized regulation through meaningful choices, the reasons behind requirements, and soliciting students' perspectives about why they need to do something, whereas the latter can hinder internalized regulation through pressure and coercion [7]. That framework is why it is important to examine teacher support and control here, albeit with the caveat that the broad teacher-support measure in the dataset is not necessarily indicative of autonomy support.

The evidence that was directly related to teaching style and behavioral executive-function measures was still sparse in the literature reviewed for this paper. Using multilevel models, Sosic-Vasic et al. analyzed 208 students [8]. Lower error-difference scores on the Flanker task were related to teacher-reported autonomy support ($B = -.014$, $p < .05$) [8]. The dots-task coefficient was also negative ($B = -.037$), but was reported only at $p < .10$ and did not meet the conventional .05 threshold [8]. Both reaction-time associations were not significant [8]. These task–specific findings provide impetus for future research and do not create a family–teacher relationship.

1.4. Research gap and contribution of the present study

Research studies on parenting and EF have been conducted longitudinally and through meta-analysis. But the studies reviewed here provide weak direct evidence for associations between family and classroom environments and inhibitory control. The ecological approach to development prioritizes development within interrelated contexts [9]. In this study, the question is whether there is an association between parental control and inhibitory performance that varies with teacher control. By estimating a "product term", this possibility can be investigated in addition to the separate relationships of the environmental variables.

In the present report, an ordinary regression model is used and applied to an existing workbook with scores named parental control, parental warmth, teacher support, teacher control, and Go/No-Go commission errors. The regression test assesses whether there is a different slope relating parental control to recorded commission errors for different levels of teacher control. Contribution is restricted to the reproducible analysis of the recorded values and evaluation of their evidential boundaries as well as planning of future validation. It does not assert that the numbers were obtained as a result of independent human-participant testing. The results of previous studies related to the proposed research question are summarized in Table 1.

Table 1. Primary evidence directly relevant to research question

Study	Design/sample	Key quantitative information	Relevance and evidential boundary
Bernier et al. [4]	Prospective; N = 80	Autonomy support was the strongest family predictor of executive function at 18 and 26 months	Supports the family as a proximal regulatory context; participants were much younger than in the present study
Valcan et al. [5]	Meta-analysis; 42 studies	Positive r = .25; negative r = −.22; cognitive support r = .20	Provides a quantitative benchmark; most participants were 0–8 years old

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Table 1. (continued)

Sosic-Vasic et al. [8]	Cross-sectional multilevel analysis; N = 208	Flanker: B = −.014, p < .05; dots: B = −.037, marked at p < .10	Task-specific evidence; the dots result was not marked significant at .05; no family–teacher interaction tested
Wu et al. [6]	Profiles in Chinese families; 699 families	4 profiles among families of boys and 3 among families of girls	Cultural and measurement context; not an inhibitory-control study
Present analysis	Existing workbook; 58 retained records	Family and teacher variables in one model; Control × Control interaction tested	Recorded task summaries; collection provenance and original measures unverified

2. Study design and method

2.1. Research questions and hypotheses

This is an exploratory report for an existing workbook that the researcher describes as a cross-sectional, nonexperimental data set. The model consists of four recorded environmental dimensions, age, and a Parental Control × Teacher Control product term. The directionality expectations explored in the recorded scores are listed in Table 2 and not as preregistered confirmatory tests. Sex was not included as a covariate due to the inability to reconcile the corrected aggregate counts with the source records at ID level.

Table 2. Testing research hypotheses and statistical decision criteria

ID	Statement	Decision rule
H1	Parental control is expected to relate positively to the No-Go commission error rate	Conditional coefficient positive
H2	Parental warmth and teacher support are expected to relate negatively to the No-Go commission error rate	Both coefficients negative
H3	Teacher control is expected to relate positively to the No-Go commission error rate	Conditional coefficient positive
H4	The parental-control slope depends on the level of teacher control	Mean-centered product term statistically significant

2.2. Sample and evidence status

In total, there were 70 different student IDs in the workbook. Twelve records were flagged as incomplete and dropped, resulting in 58 records for analysis (82.9%), which all matched the analytic-sample worksheet, and all 70 records actually had complete core summary fields. Later, the researcher blamed the 12 exclusions on "poor task involvement" with apparently random answers and accidental back navigation to a previous task page. There are no per-reason counts or operational criteria or contemporaneous case-level observations to verify this against. Ages in the

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retained records spanned 6–16 years (M = 10.76, SD = 3.00). Additionally, the researcher provided the total counts of boys (43, 74.1%) and girls (15, 25.9%), which were not matched up with individual registration data and thus sex was not included in the model.

