EVALUATION REPORT

Trauma Informed Schools UK (TISUK)

Efficacy Trial Report

Ipsos UK, The University of Kent, TONIC

Principal investigators: Dr Facundo Herrera, Jemuwem Eno-Amooquaye, Kim Bohling, Marzieh Azarbadegan, Luisa Gomes, Dr Amanda Carr, Prof. Simon Coulton

About the Youth Endowment Fund

The Youth Endowment Fund (YEF) is a charity with a mission that matters. We exist to prevent children and young people from becoming involved in violence. We do this by finding out what works and building a movement to put this knowledge into practice.

Children and young people at risk of becoming involved in violence deserve services that give them the best chance of a positive future. To make sure that happens, we'll fund promising projects and then use the very best evaluations to find out what works. Just as we benefit from robust trials in medicine, young people deserve support grounded in evidence. We'll build that knowledge through our various grant rounds and funding activities.

And just as important is understanding children and young people's lives. Through our Youth Advisory Board and national network of peer researchers, we'll ensure they influence our work and that we understand and are addressing their needs. But none of this will make a difference if all we do is produce reports that stay on a shelf.

Together, we need to look at the evidence and agree what works, then build a movement to make sure that young people get the very best support possible. Our strategy sets out how we'll do it. At its heart, it says that we will fund good work, find what works and work for change. You can read about it here.

For more information about the YEF or this report, please contact:

Youth Endowment Fund
C/O Impetus
10 Queen Street Place
London
EC4R 1AG

www.youthendowmentfund.org.uk

hello@youthendowmentfund.org.uk

Registered Charity Number: 1185413

Contents

About the evaluator

The evaluation was undertaken by an independent, multidisciplinary research consortium comprising Ipsos UK, TONIC and the University of Kent. The team brings extensive expertise in conducting randomised controlled trials and fieldwork within educational and mental health settings, with particular experience in research involving vulnerable and at-risk young people.

The project was directed by Dr Facundo Herrera of Ipsos UK, who served as Principal Investigator. Kim Bohling was the Quality Director for this report. The core evaluation team included Marzieh Azarbadegan as Project Manager, overseeing the day-to-day coordination and delivery of all evaluation activities; Luisa Gomes as Survey Lead, responsible for the design and implementation of all quantitative data collection and Jemuwem Eno-Amooquaye as Implementation and Process Evaluation Lead, leading the qualitative research components, including interviews, observations and case studies. This multidisciplinary team ensured the robust integration of quantitative and qualitative methods throughout the evaluation.

For further information regarding this evaluation, please contact the Principal Investigator at Facundo.herrera@ipsos.com.

Acknowledgements

We would like to express our sincere gratitude to the Youth Endowment Fund for their invaluable guidance, support and expertise throughout this evaluation. Their commitment to generating rigorous evidence and improving outcomes for young people has been instrumental in shaping this research.

We extend particular thanks to Peter Sakis, who served as Project Manager during the crucial initial stages of this evaluation. His leadership in trial set-up, protocol development and the establishment of the baseline survey laid the foundation for this rigorous evaluation.

We extend our deepest appreciation to the Trauma Informed Schools UK delivery team members for their exceptional dedication and professionalism in implementing the intervention across participating schools. Their flexibility, expertise and collaborative spirit throughout the evaluation process made this complex trial possible.

Most importantly, we thank all the schools, senior leaders, teachers, support staff and young people who generously gave their time to participate in this evaluation. Despite the considerable pressures facing schools, their willingness to engage with surveys, interviews, observations and case studies has provided invaluable insights into trauma-informed practice in educational settings. This evaluation would not have been possible without their commitment to improving support for vulnerable children and young people.

Executive summary

The project

The Trauma Informed Schools UK (TISUK) training and implementation programme aims to reduce pupils' behavioural difficulties and improve their mental wellbeing by supporting secondary schools to adopt whole-school approaches to trauma-informed practices. It is intended to benefit all pupils while also strengthening schools' abilities to respond to children who have experienced trauma. Trauma happens when someone experiences something deeply distressing or harmful that has lasting effects on their wellbeing. Trauma-informed practice means shaping services and support in ways that recognise this impact and aim to avoid causing further harm. The programme was delivered by TISUK over four school terms (from January 2024 to March 2025), and the implementation with pupils occurred in the final six to seven months, following the initial training period. It combined multiple strands of support, including whole-staff training delivered in two 3-hour sessions, two days of training for senior leadership, 11 days of training for a small group of school staff, workshops with an experienced professional to help leaders embed policy and cultural changes, three network meetings to share experiences across schools, four days of workshops for staff to support each other and share best practices and webinar resources for staff and pupils.

The Youth Endowment Fund (YEF) and the Home Office funded a randomised controlled trial of TISUK. The trial aimed to establish whether the programme reduces pupils' externalising behaviour, measured using the Strengths and Difficulties Questionnaire amongst the Year 8 cohort. It also tested impacts on secondary outcomes, such as internalising behaviour, prosocial behaviour, psychological distress, overall wellbeing and school connectedness, school-level exclusions, suspensions and attendance. The evaluation also aimed to measure staff outcomes, such as staff wellbeing and attitudes to trauma-informed care. 78 secondary schools (across 10 areas in England) and 12,725 Year 8 pupils took part in the trial: 40 schools received the TISUK programme, and 38 continued with business-as-usual practices. The evaluation included a quasi-experimental design study that aimed to evaluate a targeted component of the programme that provided one-to-one or small-group support to children with higher needs. The study also included an implementation and process evaluation to examine barriers and facilitators and how both pupils and staff experienced the programme. This drew on interviews with school staff and TISUK trainers, observations of training delivery and school case studies, including pupil focus groups. The study ran from June 2023 to December 2025. Of the 12,725 pupils involved in the trial, 65% were White; 11% Asian or Asian British; 7% mixed or multiple ethnic groups; 6% Black, Black British, Caribbean or African; 3% from another ethnic group; and 9% with no ethnicity data recorded.

Key conclusions
TISUK had a **small positive impact** on children's externalising behaviours. After the programme, children who received TISUK had lower levels of externalising behaviours compared to those who did not receive TISUK. This result has a **very low** security rating.
TISUK had mixed results on secondary outcomes: a small positive impact on children's internalising behaviours, emotional and behavioural difficulties and psychological distress; no effect on prosocial behaviours or wellbeing; and a small negative impact on school connectedness. TISUK had a large positive impact on staff attitudes towards trauma-informed care and on staff wellbeing. These are secondary outcomes and should be interpreted with caution. There is also statistical uncertainty regarding almost all outcomes.
These findings have a very low security rating because the trial suffered from a very high level of attrition. 27% of the schools and 69% of the children who started the trial were not included in the final analysis. All impact findings should therefore be treated with caution.
Attendance at TISUK training was generally high, although some schools reported difficulties sustaining consistent participation due to operational pressures, lengthy sessions and staff turnover. Staff valued the programme and reported increased confidence. Perceived benefits included a shift from punitive approaches towards more relational and restorative practices, reported reductions in exclusions and improvements in behaviour.
The evaluation found evidence of positive effects on staff outcomes, but this did not translate into measurable improvements in pupil outcomes within the planned timeframe.

YEF security rating

These findings have a **very low** security rating. The trial was a well-designed two-armed randomised controlled trial, but it experienced a very high level of attrition: The trial did not include as many children as originally intended, reducing the trial's ability to accurately estimate the impact of TISUK. 69% of the children involved in the trial at the start did not complete the endline data collection, due to school dropout from the evaluation (largely due to staffing and capacity issues) and challenges in engaging schools to complete endline measures. We do not know whether the effect found for TISUK would be the same if the children missing from the final analysis were included.

Interpretation

These findings have a **very low** security rating, so they should be interpreted with caution. Within this uncertainty, the TISUK programme had a small positive impact (on the boundary with no impact) on children's externalising behaviours. After the programme, children who had received it had slightly lower levels of externalising behaviours compared to children who did not receive it.

TISUK had a **small positive impact** on children's internalising behaviours, emotional and behavioural difficulties and psychological distress; no effect on prosocial behaviours or wellbeing; and a small negative impact on school connectedness. TISUK had a large positive impact on staff attitudes towards trauma-informed care and on staff wellbeing. These are secondary outcomes and should be interpreted with caution. There is also statistical uncertainty regarding almost all outcomes. The analysis on the impact of TISUK on school suspensions and exclusions and staff sickness was affected by attrition, low response and data quality, and it is not considered sufficiently reliable. The evaluation of the programme's targeted component found slightly worse outcomes for targeted children, although findings should be interpreted with caution.

Attendance at the TISUK training activities was generally high. 75% of the participating schools recorded very high attendance, although some schools reported challenges with consistent attendance due to daily operational pressures, long training sessions and staff turnover, within tight project timelines. The programme responded to capacity challenges by shifting from the intended fewer longer blocks to more shorter interactions. Online webinars were the most common activity missed, with one-third of participants not attending any. Staff valued the programme and reported increased confidence and capability in their roles. Perceived benefits included a shift from punitive approaches towards more relational and restorative practices, alongside reported reductions in exclusions and improvements in attendance and behaviour. The evaluation identified several implementation challenges. In some schools, trauma-informed practice was not fully embedded, particularly where senior leadership support was limited or where staff turnover and inconsistent attendance disrupted continuity. Engagement varied across staff groups, with some resistance linked to workload pressures or scepticism about the approach. More broadly, cultural change requires sustained time and effort before improvements are evident at the pupil level.

This evaluation is one of the first robust evaluations of trauma-informed practice in education in the UK, and it contributes to filling existing evidence gaps. This evaluation adds to the evidence that caution is warranted when commissioning trauma-informed programmes to reduce youth violence. Whilst such approaches may improve understanding of trauma, this does not necessarily translate into improved outcomes for children. Other interventions may offer greater potential to reduce children's involvement in violence. This report and the primary and secondary outcome findings only present the findings of one study. When considering implications, frontline professionals, policymakers and service commissioners should carefully consider the process evaluation, the wider evidence base and their own professional judgement.

Summary of impact

Outcome	Effect size (95% confidence interval)	Impact	Evidence security	No. of children	p-value
Externalising behaviour	-0.02 (-0.10, 0.06)	Small positive		3685	0.64

Introduction

Background

Context

Numerous scientific studies have confirmed the substantial impact of multiple adverse childhood experiences (ACEs), such as abuse or neglect, on children and young people (CYPs; Basto-Pereira et al., 2022; Fujiwara, 2022; Meeker et al., 2021; Webster, 2022). These experiences are associated with an increased risk of developing mental health problems, behavioural issues, social difficulties and learning difficulties as well as with contributing to poverty, long-term unemployment, self-harm and engagement in violent and criminal activities (Baglivio, Wolff, DeLisi & Jackowski, 2020; Felitti et al., 1998; Giampetruzzi et al., 2023; Li et al., 2023; Neil et al., 2022; Qu et al., 2024; Yohros, 2023).

This issue is particularly concerning in the UK, with over two million young people facing mental health difficulties, leading to more than 66,000 recorded referrals to Child and Adolescent Mental Health Services (CAMHS) in 2022. Approximately 25% of those referred to CAMHS present with suicidal ideation, yet the threshold for accessing these services remains prohibitively high – outside of neurodevelopmental assessment pathways, young people often need to have made an active suicide attempt before they can qualify for a referral (Gilmore et al., 2022). The End the Wait Campaign reported that 25% of young people attempted suicide whilst waiting for mental health support (Young Minds, 2023). This highlights not only the dire state of waiting lists in many UK areas but also the systemic barriers that prevent early intervention for vulnerable young people.

ACEs can have long-lasting effects and increase the risk of serious, violent and chronic juvenile offending due to trauma-induced adverse changes in the brain and body, such as an overactive threat-response system, abnormal hormonal axis functioning, impaired reward processing and diminished executive functions in the frontal lobes (Blankenstein et al., 2022; McCrory et al., 2011; Van Voorhees & Scarpa, 2004).

A recent Youth Endowment Fund (YEF) analysis of UK longitudinal data confirms this dose–response relationship, finding that children with six or more ACEs showed 45% higher risk of assault perpetration, 150% higher risk of weapon involvement and 154% higher risk of gang involvement compared to those with no ACEs (YEF, 2025). Importantly, while Black children showed higher rates of violence perpetration than White children, these ethnic disparities largely disappeared after controlling for socioeconomic factors and differential exposure to ACEs and protective factors, suggesting that structural inequalities rather than ethnicity itself drive these differences.

Studies also demonstrate differences in exposure to traumatic events, with ethnic minorities, those who identify as sexual minority individuals, people from lower socioeconomic backgrounds and those with developmental disabilities being more exposed to these events (Hatch & Dohrenwend, 2007; Lloyd & Turner, 2008; Merrick, Ford, Ports & Guinn, 2018; Substance Abuse and Mental Health Services Administration [SAMHSA], 2014a). More recently, studies have shown that sexual minority subgroups, particularly those who identify as female and bisexual, experience a higher number of ACEs (Giano et al., 2024).

In schools, traumatised children often display behavioural and relational issues, conduct disorders or antisocial behaviour. Unfortunately, the punitive measures and zero-tolerance policies used to address these behaviours may worsen mental health problems instead of addressing their root causes (Ijaz et al.,

2024). Such approaches negatively affect traumatised children's wellbeing and fail to encourage positive behavioural changes (Le, Abdinasir & Rainer, 2023).

Consequently, these vulnerable children are often suspended or excluded from schools instead of receiving the necessary support to cope with their trauma, contributing to the school-to-prison pipeline phenomenon (Bacher-Hicks, 2024). A recent YEF analysis demonstrates this pathway starkly: 36% of suspended or excluded children self-reported violence perpetration, compared with just 8% of those never suspended, with excluded children being four to five times more likely to have police records, even after controlling for individual and household factors (YEF, 2024). Research also shows that a significant proportion of the 85,000 people in UK prisons (63%) were previously excluded from school during childhood (IPPR.org, 2017). The correlation between school disengagement, exclusion and an increased risk of youth violence is emphasised by the Youth Safety Taskforce (Gill, Quilter-Pinner & Swift, 2017). Hence, adopting alternative strategies to support this vulnerable pupil population is crucial for society.

An alternative approach to dealing with CYPs in a school setting who have suffered ACEs is a trauma-informed practice (TIP) framework. TIPs encompass four core principles: (a) understanding that trauma affects individuals broadly and that multiple pathways exist for healing, (b) acknowledging that trauma experiences manifest through particular indicators and symptoms, (c) ensuring that people who have experienced trauma receive holistic and integrated support from practitioners, programmes, institutions and wider systems and (d) actively working to avoid triggering or re-exposing individuals to traumatic experiences (SAMHSA, 2014a).

Previous research on TIP indicates that TIP training can immediately affect staff awareness and knowledge (Thomas et al., 2019). Additionally, it appears to enhance positive interactions between young individuals and practitioners. Recent research has shown improvements in behaviour as well as improved relationships between students and staff (Diggins, 2021; Schimke et al., 2022; Stokes, 2022). However, initial findings propose that a comprehensive approach is necessary for substantial changes in practice – a pivotal factor in the TIP theory of change (ToC) influencing child-level outcomes.

The expansion of TIPs reflects a shift in how services conceptualise and respond to trauma. In the United States, efforts to create trauma-informed systems have accelerated at the local, state and federal levels (Lang, Campbell & Vanderploeg, 2015), culminating in regulatory mandates, such as the Centres for Medicare and Medicaid Services requirement that skilled nursing facilities provide culturally-competent trauma-informed care (TIC) to all trauma survivors – a mandate effective since November 2019 (Department of Health and Human Services, 2016; O'Malley et al., 2023). However, implementation remains inconsistent across sectors and nations. For example, formalised TIPs are not yet widespread in Canadian schools despite growing awareness, with limited empirical literature documenting effectiveness (Record-Lemon & Buchanan, 2017). Similarly, in the UK, teachers have limited knowledge on trauma and how to manage behaviour in the classroom with a trauma-aware framework (McKee & Dillenburger, 2009; Sitler, 2009). As Becker-Blease (2017) notes, whilst the world is becoming trauma-informed, substantial work remains to establish whether these widespread implementations achieve their intended outcomes.

The YEF Toolkit (YEF, 2021) synthesis also underscores this critical paradox in current educational practice: Whilst trauma-informed approaches have become widespread, there is insufficient evidence to determine their impact on youth violence and behavioural outcomes. The YEF Toolkit explicitly states that "there is very little research on the impact of training staff and redesigning systems with the primary aim of recognising and responding to trauma" and notes that there is "insufficient evidence to calculate an estimate

of the impact of this activity on crime and violence" (YEF, 2021). Other researchers echo this, with very few randomised controlled trials (RCTs) focused on TIPs, with most in early-years settings (Sun et al., 2024). This evidence vacuum is particularly concerning given the substantial resources being invested – the YEF Toolkit highlights that trauma-informed training has proliferated despite lacking a clear specification of what practitioners should do differently or how to assess whether intended practice changes occur.

Intervention

Overview

TISUK's approach combines training using whole-organisation strategies, involving senior leadership, training frontline practitioners and creating supportive environments that acknowledge and assist practitioners in dealing with their own or vicarious trauma. Merely conducting training in isolation is unlikely to achieve the desired results (Molloy, 2022), whilst empowering a school as a whole to understand and commit to addressing trauma can create a healing and healthy school community (Watson & Astor, 2025).

TISUK has developed a TIP framework that offers a comprehensive training package for schools to equip staff with insights into the neuroscience and psychology of psychological trauma and its impact on behaviour, learning, relationships and mental health. TISUK assists school leadership in fostering a mentally healthy culture by developing trauma-informed policies and practices, as well as by supporting school staff in implementing trauma-informed approaches when dealing with conflict. Upskilling and supporting school staff is particularly important as school staff are most young people's trusted adults outside of home (58%; YEF, 2024). This approach is in line with internationally recognised best practices (Martin et al., 2024).

TISUK's approach is rooted in a deep knowledge of emotionally healthy and unhealthy organisational environments (Høidal & Hanssen, 2022; Marmot & Brunner, 2005; McIntyre & Mazza, 2020; Sapolsky & Share, 2004) and addresses disaffection, feelings of undervaluation, isolation, anxiety, depression and stress-related issues. TISUK also trains staff in creating supportive environments where individuals feel valued and seen, especially during times of stress, enhancing staff members' ability to provide positive pupil experiences.

TISUK values diversity and difference and actively works to ensure training is non-discriminatory and inclusive in nature. It provides and signposts to equality and diversity training and directs all TISUK trainers to this type of training. All training identifies racial discrimination as an ACE and explains the findings of the ACE study in terms of the impact of discrimination. In addition, there is content in the Diploma course and a webinar for all staff that directly addresses this issue.[1]

This section describes the intervention following the Template for Intervention Description and Replication.[2] For more information on other trauma-based interventions see the YEF Toolkit.

Brief name: TISUK Training and Implementation

[1] It should be noted that TISUK's training materials have changed since the design of this project, and issues of social injustice are now woven more thoroughly throughout the Diploma course.

[2] Cochrane Collaboration and Training, C., 2022. Template for Intervention Description and Replication.

Who (recipients of the intervention)

The intervention was implemented across the whole secondary school by improving school staff awareness of TIPs, leading to changes in policies and practices that were consistent with TIP. The main recipients were school staff as well as students across the school.

There were three distinct tiers of staff recipients:

- All staff: All staff received mandatory training in intervention schools. No selection was required, as this aimed for universal coverage to ensure a shared language and understanding across the whole school community.
- Senior leadership: 4–10 members of each school's senior leadership team (SLT) were chosen for targeted training. Schools selected participants based on strategic roles, including Headteachers, Deputy Headteachers and Assistant Headteachers, who were responsible for behaviour, safeguarding or pastoral care. The aim was to ensure that those with decision-making power over policies and resource allocation were equipped to drive systemic change.
- Other staff: 5–7 staff members (one member of staff per 250 pupils) were chosen for more intense 11-day Diploma training. TISUK recommended specific role profiles, including at least one SLT member with strategic oversight, Special Educational Needs Co-ordinators (SENCOs) and Safeguarding/Designated Safeguarding Leads, Pastoral Leads and Heads of Year, key Teaching Assistants (TAs) working directly with young people and Behaviour or Inclusion Managers. The selection prioritised staff who had both direct contact with vulnerable pupils and the capacity to cascade learning to colleagues.

Moreover, **all pupils** in participating schools were beneficiaries through the whole-school culture change, though **outcomes were measured only for Year 8 cohorts**. Additionally, a subgroup of more vulnerable CYPs **across all year groups** received extra targeted support from these practitioner-trained school staff members, with **the evaluation focusing on those in Year 8**.

What (physical or informational materials used in the intervention)

The intervention used the following resources for school staff:

- Four webinars with discussion guides and e-books
- A Delegate Handbook (Diploma)
- "Helping teenagers talk about their lives" cards
- A Senior Lead Training e-book
- A School wellbeing audit and staff wellbeing questionnaire
- An implementation checklist and Red–Amber–Green (RAG) rating for network consultancy
- A webinar alternative for whole-staff training
- TISUK emotion cards and posters

What (procedures, activities and/or processes used in the intervention)

The intervention consisted of six activities:

1. **Whole-staff training**: The entire school staff, including support staff, administrators and others, participated in two 3-hour sessions. These sessions aimed to provide staff with an overview of what constitutes trauma, its potential impact, ACEs, protective factors, the neuroscience of trauma and

relational approaches. The two sessions were scheduled with a gap of two or three terms, strategically timed to address the decrease in momentum that typically occurs after an initial project launch. The sessions were conducted virtually to accommodate multiple schools in each training session. These sessions served as the initial catalysts for staff members. The aim was to ignite interest and change attitudes to distressed and vulnerable CYPs whilst outlining what was needed to implement a whole-school approach. The whole-school training laid the foundation for the further development of staff understanding and skills, as well as empowering each member of the school community to see that they had a role to play.

2. **Senior leadership training**: This training involved approximately six members (4–10, depending on the size of the school) of the SLT from each school. They attended a two-day training session that focused on the key elements of creating a trauma-informed and mentally healthy culture through ethos, policy and practice. The training aimed to increase understanding of the effects of trauma and develop the skills and understanding to implement effective relational practices, procedures and interventions to break the cycle from ACEs to criminal behaviour and violence. Leadership was equipped with the tools, materials and knowledge to evaluate and track TIPs within their schools. There was a focus on fostering psychologically safe and inclusive environments, and delegates were asked to identify priorities for staff wellbeing and ensure access to emotionally available adults (EAAs) for all. The training was conducted in cohorts across the project schools, with SLT members from each school attending the training at different times, minimising the impact on the day-to-day running of the schools.

3. **Network consultancy support**: Schools were provided with three consultancy support meetings from an experienced educational consultant at TISUK. This was offered to two to four staff from each school. The purpose of this was to support school leaders to embed changes in culture, policy and practice; they identified barriers to implementation within the unique context of their schools, monitored the quality of provision and identified priorities for improvement as well as opportunities for peer support and the sharing of best practices, ideas and resources. Staff members who attended the consultancy sessions were also invited to two online networking sessions attended by staff from across schools in the trial and facilitated by TISUK consultants. These sessions were designed as spaces for staff to share experiences, challenges and examples of good practices.

4. **Diploma practitioner training**: Approximately six staff members (one member of staff per 250 pupils enrolled in a school) were chosen from each school to undergo a comprehensive 11-day Level 5 Diploma Practitioner Training. This training was conducted over five to six months in two-day blocks, with a final assessment to complete the accreditation. Delegates were required to deliver a presentation on the implementation of their new knowledge and skills. TISUK recommended including at least one member from the SLT who had strategic oversight and could oversee the organisation of resources and interventions and embed practice into policy. Other staff members were likely to be teachers, SENCOs, Pastoral Leads or TAs. The training aimed to provide these individuals with a deeper understanding of trauma and its recovery process, equipping them with the skills, knowledge and understanding to respond effectively. It covered topics such as identifying protective factors and safety measures within school cultures, developing skills and knowledge to provide targeted support to vulnerable children through individual or small-group interventions, implementing strategies and approaches to support staff, fostering a trauma-informed school culture and enhancing communication skills to promote and challenge the practices of others. Once trained, these practitioners assumed the role of champions within the schools; they provided support to pupils

directly and enabled them to reflect on painful life experiences. They also collaborated with the SLT to bring about cultural, policy and practice changes within the schools, which included updating and overseeing the schools' referral processes for vulnerable pupils and integrating TISUK measurement tools into this where appropriate. They received mentoring throughout the process from TISUK.

5. **Reflective supervision workshops**: Reflective supervision was a supportive, respectful and purposeful process and is a vital component of a TISUK community. The aim of reflective supervision is for staff to feel supported enough to reflect with one another and to gain further professional insight into work-related interactions and patterns of behaviour or underlying emotional need. The process is fundamentally concerned with how to best support practitioners to best support the children with whom they worked. TISUK offered reflective supervision training to two practitioners from each school who had completed the 11-day Diploma training. These two practitioners accessed external supervision themselves, but the training aimed to equip them with the necessary skills to establish effective, sustainable reflective supervision models for key staff across their settings.

6. **Webinar viewing for staff and CYPs**: Schools were given access to three webinars for staff and young people to view at convenient times: 1) *The Psychology of Emotion, Relationships and Mental Health Part 1*, 2) *The Psychology of Emotion, Relationships and Mental Health Part 2* and 3) *Lived Experiences From Mark and Aliyah*.

   Teachers were asked to lead a discussion after each of these; guidance was provided to staff on how to facilitate this. There was also one webinar for all staff members to view*: Conversations that matter; from racial trauma and discrimination in schools and communities, to respecting and celebrating difference and diversity (policy & implementation).* This webinar was also part of the Diploma training and included discussion time.

Who (intervention providers/implementers)

The intervention was developed and implemented by quality-assured TISUK trainers and consultants. All TISUK trainers and consultants undergo a rigorous interview, training and quality assurance process to become accredited by TISUK. All trainers had extensive experience in education and/or therapeutic work. TISUK's quality assurance process involves observing trainers and providing specific descriptive feedback and development points, support from the senior training team, access to reflective supervision sessions and annual trainer development reviews.

How (mode of delivery)

All trainings took place remotely via Zoom, led by TISUK trainers and consultants. Schools facilitated webinar viewings at times convenient for them. Webinar or in-person alternatives were offered for whole-staff training where a live remote session was not possible for a school within the timeframe of the trial.

Where (location of the intervention)

The intervention was delivered online, with trainings attended by staff from multiple schools, except for consultancy sessions. Where it was deemed necessary for staff engagement, an in-person visit was made to

schools to deliver training and provide consultancy support. Secondary schools were recruited from the following regional geographic areas[3]:

- Greater Manchester (Manchester, Tameside, Salford, Bolton)
- West Midlands (Birmingham, Staffordshire, Sandwell, Dudley, Solihull, Stoke-on-Trent, Wolverhampton)
- Southwest (Gloucestershire, Somerset, Bristol City, Bournemouth/Christchurch/Poole, Devon, Wiltshire)
- London (Harrow, Newham, Bromley, Lambeth, Hounslow)
- East of England (Bedford, Norfolk, Cambridgeshire, Essex, Hertfordshire)
- Yorkshire and the Humber (East Riding of Yorkshire, Leeds, Wakefield, North Yorkshire)
- East Midlands (Nottinghamshire, Lincolnshire, Leicestershire, Derby, Northamptonshire)
- Northeast (Northumberland, Hartlepool, Darlington, Sunderland, North Tyneside, South Tyneside)
- Northwest (Blackburn with Darwen, Cheshire West and Chester, Cheshire East)
- Southeast (Kent, Hampshire, Surrey, Brighton and Hove, Bracknell Forest)

When and how much (duration and dosage of the intervention)

The intervention encompassed various components, including: two 3-hour training sessions for the entire staff, 11 hours of training for Senior Leaders, 7.5 hours of network consultancy, 20 hours of reflective supervision workshops, 6 hours of webinar input with discussion for staff and pupils and 60.5 hours of training for Diploma practitioners. Given the gap between training staff and implementation, the intended dosage for pupils was less than 12 months.

The recruitment of schools took place from June 2023, when information sheets were sent out. The Memoranda of Understanding (MoUs) (see Appendix 5) were returned by schools up until the baseline data collection, which was undertaken in the Autumn term of 2023 and January 2024. The duration of the TISUK intervention was four school terms, from January 2024 to March 2025.

All intervention and control schools received an overall £500 for completing the surveys. This was distributed at two different time points – £250 for completing the baseline survey and £250 for completing the follow-up survey. In addition to this, any intervention school that participated as a case study received an additional £500 after data collection was completed.

Theory of change

A **ToC** is depicted in Appendix 1.

The ToC begins with recognising two main issues. Firstly, CYPs experience a range of challenges, including ACEs, which can lead to long-term physical and mental health issues, school exclusions and various social problems, such as crime, violence, addiction and poverty. Secondly, the school staff lacks adequate empowerment and training in TIP. The intervention is a whole-school strategy that aims to empower school staff in delivering changes in policies and practices following TIP.

[3] The areas of scope broadened during the co-design phase to increase the achieved sample at the school (cluster) level and ensure sufficient statistical power.

The delivery team expected staff-level changes to emerge within the evaluation period. However, all partners recognised that whole-school culture change and subsequent pupil outcomes may require longer timeframes than this evaluation permitted, although emerging changes were expected to be observed within this evaluation.

The successful implementation of the intervention depends on critical resources: TISUK trainers, delegates chosen from schools, school personnel, senior school leaders, consultants, training materials and the quality assurance of TISUK trainers. The section above has described the set of activities as part of the intervention.

The central hypothesis of this intervention is that training school staff and raising awareness through workshops and webinars (activities) will result in a change in policies and practices at the school level (outputs). Thus, the resulting outputs of such activities would be:

- New policies and practices fostering psychological safety, emotional regulation and mentalisation4
- New systems to identify and address the needs of all CYPs
- New reflective supervision for key staff
- Schools educating CYPs about the neurochemistry of mental health
- Schools identifying skilled EAAs for vulnerable children
- New targeted interventions for CYPs with ACEs

The short-term outcomes of such cultural changes and new practices at the whole-school level would be:

- New restorative practices
- Enhanced staff awareness of childhood adversity
- Shifts in staff attitudes towards distressed and vulnerable CYPs
- Changes in staff relationship with CYPs from positions of social engagement rather than social defence
- School staff being more able to effectively manage children who experience dysregulation
- CYPs gaining a better understanding of the neurochemistry of mental health
- Reflective conversations with CYPs about painful life experiences

The intervention pathways would bring about several positive impacts:

- CYPs' externalising behaviours decrease.
- School exclusions for CYPs reduce.
- CYPs experience improved mental wellbeing.
- CYPs develop a stronger sense of belonging.

Additionally, the intervention would impact school staff positively. Their attendance, engagement, retention and wellbeing are expected to improve. Ultimately, the school community would transform into one with a mentally healthy culture where everyone feels valued and included. It would become a welcoming and inclusive environment that promotes psychological safety.

[4] Mentalisation refers to the capacity to understand and interpret behaviours (both one's own and others') in terms of underlying mental states – such as thoughts, feelings, beliefs, desires and intentions.

These pathways towards change rely on a few key assumptions that were tested through the implementation and process evaluation (IPE) – see the Implementation and process evaluation section. The **main underlying assumptions** are:

1. Resources are in place for the delivery of the activities.
2. All relevant stakeholders (delivery team, school staff) are actively engaged with the intervention.
3. Activities are delivered at the frequency and dose intended.
4. Outreach is achieved in the numbers and categories of stakeholders/participants intended.
5. Activities are delivered by TISUK with high quality.
6. Schools implement trauma-informed changes as a result of the intervention (intended outputs).
7. School staff attitudes and awareness change, resulting from the new practices (outcomes).

Preliminary evidence and rationale for equipoise

Over the last six years, TISUK has collaborated with over 5,000 educational and community settings across the UK, including every school in Cornwall, through the HeadStart Kernow project funded by Big Lottery Fund. According to monitoring data collected by TISUK, the experience of schools implementing the TISUK approach suggests positive outcomes, including decreased permanent exclusions, suspensions and incidents of physical restraint. Additionally, there has been an improvement in staff and pupil wellbeing, learner engagement, attainment and attendance. Staff have reported that vulnerable CYPs develop trusting relationships with EAAs, leading to transformative changes in their trauma recovery. Many school staff delegates have expressed that the training not only transformed their relationships with pupils at school but also improved their relationships with family members at home. This qualitative evidence suggests that schools could play a role in mitigating mental health disorders and behavioural issues amongst traumatised children and young individuals.

Furthermore, TISUK's comprehensive efforts align with the UK Government's Green Paper on Children and Young People's Mental Health. The green paper emphasises the effectiveness of adequately trained and supported school staff, such as teachers, school nurses, counsellor and TAs, in dealing with mild-to-moderate mental health concerns. These concerns encompass issues such as anxiety, conduct disorder, substance use disorders and post-traumatic stress disorder (Department of Health and Social Care & Department for Education, 2018).

Despite the intervention showing promise and offering potential benefits, its effectiveness remains uncertain due to the limited availability of robust evidence supporting this approach. Consequently, this trial aims to address this gap and provide valuable insights.

Note on racial equity in the programme design

The TISUK intervention was developed and selected for evaluation before current YEF frameworks for racial equity in programme design were fully established. Whilst the intervention addresses universal trauma responses, it does not explicitly account for how structural racism and discrimination may compound trauma exposure for minority ethnic pupils or create additional barriers to help-seeking. This evaluation examines both the intervention's overall effectiveness and whether it inadvertently widens or narrows existing ethnic disparities in school outcomes.

Evaluation objectives

The research questions for this trial are split by methodology: Some focus on the impact evaluation and others on the IPE.

Within the impact evaluation, the trial is designed to address primary and secondary research questions related to the respective primary and secondary outcomes at the CYP (in Year 8 at the start of the intervention) and school staff levels. For further information, please refer to the evaluation protocol or statistical analysis plan (SAP).

Research questions (efficacy)

Primary research questions

The primary **research question at the CYP level** is:

- ERQ1: What is the mean difference in externalising behaviour at follow-up, measured by the **Strengths and Difficulties Questionnaire (SDQ) subdomains of Conduct Problems and Hyperactivity**, between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving business as usual (BAU)?

Secondary research questions

The secondary **research questions at the CYP** level are:

- ERQ2: What is the mean difference in internalising behaviour at follow-up, measured by the **SDQ subdomains of Emotional Problems and Peer Problems**, between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving BAU?
- ERQ3: What is the mean difference in prosocial behaviour at follow-up, measured by the **SDQ subdomain of Prosocial Behaviour**, between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving BAU?
- ERQ4: What is the mean difference in total difficulties at follow-up, measured by the **SDQ subdomain of Conduct Problems, Hyperactivity, Emotional Problems and Peer Problems**, between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving BAU?
- ERQ5: What is the mean difference in non-psychotic psychological distress at follow-up, measured by the **General Health Questionnaire (GHQ)**, between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving BAU?
- ERQ6: What is the mean difference in wellbeing at follow-up, measured by the **Short Warwick–Edinburgh Wellbeing Scale (SWEMWBS)**, between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving BAU?
- ERQ7: What is the mean difference in the sense of connectedness at follow-up, measured by the **School Connectedness Scale**, between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving BAU?

- ERQ8: What is the mean difference in the percentage of exclusions[5] at follow-up between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving BAU?
- ERQ9: What is the mean difference in the percentage of suspensions at follow-up between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving BAU?
- ERQ10: What is the mean difference in the percentage of attendance at follow-up between CYPs in intervention settings receiving TISUK training and CYPs in control settings receiving BAU?
- ERQ11: What is the mean difference in all primary and secondary CYP outcomes between CYPs in intervention settings who received TISUK training and CYPs in control settings receiving BAU, considering subgroup analyses by sex, ethnicity and free school meal (FSM) eligibility?

The secondary **research questions at the school staff level** are:

- ERQ12: What is the difference in attitudes related to TIC of school staff at follow-up, measured by the **Attitudes Related to Trauma-Informed Care** (**ARTIC**) survey, between school staff in intervention settings receiving TISUK training and school staff in control settings receiving BAU?
- ERQ13: What is the difference in wellbeing at follow-up, measured by the **SWEMWBS**, between school staff in intervention settings receiving TISUK training and school staff in control settings receiving BAU?

The secondary **research questions at the school level** are:

- ERQ14: What is the difference in the percentage of school staff retention[6] at the school level at follow-up between schools in intervention settings receiving TISUK training and schools in control settings receiving BAU?
- ERQ15: What is the difference in the percentage of school staff sickness at the school level at follow-up between schools in intervention settings receiving TISUK training and schools in control settings receiving BAU?
- ERQ16: What is the difference in the percentage of CYP suspensions at the school level at follow-up between schools in intervention settings receiving TISUK training and schools in control settings receiving BAU?
- ERQ17: What is the difference in the percentage of CYP exclusions at the school level at follow-up between schools in intervention settings receiving TISUK training and schools in control settings receiving BAU?
- ERQ18: What is the difference in the percentage of CYP school attendance in the targeted years at the school level at follow-up between schools in intervention settings receiving TISUK training and schools in control settings receiving BAU?
- ERQ19: What is the difference in the percentage of CYP school suspensions/exclusions/attendance in the targeted years at the school level at follow-up between schools in intervention settings receiving TISUK training and schools in control settings receiving BAU, considering subgroup analyses by sex, ethnicity and FSM eligibility?

[5] ERQ8 to ERQ11 are based on administrative data.

[6] ERQ14 to ERQ19 are based on administrative data.

In addition to the above research questions, the evaluation team sought to monitor any unintended consequences of the intervention and serious adverse effects.

Embedded quasi-experimental study

In addition to the main trial, an exploratory quasi-experimental study examined the targeted intervention component. In intervention schools, Diploma-trained practitioners provided intensive one-to-one or small-group support to pupils identified as particularly vulnerable. As this targeted support was only identifiable in intervention schools, a randomised comparison was not possible. Instead, we used propensity score matching (PSM) to construct a comparison group from control school pupils with similar baseline characteristics. This exploratory analysis aimed to estimate whether pupils receiving targeted support showed differential outcomes compared to matched peers. Given the non-randomised design, findings should be interpreted with caution.

Research questions (implementation and process evaluation)

The IPE is framed by nine broad research questions, as shown below.

- IPE1: To what extent do TISUK staff adhere to the intended delivery model?
- IPE2: To what extent has the intervention been delivered in the intended dosage?
- IPE3: To what extent do school staff and leadership engage with the intervention?
- IPE4: To what extent are the different components of the intervention delivered at a high quality?
- IPE5: What is the participation rate of the intended recipients (school staff and leadership)?
- IPE6: What is the perceived need for and benefit of the intervention amongst school staff and leadership?
- IPE7: What strategies and practices are used to support high-quality implementation?
- IPE8: How do structural factors (e.g. institutional racism, lack of diversity in the workforce) affect CYPs from Black, Asian and minority ethnic backgrounds?
- IPE9: How do CYPs from different sexes and Black, Asian and minority ethnic backgrounds experience the intervention?

Ethics and trial registration

The trial was approved by the Ethics Board at Ipsos UK. Reference number: 23-019045-01. The trial is registered with ISRCTN 64982435.

Data protection

This trial was conducted in full compliance with UK data protection regulations, with all measures detailed in the MoUs (Appendix 5), information sheets and privacy notices provided to participants. Personal data were processed under UK GDPR Article 6(1)(e) for tasks in the public interest, while special category data were processed under Article 9(2)(j) for research and statistical purposes. These legal bases align with the evaluation's public task remit to contribute to the wellbeing of CYPs and school staff.

The evaluation team implemented comprehensive GDPR compliance measures, including: the protection of data subjects' rights, with clear mechanisms for access and erasure; the transparent communication of data-processing purposes through privacy notices; restricted data access on a need-to-know basis with secure storage on encrypted UK servers; defined retention periods aligned with research needs; formal

information-sharing agreements between teams; and secure communication channels using VPN and AES 256 encryption. Ipsos UK served as the data controller, with the evaluation partners (TONIC and University of Kent) and TISUK acting as data processors. Full details of these measures are provided in Appendix 15.

Consent collection followed a tiered approach appropriate for the participant groups. For surveys, pupils and school staff were provided opt-out opportunities, with parental opt-out for pupils and assent obtained immediately before participation. Qualitative research with pupils required both written parental consent and pupil assent, while adult participants provided written or verbal consent after receiving information sheets and privacy notices. All schools involved had comprehensive data sharing agreements and data protection impact assessments conducted in accordance with UK GDPR requirements (see Appendices for templates and full procedures).

Project team and stakeholders

Funders

The Home Office and the YEF co-funded TISUK to deliver the project, and YEF representatives attended project steering groups and other meetings. The YEF funded the evaluation and commissioned Ipsos UK, TONIC and the University of Kent to deliver the evaluation. The YEF monitored the evaluation and reviewed the study plan and research tools.

Delivery team

The delivery team was made up of:

Rowan Jones (TISUK): Project Lead – Main point of contact for YEF trial and project schools. Responsible for school recruitment and reporting.

Rachel Toller (TISUK): Director of Operations and Training – Responsible for overseeing project management, staffing, HR and the coordination of training as well as director consultancy support across all internal TISUK teams.

Julie Harmieson (TISUK): Director of Education and National Strategy – Responsible for providing director consultancy support across all internal TISUK teams, including training and consultancy.

