

Recognizing dyslexia: school experience, recorded care and family circumstances in Denmark

Bolero

Abstract

Administrative recognition of a reading difficulty can change what schools, families and healthcare services do, as well as how children understand their experiences. Those responses need not move together. We study retained national dyslexia assessments among Danish children aged 9–17 and their subsequent school reports, attendance, assessed achievement, care, family circumstances and age-eligible educational and material transitions. A documented launch-period classification rule motivates an individual score-margin comparison for pupils assessed in spring 2015. Descriptions cover retained assessments in 2015–2024. Initial-assessment retention, grade correspondence, score precision, local sorting and recorded classification are assessed before a causal interpretation is allowed. The empirical scope is *[pending]*, and the historical assignment judgment is *[pending]*. Separately registered outcomes preserve the distinction between experienced symptoms and recorded care. The resulting symptom/care pattern is *[pending]*; the family and early-adult patterns are *[pending]* and *[pending]*. All empirical fields remain unfilled in this pre-data manuscript. The design can identify a local classification-policy contrast only when its institutional, measurement and continuity conditions hold. It cannot identify the inherent effect of dyslexia, a particular support dose or the share of a healthcare response attributable to detection.

Keywords: Dyslexia recognition; regression discontinuity; school experience; child health; family outcomes.

PRE-DATA DRAFT: empirical placeholders are not findings.
USER-FROZEN DRAFT: packaged at user direction, not independently scientifically accepted. No further design iterations were run.

Introduction

Being told that a persistent reading difficulty has a name may matter well beyond a test score. Recognition can change access to teaching and assessment, reduce uncertainty within a family, and prompt a child or parent to seek help. It can also

change expectations, expose a child to stigma, or alter how difficulties are reported. Educational achievement is therefore an important but incomplete account of what classification does. A child can participate more successfully in assessment while continuing to experience loneliness or headaches. A rise in healthcare contact can reflect a welcome recognition of unmet needs, a deterioration in health, or both.

Denmark’s national Ordblindetesten offers a setting in which these distinctions can be studied with administrative and school records. It is an individual assessment used when dyslexia is suspected, rather than an entrance examination or universal school-entry screen. The Ministry introduced it in February 2015. Its administrative classification boundaries apply to a phonological test score, not to a natural dividing line between people with and without all reading difficulties. The test-development study emphasizes that choosing a diagnostic threshold is partly an institutional decision. [Poulsen, Juul and Elbro \(2023\)](#) The [2026 correction](#) revises Table3 sensitivity/specificity calculations; no such numerical accuracy claim is used to establish this study’s historical assignment rule or causal assumptions.

The central question is how recognition relates to school experience, recorded care, educational opportunity and family/material circumstances, and where the documented classification margin permits a local causal interpretation. The child and the family are both substantive units of interest. Parents’ care and resources and siblings’ school experience are not merely controls for the child’s achievement. Later participation, earnings and living arrangements are examined together where calendar and age permit; employment is one dimension of inclusion.

This question builds on substantial existing Danish work. Nielsen and Ibsen already examine a national-test threshold for school performance, wellbeing, absence and educational continuation. Their updated conference abstract includes completion and age-20 NEET. We treat the corresponding school and transition outcomes as benchmarks, rather than claim another threshold estimate is inherently new. Their earlier study also had information on pretest support and membership that is not assumed in this delivery. [Ibsen thesis, chapter 4](#); [ESPE 2026 abstract 440](#)

Kraka’s national report supplies close descriptive child-health, parental-work and sibling comparisons. Danish FOCA research connects self-reported reading/writing difficulties with self-rated health, while a wider scoping review documents many psychosocial pathways. These precedents make the general association between dyslexia and disadvantage familiar territory. Our potential contribution is the specific classification-policy contrast for separately recorded symptom, service and family outcomes, together with an explicit account of their observation processes. The updated threshold study’s full outcome inventory remains unavailable, so priority is provisional. If identification fails, the contribution becomes transparent replication and ascertainment knowledge rather than a newly discovered causal association. [Kraka Economics \(2024\)](#); [Kjeldsen et al. \(2019\)](#); [Wilmot et al. \(2023\)](#)

Recognition and its possible consequences

A useful distinction is between the reading difficulty, administrative recognition, eligibility for assistance and assistance actually received. A cutoff comparison potentially

changes recognition and the responses organized around it. It does not randomly create dyslexia, and a red category does not document that a particular program was delivered. Schools and providers exercise judgment, other assessments exist, and support may occur on either side of the national threshold. The policy contrast includes downstream information, referrals and institutional responses that crossing the rule generates.

Three competing accounts guide interpretation. An explanatory-relief account predicts that a credible explanation and more suitable school responses can improve reported belonging and reduce distress. An observation account predicts that recognition changes assistance in answering questionnaires, the meaning attached to symptoms, or contact with clinicians even when underlying health changes little. A mixed-response account allows educational opportunities to improve while symptoms, care or family resource demands move differently. These accounts generate informative comparisons, but none can be identified solely from agreement or disagreement of coefficients.

The family provides additional pathways. Shared developmental liability and resources can explain similarities between children and parents without a spillover from classification. At the same time, recognizing a child’s difficulty can mobilize advocacy or lead a parent to recognize their own difficulty. Changes in parental paid hours therefore cannot be equated with the time cost of caring for the child. Evidence from a selective Finnish follow-up cautions against an inevitable persistent mental-health penalty from childhood reading difficulty alone. Recent Chinese longitudinal questionnaire work also examines parent-child psychological pathways, but its mediation model does not identify the effect of Danish classification. [Aro, Özbek and Torppa \(2024\); 2026 family-pathway study](#)

Educational transitions create a further distinction between current resources and future possibilities. Participation in education may reduce current employee earnings without reducing lifetime welfare. Co-residence can reflect support, preferences, housing costs or constraints. We therefore retain resources and living arrangements as outcomes in their own right and avoid assigning them an automatic favorable direction. An ordinary regression of health or resources on treatment-induced education would condition on an outcome of recognition; it would not decompose a causal mechanism.