The origin of these records is not known. In a supplementary account dated 16 September 2026, the researcher reported convenience recruitment of volunteers from one chess club, with students attending different schools and classes. Oral consent from parents and students was reported with contemporaneous consent documents and any ethics approval or exemption not found; no claim of ethics clearance is made here. Fifty-eight entries were specifically noted as being simulated data for software testing (with no human subject involvement); these were not included in the analyses reported here. The newly drafted questionnaire FTI-D01 was in the same plight as the original questionnaire, as it was unable to ascertain which instrument was actually administered.

2.3. Measures

The measures, variables and availability of the corresponding data are summarised in Table 3.

Table 3. Availability of measures, variables and data

Measure	Dimension/indicator	Role in the study	Current data status
Parenting-style questionnaire	Parental control and parental warmth; 6 items each; scores 1–5	Recorded dimension scores	Included; no formal source, item wording, or scoring key available
Teaching-style questionnaire	Teacher support and teacher control; 6 items each; scores 1–5	Recorded dimension scores	Included; student report known only retrospectively; original items unavailable
Raven reasoning test	General reasoning ability	Planned covariate	No score in the workbook, so no adjustment can be claimed
Go/No-Go task	No-Go commission error rate, mean reaction time, and d′	Primary and supplementary behavioral outcomes	Task parameters and trial-level logs both unavailable

2.3.1. Parenting-style questionnaire

Parental control and parental warmth were measured with six items each, which were all coded with integers (1–5), with scores on the dimensions being recorded on the same 1–5 scale. Means and recorded scores were highly correlated ($r > .99$). However, 55 of the 58 parental-control scores and 54 of the 58 parental-warmth scores were different from the rounded-off item means. The absolute differences ranged from .035 to .044 points when averaged over the four dimensions, which is too big to account for by rounding alone. Scale sources, item wording, scoring keys, and validity evidence were not found. The sensitivity analysis involved an exploratory analysis that treated the item means as equal, using the unweighted scores recorded for the items in the test, and not using any reverse scoring.

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2.3.2. Teaching-style questionnaire

Teacher support and teacher control were also structured in the same way (six items each) with dimension scores ranging from 1 to 5. The family questionnaire was completed by the students in the researcher's supplementary account, as well as the teacher ratings, which were directed toward the school homeroom teacher, not the chess coach, and the students also completed these. However, that was a retrospect and administered forms were not found. Because original item wording is unavailable, the teacher-support score cannot be identified specifically as emotional warmth or autonomy support and was retained under its original label.

2.3.3. Raven reasoning test

The Raven scores were planned as a covariate and the supplementary file containing the scores for each participant, which was supposed to be available, was never available. Hence, no role is attributed to the performance on the Raven and no correction for general reasoning ability is claimed.

2.3.4. Go/No-Go task

The data set included No-Go commission error rates, mean reaction times and d′ values per participant, and the main outcome was the commission error rates of the No-Go condition. The typical approach to Go/No-Go is that Go stimuli elicit a response while No-Go stimuli elicit a non-response. The items that were not recoverable were the study-specific details: the stimuli, the duration of presentation of the stimuli, the practice criteria, the number of trials, the Go/No-Go ratio, and the implementation of the scoring. The task can thus not be reproduced from the materials available. The researcher recalled pressing the space bar to answer the question and that there were "20 items" which may refer to practice trials, one block or the entire task. Of the 58 recorded commission error rates, 54 cannot be reproduced to three decimal places as a single ratio of integer error counts to an integer No-Go trial count of no more than 20. That is merely a check that does not lock down the actual denominator or where the error is occurring.

