



STUDY PROTOCOL

Impact of special educational needs provision on hospital utilisation, school attainment and absences for children in English primary schools stratified by gestational age at birth: A target trial emulation study protocol [version 1; peer review: awaiting peer review]

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V1 First published: 21 Nov 2023, 3:59
<https://doi.org/10.3310/nihropenres.13471.1>
Latest published: 21 Nov 2023, 3:59
<https://doi.org/10.3310/nihropenres.13471.1>

Open Peer Review

Approval Status *AWAITING PEER REVIEW*

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Abstract

Introduction

One third of children in English primary schools have additional learning support called special educational needs (SEN) provision, but children born preterm are more likely to have SEN than those born at term. We aim to assess the impact of SEN provision on health and education outcomes in children grouped by gestational age at birth.

Methods

We will analyse linked administrative data for England using the Education and Child Health Insights from Linked Data (ECHILD) database. A target trial emulation approach will be used to specify data extraction from ECHILD, comparisons of interest and our analysis plan. Our target population is all children enrolled in year one of state-funded primary school in England who were born in an NHS hospital in England between 2003 and 2008, grouped by gestational age at birth (extremely preterm (24-<28 weeks), very preterm (28-<32 weeks), moderately preterm (32-<34 weeks), late preterm (34-<37 weeks) and full term (37-<42 weeks). The intervention of interest will comprise categories of SEN provision (including none) during year one (age

five/six). The outcomes of interest are rates of unplanned hospital utilisation, educational attainment, and absences by the end of primary school education (year six, age 11). We will triangulate results from complementary estimation methods including the naïve estimator, multivariable regression, g-formula, inverse probability weighting, inverse probability weighting with regression adjustment and instrumental variables, along with a variety for a variety of causal contrasts (average treatment effect, overall, and on the treated/not treated).

Ethics and dissemination

We have existing research ethics approval for analyses of the ECHILD database described in this protocol. We will disseminate our findings to diverse audiences (academics, relevant government departments, service users and providers) through seminars, peer-reviewed publications, short briefing reports and infographics for non-academics (published on the study website).

Plain Language summary

One third of all children need extra help with learning in school, such as support from a teaching assistant. Children born preterm are more likely to need extra help compared to those born at term. In England, this help is called special educational needs (SEN) provision. The aim of this study is to find out whether special educational need provision affects education and health outcomes. We will use information collected by hospitals and schools for all children who were born in England between 2003 and 2008. We will compare those with who received and did not receive extra help in school who have a similar gestational age at birth.

Keywords

Gestational age, Intervention, Special educational needs, Trial emulation

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Competing interests: No competing interests were disclosed.

Grant information: This project is funded by the National Institute for Health Research (NIHR) under its Programme Grants for Applied Research Programme (Grant Reference Number NIHR202025). RG is supported by a NIHR Senior Investigator award. ECHILD is supported by ADR UK (Administrative Data Research UK), an Economic and Social Research Council (part of UK Research and Innovation) programme (Grant Reference Numbers ES/V000977/1, ES/X003663/1, ES/X000427/1). Research at UCL Great Ormond Street Institute of Child Health is supported by the NIHR Great Ormond Street Hospital Biomedical Research Centre. The ECHILD Database uses data from the Department for Education (DfE). The DfE does not accept responsibility for any inferences or conclusions derived by the authors. This work contains statistical data from ONS which is Crown Copyright. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates. This research contributes to but was not commissioned by the NIHR Policy Research Programme. The views expressed are those of the author(s) and not necessarily those of the NIHR or the Department of Health and Social Care.

The funders had no role in study design, data collection and analysis, decision to publish, or preparation of the manuscript.