Institutions, records and study populations

The analysis uses only FSV tables allowed by the explicit planning boundary. The full schema was searched, followed by narrow retrieval of test, school, health, population, family, education and income documentation. This established candidate constructs and known recording breaks, not mounted data, sample size, missingness, score variation or overlap. No confidential records were accessed to prepare the manuscript.

Our descriptive cohort consists of children aged 9–17 with a retained national-test record from February 2015 through December 2024. We use the earliest retained date per person and describe red, yellow, green and unknown categories separately. This is a description of retained assessments, not dyslexia prevalence or the probability of gaining access to assessment among all children. A person may previously have

been assessed through another instrument, and a retained date may not be the first national test.

For the provisional causal cohort, we use ordinary public-school pupils in grades 4–9 assessed from March through June 2015. An original ministry note describing the launch period specifies upper red score boundaries of 66, 70, 73, 77, 79 and 81 for those grades, with equality assigned to red. Current guidance includes seasonal rules and is not backdated. This narrow cohort is chosen for historical evidentiary support, not favorable precision; it may prove too limited for the local design. Actual assessment grade must be documented in the test delivery and checked against the preceding month’s school record. Age or later educational attainment cannot substitute for grade at test. [Ministry launch-period documentation, p.1](#)

Retention is a central threat. STIL’s warehouse documentation describes keeping an approved result and sometimes replacing a third-grade test with a subsequent assessment. It also distinguishes the national test from other dyslexia assessments. The FSV delivery lists a score but lacks explicit approval, retest and test-version fields. The warehouse process cannot simply be presumed to be the FSV process. We must establish compatible first-assessment provenance, including prior third-grade tests, before interpreting the score comparison causally. [STIL documentation, pp.2–3](#)

All follow-up is capped at December 2024. The first complete school year after assessment defines attendance and questionnaire follow-up for baseline grades 4–7. Reading is assessed at the predetermined next scheduled grade-6 or grade-8 national test; Danish teacher grades are assessed in the scheduled grade-9 year for baseline grades 4–8. These clocks avoid selecting a later successful or observed assessment. Care follows the child for 730 days, ending observation of domestic recorded events at death or emigration. Material family measures use the calendar year after assessment. Educational participation is measured at 18, and completion, employee participation, income, resources and co-residence at 20 or its calendar year. Eligibility depends on birth date and the observation cap, never realized graduation or employment.

Parents and siblings are identified from the last annual population snapshot before assessment. Both recorded parent roles are retained, including nonresident parents; missing links do not establish an absence of parents. Siblinghood requires a shared nonmissing parent. Family analyses select the first eligible assessed child in a baseline family component, with an outcome-independent tie rule. Later assessments of relatives are responses, not exclusions or adjustment variables. Parent and sibling outcomes are averaged within the fixed roster before comparing index children, preserving a child-weighted family estimand.

Outcomes and measurement

Four school-report outcomes are kept separate: frequent loneliness, frequent stomach ache, frequent headache and low school belonging. The official older-pupil questionnaire identifies the relevant items and answer labels. The correspondence to FSV Q9/Q10/Q11/Q33 and its year-specific numeric encoding must be verified; a field name alone is insufficient. Refusals and missing responses remain missing. These reports are not a clinical depression scale or general quality of life, and reading assistance can affect how they are observed. [Official questionnaire](#)

Total and sickness absence are separate proportions of registered active school days. Sickness absence records a school classification, not a medical diagnosis. Recalculated national-test THETA and final Danish teacher grades measure demonstrated achievement in their documented scales. Neither isolates unaided decoding from accommodations or expectations. Missing scheduled tests, grades, enrollment months and questionnaires are reported with their denominators rather than silently removed.

Child healthcare has four distinct endpoints. Depressive encounters use valid-date SKS DF32/DF33 codes; anxiety/stress encounters use DF40/DF41/DF43. These are observed action/main or secondary diagnoses attached to hospital encounters, including recurrent care, not first onset of illness. Public-reimbursed community psychiatric/psychologist contacts and GP contacts form two additional service outcomes. Billing corrections and chronic-care honoraria are handled explicitly, and fee-week timing is distinguished from treatment dates. The original 2015 cohort's two-year hospital window precedes the 2019 hospital-system transition; later descriptive cohorts retain separate recording eras. [DST specialty definitions](#); [DST recording and processing cautions](#)

Income provenance is substantive measurement evidence. DST documents historical tax-free-assistance input problems in 2014–2016, while its corrected distribution/SDG products do not establish correction in the delivered IND or FAIK table. We therefore identify the exact delivery-variable-year status before reading income. Only documented propagation of the correction licenses the intended equivalized-resource interpretation; an uncorrected or unresolved version is analyzed solely as the fixed recorded-income proxy. No municipality is dropped and no correction is guessed from the suffix or a recent delivery date. Baseline balance then concerns recorded income only. The baseline status is *[pending]*, and family-outcome status is *[pending]*. [DST family-income definition](#); [DST person-level definition](#)

The common-scale reading judgment is *[pending]*. A pooled THETA effect requires documentary identification of the same reading construct, scale origin/unit and linked item calibration across all planned grade/year strata. If this cannot be established, we report the separate descriptive scales and a pooled-reading boundary or unresolved-calibration disposition. Observed-score standardization or a different test cannot repair that conclusion within this registration.

Family outcomes comprise parents' recorded depressive/anxiety/stress care, siblings' school belonging, current family equivalized resources, parents' paid employee hours, parents' wage income and subsequent retained parental national assessments. These indicators cover different aspects of family circumstances. They do not measure subjective parental stress, advocacy time, private tutoring costs or all adult dyslexia instruction. Available Nota membership and VEUV course documentation cannot establish comprehensive support receipt or a historical OBU treatment contrast.

