

The SWIS Trial: An evaluation of school based social work

Intervention Developer	What Works for Children's Social Care (WWCSC)
Delivery Organisation	WWCSC
Evaluator	Children's Social Care Research and Development Centre (CASCADE) and Centre for Trials Research (CTR), Cardiff University, with the Nuffield Department of Primary Care Health Sciences, University of Oxford
Principal Investigator	David Westlake
Protocol Author(s)	David Westlake, Philip Pallmann, Fiona Lugg-Widger, James White, Donald Forrester, Stavros Petrou, Shahd Daer
Date and version of Protocol	May 2022 - Version 3
Type of Trial	Pragmatic cluster randomised controlled trial with two arms (one intervention and one control group)
Age or Status of Participants	Children and young people aged 11 – 18; social workers and social care managers; school staff
Study setting	Secondary schools in England (n=291), across 21 Local Authorities (LAs)
Number of Participating Local Authorities	21
Number of Schools	291 (268 mainstream, 23 non-mainstream); up to 16 per LA
Primary Outcome(s)	Child protection (s.47) enquiries
Secondary Outcome(s)	Referrals to CSC, child in need (s.17) assessments, days in care, attendance (%), attainment at Key stages 3 and 4

Summary

This study will evaluate an intervention which bases social workers in schools (SWIS) with the aim they work more effectively with education colleagues and with children and families. The research design is a pragmatic cluster randomised controlled trial (RCT) and it builds on three smaller feasibility studies which found SWIS to be a promising approach and recommended it is explored further.

Social workers will work within schools across 21 Local Authorities (LAs) in England, and the study will evaluate the impact of the programme by comparing outcomes between schools that have a social worker and those that continue as normal, without a social worker based on the premises. Schools will be selected randomly from a pool of schools put forward by LAs to receive a social worker, so that we can be confident any differences we observe are due to the intervention and not another difference between the groups.

The primary outcome we are testing will be Child Protection (Section 47) enquiries, but we will also analyse other social care and educational outcomes to see what impact the intervention has on these. The study also includes an economic evaluation, which will calculate the costs of SWIS, and an implementation and process component which will explore how and why the intervention works as it does. Interim findings are expected in August 2021 and a final report will be published in August 2022.

Changes made in August 2021

Since the study protocol was published the time period of the intervention has been extended, and the Covid-19 pandemic has caused us to change some activities that were planned. This has affected the timing of analysis and reporting, and changed the nature of some data collection activities. This document has therefore been updated to reflect this. The main changes made are as follows:

- Timing of analysis and reporting updated to reflect the extended intervention period (e.g. meaning that the main impact analysis would be reported in 2022 rather than 2021, and the follow up analysis and final report would be published in June 2023 instead of June 2022).
- Changes to the data collection activities in the IPE, to reflect the fact that all case studies were forced online by the pandemic and conducted remotely. This meant that to date no observation or child interviews have taken place, and these are now scheduled for terms 4 and 5.
- Additional data collection activities in the two extra terms, e.g. interviews with key decision makers in local authority 'front door' teams.
- The use of the 2018/19 school year as a baseline, rather than the 2019/20 school year as originally planned. This is because the 2018/19 year is the most recent year not affected by the Covid-19 Pandemic and therefore a more realistic baseline to use.
- Changes to the economic evaluation to reflect discounting to costs beyond 12 months from the start of the intervention, to present values using nationally recommended discount rates.

Changes made in March 2022

In March 2022 the intervention period was extended for a second time, to the end of the 2021/22 school year. As with the first extension detailed above, the decision to extend the intervention and evaluation period for a second time was made by the funder. This meant social workers assigned to schools would be employed until August 2022, and work in schools until the end of the summer term (July 2022). This extension meant that timelines for the evaluation were extended, and dates for data collection and reporting were amended. In addition, the Department for Education commissioned us to undertake a nested study which focusses on domestic abuse. This was prompted by a policy interest in this issue, and a similar request was issued to researchers undertaking other relevant studies. This will use data collected as part of the main trial, in addition to some extra questions which will be added to interviews and surveys, and a short report will be submitted to the funder alongside the main trial report. Additional changes prompted by the March 2022 extension can be summarised as follows:

- Timing of analysis and reporting updated to reflect the extended intervention period (e.g. meaning that the main impact analysis will now be published in January 2023, and the follow up analysis and final report will now be published in January 2024).
- Termly surveys of social workers and school staff extended to a 6th term (one additional mail out of surveys)
- Retrospective interviews with team managers moved from term 5 to term 6
- Questions added to data collection tools (interviews schedules and surveys) which focus on domestic abuse.
- Additional interim report to the funder in May 2022.
- A short report added to the deliverables agreed, focussing on domestic abuse, to be submitted to the funder in November 2022 and published in January 2023.

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Introduction

This study will evaluate a programme that aims to embed social workers into schools (SWIS) so that they can work more effectively with education colleagues and with children and families. Education and Children’s Social Care (CSC) have an important inter-agency relationship, and both play a vital role in keeping children safe and promoting their wellbeing. Schools have long been among the major sources of referrals to CSC, contributing the second highest proportion (18%) of all referrals in 2018/19, behind the police (29%) (Department for Education, 2019). Yet the two agencies have different roles and priorities, and significant cultural and organisational differences add to the complexity of working effectively together. The impact of the Covid-19 pandemic is also putting additional pressure on both schools and social workers. Policymakers have been increasingly interested in finding ways to improve how education and CSC work together to respond to safeguarding concerns and protect children, and in this context these efforts are likely to intensify.