2.4. Procedure

Analyses proceeded with the scores from the dimensions on the questionnaires and summaries of the participants on the task and the original inclusion list of 58 cases. The testing was reported in the account that was submitted on 16 September 2026, and was conducted at the club throughout three days and three batches, arranged by Liu Yi with assistance from an administrator teacher. According to that, students read and filled out the paper questionnaires independently, while all task testing was conducted on the same computer. Original data collection cannot be independently verified by these retrospective descriptions of dates, batch sizes, the questionnaire–task order, the device specifications, the setting of the software, the rest intervals or quality control logs from the time. Nor could the original 'incomplete' exclusion label be compared with the later report of inadequate task engagement, as no contemporaneous case records are available.

2.5. Data processing and analysis

Table 4 demonstrates the entire analysis process from data verification to descriptive statistics and correlations to robust regression and the interaction test.

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Table 4. The data were processed and analyzed statistically

Step	Method	Analytic convention
Data verification	Check sample size (N), exclusion flags, student-ID consistency, and missingness in the analytic sample	Analyze 58 complete records; exclude the 12 records flagged as incomplete
Scale quality	Cronbach's α, together with alignment between item means and summary scores	Interpret cautiously when $\alpha < .70$
Descriptive statistics	Means (M), standard deviations (SD), medians, ranges, and sex counts	N = 58
Correlation analysis	Pearson r with two-sided p values	Environmental variables in relation to the primary outcome
Robust regression	OLS with HC3 robust SEs and 95% confidence intervals	Adjust for age; sex used only in the aggregate description
Interaction test	Mean-center the continuous variables; form the Parental Control × Teacher Control product	Product-term p value and confidence interval determine the conclusion
Supplementary measure	Bivariate correlations of d′ with the environmental variables	Does not replace the primary-outcome model

The regression model used was $Y = \beta_0 + \beta_1 PC + \beta_2 PW + \beta_3 TS + \beta_4 TC + \beta_5 Age + \beta_6 (PC \times TC) + \varepsilon$, with Y representing the commission error rate with No-Go, PC and PW representing parental control and parental warmth, respectively, TS and TC representing teacher support and teacher control, respectively, and Age representing age. All continuous predictors were mean-centred prior to building the product term. Inference was based on HC3 heteroskedasticity-consistent standard errors [10] and two-sided tests at $\alpha = .05$, and 95% confidence intervals. Unstandardized coefficients are reported in percentage points. The model only captures association within the sample and is not causal. The primary HC3 tests and intervals were based on Student's t distribution with 51 residual degrees of freedom. For the exploratory post hoc checks, unweighted item means were used, the recorded score model was refit (with all 70 summary records) and a reaction-time–commission-error correlation was examined. The all-record analysis did not determine eligibility of the excluded samples and assessed sensitivity to exclusions.

3. Data analysis and results

3.1. Data quality and internal consistency

The 58 kept records were matched to participant ID in the analytic-sample sheet and the core analytic variables were 100% complete. Cronbach's α of .563 to .637 on the four six-item dimensions (Table 5). All four alphas were lower than the usual .70 benchmark used as a cutoff. Effect sizes and confidence intervals were also read in conjunction with the significance tests given that measurement error could occur for both the main associations and the interaction estimate.

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Table 5. Internal consistency and alignment of summary scores

Dimension	Items	Cronbach's α	Item mean–summary score r
Parental control	6	0.596	0.998
Parental warmth	6	0.563	0.998
Teacher support	6	0.603	0.998
Teacher control	6	0.637	0.999

3.2. Descriptive statistics

Table 6. Description of the 58 retained records

Variable	M	SD	Median	Range
Age (years)	10.76	3.00	11.00	6–16
Parental control	3.26	0.81	3.24	1.20–4.93
Parental warmth	3.67	0.76	3.76	1.78–5.00
Teacher support	3.65	0.82	3.66	1.94–5.00
Teacher control	3.17	0.89	3.23	1.00–5.00
No-Go commission error rate	0.297	0.061	0.295	0.113–0.441
Mean reaction time (ms)	533.0	46.2	537.9	402.6–644.7
d′	1.77	0.35	1.77	1.06–2.46

These 58 records are summarized in Table 6. The workbook gives a mean No-Go commission error rate of .297 (29.7%), a mean reaction time of 533.0 ms, and a mean d′ of 1.77. All these are summaries of workbooks and the method of obtaining the summaries from the original responses has not been checked. There were no trial-level records to verify the number of trials, the percentage of trials omitted, the distribution of the reaction times or the percentage of extreme values excluded; the medians were calculated from the decimal values recorded with conventional decimal rounding.