Early-adult outcomes separate formal educational participation at 18, upper-secondary-or-higher attainment by 20, employee participation, wage income, equivalized resources and parental co-residence. Program and qualification classifications require dated official crosswalks. Employee records cannot supply a complete NEET definition or all self-employment. Resources are registry-defined equivalized amounts; they are neither consumption nor observed parental transfers. Co-residence requires

shared dated addresses and is not assigned a welfare sign. Recent child cohorts cannot support mature-adult outcomes, and none are promised.

Analysis

The adaptive schedule is fixed before outcome access. Scientific validation units assess retained events, the historical initial-assessment margin, and each grade's joint evidence from score resolution, density, baseline continuity and the recorded red-category jump. Each endpoint requires all grades in its frozen target; a grade9 failure does not restrict grades4–7 school outcomes. All-complete or globally constant baseline indicators receive arithmetic descriptions without invented zero-standard-error tests. Diagnostic nonrejection and recorded-income continuity remain limited evidence, never proof of exchangeability. Descriptive outcomes do not depend on the success of this causal sequence. Each family and horizon can retain its own description or measurement boundary when another source fails. The complete registration specifies local states and the exact Boolean activation rules; the paper reports every member's disposition, including nonactivation and operational failure.

Descriptive means and proportions are shown by retained-test calendar cohort and category, with eligible, observed and missing counts. Reading additionally separates target grade, scheduled year and calibration; teacher grades separate scheduled year, retaining the pandemic2020 stratum. Every age18/20 description separates birth year and outcome year, and all annual nominal family amounts retain their calendar year. The single primary contrast remains its predeclared baseline-weighted aggregate over those eligible calendars, not a constant-price or common-year effect. Public suppression changes displayed cells without changing which internal analyses are required. Intervals quantify descriptive sampling variation under family clustering, not all measurement uncertainty. No descriptive p-values are used to choose a later analysis.

Conditional on the required design evidence, we estimate grade-specific local-linear score discontinuities with triangular kernels and local-quadratic bias correction. The score is centered at the documented grade threshold, with equality on the red side. Coverage-error-rate bandwidth selection is capped at six original score points; grade contrasts are pooled using baseline count weights fixed before outcome values. Inference uses HC0 residuals from the side-specific quadratic fit and the explicitly registered bias-correction weights. School and baseline-family cluster sums are combined as school plus family minus intersection, each with its own $G/(G-1)$ factor; t degrees of freedom are the smaller effective cluster count minus one. The numerical core is checked against rdrobust4.0.0 at unequal as well as equal estimation/bias bandwidths. Recorded scores that are too coarse for the registered support safeguards do not receive artificial jitter or an improvised local-randomization interpretation. The target is the local threshold-policy contrast, including its induced responses; no support-receipt ratio is estimated.

There are 24 fixed primary endpoint/horizon slots, controlled as one study-wide family using Bonferroni FWER 0.05. Inactive, infeasible and failed slots remain in the denominator. Point estimates, raw and adjusted uncertainty, support and effective sample information are reported together. There is no defensible common clinical or

welfare equivalence threshold across these constructs; an interval including zero never licenses a claim of no meaningful effect.

For conceptually defined bounded outcomes, missing responses generate extreme-value bounds for the full baseline-eligible contrast, with simultaneous outer confidence limits. A point estimate conditional on being observed is a sensitivity description, not the primary causal claim. For unbounded scores, resources, hours and wages, unresolved postbaseline missingness leaves the point contrast selection-limited. A scheduled response or circumstance undefined after death remains selection-limited even when its scale is bounded. A degenerate outcome or invalid covariance that yields no valid interval is explicitly unestimable: the paper reports its reason and omits nonexistent limits and p-values. This differs from an interval spanning zero and from a software/access failure. Event outcomes measure recorded occurrence before death or emigration and report those competing events. None assumes that absence of a record means absence of illness or support.

Each primary contrast has fixed bandwidth and exact-cutoff sensitivity analyses. All variants appear; a favorable variant cannot replace the primary. The three Molecule interpretation policies reuse registered endpoint results to constrain the narrative. They are exploratory compositions with no new hypothesis test, holdout or causal mediation claim. Familiar school and transition results remain replication benchmarks even if they are precisely estimated.

Results - empirical fields awaiting accepted evidence

Who is represented and what can be identified

The retained-assessment composition is *[pending]*. The underlying event-record judgment is *[pending]*, the dated family-link judgment is *[pending]*, and the age/calendar scope is *[pending]*. Figure 1 displays retained cohort/category composition with unavailable and suppressed cells explicit. Table 1 reports these denominators and the design dispositions rather than treating an approved table listing as an analysis sample.

The historical grade/score mapping is *[pending]*, and the assessment-retention judgment is *[pending]*. Score support is *[pending]*, the density diagnostic is *[pending]*, and baseline continuity is *[pending]*. The recorded-classification jump is *[pending]*. If these conditions are compatible, subsequent causal estimates concern the registered launch-period margin. If any necessary condition is unresolved or contradicted, the corresponding causal rows below report that restriction while the independently valid descriptions remain. No failed gate is a policy null.

Hospital recording-era evidence is *[pending]*. The support-measurement judgment is *[pending]*. A limited-marker disposition rules out attributing any estimated contrast to a particular assistance package. An unavailable new hospital model does not itself invalidate the old-model two-year window.

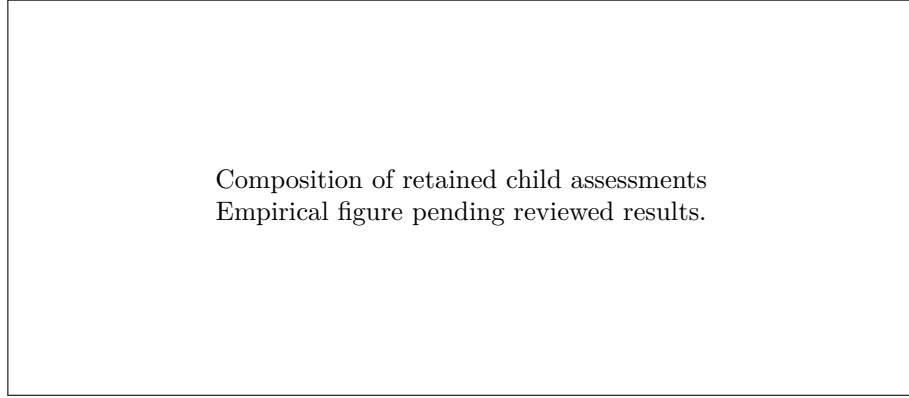


Fig. 1 Retained national assessments by calendar band and recorded category, including unknown categories. Fractions are of retained assessed children, not population prevalence. Suppressed/unavailable cells stay labeled; no count is inferred from the schema. *[pending]*

Table 1. Assessment basis and independently adjudicated design limits.