SWIS is a promising approach to doing this. The central idea is that having a social worker linked to and based within a secondary school can have a range of benefits. In particular, to improve the service delivered to children and families, enhance interagency working, reduce risks to children and lead to better outcomes. Building on a recent set of three pilot studies (Westlake et al, 2020), this project is an evaluation of the next stage of development of SWIS: a scaling up trial involving 21 local authorities and up to 297 schools across England (half of which will receive a social worker). Aside from the SWIS pilots, there are several examples of social workers working in schools (Wigfall, 2008; Bagley and Pritchard, 1998). However, the scale of the SWIS programme makes it the largest example of the approach by some distance.

The three pilot studies have been central to the development of SWIS. They took place in Southampton, Lambeth and Stockport in 2018-19. The pilot evaluation generated qualitative evidence that the intervention was positively received by schools, social care, children and families. A range of benefits were evidenced, including the opportunity for social workers to do more meaningful direct work with children, help a wider range of children and families than they otherwise would, and support schools to work through safeguarding issues. The evaluation also found indications of a reduction in child in need and child protection work. However, the strength of the evidence was limited by the quasi-experimental design, small sample size, low incidence rates of key outcomes, and short follow-up. Taken together, the qualitative and quantitative findings from the pilots prompted the Department for Education to fund this larger trial of SWIS. Delivered through What Works for Children’s Social Care, this scale-up allows us to understand the potential effectiveness of SWIS more robustly.

Intervention and Theory of Change

Following the pilot evaluations, an intervention manual has been developed (WWCSC, 2020). This distils the key messages from the pilots into a practical format designed to assist the 21 local authorities in delivering SWIS. Its stated aim is to provide “a framework to refine the SWIS Scale-up programme and encourage more consistent and effective integration of social workers into schools”, although the need for flexibility is acknowledged. The manual offers a number of recommendations for implementers. Among them, the following relate to programme delivery:

- Social workers should be embedded within secondary schools, but can work with feeder primaries.
- Social workers linked to schools should be experienced (being in practice for at least 2 years).
- The focus should be on statutory social work with additional opportunities for “preventative” aspects, which could involve “advising staff, families and young people” and working with children who are not at the threshold for formal involvement.

- Caseloads should be managed by the SWIS team manager and be in line with LA averages, and where possible the carry-over of existing caseloads prior to the launch should be minimal. To avoid disrupting existing relationships SWIS workers are expected to take on new cases.
- “Social workers should be embedded in schools as far as possible”, with their own office space in the school and opportunities to integrate.
- Face to face contact should be the basis for the intervention, in order to build strong relationships with school staff, children and families.