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How to cite this article: Nguyen V, Lewis KM, Gilbert R *et al.* **Impact of special educational needs provision on hospital utilisation, school attainment and absences for children in English primary schools stratified by gestational age at birth: A target trial emulation study protocol [version 1; peer review: awaiting peer review]** NIHR Open Research 2023, 3:59
<https://doi.org/10.3310/nihropenres.13471.1>

First published: 21 Nov 2023, 3:59 <https://doi.org/10.3310/nihropenres.13471.1>

Background

In the state-funded educational system in England, the system of reasonable adjustments to support children who experience difficulties learning is known as special educational needs (SEN) provision. The current version of SEN provision falls under two categories: SEN support and Educational and Health Care Plans (EHCPs) (Long & Danechi, 2023). SEN support provides classroom-based support, such as extra help from a teacher (or assistant) or access to special learning programmes. EHCPs provide support for pupils who require more support than is available through SEN support. Due to the funding and organisational streams of SEN provision, allocation of SEN provision has been changing over time, impacted by changes in legislation, school governance structure and local authority (Liu *et al.*, 2020). SEN provision is provided more frequently to children with health problems associated with low academic attainment such as children born preterm (Alterman *et al.*, 2021), with congenital anomalies, such as cleft lip and palate (Fitzsimons *et al.*, 2018), or with congenital heart defects (Glinianaia *et al.*, 2021). However, the potential impact of SEN provision on educational and health outcomes during primary school has not been evaluated.

Children who are born preterm (i.e. <37 weeks gestation) disproportionately experience long-term difficulties compared to their full-term peers, including lower educational outcomes (Libuy *et al.*, 2023), higher burden of comorbidities (particularly in very premature births) (Mowitz *et al.*, 2022) and higher contact with health services and emergency health services (Coathup *et al.*, 2020). Increasing rates of SEN provision with earlier gestational age at birth in primary schools in England has been previously documented (Libuy *et al.*, 2023). There are also descriptive publications showing increased hospital utilisation by gestation age (Coathup *et al.*, 2020), and education performance by gestation age (Libuy *et al.*, 2023). However, there is limited evidence on the impact of SEN provision on academic performance, school absences and hospital utilisation in pupils who need SEN provision.

We will emulate a pragmatic target trial study using linked administrative school and hospital records in the ECHILD database. We will separately analyse children grouped according to gestational at birth who, particularly in the most premature groups, have a similar need for SEN provision (Libuy *et al.*, 2023). For each gestational age group, we will estimate the causal effect of SEN provision in year one of primary school on school attainment, school absences and rates of unplanned hospital admissions by the end of primary school (year six, age 10/11).

The emulated target trial aims to reduce risk of confounding and selection bias. Firstly, using known and presumed confounders of the relationship between SEN provision and our outcomes, we will evaluate the assumptions to be invoked for the estimation of causal links between them. In particular, the positivity assumption for the probability of receiving different categories of SEN provision (no SEN provision, SEN

support in mainstream school, EHCP in mainstream school, special school attendance) within each gestational age group; that is that, for all combinations of covariates, there is a non-zero probability of recording each category of SEN provision. Secondly, for each gestational age group where the positivity assumption holds (Zhu *et al.*, 2021), assuming there is no unmeasured confounding, we will estimate, and compare potential educational and health outcomes under differential treatment regimens (no SEN provision, SEN support in mainstream school, EHCP in mainstream school, special school attendance).

Methods

Patient and Public Involvement

Prior to developing this protocol, independent meetings were conducted with stakeholders (parents, pupils, teachers) from existing patient advocacy groups including the Young Person's Advisory Group (YPAG), Council for Disabled Children's group (FLARE) and the Great Ormond Street National Children's Bureau Families Research Advisory Group (FRAG). On 14 November 2020, FLARE were introduced the ECHILD dataset and its use of linked administrative data and to the observational study design with warm reception. Further meetings were held with FLARE on the 18th of September 2021 and with YPAG for research at Great Ormond Street Hospital on the 27th of November 2021. This engagement identified that school entry is an important key milestone when SEN provisions are required. Therefore, in the proposed study, we have used school start as our entry point and will generate further target trials based upon further stakeholder engagement. The Great Ormond Street Hospital for Children's NHS Foundation Trust Young People's Forum voiced that school absences were an important topic for research on 20 March 2021. Therefore, using these interactions, we've created our research question, which was presented to the HOPE study steering committee, and includes parents of children with disabilities who will review and advise on the presentation and dissemination of the study findings. Records **and learnings** from public engagements can be found [here](#).