Dr Margot Sunderland (TISUK): Director of Innovation and Research – Responsible for providing director consultancy support across all internal TISUK teams, including training and consultancy.

Lisa Aire (TISUK): Training and Admissions Coordinator – Responsible for coordinating training and providing technical and administrative support for training, ensuring school engagement.

Marie Bryant (TISUK): Admissions and Operations Coordinator – Responsible for processing training applications, tracking attendance, assisting delegates to make up missed days and sending out handouts and feedback forms.

Derrick Hopf (TISUK): Business and Project Development Manager – Responsible for financial monitoring.

Evaluation team

The evaluation team for this trial was as follows:

Dr Facundo Herrera (Ipsos UK): Project Director – Responsible for all aspects of the study and overall direction. Reporting Lead – Responsible for the statistical approach and analysis.

Marzieh Azarbadegan (Ipsos UK): Project Manager – Responsible for day-to-day management and communications with the YEF, delivery partners and other stakeholders; had a key role in reporting.

Dr Jessica Ozan (Ipsos UK): Youth Board Advisor – Responsible for advising on the IPE, children's participation and ethics.

Dr Amanda Carr (TONIC): TIP Director – Responsible for advising on TIP.

Karl Ashworth (Ipsos UK): RCT Design Advisor – Responsible for advising on the statistical design of the trial.

Prof. Simon Coulton (University of Kent): Trial Design Director, Statistical Lead – Responsible for advising on efficacy research design and statistical analysis.

Jemuwem Eno-Amooquaye (Ipsos UK): IPE Lead – Responsible for engaging with schools regarding IPE fieldwork, collecting qualitative data and reporting.

Luisa Gomes (Ipsos UK): School Liaison Officer – Responsible for engaging with schools.

The team was supported by Ipsos UK Consultants and Research Executives who were involved in data collection and analysis.

Methods

This section describes the trial design, participant selection, outcome measures, sample size, randomisation procedures and statistical analysis approach.

Trial design

Table 1 presents the trial design, including the randomisation approach, the outcomes measured and the data sources.

Table 1: Trial design

Trial design		**Two-arm and cluster – randomised at the school level**
Unit of randomisation		**School**
Stratification variables (if applicable)		**Median of percentage of pupils eligible for free school meals**
Primary outcome	variable	**(CYPs) Externalising behaviour**
	measure (instrument, scale, source)	**SDQ – Combined conduct and hyperactivity scale scores (0–20) (survey)**
Secondary outcome(s)	variable(s)	**(CYPs) Internalising behaviour** **(CYPs) Prosocial behaviour** **(CYPs) Total difficulties** **(CYPs) Non-psychotic psychological distress** **(CYPs) Wellbeing** **(CYPs) Sense of connectedness** **(CYPs) Exclusions and suspensions** **(CYPs) School attendance** **(School staff) Attitudes related to TIC** **(School staff) Wellbeing** **(School) Staff retention** **(School) Staff sickness** **(School) Exclusions & suspensions of CYPs** **(School) School attendance of CYPs**
	measure(s) (instrument, scale, source)	**(CYPs) Internalising behaviour:** SDQ – Combination of Emotional Regulation and Peer Problems (0–20) (survey) **(CYPs) Prosocial behaviour:** SDQ – Sub-dimension of Prosocial Behaviour (0–10) (survey) **(CYPs) Total difficulties:** SDQ – Combination of sub-dimensions of Conduct, Hyperactivity, Emotional Regulation and Peer Problems (0–20) (survey)

		(CYPs) Non-psychotic psychological distress: GHQ-12 (0–12) (survey) **(CYPs) Wellbeing**: SWEMWBS (7–35) (survey) **(CYPs) Sense of connectedness:** School Connectedness Scale (survey) **(CYPs) Exclusions and suspensions:** Administrative records **(CYPs) School attendance:** Administrative records **(School staff) Attitudes related to TIC:** ARTIC (survey) **(School staff) Wellbeing:** SWEMWBS (7–35) (survey) **(School) Staff retention:** Administrative records **(School) Staff sickness:** Administrative records **(School) Percentage of exclusions and suspensions of CYPs:** Administrative records **(School) Percentage of school attendance of CYPs:** Administrative records
Baseline for primary outcome	variable	**(CYPs) Externalising behaviour**
	measure (instrument, scale, source)	**SDQ – Combined conduct and hyperactivity scale scores (survey)**
Baseline for secondary outcome	variable	**(CYPs) Internalising behaviour** **(CYPs) Prosocial behaviour** **(CYPs) Total difficulties** **(CYPs) Non-psychotic psychological distress** **(CYPs) Wellbeing** **(CYPs) Sense of connectedness** **(CYPs) Exclusions and suspensions** **(CYPs) School attendance** **(School staff) Attitudes related to TIC** **(School staff) Wellbeing** **(School) Staff retention** **(School) Staff sickness** **(School) Percentage of exclusions and suspensions of CYPs** **(School) Percentage of school attendance of CYPs**
	measure (instrument, scale, source)	**(CYPs) Internalising behaviour:** SDQ – Combination of Emotional Regulation and Peer Problems (0–20) (survey) **(CYPs) Prosocial behaviour:** SDQ – Sub-dimension of Prosocial Behaviour (0–10) (survey) **(CYPs) Total difficulties:** SDQ – Combination of sub-dimensions of Conduct, Hyperactivity, Emotional Regulation and Peer Problems (0–20) (survey) **(CYPs) Non-psychotic psychological distress**: GHQ-12 (survey)

		(CYPs) Wellbeing: SWEMWBS (survey) **(CYPs) Sense of connectedness:** School Connectedness Scale (survey) **(CYPs) Exclusions and suspensions:** Administrative records **(CYPs) School attendance:** Administrative records **(School staff) Attitudes related to TIC:** ARTIC (survey) **(School staff) Wellbeing:** SWEMWBS (survey) **(School) Staff retention:** Administrative records **(School) Staff sickness:** Administrative records **(School) Number of exclusions and suspensions of CYPs:** Administrative records **(School) School attendance of CYPs:** Administrative records

Notes: CYPs = children and young people; TIC = trauma-informed care; GHQ-12 = General Health Questionnaire-12; SWEMWBS = Short Warwick–Edinburgh Mental Wellbeing Scale; ARTIC = Attitudes Related to Trauma-Informed Care; SDQ = Strengths and Difficulties Questionnaire.

This was a two-arm cluster randomised controlled efficacy trial. We randomised the schools (the unit of randomisation) rather than individual pupils, as the TISUK intervention targeted whole-school changes in policies and practices. We allocated the schools equally (one-to-one ratio) to either the intervention group or the control group through simple randomisation. This equal allocation maximised statistical power whilst ensuring fairness in access to the intervention.

Control condition

Schools in the control group continued with BAU practices. We captured their existing approaches to trauma and support for vulnerable CYPs through a mapping exercise at baseline. This exercise documented staff ratios, the specialist support available (such as counsellors or speech and language therapists) and other programmes the schools were engaging with. We also conducted short surveys with SLT members in the control schools at six and 12 months from the start of the intervention to monitor any changes in their trauma-informed training or mental health initiatives during the trial period. These surveys focused on the support available rather than on detailed implementation characteristics, such as delivery intensity or target groups, aiming to minimise the burden on the control schools. This approach balanced the need to understand usual practices against minimising the data collection burden on the control schools. We did not conduct equivalent mapping in the intervention schools, though delivery team consultants monitored for any major concurrent initiatives.

Incentives

Both the intervention and control schools received financial incentives of £500 per school to recognise the staff time and effort required for data collection. We paid £250 upon completion of the baseline data collection and £250 at the end of the follow-up data collection. The schools selected for case studies received an additional £500 upon completion of the data collection activities.

As a contingency measure to engage schools in the follow-up survey, all schools in the trial received additional incentives that included two £30 vouchers for school staff who supported survey distribution as

well as access to TISUK webinars, discounts on TISUK training and free spaces on TISUK Continuous Professional Development (CPD) sessions within a given timeframe.

Primary and secondary outcomes

The primary outcome was externalising behaviour amongst Year 8 pupils, measured using the combined conduct and hyperactivity subscales of the SDQ. Pupils were in Year 8 at baseline and Year 9 at follow-up, representing the same cohort tracked over 15 months. We measured this at baseline and at 15-month follow-up.

Secondary outcomes included additional pupil-level measures (internalising behaviour, prosocial behaviour, total difficulties, psychological distress, wellbeing, sense of connectedness, exclusions, suspensions and attendance), school staff outcomes (attitudes to TIC and wellbeing) and school-level outcomes (staff retention; staff sickness; and aggregated pupil exclusions, suspensions and attendance). We collected administrative data on pupil exclusions, suspensions and attendance only for the target cohort, not the whole school population. Administrative data covered the term before the intervention began and the last term of delivery. The Outcome measures section below provides detailed descriptions of all measures.

Changes to the original trial design

We made two key changes to the original trial design. First, we removed geographic location as a stratification variable during randomisation. Initially, we planned to stratify by both FSM eligibility and geographic area. However, as we expanded recruitment across more regions to achieve the target sample size, the number of schools per stratum became too small, reducing randomisation efficiency.

Second, we conducted randomisation in two batches rather than one. The first batch occurred in early December 2023 for schools that had completed baseline testing by then. The second batch occurred in early January 2024 for the remaining schools. This change accommodated the extended baseline data collection period and Christmas break constraints whilst maximising school participation.

Equity, diversity and inclusivity considerations

The evaluation team practised reflexivity throughout the trial to address potential biases and ensure inclusive research practices. During co-design meetings between Ipsos UK and TONIC, we regularly discussed how different groups might experience the intervention and data collection processes. These discussions informed modifications to recruitment materials, survey design and consent procedures.

We established a Youth Advisory Board specifically for this evaluation. The board included young people from different genders and minority ethnic backgrounds. Their role was to ensure that recruitment materials, information sheets and survey instructions used appropriate language and were accessible to the target population. The validated survey instruments themselves (such as the SDQ) could not be modified, but board members reviewed supporting materials and administration procedures. Their input led to adjustments in how we introduced the research and explained participation to young people.

During qualitative data collection, we deliberately sought perspectives from vulnerable and minoritised groups. The team held regular debriefing sessions to reflect on emerging findings and power dynamics within the research relationships. We acknowledged our position as external researchers and worked to create space for participant voices that challenged our assumptions.

We incorporated FSM eligibility as a stratification variable and planned subgroup analyses by ethnicity and sex to examine differential effects. However, we recognise that structural inequities extend beyond what trial design can fully capture.

Rationale for evaluation design

This cluster RCT was carefully designed to answer our research questions. The school-level randomisation addressed the whole-school nature of the TISUK intervention, preventing contamination between treatment and control groups whilst capturing the organisational changes that drive individual pupil outcomes.

The two-arm design with 78 schools provided sufficient statistical power to detect meaningful effect sizes for our primary outcome, externalising behaviour, measured using the validated SDQ instrument. Practical and funding constraints determined the 15-month follow-up period. Whilst this allowed time for staff-level changes to emerge, the evaluation team recognised that whole-school culture change was possible but may require longer to translate into measurable pupil outcomes.

Our multilevel outcome framework systematically addressed the intervention's logic model: Staff-level measures (ARTIC, wellbeing) tested whether training changes attitudes and reduces burnout; school-level administrative data captured institutional changes in exclusions and attendance; and pupil-level surveys measured the hypothesised improvements in behaviour, mental health and school connectedness.

The stratification by FSM eligibility and planned subgroup analyses by ethnicity ensured we could examine whether TISUK reduces or exacerbates existing educational inequalities. Our approach was designed to deliver robust causal inference about both the intervention's overall effectiveness and its differential impacts across diverse school populations.

Participant selection

School eligibility and recruitment

The delivery team (TISUK) recruited secondary schools from 10 regions across all of England: Greater Manchester, West Midlands, Southwest, Greater London, East of England, Yorkshire and the Humber, East Midlands, Northeast, Northwest and Southeast. TISUK managed school recruitment from June 2023 through October 2023.

Schools were eligible if they were located within our target regions. We excluded schools that:

- Had staff who completed the TISUK 11-day Diploma training
- Had received TISUK whole-staff training within the previous three years
- Had staff who completed TISUK senior leadership training and implemented policy or practice changes
- Had received intensive TIP training from other providers within three years
- Were fee-paying schools
- Were alternative provision or special schools with a social, emotional and mental health focus

TISUK and Ipsos UK screened schools through initial expressions of interest and signed MoUs before baseline data collection (see Appendix 5). No pre-test scores were used as eligibility criteria for schools.

Target numbers and achieved recruitment

TISUK aimed to recruit 100 schools with 16,000 Year 8 pupils. About 350 schools were approached between June and December 2023. Full recruitment and randomisation details are presented in **Error! Reference source not found.**.

Pupil participants

Our primary participants were all Year 8 pupils (aged 12–13 years) in the recruited schools. We targeted this year group based on co-production workshops with teachers and our Youth Advisory Board, who identified Year 8 as appropriate for measuring intervention effects whilst minimising the burden on schools.

We used an opt-out consent approach for pupil surveys. Schools distributed opt-out letters to parents and carers of all Year 8 pupils before baseline testing. Pupils provided assent immediately before completing surveys, and parents were allowed to opt out. We did not use pre-test scores or any other baseline measures as eligibility criteria for pupil participation.

Staff participants

School staff participated in surveys that measured attitudes to TIC and wellbeing. All staff in intervention schools were eligible for whole-staff training. SLTs (4–10 members per school) participated in leadership training, whilst selected staff (1 per 250 pupils on roll) undertook Diploma practitioner training.

Targeted support subgroup

Within intervention schools, Diploma-trained practitioners identified vulnerable pupils for additional individual or small-group support. Selection followed a pastoral approach rather than standardised criteria. School staff used their professional judgement to identify pupils with higher needs who would benefit from targeted support. This approach reflected how schools typically identify vulnerable pupils in practice. We estimated approximately 15 pupils per school would receive this targeted intervention.

For the quasi-experimental analysis, we could not identify equivalent pupils in control schools through the same process. Instead, we used PSM to construct a comparison group based on observable baseline characteristics. We used this subgroup for our embedded quasi-experimental study, and targeted pupils remained part of the main trial analysis.

Recruitment procedures and responsibilities

TISUK led school recruitment by contacting virtual heads and local authority mental health leads, attending virtual head meetings, encouraging partner organisations to share information in neighbouring regions, distributing briefing materials and hosting online information sessions.

TISUK conducted project-specific information briefings throughout the recruitment period. Schools that expressed interest attended these sessions to learn about TISUK and the evaluation requirements.

Settings and locations

TISUK delivered the intervention remotely via Zoom for all training components due to the number of schools required for the trial. Where it was deemed necessary for staff engagement, schools were visited in person. Schools facilitated webinar viewings at convenient times. Pupil data collection occurred in school computer rooms or ICT suites (Information and Communication Technology), with one pupil per computer

and school staff present throughout. We provided paper copies of pupil surveys, upon schools' requests, for 10 schools overall. We provided schools with standardised administration guidance, including scripts for introducing the survey that mirrored the instructions embedded in the online survey.

Outcome measures

Baseline measures

We collected baseline data on all outcome measures between October 2023 and January 2024, prior to randomisation. We used the same tools at baseline as for follow-up measurement (pupil surveys, staff surveys and administrative data). Baseline measurement allowed us to control for pre-existing differences between schools and track changes over time. We baselined pupils in Year 8 and collected follow-up data 15 months later, when the same cohort had progressed to Year 9.

The trial followed this timeline:

- School recruitment: June–October 2023
- Baseline data collection: October 2023–January 2024
- Randomisation: December 2023 (Batch 1) and January 2024 (Batch 2)
- Intervention delivery: January 2024–February 2025
- Follow-up data collection: March–April 2025

Primary outcome

Externalising behaviour was our primary outcome, measured using the SDQ. Pupils were in Year 8 at baseline and Year 9 at follow-up, representing the same cohort tracked over 15 months. The SDQ measures behavioural patterns in children and adolescents aged 3–16 years using 25 items that cover psychological attributes. We administered all 25 SDQ items. Internal consistency for UK adolescent samples is strong (Cronbach's α is typically 0.70–0.80 for subscales). In our sample, Cronbach's α was 0.76 for the externalising subscale.

We combined the Conduct Problems and Hyperactivity subscales to create our externalising behaviour measurement, with scores ranging from 0 to 20 (higher scores indicating greater difficulties). Combining these subscales to measure externalising behaviour is an established practice in developmental psychology and is standard in YEF-funded evaluations (Goodman et al., 2010). The subscales were summed following SDQ scoring guidelines. This combination aligns with the logic model's focus on reducing disruptive behaviours that can lead to exclusions and involvement in violence.

The SDQ is widely used in research and has strong psychometric properties for UK populations. We used the self-report version for Year 8 pupils, which takes approximately five minutes to complete. The YEF uses the SDQ across most projects to ensure consistency and enable comparisons between evaluations.

We measured the primary outcome at 15-month follow-up (April–June 2025), which provided sufficient time for the intervention to produce measurable effects whilst pupils remained in Year 8 or late Year 9. Practical and funding constraints determined this timeframe. The delivery team expected staff-level changes to emerge within this period whilst acknowledging that pupil-level behavioural outcomes were possible but may require longer.

Secondary outcomes

We measured secondary outcomes across three levels: individual pupils, school staff and whole schools.

- **Internalising behaviour** used the combined Emotional Problems and Peer Problems subscales of the SDQ (range 0–20), following guidance from the instruments' authors. This measured emotional difficulties and relationship problems that may improve through trauma-informed approaches.
- **Prosocial behaviour** used the SDQ Prosocial subscale (range 0–10), measuring positive social behaviours expected to increase through the intervention.
- **Total difficulties** combined all SDQ problem subscales: Conduct, Hyperactivity, Emotional Problems and Peer Problems (range 0–40).
- **Non-psychotic psychological distress** was measured using the 12-item GHQ (GHQ-12), a validated screening tool for minor psychiatric disorders. We used it to assess broader mental health impacts beyond specific behavioural measures.
- **Wellbeing** was measured using the SWEMWBS, a seven-item scale that focuses on positive mental health and functioning. Scores range from 7 to 35, with higher scores indicating better wellbeing.
- **Sense of connectedness** used the School Connectedness Scale, a 10-item instrument that measures pupils' sense of belonging within their school environments (range 1–6 on each item).
- **Exclusions, suspensions and attendance** were collected from school administrative records (School Information Management Systems – SIMS - database and requested from schools), providing objective measures of school engagement and disciplinary outcomes.

School staff secondary outcomes

Attitudes related to TIC were measured using the ARTIC (ARTIC-45) scale. This psychometrically validated instrument assesses positive or negative attitudes towards trauma-informed approaches amongst service providers.

Staff wellbeing used the same SWEMWBS scale as that used for pupils, adapted for adult respondents.

School-level secondary outcomes

Staff retention, staff sickness and school-level exclusions, suspensions and attendance were collected from administrative records shared by schools, providing aggregate measures of school functioning.

Instrument accessibility

The SDQ, SWEMWBS and School Connectedness Scale are publicly available instruments and are shown in Appendix 8. We cannot reproduce the commercial ARTIC-45 scale in the appendices, but we provide full citations.

Data collection procedures

Pupil surveys

Ipsos UK collected pupil self-report data electronically using its Dimensions platform. We administered surveys at baseline (October 2023–January 2024, prior to randomisation) and at 15-month follow-up

(March–April 2025). Pupils completed the SDQ, GHQ-12, SWEMWBS and School Connectedness Scale at both time points.

School staff administered surveys in computer rooms with one pupil per computer and supervised throughout. We provided detailed administration scripts and technical support through our School Liaison Officer. 10 schools requested paper surveys during the follow-up data collection. We entered these responses into the system and checked them for errors.

Administrative data

Our School Liaison Officer coordinated the collection of administrative data from schools at baseline and follow-up. We requested pupil-level exclusions, suspensions and attendance data for the target cohort, covering the term before intervention and the last term of the delivery period. We also collected school-level data on staff retention and sickness for the same periods.

Staff surveys

Staff completed surveys online via the same Dimensions platform, with their consent obtained before participation. We administered staff surveys at baseline and 15-month follow-up. Our School Liaison Officer tracked response rates weekly and provided ongoing support to maximise participation.

Data collection equity considerations

We designed data collection to be inclusive and accessible. Our Youth Advisory Board, comprising diverse young people, reviewed all participant-facing materials. For example, board members suggested simplifying information sheet language and clarifying what participation would involve. We offered verbal completion for pupils with literacy difficulties, facilitated through our School Liaison Officer. Survey introductions used accessible, child-friendly language to explain confidentiality and withdrawal rights. We also provided guidance to supervising staff on how to support pupils who might need additional time or clarification when completing surveys.

Quality assurance and blinding

Data collectors were not blinded to school allocation, as the nature of the intervention made this impractical. However, we used standardised administration procedures and electronic data collection to minimise bias. All surveys used validated instruments with established scoring procedures.

Follow-up timing

We collected follow-up data 15 months after randomisation (March–April 2025). This single follow-up point constituted our primary endpoint, providing sufficient time for intervention effects whilst maintaining sample retention.

Changes from the original protocol

We made no changes to the planned outcome measures from the original protocol.

Multiple testing considerations

We designated externalising behaviour as our single primary outcome to avoid multiple testing issues. Secondary outcomes were clearly distinguished as exploratory, and we interpreted the results with the increased risk of Type I errors from multiple comparisons in mind.

Sample size

We determined sample size *a priori* through power calculations designed to detect a minimum detectable effect size (MDES) below 0.20, as required by the YEF for efficacy evaluations. We used Stata 17© and PowerUp! tools with conservative assumptions to ensure adequate statistical power for meaningful intervention effects.

Our calculations assumed zero pre–post correlation (no reliable estimates available), UK population norms for the SDQ externalising scale (mean 6.0, standard deviation [SD] 1.74[7]), 10% pupil-level attrition and an intracluster correlation coefficient (ICC) of 0.03 based on Shackleton, Hale, Bonell and Viner (2016) and Parker, Nunns, Xiao, Ford and Ukoumunne (2021) for adolescent health outcomes in UK secondary schools.[8] With an average Year 8 cohort of 200 pupils and 80% consent rate, we expected 160 participating pupils per school, producing a design effect of 5.8 and requiring 6,798 pupils total (ONS, 2023). We targeted 100 schools (50 per arm) rather than the minimum 25 per arm to ensure robust subgroup analyses, accommodate potential school attrition and provide sufficient power for our embedded quasi-experimental study.

Our primary population was Year 8 pupils in participating secondary schools. Although the intervention targeted whole-school changes that affect all pupils and staff, we focused outcome measurement on Year 8 pupils to balance statistical power with practical feasibility, selected through co-production with teachers and our Youth Advisory Board.

Sample at randomisation

The achieved sample at randomisation included 78 schools (40 intervention, 38 control) and 12,725 pupils, with baseline data on the primary outcome. This reflected recruitment challenges and four school withdrawals from the intervention group after randomisation. The ICC estimated on baseline data was 0.015, lower than the 0.03 estimated in the protocol. Based on this sample, the MDES at randomisation was 0.094.

Sample at analysis

After matching baseline and follow-up data, the final analytical sample comprised 3,970 pupils (2,512 in the intervention, 1,458 in the control) across 55 schools (34 intervention, 21 control). The observed ICC at analysis was 0.017, lower than the 0.03 assumed in our power calculations. The pre–post correlation was 0.59. However, differential attrition between groups (discussed below) means we interpret these power calculations cautiously.

[7] The SD has been estimated as the standardised SD using the same raw data for both scales.

[8] https://www.sdqinfo.org/norms/UKNorm1.pdf

Table 2: Sample size calculations and effect size

		Protocol	Randomisation
Minimum detectable effect size		**0.1064**	**0.094**
Pre-test/post-test correlations	Level 1 (participant)	n/a	n/a
	Level 2 (cluster)	n/a	n/a
Intracluster correlation coefficients	Level 1 (participant)	n/a	n/a
	Level 2 (cluster)	**0.03**	**0.015**
Alpha		0.05	0.05
Power		0.8	0.8
One-sided or two-sided?		Two-sided	Two-sided
Average cluster size		**160**[9]	**166**[10]
Number of clusters	Intervention	50	40
	Control	50	38
	Total	100	78[11]
Number of participants	Intervention	8,000	6,941
	Control	8,000	5,784
	Total	16,000	12,725[12]

Randomisation

We used minimisation to generate random allocation, stratifying schools by the percentage of pupils eligible for FSM6[13] above or below the median across all recruited schools. The median FSM proportion was 28%.

[9] The average school size in England is 986, and the average year size is 200. Accounting for a potential 20% not consenting, hence the cluster size is 160.

[10] This is the actual cluster average size.

[11] Although the initial plan was to allocate schools equally to the intervention and control groups at the randomisation stage, the achieved sample showed a slight difference due to attrition during the recruitment and baseline testing process. As a result, there is a difference of two schools between the intervention and control groups, partly because randomisation occurred over two waves of recruitment.

[12] The randomised sample included 12,725 pupils (6,941 intervention, 5,784 control) across 78 schools. After matching baseline and follow-up data, the final analytical sample comprised 3,970 pupils (2,512 intervention, 1,458 control) across 55 schools.

[13] FSM indicates pupils who have been eligible for FSMs at any point during the previous six years. We calculated the proportion of FSM pupils within each school and classified schools as above or below the median proportion across all recruited schools.

Initially, we planned to stratify by both FSM and geographic region, which justified using minimisation over simple stratified randomisation. However, as recruitment expanded across 10 English regions, the number of schools per stratum became too small. We therefore removed geographic location, leaving FSM as the sole stratifier. Whilst simple stratified randomisation would have been sufficient with a single binary stratifier, we retained the minimisation approach to remain consistent with our pre-registered protocol.

We conducted randomisation in two batches to accommodate extended baseline data collection and school scheduling constraints. Batch 1 (December 2023) included schools that completed baseline testing by early December, allowing intervention schools to begin training in January 2024 as planned. Batch 2 (January 2024) included schools that completed baseline testing throughout December and early January, accommodating those affected by the Christmas break and Ofsted inspections. Minimisation was applied separately within each batch, maintaining balance on FSM across both randomisation waves.

The University of Kent statistics team conducted the randomisation procedure, operating independently from the evaluation team at Ipsos UK. TISUK enrolled schools through its recruitment process, obtaining signed MoUs before baseline data collection. The University of Kent assigned schools to interventions using anonymised school ID codes rather than names, maintaining allocation concealment until baseline data collection was complete.

Due to the nature of the whole-school intervention, we could not blind participants, delivery staff or data collectors to group allocation after randomisation. However, we maintained blinding where possible through pre-specified analysis procedures, baseline data collection before randomisation and standardised data collection protocols. The randomisation process successfully maintained allocation concealment until baseline measures were complete, ensuring unbiased pre-intervention measures and recruitment decisions made without knowledge of treatment assignment.

Statistical analysis

Primary analysis

We conducted an intention-to-treat (ITT) analysis that included all available data, maintaining participants as members of their originally allocated groups. We used a two-level multilevel model with random effects at the school level to account for the clustering of pupils within schools. The multilevel model assumes that residuals are normally distributed at both the pupil and school levels and that pupil-level residuals are independent after accounting for school-level clustering. The random intercept captures the correlation between pupils within the same school. We report 95% confidence intervals (CIs) to reflect statistical uncertainty around all effect estimates.

Our primary analysis is described by:

$$Y_{ij} = \beta_1 + TISUK_j\tau + \left(Baseline_{ij}\right)\beta_2 + \mu_j + \varepsilon_{ij}$$

Where:

- Y_{ij} is the outcome for a pupil in school *j* at follow-up.
- $TISUK_j$ is a binary variable denoting whether a school is assigned to the intervention (1) or the control (0).
- $Baseline_{ij}$ captures baseline outcome scores and pupil characteristics (age, sex, ethnicity, Index of Multiple Deprivation [IMD] score) for pupil in school *j*.
- μ_j is the school-level residual [$\mu_j \sim i.i.d\ N(0, \sigma_\mu^2)$].
- ε_{ij} is the individual-level residual [$\varepsilon_{ij} \sim i.i.d\ N(0, \sigma_\varepsilon^2)$].

We substituted the IMD for the pupil-level FSM status in our analysis model. During co-design, we agreed that asking pupils directly about their FSM eligibility was inappropriate. This question risked stigmatising pupils, added unnecessary survey burden and relied on self-report for sensitive information that pupils might not accurately know or wish to disclose. Both FSM and IMD measure socioeconomic disadvantage and are strongly correlated in school-based research. IMD captures area-level deprivation, whilst FSM captures household-level benefit eligibility, but both identify similar pupil populations for subgroup analysis (Gorard, 2012).

We planned to obtain FSM data from school administrative records instead. However, administrative data coverage was substantially lower than anticipated. Using FSM would have reduced our analytical sample size and statistical power for the primary analysis. The IMD, derived from pupils' postcodes and converted into quintiles, provided comparable socioeconomic information without these limitations whilst maintaining pupil privacy. We made one planned deviation from our protocol regarding socioeconomic measurement.

We implemented the analysis using Stata 17©.

The primary outcome analysis was confirmatory. Secondary outcome analyses were pre-specified with distinct research questions for each outcome. Subgroup analyses and the embedded quasi-experimental study were exploratory.

Secondary analysis

We analysed secondary outcomes using the same ITT multilevel approach. For pupil-level continuous outcomes, we used the same model structure as the primary analysis. Covariates included baseline measurements for each outcome and pupil-level characteristics (age, ethnicity, FSM status). The model also included the school-level stratification variable used at randomisation, indicating whether the pupil's school was above or below the median FSM proportion.

For school-level outcomes, we used linear regression with robust standard errors (SEs), since clustering was not applicable with one observation per school, as follows:

$$Z_j = \gamma_0 + \gamma_1 Intervention_j + \gamma_2 BaseS_j + \gamma_3 FSM_j + \mu_j$$

$$\mu_j \sim N(0, \sigma_\mu^2)$$

Where:

- Z_j is the outcome at the school level.
- γ_0 is the intercept.
- $Intervention$ is a binary variable that equals 1 if the school is within the intervention arm and 0 otherwise.
- $BaseS_j$ represents the baseline covariate at the school level.
- FSM_j is a binary variable that indicates whether a school's proportion of FSM pupils is above or below the median in the sample of schools participating in the trial.
- μ_j is the random error across all schools.
- σ_μ^2 is the residual/error variance between schools.

Analysis in the presence of non-compliance

We measured school-level compliance using an engagement tool that assessed five intervention dimensions: individual training (45 points), whole-staff training (25 points), consultancy sessions (15 points), reflective supervision (10 points) and webinar usage (5 points). Each dimension was scored against specific criteria, such as the number of staff trained and attendance at meetings. Points were summed to produce a total score out of 100, then converted to a percentage. Schools scoring ≥ 76% were classified as compliant. This threshold indicated "good engagement" with the intervention. Further details on compliance levels are presented in the results section.

We estimated complier average causal effects (CACE) by dividing ITT estimates by the proportion of pupils in compliant schools, following Jo et al. (2008) and Schochet and Chiang (2011). This approach adjusts effect sizes to reflect impacts amongst schools that engaged fully with the intervention. We tested sensitivity to different compliance thresholds (76%, 80%, 90%) to examine how engagement levels affected outcomes.

Missing data analysis

We examined patterns of missingness through cross-tabulation and logistic regression. We modelled whether data were missing as a function of baseline characteristics and treatment allocation.

We did not implement multiple imputation as originally planned in the SAP. School-level attrition was substantial and differed markedly between trial arms. Of the 78 schools randomised, we collected follow-up data from 34 intervention schools (85% retention) and 23 control schools (61% retention).

Pupil-level attrition also exceeded our initial 10% assumption. In the intervention group, 37% of the randomised pupils provided follow-up data (63% attrition). In the control group, 26% provided follow-up

data (74% attrition). This attrition substantially exceeded expectations and became apparent only during follow-up data collection. Moreover, pupils with follow-up data but missing baseline measures could not be included in the primary analysis, which required matched data at both time points.

This substantial differential attrition between arms created major challenges for imputation. Multiple imputation relies on the assumption that data are missing at random (MAR), conditional on observed variables. When missingness exceeds 40% or systematically relates to treatment allocation, these assumptions become implausible (Sterne et al., 2009). The scale and pattern of attrition in our trial meant imputation would likely produce misleading results.

We therefore conducted a complete case analysis for all outcomes. We examined whether baseline characteristics predicted missingness using logistic regression. We also tested whether attrition rates differed significantly between the treatment and control groups. These analyses informed our interpretation of the results and our assessment of potential bias from differential attrition.

Subgroup analyses

We conducted exploratory subgroup analyses, as specified in the SAP and protocol. We originally planned two approaches: latent class analysis (LCA) to identify distinct subgroups with differential effects and multilevel models with interaction terms between treatment and subgroup categories.

The LCA did not converge beyond a single-class solution. This indicated insufficient heterogeneity in the data for identifying meaningful latent subgroups. We therefore relied on the multilevel modelling approach with interaction terms.

We estimated treatment effects by category for two subgroups. For ethnicity, we used six categories, as recorded in the pupil survey data. For socioeconomic status, we indexed pupil postcodes to quintiles of the IMD. The SAP specified interaction models for subgroup analysis. However, given the multi-category natures of ethnicity (six groups) and IMD (five quintiles), we estimated treatment effects separately within each category rather than fitting a single interaction model. This approach avoided power limitations from testing multiple interaction terms simultaneously.

We originally planned to analyse FSM eligibility as a third subgroup. FSM data are held in school administrative systems. We conducted this analysis separately, without linking to survey data. We analysed FSM separately because this data came from school administrative systems rather than the National Pupil Database. Linking FSM data to survey responses would require additional data matching. This would risk further reducing our analysis sample, which already faced substantial attrition.

Additional analyses and robustness checks

We explored whether schools that provided follow-up data differed structurally from those that were lost to attrition. We compared baseline characteristics between schools that completed follow-up and those that withdrew. This analysis examined the intervention and control groups separately.

This helped us assess whether attrition created systematic differences between treatment arms. If schools that reached follow-up had different baseline profiles, this could bias our effect estimates.

Estimation of effect sizes

We calculated Hedges' g for cluster-randomised trials:

$$ES = \frac{\bar{Y}^T - \bar{Y}^C}{\sqrt{\sigma_s^2 + \sigma_{pupil}^2}}$$

We used adjusted mean differences from multilevel models and population SDs from empty models. We calculated 95% CIs using the delta method and presented the standardised effects and raw mean differences for practical interpretation.

Estimation of intracluster correlation coefficients

We calculated ICCs using empty multilevel models at baseline and follow-up:

$$ICC = \frac{\sigma_s^2}{\sqrt{\sigma_s^2 + \sigma_{pupil}^2}}$$

The baseline ICC for externalising behaviour was 0.017, lower than our assumed 0.03, improving statistical power. We used Stata's "estat icc" command and reported ICCs for primary and secondary outcomes at the pupil level.

Longitudinal analysis

We conducted a single follow-up assessment 15 months after randomisation. Our multilevel models inherently accounted for the longitudinal structure by including baseline values as covariates, as described in the equation for our primary analysis above. This approach provided more efficient estimates than change score analysis whilst maintaining longitudinal design benefits.

Embedded quasi-experimental design study on targeted intervention

Design overview

We embedded a quasi-experimental study within the main trial to examine the targeted intervention component. In intervention schools, Diploma-trained staff identified Year 8 pupils for additional individual or small-group support (estimated 15 pupils per school). Selection followed a pastoral approach rather than standardised eligibility criteria. Staff used professional judgement to identify pupils with higher needs, such as those with behavioural difficulties or those who required additional support. This reflected typical school practices but meant vulnerability was not uniformly defined across schools. Since we could not identify equivalent pupils in control schools without set parameters, we used PSM to create a comparison group from control-school pupils with similar observable characteristics.

Sample and outcomes

We conducted two separate quasi-experimental design (QED) analyses using different data sources and samples.

Survey outcomes

At follow-up, we asked pupils in intervention schools whether they had received individual or small-group support from trained staff. We used this self-reported measure to identify the targeted support group. We examined externalising behaviours (primary outcome) plus internalising behaviours, prosocial behaviours, total difficulties, psychological distress, wellbeing and sense of connectedness.

Administrative outcomes

We also asked intervention schools to identify in their management information systems (MIS) pupils who received targeted support. We used these records to examine exclusions, suspensions and attendance.

The two analyses used independent samples, as we did not link survey responses to administrative records at the individual pupil level. Both tested whether targeted support improved outcomes, but they used different identification methods and outcome domains. The full methodological details are provided in the trial protocol and the SAP.

Matching approach

We implemented PSM using the Stata command "teffects psmatch". This command provides asymptotically efficient SEs, as developed by Abadie and Imbens (2006, 2016).

We selected matching covariates based on factors likely to influence both selection into targeted support and outcomes. Socio-demographic variables (sex, ethnicity, age, IMD score) captured the individual characteristics associated with vulnerability and behavioural outcomes. Baseline pupil-level outcomes controlled for pre-existing differences in the measures we aimed to assess. School-level FSM status accounted for institutional contexts that may affect both identification practices and pupil outcomes.

We estimated propensity scores using a logit regression model. We assessed matching quality using the tebalance suite of post-estimation commands in Stata.[14] These diagnostics compare covariate distributions between matched groups through standardised differences and variance ratios. Standardised differences below 0.1 (10%) indicate adequate balance. Variance ratios close to 1 suggest similar distributions across groups. We also examined graphical balance plots to visually confirm matching quality.

Analysis

We conducted an ITT analysis using multilevel models that account for pupil clustering within schools and incorporate inverse propensity weights for each participant. We implemented extensive robustness checks, including alternative matching methods (nearest neighbour, kernel matching, different callipers), sensitivity analyses for unobserved confounding using Rosenbaum bounds and balance tests. We calculated conditional average treatment effects for subgroups by sex and ethnicity where sample sizes permitted.

[14] The tebalance diagnostics are conceptually similar to Rubin's B and R statistics, referenced in the SAP. Both approaches assess whether matching has achieved adequate covariate balance between groups

Limitations

The main limitation was our inability to control for unobservable characteristics that might influence both selection for targeted support and outcomes. Pupils receiving targeted support were identified through staff professional judgement rather than standardised criteria. Unobserved factors, such as motivation, family circumstances or severity of difficulties, may have influenced both selection and outcomes. This is a common limitation of QEDs that rely on observational data.

Alternative approaches, such as instrumental variable estimation or regression discontinuity, were not feasible, given the pastoral selection process. We mitigated potential bias through rigorous matching on observable characteristics, but readers should interpret these findings cautiously. The exploratory nature of this analysis means results indicate associations rather than confirmed causal effects.

Full methodological detail on the QED, including sample size calculations, matching variables and balance diagnostics, is provided in the trial protocol and SAP.

Implementation and process evaluation

Research questions

The IPE is framed by nine broad research questions, as shown below.

- IPE1: To what extent do TISUK staff adhere to the intended delivery model?
- IPE2: To what extent has the intervention been delivered in the intended dosage?
- IPE3: To what extent do school staff and leadership engage with the intervention?
- IPE4: To what extent are the different components of the intervention delivered at a high quality?
- IPE5: What is the participation rate of the intended recipients (school staff and leadership)?
- IPE6: What is the perceived need for and benefit of the intervention amongst school staff and leadership?
- IPE7: What strategies and practices are used to support high-quality implementation?
- IPE8: How do structural factors (e.g. institutional racism, lack of diversity in the workforce) affect CYPs from Black, Asian and minority ethnic backgrounds?
- IPE9: How do CYPs from different sexes and Black, Asian and minority ethnic backgrounds experience the intervention?

These questions were answered with primary data collected through various research methods (interviews, case studies, observations) and administrative records.

Research methods

Table 3 provides an overview of the data collection methods used to address the IPE research questions.

Table 3: Implementation process evaluation methods overview

Data collection and data/sources	Sample size and population	Data analysis methods	Research questions addressed	IPE/ToC relevance	Timing
Semi-structured longitudinal staff interviews	18 school staff Sampled across 9 schools, 2 per school	Thematic analysis	IPE1, IPE3, IPE4, IPE5, IPE6, IPE9	Answered IPE questions and tested ToC's assumptions	Intervention phase (May–June 2024)
Semi-structured interviews with TISUK trainers and consultants	10 TISUK consultants and trainers purposively sampled	Thematic analysis	IPE1, IPE3, IPE4, IPE5, IPE6, IPE9	Answered IPE questions and tested ToC's assumptions	Intervention phase (Nov–Dec 2024)
Observations	2 observations of whole staff training (Session 1 and Session 2) 4 observations of network consultancy meetings 1 observation of Diploma training (Day 11) 1 observation of senior leadership training	Narrative analysis	IPE3, IPE4, IPE7	Answered IPE questions and tested ToC's assumptions; focused on the mechanisms for good quality delivery and engagement	Intervention phase (Jan–Nov 2024)
Case studies	5 case studies from schools in the treated arm 3–5 interviews with school staff and 2 with TISUK trainers/consultants One focus group with 4–6 children and young people (aged 11–18) per case 2–3 one-to-one interviews with pupils *(Fieldwork per case involves about 3 on-site visits in school, to allow 3 interviews with staff face-to-face)*	Thematic analysis	IPE3, IPE4, IPE6, IPE7, IPE8, IPE9	Answered IPE questions and tested ToC's assumptions; focused on the mechanisms that enable the changes from activities/outputs to impacts	Post-intervention phase (Mar–May 2025)
Monitoring data	Monitoring data collected by TISUK • Implementation checklists and attendance data (Monitoring Information provided by TISUK) • School Red–Amber–Green rating (used by TISUK to monitor engagement/participation) • School staff wellbeing audit • School bonding questionnaire	Descriptive statistics, cross-tabulation	IPE1, IPE2, IPE3, IPE5, IPE9	Answered IPE questions with a focus on factual and quantitative data	Intervention phase (Jan 24–Mar 25)

Notes: IPE = implementation and process evaluation; ToC = theory of change; TISUK = Trauma Informed Schools UK.