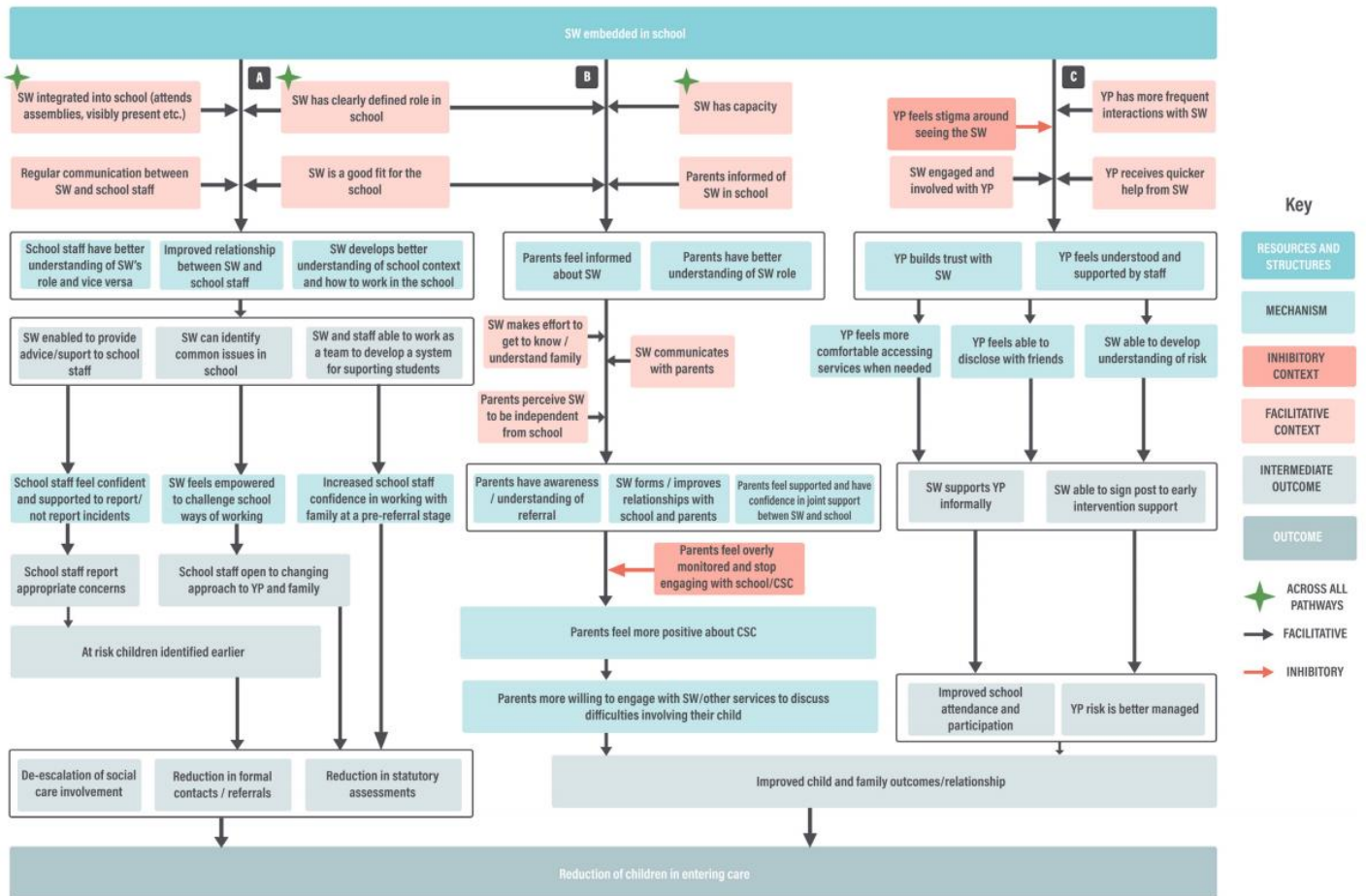
Logic model

A key output of the pilots was a logic model that describes three key pathways:

- A. Enhanced school response to safeguarding issues
- B. Increased collaboration between social worker and school staff, and parents
- C. Improved relationships between social worker and young people

Pathway A is based around regular communication between the social worker and school staff, and requires the social worker’s expertise and contribution to be welcomed by the school. The advice and support given to school staff increases their confidence in safeguarding issues and improves the quality of school referrals. Pathway B is about working directly with families and improving relationships between social workers and parents. Pathway C is more about working with children and young people directly. Frequent interactions with the social worker enable the young person to trust the social worker and to feel understood and supported. This is theorized to lead to improved school attendance and participation, better management of a young person’s risks and improved outcomes. In all three pathways, improved child and family outcomes are theorized to lead to a reduction of the number of children in care.

Figure 1: Logic Model



Impact Evaluation

Primary Research Question

What is the impact of SWIS in reducing rates of Section 47 enquiries (across 2 academic years, starting on 2nd September 2020 and measured 23 months later), compared to usual practice (**RQ1**)

Secondary Research Questions

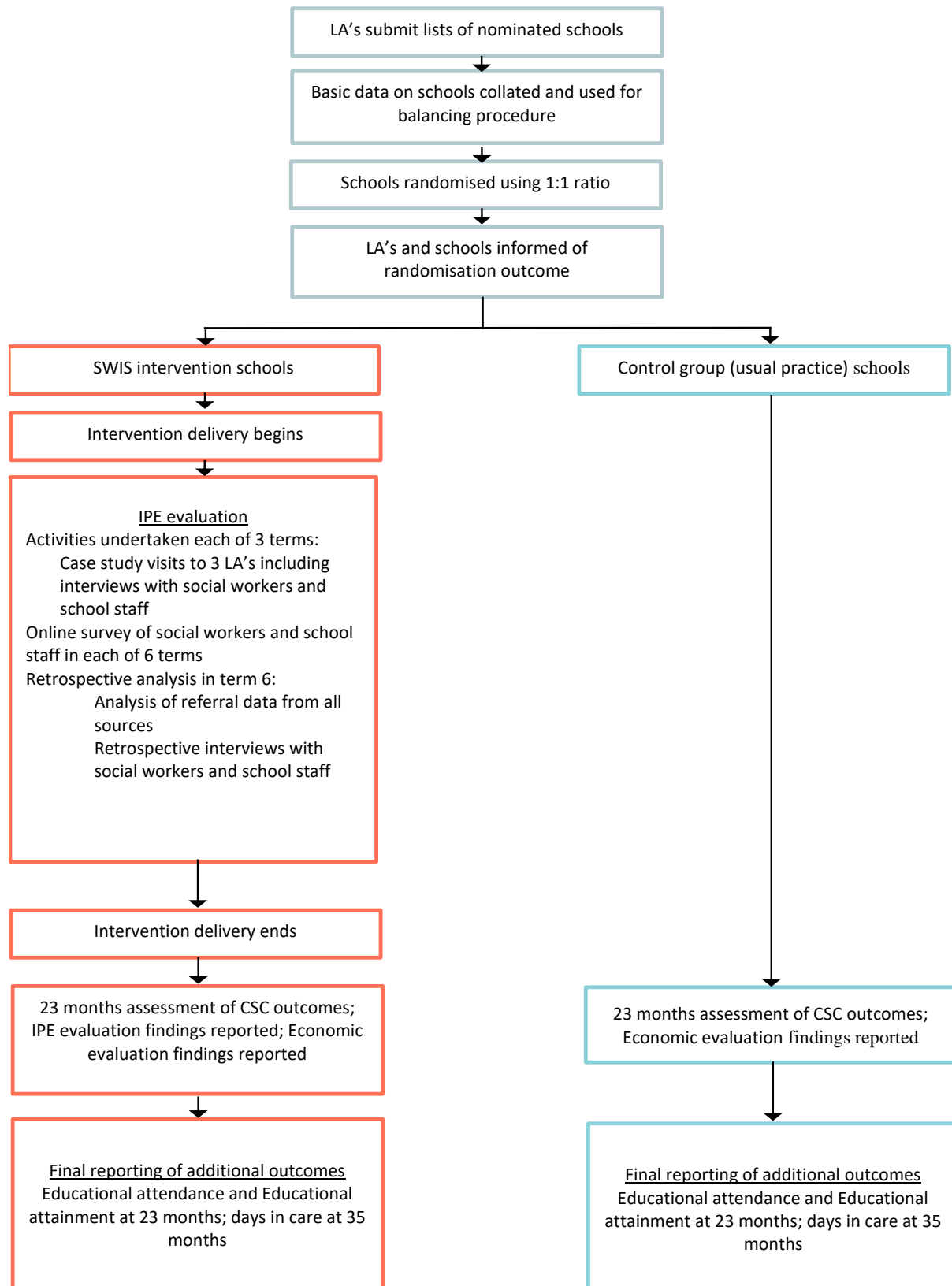
What is the impact of SWIS on:

- rates of referral to CSC and Section 17 assessments? (across 2 academic years, starting on 2nd September 2020 and measured 23 months later) (**RQ2**)
- the number of days children spend in care? (across 2 and 3 academic years, starting on 2nd September 2020 and measured 23 and 35 months later) (**RQ3**)
- educational attendance (recorded termly across 2 academic years, starting on 2nd September 2020) (**RQ4**) and attainment (recorded June 2021 and 2022)? (**RQ5**)

Design

Trial type and number of arms		Pragmatic cluster randomised controlled trial with two arms (one intervention and one control group)
Unit of randomisation		Schools (Mainstream secondary)
Primary outcome	variable	Child protection (s.47) enquiries (RQ1)
	measure (instrument, scale)	An integer measure that records a count of the number of s.47 enquiries that took place between the start of the academic year (2 nd September, 2020) and the date 23 months later
Secondary outcome(s)	variable(s)	Referrals to CSC, child in need (s.17) assessments, days in care
	measure(s) (instrument, scale)	An integer measure that records a count of the number of CSC referrals, s.17 assessments and days in care that took place between the start of the academic year (2 nd September, 2020) and the date 23 months later (and 35 months later for days in care)
	variable(s)	Attendance: Unauthorised absence (%) Attainment at Key Stage 4 (KS4): Attainment 8; EBacc average point score; English and maths grade 5 and above (%)
	measure(s) (instrument, scale)	Source: National Pupil Database Field Names: (attendance); ATT8; EBACCAPS_PUPIL; GCSE_ENG_95; GCSE_MATH_95

Figure 2: Trial schema diagram



Randomisation

The recruitment of schools will be completed for each local authority before that list of schools is passed on to the trial statistician for randomisation. The statistician is not directly involved in the recruitment of schools. Schools will be considered recruited once the LA confirms that they have agreed to take part, and randomised in blocks of up to 16, with each local authority acting as a block.

Mainstream schools will be allocated to SWIS or usual practice in a 1:1 ratio whilst minimising covariate imbalance within and across blocks using a balancing method for clusters (Carter and Hood, 2008).

Balancing algorithm

We will use the balancing algorithm for cluster-randomised trials with multiple blocks as proposed by Carter and Hood, 2008. For the first block, the standard imbalance metric (Equation 1 in Carter and Hood, 2008) will be used. The allocation of subsequent blocks will be conditional on blocks already allocated, using a modified imbalance metric (Equation 2 in Carter and Hood, 2008). Balancing variables will be school size (total number of students enrolled in year 7 and upward) and proportion of students eligible for free school meals (FSM). Both balancing variables will be weighted equally, and adjusted for in the final statistical analysis. The rationale for selecting these variables is as follows:

School size (total number of students enrolled)

The size of the school and number of students is likely to have an effect on how the social worker engages with and works within the school. In a larger school their time and resources may be spread across more students, and there may be more professionals to work with. It is reasonable to expect that this is a factor that will shape the implementation of SWIS and therefore balancing school size between groups is sensible.

Proportion of students eligible for free school meals

FSM are provided for children resident in lower income households. They are designed to ensure that all children have access to adequate nutrition when they are at school, and thereby improve health and educational outcomes. Eligibility for FSM is a reliable indicator that a child is from a low income household (Hobbs and Vignoles, 2009). This is important because the work of CSC tends to be focussed on children from lower income households, and it is these children who are more likely to require a service from CSC.

Sample size/Minimum Detectable Effect Size (MDES) Calculations

MDES (Risk ratio)	0.832 (reduction from 12.6 to 10.48 enquiries per 1000 pupils per school year)	
Alpha	5%	
Power	80%	
One-sided or two-sided?	Two-sided	
Sample Size (Mainstream schools)	Intervention	140
	Control	140
	Total	280

Assuming an average of 925 students per school, an average base rate of 12.6 s.47 enquiries per 1000 students per school year under usual practice conditions, and a between-school coefficient of variation of 0.45 within LAs (these estimates are all based on comparator school data from the three pilot studies in Lambeth, Stockport and Southampton (Westlake et al, 2020)), randomising 140 mainstream schools to each group provides 90% power to detect a decrease in rates from 12.6 to 10.48 per 1000 pupils per school year (i.e. a rate ratio of 83.2%). This is using a two-sided 5% type I error level when using a Poisson regression model accounting for cluster randomisation (Hayes & Bennett 1999). R code is provided in the Appendix.

Outcome Measures

Primary outcome:

- **Child protection (Section 47) enquiries**

Child protection (s.47) enquiries are investigations CSC carry out when they have “reasonable cause to suspect that a child who lives, or is found, in their area is suffering, or is likely to suffer, significant harm” (Children Act, 1989). This is a key point in the work of CSC; an enquiry would normally involve an assessment of the child’s needs and the ability of family members of carers to meet them. Social workers would normally interview family members, children (if they are old enough), and use information from other agencies such as schools and health. This data will be collected from LA CSC departments, based on a data sharing agreement between each LA and the research team.