3.3. Correlation analysis

Table 7. Pearson correlations of study variables with the No-Go commission error rate

Variable	r	p
Parental control	0.616	< .001
Parental warmth	−0.461	< .001
Teacher support	−0.127	0.341
Teacher control	0.511	< .001
Age	−0.030	0.825
d′	−0.496	< .001

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The correlations between the recorded variables are presented in Table 7. Parental control was the most positively correlated with commission errors (r = .616), followed by teacher control (r = .511) on the positive side, and parental warmth (r = −.461) on the negative side. However, teacher support and age were never found to be statistically significant. Commission errors correlated negatively with d′ (r = −.496, p < .001). As should be emphasized, none of these numerical relationships provides an independent verification of the actual task responses and the validity of the variable labels.

3.4. Robust regression analysis

The four environmental dimensions, the age, and the Parental Control × Teacher Control term were entered into the regression (Table 8). It yielded R^2 = .661 and adjusted R^2 = .621. This sort of fit relates to the sample in hand, not causal contributions or predictive power of the model for the next sample. HC3 standard errors were used for all coefficient tests.

Table 8. OLS regression predicting the No-Go commission error rate with HC3 robust standard errors (N = 58)

Predictor	B (pp)	SE (HC3)	t	p	95% CI (pp)
Intercept	29.84	0.53	56.82	< .001	[28.79, 30.90]
Parental control	4.02	0.68	5.88	< .001	[2.65, 5.39]
Parental warmth	−2.59	0.70	−3.67	< .001	[−4.00, −1.17]
Teacher support	−0.19	0.62	−0.30	0.762	[−1.43, 1.05]
Teacher control	2.20	0.72	3.05	0.004	[0.75, 3.65]
Age	0.05	0.20	0.22	0.824	[−0.36, 0.45]
Parental Control × Teacher Control	−0.92	0.84	−1.09	0.279	[−2.60, 0.77]

Note: pp = percentage points. All continuous predictors were centered. The conditional parental-control coefficient is based on the mean teacher-control score and the conditional teacher-control coefficient is based on the mean parental-control score. Sex was not included because the aggregate counts could not be reconciled with participant-level records. The intercept is the prediction when all the centred predictors are null.

At the mean teacher-control score, a one-point increase in parental control was associated with a 4.02 percentage-point higher recorded commission error rate (p < .001). For every one-point increase in the parental-warmth score, the commission error rate was 2.59 percentage points lower (p < .001). Every point on the teacher-control scale represented a 2.20 percentage-point increase in the predicted commission error rate with parental control held constant at its sample mean (p = .004). The estimates are adjusted for the other variables in the model. The adjusted coefficient for teacher support was not significantly different from 0 (B = −0.19, 95% CI [−1.43, 1.05], p = .762). The age coefficient also did not reach statistical significance (B = 0.05, 95% CI [−0.36, 0.45], p = .824). Environmental coefficients and focal interaction are shown in Figure 1 with 95% confidence intervals (grey markers indicate p ≥ .05).

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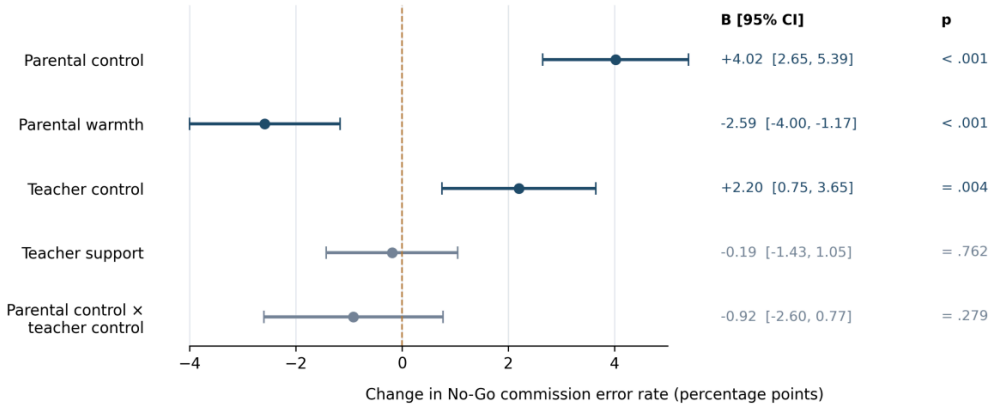