RAG rating: The RAG rating was used by TISUK as a monitoring tool to assess schools' engagement with the intervention over time. Ratings were updated periodically and were informed by multiple indicators of participation and implementation, including:

- A consultancy meeting – whether a meeting had been scheduled, which was considered an indicator of intended engagement
- The completion of the whole-school training preferences form – completion of the training preferences form, used as a proxy measure for anticipated attendance at training
- The completion of whole-school training – completion status of the training
- Diploma training – whether training was completed, including the number of sessions completed, and whether the minimum required threshold had been met or exceeded
- Senior leadership training – whether training was completed, including the completion of Diploma Practitioner training, assessed by the number of sessions delivered and whether the minimum required threshold had been met or exceeded
- Qualitative information from trainer notes, such as the extent to which schools required repeated follow-up or were experiencing contextual challenges (e.g. staff shortages)

Based on these indicators listed above, TISUK assigned each school a RAG classification of **red** (not engaged or at risk of withdrawal), **amber** (partially engaged) or **green** (highly engaged).

Longitudinal interviews with school staff

The aims of this data collection activity were to:

- Explore school staff perceptions around the need for and benefit of the TISUK interventions
- Understand how schools have experienced and engaged with the different elements of the intervention
- Explore the factors affecting the implementation of a whole-school trauma-informed approach, including facilitators and barriers
- Consider the different strategies and practices used by schools to support the high-quality implementation of TIP
- Understand the mechanisms for change that support a cultural shift in schools
- Explore whether any structural factors might affect the reach and impact of the intervention
- Examine perceptions of changes to staff wellbeing and attitudes towards TIP
- Understand what lessons can be learned from this intervention to inform any future scale-up of the intervention

This data collection activity involved 36 semi-structured interviews (18 interviews at two time points) with 18 staff members across nine intervention schools (i.e. two per school). Each staff member participated in two online interviews. The first interview was conducted whilst TISUK delivered its intervention within the school. The second interview took place during the last two months of the intervention phase. This gave staff members time to reflect and allowed the evaluation to capture how experiences and perceptions changed over time as well as explore how staff saw the schools amend their approaches to working with CYPs following the TISUK intervention. All interviews were conducted by Ipsos UK staff who had been trained in trauma-informed approaches to moderating interviews.

Whilst key topics were covered in each interview, the semi-structured approach ensured discussions were responsive and remained open to new areas and unexpected information. The sample for the schools considered the following factors: Ofsted ratings and TISUK's assessment of engagement (RAG ratings). Ipsos UK liaised with each of the 10 schools to identify the two members of staff with the highest engagement with the TISUK interventions. Amongst the 18 school staff members interviewed, there was good representation across a range of roles, including Senior Leaders, staff who had taken part in the Diploma Practitioner Training, SENCO Leads and Pastoral Leads.

Table 4: Implementation and process evaluation longitudinal sampling framework

School ID	Engagement Red–Amber–Green rating	Ofsted rating
School 1	Red	3
School 2	Yellow	2
School 3	Yellow	2
School 4	Yellow	2
School 5	Yellow	2
School 6	Green	1
School 7	Green	2
School 8	Green	2
School 9	Green	2

Interviews with TISUK trainers and consultants

Interviews were conducted with 10 trainers and consultants involved in delivering the TISUK intervention in schools. Participants were invited in a way that aimed to capture a broad range of experiences and perspectives. As most training would be delivered online, geographical location was not a key consideration. Instead, we sought to include trainers with different roles and levels of involvement, including those delivering consultancy, whole-staff training and Diploma courses; those specialising in particular programmes; and staff with varying levels of seniority, such as Senior Trainers and Directors. Where possible, we also aimed to include trainers from a range of demographic backgrounds. Participation was open and voluntary, and no one was selected on the basis of performance. These interviews were conducted online by Ipsos UK moderators. Similar to the longitudinal school staff interviews, the interviews were semi-structured.

This activity:

- Assessed whether the TISUK programme was delivered as intended
- Explored what worked well and less well in how interventions were delivered and what could be improved
- Aimed to understand how well schools adhered to the intended delivery model and explored what helped or hindered this
- Explored the factors that affected implementation and school engagement, including facilitators and barriers
- Considered what strategies and practices schools used to support the high-quality implementation of TIP

- Explored TISUK staff perspectives on the need for and benefit of the intervention
- Aimed to understand what lessons could be learned from this intervention to inform any future scale-up of the intervention
- Provided valuable implementation evidence around TIP training and support that the wider sector can use

Observations

This activity aimed to understand:

- How the training courses were delivered in practice
- How attendees responded to and engaged with the training
- How TISUK supported schools to implement the learnings captured through the training sessions

One member of the research team attended each of the following sessions to gain deeper insights into the delivery of TISUK.

Before the observations, the research team met with facilitators and teachers to explain the purpose of the observations. Information sheets were shared in advance, and staff were made aware ahead of time that a researcher would be attending the sessions, as outlined in the protocol. They were given the opportunity to object, with the understanding that if anyone was uncomfortable, the observation would not go ahead. Researchers conducted observations online and in non-intrusive ways, such as by joining sessions as silent observers, keeping microphones off during sessions and not interacting with participants or facilitators during delivery.

A template to record observations across multiple thematic areas was developed to ensure reflections were collected consistently and systematically across all observations (Appendix 16). The thematic areas were as follows: materials, activities, session topics/contents, interactions/discussions and participants.

The following sessions were observed:

Whole-staff training – two 3-hour whole-staff training sessions to observe the two components of the training (Session 1 and Session 2).

Network consultancy meetings – four 2-hour network consultancy sessions to observe conversations between school delegates and TISUK trainers around the implementation of learnings and the process of troubleshooting issues/risks.

Diploma training – full-day observation on Day 11 of the Diploma training course. On Day 11, delegates presented their understanding of the training and shared how they implemented the learnings within their relevant schools.

Senior leadership training – full-day observation of online senior leadership training. Ipsos UK liaised with TISUK trainers to determine which of the two-day training sessions was most appropriate for attendance.

The research team did not observe reflective supervision training and sessions due to ethical constraints. Reflective supervision sessions can involve private and sensitive conversations about pupils or other matters, which would not be appropriate for evaluators to observe.

Case studies

The key aims of this data collection activity were to:

- Explore perceptions around the need for and benefits of the intervention
- Explore how the programme was delivered within a particular school as well as what worked well or less well in delivery
- Understand how staff experienced and engaged with the different elements of the intervention
- Explore the factors (e.g. quality of facilitation, school leadership, existing school policies and initiatives, school size and demographics, cultural responsiveness) that affected the implementation of a whole-school trauma-informed approach
- Consider the different strategies and practices used by schools to support the implementation of TIP
- Understand the mechanisms for change that support a cultural shift in schools
- Explore to what extent the schools adhered to the intended delivery model and what factors influenced this
- Understand the experiences of young people who received targeted support
- Explore whether any structural factors might affect the reach and impact of the intervention
- Understand what lessons could be learned from this intervention to inform any future scale-up of the intervention

Five schools were selected from the intervention arm that had not already been selected for the longitudinal interviews. The sample considered the following factors: engagement RAG, urban/rural classification, deprivation level and geographical region. An initial 10 schools were identified as potential case study candidates using this sampling strategy and were invited to participate; only five were retained on a first-come, first-served basis.

Table 5: Case study framework

School ID	Engagement Red–Amber–Green rating	Geographical region	Urban/rural classification	Deprivation level (index of multiple deprivation decile)
Case Study 1	Green	East Midlands	Urban	**Decile 2** (high deprivation)
Case Study 2	Green	East of England	Semi-rural/suburban	**Decile 8–9** (low deprivation)
Case Study 3	Green	East of England	Urban	**Decile 3–4** (moderate to high deprivation)
Case Study 4	Yellow	Southwest England	Rural	**Decile 5–6** (moderate deprivation)
Case Study 5	Yellow	Yorkshire and Humber	Rural	**Decile 7–8** (low deprivation)

This data collection activity was led by TONIC. This activity included carrying out in-depth case studies across five schools. For each of the five case studies, the following fieldwork took place:

- Three to five interviews with staff members within the school who were involved in the TISUK intervention.
- One interview with a TISUK network consultant who had worked directly with that school, helping staff members use the training they had received to implement a whole-school trauma-informed approach to working with young people.
- One focus group with four to six pupils. The focus group lasted around 1.5 hours and took place in person during a school visit from the evaluation team. A private room on the school premises was secured for the focus group.
- Two or three one-to-one interviews with young people. The interviews lasted around 45 minutes and took place in person during a school visit from the evaluation team. The TONIC team worked with the school to ensure a private and familiar space was used for interviews to make participants feel as comfortable as possible. Young people were given the option of having another trusted adult in the room.

Table 6: Case study participant framework

School ID	Headteacher	Senior leadership team	Teachers	Pastoral support	Pupils (targeted)	Pupils (general)	Trauma Informed Schools UK consultant
Case Study 1	1	1	1	2	2	5	1
Case Study 2	0	3	0	3	2	5	
Case Study 3	1	1	1	2	2	6	1
Case Study 4	0	1	1	4	3	4	1
Case Study 5	1	1	1	2	2	5	

A Youth Advisory Board for the evaluation, including diverse young people, was established and run by TONIC. All the research tools and questions directed at young people were co-produced with this group, which helped ensure that the materials developed were inclusive and relevant to a wide range of experiences. This is particularly true of the guides used to run focus groups and interviews with young people.

The three to five interviews with school staff included those most involved in the TISUK interventions. This included Senior Leaders, staff who had taken part in the Diploma Practitioner Training, SENCO Leads, Safeguarding Leads or a staff member in a pastoral position. Interviews also included a member of staff who provided one-to-one targeted support to young people within that school.

Case study activities with young people

Focus groups were conducted with young people in Year 9. In addition, one-to-one interviews were undertaken with a subset of Year 9 students who had received more intensive support from school staff. Students were not permitted to participate in both data collection methods in order to minimise participant burden and avoid the duplication of perspectives. The evaluation team worked in collaboration with the

school to support the identification and recruitment of participants for both the interviews and focus groups. Sampling was purposive and sought to capture variation across the following characteristics:

- **Demographic diversity (focus groups and one-to-one interviews):** The sample aimed to include students who represented a range of genders and ethnic backgrounds. However, this criterion was not realised due to low levels of engagement within this cohort.
- **Duration of support (one-to-one interviews):** Students were selected to reflect variation in the length of intensive support they received from staff members. This criterion was met.
- **Prior support provision (one-to-one interviews):** The sample included a mix of students who accessed relevant support for the first time following the implementation of TISUK and those who had previously received pastoral or counselling support. This criterion was achieved.

In-person interviews were made accessible through the use of age-appropriate language, flexible scheduling during the school day and in familiar school settings; robust safeguarding procedures were put in place, including researcher DBS checks, school oversight and a clear protocol for managing disclosures in line with school safeguarding policies.

Pupils were informed in advance about confidentiality, its limits and how disclosures would be handled. They were supported through rapport-building, voluntary participation, the right to skip questions or withdraw at any time and the option to have a trusted adult present during the interview. The recruitment of pupils from minority ethnic backgrounds was supported through the selection of schools in urban areas which had higher numbers of minority ethnic pupils and targeted outreach via trusted school staff, the use of inclusive and culturally sensitive information materials and the monitoring of sample diversity during recruitment.

Adherence

We used quantitative data from the delivery team to populate an engagement tool for assigning scores at the school level. This tool measured the extent of each school's engagement with intervention components. We designed it in consultation with the delivery team. This engagement tool was separate from the RAG rating used by TISUK for ongoing school monitoring.

Adherence and compliance were related but distinct. Adherence captured school staff's participation in intervention activities. Compliance, as used in our CACE analysis, required judgement about whether schools engaged sufficiently to receive the intervention as intended. We defined schools that scored 76% or above on the adherence tool as compliant. This threshold indicated "good adherence" across intervention components.

It consisted of five key dimensions, each weighted differently to reflect its relative importance in the overall implementation:

1. Individual training (45 points): This dimension assessed the number of staff who had completed specialised training courses, specifically the Specialist Leader of Teaching and Diploma courses. The scores ranged from 0 points (no staff trained) to 30 points (optimal ratio of trained staff to pupils), with intermediate scores based on the number and type of staff trained.
2. Whole-staff training (25 points): This evaluated the extent of training the entire staff received. Points were awarded based on the number of completed training hours, ranging from 0 points for no training to 25 points for the full six hours.

3. Consultancy (15 points): This dimension was divided into two subcomponents: attendance at termly meetings and adherence to guidelines for staff involvement ; and adherence to staff involvement guidelines. (up to 5 points).
4. Reflective supervision (10 points): This assessed staff participation in reflective supervision training. Points were awarded based on the number of training days completed, from 0 points for no attendance to 10 points for completing all four training days.
5. Webinars (5 points): This dimension evaluated the schools' utilisation of the provided webinars. Schools received 0 points for no webinar use, 3 points for viewing one to three webinars or 5 points for viewing all four webinars.

The tool used a 100-point scale, with each dimension contributing maximum points. Points were summed across all dimensions and converted to percentages to calculate the overall engagement scores. This scoring system allowed for a nuanced assessment of engagement, considering various aspects of the intervention, from staff training to resource utilisation.

The tool is displayed in tabular format in Appendix 9.

Business as usual

Although BAU did not constitute a formal research question, it was explored qualitatively to provide important contextual insight. Information on BAU was captured primarily through in-depth case studies and longitudinal interviews with school staff. These qualitative methods enabled the research team to document existing practices and routines and to examine how BAU interacted with or was influenced by the intervention. Insights into BAU and its interaction with the intervention are included within the IPE findings.

Youth Advisory Board

A Youth Advisory Board was established for the evaluation, comprising a diverse group of young people. Recruitment deliberately targeted young people from minority ethnic backgrounds through outreach to local community groups, school networks and youth organisations to ensure that a wide range of perspectives were represented. The board played an active role in co-producing all research tools and questions, providing critical input to ensure that materials for data collection were inclusive, accessible and relevant to different experiences. Young people's contributions helped refine the language, format and focus of the interview and focus group guides, ensuring that the content was understandable, engaging and sensitive to participants from diverse backgrounds. Young people's involvement was instrumental in shaping the evaluation approach and enhancing the cultural and experiential relevance of the research instruments.

Analysis

The main focus of the IPE was to test the ToC to check whether the intervention was operating as hypothesised. The analysis employed a framework analysis approach outlined by Smith and Davies (2010), which was primarily deductive but flexible enough to capture any emerging or unforeseen themes that arose from the data.

All interviews were recorded with consent and transcribed in full. These transcripts were then analysed using a framework that incorporated the research questions and main elements of the ToC, with appropriate nodes and subnodes accompanied by detailed descriptions to ensure consistent coding. This approach involves obtaining an initial understanding of the data, constructing a preliminary framework based on the

research questions, coding or charting the data in detail according to the themes within the framework and ultimately interpreting the data within the established framework. In the initial stages of analysis, the IPE team coded several transcripts using the framework as a guide, then held a team analysis meeting to evaluate inter-rater reliability of coding decisions and address any inconsistencies.

Data synthesis aimed to triangulate evidence from different sources and methods, namely case studies, interviews, observations, focus groups and monitoring data. This approach allowed us to gather evidence from multiple perspectives, enhancing the validity and reliability of our findings. By integrating findings from diverse sources, we were able to establish robust evidence and draw well-rounded conclusions about the assumptions in the ToC, what worked well and obstacles and enablers for change.

Once the data collection was achieved, **the analysis and triangulation process** was structured on the following steps:

1. Data coding: Once the data were collected, evidence from each source was coded and categorised, identifying common themes and patterns within the data.
2. Data comparison: Findings across different sources were compared and contrasted to look for convergence/divergence.
3. Data integration: Findings from different sources were integrated to form a coherent and holistic picture. This involved identifying common themes, trends or critical insights across various data sources and complementary or contradictory evidence that added depth and richness to the analysis.
4. Triangulation: This involved triangulation of the data to understand the connections and relationships between the different data sources.
5. Interpretation and synthesis: The triangulated evidence was analysed in light of the research questions.

Limitations

The qualitative sample of young people lacked ethnic diversity, limiting our ability to understand how pupils from Black, Asian and minority ethnic backgrounds experienced the intervention. This was a significant limitation, given the evaluation's aim to assess structural factors, such as institutional racism.

Several factors contributed to this gap. Recruitment relied on schools identifying pupils for interviews, which may have introduced selection bias. We did not collect demographic data on pupils receiving targeted support until late into the study, which prevented us from targeting recruitment at underrepresented groups. Time constraints during fieldwork also limited our ability to conduct additional outreach.

For future research, we would recommend three changes. First, collect demographic data on targeted pupils early to inform purposive sampling. Second, work with schools to conduct targeted outreach to underrepresented groups before fieldwork begins. Third, allow longer fieldwork periods to enable flexible recruitment strategies.

The staff sample achieved better diversity, and findings on how minority ethnic staff perceived the intervention are reported below.

Participant flow, including losses and exclusions

Error! Reference source not found. shows the movement of the schools and pupils through each stage of the trial. We initially approached 350 schools. Of these, 78 schools and 12,725 Year 8 pupils were randomised to the intervention group (40 schools, 6,941 pupils) or the control group (38 schools, 5,784 pupils).

Schools were randomised in two waves to allow for flexible recruitment timelines. Batch 1 (December 2023) randomised 45 schools, producing 22 intervention and 23 control schools. Batch 2 (January 2024) randomised 33 schools, producing 18 intervention and 15 control schools.

Pupil-level data became available only from the baseline data collection stage. Individual pupil numbers were not collected during initial recruitment.

School-level attrition

At follow-up, we collected data from 57 schools (34 intervention, 23 control). In the intervention arm, six schools (15%) did not provide follow-up data. In the control arm, 15 schools (39%) did not provide follow-up data. This differential attrition favoured the intervention group. Schools were not excluded by the evaluation team, but they simply failed to return follow-up assessments.

Pupil-level attrition

The follow-up data collection achieved responses from 6,024 pupils, representing 47.3% of the original sample. Two control schools provided follow-up data but were excluded from analysis because their pupils lacked matching baseline data. The final analysis, therefore, included 55 schools (34 intervention, 21 control).

Of the 6,024 pupils with follow-up data, 3,970 had complete data at both baseline and follow-up, forming the final analytical sample. This sample represents 31.2% of the pupils originally randomised. Pupils categorised as “not analysed” had completed follow-up assessments but lacked the baseline measures necessary for inclusion in the primary analysis.

Attrition occurred at both the school and pupil levels. School-level attrition was substantial and differential between the arms. Pupil-level attrition within the responding schools further reduced the analytical sample. The following demographic characteristics describe the pupils at each stage of data collection.

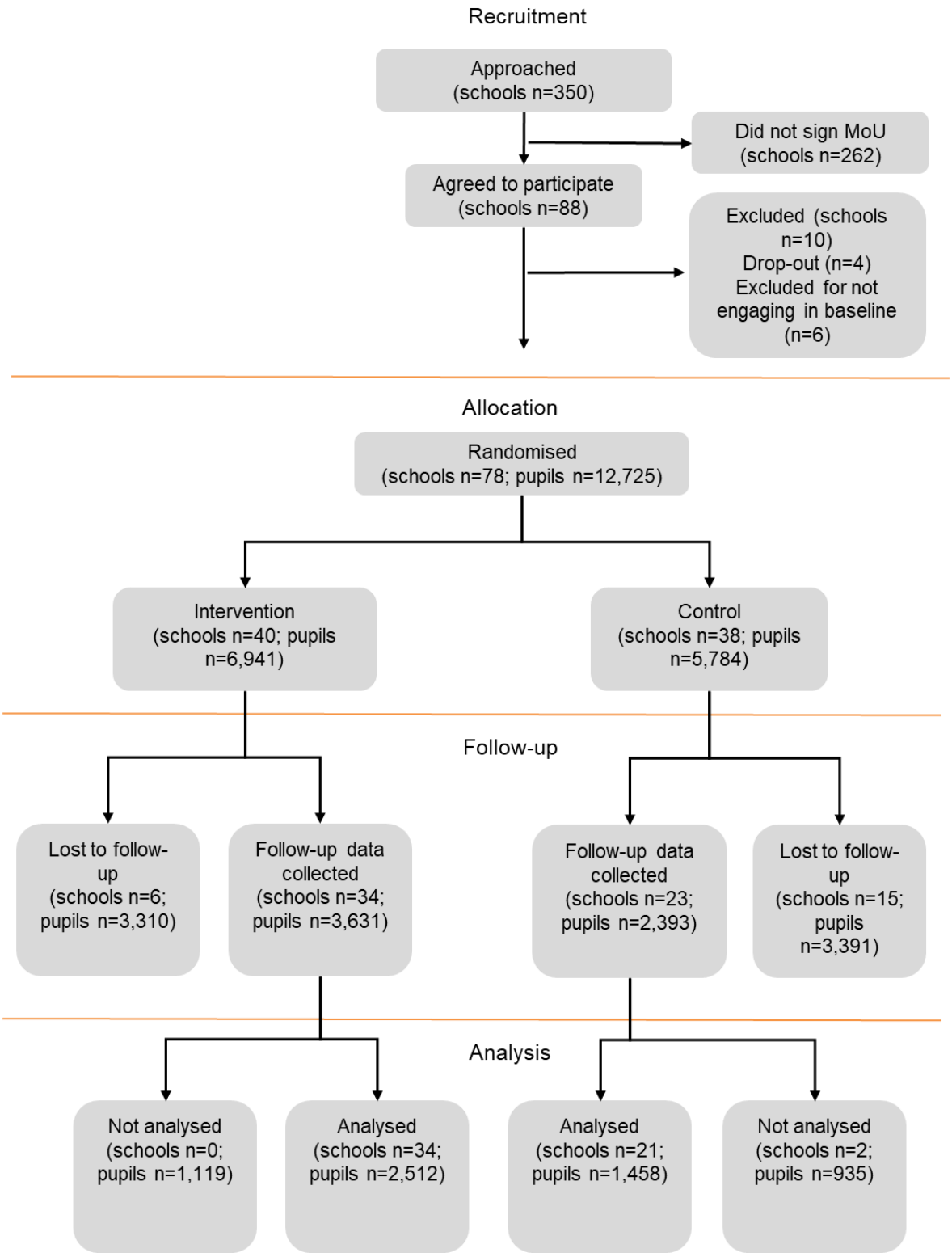


Figure 1. Participant flow

Table 7 presents the demographic composition based on the available data at different stages of the trial. Neither the delivery team nor the evaluators were able to collect individual-level data during the "Referred" and "Recruited" phases; hence, these columns remain empty.

The "Randomised" and "T0 data" columns show identical figures because demographic data collection began only after randomisation during baseline testing. "T1 data" represents pupils with valid primary outcome measurements at follow-up. The "Final sample" comprises pupils who provided data at both baseline and follow-up, forming the matched dataset for the primary analysis.

This structure reflects the timing of data collection, with comprehensive pupil demographics only becoming available once schools had committed to baseline participation.

Table 7: Demographic characteristics (pupil level for primary outcome)

	Referred	Recruited	Randomised	T0 data	T1 data	Final sample
Ethnicity						
Asian or Asian British	--	--	1,428	1,428	676	425
Treatment	--	--	798	798	469	301
Control	--	--	630	630	207	124
Black, Black British, Caribbean or African	--	--	705	705	370	206
Treatment	--	--	398	398	226	134
Control	--	--	307	307	144	72
Mixed or multiple ethnic groups	--	--	844	844	399	264
Treatment	--	--	483	483	253	175
Control	--	--	361	361	146	89
White	--	--	8,228	8,228	4,001	2,870
Treatment	--	--	4,479	4,479	2,337	1,765
Control	--	--	3,749	3,749	1,664	1,105
Other ethnic group	--	--	376	376	171	116
Treatment	--	--	205	205	103	73
Control	--	--	171	171	68	43
Sex						
Male	--	--	6,343	6,343	3,046	2,122
Treatment	--	--	3,457	3,457	1,800	1,326
Control	--	--	2,886	2,886	1,246	796
Female	--	--	5,716	5,716	2,665	1,771
Treatment	--	--	3,152	3,152	1,652	1,137
Control	--	--	2,564	2,564	1,013	634
Prefer not to say	--	--	244	244	117	62
Treatment	--	--	134	134	78	40
Control	--	--	110	110	39	22

Table 8 presents the school staff's demographic characteristics, following the same data collection structure described above for pupils. Staff demographic data were only available from baseline onwards, with the final sample representing staff who completed surveys at both time points.

Table 8: Demographic characteristics (school staff level)

	Referred	Recruited	Randomised	T0 data	T1 data	Final sample
Ethnicity						
Asian or Asian British	--	--	182	182	35	22
Treatment	--	--	82	82	29	18
Control	--	--	100	100	6	4
Black, Black British, Caribbean or African	--	--	68	68	22	9
Treatment	--	--	33	33	20	8
Control	--	--	35	35	2	1
Mixed or multiple ethnic groups	--	--	119	119	25	15
Treatment	--	--	63	63	18	10
Control	--	--	56	56	7	5
White	--	--	3,540	3,540	1,099	614
Treatment	--	--	1,893	1,893	893	501
Control	--	--	1,647	1,647	206	113
Other ethnic group	--	--	18	18	8	4
Treatment	--	--	10	10	7	3
Control	--	--	8	8	1	1
Sex						
Male	--	--	1,048	1,048	329	166
Treatment	--	--	539	539	253	123
Control	--	--	509	509	76	43
Female	--	--	2,918	2,918	867	499
Treatment	--	--	1,561	1,561	720	417
Control	--	--	1,357	1,357	147	82
Prefer not to say	--	--	19	19	7	3
Treatment	--	--	12	12	7	3
Control	--	--	7	7	0	0

Attrition

Table 9 presents substantial attrition rates across both trial arms, with 68.8% of randomised pupils lacking primary outcome data at follow-up. The control group experienced notably higher attrition (74.8%) compared with the intervention group (63.8%). This pattern reflects differential school-level non-response between arms. Control schools were more likely not to return follow-up assessments, likely due to internal capacity constraints and competing priorities rather than active withdrawal from the trial.

Attrition occurred at both the school and pupil levels. At the school level, 21 schools (27%) did not return follow-up assessments. Control schools were more likely to disengage, likely because intervention schools remained connected through ongoing programme delivery. At the pupil level, even within responding schools, many pupils who completed baseline assessments did not complete follow-up assessments. This reflected timetabling difficulties, pupil absence on assessment days or schools' prioritisation of certain classes.

A further challenge was matching baseline and follow-up records. Identifying information (name, date of birth, ethnicity, postcode) was self-reported by pupils. Although these fields were mandatory, pupils could vary their responses across time points, whether unintentionally or to avoid identification. This prevented us from linking records even when both assessments were completed.

We maintained regular contact with schools through our School Liaison Officer, which included weekly tracking and ongoing support. However, control schools lacked the regular touchpoints that intervention schools had through the training activities. Future trials might consider additional engagement strategies for control schools and more robust pupil identification procedures, such as the use of school-assigned identifiers rather than self-reported information.

Parental opt-out was minimal, affecting only 28 individuals at baseline, with no additional opt-outs at follow-up. Attrition, therefore, reflected logistical and data-matching challenges rather than the active withdrawal of consent.

Table 9: Pupil-level attrition from the trial (primary outcome)

		Intervention	Control	Total
Number of participants	Randomised	6,941	5,784	12,725
	Analysed	2,512	1,458	3,970
Participant attrition (from randomisation to analysis)	Number	4,429	4,326	8,755
	Percentage	63.8%	74.8%	68.8%

Table 10 and Table 11 present the demographic composition of the pupils who were retained versus those who were lost to follow-up, with valid answers for the respective demographic questions.

The distributions of ethnicity and sex remained broadly similar between the two groups, with no substantial differences in representation across demographic categories. For instance, White pupils comprised 74% of those retained compared with 69.6% of those lost to follow-up, while the sex distribution showed similarly small variations. Formal tests reported in the missing data analysis (see Outcomes and analysis) confirmed no statistically significant association between attrition and pupil characteristics, including ethnicity and sex.

The absence of systematic differences in individual pupil characteristics suggests that attrition was not driven by selective dropout based on ethnicity or sex. This provides some reassurance that estimates are unlikely to be biased by these observable individual-level factors. However, attrition was concentrated at the school level, where unobserved factors – such as leadership changes, resource constraints or competing priorities – may still confound the relationship between treatment assignment and outcomes.

Table 10: Ethnicity distribution by attrition status

Ethnicity	Retained (%)	Lost to follow-up (%)	Total (%)
Asian or Asian British	10.9	13.0	12.3
Black, Black British, Caribbean or African	5.3	6.5	6.1
Mixed or multiple ethnic groups	6.8	7.5	7.3
White	73.9	69.6	71.1
Other ethnic group	2.9	3.4	3.3
Sample sizes (n)	3,881	7,700	11,581

Table 11: Sex distribution by attrition status

Sex	Retained (%)	Lost to follow-up (%)	Total (%)
Male	53.7	50.6	51.6
Female	44.8	47.3	46.5
Prefer not to say	1.6	2.2	1.9
Sample sizes (n)	3,955	8,348	12,303

Participant characteristics

Table 12 shows the baseline characteristics for the intervention and control groups. Both the categorical variables (such as ethnicity and sex) and continuous measures show generally small differences across the trial arms, indicating successful randomisation. For survey-based outcomes, standardised effect sizes ranged from −0.13 to 0.09. Administrative data showed similarly small effect sizes. The IMD score showed a slightly larger difference (Hedges' g = 0.17), with intervention schools serving marginally more deprived areas. This reflects area-level rather than individual-level variation and was controlled for in all outcome analyses.

Table 12: Baseline characteristics of groups as randomised

School level (categorical)	National level mean	Intervention group		Control group		
		n/N (missing)	Count (%)	n/N (missing)	Count (%)	
Above the median of free school meals at the school level		20/40 (0 missing)	50.00%	19/38 (0 missing)	50.00%	
Pupil level (categorical)		**n/N (missing)**	**Count (%)**	**n/N (missing)**	**Count (%)**	
Asian or Asian British	9.3%	800/6,363 (578 missing)	12.60%	632/5,218 (566 missing)	12.10%	
Black, Black British, Caribbean or African	4.0%	398/6,363 (578 missing)	6.30%	307/5,218 (566 missing)	5.90%	
Mixed or multiple ethnic groups	2.9%	484/6,363 (578 missing)	7.60%	361/5,218 (566 missing)	6.90%	
White	81.7%	4,478/6,363 (578 missing)	70.40%	3,744/5,218 (566 missing)	71.80%	
Other ethnic group	2.1%	203/6,363 (578 missing)	3.20%	174/5,218 (566 missing)	3.30%	
Male	49%	3,457/6,741 (200 missing)	51.30%	2,887/5,559 (225 missing)	51.90%	
Female	51%	3,151/6,741 (200 missing)	46.70%	2564/5,559 (225 missing)	46.10%	
Prefer not to say	N/A	133/6,741 (200 missing)	2.00%	108/5,559 (225 missing)	1.90%	
Pupil level (continuous)		**n/N (missing)**	**Mean (SD)**	**n/N (missing)**	**Mean (SD)**	**Effect size**
Externalising behaviour (primary)		6,941/6,941 (0 missing)	7.61 (4.35)	5,784/5,784 (0 missing)	7.49 (4.26)	−0.03
Internalising behaviour		6,941/6,941 (0 missing)	6.07 (3.73)	5,783/5,784 (1 missing)	5.95 (3.73)	−0.03

Prosocial behaviour		6,941/6,941 (0 missing)	6.99 (2.01)	5,784/5,784 (0 missing)	6.92 (2.04)	−0.03
Total difficulties		6,941/6,941 (0 missing)	13.68 (6.73)	5,783/5,783 (1 missing)	13.44 (6.61)	−0.04
School connectedness		6,847/6,847 (94 missing)	2.48 (0.33)	5,693/5,693 (91 missing)	2.48 (0.32)	0.02
Wellbeing (SWEMWBS)		6,778/6,778 (163 missing)	22.60 (5.38)	5,627/5,627 (157 missing)	22.72 (5.37)	0.02
Psychological distress (GHQ-12)		6,941/6,941 (0 missing)	12.55 (7.64)	5,784/5,784 (0 missing)	12.22 (7.43)	−0.04
Age (years)		6,549/6,549 (392 missing)	12.25 (0.5)	5,609/5,609 (175 missing)	12.26 (0.45)	0.01
IMD score[15]		2,494/2,494 (4,447 missing)	24.41 (17.01)	1,455/1,455 (4,329 missing)	20.41 (14.87)	−0.25
Staff-level (categorical)		**n/N (missing)**	**Count (%)**	**n/N (missing)**	**Count (%)**	
Asian or Asian British	5.8%	82/2,081 (95 missing)	3.90%	100/1,846 (82 missing)	5.40%	
Black, Black British, Caribbean or African	2.9%	33/2,081 (95 missing)	1.60%	35/1,846 (82 missing)	1.90%	
Mixed or multiple ethnic groups	1.9%	63/2,081 (95 missing)	3.00%	56/1,846 (82 missing)	3.00%	
White	87.3%	1,893/2,081 (95 missing)	91.00%	1,647/1,846 (82 missing)	89.20%	
Other ethnic group	0.9%	10/2,081 (95 missing)	0.50%	8/1,846 (82 missing)	0.40%	
Male	24.4%	539/2,112 (64 missing)	25.50%	509/1,873 (55 missing)	27.20%	
Female	75.6%	1,561/2,112 (64 missing)	73.90%	1,357/1,873 (55 missing)	72.50%	
Prefer not to say	N/A	12/2,112 (64 missing)	0.60%	7/1,873 (55 missing)	0.40%	
Staff level (continuous)		**n/N (missing)**	**Mean (SD)**	**n/N (missing)**	**Mean (SD)**	**Effect size**
ARTIC total score		2,176/2,176 (0 missing)	5.10 (0.62)	1,928/1,928 (0 missing)	5.13 (0.62)	0.04
ARTIC: Understanding trauma causes		2,176/2,176 (0 missing)	4.68 (0.75)	1,928/1,928 (0 missing)	4.70 (0.75)	0.03

[15] The IMD score measures relative deprivation across small areas in England. It combines 39 indicators across seven domains: income, employment, health, education, crime, housing and living environment. Higher scores indicate greater deprivation. National scores range from approximately 0 to 85.

ARTIC: Appropriate responses		2,176/2,176 (0 missing)	5.06 (0.84)	1,928/1,928 (0 missing)	5.06 (0.84)	0
ARTIC: Workplace behaviour		2,176/2,176 (0 missing)	5.34 (0.71)	1,928/1,928 (0 missing)	5.35 (0.70)	0.01
ARTIC: Self-efficacy		2,176/2,176 (0 missing)	5.28 (0.93)	1,928/1,928 (0 missing)	5.36 (0.91)	0.09
ARTIC: Personal reactions		2,176/2,176 (0 missing)	5.15 (0.83)	1,928/1,928 (0 missing)	5.16 (0.87)	0.01
Wellbeing (SWEMWBS)		2,102/2,102 (74 missing)	24.49 (4.17)	1,872/1,872 (56 missing)	24.68 (4.11)	0.05
Pupil level – administrative data (continuous)		**n/N (missing)**	**Mean (SD)**	**n/N (missing)**	**Mean (SD)**	**Effect size**
Number of suspensions		4,500/6,268 (1,768 missing)	0.08 (0.57)	1,619/1,786 (167 missing)	0.10 (0.55)	0.03
Permanent exclusion		6,256/6,268 (12 missing)	0.95 (0.22)	1,759/1,786 (27 missing)	0.85 (0.36)	0.07
Authorised absences (sessions)		4,694/6,268 (1,574 missing)	5.80 (8.37)	1,721/1,786 (65 missing)	6.71 (9.15)	−0.04
Unauthorised absences (sessions)		4,607/6,268 (1,661 missing)	2.88 (8.36)	1,721/1,786 (65 missing)	3.21 (10.32)	−0.01
School level – administrative data (continuous)		**n/N (missing)**	**Mean (SD)**	**n/N (missing)**	**Mean (SD)**	**Effect size**
Total suspensions		20/20 (0 missing)	121.20 (124.4)	7/7 (0 missing)	51.57 (66.9)	0.59
Total permanent exclusions		20/20 (0 missing)	0.90 (1.96)	7/7 (0 missing)	0.29 (0.5)	0.42
Total authorised absences		19/20 (1 missing)	1,5058.9 (4,2276.2)	7/7 (0 missing)	3,532 (2,762.9)	0.31
Total unauthorised absences		19/20 (1 missing)	3,670.4 (3,644.3)	7/7 (0 missing)	2,350.6 (2,852.5)	0.37
Staff outcomes at the school level – administrative data (continuous)		**n/N (missing)**	**Mean (SD)**	**n/N (missing)**	**Mean (SD)**	**Effect size**
Total sick days (teaching staff)		17/28 (11 missing)	188.4 (101)	7/11 (4 missing)	170.6 (103.1)	0.17
Staff departures		17/28 (11 missing)	2.3 (2.4)	7/11 (4 missing)	7 (11.7)	−0.68

Notes: SD = standard deviation; IMD = index of multiple deprivation; GHQ-12 = General Health Questionnaire-12; SWEMWBS = Short Warwick–Edinburgh Mental Wellbeing Scale; ARTIC = Attitudes Related to Trauma-Informed Care.

Histograms showing the distribution of baseline and follow-up scores for all survey outcomes are presented in Appendix 2. The distributions are approximately normal, with no notable floor or ceiling effects, supporting the use of multilevel linear models.

Outcomes and analysis

This section presents ITT analyses for all primary and secondary outcomes. All analyses were pre-specified in the SAP. We have reported adjusted mean differences and standardised effect sizes (Hedges' g) with 95% CIs. Multilevel models account for pupil clustering within schools, controlling for baseline outcomes and pupil characteristics. Baseline characteristics and pre-test differences between groups are reported as effect sizes in Table 12 above. Histograms showing the distribution of baseline outcomes are provided in Appendix 2.

The YEF classifies effect sizes as follows: −0.02 to 0.02 as no effect, +/−0.02 to +/−0.10 as low effects, +/−0.10 to +/−0.25 as moderate effects and above +/−0.25 as large effects. These thresholds reflect the typical effect sizes for interventions that target young people at risk of violence rather than general social science benchmarks.

Intracluster correlation coefficients

ICCs measure the proportions of variance attributable to differences between schools. For the primary outcome (externalising behaviour), the ICC at the school level was 0.021 at follow-up. This indicates that 2% of the variation in pupil outcomes was due to school-level differences, with the remainder reflecting individual pupil variations. All effect sizes were calculated using pooled SDs from empty multilevel models, ensuring they accounted for clustering at the school level. ICCs for secondary outcomes are reported at the end of this section.

Primary analysis

Table 13 shows the results of the headline ITT analysis for the primary outcome. The externalising behaviour measure is scored 0–20, with higher scores representing more externalising behaviours. A negative effect size would indicate fewer behavioural problems.

The effect size of −0.02 (95% CI: −0.10 to 0.06, p-value = 0.64) provides no evidence that the programme reduced externalising behaviours. The intervention group scored an average of 7.00 compared with 6.94 in the control group. This difference is at the boundary between what the YEF classifies as no effect and a small positive effect.

The CI should be interpreted cautiously. Although relatively narrow, precision estimates assume that attrition was unrelated to outcomes. Given the differential attrition between trial arms, this assumption may not hold. The CI ranges from −0.10 to 0.06. The upper bound falls within the range that the trial lacked power to detect. The lower bound marginally exceeds the original MDES of ±0.094, but both values represent trivially small effects under the YEF's classification system.

Outcomes have different scoring directions. The "Expected direction" column shows whether reductions (negative coefficients) are beneficial (+) or harmful (−). For example, lower externalising behaviours are positive (+), whilst lower wellbeing is negative (−).

Table 13: Primary analysis – pupil level

	Unadjusted means				Effect size			Expected direction
	Intervention group		Control group					
Outcome	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	p-value	
Externalising behaviour (primary)	2,314 (0)	7.00 (6.85, 7.16)	1,371 (0)	6.94 (6.74, 7.14)	3,685 (2,314; 1,371)	−0.02 (−0.10, 0.06)	0.64	(+)

Notes: CI = confidence interval. All analyses were adjusted for baseline outcome, sex, ethnicity and free school meals, with random effects for schools. Effect sizes for all outcomes were calculated using Hedges' g. The denominator is the pooled standard deviation from an empty multilevel model, which accounts for clustering at the school level.