Child protection (s.47) enquiry starts will be recorded as a binary (no enquiry/ enquiry started) variable. The source of the data is local authority CSC departments. The s.47 start date is routinely recorded by LAs as it is required for the Children in Need Census. If the s.47 start date lies between and including the start date of the academic year (2nd September, 2020), and 31st July 2022, it will be coded 1, otherwise it will be coded 0. Enquiries will be recorded at the individual-level, then aggregated at the school-level and shared with the research team. This aggregated count variable will be our outcome measure.

Secondary Outcome Measures

The following CSC data will be collected from LA CSC departments, based on a data sharing agreement between each LA and the research team.

- **Referrals to CSC**

Referrals are made to CSC when someone thinks a child is at risk, and schools are typically the second highest agency referrer (after the police). CSC undertake various activities to ascertain whether a full ‘Child in Need’ assessment should be carried out under Section 17 of the Children Act 1989. A reduction in referrals would indicate reduced concern about children. We define a referral according to the DfE guidance to LA’s for completing CiN census returns:

“a request for services to be provided by local authority children’s social care via the assessment process outlined in Working Together 2018 and is either in respect of a child not previously known to the local authority, or where a case was previously open but is now closed.” (DfE, 2018; p.34).

Referral dates will be recorded and referrals will be recorded as a binary (no referral/ referral) variable. If referral date lies between and including the start date of the academic year (2nd September, 2020), and 31st July 2022, it will be coded 1, otherwise it will be coded 0. These will be recorded at the individual-level, then aggregated at the school-level and shared with the research team. This aggregated count variable will be our outcome measure.

- **Child in need (s.17) assessments**

A Child in Need assessment aims to identify the needs of a child or children within a family, and ascertain what support the family needs to meet them. Similarly to referrals, reduced Child in Need assessments could be taken to indicate that fewer children are thought to be at risk. Start dates for assessments will be recorded and assessments will be recorded as a binary (no assessment/ assessment) variable. If the start date for assessment lies between and including the start date of the academic year (2nd September, 2020), and 31st July 2022, it will be coded 1, otherwise it will be coded 0. These will be recorded at the individual-level, then aggregated at the school-level and shared with the research team.

- **Days in care**

Numbers of children in care have grown substantially in recent years, and policymakers are seeking to safely reduce the use of care. Across the cohort, reducing the number of days children spend in care would therefore also indicate that risks to children are reduced. We will record the total number of days children spend in care for a period from the start date of the academic year (2nd September, 2020). We will create two variables, one that counts the number of days spent in care between and including this date and 31st July 2022, and the other that counts the number of days spent in care between and including 31st July 2023. The inclusion of days in care at the 35 month timescale (as well as measuring it along with other CSC outcomes at 23 months) is because impact on this outcome is more likely to become clear with a longer follow up. This will be recorded at the individual level, then aggregated at the school-level and shared with the research team. This aggregated count variable will be our outcome measure.

The following data will be collected from the National Pupil Database (NPD) and made available anonymised at an individual level (identifiers will be stripped from the dataset) (Jay et al, 2019). One application will be made to the NPD in early 2023 to request the following datasets.

- **Educational attendance: unauthorised absences (%)**

This is the percentage of sessions children are absent without being authorised, out of the number of sessions possible. It is an important variable because regular or long-term non-attendance has been consistently linked to poorer outcomes. For example, research by the Department for Education (2016) showed a negative link between attendance and attainment, meaning “every extra day missed was associated with a lower attainment outcome” (DfE, 2016; pp4). Educational attendance is available via the absence dataset. For the 2020/21 academic year, these data will be released by the NPD in March 2022, and for the 2021/22 year they will be released from October 2022 (unamended) to March 2023 (final); access will be requested for the final datasets in April 2023. Defined by the number of sessions missed due to unauthorised absence per term (Autumn, Spring, Summer) out of the number of sessions possible per term. The field names for this outcome will include:

UnauthorisedAbsence_Spring;

UnauthorisedAbsence_Autumn;

UnauthorisedAbsence_Summer.

- **Educational attainment at Key stage 4**

Key stage 4 (General Certificate of Secondary Education: GCSE) results are the key educational outcome measure for secondary school pupils. They are regularly used as part of school regulatory and inspection procedures, and to predict future outcomes for individuals. Educational attainment at Key Stage 4 (KS4) for the 2020/21 academic year will be released in April 2022 for analysis, and in April 2023 for the 2021/22 academic year; access will also be requested in April 2023. The following outcomes will be reported for all pupils completing

GCSE exams in 2021 or subject to equivalent grading exercise (a subset of those pupils included in the trial):

- Attainment 8
 - EBacc Average Point Score
 - % English and maths, grade 5 and above. This will be a binary variable coded 1 if the pupil achieved a level 5 or higher in both English and maths, and coded 0 otherwise
- The field names for these outcomes will include: ATT8; EBACCAPS_PUPIL; GCSE_ENG_95; GCSE_MATH_95

Timescales for data availability

As per the current timescales for the trial it will be possible to access Key Stage 4 attainment and attendance data for the trial cohort and report in November 2023.

Figure 3: Educational outcome data availability timeline

(Updated in light of extension)

Milestone	2021												2022												2023											
	M	J	J	A	S	O	N	D	J	F	M	A	M	J	J	A	S	O	N	D	J	F	M	A	M	J	A	S	O	N						
Month no.	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38						
Intervention end	█														█																					
GCSE data available												█													█											
Absence data available (final)											█												█													
Days in care 2nd point (12m after intervention end)											█																	█								
First report submitted (interim: IPE and exploratory subgroup)				█																																
Second report submitted (main: CSC outcomes & IPE with economic analysis)				█																		█														
Third report submitted (final: educational outcomes & days in care)															█															█						

Original / Updated

Data Management

A detailed data management plan will be developed by the trial team for internal use, based around the following principles.