Dimension	Registered evidence or disposition
Retained composition	<i>[pending]</i>
Event-record semantics	<i>[pending]</i>
Historical rule mapping	<i>[pending]</i>
Approval and retention	<i>[pending]</i>
Score support	<i>[pending]</i>
Sorting diagnostic	<i>[pending]</i>
Baseline continuity	<i>[pending]</i>
Recorded classification	<i>[pending]</i>
Age and calendar scope	<i>[pending]</i>
Baseline family links	<i>[pending]</i>
Support construct limits	<i>[pending]</i>
Hospital recording eras	<i>[pending]</i>

School experience and demonstrated achievement

Figures 2 and 6 and Table 2 keep daily experiences, participation and performance separate. A decrease in reported frequent symptoms or low belonging would support a change in those observed experiences, subject to response bounds and the design restrictions. It would not alone establish better latent mental health. Lower absence and higher achievement would provide different educational evidence. If achievement increases while symptoms remain unresolved, the evidence would support demonstrated performance without resolving the symptom question. If signs diverge, the paper will retain that discrepancy rather than combine the outcomes into a success index.

Table 2. Separate registered outcome and horizon judgments.

Outcome and horizon	Observed distribution	Primary policy contrast or boundary	Registered sensitivity
Frequent loneliness; first full school year	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Frequent stomach ache; first full school year	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Frequent headache; first full school year	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Low school belonging; first full school year	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Total school absence; first full school year	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Sickness absence; first full school year	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Assessed reading; scheduled next reading test	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Teacher-assessed Danish; scheduled grade 9 year	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>

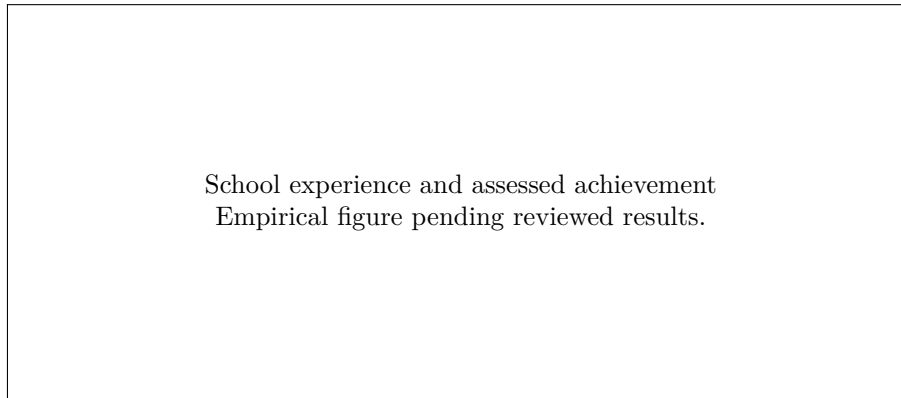


Fig. 2 School reports, attendance and demonstrated achievement are distinct outcomes. Reports can change through experience, assistance and response; achievement cannot isolate unaided reading. Panels show descriptive cohort/category means with 95% descriptive intervals when available, eligible/observed counts and explicit missing cells. Axes retain original units; these category comparisons are noncausal. The registered year, birth-year, grade and reading-calibration strata are displayed separately. Reading calibration groups have separate axes. The 2020 teacher-grade stratum remains labeled. Suppressed/unestimable cells retain labels and no invented values.

Recorded child care

Figure 3 and Table 3 distinguish depressive encounters, anxiety/stress encounters, community specialist contacts and GP contacts. An increase in contact could reflect

new recognition or referral as well as health need. A decrease could reflect reduced need, changed access or other recording processes. It therefore receives no automatic health-welfare sign. The joint symptom/care interpretation is *[pending]*. A divergence in resolved directions is compatible with several mechanisms; concordant declines are also insufficient to identify latent health or isolate detection. A qualified decrease in loneliness alongside a qualified increase in either care endpoint supports only the named pair; the other care endpoint is reported explicitly even if it has a measurement boundary, selection restriction or operational failure. Each used direction requires its own stable robustness and a valid adjusted interval or outer bound interval. Concordant declines require all three qualified decreases. These outcomes have different baseline grade sets and follow-up periods, so any resolved pattern is labeled *cross_target* and describes observed directions side by side; it does not show that a common population experienced both changes or isolate latent health, detection or mediation. Unqualified pairs remain unresolved, while a required primary that never activates leaves the synthesis *not_activated* and unknown required evidence keeps it pending.

Table 3. Separate registered outcome and horizon judgments.