Study design

Trial emulation framework applied to observational educational data linked to healthcare data. Analyses will be conducted in the Office for National Statistics Secure Research Service using Stata 17 and R version 4.0.2 (open source, free software). Once written, the code for the study, including algorithms to identify the population, exposure, outcomes, and confounders, will be made publicly available on publication of the full manuscript.

Data source and linkage

We will use the ECHILD database, a pseudo-anonymised dataset that links Hospital Episode Statistics (HES) with the National Pupil Database (NPD). A linkage rate of 95% has been reported between NPD and HES in ECHILD, with high linkage rates attributed to a two-stage linkage process (Libuy *et al.*, 2021).

In brief, the ECHILD’s extract of NPD contains pupil-level data from state schools in England for academic terms between 2006 and 2020 (Mc Grath-Lone *et al.*, 2022). This includes school, local authority, age, gender, ethnicity, first language, socioeconomic status, free school meal status, recorded absences, social care/children in need related data and SEN status. In addition to the NPD, school level characteristics such as school type (including special or mainstream), school rating, and governance are available through the Department for Education’s open-source ‘Get Information about Schools’ (GIAS) register, and linkable to ECHILD using the school’s unique reference number (GOV.UK, 2022).

The ECHILD’s extract of HES contains details on admitted patient care, outpatient appointments, accident and emergency utilisation, and critical care between 1997 until 2021. It contains details on admission and discharge dates, patient characteristics (e.g., sex, ethnicity, area of residence) and clinical information recorded during hospital admissions (such as, details of diagnoses and operations). HES covers 99% of public hospital activity in England (Herbert *et al.*, 2017). HES also contains birth records which record characteristics such as gestational age, birthweight, maternal age; missingness in an individual’s birth record can be complemented using the corresponding mother’s delivery record. Furthermore, since 1998, HES records are also linked to ONS Mortality data covering information on mortality causes and timing of deaths.

Further details of the ECHILD dataset are documented by Mc Grath-Lone *et al.*, 2022.

Population and follow-up

Our population is singleton children who were born in NHS-funded hospitals in England between 1 September 2003 and 31 August 2008 and were enrolled in year one of a state-funded primary school in England at age five/six years (see Figure 1). Children will be excluded if they do not have complete information on gestational age. Child will also be excluded from analyses of educational outcomes if they have missing data on the early years foundation stage profile (in reception, age four/five). We will also exclude children with a gestational age of <24 or >44 weeks or those with implausible gestational ages based on birthweight because of a high risk of misclassification. Comparisons of included and

excluded children will help to inform whether there are issues of selection bias. This population was chosen as these children can be followed up to the end of primary school (year six, age 10/11) in ECHILD, with the latest academic year of follow up before the COVID-19 pandemic.

The study population will be followed-up from the January census in year one (age five/six) until the first chronological event of: end of primary school (year six, age 11 at exit), lost to follow-up or end of study (30th July 2019). Children will be considered lost to follow-up if they no longer appear in any NPD school census; this may be due to transfer to a non-government funded school or alternative provision, off-rolling (where pupils are illegally excluded from school) (Jay *et al.*, 2022), emigration or death. We begin follow up in year one rather than reception (the first year of primary school in England) as it is the first full school year when education is compulsory for all children. We use the January census (rather than the October census) to allow for time for pupils to be assigned SEN provision.

Subgroups

We expect the impact of SEN to vary according to the child’s need for SEN, which is correlated with decreasing gestational age at birth (Libuy *et al.*, 2023). We will therefore conduct all analyses separately for five subgroups, defined by completed weeks gestation at birth: extremely preterm (24-<28 weeks); very preterm (28-<32 weeks); moderately preterm (32-<34 weeks); late preterm (34-<37 weeks); full term (37 to <42 weeks) (ONS, 2015).