Figure 1. Environmental coefficients and the focal interaction with 95% confidence intervals

3.5. Test of the focal interaction

The Parental Control × Teacher Control coefficient was −0.92 percentage points (HC3 SE = 0.84, t = −1.09, p = .279, 95% CI [−2.60, 0.77]). The estimated parental-control slope was 0.92 percentage points lower per one-unit increase in teacher control. However, the confidence interval encompasses zero, and H4 is not supported. A negative point estimate does NOT imply a buffering effect.

Figure 2 shows the model's prediction at one standard deviation above and below the mean of teacher control (±1 SD), with the means of teacher support and parental warmth held constant at their recorded value. The shaded bands give pointwise 95% confidence intervals for the mean predictions. The estimated product term, as well as its confidence interval, form the basis for any inference to be made concerning the interaction. The estimate is not very precise, with 58 records and 7 estimated parameters. These intervals assume that the statistical model is correct and do not reflect the uncertainty in collection provenance or score construction.

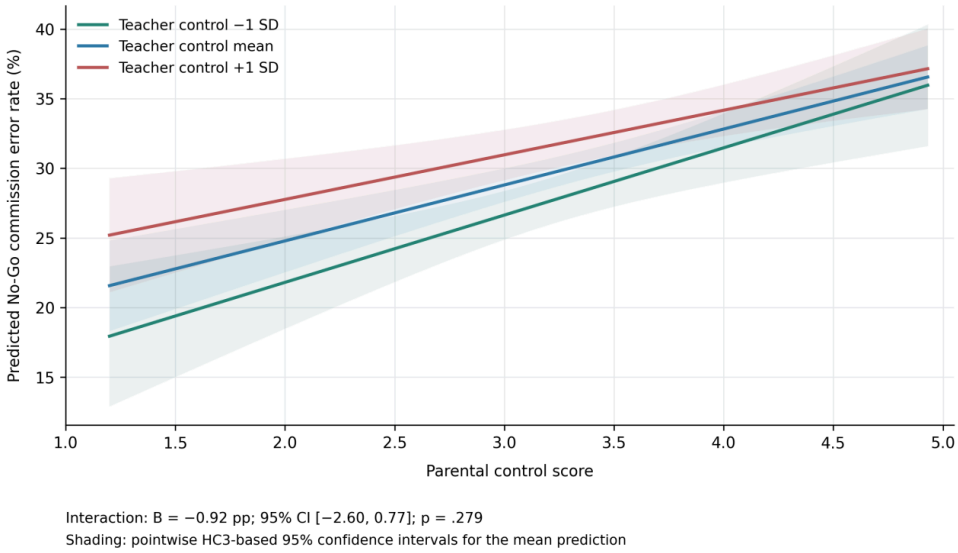

Figure 2. Three teacher-control levels of predicted commission errors at each parental-control level

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3.6. Supplementary measure: d′

Table 9. Pearson correlations of the environmental variables with d′ as a supplement

Variable	r with d′	p
Parental control	−0.598	< .001
Parental warmth	0.313	0.017
Teacher support	0.319	0.015
Teacher control	−0.302	0.021
Age	0.083	0.536

Table 9 provides additional correlations with d′: parental control r = −.598, parental warmth r = .313, teacher support r = .319, teacher control r = −.302, and age r = .083. The four environmental correlations met the unadjusted .05 threshold; however, age did not. The recorded teacher-support score showed a positive correlation with d′, and a small and non-significant correlation with commission errors. These bivariate tests were not adjusted for covariates or multiple comparisons, and should not be used in lieu of the primary model. The difference in significance between the two correlations is not itself a test of whether the two correlations differ in magnitude. It means that the original responses of each trial, and evidence that the measures measure what they are meant to measure, are needed.