Outcome and horizon	Observed distribution	Primary policy contrast or boundary	Registered sensitivity
Depressive encounters; days 1–730 before exit	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Anxiety stress encounters; days 1–730 before exit	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Community specialist contact; whole billing weeks to 730	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
General practitioner contact; whole billing weeks to 730	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>

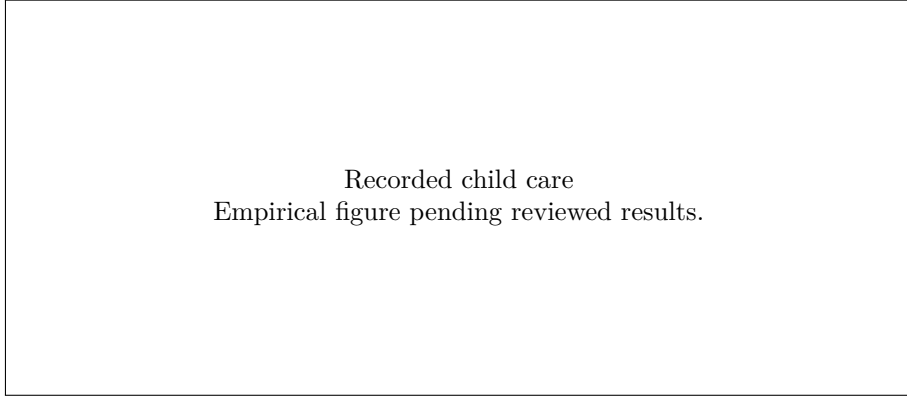


Fig. 3 Depressive/anxiety encounters, public-reimbursed specialist and GP contacts measure different recording processes; a contact increase has no automatic adverse-health meaning. Panels show descriptive cohort/category means with 95% descriptive intervals when available, eligible/observed counts and explicit missing cells. Axes retain original units; these category comparisons are noncausal. Hospital-based descriptions retain separate legacy, transition and new recording windows within the registered cohorts. The registered year, birth-year and grade strata are displayed separately. Suppressed/unestimable cells retain labels and no invented values.

Family circumstances

Figure 4 and Table 4 report parents’ care, siblings’ experience, resources, paid activity and subsequent retained parental assessment. These are outcomes of different family members and cannot be reduced to parental employment. The registered family-pattern judgment is *[pending]*. An increase in retained parental assessments would support an observed recognition pathway while leaving first lifetime diagnosis and the reasons for testing unknown. If the parental-assessment interval contains zero, its direction remains inconclusive even when the point estimate is negative or robustness is stable; a directional care result is reported separately, and the joint pattern remains unresolved. Only an adjusted interval wholly below zero supports a resolved assessment decrease. Even directional care alongside that decrease cannot establish a pure caregiving mechanism because alternative assessments, unrecorded help and shared circumstances remain possible.

Table 4. Separate registered outcome and horizon judgments.

Outcome and horizon	Observed distribution	Primary policy contrast or boundary	Registered sensitivity
Parents recorded care; child days 1–730	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Sibling school belonging; first full school year	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Family equivalized income (provenance-qualified); calendar year after test	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>

Outcome and horizon	Observed distribution	Primary policy contrast or boundary	Registered sensitivity
Parents paid hours; calendar year after test	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Parents retained assessment; child days 1–730	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Parents wage income; calendar year after test	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>

Figure 4 and the family-income description distinguish each reference year and delivery. Where correction provenance is verified as corrected, the values describe documented equivalized disposable resources. Where the exact year’s correction is known uncorrected or adjudicated unresolved, they describe only a recorded equivalized-income proxy; no intended-resource or welfare interpretation is permitted. Where the provenance binding is unavailable, no numerical resource statement is reported. These qualifications apply even to suppressed cells. Mixed-year displays retain each year’s own disposition, and a2016 judgment never certifies another year. Household composition remains part of the measure; neither corrected income nor the proxy measures consumption or direct transfers.

The family-income row in Tables4/6 and Figures6/7 follows the documented delivery status. Corrected provenance permits the registered disposable-resource interpretation. Known uncorrected or unresolved provenance supports only the recorded equivalized-income proxy; its exact correction status and prohibition on intended-resource or welfare interpretation accompany every primary and sensitivity estimate. Missing provenance metadata is displayed as unavailable, without a numerical resource claim. Stable sensitivity does not repair measurement provenance.

Distinct family responses
Empirical figure pending reviewed results.

Fig. 4 Baseline relatives define the target; large families do not gain extra index-child weight. Paid activity, resources, retained parental assessment and relatives' experience are not one burden index. Panels show descriptive cohort/category means with 95% descriptive intervals when available, eligible/observed counts and explicit missing cells. Axes retain original units; these category comparisons are noncausal. Hospital-based descriptions retain separate legacy, transition and new recording windows within the registered cohorts. All required year, birth-year and grade strata are displayed separately under D10; nominal money is never pooled across calendar years in descriptions. Suppressed/unestimable cells retain labels and no invented values. Family equivalized income is displayed separately by reference year and delivery with verified provenance. Corrected years permit documented-resource wording; known_uncorrected and adjudicated unresolved years are recorded equivalized-income proxies only, with no intended-resource or welfare interpretation. Unavailable binding permits no numeric resource statement. All restrictions appear even when values are suppressed; one year's correction state is never applied to another. No nominal-income pooling across years is performed.

Early-adult education and material circumstances

Figure 5 and Table 5 use only the predeclared age-eligible horizons. Completion and participation are benchmark questions; resources and co-residence assess additional circumstances. The composable material judgment is *[pending]*. Education and material results are first compared using frozen population, grade-weight and horizon metadata. The registered birthday18/birthday20 and annual20 horizons differ, so opposing signs ordinarily receive side-by-side different-scope narration, not a within-population divergence claim. They cannot establish financing constraints or lower lifetime welfare. Co-residence would retain its literal address interpretation regardless of direction. Later cohorts without an age-20 window have an explicit horizon boundary, not an imputed adult outcome.

Table 5. Separate registered outcome and horizon judgments.