Intervention variable

Our intervention consists of four categories of recorded SEN provision in the January census of year one of school: none; SEN support (previously known as School Action/School Action Plus) at mainstream school; EHCP (previously known as statement of SEN) at mainstream school; and special school attendance (where the vast majority of children have an EHCP). Whilst SEN provision can change throughout a child’s educational journey, our implementation of trial emulation focusses on an observational-analogue of intention-to-treat analysis (ITT) of SEN at the start of compulsory education. This analyses the assignment of treatment and not whether treatment was adhered to or provided. We choose the start of compulsory education as we believe this is a population in

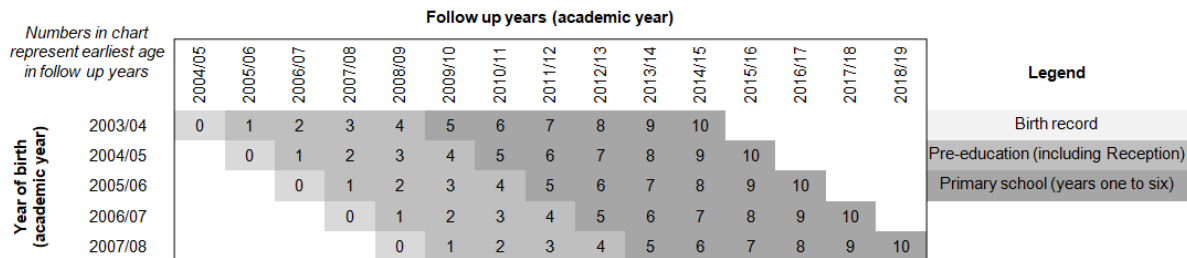


Figure 1. Expected age at entry into primary school years one to six, by birth year and follow-up year; Y = year; ^adefined according to the academic calendar (i.e., 2003/04 includes 1 September 2003 to 31 August 2004, inclusive).

need of SEN provision from the start of their educational journey based upon prior evidence of educational (Libuy *et al.*, 2023) and healthcare needs (Coathup *et al.*, 2020).

Outcome variables

We will evaluate both health and educational outcomes.

For health outcomes, we will evaluate unplanned hospital utilisation, consisting of the number of unplanned admissions to hospital (defined by the admission method in the first episode of care) and contacts with an accident and emergency departments between January of start of year one (age five/six) and at the end of year six (age 11) (Harron *et al.*, 2018).

For educational outcomes, we will evaluate key stage two English and mathematics assessments (taken in Year six, at ages 10/11), including whether assessments are taken (yes or no) and, if taken, attainment in the assessments. To account for time-varying changes in recording of educational outcomes, we will use standardised scores within academic year.

We will also evaluate the number of absences during primary school (January year one to the end of year six) including unauthorised absences and absences related to illness and dental or medical appointments.

Covariates

To account for determinants of SEN provision assignment in children with similar gestational ages, we will use information on covariates known or suspected to influence (or be associated with) SEN provision based upon prior literature (Coathup *et al.*, 2020; Hutchinson, 2021; Libuy *et al.*, 2023). Table 1 shows our preliminary list of sociodemographic, educational and health related covariates which are related to SEN provision and both educational and health related outcomes. We will use DAGitty version 3.0, an open-source piece of software create directed acyclic graphs (DAGs) to guide our selection of variable adjustment set to reduce the risk of unaccounted confounding, overadjustment and potentially mediating away any true effects.

Bias

To reduce confounding and other sources of bias impacting data collected outside of a randomised controlled trial setting, we will adopt the Target Trial Emulation (TTE) framework (Hernán *et al.*, 2022). TTE maps observational data to a hypothetical target experimental trial counterpart by creating the specification of an ideal (pragmatic) trial and using this as a basis to shape the observational study design. TTE consists of firstly, defining the specifications of a hypothetical, ideal experimental trial of the causal question of interest (including the corresponding causal contrast), secondly, emulating the specifications of the ideal target trial using observational data and thirdly, estimating the effects of interest using the emulated trial data. The first component of TTE includes defining an inclusion/exclusion criterion on entry, a treatment strategy (including time of assignment and entry), follow-up frequency and modality, outcome measures, causal contrasts

of interest and the analytical estimation methods for an ideal trial. Using the second component of TTE, observational data are wrangled to emulate the distribution of the data if it were to have been gathered prospectively in the ideal trial. Finally, the third component of TTE requires using methods to adjust for known and suspected confounding. In Table 2, we describe the ideal target trial that would be designed to investigate the causal effect of SEN provision (by the upcoming January Census in the first year of compulsory education) on the relevant outcomes and the equivalent emulated trial to be generated from ECHILD.