3.7. Exploratory sensitivity analyses

The pattern of associations using unweighted item means was similar to that obtained using the recorded dimension scores (R^2 = .664). The interaction remained uncertain (B = −0.87 percentage points, 95% CI [−2.55, 0.82], p = .306). Including all 70 recorded summary cases also yielded an uncertain interaction (B = −0.58, 95% CI [−2.32, 1.15], p = .502; R^2 = .601). Parental control and teacher control had positive conditional associations in both checks, and parental warmth had negative conditional association, while teacher-support coefficient was statistically nonsignificant. These post hoc checks are not used to validate the original scoring/exclusion decisions.

Correlation between recorded mean reaction time and commission errors was positive (r = .477, p < .001). More errors were related to longer rather than shorter mean reaction time at the level of the summary records. The values of the aggregates cannot set a response strategy at the trial level, nor does the numerical relation between the values and the reactions recorded demonstrate that the reactions were produced during human testing.

3.8. Summary of patterns in the recorded scores

Table 10 shows the patterns in the workbook as determined by the model. The decisions are not in themselves verifying of the proposed relationships in children.

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Table 10. The statistical distribution of the recorded scores

ID	Expected pattern	Within-dataset result	Statistical basis
H1	Parental control → higher commission error rate	Expected sign; $p < .05$	B = +4.02 pp, $p < .001$
H2	Parental warmth/teacher support → lower commission error rate	Mixed pattern	Parental-warmth coefficient significant; teacher-support coefficient nonsignificant
H3	Teacher control → higher commission error rate	Expected sign; $p < .05$	B = +2.20 pp, $p = .004$
H4	Parental Control × Teacher Control	No statistical support	B = −0.92 pp, $p = .279$

4. Discussion

4.1. Main findings and interpretation

Parental control and teacher control showed positive conditional associations with commission errors, while parental warmth showed negative associations with the errors, in the provided workbook. Directions are similar in nature to those modes of relations described in the literature for parents [4, 5]. But a trend similar to the one published by others does not prove the source of these data or replication. The calculations of the present results show that the values recorded and the interpretation of relationships in children depend upon the source and verification of measurements.

Teacher support was not statistically significant with the commission errors, but the unadjusted correlation with d′ was positive and significant. Although the two indices reflect different aspects of task performance (the commission errors pertain to No-Go trials, and d′ considers both hits and false alarms), in the absence of the original responses and verified scoring, this difference cannot be translated into an explanation at the level of attention, motivation, or any specific inhibitory mechanism. Such questions remain for a future study.

Nor do the significant conditional associations of parental and teacher control add up to an interaction. The range for the product term is negative to positive, and it is unclear whether the parental-control slope varies with teacher-control levels. This result provides no clear evidence of buffering or risk amplification, but it does not establish the absence of an interaction.

Care must be taken when comparing the teacher's findings with prior work. Sosic-Vasic et al. reported their Flanker association at $p < .05$ and the dots-task coefficient was only significant at $p < .10$ marker [8]. The adjusted teacher-support coefficient failed to attain significance in the present data, but the unadjusted d′ correlation did. There is considerable variation among these studies in what was measured, who provided the ratings, for which tasks and age groups the studies were conducted, and which models were fit, making a direct comparison difficult. Instead, Wu et al. investigated parenting profiles and decisional procrastination, providing cultural context but not a direct test of the interaction studied in the present research [6].

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4.2. Methodological implications

The analysis does reveal some directions that are methodological. In this way, the joint model maintains the conditional coefficients and product-term interactions separately, and alternative forms of scores and inclusion rules can be studied openly. Computational consistency is achieved by getting the coefficients to reproduce from a workbook, but this is not evidence of construct validity or collection provenance. While the study question is still theoretically motivated, this report does not, as such, justify the measures and the suggested developmental relationships.

4.3. Limits on educational interpretation

The present analysis is not sufficient, as an evidential basis, to influence educational practice, to identify children who should be targeted for intervention, or to evaluate individual parents or teachers. The coefficients reported here are not the "safer ground" for recommendations about supportive family and classroom environments.