Outcome and horizon	Observed distribution	Primary policy contrast or boundary	Registered sensitivity
Formal education at 18; 18th birthday	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>

Outcome and horizon	Observed distribution	Primary policy contrast or boundary	Registered sensitivity
Attainment by 20; 20th birthday	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Employee participation at 20; calendar year turning 20	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Wage income at 20; calendar year turning 20	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Equivalized resources at 20; calendar year turning 20	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>
Parental co-residence at 20; 20th birthday	<i>[pending]</i>	<i>[pending]</i>	<i>[pending]</i>

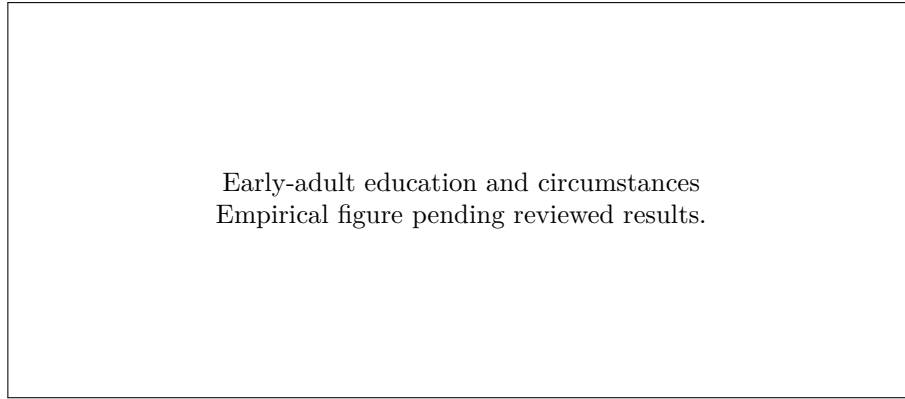


Fig. 5 Only birth-date-eligible cohorts contribute at 18/20. Employee work is not full NEET; resources and co-residence do not identify welfare or direct parental transfers. Panels show descriptive cohort/category means with 95% descriptive intervals when available, eligible/observed counts and explicit missing cells. Axes retain original units; these category comparisons are noncausal. All required year, birth-year and grade strata are displayed separately under D10; nominal money is never pooled across calendar years in descriptions. Suppressed/unestimable cells retain labels and no invented values.

All registered local classification-policy contrasts
Empirical figure pending reviewed results.

Fig. 6 All24 endpoint/horizon slots from the spring 2015 margin, separated by units. Points denote primary estimates, segments show study-wide Bonferroni intervals; a segment without a point denotes an outer missing-data bound. Nonactivation, boundaries, operational failure and selection-limited results remain labeled. A favorable variant never replaces primary. School, care, family and early-adult outcomes retain their separate claim ceilings. Exactly one frozen aggregate estimand appears per endpoint; required calendar strata are descriptive in Figures2–5/Tables2–5. Uncertain with no valid interval is displayed as unestimable with its reason. Family income is labeled as documented disposable resources only with corrected provenance and verified scope binding; known_uncorrected or unresolved provenance is visibly labeled Recorded equivalized-income proxy, with the correction status and no intended-resource or welfare interpretation printed below the panels. Unavailable meta-data is explicitly labeled and cannot carry numerical resource evidence.

Sensitivity and unresolved scope

Figure 7 and Table 6 display every registered local-support perturbation. The sensitivity column in Tables 2–5 binds each contrast to its own robustness judgment. If a contrast is sensitive, its primary estimate stays visible with that qualification; if a variant cannot be computed and no other variant establishes sensitivity, the robustness judgment is unresolved rather than stable. When a computable variant contradicts primary and another fails, sensitivity takes precedence and both reasons remain reported. Primary intervals retain their24-slot adjustment; nonprimary variants have individual exploratory95% intervals, with no simultaneous-coverage claim across specifications. The compiled graph atlas will report every scientific unit and its internal primary, ascertainment and robustness evidence, including pending, boundary, not-activated and operational no-result routes. Cross-domain compositions are reported under the Molecule interpretation. No endpoint is silently dropped from the multiple-testing family.

Table 6. Primary, half-bandwidth, wider-bandwidth and donut results in that fixed order. Each cell retains the four registered intervals or unavailable dispositions; no specification is selected for emphasis.

Outcome	Four-specification evidence and local judgment
Frequent loneliness	[pending]

Outcome	Four-specification evidence and local judgment
Frequent stomach ache	<i>[pending]</i>
Frequent headache	<i>[pending]</i>
Low school belonging	<i>[pending]</i>
Total school absence	<i>[pending]</i>
Sickness absence	<i>[pending]</i>
Assessed reading	<i>[pending]</i>
Teacher-assessed Danish	<i>[pending]</i>
Depressive encounters	<i>[pending]</i>
Anxiety stress encounters	<i>[pending]</i>
Community specialist contact	<i>[pending]</i>
General practitioner contact	<i>[pending]</i>
Parents recorded care	<i>[pending]</i>
Sibling school belonging	<i>[pending]</i>
Family equivalized income (provenance-qualified)	<i>[pending]</i>
Parents paid hours	<i>[pending]</i>
Parents retained assessment	<i>[pending]</i>
Formal education at18	<i>[pending]</i>
Attainment by20	<i>[pending]</i>
Employee participation at20	<i>[pending]</i>
Wage income at20	<i>[pending]</i>
Equivalized resources at20	<i>[pending]</i>
Parental co-residence at20	<i>[pending]</i>
Parents wage income	<i>[pending]</i>

Sensitivity to local support and exact-cutoff observations
Empirical figure pending reviewed results.

Fig. 7 Every registered contrast appears with primary, half-bandwidth, wider-bandwidth and exact-cutoff-donut specifications in fixed order. All carry the primary measurement/selection ceiling. Donut estimates change the extrapolation target. Missing variants are labeled, and consistency across variants is not identification or equivalence. The primary interval uses the fixed24 Bonferroni family; nonprimary variants have individual exploratory 95% intervals with no joint coverage across the display. A computable contradiction takes precedence over another variant failure; both reasons are shown. Family income is labeled as documented disposable resources only with corrected provenance and verified scope binding; known_uncorrected or unresolved provenance is visibly labeled. Recorded equalized-income proxy, with the correction status and no intended-resource or welfare interpretation printed below the panels. Unavailable metadata is explicitly labeled and cannot carry numerical resource evidence.