Statistical analysis

Data wrangling. Based upon the proportion and mechanisms of missingness in the data, we will first use future recordings to complement missing baseline covariates such as gender; secondly, we will complement non-missing data in HES and NPD prior to data imputation; for example, using sex variable from HES to complement missing values in the NPD variable gender (Azur *et al.*, 2011).

Exploratory Analysis. We will first analyse the feasibility counts of the ECHILD data, including gestational age subgroups, the distribution of variables including our exposure (SEN provision) and confounders (Table 1). This will include assessing the feasibility of including children attending alternative provision (including pupil referral units) in our eligibility criteria and follow up; these groups are assumed to have small numbers and hence, their inclusion, may pose violations the positivity assumption.

To understand whether there are violations of the positivity assumption (i.e., whether pupils who are recorded to be requiring different categories of SEN provision are comparable), we will calculate and compare the propensity score distributions for each SEN category within each gestational age group. We will compare the density distribution between each pairwise of groups (Rassen *et al.*, 2013), for example, none *versus* SEN support in mainstream school, SEN support *versus* EHCP in mainstream school, none *versus* EHCP in mainstream and so on. Propensity scores for each SEN provision category will be estimated using logistic regression; to assess their robustness, binary machine learning predictors of each SEN provision category, such as tree-based algorithms, will be used and the resulting propensity scores compared to those obtained when using logistic regression (Lee Brian *et al.*, 2009).

Causal inference. Our causal analyses will be conducted for pairs of interventions where the causal assumptions of non-interference, consistency, positivity, and conditional exchangeability are assumed to hold (Hernán, 2012) (see Table 3). For health outcomes and school absences (which are count data) and educational outcomes (which are continuous variables), we aim to triangulate results from three groups of methods: methods traditionally used in epidemiology, methods that rely on the no-unmeasured confounders assumption and, if possible, methods that exploit instrumental variables or difference in difference methods.

Table 1. Potential confounders.

Covariate Group	Covariate	Categories of measurement	Source
Clinical	Biological sex	Female Male Unknown (depending on numbers)	HES
	Major congenital anomaly	Presence of congenital anomaly (yes or no), based on the Hardelid UK chronic condition ICD-10 code list identified in infant hospital admissions up to age 2 (Hardelid <i>et al.</i> , 2014)	HES
	Prior unplanned hospitalisation usage before year one of school	Number of days in which a child is recorded as attending an accident and emergency department or admitted to hospital in an emergency adjusted for person-time	HES
Education	Early years foundations stage profile (English and mathematics score)	Standardised z-score for English and mathematics within academic year	NPD
	School Governance Type	Local authority managed Academy Other	GIAS
	School Type	Mainstream Special Alternative Provision Pupil Referral Unit	GIAS
	Pupil Teacher Ratio	Ratio depicting the number of pupils per teacher in the school	GIAS
Socio-demographic	Child's ethnic group	Asian, Black, Mixed or multiple ethnic groups, White, other	NPD
	Maternal age at birth	Continuous values between 10–60. We will censor ages below 10 and above 60 because of a high risk of misclassification	HES
	Free school meal	Eligible for free school meals Not eligible for free school meals	NPD
	Month of birth	January to December	HES and NPD must match
	Deprivation at birth	IMD deciles	HES
	Deprivation at start of school	IDACI quintiles	NPD
	English as a first language	Recorded as English Not recorded as English Unknown	NPD

HES = hospital episodes statistics, GIAS = get information about schools, NPD = national pupil database