CSC data

Data sharing agreements will be set up between LAs (Data Controller) and Cardiff University to enable sharing of data related to CSC. Data fields will be included in this data sharing agreement, data will contain no identifiers except for a school ID and/or trial allocation. A data lead will be identified at each LA, the data manager will liaise with each data lead to confirm required fields. Building on our learning from the pilot, each LA will pilot data transfer during the set-up phase to ensure the data can flow and is in the correct format. These checks will be detailed in the data cleaning plan. Once any issues are addressed in this pilot, data will be shared soon after the intervention ends in July 2022 (23 months) and again in July 2023 (35 months). Data will be reported by school and by month and

provided to the Cardiff team in a proforma developed and agreed during the pilot. Data will be aggregated, and no individual level data will be sent. These data will be securely transferred to Cardiff University and checked by the data manager. All data will be stored on Cardiff University servers in restricted folders available only to those on the trial team who require access. This will be detailed in the delegation log.

LA cost data

As per the data sharing agreement with each LA, data on costs will be sent to Cardiff using the same proforma developed for the CSC outcomes. This will be checked and prepared for onward sharing to the health economics team based in the University of Oxford.

Education data

Education attainment and attendance data will be accessed via the National Pupil Database (NPD), Dept. For Education. LAs will send school identifiers (school number, Establishment number, URN) and trial allocation (control; intervention) to NPD to enable them to identify pupils in those schools.

Education data for those pupils will be made available to the study team via their Secure Research Service – a remote access data safe haven (hosted by the Office for National Statistics) as per their data sharing processes (DfE, 2020).

Data security

All data will be stored in a secure manner and processed in accordance with data protection legislation (in accordance to GDPR) and Good Clinical Practice (GCP)

Impact Analysis Plan

Primary analysis

All primary analyses will be ‘intention to treat’ (i.e. schools will be analysed in the groups to which they were randomised, regardless of adherence to the intervention) and missing outcome data will not be replaced. We will perform null imputation for missing covariates, where we will replace missing values with 0, and create a dummy indicator for the covariate coded 1 if the value was missing, and 0 otherwise, and include the dummy in the regression. Statistical tests and confidence intervals (CI) will be two-sided. Comparisons between groups will be presented with 95% CIs wherever possible.

A quasi-Poisson regression model will be fitted with fixed effects for LAs and cluster robust standard errors reflecting the clustering structure (schools within LAs) to compare rates of s.47 enquiries at 19 months (i.e. using data for the whole school year) by arm, using the number of students per school as the exposure scaling variable, and s.47 enquiries for the 2018/19 academic year (baseline) and % eligible for FSM as fixed-effect covariates. Poisson regression is a standard technique for modelling incidence rates, and using a quasi-likelihood model accounts for possible over- or underdispersion i.e. variance in the data that is higher or lower than expected under the standard Poisson model. Allocation will be included in the model as a fixed effect, and the intervention effect (model coefficients transformed into rate ratios) will be presented as point estimate with 95% CI and p-value. If the estimated rate is lower in the SWIS arm than in the comparator arm and the 95% CI around the estimated rate ratio excludes the null effect (rate ratio=1), effectiveness of SWIS at the 5% level will be concluded. A fixed-effects model with cluster robust standard errors, rather than a multi-level model with random effects, was chosen as the primary analysis method at the request of the funder, mainly for consistency with other projects in their portfolio and compliance with their statistical analysis guidance¹.

¹ Due to be published on their website soon (www.whatworks-csc.org.uk), and available on request from research@whatworks-csc.org.uk

Secondary analysis

Secondary and subgroup analyses of the primary outcome

A secondary analysis will use per-term outcome data and include term as an additional covariate in the model to explore potential implementation effects and/or seasonality. Another secondary analysis will assess the hypothesised mediators of change outlined in the Westlake et al (2020) logic model at the 23-month follow-up by fitting an interaction term between allocation and category of implementation fidelity (a Gold, Silver, Bronze categorisation will be developed based on a re-analysis of pilot data and other insights from Strand 2 – see p.15 below. Any other subgroup analyses (e.g. by age group) will be agreed with service user input. The p-values generated from these secondary and subgroup analyses will be adjusted for multiplicity using Hochberg's step-up procedure.

Secondary outcomes

Secondary outcomes will be analysed in a similar way as the primary outcome, by fitting regression models with cluster robust standard errors depending on the type of outcome: quasi-Poisson for rates (referrals to CSC, s.17 assessments) and linear for continuous variables (days in care, educational attendance and attainment). For the school-level variables (referrals to CSC, s.17 assessments, days in care) we will include the same fixed-effect covariates in the model as for the primary outcome (allocation, LA, baseline outcome from 2018/19, % eligible for FSM) and also use the number of students per school as the exposure scaling variable. For the student-level variables (educational attendance and attainment) we will additionally include gender and ethnicity as covariates, and the baseline outcomes used will be 2018/19 attendance (for attendance) and KS4 results (for attainment). The p-values generated from the secondary outcome analyses will be also adjusted for multiplicity using Hochberg's step-up procedure.

Sensitivity analyses

To assess the impact of non-compliance (if present), we will exclude intervention arm schools that did not adopt the intervention at all and then repeat the primary analysis, and we will also perform a complier average causal effect (CACE) analysis. If more than 5% of outcome data is missing, we will consider repeating the primary analysis after multiple imputation. We will also fit a two-level mixed-effects model with random LA effects as an additional sensitivity analysis. No multiplicity adjustment will be required for any of these robustness checks.