An additional study in the future might be to use a behavioral task and then have specific family and classroom reports, using this as a complement to the behavioral task. Validated constructs, reliable scoring, documented testing, age-appropriate administration all need to be taken into account as to whether such measures would be interpretable. None of these is present in the present scale coefficients and the recorded task scores.

4.4. Limitations of validity and evidence

First, there is the limitation dealing with traceability. The worksheet has been provided for recalculation of the statistics, but the questionnaires given, the responses at the trial, the contemporaneous incident reports and ethics clearance/exemption documents were not available. The researcher's personal recollection of the testing and oral consent provides valuable background, but background is no substitute for contemporaneous records or independent verification. The separate CSV of simulated data is not evidence of the original administration of the data, nor is the questionnaire drafted after the study evidence of anything here. The weaknesses in the gaps work both ways; they make it less easy to trace the evidence of the study, and, with it, less easy to trace the measure's validity, because the validity of the linking of the scores to the administration has never been verified independently. All statistical figures in this report should therefore be viewed as an exploratory pattern among the existing records.

The second concern is with construct validity and score reliability. Original wording of items, scale sources, response anchors and scoring keys have not been verified. The "control" scores might be psychological control, behavioral monitoring or rule setting; it is impossible to identify, and "teacher support" is not necessarily autonomy support or emotional warmth. Available coding revealed a range of internal consistency from $\alpha = .563$ to .637, and the difference between dimension scores and the mean of the items was not due to rounding error. Alpha alone does not provide proof of either the validity or invalidity of the construct. The researcher's supplementary account of both questionnaires was filled out by the same students, thus the questionnaires could have similar response tendencies; and reading comprehension was not measured at all in this age range (6–16). These problems, combined, increase the uncertainty regarding the meaning of the scores and the estimated association themselves.

The third issue is related to the behavioral indicators, which have not been validated. The researcher's memory of "20 items" does not correspond with the number of commission error rates

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most commonly recorded from a simple count-based reading of these data, rounded to three decimal places: 54 out of 58 records do not fit with the assumption of a single integer count ratio and a denominator of no more than 20. A check of this conditional kind establishes neither the actual trial count nor where the discrepancy comes from. In the absence of original responses, omission data, reaction-time data filtering rules and the correction used to calculate d′ make it difficult to distinguish inhibitory failures from fluctuations in attention, comprehension of the task or response strategies. The associations, moreover, do not qualify as an independent replication, because d′ also folds in false alarms. Good model fit or consistent sensitivity analyses cannot resolve questions relating to the source of these indicators, nor about their meaning.

Internal validity and causal interpretation is the fourth. Even if the original collection were subsequently substantiated, cross-sectional data cannot establish temporal order. Reverse causation remains possible: adults may have responded with more controlling behaviors to children's regulatory issues. General reasoning ability, and other potential confounds, were not accounted for by the model, sex was not included as a covariate and age was only entered as a linear term. Nor was there an estimate made of dependence within schools, classes or testing batches; HC3 standard errors make no attempt to account for such dependence. Thus, these correlations are far from demonstrating that controlling behavior has a detrimental effect on inhibitory control.

The fifth is the accuracy and reliability of the statistical conclusions. No a priori sample-size justification was available for the 58 records, and 6 predictor terms plus an intercept were estimated. The interaction confidence interval included zero, and there was still room for the interaction to act in either direction, so that the lack of significance cannot be attributed to low power alone. There was no adjustment for the multiple correlation tests and sensitivity analyses were conducted post hoc and R^2 only indicates the within-sample fit. The intervals reported, however, do not account for the uncertainty associated with unverified provenance or scoring rules and therefore the apparent precision of the reported intervals overstates the reliability of the evidence reported in this study.

Finally, external validity and sample selection are limited. Convenience sampling at one chess club meant that the researchers could not generalize their findings to other children and adolescents, and their study did not test chess training itself. The 43 males and 15 females were the aggregate from the researcher and there was no record to check on each of them individually. There is no case-level behaviors or operational criteria behind this reason and the fit with the existing "incomplete data" label is unverified, which is the following: "Insufficient task engagement" (offered as a reason for exclusion, after the fact). In short, selection bias is not to be excluded. A larger sample may provide greater precision and coverage, although this would not correct the problems with the source evidence.