Our first group of methods will implement the naïve and adjusted estimators using general linear models as part of our traditional epidemiological estimates including Poisson based link functions (with the logarithm follow-up time as an offset) for counts of individual health outcomes and absences, and linear link functions for individual educational scores (Arnold *et al.*, 2021). The second group of methods includes outcome-based methods which rely on the no-unmeasured confounding assumption and expand on traditional epidemiological methods by focussing on marginalising

results over the population using models such as the parametric g-formula, inverse probability weighting, and inverse probability weighting using regression adjustment (Smith *et al.*, 2022). With these methods inference will be based upon bootstrapping. For both health and educational outcomes, we will calculate and compare the following causal contrasts: observational analogue of the ITT, the overall average treatment effect, the average treatment effect in the treated and the average treatment effect the not treated (see Table 4 for definitions).

Table 2. Summary protocol: target trial emulation to evaluate the effect of SEN provision on assessment scores and unplanned hospital contacts.

Protocol component	Target trial specification	Emulation study	Potential challenges and possible solutions
Eligibility criteria	<p>Born in England between 1 September 2003 and 31 August 2008.</p> <p>Started year one in a state-funded mainstream or special school in England between 2009/10 and 2013/14.</p> <p>Taken part in the EYFSP assessments before year one.</p>	<p>Born in England between 2003 and 2007 with gestational age recorded in birth/delivery record.</p> <p>Linked HES-NPD records.</p> <p>Recorded start of year one between 2009/10 and 2013/14 in a mainstream or special school in any termly School Census of the National Pupil Database.</p> <p>EYFSP assessment is recorded.</p>	<p>Based upon prior experience of using these administrative data, we expect some children appear in year one twice - these will be removed due to uncertainties about the reliability of these data; not all pupils have EYFSP and teacher strikes are expected to reduce key stage 2 assessment availability - missingness patterns will be examined and when a MAR assumption is defensible, multiple imputation will be performed and then the potential selection bias of an incorrect MAR assumption evaluated in sensitivity analyses</p>
Study design	Randomised controlled trial	Trial emulation framework applied to linked observational hospital-school data	Potential residual or uncontrolled confounding by indication
Data structure	Prospective data collection as part of the randomised controlled trial	Retrospective wrangling of administrative data leading to prospective information	Missingness patterns will be examined and when a MAR assumption is defensible, multiple imputation will be performed and then the potential selection bias of an incorrect MAR assumption evaluated in sensitivity analyses
Outcome	<ul style="list-style-type: none"> Key stage 2 assessments School absences (unauthorised and health related) Unplanned hospital utilisation 	<ul style="list-style-type: none"> Key stage 2 assessments School absences (unauthorised and health related) Unplanned hospital utilisation 	Teacher strikes are expected to reduce key stage 2 assessment availability – missingness patterns will be examined as outlined above
Treatments to be compared	Categories of SEN provision: none, SEN support in mainstream school, EHCP in mainstream school, special school attendance	Categories of SEN provision where there exists pairwise common support: none, SEN support in mainstream school, EHCP in mainstream school and special school attendance	As there may be a delay in applying for SEN provision; we will consider a sensitivity analysis where our treatment assignment will be by year two instead of year one
Causal contrasts	Intention to treat for SEN provision assignment in the first full year of compulsory education (year one, age five on entry), with none as the reference category	Observational analogue of the intention to treat for SEN provision as recorded in the first full year of compulsory education (year one, age five on entry). Additionally, the average treatment effect in the non-treated; the average treatment effect in the non-treated (see definitions in Table 4)	
Analysis plan	<p>Logistic and linear regression for key stage 2 results</p> <p>Poisson (or negative binomial) regression models, as appropriate for school absences and unplanned hospital utilisation</p>	<p>Educational outcomes - logistic and linear regression modelling with appropriate control for confounding adjustment and standardisation (such as regression adjustment and standardisation, propensity score-based methods).</p> <p>Clustering by school and/or local authority to be dealt with using either mixed effects models or robust inference (e.g. generalised estimating equations).</p> <p>Health outcomes - Poisson (or negative binomial) regression models with appropriate control for confounding, followed by standardisation.</p>	

EHCP= education, health and care plan; EYFSP=Early years foundation stage profile; HES=hospital episode statistics; LA=local authority; MAR=missing at random; MCA=Major congenital anomalies; MSOA=Middle Layer Super Output Area; NPD=national pupil database; SEN=special educational needs

Table 3. Identifiability assumptions.