Secondary analysis

Table 14 presents the results for secondary outcomes, using the same ITT analysis approach as the primary outcome. The measures use different scales: internalising behaviour and total difficulties (0–40, higher scores indicating more problems), prosocial behaviour (0–10, higher scores indicating better outcomes), school connectedness (1–6, higher scores indicating stronger connection), wellbeing (7–35, higher scores indicating better wellbeing) and psychological distress (0–12, higher scores indicating more distress).

Internalising behaviour shows a low impact in the beneficial direction (Hedges' g = −0.08, 95% CI: −0.17 to 0.00, p = 0.04). This suggests the programme may have reduced emotional problems or peer difficulties, though the effect was small. Total difficulties also show a low impact in the beneficial direction (g = −0.06, 95% CI: −0.15 to 0.03, p = 0.17), as does psychological distress (g = −0.05, 95% CI: −0.14 to 0.05, p = 0.36).

School connectedness shows a low impact but in the unintended direction (g = −0.04, 95% CI: −0.14 to 0.06, p = 0.41). This indicates slightly lower connectedness in the intervention group, though the CI includes zero and effects in both directions.

Prosocial behaviour (g = −0.01, 95% CI: −0.10 to 0.08, p = 0.76) and wellbeing (g = 0.01, 95% CI: −0.07 to 0.09, p = 0.77) show no meaningful difference between the groups under the YEF's classification.

Table 14: Secondary analysis – pupil level

	Unadjusted means				Effect size			Expected direction
	Intervention group		Control group					
Outcome	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	p-value	
Internalising behaviour	2,314 (0)	5.73 (5.59, 5.87)	1,368 (3)	5.96 (5.77, 6.15)	3,682 (2,314; 1,368)	−0.08 (−0.17, 0.00)	0.04	(+)
Prosocial behaviour	2,314 (0)	7.01 (6.93, 7.09)	1,371 (0)	7.05 (6.95, 7.15)	3,685 (2,314; 1,371)	−0.01 (−0.10, 0.08)	0.76	(−)
Total difficulties	2,314 (0)	12.73 (12.47, 12.99)	1,368 (3)	12.89 (12.56, 13.23)	3,682 (2,314; 1,368)	−0.06 (−0.15, 0.03)	0.17	(+)
School connectedness	2,251 (49)	2.45 (2.44, 2.47)	1,338 (28)	2.49 (2.47, 2.51)	3,589 (2,251; 1,338)	−0.04 (−0.14, 0.06)	0.41	(−)
Wellbeing (SWEMWBS)	2,212 (76)	23.13 (22.92, 23.34)	1,320 (44)	23.26 (22.98, 23.54)	3,532 (2,212; 1,320)	0.01 (−0.07, 0.09)	0.77	(+)
Psychological distress (GHQ-12)	2,314 (0)	11.48 (11.22, 11.74)	1,371 (0)	11.79 (11.44, 12.15)	3,685 (2,314; 1,371)	−0.05 (−0.14, 0.05)	0.36	(+)

Notes: CI = confidence interval; SWEMWBS = Short Warwick–Edinburgh Mental Wellbeing Scale; GHQ-12 = General Health Questionnaire-12. All analyses were adjusted for baseline outcome, sex, ethnicity and free school meals, with random effects for schools. Effect sizes for all outcomes were calculated using Hedges' g. The denominator is the pooled standard deviation from an empty multilevel model, which accounts for clustering at the school level.

Table 15 presents the results for staff-level secondary outcomes, examining changes in TIC attitudes and wellbeing. The ARTIC total score measures attitudes towards TIC (1–7 scale, with higher scores indicating more positive attitudes), while wellbeing uses the same SWEMWBS scale as that used for pupils (7–35, with higher scores indicating better wellbeing). We used the ARTIC-35 version, chosen at co-design, to minimise the burden on teachers.

The ARTIC total score shows a high impact in the beneficial direction (Hedges' g = 1.24, 95% CI: 0.05 to 2.42, p = 0.041). This suggests staff developed more positive attitudes towards TIP. The disparity in sample sizes between groups (544 intervention, 125 control) reflects the differential school-level attrition described earlier. Control schools were less likely to return follow-up assessments, resulting in substantially fewer staff responses.

This large effect size should be interpreted cautiously for two reasons. First, the small control group sample may have produced unstable variance estimates, inflating the effect size. Second, staff who remained engaged with the evaluation may have differed systematically from those who did not respond. The wide CI (0.05 to 2.42) reflects this uncertainty. Whilst the direction of the effect aligns with the programme's ToC, the magnitude may overstate the true impact.

Several ARTIC subscales show large effects, though all should be interpreted with the same caution as the total score. "Understanding trauma causes" (g = 0.81, 95% CI: −0.11 to 1.74, p = 0.085) captures staff beliefs about the origins of challenging behaviours. "Self-efficacy" (g = 0.51, 95% CI: −0.58 to 1.61, p = 0.357) measures confidence in supporting traumatised young people. "Personal reactions" (g = 3.90, 95% CI: −0.46

to 8.27, p = 0.079) reflects staff capacity to manage vicarious trauma and emotional demands. These dimensions are important for workforce sustainability, as staff who feel equipped to support vulnerable pupils and manage their own responses are more likely to remain in post.

Staff wellbeing shows a large effect (g = 0.32, 95% CI: −0.66 to 1.31, p = 0.52), though the wide CI, which spans zero, reflects uncertainty from the smaller sample. The ARTIC findings represent the clearest evidence of intervention impact, aligning with the programme's ToC, which posits staff attitude change as a precursor to pupil-level effects.

Table 15: Secondary analysis – staff level

	Unadjusted means				Effect size			Expected direction
	Intervention group		Control group					
Outcome	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	p-value	
ARTIC total score	544 (0)	5.34 (5.29, 5.40)	125 (0)	5.23 (5.12, 5.35)	669 (544; 125)	1.24 (0.05, 2.42)	0.041	(+)
ARTIC: Understanding trauma causes	544 (0)	4.92 (4.86, 4.99)	125 (0)	4.79 (4.64, 4.93)	669 (544; 125)	0.81 (−0.11, 1.74)	0.085	(+)
ARTIC: Appropriate responses	544 (0)	5.29 (5.22, 5.36)	125 (0)	5.08 (4.92, 5.25)	669 (544; 125)	0.51 (−0.23, 1.24)	0.179	(+)
ARTIC: Workplace behaviour	544 (0)	5.56 (5.50, 5.62)	125 (0)	5.52 (5.39, 5.65)	669 (544; 125)	1.42 (−0.81, 3.66)	0.212	(+)
ARTIC: Self-efficacy	544 (0)	5.54 (5.47, 5.62)	125 (0)	5.48 (5.32, 5.64)	669 (544; 125)	0.51 (−0.58, 1.61)	0.357	(+)
ARTIC: Personal reactions	544 (0)	5.39 (5.32, 5.46)	125 (0)	5.30 (5.15, 5.45)	669 (544; 125)	3.90 (−0.46, 8.27)	0.079	(+)
Wellbeing (Short Warwick–Edinburgh Mental Wellbeing Scale)	530 (14)	25.47 (25.11, 25.82)	124 (1)	24.73 (23.98, 25.49)	654 (530; 124)	0.32 (−0.66, 1.31)	0.52	(+)

Notes: CI = confidence interval; ARTIC = Attitudes Related to Trauma-Informed Care.

Administrative data

The administrative data analysis examines secondary outcomes at three levels: 1) pupil-level individual data on suspensions, exclusions and absences; 2) school-level aggregates of these same pupil outcomes; and 3) school-level staff data on sick days and departures. Unlike the survey data collected directly by the evaluation team, the administrative data required the schools to extract and submit records from their management information systems. This placed an additional burden on school staff, and coverage varied across outcomes as a result. Financial contributions were provided to all schools to support their participation in data collection activities, as described in the Methods section.

Pupil individual-level data use multilevel models that account for clustering within the schools. The covariates include the baseline measurement of each outcome and pupil-level characteristics (age, ethnicity, FSM status). The model also includes the school-level stratification variable used at randomisation, indicating whether the pupil's school was above or below the median FSM proportion. School-level pupil

aggregates and staff outcomes use ordinary least square (OLS) regression with robust SEs, as each school provided a single observation. All models control for baseline values of the outcome.

Table 16 shows the number of schools and pupils that provided valid outcome data at the individual level.[16] These data cover the target population (Year 8 pupils only). Coverage is highest for permanent exclusions (39 schools, 8,015 pupils) and lowest for absence measures (38 schools, approximately 6,200 pupils). Submission rates differed between trial arms: 28 intervention schools and 11 control schools provided records, reflecting the differential school-level attrition described earlier.

Table 16: Pupil-level sample sizes by outcome (administrative data)

Pupil-level outcome	Number of schools (pupils)		
	Treatment	Control	Total
Suspensions	28 (4,397)	11 (1,691)	39 (6,088)
Permanent exclusions	28 (6,256)	11 (1,759)	39 (8,015)
Authorised absences	27 (4,593)	11 (1,681)	38 (6,274)
Unauthorised absences	27 (4,525)	11 (1,680)	38 (6,205)

[16] 78 schools were randomised. Four dropped out after randomisation (all intervention), leaving 74 in the trial. Of these, 39 provided administrative data.

Table 17 shows pupil-level administrative outcomes for Year 8 pupils. Although 39 schools submitted records, the regression samples are substantially smaller due to missing covariate information. For example, suspension data were available for 6,088 pupils, but only 2,328 had complete information on the baseline outcomes, ethnicity, age and FSM status required for the regression model. This represents a 62% reduction from the submitted data. Wide CIs throughout Table 17 reflect the reduced analytical samples and associated uncertainty.

Suspensions show a large effect in the unintended direction (g = 0.29, 95% CI: −0.37 to 0.95), indicating more suspensions in intervention schools. Permanent exclusions show a similar pattern, with a large effect in the unintended direction (g = 0.35, 95% CI: −0.48 to 1.19). However, the wide CIs for both outcomes include zero and effects in either direction, so these findings should be interpreted cautiously.

Authorised absences show a low impact in the beneficial direction (g = −0.09, 95% CI: −0.33 to 0.15), whilst unauthorised absences show a low impact in the unintended direction (g = 0.07, 95% CI: −0.18 to 0.31). Both CIs span zero, indicating no clear effect.

Table 17: Secondary analysis – pupil level (administrative data)

	Unadjusted means				Effect size			Expected direction
	Intervention group		Control group					
Outcome	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	p-value	
Number of suspensions	1,925 (1,871)	0.11 (0.09, 0.12)	403 (95)	0.07 (0.05, 0.09)	2,328 (1,925; 403)	0.29 (−0.37, 0.95)	0.39	(−)
Permanent exclusion	2,327 (12)	0.90 (0.89, 0.90)	505 (27)	0.80 (0.78, 0.82)	2,832 (2,327; 505)	0.35 (−0.48, 1.19)	0.41	(−)
Authorised absences (sessions)	2,250 (1,675)	6.22 (5.93, 6.50)	503 (105)	7.73 (7.08, 8.37)	2,753 (2,250; 503)	−0.09 (−0.33, 0.15)	0.46	(+)
Unauthorised absences (sessions)	2,250 (1,743)	3.51 (3.17, 3.84)	503 (106)	3.52 (2.94, 4.11)	2,753 (2,250; 503)	0.07 (−0.18, 0.31)	0.60	(−)

Notes: CI = confidence interval; ARTIC = Attitudes Related to Trauma-Informed Care.

Table 18 shows the number of schools that provided aggregate pupil outcome data. These are school-level totals (such as total suspensions across all pupils) rather than individual pupil records. Coverage ranges from 26 to 27 schools, depending on the outcome.

Table 18: Sample sizes for school-level pupil outcomes (administrative data)

Outcome	Treatment	Control	Total
Total suspensions	20	7	27
Total permanent exclusions	20	7	27
Total authorised absences	19	7	26
Total unauthorised absences	19	7	26

Table 19 presents school-level aggregate data from 27 schools (20 intervention, 7 control). The small number of schools, particularly in the control group, limits the reliability of these comparisons, and findings should be interpreted with considerable caution.

Total suspensions show a low effect in the unintended direction (g = 0.23, 95% CI: 0.01 to 0.46, p = 0.06), with intervention schools reporting higher suspension counts than control schools. Total permanent exclusions show a low impact in the unintended direction (g = 0.17, 95% CI: −0.48 to 0.81, p = 0.62). Authorised absences show no meaningful difference between groups (g = 0.01, 95% CI: −0.03 to 0.04, p = 0.72), whilst unauthorised absences show a low impact in the unintended direction (g = 0.10, 95% CI: −0.20 to 0.40, p = 0.53). With the exception of total suspensions, CIs are wide and include zero, reflecting the limited sample.

Table 19: Secondary analysis – school level (administrative data)

	Unadjusted means				Effect size			Expected direction
	Intervention group		Control group					
Outcomes at follow-up	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	p-value	
Total suspensions	20 (0)	113.45 (68.23, 158.67)	7 (0)	37.57 (13.72, 61.42)	27 (20; 7)	0.23 (0.01, 0.46)	0.06	(–)
Total permanent exclusions	20 (0)	0.45 (0.12, 0.78)	7 (0)	0.29 (−0.08, 0.65)	27 (20; 7)	0.17 (−0.48, 0.81)	0.62	(–)
Total authorised absences	19 (1)	14,766.04 (−3,060.32, 32,592.40)	7 (0)	3,685.00 (1,616.93, 5,753.07)	26 (19; 7)	0.01 (−0.03, 0.04)	0.72	(–)
Total unauthorised absences	19 (1)	3,633.76 (2,218.18, 5,049.34)	7 (0)	2,142.29 (−158.52, 4,443.09)	26 (19; 7)	0.10 (−0.20, 0.40)	0.53	(–)

Notes: CI = confidence interval.

Table 20 shows the number of schools that provided staff outcome data. These are school-level aggregates (total sick days and departures for all teaching staff) rather than individual staff records. 25 schools provided valid data across both outcomes (18 intervention, 7 control).

Table 20: Number of non-missing observations by staff-level outcomes (administrative data)

Outcome	Treatment	Control	Total
Total sick days	18	7	25
Staff departures	18	7	25

Table 21 shows staff outcomes from 24 schools (one school was excluded due to missing covariates). As with pupil administrative data, the small number of schools – particularly in the control group – limits the reliability of these comparisons.

Total sick days show a low impact in the beneficial direction (g = −0.08, 95% CI: −0.41 to 0.25, p = 0.64), indicating slightly fewer sick days in intervention schools. Staff departures show a low effect in the unintended direction (g = 0.05, 95% CI: −0.28 to 0.38, p = 0.77), indicating slightly more departures in the intervention schools, though the CI spans zero. Both CIs are wide and include zero, reflecting the limited sample. These findings suggest minimal intervention impact on staff wellbeing or retention as measured through administrative records, though the small sample sizes mean we cannot draw firm conclusions.

Table 21: Secondary analysis – staff level (administrative data)

	Unadjusted means				Effect size			Expected direction
	Intervention group		Control group					
Outcome	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	p-value	
Total sick days (teaching staff)	17 (10)	197.66 (141.84, 253.48)	7 (4)	180.64 (64.85, 296.43)	24 (17; 7)	−0.08 (−0.81, 0.65)	0.83	(+)
Staff departures	17 (10)	2.28 (1.19, 3.36)	7 (4)	6.43 (−2.16, 15.01)	24 (17; 7)	0.05 (−0.18, 0.29)	0.67	(−)

Notes: CI = confidence interval.

The administrative data show no consistent evidence of intervention effects across pupil, school or staff outcomes. The limited school coverage and variable data quality constrain interpretation. These administrative measures complement the primary survey-based outcomes but provide no additional evidence of intervention impact.

Analysis in the presence of non-compliance

The SAP specified a CACE analysis using two-stage least squares regression with instrumental variables. However, the models did not converge due to limited variation in compliance at the school level. We therefore adopted an alternative approach, dividing the ITT effect by the proportion of pupils in compliant schools. This method provides a sensitivity analysis that tests whether accounting for differential engagement changes the interpretation of the results.

Compliance was defined through an engagement tool that measured school participation across five key activities: individual training, whole-staff training, consultancy sessions, reflective supervision and webinar usage. This tool corresponds to the RAG assessment described in the IPE section. Schools that scored 76% or above were classified as compliant.

Table 22 presents compliance rates at different thresholds. The "% of treatment pupils" column shows what proportion of all intervention pupils attended schools meeting at each threshold. For example, at the 76% threshold, 27 schools (79% of intervention schools) were compliant, covering 2,100 pupils (84% of all intervention pupils). Higher thresholds naturally reduce coverage, as fewer schools meet stricter criteria.

Table 22: Pupil-level compliance rates by threshold

Compliance thresholds	Pupils in compliant schools	% of treatment pupils in compliant schools	Number of compliant schools	Total treatment schools
≥76%	2,100	84%	27	34
≥80%	1,951	78%	25	34
≥90%	1,477	59%	19	34

Table 23 presents the CACE analysis for primary and secondary outcomes at the 76% compliance threshold. The CACE effect is calculated by dividing the ITT effect by the proportion of pupils in compliant schools (84%). This inflates the effect estimate, reflecting the hypothetical impact if all intervention schools had achieved full compliance. Estimates include SEs in parenthesis.

Table 23: Complier average causal effect analysis on primary and secondary outcomes for the 76% threshold

Outcome	ITT effect (SE)	ITT Hedges' g (95% CI)	p-value	CACE effect (SE)	CACE Hedges' g (95% CI)
Externalising behaviour (primary)	−0.074 (0.159)	−0.02 (−0.10, 0.06)	0.64	−0.089 (0.190)	−0.02 (−0.12, 0.07)
Internalising behaviour	−0.309 (0.153)	−0.08 (−0.17, 0.00)	0.04	−0.370 (0.183)	−0.10 (−0.20, 0.00)
Prosocial behaviour	−0.028 (0.091)	−0.01 (−0.10, 0.08)	0.76	−0.034 (0.109)	−0.02 (−0.12, 0.09)
Total difficulties	−0.390 (0.285)	−0.06 (−0.15, 0.03)	0.17	−0.467 (0.341)	−0.07 (−0.17, 0.03)
School connectedness	−0.013 (0.016)	−0.04 (−0.14, 0.06)	0.41	−0.015 (0.019)	−0.05 (−0.17, 0.07)
Wellbeing (SWEMWBS)	0.060 (0.207)	0.01 (−0.07, 0.09)	0.77	0.071 (0.248)	0.01 (−0.08, 0.11)
Psychological distress (GHQ-12)	−0.305 (0.333)	−0.05 (−0.14, 0.05)	0.36	−0.365 (0.399)	−0.05 (−0.17, 0.06)

Notes: CACE = complier average causal effect; ITT = intention-to-treat; CI = confidence interval; SE = standard error; GHQ-12 = General Health Questionnaire-12; SWEMWBS = Short Warwick–Edinburgh Mental Wellbeing Scale. All CACE estimates are based on a pupil compliance rate of 84% (2,100 pupils in 27 compliant schools).

For the primary outcome (externalising behaviour), the CACE estimate (g = -0.02, 95% CI: -0.12 to 0.07) shows no meaningful difference from the ITT estimate (g = -0.02), indicating that accounting for compliance does not change the interpretation. Under the YEF's classification, both estimates fall at the boundary between no effect and low effect.

Internalising behaviour shows a low impact in the beneficial direction under both ITT (g = -0.08) and CACE (g = -0.10) analyses. This is the only outcome where the effect size increases when adjusting for compliance, suggesting a potentially stronger benefit amongst pupils in schools that engaged fully with the intervention.

The remaining secondary outcomes show no meaningful differences between ITT and CACE estimates, with all effect sizes falling within or close to the range indicating no meaningful effect.

Table 24 demonstrates how varying the compliance threshold affects the CACE estimate for the primary outcome (externalising behaviour). The ITT effect remains constant at -0.074 across all thresholds, as this reflects the overall trial result. Higher thresholds reduce the proportion of compliant pupils, which mathematically inflates the CACE estimate. At the 76% threshold (84% compliance), the CACE Hedges' g is -0.02; at a 90% threshold (59% compliance), it increases to -0.03. All estimates fall at or near the boundary between no effect and low effect under the YEF's classification. The sensitivity of the CACE estimates to threshold choices, combined with the consistently small effect sizes, suggest that differential compliance does not explain the null findings for the primary outcome.

Table 24: Complier average causal effect analysis for primary outcome on different thresholds

Compliance threshold	ITT effect	CACE effect	CACE Hedges' g	Pupil compliance rate
≥ 76%	-0.074	-0.089	-0.023	83.6%
≥ 80%	-0.074	-0.096	-0.024	77.7%
≥ 90%	-0.074	-0.127	-0.032	58.8%

Notes: CACE = complier average causal effect; ITT = intention-to-treat. CACE estimates the hypothetical effect if all intervention schools had achieved full compliance. Formula: CACE effect = ITT effect ÷ pupil compliance rate. The total number of treatment pupils at baseline was 2,512 across 34 schools.

Missing data analysis

Given the extent and uneven pattern of missingness, we were unable to conduct multiple imputation, as discussed in the Methods section. Instead, we tested different analytical scenarios to examine the sensitivity of our primary findings. All models include the same covariates as the primary analysis (baseline outcome and school-level FSM stratification variable) to ensure comparability, except where noted.

Table 25 shows five approaches to handling missing data. Sample sizes vary across the models due to two factors: 1) the availability of baseline versus follow-up data and 2) the requirement for complete covariate information in all adjusted models.

Model 1 (complete cases) is our primary analysis. It includes only pupils with both baseline and follow-up data and complete covariate information (n = 3,685). This assumes data are MAR conditional on the observed covariates.

Model 2 (follow-up only) includes all pupils who completed follow-up surveys, regardless of baseline completion (n = 5,616). This model omits baseline externalising behaviour as a covariate, since many pupils lacked baseline data. The larger sample size reflects the inclusion of pupils who only completed follow-up assessments.

Model 3 (imputed baseline) replaces missing baseline externalising scores with school-level averages for pupils who have follow-up data (n = 3,707). This model retains baseline as a covariate. The sample is smaller than Model 2 because it requires complete data on all other covariates, which were only available for pupils in the original baseline sample.

Models 4 and 5 (extreme bounds) test worst-case and best-case scenarios using pupils who had baseline data (n = 3,738). The worst-case scenario assigns maximum externalising scores (20) to all intervention pupils lost to follow-up and minimum scores (0) to control pupils lost to follow-up. The best-case scenario reverses this assignment. These scenarios create upper and lower bounds for possible treatment effects under the assumption that data are missing not at random. The sample size exceeds Model 1 because imputed follow-up values allow the inclusion of pupils who completed baseline but not follow-up.

Table 25: Sensitivity analysis of the primary outcome under different missing data assumptions

Model	N	Coefficient	Standard error	95% CI lower	95% CI upper	Hedges' g	p-value	Missing data assumption
1. Complete cases	3,685	−0.074	0.159	−0.385	0.237	−0.019	0.639	MAR with baseline (primary analysis)
2. Follow-up only	5,616	−0.074	0.191	−0.447	0.3	−0.019	0.7	MAR without baseline
3. Imputed	3,707	−0.073	0.16	−0.387	0.24	−0.018	0.646	Imputed baseline with school means
4. Worst case	3,738	−0.169	0.158	−0.479	0.141	−0.043	0.286	MNAR: missing outcomes = 20
5. Best case	3,738	−0.043	0.171	−0.378	0.292	−0.011	0.802	MNAR: missing outcomes = 0

Notes: CI = confidence interval; MAR = missing at random; MNAR = missing not at random.

The results show consistency across all approaches. Effect sizes range from -0.01 to -0.04 (Hedges' g), indicating low or no meaningful impact under the YEF's classification. The primary analysis estimate (g = -0.02) sits within this range. All CIs include zero and span effects in both directions.

Even under extreme assumptions about missing pupils (Models 4 and 5), the effect sizes remain small, and the direction consistently favours the intervention. This stability suggests that differential attrition has not substantially biased our findings. Whether we apply stricter data requirements or make extreme assumptions, the intervention shows small but statistically non-significant effects on externalising behaviours.

Predictors of attritionTable 27 Table 26 and Table 27 examine whether demographic characteristics differed between the pupils retained in the trial and those lost to follow-up. This analysis tests whether certain pupil groups were systematically more likely to drop out.

The ethnic composition shows minimal differences between the retained and lost pupils. White pupils comprise 74% of those retained compared to 70% of those lost to follow-up. Asian pupils represent 11% of the retained pupils versus 13% of those lost. Other ethnic groups show similarly small variations.

The sex distribution remains nearly identical between the groups. Males represent 54% of the retained pupils compared to 51% of those lost to follow-up. Female representation is 45% and 47%, respectively.

Table 26: Distribution of ethnicity by retained/lost to follow-up pupils

Ethnicity	Retained (%)	Lost to follow-up (%)	Total (%)
Asian or Asian British	10.9	13.0	12.3
Black, Black British, Caribbean or African	5.3	6.5	6.1
Mixed or multiple ethnic groups	6.8	7.5	7.3
White	73.9	69.6	71.1
Other ethnic group	3	3.4	3.3
Sample sizes (n)	3,881	7,700	11,581

Table 27: Distribution of sex by retained/lost to follow-up pupils

Sex	Retained (%)	Lost to follow-up (%)	Total (%)
Male	53.7	50.6	51.6
Female	44.8	47.3	46.5
Prefer not to say	1.6	2.2	2
Sample sizes (n)	3,955	8,348	12,303

Table 28 presents a logistic regression that examines whether baseline characteristics predicted individual pupil dropout. Odds ratios (ORs) greater than 1.0 indicate an increased likelihood of attrition, while values below 1.0 indicate reduced likelihood.

Treatment assignment shows a marginally lower likelihood of attrition for intervention pupils (OR = 0.61, p = 0.077), though this does not reach conventional significance levels. This may appear counterintuitive, given

the higher school-level dropout in the intervention arm. However, within schools that remained in the trial, intervention pupils were somewhat more likely to complete follow-up assessments.

None of the baseline variables significantly predicted attrition. Treatment assignment shows no significant association with dropout (OR = 0.61, p = 0.250) after accounting for clustering at the school level. Sex (OR = 1.00, p = 0.998), school FSM above the median (OR = 1.06, p = 0.893), IMD score (OR = 0.98, p = 0.165) and age at baseline (OR = 0.69, p = 0.227) also show no significant associations. Ethnicity was entered as a categorical variable, with Asian or Asian British as the reference group; no individual ethnic group shows significantly different attrition rates, and the joint test confirmed ethnicity did not predict attrition overall ($\chi^2 = 3.32$, p = 0.506). These results suggest that missingness was not systematically related to observed pupil characteristics.

Table 28: Baseline predictors of attrition in the analytical sample

Predictor variable	Odds ratio	Standard error	p-value	CI lower	CI upper
Treatment (ref: control)	0.61	0.26	0.25	0.26	1.41
Externalising behaviour baseline	1.01	0.05	0.87	0.92	1.11
Sex (ref: male)					
— Female	1	0.37	1.00	0.49	2.06
Ethnicity (ref: Asian or Asian British)					
— Black, Black British, Caribbean or African	2.1	1.43	0.27	0.56	7.97
— Mixed or Multiple ethnic groups	0.47	0.55	0.52	0.05	4.54
— White	1.75	0.91	0.28	0.63	4.84
— Other ethnic group	2.36	1.75	0.25	0.55	10.13
School FSM above the median	1.06	0.43	0.89	0.47	2.36
IMD score	0.98	0.01	0.17	0.96	1.01
Age at baseline	0.69	0.21	0.23	0.39	1.25

Notes: CI = confidence interval; IMD = Index of Multiple Deprivation; FSM = free school meals. Standard errors were adjusted for clustering at the school level (55 clusters). Reference categories: control (treatment), male (sex), Asian or Asian British (ethnicity). "Prefer not to say" for sex was omitted due to the perfect prediction of non-attrition (n = 48). Joint test for ethnicity: $\chi^2 = 3.32$, p = 0.506.

Subgroup analyses

The SAP specified testing treatment × subgroup interactions within multilevel models. However, for categorical variables with multiple categories (ethnicity with five groups, IMD with five quintiles), this approach would require estimating numerous interaction terms, substantially reducing statistical power and complicating interpretation. We therefore adopted a stratified approach, estimating treatment effects separately within each subgroup. This approach provides descriptive evidence of effect consistency across groups, though it does not formally test whether effects differ significantly between groups. These analyses are exploratory.

Table 29 examines whether intervention effects on externalising behaviours varied across demographic subgroups. We compared intervention and control pupils separately within the categories of sex, ethnicity and socioeconomic status (IMD quintiles).

Effect sizes ranged from no effect to moderate effect across subgroups, with all CIs, including zero. For sex, Hedges' g ranged from −0.07 (prefer not to say; low effect) to −0.00 (female; no effect). For ethnicity, effects ranged from −0.13 (Asian or Asian British; moderate effect) to −0.01 (Black, Black British, Caribbean or African; no effect). For IMD quintiles, effects ranged from −0.07 (least deprived) to 0.04 (most deprived), both representing low effects.

The consistency across subgroups suggests that the intervention did not differentially benefit particular demographic groups. However, wide CIs limit precision. The "prefer not to say" sex category (n = 62) and some minority ethnic groups have particularly wide intervals due to small sample sizes.

Table 29: Subgroup analysis – primary outcome (externalising behaviour)

	Unadjusted means				Effect size		
	Intervention group		Control group				
Subgroup	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (intervention; control)	Hedges' g (95% CI)	p-value
Male	1,326 (0)	6.67 (6.47; 6.88)	796 (0)	6.55 (6.28; 6.83)	2,122 (1,326; 796)	−0.03 (−0.12; 0.06)	0.52
Female	1,137 (0)	7.34 (7.10; 7.57)	634 (0)	7.33 (7.02; 7.63)	1,771 (1,137; 634)	−0.00 (−0.10; 0.09)	0.94
Prefer not to say	40 (0)	8.13 (6.67; 9.58)	22 (0)	8.91 (7.45; 10.37)	62 (40; 22)	−0.07 (−0.52; 0.37)	0.74
Asian or Asian British	301 (0)	5.59 (5.19; 6.00)	124 (0)	5.61 (4.95; 6.28)	425 (301; 124)	−0.13 (−0.31; 0.05)	0.16
Black Black British Caribbean or African	134 (0)	6.16 (5.54; 6.79)	72 (0)	5.83 (4.99; 6.68)	206 (134; 72)	−0.01 (−0.25; 0.23)	0.92
Mixed or multiple ethnic groups	175 (0)	7.38 (6.75; 8.02)	89 (0)	6.67 (5.85; 7.50)	264 (175; 89)	−0.06 (−0.27; 0.15)	0.58
White	1,765 (0)	7.23 (7.05; 7.41)	1,105 (0)	7.17 (6.93; 7.40)	2,870 (1,765; 1,105)	−0.00 (−0.09; 0.08)	0.96
Other ethnic group	73 (0)	7.22 (6.17; 8.27)	43 (0)	6.79 (5.63; 7.95)	116 (73; 43)	−0.03 (−0.34; 0.28)	0.85
IMD Quintile 1 (most deprived)	758 (0)	7.45 (7.16; 7.73)	274 (0)	7.31 (6.87; 7.75)	1,032 (758; 274)	0.04 (−0.09; 0.17)	0.56
IMD Quintile 2	458 (0)	6.98 (6.62; 7.35)	266 (0)	7.09 (6.57; 7.60)	724 (458; 266)	−0.05 (−0.18; 0.08)	0.48
IMD Quintile 3	359 (0)	6.94 (6.53; 7.34)	218 (0)	6.93 (6.42; 7.43)	577 (359; 218)	−0.01 (−0.16; 0.13)	0.87
IMD Quintile 4	329 (0)	6.68 (6.27; 7.10)	390 (0)	6.91 (6.51; 7.30)	719 (329; 390)	0.00 (−0.13; 0.13)	0.97
IMD Quintile 5 (least deprived)	561 (0)	6.57 (6.25; 6.88)	270 (0)	6.44 (5.99; 6.89)	831 (561; 270)	−0.07 (−0.21; 0.07)	0.33

Notes: IMD = index of multiple deprivation; CI = confidence interval.

We could not complete the planned LCA because the model failed to converge. Attempts to fit two- or three-class models using demographic variables (ethnicity, sex, age) failed to converge. This suggests either insufficient variation to identify distinct subgroups or excessive noise preventing stable classification. Only single-class models ran successfully, indicating no identifiable heterogeneity in response patterns.

Convergence problems prevent the ability to draw conclusions about whether distinct subpopulations responded differently to the intervention. This represents a missed opportunity to explore heterogeneous treatment effects beyond pre-specified demographic subgroups.

Estimation of effect sizes

Estimation of intraclass correlation coefficients

Table 30 reports ICCs, which quantify the proportions of total variance attributable to differences between schools. The ICCs range from 0.015 for wellbeing to 0.054 for school connectedness, indicating relatively low clustering effects.

The primary outcome of externalising behaviour shows an ICC of 0.021, meaning that 2.1% of the total variation in these behaviours occurs between schools, with the remaining 97.9% reflecting within-school variation. This within-school variance captures differences between individual pupils, though it may also include unobserved classroom-level effects that we could not model. School connectedness displays the highest ICC (0.054), which is expected given that pupils' sense of belonging is likely shaped by school-level factors, such as culture and policies.

The generally low ICCs indicate that most of the variation in outcomes occurs within the schools rather than between them. These values are lower than the 0.03 assumed in the original sample size calculations. However, any implications for statistical power should be interpreted cautiously. The post-hoc power estimates assume that attrition was unrelated to the outcomes. Although our analysis found no evidence that attrition was systematically related to the observed school or pupil characteristics, the substantial level of attrition means we cannot rule out bias from unobserved factors.

Table 30: Intracluster correlation coefficients

Outcome variable	Intracluster correlation coefficient (school level)	Standard error	95% CI lower	95% CI upper
Strengths and Difficulties Questionnaire externalising behaviour (primary)	0.021	0.006	0.009	0.036
Internalising behaviour	0.039	0.009	0.018	0.057
Prosocial behaviour	0.027	0.009	0.009	0.042
Total difficulties	0.036	0.009	0.018	0.054
School connectedness	0.054	0.012	0.027	0.078
Wellbeing (Short Warwick–Edinburgh Mental Wellbeing Scale)	0.015	0.006	0.003	0.027
Psychological distress (General Health Questionnaire-12)	0.035	0.009	0.015	0.054

Notes: CI = confidence interval.

Quasi-experimental design

This section presents the quasi-experimental analysis that examined the targeted intervention component delivered to vulnerable pupils within the intervention schools. These findings should be interpreted with considerable caution due to the methodological limitations described below.

As described in the protocol, Diploma-trained practitioners provided individualised support to the pupils identified as having trauma histories. The original design intended to identify comparable vulnerable pupils in control schools prior to randomisation, enabling a cleaner comparison. However, this was not feasible because control schools had no equivalent identification process, and we could not impose an additional burden on schools to prospectively identify vulnerable pupils outside the intervention framework.

We therefore adopted PSM to construct a comparison group from control school pupils with similar baseline characteristics. This approach has limitations: Matching occurs post-hoc on observed characteristics only and cannot account for unobserved factors that may have influenced selection for targeted support.

Identifying pupils for targeted support analysis

Pupils receiving targeted support were identified through **self-reported survey responses** at follow-up. This approach was specified in the original protocol and SAP, which acknowledged the inherent challenge: We cannot directly observe which pupils received support from TISUK-trained Diploma practitioners.

The survey asked pupils: “Have you received any targeted support? (Yes/No)”. The question did not reference TISUK specifically because the intervention trained existing school staff to deliver support within their normal roles. The pupils would not distinguish support from a TISUK-trained teacher from other school-based assistance.

The survey included a follow-up question that listed specific types of targeted support, using an inventory provided by TISUK. However, many support types – such as one-to-one conversations, small-group work or emotional regulation support – could be delivered by any school staff member, not exclusively Diploma practitioners. Pupils also had the option to name the person providing the support, but response rates were low and could not be reliably matched against Diploma practitioner records. Consequently, we could not confirm whether the reported support was delivered by TISUK-trained Diploma staff or through existing school provisions.

Important limitations apply to this analysis. Self-reported data may include recall errors, misattribution (pupils reporting non-TISUK support) or under-reporting (pupils unaware they had received support from a trained practitioner). We were unable to validate responses against school records due to incomplete data returns from intervention schools. These limitations mean the analysis estimates the effect of self-reported targeted support, which may not precisely capture TISUK’s Diploma practitioner intervention.

The survey responses revealed an important pattern. In the treatment group, 613 pupils (18.53% of the matched sample) reported receiving targeted support, aligning with intervention design expectations. However, 434 control group pupils also reported receiving targeted support – likely other forms of intensive support, such as pastoral care, counselling or learning support programmes.

This finding led us to adopt a two-pronged analytical strategy. Our main analysis treats control group pupils who reported receiving support as non-recipients (coded as zero), creating a clean comparison between TISUK intervention recipients and control pupils. This conservative approach ensures we isolate the specific

effect of TISUK's targeted support component. A sensitivity analysis below examines whether the results change when the control pupils who reported receiving intensive support are coded as recipients. This approach acknowledges that schools provide various forms of pupil support and tests whether our findings remain robust in this alternative classification.

Balance assessment for propensity score matching

Figure 2 shows the distribution of propensity scores before and after matching for the primary outcome analysis. The left panel displays the raw (unmatched) sample. The treatment and control groups exhibit different distributions, with the treated pupils (dashed line) having higher propensity scores on average. This indicates systematic differences in the observed characteristics between groups before matching.

The right panel shows the matched sample. The distributions now overlap substantially, with both groups centred around similar propensity score ranges (0.2 to 0.4). This improved overlap demonstrates that matching successfully created comparable treatment and control groups. The remaining slight differences reflect the fact that matching balances the groups on average rather than achieving perfect overlap. Good balance in the matched sample strengthens confidence that differences in outcomes reflect treatment effects rather than pre-existing differences between the groups.

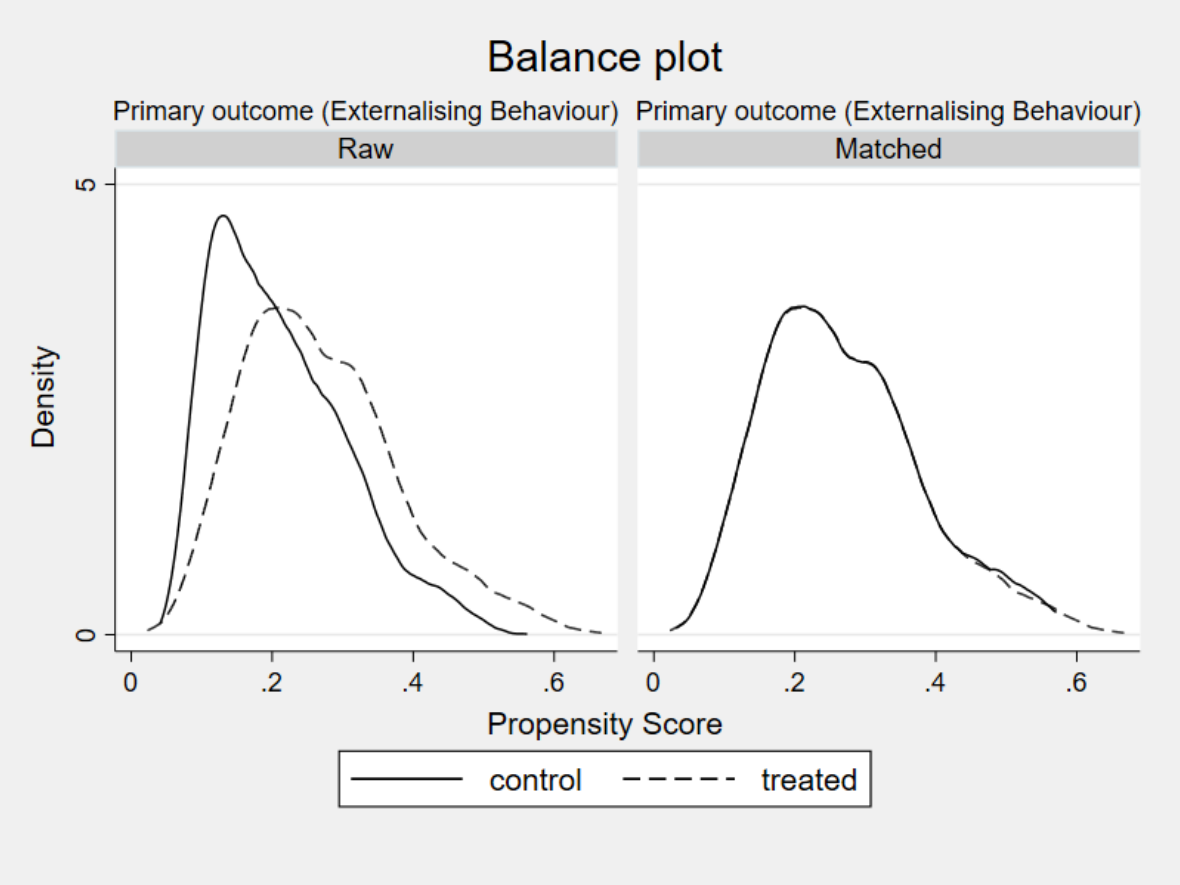


Figure 2: Balance plot for the primary outcome (externalising behaviour)

Table 31 presents balance statistics that compare the treatment and control groups before and after matching. The standardised difference measures how far apart the groups are on each covariate. Values

between –0.10 and 0.10 indicate good balance (Nguyen, 2017). The variance ratio shows whether the groups have similar spreads in their characteristics. Values near 1.00 indicate comparable variation.