4.5. Future directions and improvements

It is to recover and reconcile the evidence underlying the existing records that is the first. Anonymous IDs should be able to link the questionnaires administered, the scoring keys, the exports of the tasks created, the settings of the program, the registration of the participants and the exclusion records—materials used at the time of administration and recorded should not be confused with those that are retrospective. There are three aspects that must be pinned down before any re-calculation from original responses: the commission-error denominator, the manner in which scores accumulate across blocks, and the d′ algorithm. There are two cautions that ensue from this. No gaps in the case-level data shall be made up by "inventing" cases, and no synthetic data or new questionnaires shall be presented as data from the previous study. If the essential materials are

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indeed unrecoverable, the honest stance to take is the one taken in this report: exploratory analysis of recorded materials, with the research question left to the new documented collection.

All new collections would need to begin with the paperwork: the institution with responsibility for the collection identifying any ethics and consent requirements, determining whether they are required or exempt, and securing informed guardian consent and age-appropriate child assent. In terms of measurement, psychological control, behavioral structure, autonomy support, and emotional warmth should be conceptually separate, with instruments having supporting evidence and appropriate for the targeted ages. First, new items would be subjected to expert content review, short interviews to see how kids responded to the questions, and a pilot study. Then the scoring procedures would be written out and assessed with score reliability and any evidence for the proposed factor structure. Especially for younger children, there should be frequent checks for understanding, and standard cues should be provided so that the meaning of an item is not subtly changed by an undocumented explanation. One line must remain set: Approval and validation for a new study can not be given to the previous one retroactively.

Preregistration should also occur prior to formal collection, which includes the primary outcome, the Control × Control interaction, the required covariates, missing-data protocol, and exclusion protocol. Sample size requires a justification based on a substantively meaningful effect size or desired confidence interval width (accounting for the measurement reliability, attrition or unusable records, and school or class clustering) and not on the within-sample R^2 of the current analysis or significance. Confirmatory and exploratory tests would then be separated, and multiple comparisons and sensitivity analyses would be pre-planned. What would not help is to keep adding interactions and/or covariates to the current small data set to see if they were significant.

An archived task script and version are the starting points for the testing itself and fixed stimuli, response windows, practice criteria, Go/No-Go proportions and documented actual trial counts. They would be provided with standard equipment and instructions, and events would be documented at the time rather than post-hoc. Trial numbers, stimulus types, responses, accuracy, reaction times, and timestamps should be maintained for every session and anonymous identifiers should be used for linking questionnaires, registration of participants and results of the tasks. Checks for quality of the responses would then focus on pre-specified conditions like comprehension, omissions, anticipatory responses, interruptions, etc., rather than just dropping students because of a high commission error rate. Corrections ought to keep their supporting evidence and change history. New observations must retain their actual collection dates and must not be presented as part of the original dataset.

Once the evidence was traceable and the measures usable, broader recruitment would make sense, involve more schools or organisations and having a better age/sex balance. Where the data and sample structure allow, school, class, teacher and testing-batch identifiers should be retained, with multilevel models or appropriate cluster adjustments being used when necessary. Incorporating parent, student, and teacher reports, along with classroom observations would reveal whether or not there was agreement among information sources, while repeated or longitudinal measurement would allow for the tracking of change and temporal order. Nevertheless, confounding must be addressed, and none of these designs is automatic and confers causality. The use of neuroimaging (or any other measure used to address a well-specified mechanistic question) should be deferred until reliable behavioral measurement has been established in an independent sample.

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5. Conclusion

The analysis summarized associations within 58 retained workbook records. Parental control and teacher control showed statistically significant positive conditional associations with commission errors, whereas parental warmth showed a statistically significant negative adjusted association. The teacher-support coefficient was nonsignificant, and the interaction estimate remained uncertain (B = −0.92 percentage points, 95% CI [−2.60, 0.77], p = .279). Because the original collection and measurement evidence remains unresolved, these results are not independently verified empirical findings about children. The report provides an exploratory analytic workflow and identifies the evidence required for a documented prospective study.

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