Identifiability assumption	Application to this study	Testing the assumption: can we meet it?
<i>No interference:</i> an individual's (or unit's) potential outcome does not depend on other individuals' (units') treatment assignment, $Y_i(T_1, T_2, \dots, T_n) = Y_i(T_i)$, where $Y_i(t)$ is the potential outcome when the intervention T is set to take the value t	Key stage two results/ the number of hospital contacts/absences do/does not depend on whether other children receive SEN provision.	Theoretical; we suspect there is residual interference, given the nature of SEN provision (e.g., learning support assistants) in the classroom setting – therefore estimates of the ATE could be biased because of a spill-over effect. We expect the AT(N)T to be less likely impacted by interference.
<i>Consistency:</i> The intervention is well-defined and corresponds to what is captured in the data. Put another way, the exposure definition must have enough precision that any variation in that exposure does not lead to a different outcome: $Y_i(t_1) = Y_i(t_2)$, if t_1 and t_2 are different versions of the intervention.	We assume that the potential outcome for a given category of SEN provision is the same for all children, even if that provision is delivered differently. Also assumes that school-recorded SEN provision is a good proxy for receipt of SEN provision.	If this assumption is not defensible, we will interpret $E[Y(0)]$ and $E[Y(1)]$ as the averages of the various potential outcomes that would arise from the multiple versions of the exposure seen in the data.
<i>Positivity:</i> all individuals have a probability greater than 0 (a positive probability) of being assigned each value of the intervention, in every stratum defined by the covariates used to control for confounding, C , that allow for the conditional exchangeability assumption to be met, i.e., the confounders. $0 < P(T_i C_i) < 1$ for all C_i	That, when studying educational/health outcomes, there is a non-zero probability of receiving any of the categories of SEN provision, given the relevant confounders.	We will examine propensity score overlap between pairwise comparison groups and limit the analyses to comparisons where common support is found.
<i>Conditional exchangeability:</i> The assignment mechanism is unrelated to potential outcomes, conditional on covariates, $Y(0), Y(1) \perp\!\!\!\perp T C$, where T is the intervention, C the covariates, and $\perp\!\!\!\perp$ indicates “independence”	After controlling for covariates, individuals in different intervention groups have similar characteristics, i.e., are exchangeable.	Yes, if the correct confounding adjustment set is identified and adjusted for. Alternative estimation methods that do not rely on these assumptions and that target the same causal contrasts will be also pursued (e.g., exploiting <i>IVersus</i>), with results compared.

C=covariates/controls; E=expectation; i=units; P=probability; SEN=special educational needs; T=treatment (or intervention); Y(t)=potential outcome; Y(0)=potential outcome when untreated; Y(1)=potential outcome when treated; IV=instrumental variable

The third group of methods includes instrumental variable and difference-in-difference methods and are only suitable if instruments for SEN provision are identified, for example if there are policy changes in provision that are implemented at different times across local authorities (Greenland, 2018). These would lead to estimate (under the assumption of individual homogeneity of effects) the observational analogue of the ITT. Related to these are difference-in-difference based methods that to estimate group differences against predicted trajectories between different groups of recorded SEN provision, leading to estimating the ATT (Richardson *et al.*, 2023). See Table 4 for the research these causal contrasts are addressing.

Missing data. To deal with missing covariate values (there are no missing exposure data by design) we will use Imputation using Chained Equations (ICE) as part of the bootstrap-based estimation of confidence intervals of point estimates, we will use in each replicant as part of bootstrap imputation (Schomaker & Heumann, 2018). All variables will be used to predict missing data including the exposure

and the outcome, and any other variables assumed to be informative of the missing values (Azur *et al.*, 2011).