The protocol specified the balance would be assessed using Rubin's B (standardised difference < 0.25) and Rubin's R (variance ratio between 0.5 and 2.0). The balance diagnostics from Stata's "tebalance summarize" command are equivalent measures. All matched covariates meet these thresholds, with standardised differences below 0.10 and variance ratios between 0.89 and 1.65.

Before matching, the groups differed substantially on several characteristics. Baseline externalising behaviour showed a standardised difference of 0.32. Sex showed considerable imbalance (0.37), indicating more females in the treatment group. IMD scores also differed (0.32).

After matching, balance improved across most covariates. All standardised differences fell below 0.10, and variance ratios moved closer to 1.00. One exception was White ethnicity, where the standardised difference increased slightly from 0.02 to 0.09 after matching. Although this remains within acceptable thresholds, it indicates marginally less balance on this covariate post-matching. We did not include interaction terms or polynomial transformations of baseline variables, as the primary specification achieved acceptable balance across all covariates.

The analytical sample comprises 403 treated pupils matched to 403 control pupils. The treated sample is smaller than the 613 pupils who reported receiving targeted support because matching required complete data on all baseline covariates. Pupils with missing baseline information were excluded from the matching procedure.

Table 31: Covariance balance before and after matching

Covariate	Standardised difference		Variance ratio	
	Raw	Matched	Raw	Matched
Sample composition				
Total observations	1,797	806	---	---
Treated pupils	403	403	---	---
Control pupils	1,394	406	---	---
Externalising behaviour at baseline	0.315	–0.043	1.202	1.041
Ethnicity groups				
Black, Black British, Caribbean or African	–0.047	–0.025	0.812	0.894
Mixed or multiple ethnic groups	0.081	0.000	1.308	1.000
White	0.024	0.088	0.970	0.895
Other ethnic groups	–0.053	0.000	0.717	1.000
Sex groups				
Female	0.365	0.010	0.968	0.995
Prefer not to say	0.076	0.071	1.714	1.650
Index of multiple deprivation score	0.315	–0.009	1.420	1.056

Notes: Standardised differences below 0.10 indicate good balance. Variance ratios near 1.00 indicate a similar spread between groups. Balance statistics calculated using the "tebalance summarize" command in Stata.

We explored alternative specifications, including interaction terms and polynomial transformations of the baseline variables. These did not yield consistent improvements in balance – gains on some covariates were offset by losses on others. We therefore retained the primary specification, which achieved an acceptable balance across all covariates.

Primary analysis

Table 32 presents results from the PSM analysis that examined targeted individual support effects on externalising behaviour, the primary outcome. Of the 613 treatment pupils who reported receiving targeted support, 406 had complete data for all matching covariates and were included in the propensity score analysis.

The SAP specified a broader set of matching variables, including sex, age, ethnicity, FSM status, attendance, exclusions, suspensions and pre-test outcomes. However, the administrative data collection yielded fewer observations than the survey data, which would have substantially reduced sample sizes for the QED analysis. We therefore focused on the most theoretically important covariates: baseline outcome measures (specific to each analysis), ethnicity, sex, age and IMD score.

These variables capture the key sources of selection bias whilst maximising sample sizes and therefore analytical power. Socio-economic status, originally planned to be measured through FSM eligibility, is captured through the pupil-level IMD. This focused matching strategy balances identification strength with statistical power, retaining sufficient observations to detect meaningful treatment effects whilst controlling for the primary sources of confounding.

The results show that the pupils receiving targeted support had higher externalising behaviour scores than the matched controls (coefficient = 1.032, $p < 0.001$, Hedges' g = 0.395). Under the YEF's classification, this represents a large unfavourable effect, suggesting that pupils receiving targeted support showed more behavioural difficulties than comparable pupils in control schools.

Table 32: Quasi-experimental design main analysis on primary outcome – average treatment effects on the treated (targeted support)

Outcome	Coefficient	Standard error	Hedges' g	Hedges' g (95% confidence interval)		p-value	N	Pooled standard deviation	Expected sign
Externalising behaviour (primary)	1.03	0.40	0.26	0.06	0.45	0.01	1,759	4.05	(–)

Notes: Coefficients represent average treatment effects on the treated in original outcome units. Effect sizes were calculated as Hedges' g using the pooled standard deviations from the matched samples. Sample sizes vary due to missing data patterns. Positive coefficients for SDQ outcomes indicate higher problem scores amongst treated pupils.

Secondary analysis

Table 33 presents the results for six secondary outcomes using the same matching methods. Pupils receiving targeted support show higher internalising behaviour scores (Hedges' g = 0.24, 95% CI: 0.11 to 0.36), representing a moderate unfavourable effect. Total difficulties (Hedges' g = 0.26, 95% CI: 0.11 to 0.40) and psychological distress (Hedges' g = 0.27, 95% CI: 0.13 to 0.41) show large unfavourable effects. These patterns suggest targeted support reached pupils with greater pre-existing needs.

Pupils receiving support also show lower school connectedness (Hedges' g = −0.18, 95% CI: -0.34 to -0.03), a moderate unfavourable effect, and lower wellbeing (Hedges' g = -0.29, 95% CI: -0.48 to -0.11), a large unfavourable effect. Prosocial behaviours show a moderate unfavourable difference (Hedges' g = -0.12, 95% CI: -0.26 to 0.03).

This pattern indicates that pupils who reported receiving targeted support experienced more severe difficulties across multiple domains. However, matching on a single baseline time point cannot fully account for differing trajectories. Pupils receiving support may have been experiencing deteriorating difficulties at the point of identification, meaning follow-up scores could partly reflect ongoing decline rather than intervention effects.

The lower wellbeing and connectedness scores also require careful interpretation. These differences likely reflect the challenging characteristics of this vulnerable group. Despite matching on observed characteristics, unobserved factors – such as the severity of trauma history or family circumstances – may differ systematically between groups. As with all quasi-experimental analyses, these findings should be interpreted alongside the methodological constraints outlined above. Emerging evidence suggests that targeted support by school staff can sometimes produce iatrogenic harm (unintended negative effects from well-intentioned interventions) (Foulkes & Stringaris, 2023).

Table 33: Quasi-experimental design main analysis on secondary outcomes – average treatment effects on the treated (targeted support)

Outcomes	Coefficient	Standard error	Hedges' g	Hedges' g (95% confidence interval)		p-value	N	Pooled standard deviation	Expected sign
Internalising behaviour	0.91	0.25	0.24	0.11	0.36	0.00	1,758	3.84	(−)
Prosocial behaviour	−0.23	0.15	−0.12	−0.26	0.03	0.11	1,764	1.99	(−)
Total difficulties	1.73	0.49	0.26	0.11	0.40	0.00	1,756	6.72	(−)
School connectedness	−0.06	0.03	−0.18	−0.34	−0.03	0.02	1,721	0.31	(−)
Wellbeing (Short Warwick–Edinburgh Mental Wellbeing Scale)	−1.60	0.52	−0.29	−0.48	−0.11	0.00	1,701	5.48	(−)
Psychological distress (General Health Questionnaire-12)	1.97	0.53	0.27	0.13	0.41	0.00	1,793	7.29	(−)

Notes: Coefficients represent average treatment effects on the treated in original outcome units. Effect sizes were calculated as Hedges' g using pooled standard deviations from the matched samples. 95% CIs are based on robust standard errors. Sample sizes vary due to missing data patterns. Positive coefficients for Strengths and Difficulties Questionnaire outcomes indicate higher problem scores amongst treated pupils; negative coefficients for wellbeing measures indicate lower scores.

Robustness checks

We tested whether our findings depend on how we define the comparison group. In the follow-up survey, pupils reported whether they received targeted individual support. Our main analysis used strict assignment rules: Pupils in the intervention schools who reported receiving support were classified as treated (n = 613), while all control pupils were classified as untreated (n = 2,696). However, 434 control pupils (16% of controls) also reported receiving targeted support, likely reflecting BAU provisions in their schools.

The nature of this support is unknown. In the intervention group, support may have been delivered by Diploma-trained practitioners using TISUK approaches, though we cannot verify this from self-reported data. Control pupils may have received a range of provisions unrelated to TIP.

One approach would be to exclude these 434 control pupils entirely, comparing TISUK-supported pupils only against controls known not to have received any targeted support. However, this would reduce the sample size and potentially introduce selection bias if the pupils who received BAU support differ systematically from those who did not.

We therefore tested an alternative specification that reclassifies these 434 control pupils as treated. This approach asks a different question: whether the results hold when comparing all pupils receiving any form of targeted support against those receiving none, regardless of trial arm. If the findings remain consistent, this strengthens confidence that effects are not driven by our classification rules.

The alternative specification yields a smaller but still statistically significant treatment effect, as shown in Table 34. The coefficient decreased from 1.03 (SE = 0.40, p = 0.01) in the main analysis to 0.90 (SE = 0.26, p = 0.00). The effect size fell from Hedges' g = 0.26 to g = 0.22. This 15% reduction suggests some attenuation when including control pupils receiving BAU support. However, both specifications indicate that pupils receiving targeted support showed more externalising behaviours than matched peers receiving no support.

Table 34: Sensitivity to treatment definition – primary outcome results using strict versus inclusive classification

	Coefficient	Standard error	Hedges' g	Hedges' g (95% confidence interval)		p-value	N	Pooled standard deviation
Externalising behaviour	0.90	0.26	0.22	0.10	0.35	0.00	1,759	4.05

Alternative matching specifications

We tested whether the findings depend on the matching method used. Table 35 presents results from four alternative specifications. All use the same covariates (baseline outcome, ethnicity, sex, deprivation and age) but different matching algorithms.

Two propensity score specifications used calliper widths of 0.15 and 0.25 SDs. Both produced identical results: treatment effects of 1.03 points (95% CI: 0.26 to 1.81). The callipers restricted the matches to pupils with similar propensity scores, reducing bias. Both widths achieved sufficient overlap to match all treated pupils.

Nearest neighbour matching paired each treated pupil with their five closest controls. This produced slightly larger effects, with a 1.09-point increase (95% CI: 0.67 to 1.50) and narrower intervals. Inverse probability weighting reweighted the entire sample rather than the selected pairs. This yielded an effect size of 1.02 points (95% CI: 0.64 to 1.40).

Treatment effects remained consistently positive across all specifications, ranging from 1.02 to 1.09 points. All CIs excluded zero. This consistency suggests that the findings are robust in the chosen matching algorithm.

Table 35: Different matching specifications

Specification	ATT[17] coefficient	Abadie–Imbens robust standard error	z-statistic	p-value	95% CI lower	95% CI upper
Calliper matching (0.15)	1.03	0.39	2.61	0.009	0.26	1.81
Calliper matching (0.25)	1.03	0.39	2.61	0.009	0.26	1.81
Nearest neighbour matching (5)	1.09	0.21	5.14	0.000	0.67	1.50
Inverse probability weighting	1.02	0.19	5.27	0.000	0.64	1.40

Notes: CI = confidence interval. All specifications control for baseline externalising behaviour, ethnicity, sex, deprivation score and age. Propensity score matching uses calliper widths in standard deviations and Abadie-Imbens robust standard errors. Nearest neighbour matching uses five matches per treated pupil, with Abadie-Imbens robust standard errors. Inverse probability weighting uses heteroscedasticity robust standard errors.

We assessed the covariate balance for each alternative specification using standardised differences and variance ratios. All methods achieved an acceptable balance, with standardised differences generally below 0.10 and variance ratios within the 0.5 to 2.0 range specified in the protocol. Inverse probability weighting achieved the tightest balance, with all standardised differences below 0.03. Nearest neighbour matching showed marginally less precise balance on baseline externalising behaviours (standardised difference = 0.09) and IMD score (0.07), though both remained within acceptable thresholds. The consistency of balance across methods supports the robustness of the primary findings. Full balance diagnostics for each specification are available from the authors upon request.

[17] Average Treatment Effect on the Treated

Sensitivity to unobserved confounding

PSM assumes we have controlled for all factors that influence both treatment assignment and outcomes. However, some important factors may be unobserved. Rosenbaum's bounds test how sensitive our findings are to this "hidden bias".

Table 36 shows results for 388 matched pairs. Gamma (Γ) represents the strength of hidden bias. At Γ = 1, there is no hidden bias – our assumption holds perfectly. Higher values indicate stronger potential confounding from unobserved factors. For example, Γ = 1.5 means that two pupils with identical observed characteristics could differ by 50% in their odds of receiving treatment due to unobserved factors.

The sig+ column shows the upper-bound p-value at each Γ level. This indicates whether our findings remain statistically significant when we account for the possibility of hidden bias. At Γ = 1 (no bias), p = 0.00, confirming our main result. At Γ = 1.2, p = 0.02, still significant at conventional levels. However, at Γ = 1.3, p = 0.07, the result loses significance at the 5% level. At Γ = 1.4 and above, p > 0.10.

This indicates that our findings are sensitive to unobserved confounding. Even modest differences in unmeasured variables – where pupils with identical observed characteristics differ by just 30% in their odds of receiving treatment – would render our results non-significant. Combined with other limitations of the QED component, including post-hoc identification of the treated pupils and reliance on self-reported receipt of support, this sensitivity analysis reinforces the need for caution when interpreting these findings.

Table 36: Rosenbaum bounds for delta (N = 388 match pairs)

Gamma	sig+	sig−	t-hat+	t-hat−	CI+	CI−
1	0.00	0.00	1.00	1.00	0.50	1.50
1.1	0.00	0.00	1.00	1.50	0.50	2.00
1.2	0.02	0.00	0.50	1.50	0.00	2.00
1.3	0.07	0.00	0.50	1.50	0.00	2.00
1.4	0.19	0.00	0.00	2.00	-0.50	2.50
1.5	0.38	0.00	0.00	2.00	-0.50	2.50
1.6	0.60	0.00	0.00	2.00	-0.50	3.00
1.7	0.77	0.00	0.00	2.50	-1.00	3.00
1.8	0.89	0.00	-0.50	2.50	-1.00	3.00
1.9	0.95	0.00	-0.50	2.50	-1.00	3.00
2	0.98	0.00	-0.50	2.50	-1.00	3.50

Notes: CI = confidence interval. Rosenbaum bounds test sensitivity to unobserved confounding. Γ = OR of differential treatment assignment due to hidden bias (Γ = 1 = no bias). sig+ and sig− = upper/lower p-value bounds; t-hat+/t-hat− = upper/lower Hodges-Lehmann point estimates; CI+/CI− = 95% CI bounds. Results are considered robust if sig+ < 0.05 at higher Γ values.

Table 37 presents the falsification test, applying the primary analysis to the outcome measured at baseline. The coefficient is near zero and not statistically significant, suggesting no pre-existing differences and lending credibility to the identification strategy.

Table 37: Quasi-experimental design falsification test – main analysis on primary outcome at baseline

	Coefficient	Standard error	Hedges' g	Hedges' g (95% confidence interval)		p-value	N	Pooled standard deviation
Externalising behaviour	−0.01	0.26	0.00	−0.12	0.12	0.960	1,786	4.28

Subgroup analysis

This analysis examines whether the targeted support effects varied across demographic subgroups, analysing sex, ethnicity and socioeconomic status (IMD quintiles) separately.

Several subgroups could not be analysed. Models failed to converge for Black pupils and IMD quintiles 2, 3 and 4 due to insufficient observations. PSM requires adequate overlap in characteristics between the treatment and control groups.

Amongst the analysable subgroups, White pupils show the largest effect (coefficient = 1.22, Hedges' g = 0.30, 95% CI: 0.16 to 0.45, n = 1,368). Males show effects of 0.81 points (Hedges' g = 0.20, 95% CI: -0.02 to 0.42, n = 890). Females showed smaller effects of 0.49 points (Hedges' g = 0.12, 95% CI: -0.07 to 0.31, n = 840). Asian pupils showed effects of 0.43 points (Hedges' g = 0.11, 95% CI: -0.41 to 0.64, n = 144), with wide intervals reflecting a small sample size.

For socioeconomic status, only extreme quintiles could be analysed. The most deprived group (IMD Q5) showed effects of 1.50 points (Hedges' g = 0.38, 95% CI: 0.13 to 0.63, n = 345). The least deprived (IMD Q1) showed 0.85 points (Hedges' g = 0.21, 95% CI: −0.10 to 0.52, n = 398).

These patterns suggest targeted support effects may vary across groups. However, missing estimates for multiple subgroups prevent a comprehensive comparison. Small samples produce wide CIs. These analyses remain exploratory and require cautious interpretation.

Table 38: Demographic subgroup analysis on primary outcome

Subgroup	Coefficient	Standard error	Hedges' g	Hedges' g (95% confidence interval)		p-value	N	Pooled standard deviation
Males	0.81	0.44	0.20	−0.02	0.42	0.068	890	4.03
Females	0.49	0.39	0.12	−0.07	0.31	0.206	840	3.99
White	1.22	0.30	0.30	0.16	0.45	0	1,368	4.03
Asian	0.43	1.02	0.11	−0.41	0.64	0.673	144	3.78
Black	--	--	--	--	--	--	--	--
IMD Q1	0.85	0.63	0.21	−0.10	0.52	0.176	398	4.00
IMD Q2	--	--	--	--	--	--	--	--

IMD Q3	--	--	--	--	--	--	--	--
IMD Q4	--	--	--	--	--	--	--	--
IMD Q5	1.50	0.50	0.38	0.13	0.63	0.003	345	3.97

Notes: IMD = index of multiple deprivation.

Implementation and process evaluation results

This section presents the findings across five key areas: intervention dosage and reach, delivery quality, staff engagement, school-level implementation and perceived impacts. We have structured the findings around the IPE's research questions:

- IPE1: To what extent do TISUK staff adhere to the intended delivery model?
- IPE2: To what extent has the intervention been delivered in the intended dosage?
- IPE3: To what extent do school staff and leadership engage with the intervention?
- IPE4: To what extent are the different components of the intervention delivered at a high quality?
- IPE5: What is the participation rate of the intended recipients (school staff and leadership)?
- IPE6: What is the perceived need for and benefit of the intervention amongst school staff and leadership?
- IPE7: What strategies and practices are used to support high-quality implementation?
- IPE8: How do structural factors (e.g. institutional racism, lack of diversity in the workforce) affect CYPs from Black, Asian and minority ethnic backgrounds?
- IPE9: How do CYPs from different sexes and Black, Asian and minority ethnic backgrounds experience the intervention?

For each area, we report what was delivered, how implementation varied across the schools and what factors supported or hindered delivery.

Business as usual

Although BAU was not one of the main IPE questions, it is important to determine the context in which the intervention was run and similar programmes were being implemented in control schools.

Prior to the intervention taking place, treatment schools were characterised from interviews with school staff and TISUK consultants by the following:

Punitive and consequence-driven behavioural management: Prior to the implementation of the programme, behavioural management in schools was predominantly characterised by punitive, consequence-driven approaches. Trainers described the widespread use of centralised and escalating systems, such as detention hierarchies (e.g. C1, C2, C3) and isolation rooms or booths, which prioritised consistency, control and compliance. These systems focused on correcting behaviours through sanctions rather than exploring underlying causes, reflecting a culture in which behaviours were viewed as conscious choices rather than responses to unmet needs or distress.

Staff attitudes often emphasised strictness and authority, with shouting and judgemental language (e.g. "repeat offenders") perceived as legitimate behavioural management strategies. Relational practices or TIPs were frequently dismissed as impractical or unnecessary. Overall, the prevailing BAU approach was disciplinary rather than relational, with limited emphasis on understanding students' experiences or fostering connection.

Teacher role and disempowerment: Pre-existing behavioural management systems often centralised responsibility for behaviours, which had mixed implications for classroom teachers. While centralised detention and referral systems were intended to reduce teacher workloads by shifting responsibility for consequences to senior leadership, they also limited teachers' autonomy and capacity to respond to

behaviours relationally. Teachers' roles were largely confined to enforcing rules and escalating incidents, rather than de-escalating situations, exercising professional judgement or building restorative relationships. Trainers described this as a form of disempowerment, whereby reliance on senior leadership reduced opportunities for teachers to develop and apply relational and behavioural support skills. Overall, these systems tended to be reactive, addressing behavioural incidents after escalation rather than proactively fostering psychologically safe and supportive classroom environments. The prevailing systems often removed the responsibility for behaviours from individual classroom teachers, which had mixed consequences.

Ineffective or reactive use of school time: Key periods of the school day, such as tutor time and lunch, were often approached as behavioural risks to be controlled rather than as opportunities for relationship-building. In response to behavioural challenges, schools frequently reduced unstructured time, including by shortening tutor periods and lunch breaks, as a reactive strategy to limit incidents. While intended to manage behaviours, these measures reduced opportunities for social interaction, play and connection. Assemblies were also sometimes viewed as routine or performative activities, lacking clear purpose or relational value, rather than being used as meaningful opportunities to foster community and engagement.

Siloed and reactive mental health support: Prior to the adoption of whole-school approaches, mental health support in the schools was typically siloed and reactive. Support followed a tiered model, with schools providing universal provision and referring more complex needs to external specialist services, such as CAMHS. Mental health and wellbeing were often viewed as separate from teaching and learning, with many staff perceiving these areas as outside their professional remit. Although counselling services were present in some schools, they operated as discrete provisions rather than as part of a cohesive whole-school approach. Trainers also identified a widespread lack of trauma-specific knowledge, with limited understanding of how adversity and trauma influence behaviours and learning, indicating that such perspectives were largely absent from BAU practices.

BAU for control schools focused on wellbeing interventions that were similar to the TISUK programme. BAU monitoring surveys, which were administered every six months, revealed that some control schools were actively implementing various wellbeing and trauma-related interventions, though response rates were limited (nine of 38 control schools across various waves of sharing the survey). Responding schools reported diverse mental health provisions, including Designated Senior Mental Health Lead training delivered each half-term, trauma-informed training during in-service training (INSET) days, Thrive Approach implementation, Youth Mental Health First Aiders, whole-staff emotional wellbeing training and dedicated wellbeing practitioners.

Schools specifically mentioned "implementing relational ways of working" and trauma-informed training – approaches conceptually similar to TISUK. The low response rate limits our ability to fully characterise control practices, but the reported activities indicate that BAU in the interested schools already included some trauma-informed elements.

Trauma Informed Schools UK staff adherence to the intended training delivery model and dosage

This section addresses two research questions: To what extent do TISUK staff adhere to the intended delivery model, and to what extent has the intervention been delivered in the intended dosage? (IPE1 and IPE2)

This section focuses on delivery via TISUK staff to school staff. School-level implementation is addressed in the following section.

KEY FINDINGS:

- **In most schools, TISUK training was delivered to school staff as designed.**
- **Some deviations occurred due to limitations, such as teaching staff's capacity to take part in training.**
- **In practice, the dose shifted from the intended fewer, longer blocks to more, shorter interactions in response to staff availability/capacity.**
- **Some schools received booster or coaching touchpoints (for example, brief clinics or check-ins) instead of full-length sessions to maintain momentum.**

Interviews with teachers and TISUK consultants indicated that, in most schools, training delivery followed the intended programme structure; however, TISUK made adaptations to accommodate school capacity and competing demands.

Observations of sessions by evaluators, interviews with teachers and interviews with TISUK consultants suggested that, on the whole, sessions were delivered by TISUK staff to relevant school staff members. Although most TISUK consultants delivered the programme as designed, they occasionally adjusted delivery. The consultants and school staff interviewed outlined that changes were made to accommodate timetable pressures (exams, Ofsted inspections, school trips, staff absence), to manage staff turnover and to support access where pacing was challenging for staff. The adaptations made to delivery are described below, with the intended dosages, timing and delivery models indicated in bold.

- **Whole-staff training (2 × 3 hours, virtual, 2–3 terms apart):** TISUK consultants changed the intended three-hour blocks into twilight sessions or shorter segments (for example, two 90-minute sessions) or moved to blended or in-person sessions; they brought the second session forward or pushed it back to align with Ofsted inspection windows or emerging school needs. They also added optional follow-up clinics or Q&A sessions to consolidate practice while providing recordings for school staff who were unable to attend.
- **Senior leadership training (2 days, cohort model):** Some TISUK consultants broke this into three to four half-days, delivered in a hybrid format with staggered attendance, and trainees were swapped when leaders could not attend both days. TISUK consultants added short post-training action-planning clinics (60–90 minutes) to convert learning into policy and practice.
- **Network consultancy support (3 meetings + 2 networking sessions):** In some instances, TISUK consultants shifted from three longer meetings to more, shorter touchpoints and moved to one-to-one coaching with the SENCO or Deignate Safeguarding Llead (DSL) when a whole team could not attend. They replaced or supplemented networking sessions with recorded resources staff could

watch in their own time or drop-in communities and merged or rescheduled networking sessions when attendance dipped.

- **Diploma practitioner training (11 days over 5–6 months):** TISUK consultants extended timelines, created catch-up days, allowed remote modules during staff sickness and offered extra mentoring beyond the planned dose. They replaced trainees who left or changed roles with other staff from the school, adjusted assessment deadlines and shifted some practicums from group to one-to-one when particularly sensitive topics were being discussed.
- **Reflective supervision workshops (train two Diploma graduates to cascade):** In instances where there were not enough in-school supervisors (Diploma-trained teaching staff) to act as supervisors for one-to-one sessions with other teaching staff, TISUK consultants used group supervision instead. They also made sessions shorter (45–60 minutes) but held them more often at the start. The first few supervision sessions were led jointly by a TISUK consultant and the new in-school supervisor. This was done because there were too few trained supervisors within schools and limited time and to ensure the approach worked reliably before expanding it.
- **Webinars for staff and CYPs:** TISUK consultants broke down the webinars into shorter clips for tutor time and CPD and paired viewing with facilitated debriefs and curated audiences, starting with older year groups. They provided access windows and reminders and used partial viewings with repeat showings for cohorts with low attendance. These changes were made due to attention span and IT access constraints, safeguarding considerations around lived-experience content and school timetable limitations.

TISUK sustained support for schools through curated resources and by sharing new tools as intended: TISUK staff provided ongoing support not only through formal consultancy but also by curating and sharing practical resources. Participants took notes actively using the materials provided by TISUK, including feelings cards, the handbook and tools for specific interventions, such as “big empathy drawing”. During training sessions, it was common for TISUK staff to share links to new tools, such as “relational support plans”, and to highlight work by specific colleagues responsible for developing resources.

TISUK staff actively fostered a supportive community of practice between schools as intended: A key part of the TISUK staff role was connecting schools to build a community of practice and encouraging the sharing of good practice, which was an explicit purpose of one training session. The sense of a shared journey was further supported by the promotion of the formal “awards process for best practice”, which provided a clear and aspirational structure for schools as they embedded the model. To win a TISUK award, a school had to demonstrate that it had done the following: embedded the TISUK principles within its school, completed TISUK staff training and evidenced EAAs supporting children who have experienced trauma within their schools. The schools were supported in building further connections by visiting other TIUSK award-winning schools in their area.

The extent to which school staff and leadership engaged with the Trauma Informed Schools UK training

This section addresses three research questions: To what extent do school staff and leadership engage with the intervention? What is the participation rate of the intended recipients (school staff and leadership)? What is the perceived need for and benefit of the intervention amongst school staff and leadership? (IPE3, IPE5 and IPE6)

KEY FINDINGS:

- **Engagement and staff adherence were generally strong, helped by brief refreshers and peer recognition, with staff often becoming more positive mid-course.**
- **Daily operational pressures, faster-paced online days and fragmented attendance reduced engagement.**
- **Sustaining momentum was further challenged by staff turnover and limited onboarding.**
- **These factors highlighted the need for regular refreshers and a clear CPD plan to maintain coverage.**

Adherence was broadly high across the schools.

Adherence in this context is defined by **school staff attendance at the scheduled sessions** rather than by the delivery of TISUK staff. This means that adherence is measured according to whether the designated staff members were present during the sessions as planned. Focusing on staff attendance ensures that adherence reflects the participation/engagement of the school staff, independent of TISUK's adherence to delivery.

Table 39. Adherence score[18]

Adherence rating	Percentage of schools that achieved the rating
Excellent (between 76% and 100%)	75% (n = 27 schools)
Good (between 51% and 75%)	19% (n = 7 schools)
Moderate (between 26% and 50%)	6% (n = 2 schools)
Poor (25% and below)	(none)

Base = 36 schools

The mean adherence score was 83.4%, the median was 90% and the range spanned 40–100%. Three-quarters of the schools (75%) were at or above 76%. One-third (33%) had nearly perfect adherence, with a score of 95% or higher (see Appendix 14). Three-quarters of the schools achieved Excellent adherence. Shortfalls were concentrated in roughly a quarter of the schools (those rated Moderate or Poor) and were typically linked to missed webinars and reduced reflective supervision.

- **Reflective supervision:** 86% were fully adherent, with an average score of 8.9 out of 10.

[18] Data collected from all intervention schools that remained in the trial (n = 36).

- **Consultancy:** 58% were fully adherent, with an average score of 11.9 out of 15. 25% scored 10 out of 15, and 17% scored five or less.
- **Diploma training:** 56% were fully adherent, with an average score of 39.2 out of 45. 19% achieved excellent adherence (35–44 out of 45), and 18% had good adherence (24–34 out of 45).
- **Webinars:** Webinars were the most commonly missed component and the least weighted; only 8% achieved full marks, 47% recorded partial completion and 44% recorded none, with an average score of 1.8 out of 5. Notably, approximately one-third of the most adherent schools did not attend any webinars.
- **Whole-staff training: 72% of the schools were fully adherent**, with an average score of 23 out of 25.

TIUSK consultants reported that engagement varied.

While overall adherence scores were strong, TISUK consultants perceived engagement as mixed overall, with pastoral, wellbeing and Special Educational Needs (SEN) staff typically highly engaged, while engagement amongst the wider teaching staff was more variable. They observed that engagement tended to be stronger when the senior leadership, ideally the head, were visibly on board. Where heads led participant selection through internal applications rather than nominations, staff arrived more motivated. Some consultants saw a shift from early scepticism amongst school staff, voiced as "This is just another training" or "Try doing that in the classroom", once teachers tried strategies, typically by days three to six. One trainer remarked that "You saw people who were like, 'Oh my God, I really get that'," and another reported that staff had "gone back and started to have courageous conversations… changing their isolation rooms". Engagement also strengthened when teachers saw quick wins; for example, a meet-and-greet at the door was described as low-risk and associated with pupils settling to learn quicker.

Schools reported that considerable thought went into deciding who would undertake the intensive training, revealing tension between training for strategic knowledge and training for practical, on-the-ground delivery.

Schools took a variety of strategic approaches to selecting staff for the intensive Diploma course. At times, this resulted in a mismatch between the training's intention and the school's selection process. One school, for instance, chose to put all of their heads and assistant heads of year in the course. While this successfully built their knowledge base, a leader later reflected that these individuals "don't have [the] capacity to actually support the delivery". They felt that Learning Support Practitioners might have been better choices for hands-on implementation.

A trainer noted it was a "real missed opportunity" when senior leaders attended the practitioner-focused Diploma, as it was very unlikely that the Deputy Head would be doing tasks which would typically fall to a member of the pastoral team. In other cases, delegates themselves have noted the role mismatch.

> "I've been offered the opportunity to do this training, but I'm not necessarily working directly with young people." – School Staff Member

Other schools found selection strategies that suited their needs well. To ensure commitment, some schools adopted a selective process based on expressed interest, with one requiring staff to write a letter explaining their interest to "get people who are really passionate about it".

> "We made our SENCO head it up, and that fits us well. But we also gave staff in the working group space to develop expertise. We meet regularly and revisit our action plan." – School Headteacher

Barriers to engagement included limited time, high workloads and operational pressures.

Maintaining focus amidst daily operational pressures: When staff attended training on their school sites, they were sometimes pulled into on-call and triage requests, urgent pastoral issues and last-minute cover. This shifted attention back into work mode and diluted learning. Online Diploma days were more fast-paced and left participants emotionally drained. Most returned straight to duties with little protected time to reflect or complete practice tasks. Cover pressures meant some staff missed days and rejoined different cohorts. This fragmented the learning sequence and weakened continuity with peers and trainers.

Limited time: Some teachers found it challenging to secure adequate time for the Diploma alongside teaching, pastoral duties and on-call responsibilities. Some described the expectation to miss "two days out of your work week". The need to protect practitioner activity meant other tasks were deprioritised. The final Diploma presentation required additional preparation time. These time demands also entailed direct cover costs, and participants noted that this created tensions when leadership commitment was uncertain.

> "The amount of cover [costs] has been highlighted by our Headteacher many times." – School Counsellor

To mitigate these constraints, schools made use of TISUK's bite-size CPD options and webinars as more feasible alternatives. This enabled staff to sustain progress without extended release.

> "Time. I think time's a massive issue for them… They did the training and then they were back in school and back on the kind of hamster wheel straight away… There was buy-in from some, but others still did not quite know what it meant for them. We kept it alive in briefings, but it was not consistent everywhere." – TISUK Consultant

Keeping training visible: In busy secondary contexts, keeping training visible was a challenge. One school used a drip-feed approach. They built five-minute refreshers into morning briefings to revisit a single element each time – for example, one sentence stem, a regulate-and-return step or a short case note from doorstep resolution.

Leaders reinforced this with quick prompts in staff planners, classroom displays, lanyard cards with agreed-upon language and faculty micro-scripts to ensure phrasing stayed consistent across subjects. Schools also used new TISUK resources and short micro-webinars to raise the profile again. This included induction and catch-up sessions for staff who had missed earlier sessions. In some settings, this was paired with brief peer drop-ins with simple notes recognising effective co-regulation language to keep routines in view.

Staff turnover: Attrition was mainly due to staff turnover rather than dropout. When trained practitioners left, pressure on those who remained increased. Several schools moved quickly to identify and enrol new candidates. Case studies showed that even in settings with high initial coverage, long-term reach declined after staff departures. One school's already low engagement was compounded by high turnover and a lack of onboarding systems for new staff.

Achieving and sustaining whole-school coverage was rare. It depended on robust mechanisms for ongoing training and refreshers. Although outright dropout from the Diploma was uncommon, trainees often had to switch cohorts due to other commitments. This disrupted their learning sequences and group dynamics. This attrition reduced the impact of the original investment and underlined the need for a clear CPD plan to maintain a critical mass of skilled practitioners.

Quality of intervention delivery

This section addresses one research question: To what extent are the different components of the intervention delivered at a high quality? (IPE4)

Findings focus on how **school staff** received and used the training to change their practices and implement the intervention within their settings.

KEY FINDINGS:

- **The Diploma programme and reflective supervision deepened understanding, built confidence and supported the transfer of skills into classrooms and pastoral work.**
- **Consultants modelled relational practice and prioritised psychological safety, making principles concrete and reducing anxiety with sensitive material.**
- **Practical tasks and rehearsal with feedback improved staff language, decision-making and fluency.**
- **Tools and frameworks (for example, ACEs) supported day-to-day use and a shared language amongst staff.**
- **Quality was maintained through flexible delivery to fit the context.**
- **Annual content review and trainer quality assurance supported consistency and standards.**

School staff's feedback on the quality of the TISUK training and resources was generally positive. Schools described the core training as informative and both professionally and personally transformative, especially the Diploma training and reflective supervision training. One teacher noted that the training strengthened her professional identity and renewed her enthusiasm for her role. Some school staff regarded Diploma training and reflective supervision training as amongst the most professionally meaningful CPD they had undertaken.

> "I kind of felt like I'd been institutionalised… And then it brings you back, prompting reflection on why I actually do this job." – School Staff Member

Reflective supervision training was described by school staff as most effective when schools protected time and options for both group and one-to-one sessions. One school planned and introduced an internal model led by a small in-school team, including the Headteacher, who had completed the reflective supervision training. School staff viewed reflective supervision as a cost-effective way to manage the emotional load of providing targeted support to pupils.

> "The reflective supervision is really, really good. Super effective. It makes everybody feel better." – Support Staff Member

In another school, one teacher reported that supervision occurred too infrequently to meet the need (for example, once a term) or was informal for a small group. Where formal provision was not in place, some teams relied on ad hoc peer cover to "tap out" when empathy fatigue set in.

Intervention delivery was hindered where supervision was not embedded or was too infrequent; consultants observed that a lack of protected time and workload pressures meant sessions were often delayed or deprioritised as urgent pastoral issues and cover took priority. In one case study, school-only sessions were conducted only occasionally despite staff valuing supervision through the Diploma training. In these instances, coverage and continuity of training contents were limited, reducing staff capacity to process the emotional load and to feed learning back into pupil profiles, risk assessments and day-to-day practices.

Consultancy networking was most helpful, with Headteachers and senior leaders consistently attending and using sessions to align strategy, policy and day-to-day practices. Where leadership attendance was inconsistent, there was less follow-through, and as a result, key actions, such as behavioural policy updates and a single referral route, were delayed. At least one consultant felt that standard 90-minute meetings were not long enough to explore the details of implementation and preferred longer or more frequent sessions with clear actions.

> "The school was slow and steady, very strategic. Their head came to every meeting and helped think through barriers and culture change." – TISUK Consultant

Whole-staff training delivery worked better when schools followed up with brief refreshers and visual prompts; for example, posters and strategies were placed in staff planners, staff rooms and even staff toilets, and key messages were kept alive through briefings and emails. Some participants described the sessions as broad rather than deep unless leaders supplemented them with local refreshers, micro webinars and simple prompts in planners, lanyards and classroom displays.

For some school staff, the three-hour virtual sessions led to online fatigue and IT issues, and some secondary staff found the content too general, with limited tailoring for pupils with SEND and English as a Second Language (EAL). Organised group viewing sessions meant some did not complete both sessions. Some school staff highlighted that without debriefs or refreshers, transferring from training to practice was challenging, and sensitive material also needed reflection time that was often unavailable.

Senior leadership training had a greater effect when school leaders attended, modelled the approach and linked it to school improvement and policy. In several schools, leaders embedded expectations into induction and CPD, set up working groups or "champions" and aligned behavioural policies with relational practices (for example, moving away from detentions and isolation). Where leadership engagement was low, the school staff interviewed reported trauma-informed work remained fragmented and under-resourced. There were also instances of role–training mismatch, where frontline pastoral staff were placed on the shorter senior leadership course rather than the practitioner Diploma due to cover constraints, but the training was not well-aligned with pupil-facing work.

Participants considered the practitioner Diploma practical and impactful. Participants highlighted that it helped them apply tools in everyday work and increased their confidence when supporting pupils, with one teacher calling it the most valuable training since qualifying.

> "One of the most useful training things I've done in many years… It gave me the language [and] the skills, and [it] made me realise how vital this is in education." – Senior Leader

Many preferred face-to-face delivery because it supported relationship building and allowed for more reflection between activities. To manage pacing and continuity, some schools scheduled short follow-up webinars or monthly refreshers so staff could revisit key sections at a more manageable pace and "keep the language alive". Some teaching staff found the volume and pace of the content challenging, especially online. Some days were densely packed and fast-paced, and participants reported feeling overwhelmed and emotionally tired by the end of the day. A trainer acknowledged that at times, too much might have been covered.

School staff considered a core perceived strength of the programme to be its solid theoretical models. Some interviewed staff said they valued the programme's grounding in neuroscientific and psychological research, which was regularly updated to remain relevant. The models noted by school staff included ACEs, Jaak Panksepp's emotional systems and the Protect, Relate, Regulate, Reflect (PRRR) framework. The staff felt this evidence-based approach provided them with a clear roadmap of actionable strategies. One Pastoral Lead observed that the frameworks became part of everyday conversations amongst staff.

> "One of the nicest things is when you walk into a staff room now, and you're hearing the language, like 'PACE [Playful, Accepting, Curious, Empathetic]', 'ACES', those things coming out of just general conversations amongst the staff." – Pastoral Lead

School staff reported that consultants modelled relational practices and used practical tasks in the training, making the approach clear and boosting confidence and classroom use. The teachers interviewed shared that an important facet of the programme was that TISUK consultants "practised what they preached" and modelled the same relational approaches expected in classrooms in their trainings. Brief check-ins, often with emotion cards, agreed sentence stems, calm tones, measured pacing and short regulation breaks made the principles visible and usable. Several school staff said these elements were important for their learning because they could see and feel how they worked in practice. The format used breakout tasks, roleplay of repair conversations, doorstep resolution and triage using flowcharts and pupil profiles from the programme. Staff rotated through pupil, adult and observer roles and used simple observation prompts to give specific feedback on language, body language and the choices offered. These activities built perspective-taking skills, improved confidence and helped staff apply strategies in classrooms and pastoral spaces.

School staff reported that TISUK consultants prioritised psychological safety in the Diploma training, using clear strategies throughout the learning experience. TISUK consultants supported learning by building psychological safety and practice from the outset. For the Diploma programme, they held duty-of-care calls with applicants who disclosed personal trauma to discuss likely triggers and support, which participants said helped them feel prepared to engage with challenging material. During delivery, a paired training model was used: One trainer led the content, and another monitored the group's emotional state and managed the chat, which staff described as important for staying regulated and able to learn.

Small, consistent home groups with a named tutor created safe spaces for teaching staff to rehearse empathic responses and receive direct coaching; interviewed trainees reported this resulted in greater confidence, more explicit de-escalation language and stronger links to their own classrooms and pastoral work. The assessment required staff to apply their learning in their settings and reflect on its impact, which participants said helped consolidate skills and transfer them into everyday practice.