Discussion

The completed evidence will determine the breadth of the conclusions, but the relevant distinctions are fixed. A defensible local contrast would concern assessed pupils close to a particular historical rule, under their schools' and services' response environment. It would not generalize automatically to all Danish children with dyslexia or to later test versions. If retention or score resolution cannot support that contrast, the manuscript will report the narrower descriptive contribution openly. Documented outcome ascertainment and replication can still inform which experiences the registers capture, while causal priority is relinquished.

If school symptoms and care move together, a common health account remains possible but unproven. Opposing signs for different grade populations or horizons do not alone refute a common-population health account; they motivate a carefully labeled comparison of the observed targets. Recognition can affect reporting and help-seeking directly, so both patterns require caution. The conditional pattern statements above preserve these alternatives without choosing one mechanism from a favorable estimate.

Family outcomes similarly constrain a narrow individual account. Parents' own retained testing may accompany care, resources or paid activity, and siblings' belonging may change independently of the index child's achievement. Fixed pretest relationships avoid selecting intact or employed families after recognition, but they do not measure all carers or eliminate spillovers among families and schools. The study measures specific observed responses; it cannot monetize total family burden.

The early-adult component places education alongside contemporaneous material circumstances. It does not presume employment is the sole goal, earnings losses during study are harmful, or leaving the parental home is always beneficial. Differences in observation years and age eligibility will be stated before comparing outcomes. No recent child's record is extrapolated into mature adulthood.

The main limitations are structural. Historical approval and retained-test processes may be insufficiently documented. Grade and questionnaire mappings require more than field names. Clinical records combine need, access, referral and recording, while school reports can depend on assistance and response. Missingness bounds may be wide, and unbounded economic endpoints may remain selection-limited. The local analysis may have too few support points or clusters despite national registers. These are possible scientific boundaries and precision limits, not anticipated insignificant findings.

The contribution, if the data permit it, is an account of recognition whose educational, experiential, service and family dimensions can disagree. Its practical interpretation follows the exact observed constructs and their uncertainty. The study makes no prior commitment to a beneficial, adverse or null conclusion.

Supplementary evidence on validity and observation

Record construction and the historical recognition margin

The descriptive and local-design populations answer different questions. The former describes children with a retained assessment in 2015–2024; the latter concerns the spring 2015 recognition margin in ordinary public-school grades 4–9. An earliest warehouse record is not, without the registered retention evidence, a child's first assessment. The cohort flow therefore retains assessment year, grade, school sector, historical test version and the evidence about first-test retention. Removing later observations or selecting the earliest observed row cannot alone resolve a replacement or approval process that operated before the extract was created.

The running variable is the registered score relative to the historical grade-specific red threshold: 66, 70, 73, 77, 79 and 81 for grades 4–9. Equality belongs to the red category. Its meaning is a classification margin among assessed children, not randomized dyslexia, an entrance-examination pass mark, or an intervention on reading ability itself. Threshold mappings and recorded classifications are checked before a local contrast is interpreted. School assistance, family responses and service access can all be downstream of recognition; the study does not condition away those responses to manufacture a direct biological effect.

Family links are assigned from the last annual snapshot preceding the test. The registered parent set includes identified nonresident parents; sibling links require a shared nonmissing parent. The first eligible index child anchors the family component, while the stated child weighting determines the reported family quantity. These rules avoid redefining family membership according to later residence, employment or test uptake. They do not make the observed parent set an exhaustive record of care-giving. Parent, sibling and household outcomes retain their own age, observation and membership rules when displayed beside the index child's outcomes.

Measurement and comparable outcome scales

Every outcome table carries its registered unit, horizon and population. School questionnaires describe reported experience; attendance records describe recorded absence; reading scores and teacher grades measure different forms of achievement. Hospital and community-service outcomes reflect contact with a recording system as well as underlying need. A service increase is therefore not automatically deteriorating health, and a service decrease is not automatically improvement. Early-adult outcomes are observed only for cohorts reaching the specified ages within coverage; neither their values nor their absence is imputed from younger cohorts.

Pooling reading THETA requires evidence that the relevant scores share the registered scale. A variable with the same name in two deliveries does not establish that comparability. Family-income reporting likewise follows the documented 2014–2016 tax-free-input provenance at delivery, variable and year level. Corrected resources and a known-uncorrected or unresolved recorded-income proxy are different reporting scopes. When only the proxy is supported, every table, figure and discussion of that endpoint retains that label, with no welfare or intended-resource interpretation. These are measurement qualifications to existing endpoints, not alternative outcomes selected after seeing a result.

Local estimation and observation uncertainty

The primary local fit is linear with triangular weighting; the registered quadratic fit supplies bias correction. CER bandwidth selection is capped at six score points. Grade contributions use the fixed baseline weights. The uncertainty calculation retains school and family clustering, subtracts their intersection contribution, and uses the registered finite-cluster corrections and degrees of freedom. Grade support and estimability are thus part of the reported evidence: nationwide coverage cannot by itself create adequate local support, independent clusters or a valid standard error.

The main outcome family retains all 24 endpoint/horizon slots at familywise .05, including inactive or unavailable members. The separate 24-slot ascertainment family is not folded into that family or used to reduce its size. The six density, 48 baseline-continuity and six classification diagnostic slots retain their own .01 budgets. A successful diagnostic is limited evidence about its particular assumption; it does not certify every later outcome. The already-registered half-bandwidth, wider-bandwidth and donut comparisons remain within the owning scientific unit, with their distinct uncertainty labels and no selection of a preferred primary result.

Missing outcomes are handled according to their measurement support. For bounded endpoints the registered extremes can bound a full-cohort contrast. For unbounded economic outcomes, unavailable observations can leave a selection-limited quantity; finite bounds are not invented. Death-related undefined outcomes are not coded as ordinary zero outcomes or silently treated as random missingness. Table S2 consequently reports observation, exits, support and sensitivity alongside the endpoint. A missing companion endpoint does not erase a valid available component, but neither does an available component authorize a conclusion about the entire domain.