Sensitivity analyses. We aim to conduct a series of sensitivity analyses to estimate the robustness of our results. Firstly, we will adjust our assignment of recorded SEN provision from year one to year two to account for the administrative time it takes for parents/carers to apply for SEN provision. One of our criteria is that pupils must have data on their EYFSP school readiness tests as this is a major confounding variable; this may restrict our population to those able to take the test. Hence, to account for this non-participation, we will use a missingness indicator to capture the information held in missing the test and avoid excluding those without a record (Groenwold *et al.*, 2012). Furthermore, we suspect there maybe missingness in outcome data, particularly for key stage two scores based upon prior knowledge of systematic teacher strikes; in such cases we will use imputation to estimate these key stage two outcomes using year of testing in the imputation model. Finally, we propose analysing the correlation between recorded child sex (reported by physician in

Table 4. Comparison of causal contrasts.

Causal Contrast	Formal definition	Educational outcome causal question	Health outcome causal question
Average Treatment Effect, (in this setting same as the ITT)	$ATE(w) = E(Y(T=1) W=w) - E(Y(T=0) W=w)$	What would be the difference in the average assessment score at key stage two (KS2) if all children born at gestational age w were and were not set to receive SEN provision? What would be the average difference in number of absences by year six if all children born at gestational age w were and were not set to receive SEN provision?	What would be the difference in the rate of unplanned hospital admissions if all children born at gestational age w were and were not set to receive SEN provision?
Average Treatment Effect in the Treated	$ATT(w) = E(Y(T=1) T=1, W=w) - E(Y(T=0) T=1, W=w)$	What would the difference in average assessment score at KS2 be if all children born at gestational age w who received SEN provision, had not received SEN provision? What would be the average difference in number of absences by year six if all children born at gestational age w , who received SEN provision had not received SEN provision?	What would the difference in the rate of unplanned hospital admissions be if all children born at gestational age w who received SEN provision, had not received SEN provision?
Average Treatment Effect in the not treated	$ATNT(w) = E(Y(T=1) T=0, W=w) - E(Y(T=0) T=0, W=w)$	What would the difference in the average assessment score at KS2 be if all children born at gestational age w who did not receive SEN provision, had instead received SEN provision?	What would the difference in the rate of unplanned hospital admissions be if all children born at gestational age w who did not receive SEN provision, had instead received SEN provision?

ITT=intention to treat; T=SEN provision (1 alternative category of SEN provision, 0 reference category); SEN=special educational needs; W=week of gestational age, taking a value between 24 and 42 (w)

HES) and gender (submitted by parent/carer during school registration in NPD). To understand the validity of our models, we will produce a table of how using either variable impacts our point estimates of the intervention variable only.

Ethics

Permissions to use de-identified data and linked from Hospital Episode Statistics and the National Pupil Database were granted by DfE (DR200604.02B) and NHS Digital (DARS-NIC-381972); consent from patients is not required for HES as the data provided by NHS Digital is pseudo-anonymised and reduces identifiability to researchers; further information on opting out of Hospital Episode Statistics for secondary usage can be found [here](#). Ethical approval for the ECHILD project was granted by the National Research Ethics Service (17/LO/1494), NHS Health Research Authority Research Ethics Committee (20/EE/0180) and UCL Great Ormond Street

Institute of Child Health’s Joint Research and Development Office (20PE06).

Data availability

No data are associated with this article.

Acknowledgments

We gratefully acknowledge all children and families whose de-identified data are used in this research. We would like to acknowledge the contribution of the wider HOPE study team to this work: Sarah Barnes, Kate Boddy, Kristine Black-Hawkins, Lorraine Dearden, Tamsin Ford, Katie Harron, Lucy Karwatowska, Matthew Lilliman, Stuart Logan, Jacob Matthews, Jugnoo Rahi, Jennifer Saxton, Isaac Winterburn and Ania Zylbersztejn. We thank Ruth Blackburn, Matthew Jay, Farzan Ramzan, and Antony Stone for ECHILD database support.

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