Quick-win techniques: Trainers encouraged quick wins, such as pupil meet-and-greets at the door and the WINE ("I wonder, I imagine, I notice") stems; trainees reported these were easy to adopt, built early momentum and showed colleagues the practical value of the approach. Overall, school staff feedback indicated that these features made the content usable, supported emotional containment and improved staff readiness to implement the strategies with pupils.

Teachers highlighted that the specific tools and resources provided by TISUK were effective and easy to use. In addition to the training sessions, the programme included tools and resources to support day-to-day implementation:

- **Templates and reference materials:** The programme offered ready-to-use templates and quick-reference materials designed for daily implementation, including flowcharts for triage decisions, lanyard cards with sentence stems, classroom regulation posters and pupil profile forms. Some school staff shared that they kept laminated copies at their desks and regulation stations, with digital forms on shared drives for universal access. Many schools customised these templates to align with local practices while maintaining a consistent language across settings. School staff interviewed reported that these tools standardised decision-making, facilitated quick handovers and simplified de-escalation, fostering consistency and enhancing confidence in interactions. A senior leader noted, "It provided the language [and] skills and underscored its importance in education".

- **Emotion cards:** School staff reported that emotion cards were widely used for quick check-ins and resets, both at the start of the day and mid-lesson. Staff said they were especially helpful for pupils with limited vocabulary or EAL needs and when pupils were too heightened to verbalise, as pupils could point to a feeling or intensity level. In some schools, emotion card sets were placed on teacher lanyards and at regulation stations and were routinely used during doorstep resolution to agree upon a short, time-bound reset (for example, a two-minute breathing exercise or movement break) before rejoining learning. In some schools, staff shared that in classrooms, a simple one-to-five intensity scale on the board mirrored the cards, helping pupils signal need discreetly and enabling staff to log patterns alongside triggers in brief incident notes.

> "Emotion cards are really useful, especially for pupils with limited language or [pupils who are] too heightened to speak, because they can indicate a feeling and we can respond straight away." – School staff

- **Relational support plans:** The one-page, strengths-based plans were a way to put core TISUK principles into action. The plans outlined early signs of dysregulation, known triggers, preferred co-regulation strategies, key adults and agreed repair steps. In some instances, plans were co-produced with pupils and, where appropriate, parents/carers, uploaded to the MIS and reviewed after incidents or triage discussions to ensure they remained live and useful. Staff valued the inclusion of specific sentence stems and "first next then" responses, which made it easy for cover staff to act consistently. In several cases, interviewed staff said these plans replaced generic timeouts with short movement breaks plus an adult check-in, which they said reduced lesson removals and sped up return to learning.
- **Short webinars:** School staff appreciated 15–30-minute micro webinars. The topics covered practical "how-tos" (for example, co-regulation in the classroom, writing pupil profiles, running repair conversations, doorstep resolution), each with downloadable handouts, reflection prompts

and a small practice task. This format helped schools to begin implementation and gave leaders a consistent resource for onboarding new or supply staff.

TISUK maintained quality delivery primarily through an annual review and a quality assurance process for consultants.

An annual evidence-informed review kept TISUK's core content current and consistent: TISUK consultants conducted a formal annual review of the core content. The review drew on several inputs, including new research and guidance (Department for Education (DfE) and Ofsted updates, Keeping Children Safe in Education changes and relevant National Institute for Health and Care Excellence guidance, trainer debriefs and quality assurance (QA) observations, participant feedback, assessment evidence and basic implementation data, such as attendance, completion and common barriers. The review led to updated standard materials aligned with current evidence and learning from delivery while keeping the programme aims, structure and assessment stable. Updates focused on facilitator manuals, slide decks, phase-specific case studies, practice tools such as flowcharts and pupil profile templates, safeguarding signposting and adaptation guidance for SEND and EAL. Trainers were expected to deliver the updated national content to maintain parity across cohorts, with scope to tailor examples and style to context.

Consistency was maintained through a quality assurance process for all trainers: The quality assurance process involved all new TISUK consultants co-delivering with a senior trainer rather than teaching alone, and they would receive feedback on their delivery from the more senior consultant. This process was designed to ensure consistency and maintain a high standard of delivery. Evaluators observed that this structured approach did not hinder responsive and tailored delivery. While adhering to core, evidence-based content, trainers were adept at adjusting their styles and adapting examples to be more relevant to the specific audience they were working with. For instance, a trainer might employ more secondary school–oriented examples when working with such an audience, demonstrating a balance between maintaining fidelity to the model and being responsive to each school's unique context and needs.

The extent to which Trauma Informed Schools UK approaches were implemented within schools at a high quality

This section addresses one research question: What strategies and practices are used to support high-quality implementation? (IPE7)

KEY FINDINGS:

- **Deliberate strategies that translated learning into practice supported the embedding of a trauma-informed approach.**
- **These strategies ranged from reflective supervision sessions to shifts in school policies, structures and culture.**
- **High-quality, sustainable implementation was most likely when schools used a creative, multi-faceted approach that bridged training and daily routines.**
- **Barriers included limited senior leadership buy-in and friction with existing behavioural management systems.**

The implementation of TISUK approaches was mixed and dependent on senior leadership.

Across case study schools, the strongest examples of implementation were whole-school, with regulate-first policies and updated risk assessments for pupils (setting out triggers, early signs and agreed scripts).

For pupils in need of targeted support, a single referral route, overseen by a triage group, set clear thresholds, directed pupils to a menu of supports and monitored cases RAG-wise and informed by wellbeing assessments. Staff used a shared language and kept one-page pupil profiles live, reinforced through scheduled supervision and post-incident reviews.

Physical environments also reflected the approach, with isolation booths replaced by staffed reflection spaces and timetables that protected targeted TA sessions focused on regulation, coaching and repair. Senior leadership and governor buy-in were the main enablers of implementation in these schools. Leaders made the approach a strategic priority, authorised policy changes and risk-assessment updates and established the triage system with scheduled meetings and clear roles. They reallocated rooms and staffing to create reflection spaces, ring-fenced TA time for targeted work and timetabled supervision and post-incident reviews.

Expectations were embedded through induction and performance management, supported by CPD, coaching and walk-throughs, so the shared language and routines were applied consistently, with simple session logs and regular TA/teacher/SENCO check-ins required. Leaders mandated RAG monitoring and the use of wellbeing assessments at triage aligned TISUK with existing frameworks, such as When the Adults Change, and used tools such as Steplab to notice and reinforce effective practice. Making ownership visible, attending training, modelling routines and protecting time made doorstep resolution feasible, kept pupil profiles live and sustained targeted support.

However, in other instances, implementation within schools was weaker due to partial and uneven adoption. Tools were used ad hoc, shared language and repair routines did not embed consistently and thresholds for regulate-and-return varied by classroom. Targeted support was often diluted: Protected TA time was diverted to cover, scheduled sessions were cancelled during busy periods, referral routes were inconsistently applied and duplicate referrals reappeared.

Thresholds for access to mentoring or small-group work were unclear, waiting times lengthened and session logs or follow-up reviews were missed, making it harder to refine plans. Under pressure, some staff reverted to points-based sanctions or two-strikes removal, reflecting tensions with zero-tolerance behavioural policies.

Some training content was only partly completed or watched in groups, so sections were missed, and turnover meant new and supply staff did not receive inductions to the approach. Supervision was infrequent in some settings, and in one case, a single practitioner supervised colleagues without receiving supervision themselves. Staff adapted pupil-facing materials for older pupils after a poor fit (for example, replacing sand trays with discreet clay tools). In schools without clear SLT and governor ownership, the work sat outside performance objectives and was described as bottom-up, leading some colleagues to view it as optional.

Overall, the extent of implementation ranged from embedded, cross-system change to pockets of practice, largely determined by visible leadership commitment, protected time, supervision and a planned, school-wide rollout, to informal cascading.

Targeted support

The targeted support element in the TISUK evaluation centred on tailored trauma-informed interventions for pupils who were identified as having specific needs. In one case study school, of the 36 Year 9 pupils deemed at risk, 11 received this support. The school staff interviewed highlighted that the support comprised structured, relational work delivered mainly one-to-one and occasionally in small groups by Diploma-trained practitioners, such as Heads of Year, Pastoral Leads, SENCOs and members of ethos or wellbeing teams. Each pupil worked with a named EAA, who offered regular check-ins and emotion coaching to build trust, develop emotional literacy and support regulation.

The work was usually time-bound; schools often decided to deliver support in six-week blocks, with clear goals and review points so that pupils could step down when ready. Practitioners used a standard toolkit adapted to age and need, including emotion cards, empathy drawings, pen portraits, sensory materials, such as Play-Doh, and carefully scripted sentence stems, to guide reflective conversations. Brief, non-punitive uses of regulation or reflection spaces helped pupils calm down and return to learning, and in some settings, staff reflective supervision ran alongside to support consistency and staff wellbeing.

In several case study schools, these targeted cycles were guided by wellbeing assessments or pen portraits. Case study schools provided practical examples of this approach. In one school, practitioner-trained staff held caseloads, offered in-class de-escalation through a doorstep resolution model and staffed a reflection space focused on a rapid return to learning. In another school, wellbeing assessments informed six-week one-to-one plans and the targeted use of a reflection room, and pupils reported greater trust and improved self-regulation.

Across other schools, regular one-to-ones were embedded through Diploma-trained Heads of Year, weekly practitioner time was timetabled and regulation areas were redesigned, with staff noting calmer classrooms and earlier repair. Some schools brought targeted support into the classroom to avoid removal, with a practitioner joining briefly to help the pupils re-regulate and remain in the lesson.

> "Different people have found their tools – for some it's clay, for others it's empathy drawings. We've given practitioners time each week to work with children, and it's part of the timetable." – Headteacher

Referral routes into targeted support varied by school but typically included teacher referrals to pastoral or ethos teams, pupil self-referrals, parental contact and safeguarding or DSL triggers, often supported by data, such as attendance records, behavioural points or repeated classroom dysregulation. In one case study school, universal language and awareness had grown, teachers reported greater confidence to flag concerns early and pupils were more willing to ask for help before a crisis. Once a concern was raised, a pastoral or ethos team usually triaged the case, updated or created a pen portrait and assigned an EAA.

Schools commonly planned a defined block of one-to-one work, often at around six weeks, with clear aims and tools, such as emotion cards, empathy drawings, sensory materials and attuned scripts. In one case study school, wellbeing assessments were used to select pupils and shape each six-week plan; in another, a pastoral hub enabled non-teaching Heads of Year to check in proactively and address concerns before they escalated. Other case study schools risk-mapped pupils vulnerable to exclusion, paired them with a trusted adult and scheduled weekly meetings alongside in-class doorstep support, when required.

Review was built in at the end of the block: Staff and the pupils reflected on progress, adjusted goals and either stepped down to lighter-touch check-ins or extended support. Where reflective supervision was in place, practitioners brought cases to supervision to consider barriers and maintain consistency, and insights from targeted work were shared with classroom staff so that universal practices were adapted around the pupil.

Some schools replaced multiple forms with a single accessible referral form that captured need, context, urgency and previous strategies tried. A dedicated triage group, typically including the Trauma-Informed Lead, SENCO, Safeguarding or Pastoral Leads and Attendance Leads, met on a regular schedule. Decision-making was guided by clear thresholds and a menu of in-school supports, such as TA intervention, mentoring, sensory diets, timetable adjustments and restorative meetings, with escalation pathways to external services used when needed. Referrers received timely decisions and next steps, cases were monitored using a simple RAG status, duplicate referrals were reduced and waiting times were shortened.

General trauma-informed practices

In contrast to targeted support, general TIPs were implemented across the wider school population to embed a school-wide trauma-informed ethos. This included whole-staff training in trauma-informed language and strategies, the creation of shared spaces for emotional regulation and the integration of trauma-informed principles into behavioural policies and routines.

Across the case study schools, staff introduced shared language and scripts, such as WINE sentence stems and PACE phrasing, used visual prompts in classrooms and staff areas and adopted routines such as meet-and-greet. One case study school moved from a behavioural policy to a relationship policy, and another co-wrote subject-specific scripts so that empathetic language appeared in academic feedback as well as behavioural conversations. While targeted support addressed individual needs, these universal practices shaped everyday interactions and helped reduce escalation across the school community.

Some of the school staff interviewed shared examples of general support within their individual schools:

- Structural and policy integration: Protocols and pupils' behavioural assessments were updated to embed a regulate-first response. Individual risk plans recorded triggers and early signs and agreed de-escalation scripts, safe-space locations, supervision ratios, criteria for temporary removal and the repair/restore steps on return. Staff received flowcharts and sentence stems to standardise practices, and incidents were logged with a simple RAG status to enable dynamic risk assessments to be updated promptly. Post-incident reviews in supervision and triage checked adherence to protocols, refined controls, adjusted thresholds and recorded safeguarding exceptions and escalation to external services.
- Common language: Staff adopted a shared vocabulary, supported by a glossary and quick-reference guides, for terms such as dysregulation, co-regulation and repair/restore. They were coached to use non-blaming language.
- One-page pupil profiles captured each pupil's strengths, triggers, signs of dysregulation, preferred regulatory strategies, key adults and agreed responses. These were updated routinely and were accessible to relevant staff. Consistency was reinforced through visual prompts in classrooms, regular walk-throughs to rehearse and refine routines and coaching to ensure agreed responses were applied across subjects and settings.

- Policy and documentation language updates: Following language audits, behavioural policies were rewritten with trauma-informed language. For example, “defiant” was replaced with “dysregulated” and “sanction” with “repair/restore”. Some schools created standalone relationship policies. Processes were clarified with explicit repair/restore pathways after incidents, graduated responses that replaced one-size-fits-all sanctions and clearly stated safeguarding exceptions for immediate safety. Documents were updated accordingly. Reflection sheets were reworked into repair plans. Classroom posters emphasised expectations and regulation options. Staff handbooks included scripts and flowcharts to support consistent responses.
- Shifts in staff–pupil communication: Staff developed banks of sentence stems (phrases that start conversations) that centred curiosity and collaboration. Examples included: “Help me understand…”, “What do you need right now to feel settled?” and “Let’s figure this out together.” Practice components emphasised tone, body language, pauses, reflective listening and regulated choices. Explicit de-escalation steps were taught and rehearsed through CPD. These changes were intended to lower escalation, achieve faster recovery after incidents, strengthen trust and relational safety, and provide clearer modelling of self-regulation.

“Some of us are using everything – emotion cards, sand trays, empathy drawings.” – Staff Member

- Integrating TISUK with complementary models: To increase staff buy-in, the TISUK approach was combined with other established frameworks that schools were already using, such as Paul Dix’s When the Adults Change. This helped position TISUK as an enhancement of existing good practices and reduced initiative fatigue.
- Staff wellbeing and supervision: Reflective supervision, both one-to-one and in groups, was scheduled and prioritised for the staff most exposed to challenging interactions. This provision created space to process incidents, recognise secondary trauma, problem-solve using TIS-informed strategies and plan boundaries to prevent burnout. Debriefs were held after critical incidents. Access to additional help was provided where needed. Workload adjustments were made when high-intensity work patterns were identified. There was explicit recognition of the emotionally demanding nature of roles.
- Doorstep resolution models: In some schools, trained practitioners (for example, the TISUK Lead, a Pastoral Mentor or a SENCO team member) were called to the classroom door to de-escalate and co-regulate without removing the pupil from learning. The practitioner led with relational cues and agreed sentence stems (“Help me understand…”, “What do you need right now to feel settled?”). They used calm tones and body language and offered regulated choices: a brief movement/regulation break at a nearby station, a seating tweak or a simple sensory strategy. A short reset was agreed upon so the pupil could rejoin the task. Session and incident data (frequency, duration, return-to-learning rates) were reviewed in supervision and triage meetings. This reinforced the message of shared language and reduced lesson removals by bringing support to the pupil.

“We’re able to solve more issues on the doorstep, so the child doesn’t leave the classroom. Actually, we go to them… ‘How can I help you here?’” – Pastoral Support Staff Member

- Improved physical environments: Schools phased out isolation booths and reconfigured spaces to make regulation easier, more visible and routine. This signalled a cultural reset towards TIP. Former isolation rooms were co-designed with pupils to become restorative areas. They featured calm colour palettes, soft seating, adjustable lighting and sensory resources. Classrooms used colour-coded regulation zones with clear permission prompts and co-regulation scripts. Quick-access tools (breathing posters, timers, movement/sensory options) supported brief, time-bound resets. Staff were briefed on alternatives. Use was logged and reviewed in supervision. Cues were kept consistent across subjects and linked to pupil profiles and the triage/on-call system to support a rapid return to regulation.

> "The reflection space is manned by one of the trained practitioners. It's not about punishment; it's about regulation, reflection and back to class." – TISUK Consultant

Facilitators of implementation within schools

The engagement and commitment of staff in leadership positions were important factors in the successful implementation within schools.

Most teachers reported that the trauma informed practice approach would only be embedded if the head and senior leadership were visibly invested. Senior leaders set the tone and controlled priorities, budgets and timetables. Where engagement was weak, schools were less likely to cover costs, release staff or set aside time for training and supervision. This, in turn, limited consistent implementation.

A TISUK consultant confirmed that a key early indicator of success was when "I got the head to attend the meeting". This top-level commitment extended to the governing body; one member of the teaching staff at a school found it was helpful that their Chair of Governors was formerly the SEND Governor and the Mental Health Governor, as this support aided in "securing extra time and capacity for training".

Senior-level commitment was most often demonstrated through significant, visible changes that signalled profound cultural shifts. One participant, for instance, observed their school had "completely gone [away]" with its isolation booths, instead setting up a reflection room with natural light. This was described as a commitment to the TISUK project and a clear statement of its new direction. In another example of modelling behaviours, a school's SLT decided to ensure every senior leader was visible in the corridors in the mornings, modelling the meet-and-greet TIP practice with pupils.

Peer-to-peer cascading helped spread the practice.

A common strategy was to cascade learning to wider staff through short briefings and coaching. In one case, three practitioners each led a 20-minute session to share and discuss key ideas with colleagues who had not attended the training. Another school used Steplab, an online instructional coaching platform, to run brief classroom drop-ins and send notes to recognise effective relational practices that were being observed in lessons.

The case studies revealed that the most effective schools approached implementation as a strategic, systems-level change.

One effective strategy was the use of internal working groups or "trauma-informed champions", as evidenced in School 5, where a group met fortnightly to coordinate training, adapt tools and bridge reflective

communication between senior leaders and staff. The school evidenced successful implementation by retaining five pupils at risk of exclusion and helping them achieve full attendance and successfully transition to final exams, crediting these changes to the strategic use of trauma-informed tools and support.

> "We called the working group the 'trauma-informed champions'. We meet, plan and check implementation across areas like PSHE[19], shared visuals and regulation cards." – Assistant Head

At another school, a few members of the teaching staff led the implementation without a formal working group or leadership buy-in, leading to ad hoc tool usage – for example, forgetting to use emotion cards during incidents. Without a whole-school structure, there was misalignment between staff who advocated for relational approaches and those who maintained "strict behaviourist" views.

> "There's a real tension. Some staff are into 'love the kids', others are strict behaviourists. It hasn't been resolved." – Pastoral Staff Member

Barriers to implementation within schools

Friction with pre-existing behavioural systems: In some schools, hesitation to apply trauma-informed approaches persisted. This was linked to pressure from existing behavioural systems and classroom management demands.

Staff described tensions between relational approaches and zero-tolerance policies. Some colleagues felt they had to hold a line under a two-strikes-and-you-are-out system and points-based sanctions. Teachers reported that doorstep resolution and regulate-and-return conflicted with expectations to remove pupils after a second warning. Some were unsure about the thresholds for offering a brief regulation break without being seen as inconsistent.

Some non-teaching practitioners also reported that teachers perceived their attempts to deliver training as patronising and condescending. These teachers felt they were being instructed on classroom management.

Incomplete and overly general training: Implementation was uneven across schools, with staff missing trainings, not completing modules or experiencing high staff turnover. This led to inconsistencies, such as colleagues not sharing the same sentence stems or repair routines and varying thresholds for regulate-and-return and doorstep resolution. In some instances, teachers reverted to older policies, such as points-based sanctions, when under pressure.

High workloads: Workload challenges hindered the implementation of some practices, particularly reflective supervision. In some schools, supervision was infrequent or informal. In one case, a single trained practitioner supervised colleagues without receiving supervision themselves.

[19] Personal, Social, Health, Economic education

Protected time for TAs was often diverted to cover, making relational plans and in-class co-regulation harder to deliver at busy times. In these conditions, TIP was more likely viewed as an optional add-on than something embedded within day-to-day routines.

A lack of complete buy-in from senior leadership: In schools where leadership was divided, a degree of tension emerged between a trauma-informed approach and a more traditional, discipline-focused one. This was evident in one case study school, where the absence of clear SLT ownership meant that pastoral and teaching staff perceived the initiative was largely "driven bottom-up rather than top-down". A Pastoral Lead confirmed this lack of top-level integration, stating: "It wasn't part of my PDR [professional development review]; there were no targets linked to [TIP]". Consequently, the approach was viewed by some staff as a "soft thing or just something nice to do if there's time". As one consultant expressed, successful implementation required feeling that you "weren't trying to push the elephant up the stairs alone – someone was hauling from the top as well".

Some pupil-facing materials did not receive favourable feedback: Some school staff described the PSHE videos and role-play scenarios for pupils as not age-appropriate. In fact, they could be counterproductive for older pupils. Staff reported stronger uptake of simple, practical tools that were easy to adapt and use in everyday interactions.

Staff also adapted some materials to be more age-appropriate. For example, some older pupils rejected emotional regulatory tools, such as the sand tray activity (a therapeutic approach utilised to enable the exploration of an inner world), as "babyish" but used small amounts of clay as discreet fidgets. Where activities initially missed the mark, schools iterated. One team redesigned the "I wish my teacher knew…" activity after early feedback. They delivered it in smaller groups with follow-up, which improved engagement and disclosure.

Perceived impact of the Trauma Informed Schools UK training

This section addresses one research question: What is the perceived need for and benefit of the intervention amongst school staff and leadership? (IPE6)

KEY FINDINGS:

- **Schools viewed the programme as essential amidst deprivation, ACEs and post-pandemic needs.**
- **School staff reported a range of impacts, including fewer exclusions, better attendance, improved regulation and emotional literacy, earlier help-seeking and calmer classrooms through repair-focused practice.**
- **School staff reported feeling better equipped and more confident.**
- **Schools moved from punitive systems to relational, restorative practices.**
- **Consistent leadership engagement that protected time and supervision were considered to increase impact.**

Schools identified a strong need for trauma-informed approaches.

In most schools, school staff perceived the programme as essential, not just an optional extra, to navigate the complexities of modern education.

The programme's perceived necessity was linked to challenging school contexts, such as high deprivation levels and pupils with multiple ACEs. A SENCO stated that safeguarding and wellbeing needs were essential to address before considering attainment:

> "We cannot develop [curricula] and look at GCSE results if we're not doing this stuff first."
> – Teacher

This sentiment was shared by case study schools in high-deprivation areas. They described the model as essential for sustainable inclusion and as a "professional compass" in demanding environments.

The COVID-19 pandemic heightened demand for trauma-informed approaches. One TISUK consultant noted that the pandemic made school staff more receptive to understanding pupil trauma. Some staff noticed more pupil anxiety and dysregulated behaviours, alongside more disclosures of domestic abuse and family stress. Referrals to CAMHS and social care increased in some schools. Other staff noticed attendance patterns worsened, and many pupils struggled with routines. Teaching staff perceived these changes in behaviours as reflecting unmet needs rather than pupil defiance.

As a result, in some schools, training on trauma, regulation and de-escalation increased. In some cases, school pastoral teams were expanded where possible. Other schools put in predictable routines, safe spaces and regular check-ins.

Staff also shared that the TISUK training had filled important gaps in their professional education. They noted that these areas had not been covered in earlier training. Many reflected that receiving it at the start of their careers would have been most useful. They felt it would have supported early decision-making and everyday practice. Several commented that it would have helped them respond with more confidence in their first roles.

Impacts on pupils

The most frequently cited benefit of the intervention by interview participants was its positive impact on pupils. Staff viewed the trauma-informed approach as a necessary response to decrease pupil distress and as a remedy for the limitations of traditional, data-driven approaches.

Across case study settings, staff reported fewer exclusions and serious behavioural escalations.

One school recorded a 35% year-on-year reduction in suspensions and zero exclusions at the time of the fieldwork. Pupils who were previously on a path to exclusion were attending, engaging and preparing for GCSEs.

Another school reduced reliance on external alternative provision by meeting needs in-house as a result of Diploma training. They used restorative regulation spaces and timetabled practitioner time for relational work. In one case, a Year 11 pupil with repeated suspensions reached 100% attendance with no behavioural points after weekly check-ins with a trusted adult. De-escalation and regulation techniques were used.

In other settings, impact was mostly concentrated in individual de-escalations with targeted pupils, rather than being observed across the full school.

> "[Pupil] used to spend more time in corridors than in lessons… now she has weekly meetings, feels listened to and hasn't needed her hoodie [a former signal of distress]." – Senior Leader

Improvements in emotional literacy and self-regulation were widely observed amongst pupils who received targeted support.

These improvements were linked to consistent tools and a shared language. Staff observed that pupils were more adept at naming and managing feelings due to the use of emotion cards, empathy drawings and regulation spaces. In some schools, pupils sought help earlier, often before a crisis, and recommended these tools to peers.

> "We did emotion cards once. That helped me talk about my feelings and helped her understand me more, so she knew what to ask." – Pupil

Staff in other settings noted increased self-referrals to wellbeing teams.

Regular check-ins with EAAs were associated with improved attendance and calmer behaviours. One pupil described feeling "peaceful, calm" when able to talk to a trusted adult.

Pupils also reported a wider network of approachable adults:

> "There's definitely more people around now that I could talk to… having a trusted adult or someone to go to," and "Miss B is my trusted adult in the school… I just really like her." – Pupil

Classroom climates became calmer, with quicker returns to learning.

Some staff perceived that their schools shifted from punitive responses to regulation and repair as a result of whole-school training. In some schools, a doorstep resolution model enabled on-the-spot regulation, so pupils remained in class. Others repurposed isolation spaces into staffed re-regulation rooms focused on repair and rapid reintegration.

The tone of teacher–pupil interactions changed. Staff adopted consistent sentence stems (for example, "I notice…", "I wonder…"). In some cases, they co-created subject-specific "micro-scripts" that wove empathetic language into academic feedback.

Pupils recognised the difference in how teachers interacted with them.

> "Some teachers listen but don't hear it. Others actually talk to you like, 'How can I help with it?'" – Pupil

Examples of impact

Year 11 pupil at risk of exclusion: This pupil had experienced repeated suspensions, spent more time in corridors than in lessons and used a hoodie as a distress signal. After beginning weekly meetings with a trusted, trained adult and receiving consistent relational check-ins and regulation support, she achieved 100% attendance with no behavioural points or exclusions. As one senior leader put it, she felt listened to and no longer needed the hoodie. The pupil said,

> "I just like having an adult I can really talk to in school… It's peaceful, calm." – Year 11 Pupil

Year 9 pupil building trust with one adult: This pupil had previously had low trust in adults and frequent escalations. Through targeted sessions using emotion cards and empathy-based conversations with a single, consistent practitioner, he became more able to name his feelings, seek support early and de-escalate before a crisis. He said,

> "Miss B is my trusted adult in the school… I just really like her" and "We did emotion cards once. That helped me talk about my feelings and helped her understand me more." – Year 9 Pupil

Year 9 pupil reframing adult relationships: This pupil had felt staff talked down to her and disengaged as a result. During one-to-one work, a practitioner used non-judgmental, empathetic language (including WINE sentence stems), which reframed the relationship. The pupil engaged more readily and showed greater trust in support, saying,

> "She didn't speak down on me. She spoke with me… like she was my friend, not like a teacher." – Year 9 Pupil

Year 10 pupil previously on a path to exclusion: This pupil had been on a trajectory towards permanent exclusion. The school's pastoral hub, staffed by non-teaching Heads of Year, provided reliable access to an EAA alongside the consistent use of trauma-informed scripts. By the time of the fieldwork, the pupil was attending, engaging and preparing for GCSEs. The pupil noted,

> "There's definitely more people around now that I could talk to… having a trusted adult or someone to go to." – Year 10 Pupil

Year 9 pupil using a re-regulation space: This pupil's dysregulation had typically led to their removal from class. After the school redesigned its isolation space into a restorative regulation room staffed by trained adults and equipped with age-appropriate tools, such as Play-Doh, the pupil calmed more quickly and returned to learning sooner, with less stigma.

Year 9 pupil normalising help-seeking: This pupil had limited emotional vocabulary and hesitated to ask for help. Exposure to emotion cards and consistent empathetic language across staff helped him self-refer for support and even recommend tools to peers. A senior leader observed that students had started saying,

> "You should do the cards with her – it helped me." – Year 9 Pupil

Year 10 Pupil nearing permanent exclusion: This pupil had persistent behavioural incidents and poor attendance and was close to permanent exclusion. A layered package of support, including targeted one-to-one work, emotion coaching, structured check-ins and regular supervision with staff, was put in place. The pupil subsequently achieved full attendance, completed exams and took part in end-of-year milestones, such as prom.

Staff who participated in the Diploma training reported significant personal and professional benefits, feeling "better equipped" and "more confident" in their roles.

A key benefit was offering a new psychological framework that allowed staff not to take challenging behaviours personally. The training was described by some staff as "life-changing", enhancing self-awareness and professional practice. The programme also provided validation and empowerment, with staff feeling they had "permission to slow down" and respond with empathy. This shift was crucial for staff wellbeing and retention, helping to create environments where staff felt "seen and heard". Some staff expressed feeling more psychologically safe, enabling them to foster similar safety in classrooms.

On a broader level, the intervention was seen as a powerful catalyst for positive and sustainable cultural change.

According to the staff interviewed, the BAU approach to behavioural management was often characterised by punitive, compliance-driven systems. This culture was perceived as disempowering for individual teachers, treating mental health as a siloed and specialist concern and lacking a deep understanding of the root causes of challenging behaviours. Broadly, the intervention served as a catalyst for positive and sustainable cultural change, introducing a shared language and framework for moving from punitive, data-focused approaches to more compassionate, relational and restorative ones. Staff expressed disillusionment with purely data-driven directives, feeling they had been "institutionalised" by a relentless focus on "standard data". The training reconnected them with the core human purpose of teaching. School leadership reported long-term, systemic benefits, such as improved teacher retention, reduced staff absence and enhanced attainment. By moving away from one-size-fits-all behavioural systems, schools learned to "unpick what's behind the behaviour", fostering environments built on "relationship, repair and readiness to learn".

> "There's a weight off your shoulders when you can respond to a pupil with empathy instead of [a] consequence. It's not about letting them off – it's about keeping them in." – Pastoral Lead

The experience of minority ethnic trainees and school pupils

This section addresses two research question: How do structural factors (e.g. institutional racism, lack of diversity in the workforce) affect CYPs from Black, Asian and minority ethnic backgrounds? And how do CYPs from different sexes and Black, Asian and minority ethnic backgrounds experience the intervention? (IPE8 and IPE9)

KEY FINDINGS:

- **Interviews identified a perceived gap in the training relating to racial trauma and the experiences of staff and pupils of colour.**
- **School staff asked for explicit coverage in the curriculum of how trauma intersects with race and identity.**
- **School staff suggested practical guidance and tools to support diverse school communities.**
- **Addressing this gap was seen as key to inclusivity and improving the effectiveness of the TISUK programme.**

Content on racial trauma was identified as a gap in the curriculum.

Whilst the training provided a robust framework for understanding trauma generally, participants noted a lack of specific content on racial trauma in the core training modules. One participating school staff member, a self-described "woman of colour", noted that this absence was particularly apparent in her "predominantly white" school setting. TISUK staff highlighted that the webinars included a module on racial trauma and diversity for all staff; however, adherence scores showed that the usage of webinars was low across the schools.

The interviewed teachers highlighted that minority ethnic children, especially Black pupils, in secondary schools in the UK experienced a range of disparities, including being more likely to be subject to exclusions and disciplinary action, and often reported experiences of racism, stereotyping and lower teacher expectations. Minority ethnic pupils were also under-represented in higher-tier sets and some academic pathways while facing barriers to accessing pastoral support and culturally responsive teaching. Teachers felt that these inequalities were reinforced by institutional practices, such as curricula that centred predominantly on White British perspectives, limited representation of minority ethnic histories and role models, inconsistent responses to racist incidents and behavioural policies that disproportionately penalised certain groups, which together failed to fully reflect or support pupils' identities and lived experiences.

Given these known disparities and factors, teachers felt that the curriculum should address how racial discrimination and systemic inequality can be sources of trauma for young people and provide specific guidance for supporting minority ethnic pupils.

The curriculum gap on racial trauma affected staff capacity to support pupils.

The absence of specific content directly affected the ability of staff from minority ethnic backgrounds to support their pupils effectively.

A participant highlighted this challenge by noting the struggle with the lack of targeted training on racial trauma. This lack of preparation left the participant unsure of how to handle such issues with pupils. This

indicated that structural factors, such as an incomprehensive TISUK curriculum/content, can result in both staff and pupils from minority ethnic backgrounds feeling unsupported.

Consequently, this placed an additional emotional and professional burden on staff of colour from minority ethnic backgrounds. They had to navigate complex racial dynamics without a clear framework or guidance from their training.

Intersectionality matters: observations across identity markers

Observations about which school disciplinary measures most impact pupils highlighted the importance of accounting for intersectionality.

An interviewee identified that, in their school, pupils frequently sent out of class were often "White working-class boys with SEN needs".

This example, whilst not focusing on minority ethnic pupils, highlighted how the intersection of class, gender and disability can influence a pupil's interaction with the school's behavioural policies. This can potentially lead to unique experiences of vulnerability and stress for pupils in the school environment.

This evidence indicates that to be effective, the trauma-informed model must equip staff to understand and respond adeptly to these intersecting dynamics, including those related to race.

The findings suggested that the intervention experience for pupils from different backgrounds was complex and varied. Whilst there was a lack of direct data exploring racial equity, interviews provided important insights. They showed how the trauma-informed approach intersects with gender and significant structural disadvantage, particularly socioeconomic deprivation and special educational needs.

Gender and engagement

Within individual case study schools, gender was an observed factor in pupil engagement and staff delivery. One school noted an unexpected outcome, where Year 10 girls actively sought support not only for themselves but also for their peers. They encouraged each other to "go and see Miss… and ask if you can do the cards". This indicated a high level of perceived benefit and agency within this cohort.

In another school, the intervention prompted staff to consider gender in their relational approaches. For instance, a pupil's profile highlighted that a pupil living with only his mother might "not have a good relationship with male teachers". This fostered a more nuanced understanding of individual needs.

Intersectionality and structural disadvantage: a case study

The experience of one school offers a powerful case study in how TIP can respond to systemic inequality.

Set in one of the UK's most economically inequitable cities, the school serves a community facing high levels of poverty. The Headteacher noted that as the city has become more successful, it "[has pushed] families who are struggling" further to the margins, which has created a stark context of layered disadvantage.

With over 44% of pupils eligible for pupil premium and the highest number of children with disabilities in the local authority, the school faces a profound level of complexity.

"There is huge poverty. There are huge social issues… and the richer and more successful this city gets, the more that pushes [out] families who are struggling. It compounds that." – Headteacher

Staff at the school see the trauma-informed programme as important in responding to the context they are operating in. They recognise the deep intersection of poverty, trauma and SEND. Many pupils have co-occurring needs related to learning, behaviours and ACEs, such as parental addiction or imprisonment.

"Children are very aware of their socioeconomic [contexts]… this programme isn't a bolt-on – it's the only response to complexity." – SLT Member

In response, the school has sought to reorient its entire culture around its most vulnerable pupils. They use the TISUK model as a "whole-school relational compass". As one senior leader powerfully articulated: "Our job is to design a school for the most vulnerable pupil. If we do that, everyone will thrive".

The trauma-informed approach is therefore seen as a humanising framework. It enables schools to adapt their environments and practices to better meet complex needs.

However, the findings collectively point to a clear need for more explicit attention to identity, equity and inclusion in future rollouts. This would fully address the intersecting challenges related to race, gender and structural inequality.

"You've got this complexity – kids with learning needs, with parents in prison and [with] emotional challenges. And we're expected to get them to GCSEs without adapting how we teach or relate to them." – Assistant Head

"We can't keep pushing kids into systems that weren't designed for them." – Assistant Head

Cost information

Approach

We estimated intervention costs in accordance with the YEF's cost-reporting guidelines (YEF, 2022). We used a bottom-up approach, calculating costs from the perspectives of schools and the delivery organisation. This captures the full resource investment required to implement the programme.

We developed an illustrative cost model based on detailed data from one school, calibrated against evidence from case study interviews across five schools. This approach balances the need for granular cost data with validation across multiple settings.

From a commissioner's perspective, costs comprise two elements: fees paid to TISUK for training and consultancy and the staff time schools invested during implementation. All figures are reported in 2024/25 prices and exclude evaluation activities.

The YEF cost reporting guidance distinguishes between set-up costs (one-time expenses at the start of an intervention) and recurring costs (expenses that occur with each delivery cycle or cohort). This framework

works well for interventions delivered directly to participants on a per-session basis. However, TISUK operates differently. It is a whole-school capacity-building programme where the primary investment is training staff to embed trauma-informed approaches across their schools. The training itself is the intervention – once staff members complete it, they apply these approaches on an ongoing basis, without additional TISUK fees. Our cost data were collected by staff role and total hours rather than by activity type, which means we cannot reliably distinguish training time from any subsequent implementation time. Consequently, we present costs by funding source (TISUK fees versus school staff time) rather than by set-up versus recurring categories.

Data collection and validation

We collected detailed staff time data from two schools using structured templates. The schools recorded the hours spent on TISUK activities and hourly rates (including employer National Insurance and pension contributions). School 1 provided systematic data across 11 staff members. School 2's data showed inconsistencies and was not used.

We validated the illustrative model in two ways. First, we cross-checked salary rates against national pay scales for teachers and school leaders, confirming they fell within the expected ranges. Second, we conducted case study interviews across five schools. Staff members confirmed that the hours, activities and staffing configurations were representative of their experiences implementing TISUK.

The illustrative school was average in size relative to the trial sample. The staff members who attended training reflected typical choices: Pastoral Leads and Middle Leaders attended the 11-day Diploma course, while Senior Leaders attended shorter sessions. Interview evidence confirmed this pattern was common across schools, though some variation exists. The schools that sent higher-paid staff to intensive training incurred greater costs; those using more pastoral support workers incurred lower costs.

Several limitations should be noted. First, we did not collect data on administrative time for organising training, either for TISUK or schools. This likely underestimates true costs, though the magnitude is uncertain. Second, schools reported purchasing additional materials (such as sandboxes for therapeutic activities), but these costs were not systematically quantified.

Cost parameters

School staff time is reported by role. The hours primarily reflect attendance at training activities, though the data do not separate time by specific training component. Staff members who attended the 11-day Diploma course spent 82.5 hours on TISUK activities. Senior Leaders who attended shorter training sessions spent 7.5 hours each.

School staff time comes from School 1's records:

- Four Pastoral Heads of Year: 82.5 hours each at £20.46 per hour
- Director of Key Stage: 82.5 hours at £53.33 per hour
- Director of Inclusive Education: 82.5 hours at £43.99 per hour
- Assistant Headteacher: 7.5 hours at £57.07 per hour
- Deputy Headteacher: 7.5 hours at £66.66 per hour
- Headteacher: 7.5 hours at £94.11 per hour
- Safeguarding Lead: 15 hours at £35.93 per hour

- Intervention Supervisor: 82.5 hours at £18.48 per hour

TISUK delivery costs provided directly by the organisation:

- Training and consultancy: £13,400 per school
- Supplied materials (handbooks, cards, e-booklets, webinars): £687 per school
- Total: £14,087 per school

This package includes whole-staff training, senior leadership development, Diploma courses, consultancy sessions and reflective supervision.

Calculations

Table 40 presents the cost breakdowns. We calculated total costs by multiplying staff hours by hourly rates and adding TISUK delivery costs.

The cost per student uses 166 as the denominator, representing the average Year 8 cohort size across trial schools. We used this figure because Year 8 pupils were the target population for outcome measurement. However, TISUK is a whole-school intervention that affects all year groups. Using the total school roll (approximately 1,000 pupils) as the denominator would reduce the per-pupil cost to approximately £33. Commissioners should select the denominator appropriate for their planning assumptions.

Table 40: Cost breakdown

Cost component	Amount
School staff costs	
Four Pastoral Heads (4 × 82.5 hours × £20.46)	£6,752
Director of Key Stage (82.5 hours × £53.33)	£4,400
Director of Inclusive Education (82.5 hours × £43.99)	£3,629
Senior Leaders (various hours × rates)	£2,173
Intervention Supervisor (82.5 hours × £18.48)	£1,525
Subtotal: School staff	**£18,479**
TISUK delivery costs	
Training and consultancy	£13,400
Materials supplied	£687
Subtotal: TISUK	**£14,087**
Total cost per school	**£32,566**
Cost per student (÷ 166)	**£196**
Cost per student whole-school (÷ 1,000)	**£33**

Summary

Implementing TISUK costs approximately £32,566 per school. This comprises £14,087 in TISUK fees and £18,479 in school staff time. The per-pupil cost ranges from £33 (whole-school denominator) to £196 (Year 8 cohort denominator), depending on how reach is defined.