Implementation and Process Evaluation

Aims

The implementation and process evaluation (IPE) will explore how the scale up is implemented across the LAs, and the extent to which this is as intended. To assess fidelity, we will conduct a re-analysis of pilot data, alongside the 'core principles' stated in the intervention manual, to develop Gold, Silver and Bronze definitions of SWIS. Definitions will be based on adherence to the manual, including the amount of time social workers spend in the school and how far they are thought to be integrated into the school (Carroll et al, 2007). This will inform secondary analysis in the impact evaluation by identifying subgroups, and this protocol will be updated with details of this categorisation in December 2020 or January 2021 when the re-analysis is complete (and prior to impact analysis).

Research Questions

IPE research questions explore three areas - (1) implementation, (2) mechanisms and (3) impact:

Figure 4: IPE areas of enquiry

Area	RQ #	Question	Objective	Main data sources
1	RQ 6	Is SWIS implemented as intended?	Assess reach, scalability and sustainability at defined levels of fidelity (Gold, Silver, Bronze). Explore perceptions of stakeholders and acceptability of SWIS.	Re-analysis of pilot data ² , survey, case studies (including the casework logs)
2	RQ 7	What evidence is there for the mechanisms of change identified in the logic model?	Review, develop and test existing logic model	Case studies, survey
3	RQ 8	How does SWIS impact the wider social care system?	Interrogate wider system impact and unintended consequences	Referral data, case studies, casework logs

Design and Methods

The ongoing and developing situation brought about by the Covid 19 pandemic is likely to create challenges for the study. In particular, we may find that our ability to carry out fieldwork activities and collect data in person is restricted or blocked. Therefore, the IPE uses a range of remote and in-person methods that can be adjusted if necessary (for example, to shift the balance towards a greater reliance on remote methods if this is necessary). We expect the first set of case study ‘visits’ (in November 2020) will be undertaken virtually, but hope that actual visits may be possible for further case study activities³. The following linked data collection activities are designed to inform each other:

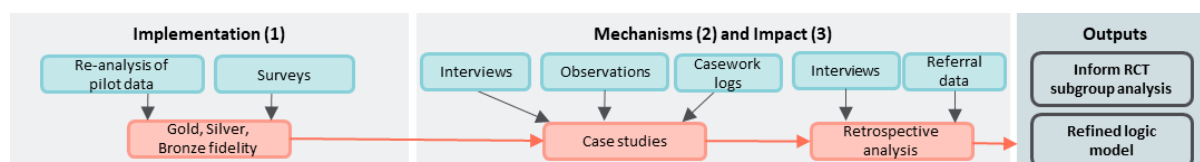
- **Online surveys** (during terms 1-6)
 - a. Of social workers and school staff (six surveys, one at the end of each term, all school SWs and all DSLs in intervention group invited; n=148 social workers and circa 100- 200 school staff) to gather programme-level data on implementation and attitudes and to assess how SWIS is implemented in each school (RQ6 and 8).
 - b. Of students (two surveys, during terms 3 and 5-6), designed to measure awareness of the social worker in school and attitudes towards having a social worker in school. The surveys will be short, taking no more than fifteen minutes to complete, and administered via Qualtrics online survey platform. Questions will be developed as part of the re-analysis of pilot data.
- **Purposive case studies** (n=9; one set of 3 per term in terms 1-3). The first set (undertaken in November 2020) will be selected to exhibit a range of LA characteristics (i.e. unitary, county, London borough, and geography). Case studies in terms 2 and 3 will target sites that demonstrate a range of implementation levels (informed by the survey data), to identify where SWIS is being implemented well, where it is not, and factors shaping implementation (RQ6, 7 and 8). Each case study will be based around a virtual site visit conducted remotely to carry out:
 - a. Interviews with SWs (n=8) and school staff (n=3-5). Interviews will usually be recorded on an audio device.
 - b. Documentary analysis of casework logs. Social workers (5-8 at each LA) will be asked to keep activity logs of the work they do for short periods to ascertain the key tasks they undertake. This will complement the observation detailed below and may be used as an alternative if a move to remote methods are required.

² The pilot data will only be used to develop the definitions of degrees of implementation.

³ A risk assessment for any such activities will be submitted to Cardiff University School of Social Sciences and signed off by the Head of School or his appointed delegate.

- **Interviews with children** (during terms 4 and 5 – 20-30 across 4-5 LAs)
Interviews with children will take place to understand their experiences of SWIS and attitudes towards having a social worker in school
- **Retrospective analysis** (during term 6)
 - a. Analysis of referral data (from local authority case management systems) from all sources to assess displacement of referrals from schools to other agencies.
 - b. Interviews with social workers and school representatives, purposively sampled to explore lessons learned at different levels of implementation.
 - c. Interviews with key decision makers in CSC ‘front door’ teams to understand the impact of SWIS on LA referral pathways

Figure 5: Implementation and process evaluation summary



IPE Analysis Plan

Qualitative data will be analysed using deductive and inductive coding (Silverman, 2011). Initial coding will use an a priori scheme based on the pilot study findings and aligned with the research questions as a means of organising the data for subsequent interpretation. We will undertake a thematic content analysis of the qualitative data, in which themes will be identified and organised into an analytic framework. We expect this to include themes from the pilots, which may be further developed and refined, and additional ‘new’ themes that arise from this study. We will use this to further refine the logic model presented in the pilot evaluation. NVivo 12 software will be used to process this data, each portion of analysis will be reviewed by a second researcher and discussed within the team to ensure rigour. Quantitative data will be analysed using statistical methods to give descriptive findings, and where appropriate we will explore relationships between these data (for example, to examine trends in referral data over time and between sources).