Supplementary result shells

Table S1 reports every grade-specific design judgment. A score-density flag in one grade restricts endpoints requiring that grade. The baseline table distinguishes informative estimates from structurally complete and globally constant indicators; an unestimable comparison is not a balance finding. The common mapping and retention judgments in Table1 still apply. Density uses six fixed diagnostic slots at familywise .01, baseline continuity uses 48 at .01, and category checks use six at .01. These budgets are separate from outcome inference.

Table S1. Grade-specific support, density, predetermined continuity and recorded-label judgments, with their exact scientific or operational dispositions.

Baseline grade	Score support	Density	Baseline continuity	Recorded classification
4	[pending]	[pending]	[pending]	[pending]
5	[pending]	[pending]	[pending]	[pending]
6	[pending]	[pending]	[pending]	[pending]
7	[pending]	[pending]	[pending]	[pending]
8	[pending]	[pending]	[pending]	[pending]
9	[pending]	[pending]	[pending]	[pending]

Table S2 treats observation as evidence in its own right. A complete recorded universe can resolve ascertainment without asserting absence of disease or detection bias. A constant observed indicator supplies its arithmetic scope, while a valid zero-containing interval is an inconclusive diagnostic. A detected observation difference retains the registered missing-data or selection restrictions. None of these states selects an endpoint or a preferred model. The companion implementation columns report per-grade bandwidths and bias bandwidths, fixed weights, effective support, competing exits, available-case sensitivity where applicable and conditional detectable contrasts. Detectable contrasts describe precision conditional on a valid standard error, never observed power; no value is reported when inference is unestimable.

Table S2. Endpoint-specific ascertainment and estimation evidence. All 48 primary and observation slots retain their separate 24-slot budgets; unavailable cells do not shrink either family.

Endpoint	Ascertainment judgment	Support, exits, selected-case sensitivity and conditional precision
loneliness	[pending]	[pending]
stomach ache	[pending]	[pending]
headache	[pending]	[pending]
low belonging	[pending]	[pending]
total absence	[pending]	[pending]
sickness absence	[pending]	[pending]

Endpoint	Ascertainment judgment	Support, exits, selected-case sensitivity and conditional precision
reading theta	<i>[pending]</i>	<i>[pending]</i>
danish grade	<i>[pending]</i>	<i>[pending]</i>
depressive encounter	<i>[pending]</i>	<i>[pending]</i>
anxiety stress encounter	<i>[pending]</i>	<i>[pending]</i>
community specialist	<i>[pending]</i>	<i>[pending]</i>
gp contact	<i>[pending]</i>	<i>[pending]</i>
parent care	<i>[pending]</i>	<i>[pending]</i>
sibling belonging	<i>[pending]</i>	<i>[pending]</i>
family resources	<i>[pending]</i>	<i>[pending]</i>
parent hours	<i>[pending]</i>	<i>[pending]</i>
parent assessment	<i>[pending]</i>	<i>[pending]</i>
education18	<i>[pending]</i>	<i>[pending]</i>
attainment20	<i>[pending]</i>	<i>[pending]</i>
employee20	<i>[pending]</i>	<i>[pending]</i>
wages20	<i>[pending]</i>	<i>[pending]</i>
resources20	<i>[pending]</i>	<i>[pending]</i>
coresidence20	<i>[pending]</i>	<i>[pending]</i>
parent wages	<i>[pending]</i>	<i>[pending]</i>

Ethics, access and reproducibility

This manuscript was prepared from public schema, institutional documentation and literature, without confidential records. Runtime person-level processing requires the appropriate governed data access and provider disclosure process; public outputs contain only approved aggregates. Programs record dictionaries, vintages, key assertions, code versions and deviations. The per-figure programs consume registered plot-ready tables and estimate no new effects. The two PDFs are pre-data documents, and compilation is not scientific acceptance. Funding, affiliations, provider agreements and conflicts were not supplied for this task and must be completed before publication rather than invented as absent.

Planned exhibits and references

Tables 1–6 are defined in the Results text and the source bindings below. The complete captions and input contracts for Figures 1–7 are in figures.yaml; their implementations await accepted aggregate data. Table 1’s design and coverage cells bind to the twelve named diagnostic/profile slots. Tables 2–5 display every endpoint’s description, primary effect/boundary and sensitivity disposition; Table 6 is the four-specification expansion already registered for each row, not another primary model.

All citations above link to the exact source read. The registration literature document records queries, dates, access levels, source versions, unsuccessful retrievals and remaining prior-art uncertainty. No unused candidate history forms part of this manuscript.

Figure6 caption: All24 local policy contrasts retain their fixed outcome/horizon and study-wide adjusted uncertainty. Family-income labels and the visible provenance note are determined by the verified scope metadata: corrected permits documented resources; known_uncorrected/unresolved means recorded equivalized-income proxy only, with no intended-resource or welfare interpretation. Missing metadata is explicit. Figure7 caption: The four fixed specifications retain this same provenance note on every family-income row. Primary intervals use the24-slot adjustment; nonprimary intervals are individual exploratory95% intervals with no joint coverage. Neither robustness nor a favorable sign overrides the proxy restriction.

Scientific-unit reporting

The analysis preserves 24 endpoint/horizon contrasts within scientific experiments. Loneliness and belonging describe social school experience; headache and stomach ache describe somatic symptoms; total and sickness absence describe attendance. Reading and teacher assessment retain separate experiments because their measurement requirements and target grades differ. Hospital distress-related encounters and community contacts remain distinct service questions. Parental care, sibling belonging, household income, parental paid work and parental recognition each have their own interpretation. Education, paid work and household circumstances in early adulthood remain separate domains. Related outcomes inside an experiment retain their own direction, uncertainty, observation and sensitivity result; no combined success score is constructed.

A unit's available result is not erased by an unavailable companion endpoint. For example, a qualified anxiety/stress increase can be described alongside a qualified loneliness decrease while depressive encounters remain unavailable; all three dispositions are stated. The 24-slot primary and separate 24-slot ascertainment families remain fixed regardless of the number of scientific units. The complete component bindings below Tables 1–6 and S1–S2 permit audit without requiring one Atom per table cell.