These costs are predominantly one-off. The training equips school staff to deliver trauma-informed approaches on an ongoing basis. Subsequent cohorts of pupils benefit without requiring additional TISUK fees, though schools may incur ongoing staff time for delivery. We were unable to estimate recurring costs separately because our data did not distinguish between training and ongoing delivery time.

For commissioners, the key consideration is that TISUK requires significant initial investment in staff training. After this investment, schools have the internal capacity to sustain the approach, though the ongoing staff time commitment will depend on the school's implementation choices.

Conclusion

Key conclusions

Table 41. Key conclusions

Key conclusions
TISUK had a small positive impact on children's externalising behaviours. After the programme, children who received TISUK had lower levels of externalising behaviours compared to those who did not receive TISUK. This result has a **very low** security rating.
TISUK had mixed results on secondary outcomes: a small positive impact on children's internalising behaviours, emotional and behavioural difficulties and psychological distress; no effect on prosocial behaviours or wellbeing; and a small negative impact on school connectedness. TISUK had a large positive impact on staff attitudes towards trauma-informed care and on staff wellbeing. These are secondary outcomes and should be interpreted with caution. There is also statistical uncertainty regarding almost all outcomes.
These findings have a very low security rating because the trial suffered from a very high level of attrition. 27% of the schools and 69% of the children who started the trial were not included in the final analysis. All impact findings should therefore be treated with caution.
Attendance at TISUK training was generally high, although some schools reported difficulties sustaining consistent participation due to operational pressures, lengthy sessions and staff turnover. Staff valued the programme and reported increased confidence. Perceived benefits included a shift from punitive approaches towards more relational and restorative practices, reported reductions in exclusions and improvements in behaviour.
The evaluation found evidence of positive effects on staff outcomes, but this did not translate into measurable improvements in pupil outcomes within the planned timeframe.

Impact evaluation and implementation and process evaluation integration

Evidence to support the logic model

Evidence from both the implementation and impact evaluations confirms that the programme successfully translated its activities into the intended outputs, in line with the ToC assumptions. However, the pathway from the outputs to pupil-level outcomes was not supported.

Activities-to-outputs pathway

In schools with strong engagement, staff integrated trauma-informed approaches into everyday practices. Participants reported policy and practice changes, such as removing isolation booths, replacing punitive systems with restorative approaches and developing new relationship policies using trauma-informed language. These changes demonstrate how training inputs led to organisational and cultural outputs in some settings.

In some schools, staff behaviours and school policy now reflect these changes. Teachers demonstrate greater empathy and shared language around trauma and psychological safety.

However, implementation varied across schools. Where senior leadership was actively involved, changes were more embedded and widespread. Where pastoral staff led the implementation without consistent senior backing, reach was more limited. The ToC's success depended on ownership from school leadership, manageable implementation expectations and sustained support throughout the change process.

Outputs to short-term outcomes

Evidence of staff attitude shifts was found in both the impact evaluation and IPE. The ARTIC total score showed a sizeable effect in the beneficial direction (Hedges' g = 1.24, 95% CI: 0.05 to 2.42, p = 0.041), indicating an increased awareness of childhood adversity and improved capacity to manage dysregulated children. However, this finding should be interpreted cautiously, given differential attrition in staff survey responses across the trial arms.

IPE findings support these quantitative results. Pupils and teachers alike noted that teachers had become more empathetic, suggesting the internalisation of trauma-informed principles. Staff reported responding to pupils with reflective understanding, moving from punitive to relational approaches. Teachers described greater confidence in managing challenging behaviours through emotional attunement.

These findings provide evidence that the programme generated its intended immediate outcomes in schools with sustained engagement: enhanced empathy, reflective practice and the integration of psychological thinking into staff–pupil relationships. These changes contributed to broader cultural shifts in participating schools, though the depth of change varied with levels of senior leadership involvement.

Short-term outcomes to impacts

The quantitative impact evaluation found limited effects on pupil outcomes. Externalising behaviour, the primary outcome, showed a small positive but statistically non-significant difference between the intervention and control groups (g = −0.02). Most secondary pupil outcomes followed a similar pattern, with effect sizes close to zero across wellbeing, psychological distress and school connectedness measures. The administrative data on absences, exclusions and suspensions showed mixed patterns, though small sample sizes and wide CIs limit interpretation.

One exception was internalising behaviour, which showed a statistically significant effect (g = −0.08, p = 0.046). This suggests the programme may have had a small positive effect on emotional and peer problems. However, this should be interpreted cautiously. The effect size is similar in magnitude to other secondary outcomes and falls within the low-impact range under the YEF's classification. Differential attrition between the trial arms introduces some uncertainty, although the analysis found no association between attrition and pupil-level socio-demographic characteristics.

Qualitative findings from the IPE present a more positive picture. Some schools reported improved pupil behaviour and overall climate as well as reductions in suspensions and exclusions. Pupils reported feeling more heard and valued, attributing this to teachers' changed communication styles. Staff described shifts from punitive discipline towards more supportive approaches.

This disconnect between the quantitative and qualitative findings may reflect the intervention's timeline. The ToC assumes a causal chain from staff training to attitude change to practice change to pupil outcomes. Evidence supports the early links in this chain. Staff attitudes shifted, and practices changed in many schools. However, the 12–15 month follow-up period may have been insufficient for these changes to translate into measurable pupil-level differences. Implementation timelines varied: Some schools had only 12 months, and targeted support in some cases began only in Autumn 2024. Whilst this timeframe was deemed viable at the study's outset, whole-school cultural change is a gradual process.

Implementation challenges that undermined assumptions

The logic model was not formally revised during the evaluation. However, the findings identified critical implementation challenges that undermined some of the assumptions, likely diminishing the programme's potential impact in this trial. These challenges would merit further consideration by TISUK to improve programme delivery and participant engagement.

Stakeholder engagement

The evaluation confirmed the importance of Assumption 2, which states that all relevant stakeholders would be actively engaged. However, implementation challenges prevented this assumption from being fully met.

The funding timeline necessitated rapid recruitment for the trial. This limited the opportunity to ensure leadership buy-in from the outset. Engagement varied widely across roles and schools. In some settings, the initiative was driven bottom-up rather than top-down, with limited senior leadership ownership. In one case, the lack of SLT involvement created tension, as champions struggled to secure institutional backing. Pastoral staff were often more committed and knowledgeable, meaning implementation relied on individual enthusiasm rather than systemic adoption.

These findings suggest that effective implementation requires stronger leadership endorsement from the start. Differentiated communication strategies may also help secure consistent buy-in across staff groups.

Delivery in intended doses

The evaluation validated the importance of Assumption 3, which assumes that all programme activities must be delivered at the frequency and dosage required to achieve the intended outcomes. However, implementation challenges prevented this assumption from being realised.

TISUK delivered sessions as scheduled, and participants rated the training quality positively. However, some schools faced challenges in engaging with the content. Participants found the volume of material overwhelming, making it difficult to absorb and to apply their learning. Missed sessions led to fragmented experiences and weaker cohesion amongst trained staff. Financial pressures and workload demands within schools hindered consistent attendance and deep engagement. Although formal dosage requirements were met, the training's intensity and format may not be sustainable within standard school operations without additional capacity.

Reach to intended recipients

The evaluation also highlights the importance of meeting Assumption 4 of the ToC, which expects that the programme will effectively reach and engage its intended recipients.

Participant selection for the Diploma training sometimes prioritised seniority over relevance, limiting direct application with pupils. Staff turnover further undermined reach and continuity. Trained staff left, and new staff joined without training, weakening schools' internal capacities. These findings reveal a gap between the ToC assumptions and the realities of implementation. Future iterations should mitigate turnover effects through internal champions, refresher training and induction resources to preserve fidelity and impact over time.

Missing elements: concurrent interventions and equity considerations

Concurrent interventions

The trial estimated the impact of TISUK compared to BAU. BAU is not an empty condition, where schools do nothing. Schools in both arms implemented behavioural and wellbeing programmes during the trial period. Intervention schools used approaches such as Paul Dix's When the Adults Change, PACE, ELSA (Emotional Literacy Support Assistant) and TISUK. Monitoring of control schools indicated they also implemented similar initiatives.

This context is important for interpreting the findings. The trial measures the additional benefit of TISUK beyond what schools typically provide. TISUK is a distinct whole-school approach, and the RCT design allows for valid causal inference about its effects relative to standard practices. However, if BAU already includes substantial wellbeing provisions, the incremental effect of any new programme may be smaller. Future evaluations should systematically document concurrent initiatives in both arms to aid interpretation.

Reductions in sanctions reported in the IPE align with ToC expectations. The low impact on internalising behaviours found in the impact evaluation also points in a beneficial direction. However, these changes occurred in a context where schools were already active in this space.

Equity considerations

The current ToC lacks an equity lens. The IPE identified a perceived gap in addressing racial trauma and cultural responsiveness. While designed for general trauma, some school staff felt the training was insufficiently equipped to recognise intersections with race and systemic inequality.

The impact evaluation found no differential effects by ethnicity, sex or FSM eligibility. However, qualitative insights highlighted the need to integrate equity considerations into the framework, including racial identity, gender, deprivation, SEND and belonging. Incorporating these dimensions would enhance inclusivity and ensure TIP reflects the diverse lived experiences of pupils and staff.

Interpretation

Situating the findings within existing evidence and policy contexts

The findings both support and complicate existing evidence on TIP in schools. Even comprehensive, high-quality training capable of shifting staff attitudes ($g = 1.24$) translated into only small, statistically non-significant improvements in externalising behaviours ($g = -0.02$).

The theoretical basis for expecting changes in externalising behaviours warrants consideration. The primary outcome was selected to align with the YEF's focus on youth violence prevention. However, it is possible that TIP primarily influences internalising rather than externalising behaviours. The low impact on internalising behaviours ($g = -0.08$) found in this evaluation, while modest, may support this interpretation.

TISUK was implemented as a whole-organisation strategy involving leadership training, staff development, consultancy support and supervision. This went beyond training in isolation. However, achieving genuine organisational shifts proved difficult without consistent leadership engagement and protected time for staff. Even with these structural elements in place, changes in staff practices did not produce rapid improvements in pupil outcomes.

The null findings on most pupil outcomes challenge optimistic narratives positioning TIP as a solution to the school-to-prison pipeline (Gill et al., 2017; IPPR, 2017). The established evidence on ACEs (Baglivio et al., 2020; Felitti et al., 1998) and their neurobiological effects (Blankenstein et al., 2022; McCrory et al., 2011)

remains unchallenged. However, this evaluation raises questions about whether school-based interventions can disrupt these pathways within evaluation-imposed timeframes. The 15-month follow-up may be insufficient to translate whole-school cultural change into pupil-level effects. These findings should also be interpreted with some uncertainty, given differential attrition between the trial arms.

The study offers the first robust RCT evidence that whole-school trauma-informed training can shift educator attitudes and practices without producing measurable benefits for most pupil outcomes within 15 months. The exception is internalising behaviours, which show a low impact in the beneficial direction. This finding is relevant to policy expectations for school-based mental health support.

These findings challenge policy expectations – such as those in the UK Government's Green Paper on Children and Young People's Mental Health (Department of Health and Social Care & DfE, 2018) – that adequately trained staff can directly address mental health needs in the short term. The evidence suggests TIP should be reframed as a long-term cultural reform requiring sustained investment, rather than a short-term intervention yielding rapid returns. Further efficacy-level evaluation is needed, alongside development of the ToC, to clarify expected pathways and timeframes.

The central paradox and the implementation gap

This evaluation presents a complex picture. Staff transformation was substantial, yet most pupil outcomes showed no measurable improvements within the trial timeframe.

Staff attitudes towards TIC showed a large effect ($g = 1.24$, $p = 0.041$). Qualitative findings confirmed a greater awareness of trauma and shifts from punitive to relational approaches. However, the primary outcome showed a marginal, statistically non-significant effect on externalising behaviours ($g = -0.02$, $p = 0.64$). Only internalising behaviours showed a statistically significant effect ($g = -0.08$, $p = 0.046$).

This disconnect – staff change without substantial pupil benefit – may reflect the limits of short-term measurement. The ToC assumes effects cascade from training to attitudes to practice to pupil outcomes. Evidence supports the early links, but the final link requires changed practice to reach pupils consistently over time.

The IPE findings reveal four implementation challenges that may explain why staff transformation did not translate into measurable pupil-level change:

1. Variable whole-school embedding: In schools lacking senior leadership support, TIP remained optional and peripheral rather than central to school culture.
2. Incomplete staff coverage and role misalignment: Engagement varied widely. Pastoral teams were enthusiastic, but many teachers resisted due to workload or scepticism. In some cases, those trained were not those working most directly with pupils.
3. Time and relationship continuity: Trauma-informed approaches rely on stable, trusting relationships built over time. Staff turnover and the fragmented attendance of training cohorts disrupted this process.
4. Systemic transformation timeline: Meaningful cultural change requires sustained effort. Even the most advanced schools described themselves as being at an early stage. This illustrates that whole-school transformation extends well beyond a 15-month evaluation window. Change must cascade through multiple levels – from leadership to staff attitudes to classroom practice to pupil experience – before effects on pupil outcomes can be expected.

Compliance and engagement effects

The CACE analysis explored whether higher programme engagement was associated with a greater impact on the primary outcome. Results found no significant association between compliance and pupil outcomes. Schools with higher engagement scores did not show meaningfully different effects compared to those with lower engagement.

This finding should be interpreted cautiously. Most schools achieved relatively high engagement scores (mean 78%), limiting variation in the sample. The small number of schools with lower engagement reduces statistical power to detect compliance effects. Nevertheless, the absence of a clear dose–response relationship raises questions about whether more intensive implementation would yield different results or whether the intervention's ToC requires reconsideration.

The targeted support paradox

The quasi-experimental analysis found that pupils who reported receiving targeted support showed worse outcomes than matched controls across several domains. However, substantial methodological limitations mean these findings should be interpreted with considerable caution.

Targeted pupils were identified through self-reported survey responses, which could not be validated against school records. We cannot confirm whether the reported support was delivered by TISUK-trained Diploma practitioners or through other school provisions. Selection approaches varied across schools, and PSM could not account for pupils' trajectories prior to identification.

The most plausible explanation for the observed pattern is selection bias. Schools likely identified pupils who were already on declining trajectories with complex needs. A sensitivity analysis (Rosenbaum bounds $\Gamma = 1.3$) confirmed that a moderate hidden bias could explain the observed differences.

Given these limitations, we cannot draw firm conclusions about the effectiveness of TISUK's targeted support component. Future evaluations would benefit from the prospective identification of pupils, validated records of who delivered support and earlier baseline measurements to capture trajectories.

Racial and ethnic disparities: the limits of universal approaches

Subgroup analyses found no differential effects across demographic groups. Effect sizes for pupils from minority ethnic backgrounds ranged from −0.13 to −0.01, similar to those for White pupils. This consistency in the quantitative findings does not necessarily indicate that the intervention was equally relevant to all groups.

The IPE identified perceived gaps in addressing racial trauma and cultural responsiveness. The programme included a webinar on racial discrimination, but some staff members felt that it was insufficient. Staff members from minority ethnic backgrounds reported lacking the tools to support pupils experiencing racial trauma. This raises questions about the depth of the curriculum in this area and about how all staff – regardless of background – can develop an understanding of the racial stress affecting pupils.

Three areas emerged from the IPE as warranting further development:

1. Curriculum content: While some material addressed racial discrimination, staff perceived it as limited. Practitioners sought more conceptual tools to address racialised harm.

2. Workforce preparedness: Staff from minority ethnic backgrounds felt under-equipped despite their lived experiences. This suggests a need for deeper engagement with racial trauma across all staff groups.
3. Intersectional awareness: The IPE highlighted opportunities to improve the training framework's approach to the intersections of race, class, gender and SEND.

It is important to acknowledge the limits of the evidence here. The evaluation included a diversity lens and sought to recruit pupils from minority ethnic backgrounds for qualitative data collection. However, recruitment did not achieve sufficient representation from these groups. The quantitative subgroup analysis showed no differential effects, but this does not confirm equal relevance. Without direct input from pupils from diverse backgrounds, we cannot draw firm conclusions about whether the intervention addressed their specific experiences and needs.

Limitations and lessons learned

This evaluation provides valuable evidence on the implementation and impact of the TISUK programme; however, several methodological and practical limitations should be acknowledged. These span the impact evaluation and IPE and have implications for both the interpretation and generalisability of results.

Impact evaluation: overall design

Attrition bias: Four schools withdrew after randomisation, all from the intervention arm. Some reported workload pressures. A pupil-level attrition analysis found no association between dropout and socio-demographic characteristics. However, differential attrition between the arms introduces uncertainty that cannot be fully ruled out.

Beyond school dropout, substantial within-school attrition occurred. Many pupils lacked matched baseline and follow-up data, reducing the analytical sample. One contributing factor was reliance on self-reported identifiers (name, date of birth) to link records across time points. Incomplete or inconsistent reporting led to lost matches. Future evaluations face a trade-off here. Administrative identifiers, such as unique pupil numbers, would improve matching accuracy but would require additional data-sharing agreements and increase the burden on schools. Early planning for data linkage, including consideration of National Pupil Database (NPD) access or platforms such as Wonde, could help balance these concerns. Engaging better with schools about data collection timing and avoiding busy periods could also improve retention.

Performance bias: Engagement and fidelity varied across schools and staff roles. Pastoral staff participated more actively than teaching staff or senior leaders. This variability may have introduced differences unrelated to the intervention itself. Future evaluations should monitor engagement systematically across all staff groups.

BAU context: Schools in both trial arms implemented wellbeing and behavioural programmes during the study period, including approaches such as Paul Dix's When the Adults Change, PACE and ELSA. This reflects the reality of school-based trials. BAU is never an empty condition, where schools do nothing. Schools continuously adopt initiatives to support pupil wellbeing and behaviour. The trial therefore estimates the additional effect of TISUK over and above standard practices, not compared to inactivity. If BAU already includes substantial provisions, the incremental effect of any new programme may be smaller. Future trials

should systematically document concurrent initiatives in both arms to aid interpretation while recognising that active comparison conditions are the norm in school settings.

Measurement bias: Self-reported data from teachers and pupils introduce potential for social desirability bias. Future studies could incorporate observational measures or administrative data to triangulate self-reports.

Multiplicity of analyses: The evaluation included a large number of outcome measures, each addressing a distinct research question. While this did not require analytical corrections for multiple testing, the breadth of outcomes increased the data collection burden on schools and participants. Future evaluations should consider prioritising a smaller set of theoretically central indicators to reduce respondent burden while maintaining a focus on key mechanisms in the ToC.

Administrative data collection: Obtaining school-level administrative data proved challenging. Schools reported data-sharing burdens and time constraints, limiting both coverage and consistency. Integration through the NPD or platforms such as Wonde was considered during the co-design phase, but project timelines made this infeasible. Future evaluations should build NPD access into project planning from the outset, as this would reduce the burden on schools and improve data completeness.

Poor quality of administrative data collected: The administrative data analysis was constrained by three factors. First, school coverage was limited, ranging from 25 to 39 schools, depending on the outcome. Second, data quality varied substantially despite the provision of standardised templates. Third, schools inconsistently reported measures, requiring extensive cleaning. These constraints mean that the administrative findings should be interpreted with considerable caution. Future trials should integrate NPD data from the outset to ensure consistency and reduce the burden on schools. Alternatively, platforms such as Wonde could automatically collect school data, reducing the burden whilst improving consistency. Both approaches would strengthen data quality and reduce demands on participating schools, though evaluators should ensure any third-party data collection aligns with the trial's analytical requirements.

Limitations of the QED: The QED analysis relied on self-reported survey data to identify pupils receiving targeted support. This approach was specified in the original protocol but carried important limitations. Pupils may have misattributed other school-based support as the TISUK intervention, failed to recognise support from Diploma-trained practitioners or experienced recall errors. We could not validate responses against school records due to incomplete data returns. The analysis, therefore, estimates the effect of self-reported targeted support, which may not precisely capture the TISUK Diploma practitioner intervention.

Implementation and process evaluation

The IPE also presented several methodological challenges.

Sample representativeness: Engagement with IPE activities varied across schools and staff roles. Those who participated may not be representative of all staff experiences. Pastoral staff and those most engaged with TISUK were more likely to participate in the interviews and focus groups, potentially skewing the findings towards more positive accounts.

Limited youth participation: The process evaluation captured young people's voices across five case study schools. However, it included a limited number of pupils from minority ethnic backgrounds, restricting our

understanding of how these pupils experienced trauma-informed approaches. The evaluation sought to recruit diverse participants but did not achieve sufficient representation.

Targeted intervention data gaps: There was insufficient exploration of targeted support sessions within schools. The interview and survey instruments did not include specific items to assess this component. The young people receiving targeted support were not specifically sampled as part of the IPE.

Overburdened participants: Staff members repeatedly described heavy workloads and limited capacity to engage with evaluation activities. This affected the quality of qualitative data collection and may have constrained reflection in focus groups or surveys. These time pressures also reflect broader systemic obstacles to teacher-led change initiatives.

Training intensity and engagement: Staff members found the volume of training content challenging to absorb alongside their existing responsibilities. The CACE analysis explored whether higher engagement was associated with greater impact. The results suggest that compliance was not significantly associated with outcomes, though interpretation is limited by the small number of schools with lower engagement scores.

Future research and publications

This evaluation highlights several areas requiring further investigation.

ToC development: The theoretical and empirical links between TIP and specific pupil outcomes remain underdeveloped. Future research should clarify the mechanisms through which TIP is expected to affect externalising behaviours, internalising behaviours and wellbeing. This would strengthen outcome selection and interpretation in future trials.

Extended follow-up: The most pressing research priority is to extend measurement beyond 15 months. Whole-school cultural change may take longer to translate into pupil-level outcomes. Future evaluations should include follow-up at two to three years to capture the full trajectory of change. This extended timeframe would also allow for subgroup analyses with greater statistical power.

Racial and ethnic equity: Future research should explicitly investigate how trauma-informed interventions work for different racial and ethnic groups. Key questions include how pupils from minority ethnic backgrounds experience these approaches when racial trauma is not directly addressed, what adaptations are necessary for cultural responsiveness and whether practitioner–pupil ethnic matching affects outcomes. Research methodologies should centre racial equity, potentially through participatory approaches involving minority ethnic communities.

Targeted support implementation: The quasi-experimental findings call for further investigation into targeted support delivery. Future research should examine optimal models for embedding targeted support within universal frameworks, strategies for maintaining relationship continuity despite staff turnover and methods for distinguishing selection effects from intervention effects through multiple baseline measurements.

Design considerations: Future evaluations could consider stepped-wedge or wait-list designs where practical constraints prevent standard randomisation. These designs would allow all schools to eventually receive the intervention while maintaining a comparison condition. However, they require careful planning

and extended timelines. Any future trial should prioritise pupil-level outcomes as the primary measure of success while also capturing implementation quality to understand variations in effects across settings.

Intersectionality: Future research should examine how pupils with intersecting characteristics – including ethnicity, SEND and socioeconomic disadvantage – experience whole-school approaches differently. Understanding what mechanisms translate cultural change into individual outcomes across subgroups remains an unknown.

References

Abadie, A. and Imbens, G.W., 2016. Matching on the estimated propensity score. Econometrica, 84(2), pp.781-807.

Abadie, A. and Imbens, G.W., 2006. Large sample properties of matching estimators for average treatment effects. E*conometrica*, *74*(1), pp.235-267.

Bacher-Hicks, A., Billings, S. B., & Deming, D. J. (2024). The School-to-Prison Pipeline: Long-Run Impacts of School Suspensions on Adult Crime. American Economic Journal: Economic Policy, 16(4), 165-193.

Baglivio, M. T., Wolff, K. T., DeLisi, M., & Jackowski, K. (2020). The role of adverse childhood experiences (ACEs) and psychopathic features on juvenile offending criminal careers to age 18. Youth Violence and Juvenile Justice, 18(4), 337-364.

Basto-Pereira, M., Gouveia-Pereira, M., Pereira, C. R., Barrett, E. L., Lawler, S., Newton, N., ... & Sakulku, J. (2022). The global impact of adverse childhood experiences on criminal behavior: A cross-continental study. Child abuse & neglect, 124, 105459.

Becker-Blease, K. A. (2017). As the world becomes trauma-informed, work to do. Journal of Trauma Dissociation, 18, 131–138.

Blankenstein, N. E., Vandenbroucke, A. R., de Vries, R., Swaab, H., Popma, A., & Jansen, L. (2022). Understanding aggression in adolescence by studying the neurobiological stress system: A systematic review. Motivation Science, 8(2), 133.

Bruhn, M., & McKenzie, D. (2009). In pursuit of balance: Randomization in practice in development field experiments. American economic journal: applied economics, 1(4), 200-232.

Department of Health and Social Care, & Department for Education. (2018). Transforming Children and Young People's Mental Health Provision: A Green Paper. Retrieved from London: Policy document:

Diggins, J. (2021). Reductions in behavioural and emotional difficulties from a specialist, trauma-informed school. *Educational and Developmental Psychologist*, *38*(2), 194-205.

Felitti, V. J., Anda, R. F., Nordenberg, D., Williamson, D. F., Spitz, A. M., Edwards, V., & Marks, J. S. (1998). Relationship of childhood abuse and household dysfunction to many of the leading causes of death in adults: The Adverse Childhood Experiences (ACE) Study. American journal of preventive medicine, 14(4), 245-258.

Foulkes L, Stringaris A. Do no harm: can school mental health interventions cause iatrogenic harm? BJPsych Bulletin. 2023;47(5):267-269. doi:10.1192/bjb.2023.9

Fujiwara, T. (2022). Impact of adverse childhood experience on physical and mental health: A life-course epidemiology perspective. Psychiatry and clinical neurosciences, 76(11), 544-551.

Giampetruzzi, E., Tan, A. C., LoPilato, A., Kitay, B., Posse, P. R., McDonald, W. M., ... & Hershenberg, R. (2023). The impact of adverse childhood experiences on adult depression severity and treatment outcomes. Journal of affective disorders, 333, 233-239.

Giano, Zachary, et al. "Intersectionality and adverse childhood experiences: Comparing subgroups of sex, race/ethnicity, and sexual orientation." American Journal of Preventive Medicine 65.1 (2023): 30-38.

Gill, K., Quilter-Pinner, H., & Swift, D. (2017). Making the difference: Breaking the link between school exclusion and social exclusion. Institute for Public Policy Research.

Gilmour L, Best C, Duncan E, Maxwell M. Characteristics and outcomes of referrals to CAMHS for children who are thinking about or attempted suicide: A retrospective cohort study in two Scottish CAMHS. Front Psychiatry. 2022 Sep 1;13:914479. doi: 10.3389/fpsyt.2022.914479. PMID: 36117645; PMCID: PMC9477378.

Goodman, R., 2001. Psychometric properties of the strengths and difficulties questionnaire. *Journal of the American Academy of Child & Adolescent Psychiatry*, *40*(11), pp.1337-1345.

Gorard, S., 2012. Who is eligible for free school meals? Characterising free school meals as a measure of disadvantage in England. British Educational Research Journal, 38(6), pp.1003-1017.

GOV.UK (2023) 'Population of England and Wales', Ethnicity facts and figures, Available at: https://www.ethnicity-facts-figures.service.gov.uk/uk-population-by-ethnicity/national-and-regional-populations/population-of-england-and-wales/latest/

GOV.UK (2023) 'Male and female populations', Ethnicity facts and figures, Available at: https://www.ethnicity-facts-figures.service.gov.uk/uk-population-by-ethnicity/demographics/male-and-female-populations/latest/

GOV.UK (2023) 'School teacher workforce', Ethnicity facts and figures, Available at: https://www.ethnicity-facts-figures.service.gov.uk/workforce-and-business/workforce-diversity/school-teacher-workforce/latest/

Hatch, S. L., & Dohrenwend, B. P. (2007). Distribution of traumatic and other stressful life events by race/ethnicity, gender, SES and age: A review of the research. American Journal of Community Psychology, 40, 313–332.

Høidal, A., & Hanssen, N. (2022). The Norwegian prison system: Halden prison and beyond: Taylor & Francis.

Humphrey, N., Lendrum, A., Ashworth, E., Frearson, K., Buck, R., & Kerr, K. (2016). Implementation and process evaluation (IPE) for interventions in education settings: An introductory handbook. Education Endowment Foundation, 1.

Ijaz, S., Nobles, J., Mamluk, L., Dawson, S., Curran, B., Pryor, R., ... & Savović, J. (2024). Disciplinary behaviour management strategies in schools and their impact on student psychosocial outcomes: A systematic review. NIHR Open Research, 4, 13.

IPPR. (2017). Half of expelled pupils suffer mental health issues in 'burningly injust system', think-tank finds. Retrieved from https://www.ippr.org/news-and-media/pressreleases/half-of-expelled-pupils-suffer-mental-health-issues-in-burningly-injustsystem-think-tank-finds

Jo, B., Asparouhov, T., Muthén, B.O., Ialongo, N.S. and Brown, C.H., 2008. Cluster randomized trials with treatment noncompliance. Psychological methods, 13(1), p.1.

Lang, J. M. , Campbell, K. , & Vanderploeg, J. J. (2015). Advancing trauma-informed systems for children. Farmington, CT: Child Health and Development Institute.

Le, H., Abdinasir, K., & Rainer, C. (2023). Behaviour and mental health in schools. Retrieved from

Li, S. M., Zhang, C., Bi, K., & Chen, M. S. (2023). Longitudinal impacts of adverse childhood experiences on multidimensional health outcomes: Predicting trajectories in mental, physical, and behavioral health. Child Abuse & Neglect, 106543.

Lloyd, D. A., & Turner, R. J. (2008). Cumulative lifetime adversities and alcohol dependence in adolescence and young adulthood. Drug and Alcohol Dependence, 93, 217–226.

MacLochlainn, J., Kirby, K., McFadden, P. and Mallett, J. (2022) 'An evaluation of whole-school trauma-informed training intervention among post-primary school personnel', *Journal of Child & Adolescent Trauma*, **15**, pp. 925–941.

Marmot, M., & Brunner, E. (2005). Cohort profile: the Whitehall II study. International journal of epidemiology, 34(2), 251-256.

Martin, K., Dobson, M., Fitzgerald, K., Ford, M., Lund, S., Egeberg, H., ... & Berger, E. (2024). International Trauma-Informed Practice Principles for Schools (ITIPPS): expert consensus of best-practice principles. The Australian Educational Researcher, 51(4), 1445-1468.

McKee, B. E., & Dillenburger, K. (2009). Child abuse and neglect: Training needs of student teachers. *International Journal of Educational Research, 48*(5), 320–330.

McCrory, E. J., De Brito, S. A., Sebastian, C. L., Mechelli, A., Bird, G., Kelly, P. A., & Viding, E. (2011). Heightened neural reactivity to threat in child victims of family violence. Current Biology, 21(23), R947-R948.

McIntyre, G., & Mazza, R. (2020). Justice reinvestment: Building communities not prisons. Brief, 47(1), 24-25. 64

Meeker, E. C., O'Connor, B. C., Kelly, L. M., Hodgeman, D. D., Scheel-Jones, A. H., & Berbary, C. (2021). The impact of adverse childhood experiences on adolescent health risk indicators in a community sample. Psychological trauma: theory, research, practice, and policy, 13(3), 302.

Merrick, M. T., Ford, D. C., Ports, K. A., & Guinn, A. S. (2018). Prevalence of adverse childhood experiences from the 2011-2014 Behavioral Risk Factor Surveillance System in 23 states. JAMA Pediatrics, 172, 1038–1044.

Molloy, K. A. T. M. T. M. D. (2022). Trauma-informed care: Understanding the use of traumainformed approaches within children's social care. Retrieved from

Morgan, K. L., & Rubin, D. B. (2012). Rerandomization to improve covariate balance in experiments.

Neil, L., Viding, E., Armbruster-Genc, D., Lisi, M., Mareschal, I., Rankin, G., . . . Martin, P. (2022). Trust and childhood maltreatment: evidence of bias in appraisal of unfamiliar faces. Journal of child psychology and psychiatry, 63(6), 655-662.

Nguyen, T.L., Collins, G.S., Spence, J., Daurès, J.P., Devereaux, P.J., Landais, P. and Le Manach, Y., 2017. Double-adjustment in propensity score matching analysis: choosing a threshold for considering residual imbalance. BMC medical research methodology, 17(1), p.78.

O'Malley, K. A., Sullivan, J. L., Mills, W., Driver, J., & Moye, J. (2023). Trauma-informed care in long-term care settings: from policy to practice. The Gerontologist, 63(5), 803-811.

ONS. (2023). Schools, pupils and their characteristics - Academic year 2022/23. Retrieved from https://explore-education-statistics.service.gov.uk/find-statistics/school-pupilsand-their-characteristics

Parker, K., Nunns, M., Xiao, Z., Ford, T., & Ukoumunne, O. C. (2021). Characteristics and practices of school-based cluster randomised controlled trials for improving health outcomes in pupils in the United Kingdom: a methodological systematic review. BMC Medical Research Methodology, 21(1), 1-17.

Qu, G., Liu, H., Han, T., Zhang, H., Ma, S., Sun, L., ... & Sun, Y. (2024). Association between adverse childhood experiences and sleep quality, emotional and behavioral problems and academic achievement of children and adolescents. European Child & Adolescent Psychiatry, 33(2), 527-538.

Record-Lemon, R. M., & Buchanan, M. J. (2017). Trauma-Informed Practices in Schools: A Narrative Literature Review. Canadian Journal of Counselling and Psychotherapy, 51(4). Retrieved from https://journalhosting.ucalgary.ca/index.php/rcc/article/view/61156

SAMHSA (Substance Abuse and Mental Health Services Administration) (2014a). SAMHSA's concept of trauma and guidance for a trauma-informed approach (HHS Publication No. SMA 14-4884). Rockville, MD: Substance Abuse and Mental Health Services

Sapolsky, R. M., & Share, L. J. (2004). A pacific culture among wild baboons: its emergence and transmission. PLoS biology, 2(4), e106.

Schochet, P.Z. and Chiang, H.S., 2011. Estimation and identification of the complier average causal effect parameter in education RCTs. *Journal of Educational and Behavioral Statistics*, *36*(3), pp.307-345.

Shackleton, N., Hale, D., Bonell, C., & Viner, R. M. (2016). Intraclass correlation values for adolescent health outcomes in secondary schools in 21 European countries. SSMpopulation health, 2, 217-225.

Schimke, D., Krishnamoorthy, G., Ayre, K., Berger, E., & Rees, B. (2022, June). Multi-tiered culturally responsive behavior support: A qualitative study of trauma-informed education in an Australian primary school. In *Frontiers in Education* (Vol. 7, p. 866266). Frontiers Media SA.

Sinha, P., Calfee, C. S., & Delucchi, K. L. (2021). Practitioner's guide to latent class analysis: methodological considerations and common pitfalls. Critical care medicine, 49(1), e63.

Sitler, H. C. (2009). Teaching with awareness: The hidden effects of trauma on learning. *The Clearing House: A Journal of Educational Strategies, Issues and Ideas, 82*(3), 119–124.

Smith, K., & Davies, J. (2010). Qualitative data analysis. Practical researcher and evaluation: A start-to finish guide for practitioners, 145-158.

Sterne, J.A., White, I.R., Carlin, J.B., Spratt, M., Royston, P., Kenward, M.G., Wood, A.M. and Carpenter, J.R., 2009. Multiple imputation for missing data in epidemiological and clinical research: potential and pitfalls. Bmj, 338.

Stokes, H. (2022, June). Leading trauma-informed education practice as an instructional model for teaching and learning. In *Frontiers in education* (Vol. 7, p. 911328). Frontiers Media SA.

Sun, Y., Blewitt, C., Minson, V., Bajayo, R., Cameron, L., & Skouteris, H. (2024). Trauma-informed interventions in early childhood education and care settings: A scoping review. Trauma, Violence, & Abuse, 25(1), 648-662.

Van Voorhees, E., & Scarpa, A. (2004). The effects of child maltreatment on the hypothalamicpituitary-adrenal axis. Trauma, Violence, & Abuse, 5(4), 333-352.

Watson, K. R., & Astor, R. A. (2025). A critical review of empirical support for trauma-informed approaches in schools and a call for conceptual, empirical and practice integration. Review of Education, 13(1), e70025.

Webster, E. M. (2022). The impact of adverse childhood experiences on health and development in young children. Global pediatric health, 9, 2333794X221078708.

Yohros, A. (2023). Examining the relationship between adverse childhood experiences and juvenile recidivism: A systematic review and meta-analysis. Trauma, Violence, & Abuse, 24(3), 1640-1655.

Young Minds. (2023). End the Wait Campaign. Retrieved from https://www.youngminds.org.uk/support-us/join-the-movement/end-the-wait

Youth Endowment Fund (2025), 'Association between school exclusion, suspension, absence, and violent crime', May, Available at: https://youthendowmentfund.org.uk/wp-content/uploads/2024/12/Association-between-school-exclusion-suspension-absence-and-violent-crime.pdf

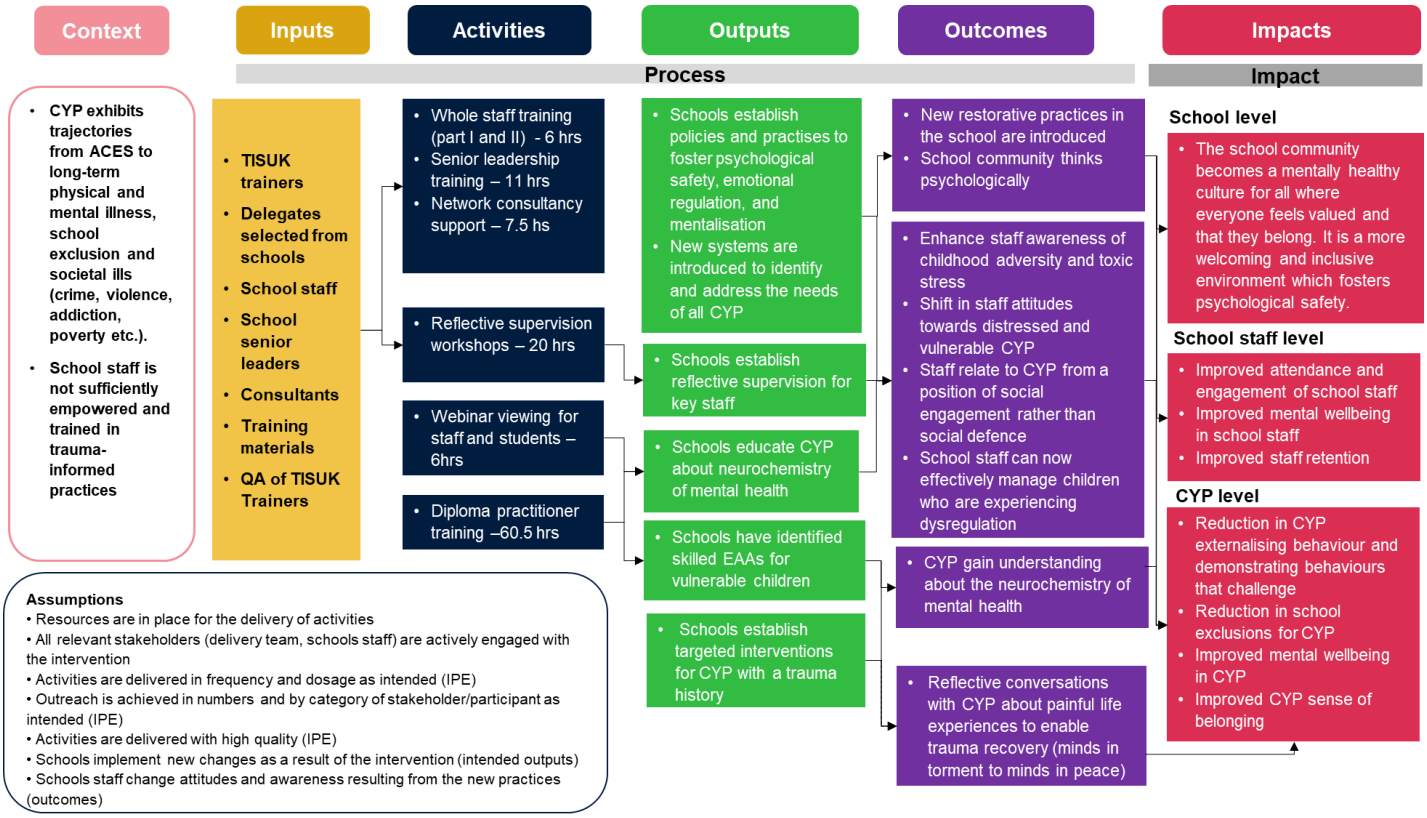
Youth Endowment Fund (2025) 'Adverse and positive childhood experiences and their associations with children's involvement in violence: analyses of data from the Millennium Cohort Study', January, Available at: https://youthendowmentfund.org.uk/wp-content/uploads/2025/01/FinalReport_ACEsPCEs_MCS.pdf

Youth Endowment Fund (2024) 'Positive activities, youth clubs and trusted adults', Available at: https://youthendowmentfund.org.uk/toolkit/positive-activities-youth-clubs-and-trusted-adults/

Youth Endowment Fund (2022) 'YEF SDQ guidance', April, Available at: https://youthendowmentfund.org.uk/wp-content/uploads/2022/04/18.-YEF-SDQ-guidance-April-2022.pdf

Youth Endowment Fund (2021) 'Trauma Informed Care Toolkit, November, Available at: Trauma-informed training and service redesign | Youth Endowment Fund

Appendix 1: Theory of change



Appendix 2: Effect size estimation and histograms

Appendix table 1: Effect size estimation

			Intervention group		Control group			
Outcome	**Unadjusted differences in means**	**Adjusted differences in means**	**n (missing)**	**Variance of outcome**	**n (missing)**	**Variance of outcome**	**Pooled variance**	**Population variance (if applicable)**
Externalising behaviour (primary)	0.06	-0.07	2314 (0)	15.56	1371 (0)	15.40	15.51	0.35
Internalising behaviour	-0.22	-0.31	2314 (0)	13.38	1368 (3)	13.85	13.57	0.50
Prosocial behaviour	-0.03	-0.03	2314 (0)	3.90	1371 (0)	3.88	3.90	0.11
Total difficulties	-0.16	-0.39	2314 (0)	42.74	1368 (3)	41.79	42.42	1.50
School connectedness	-0.03	-0.01	2251 (49)	0.10	1338 (28)	0.09	0.10	0.00
Wellbeing (SWEMWBS)	-0.17	0.06	2212 (76)	27.35	1320 (44)	28.70	27.83	0.43
Psychological distress (GHQ-12)	-0.27	-0.30	2314 (0)	44.22	1371 (0)	46.86	45.28	1.40

Distribution of outcome measures

This appendix presents histograms showing the distribution of outcome measures at baseline and follow-up, by trial arm. The distributions inform assessment of floor and ceiling effects, skewness, and suitability for multilevel modelling.