Economic Evaluation

Research Question

The economic analysis (CEA) will compare the cost-effectiveness of SWIS, versus usual practice. The CEA will be carried out from a social care perspective. The research question will be: What is the additional cost associated with SWIS and is it justified by improvements in student outcomes?

Outcome measures

The outcomes listed above will be used in the CEA.

Analysis

Costs related to the delivery of the programme will consist of social workers’ direct staff costs, including supervision, recruitment and training costs from the LA. Staff costs of managers and others involved in implementing the intervention will also be included. Data on time spent by social workers and other staff will be obtained from LAs and individuals, and the location, purpose and duration of

meetings and training activities will all be recorded. The currency used will be expressed in British Pound Sterling (£), for a base cost year 2020/2021. All costs accrued beyond 12 months' follow-up will be discounted to present values using nationally recommended discount rates (NICE, 2013). Unit costs will be obtained from routine sources such as the Personal Social Services Resource Unit (PSSRU), Office for National Statistics (ONS), or through Spinal Column Points Salary Scales 2019/2020 for each local authority. Staff costs will be based on the recorded number of social worker hours multiplied by their hourly wage. Costs of recruitment, setting up training programs and meetings, and travel costs will be obtained from costs and expenses forms.

Results

Results will be expressed in terms of incremental cost per unit changes in each of the study primary outcomes. Bivariate regression of costs and primary outcomes, with multiple imputation of missing data, will be conducted to generate within-trial estimates of incremental cost-effectiveness associated with SWIS. Probabilistic sensitivity analysis will assess any uncertainty of the CEA results, and results will be summarised using cost-effectiveness acceptability curves.

Ethics and Participation

Ethical approval has been obtained from the ethics committee of the School of Social Sciences, Cardiff University. This involved detailed consideration of key ethical issues, including participation, consent, randomisation, and the impact of Covid-19 on data collection. Agreeing to take part in the study is a condition of local authorities' participation in the programme, and all schools have also agreed to take part. For direct data collection (e.g. surveys, interviews etc) informed consent will be completed prior to all participation by individuals. Informed consent will not be required for aggregated data or the NPD data that will be accessed through the ONS's Secure Research Service.

Registration

The trial will be registered with the Open Science Framework (<https://osf.io/>) and the 'International Standard Randomised Controlled Trial Number (ISRCTN) registry (<https://www.isrctn.com/>)

Funding

The trial is funded by What Works for Children's Social Care

Data Protection

The data we collect will be used solely to address our research questions. The identities of individuals involved in the study will be kept confidential and data will only be used for research purposes. During consent processes, participants will be informed of their right not to take part or to withdraw at any time. All data will be stored securely on Cardiff University and Oxford University servers, with the exception of the NPD data which will be stored in the Office for National Statistics Secure Research Service (ONS-SRS). At the end of the study, anonymised quantitative data will be transferred to the WWCS secure data archive, which is intended to be hosted by the ONS-SRS.

Roles and Responsibilities

The project team is led by CASCADE and Centre for Trials Research (CTR) with expertise around economic evaluation from the University of Oxford:

David Westlake, Principal Investigator, Cardiff University

Dr. James White, Co-Investigator, Cardiff University

Professor Donald Forrester, Co-Investigator, Cardiff University

Dr. Philip Pallmann, Co-Investigator, Cardiff University

Dr. Fiona Lugg-Widger, Co-Investigator, Cardiff University

Professor Stavros Petrou, Co-Investigator, University of Oxford

Timeline

Dates	Activity	Staff responsible/ leading
September 2020	Schools randomised to either SWIS (intervention) or control	PP
April 2021	Interim analysis (IPE)	DW
March 2021	Interim report to funder	DW
August 2021	Interim report to funder	DW
May 2022	Interim report to funder	DW
November 2022	Main report to funder of CSC outcomes and IPE	PP, DW, JW
November 2022	Summary report on SWIS and Domestic Abuse to funder	DW
January 2023	Main report published (CSC outcomes and economic evaluation)	PP, JW, SP
May 2023	Impact analysis (educational outcomes)	PP, JW, FL-W
January 2024	Follow up report published	DW

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Wigfall, V. Hollingworth, K. and Boddy, J. (2008) London Borough of Islington: Evaluation of the Social Work in Schools Pilot Project Final Report, Thomas Coram Research Unit, Institute of Education, University of London.

Databases and resources

National Pupil Database (NPD): <https://www.gov.uk/government/collections/national-pupil-database>

Personal Social Services Resource Unit (PSSRU): <https://www.pssru.ac.uk/project-pages/unit-costs/>

Office for National Statistics (ONS):
<https://www.ons.gov.uk/peoplepopulationandcommunity/healthandsocialcare>

Appendix 1

The following R code was used to calculate the MDES for 80% power:

```
# alpha = two-sided type I error rate
# beta = power
# lambda0 = control group rate
# lambda1 = intervention group rate
# y = person-years of follow-up per cluster
# k = between-cluster coefficient of variation
clusters <- function(alpha, power, lambda0, lambda1, y, k){
  clusters <- 1 + (qnorm(1 - alpha/2) + qnorm(power))^2 * ((lambda0 + lambda1)/y + k^2 *
  (lambda0^2 + lambda1^2))/(lambda0 - lambda1)^2
  return(clusters)
}
clusters(alpha=0.05, power=0.8, lambda0=0.0126, lambda1=0.010483, y=925, k=0.45)
0.010483/0.0126
```