Pupil survey outcomes

The SDQ-based measures (externalising behaviour, internalising behaviour, prosocial behaviour, and total difficulties) show approximately normal distributions with slight positive skew. This pattern is typical for general population samples where most pupils report low to moderate difficulties. Distributions are similar across trial arms at both timepoints, with no notable floor or ceiling effects. Scores spread across the full range of each scale, supporting the suitability of multilevel linear models.

Psychological distress (GHQ-12) and wellbeing (SWEMWBS) show similar patterns, with approximately normal distributions and no evidence of problematic skew or clustering at scale boundaries. School connectedness shows a narrower distribution concentrated in the mid-range of the scale but remains suitable for analysis.

Figure A2 1: Externalising behaviour

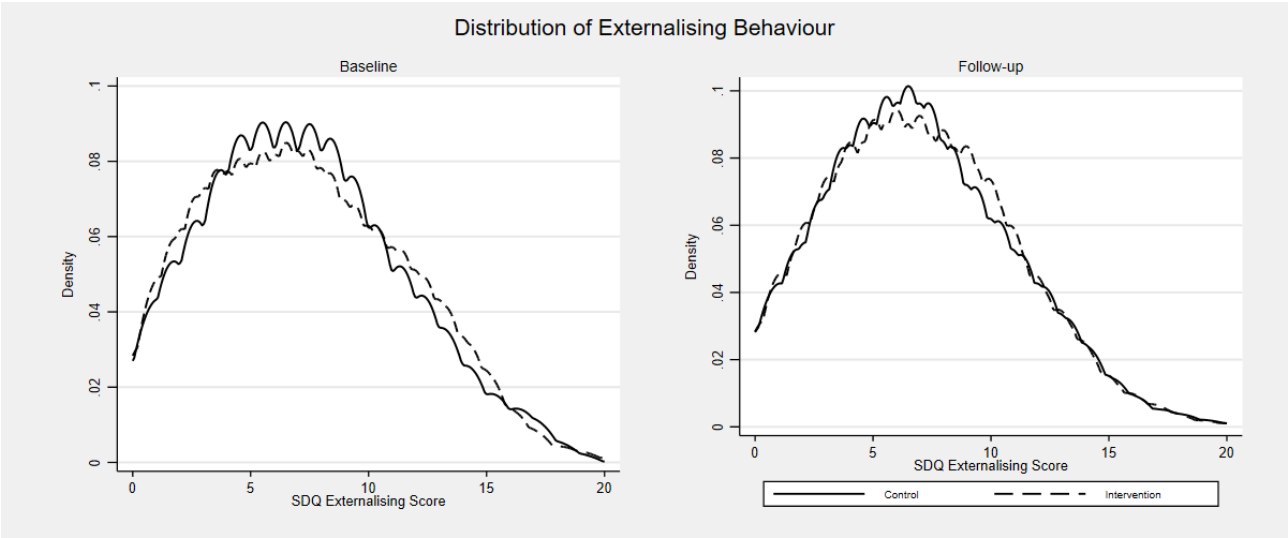


Figure A2 2: Internalising behaviour

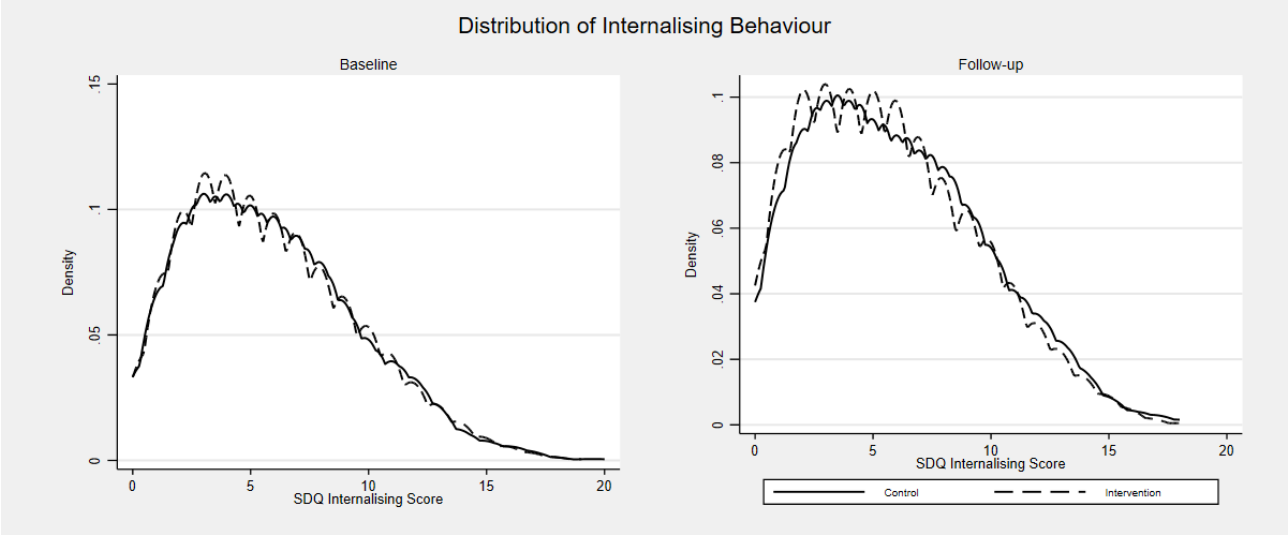


Figure A2 3: Prosocial behaviour

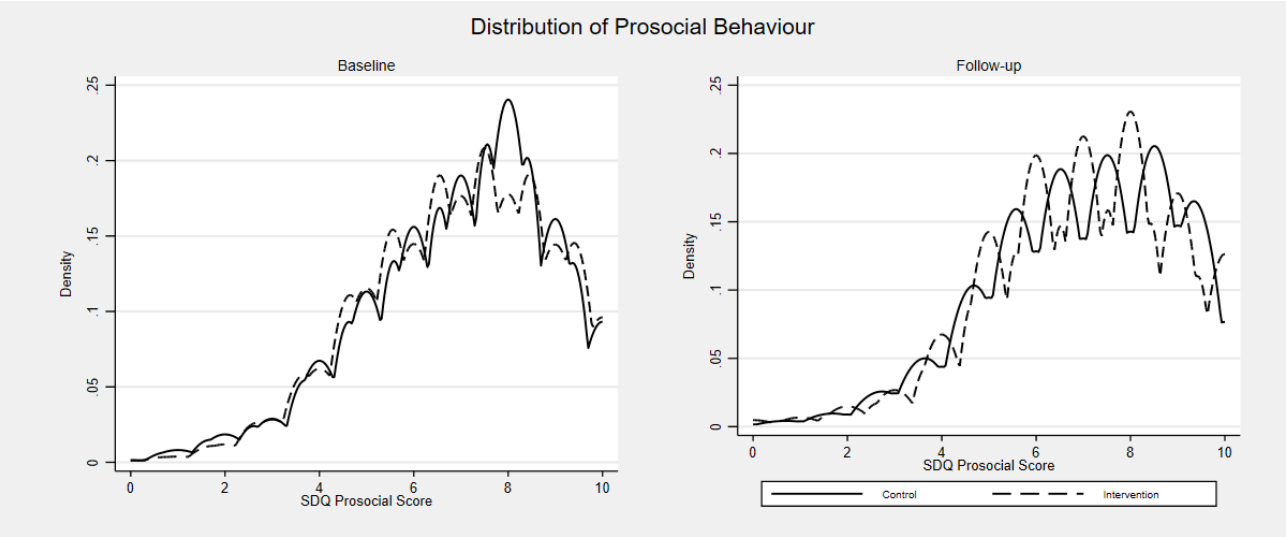


Figure A2 4: Total difficulties

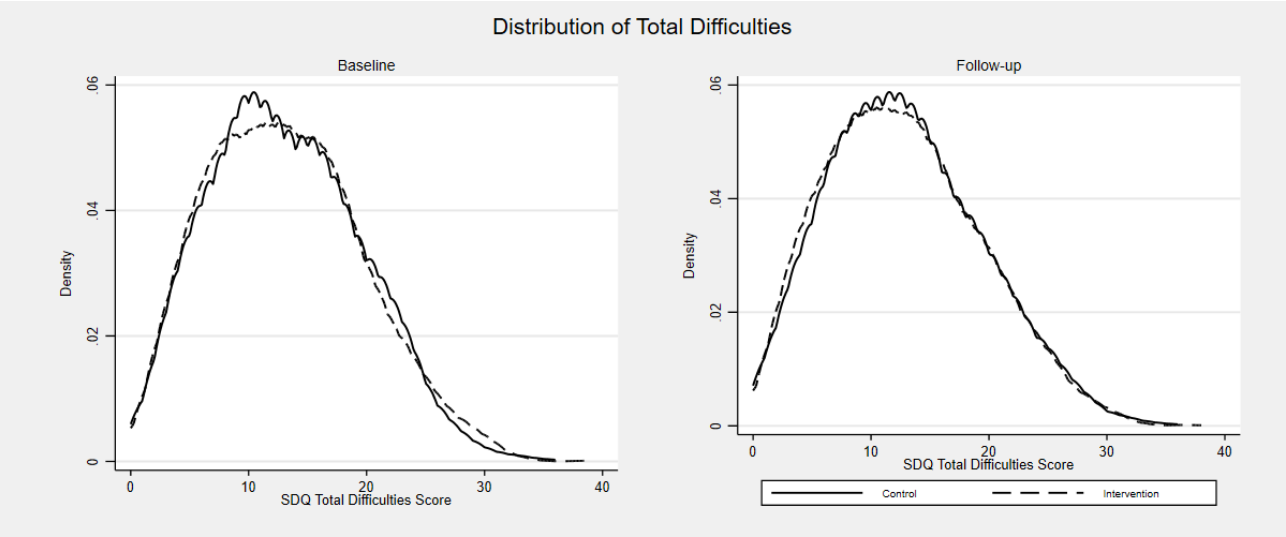


Figure A2 5: Psychological distress (GHQ-12)

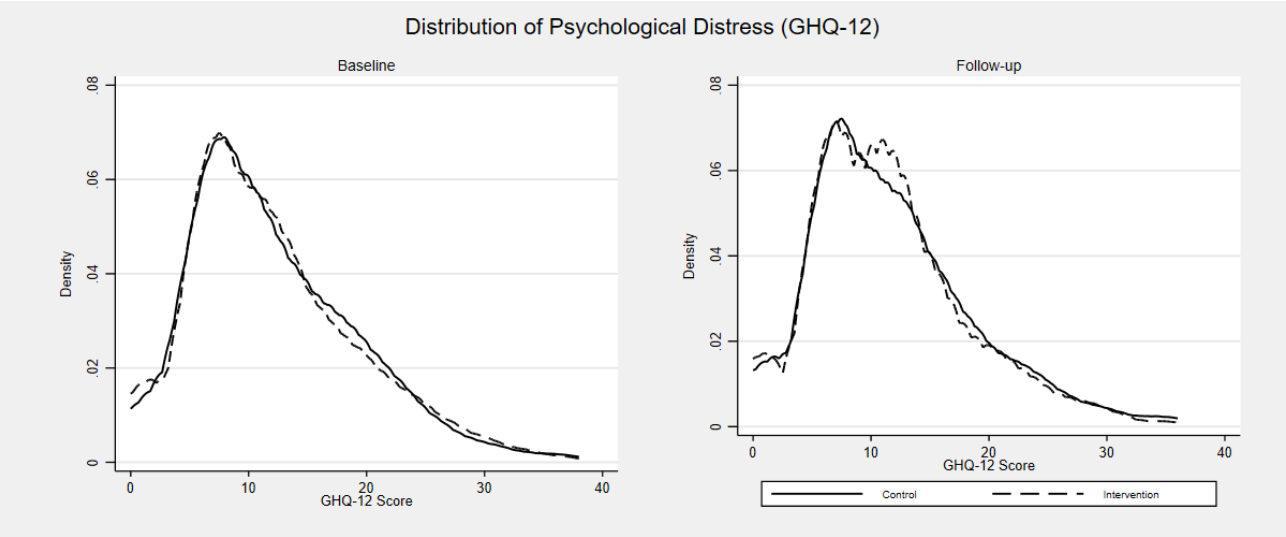


Figure A2 6: Wellbeing (SWEMWBS)

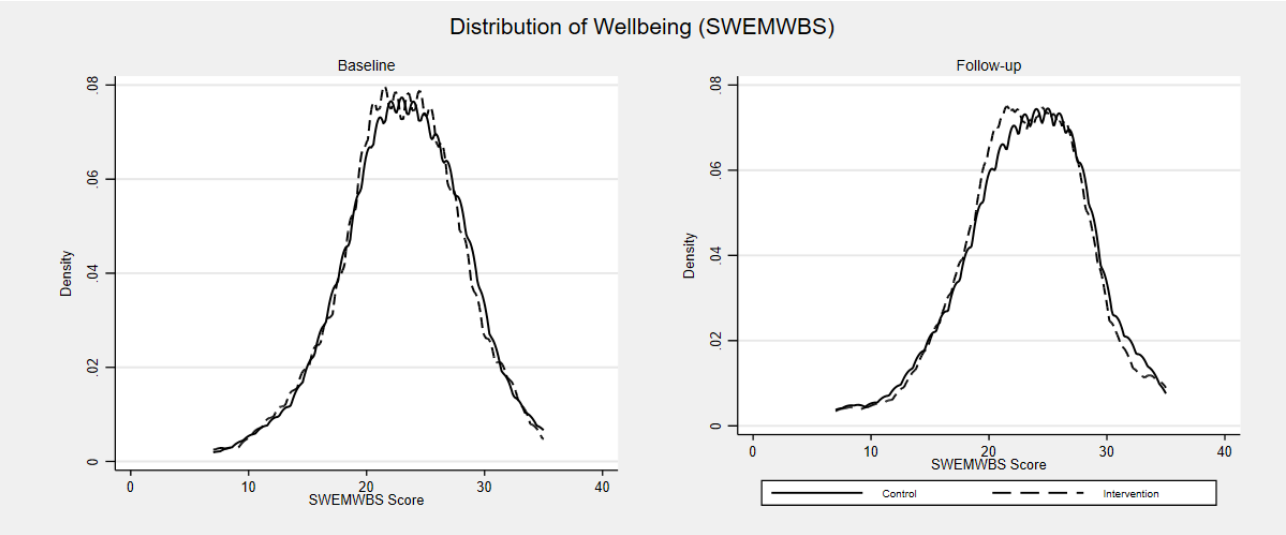
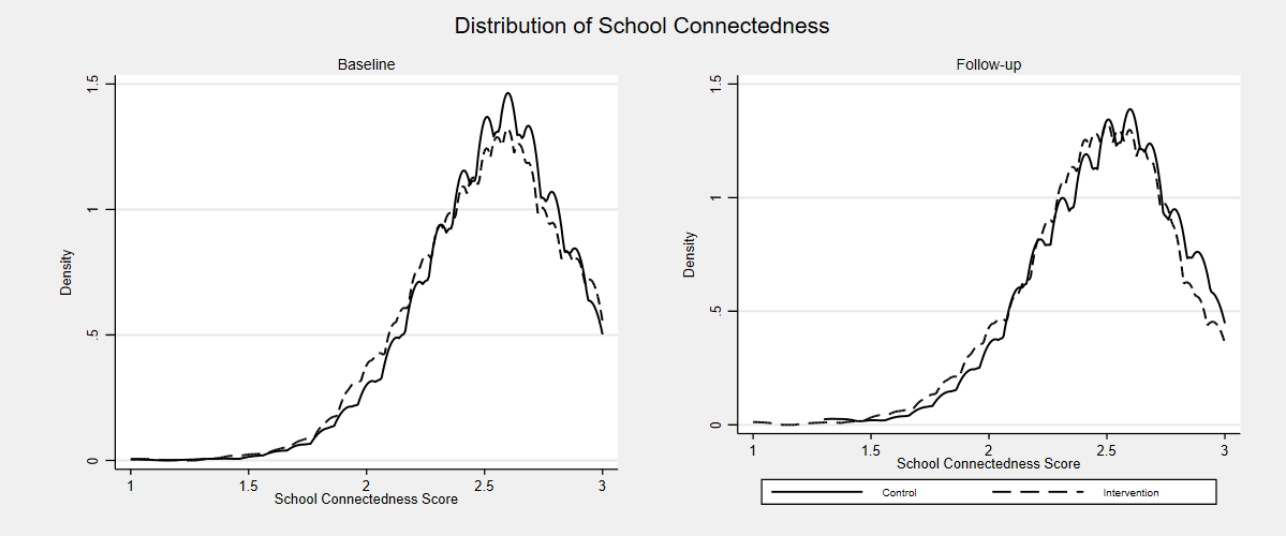


Figure A2 7: School connectedness



Staff survey outcomes

The ARTIC total score shows negatively skewed distributions, with most staff reporting relatively positive attitudes towards trauma-informed care at both timepoints. Staff wellbeing (SWEMWBS) is approximately normally distributed. The smaller staff sample produces less smooth distributions than pupil outcomes.

Figure A2 8: ARTIC total score

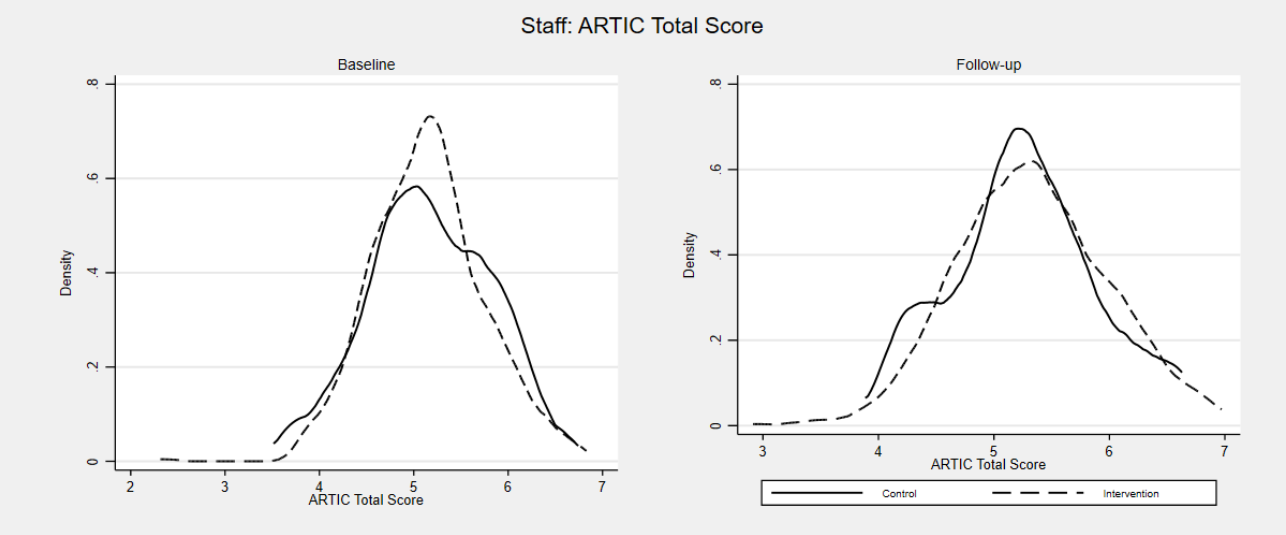
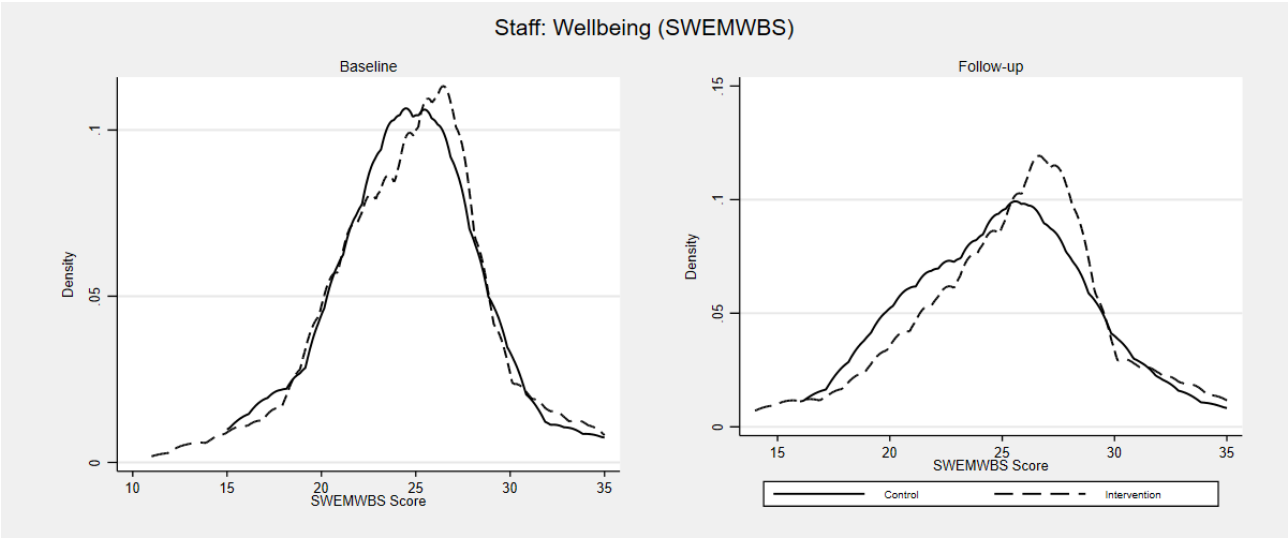


Figure A2 9: Staff wellbeing (SWEMWBS)



Administrative data outcomes

Histograms for administrative outcomes (exclusions, suspensions, and attendance) are available from the authors on request.

Appendix 3: Recruitment documents

Randomised Controlled Trial of Trauma Informed Schools UK – Privacy Notice

The Youth Endowment Fund (YEF) has asked Ipsos UK, TONIC and the University of Kent (UoK) to conduct research into the impacts of the Trauma Informed Schools UK programme.

This Privacy Notice explains who we are, the personal data we collect, how we use it, who we share it with, and your data protection rights.

Who are we?

This evaluation is conducted by an evaluation team of researchers from Ipsos UK, TONIC and UoK. When we collect and use participants' personal information as part of the study, we are the controllers of the personal information, which means we decide what personal information to collect and how it is used.

YEF, who is funding this study, is an independent charitable fund in the United Kingdom. Its purpose is to prevent young people from becoming involved in violence and crime. The fund supports and invests in projects that aim to address the root causes of youth violence and provide opportunities for young people to succeed. The Youth Endowment Fund was established in March 2019 by children's charity Impetus, with a £200m endowment and ten-year mandate from the Home Office. You can find out more about YEF here.

What are we doing?

We are evaluating a new approach to working with young people who attend school. The new approach involves staff who have been specifically trained to incorporate trauma informed practices into their day-to-day work with young people. Our study explores whether this new approach is better than the approach usually employed. To do this we're comparing outcomes for pupils and staff in schools that undertake the training, compared to schools who don't undertake the training.

What personal data will Ipsos UK and UoK receive for this study?

We will collect information about [you/your child/the child in your care] such as their name, address, contact details, date of birth, gender, and ethnicity. We will also collect information using a set of questionnaires that have been specifically designed and tested with young people. We will collect information relating to any criminal activity or delinquency [you/your child/the child in your care] have been involved with, we will collect information about [you/your child/the child in your care] behaviour, mental health, wellbeing and family. We will also collect information about whether [you/your child/the child in your care] have been suspended or excluded from school (only relevant where data is collected from pupils).

Only Ipsos UK and UoK will collect and process personal details from you. This includes your name, age, sex, ethnicity and any answers provided.

Participating in this evaluation is entirely voluntary. You do not have to participate.

Pupils can withdraw consent up until the conclusion of data analysis activities in July 2025.

How will data be used?

Data protection laws require us to have valid reason to use [your/your child's/the child in your care's] personal information. This is referred to as our 'lawful basis for processing'. We rely on the public interest basis to use their personal information (Article 6(1)(e) of the GDPR). We will only use special category information (such as information about health, religion, race or ethnic origin, or any criminal offence information) if it is necessary for research purposes or statistical purposes which are in the public interest (article 9(2)(j) of GDPR). The research team will keep your personal data and your responses in strict confidence in accordance with this Privacy Policy.

We will use the information you give us to evaluate how well the new approach has worked and to write a report about our findings based on all the questionnaires and interviews we have carried out. The final report and any other publications produced by the research team will not contain any personal information about the people who took part in the study, and it will not be possible to identify individuals from the report.

The report will be published on the YEF's website. Any personal information that [you/your child/the child in your care] gives us will be stored securely and kept confidential.

- We may share this personal information with another person or organisation if [you/your child/the child in your care] tells us something during the study that makes us concerned about them or about someone else. Our Safeguarding Policy has more information about steps that we might take if we have concerns about [your/your child/ the child in your care] wellbeing, or the wellbeing of another person (https://www.kent.ac.uk/global-lifelong-learning/safeguarding-policy-andprocedure).
- At the conclusion of the research, pupil survey data will be shared securely with Department of Education, where all personal data will be removed and replaced with a Unique Pupil Matching Reference number. Once this has been done, it is no longer possible to identify any individual young person from the study data. This process is called pseudonymisation. Once this pseudonymisation has been done the data will be transferred into the YEF data archive that is held within the Office for National Statistics (ONS) Secure Research Service. At this stage the YEF becomes the 'controller' of the data in the archive. By maintaining the archive and allowing approved researchers to access the information in the archive, the YEF is performing a public task, and this gives the YEF a lawful bases to use this information.

YEF will only allow information in the archive to be accessed by approved researchers to explore whether the TISUK programme and other YEF-funded programmes had an impact over a longer time period. This may include linking the data to other public datasets, such as education and criminal justice datasets. This will help approved researchers to find out the long-term impact of the projects funded by YEF because they'll be able to see, for example, whether being part of a project reduces a child's likelihood of being excluded from school or becoming involved in criminal activity. You can read more about the data archive here.

How is personal information secured?

The research team take its information security responsibilities seriously and applies various precautions to protect your information from loss, theft or misuse. Security precautions include appropriate physical security of offices and controlled and limited access to computer systems.

All of [you/your child's/the child in your care] personal data used and collected for this study will be stored in data centres and servers within the United Kingdom (UK). We will not move or share information about you outside the UK.

Personal data of pupils who participate in the Pupil Survey will be removed before information is stored in the YEF data archive. The archive is held in the ONS's Secure Research Service (SRS). This means that the data cannot be move in or out of this platform. A combination of secure technology, physical security measures, and extensive procedures and protocols are used to protect the data when it is being used. You can find out more about how information is kept safe in the archive here.

How will personal data be stored for?

The research team will only retain [you/your child's/the child in your care] data in a way that can identify you for as long as is necessary to support the research project and findings. In practice, this means that once we have satisfactorily reported the anonymous research report findings, paper records will be kept for five years and electronic records kept for 10 years after the end of the project. After this point all data will be removed from our systems.

In the YEF data archive, YEF will only keep data for as long as needed to carry out future research. YEF will conduct a review every five years to assess whether there is a continued benefit to storing the personal data in the archive based on its potential use in future research.

Data protection rights

Rights are set out in the General Data Protection Regulation (GDPR) as it applies in the UK, tailored by the Data Protection Act 2018. These include the right in certain circumstances to:

- be informed if your personal data is being used
- get copies of your data
- get your data corrected
- get your data deleted
- limit how we use your data
- object to the use of your data

For further information about your rights see here: https://ico.org.uk/your-data-matters/

In certain circumstances, you also have the right to ask us to erase the personal information where there is no good reason for us continuing to hold it, if we have not already erased the identifiable information.

If there are any problems with our handling of [you/your child's/the child in your care] data, we will notify [you/your child's/the child in your care], your school, and the organisation that is responsible for regulating this (The Information Commissioner's Office) where we are legally required to do so. There are other rights not listed here and exemptions may apply. For more details see here: https://ico.org.uk/for-organisations/data-protection-act-2018/ or contact our School Liaison Officer, Luisa Gomes, tisuk@ipsosresearch.com

All respondents have the right to lodge a complaint with the Information Commissioner's Office (ICO), if you have concerns on how we have processed your personal data. You can find details about how to contact the Information Commissioner's Office at https://ico.org.uk/global/contact-us/ or by sending an email to: casework@ico.org.uk.

Contact details:

Contact Ipsos UK:

Email: tisuk@ipsosresearch.com with "23-019045-01- RCT of Trauma Informed Schools UK" in the email subject line

Post RCT of Trauma Informed Schools UK, Luisa Gomes, Ipsos UK Limited, 3 Thomas More Square, London E1W 1YW

For any questions about the YEF data archive contact the Youth Endowment Fund:

Email: hello@youthendowmentfund.org.uk with "RCT of Trauma Informed Schools UK" in the email subject line

Post: RCT of Trauma Informed Schools UK, Youth Endowment Fund, 1st Floor, 64 Great Eastern St, London EC2A 3QR

Information sheet for pupils/parents

Evaluation of Trauma-Informed Schools UK

This leaflet is for pupils/their parents and/or guardians in schools participating in the Randomised Controlled Trial of Trauma-Informed Schools UK programme.

We are pleased to invite you/your child to participate in this study examining whether implementing trauma-informed practices in schools benefit young people. By participating in this study you/your child will need to complete an online questionnaire in Term 1 2023/24 and Term 2 2024/25. The survey questionnaire will ask you/your child about your/their overall wellbeing, connections to school, friends and community, as well as behaviours and general health.

You/your child may also be invited the following school year to participate in an interview or focus group. However, we will require permission from you/your child before this takes place.

Involvement in this study will help to ensure it is informed by a large enough group of young people that allows us to provide robust evidence to assess whether or not trauma-informed practices in schools benefit young people. If you have any queries about how the study will be run in class or

Who is conducting the evaluation?

Ipsos, an independent research organisation, TONIC, a social research consultancy, and the University of Kent (UoK) have been commissioned by the Youth Endowment Fund (YEF) to conduct the Randomised Controlled Trial of Trauma Informed Schools UK.

What the Trauma Informed Schools UK programme?

Trauma Informed Schools UK (TISUK) are an international training provider to schools and communities, working to date with over 5000 organisations. TISUK programme involves training staff and school leadership to create a mentally healthy culture for the whole school community and equip some staff with the skills to respond mild to moderate pupils' mental health problems. It is intended that through training a school can support pupils with trauma/mental issues, aiming for a safe, supportive environment that aids learning.

What is a randomised controlled trial (RCT)?

An RCT is a scientific study design where participants are randomly assigned to two different groups to evaluate the effects of an intervention or programme.

In October 2023, schools in your area will be **randomly assigned to either 'control' or 'intervention' group**. Being an 'intervention' school means the TISUK programme will be implemented and delivered in your school through 2024, with staff being trained by TISUK. Differently, being a "control" school means that your school will not receive the TISUK programme and there will be no changes during the study period.

The introduction of a randomly assigned control group is the best way of evaluating if an intervention has an impact on children's outcomes. It allows us to compare the efficacy of Trauma-Informed Schools UK against what would have happened if we had changed nothing. In addition, random allocation is like throwing dice that ensures there is a fair process to decide which settings receive the intervention first.

The comparison between pupils in intervention and control schools makes the study possible and worthwhile. We're examining what benefit embedding trauma-informed approaches within schools has on pupils.

Why is this evaluation taking place?

The introduction of 'trauma-informed practice' has become very popular to support services like schools to recognise and respond to these effects of trauma in staff and young people. But we don't know how well these approaches work, and so this study is aiming to fill this gap with robust evidence on the impacts of trauma-informed practice and provide information on how schools might work with young people in the future.

What will you/your child be asked?

The study involves you/your child (and other pupils) completing an online questionnaire supervised by a teacher or non-teaching staff during one class or tutor period. The teacher will provide you/your child with instructions to complete the survey.

We will ask you/your child about your overall wellbeing, connections to school, friends and community, as well as behaviours and general health. We will also ask you/your child to provide details such as name, date of birth, ethnicity and gender. We will not ask you/your child to reflect any individual experiences of trauma, distress or other hard times.

You/your child will be asked to complete a survey in Term 1 2023/24 and a second survey in Term 2 2024/25.

To get a clear understanding of what young people think about the new trauma-informed approach we might ask you/your child to take part in an interview and/or focus group with a researcher in the following school year. Focus groups and interviews will focus on you/your child's perspectives on the support provided by schools to pupils around wellbeing and mental health. Interviews and focus groups are not compulsory, you/your child do not have to participate, and parents' consent is also required. More information will be provided on a later date should you/your child by invited for any of these.

Do(es) you/your child need to take part?

No, participation is completely voluntary. You/your child do(es) not have to participate in the study you/your child does not want to. Opting out from the study will not change the support you/they will be provided with at your school.

You/your child are/is free to stop taking part at any time without giving a reason up to July 2025. If you/your child would like to opt out, please contact a tutor, teacher or the study team (details below) and/or sign the withdrawal form below. Once your/your child's anonymised survey responses goes into the YEF data archive (see below) it may not be deleted because it needs to be used for future research.

How will the information be used?

Information and answers you/your child provide/s will be put together with the answers from other people and presented as research findings and insights about TISUK in a final report that will go on the YEF's website. The report won't include your/your child's name or any information that could be used to identify you/your child.

Ipsos UK, TONIC and UoK will keep all personal data and responses in strict confidence in accordance with the Privacy Notice accompanying this document.

At the end of the study, survey data gathered during the trial will be stored by the YEF for the long term, to examine impacts that might not be captured within the period of this research. Before the data goes into this data archive, all personal information will be removed so no-one looking at this data in the archive will know who you/your child are/is. YEF will only keep data for as long as needed to carry out future research. For more information please refer to the Privacy Notice, or visit please visit https://youthendowmentfund.org.uk/evaluation-data-archive/

Please note that all data will be collected, stored, and processed in accordance with Data Regulations. You can read more about how we will use your/your child's data and your/your child's data protection rights in the Privacy Notice.

Any questions?

If you have any questions about the case studies, please contact your school or our school liaison

Opt-out form

Evaluation of Trauma-Informed Schools UK

Please sign the following form, if you/your child do/does not want to take part in this research. This form should be returned to a member of your school or the Ipsos research team (tisuk@ipsosresearch.com)

Pupil name ……

Parent name ……

Parent signature ……

Date……

Information sheet for staff

Evaluation of Trauma-Informed Schools UK

This leaflet is for staff in schools participating in the Randomised Controlled Trial of Trauma-Informed Schools UK programme.

We are pleased to invite you to participate in this study examining whether implementing trauma-informed practices in schools benefit schools staff. By participating in this study you will need to complete an online questionnaire in Term 1 2023/24 and Term 2 2024/25. The survey questionnaire will ask you about your professional attitudes to trauma-informed care, as well as your overall wellbeing.

You may also be invited to take part in an interview with the research time. However, we will require permission from you before this takes place and will provide further information at a later date should you be selected to take part.

Your involvement will help to ensure the study is informed by a large enough group of school staff that allows us to provide robust evidence to assess whether or not trauma-informed practices in schools benefits staff. If you have any queries about how the study will be run in class or would like to withdraw your participation in this study, please contact a member of the staff or study team using the contact details at the bottom of this leaflet as soon as possible.

Who is conducting the evaluation?

Ipsos, an independent research organisation, TONIC, a social research consultancy, and the University of Kent (UoK) have been commissioned by the Youth Endowment Fund (YEF) to conduct the Randomised Controlled Trial of Trauma Informed Schools UK.

What the Trauma Informed Schools UK programme?

Trauma Informed Schools UK (TISUK) are an international training provider to schools and communities, working to date with over 5000 organisations. TISUK programme involves training staff and school leadership to create a mentally healthy culture for the whole school community and equip some staff with the skills to respond mild to moderate pupils' mental health problems. It is intended that through training a school can support pupils with trauma/mental issues, aiming for a safe, supportive environment that aids learning.

What is a randomised controlled trial (RCT)?

An RCT is a scientific study design where participants are randomly assigned to two different groups to evaluate the effects of an intervention or programme.

In October 2023, schools in your area will be **randomly assigned to either 'control' or 'intervention' group**. Being an 'intervention' school means the TISUK programme will be implemented and delivered in

your school through 2024, with staff being trained by TISUK. Differently, being a “control” school means that your school will not receive the TISUK programme and there will be no changes during the study period.

The introduction of a randomly assigned control group is the best way of evaluating if an intervention has an impact on school, staff and pupil outcomes. It allows us to compare the efficacy of Trauma-Informed Schools UK against what would have happened if we had changed nothing. In addition, random allocation is like throwing dice that ensures there is a fair process to decide which settings receive the intervention first.

The comparison between pupils in intervention and control schools makes the study possible and worthwhile. We’re examining what benefit embedding trauma-informed approaches within schools has on pupils.

Why is this evaluation taking place?

The introduction of ‘trauma-informed practice’ has become very popular to support services like schools to recognise and respond to these effects of trauma in staff and young people. But we don’t know how well these approaches work, and so this study is aiming to fill this gap with robust evidence on the impacts of trauma-informed practice and provide information on how schools might work with young people in the future.

What will you be asked?

The study involves you (and other staff) completing an online questionnaire. We will ask you about your professional attitudes to trauma-informed care, as well as your overall wellbeing. We will not ask you to reflect any individual experiences of trauma, distress or other hard times. You will be asked to complete a survey in Term 1 2023/24 and a second survey in Term 2 2024/25.

To get a clear understanding of what you think about a trauma-informed approach we might ask you to take part in a series of interviews with a researcher. These interviews will occur across 2024 and early 2025. Interviews and focus groups are not compulsory, you do not have to participate. More information will be provided on a later date should you by invited for any of these.

Do you need to take part?

No, participation is completely voluntary. You do not have to participate in the study if you do not want to. You are free to stop taking part without giving a reason up to July 2025. If you would like to withdraw from this study, please contact a member of the research team (details below) or a member of staff, and/or sign the opt-out form below.

How will the information be used?

Information and answers you provide will be put together with the answers from other people and presented as research findings and insights about TISUK in a final report that will go on the YEF’s website. The report won’t include your name or any information that could be used to identify you.

You have a right to withdraw your data and involvement study until its conclusion (in July 2025). Please note that all data will be collected, stored, and processed in accordance with Data Regulations. You can read more about how we will use your data and your data protection rights in the Privacy Notice.

Any questions?

If you have any questions about the case studies, please contact your school or our school liaison officer Luisa Gomes, tisuk@ipsosresearch.com

Staff withdrawal

Evaluation of Trauma-Informed Schools UK

Please sign the following form, if you would like to withdraw from this research. This form should be returned to a member of your school or the Ipsos research team (tisuk@ipsosresearch.com)

Staff name ……

Staff signature……..

Date……

Appendix 4: Randomised Controlled Trial Trauma Informed Schools UK

Randomised Controlled Trial Trauma Informed Schools UK

Data collection guide for schools

Information and Administration Guide on Data Collection for Schools

This information is for schools taking part in the Trauma Informed Schools UK (TISUK) trial in the academic years 23-24 and 24-25.

Who is conducting this research?

Ipsos UK, an independent research organisation, TONIC, a social research organisation, and the University of Kent have been commissioned by the Youth Endowment Fund (YEF) to conduct a Randomised Controlled Trial of Trauma Informed Schools UK (TISUK).

TISUK are an international training provider to schools and communities, working to date with over 5000 organisations. They support school leadership and staff to create a mentally healthy culture for the whole school community through policy and practice, improving outcomes for all.

The aim of this trial is to understand the impact of trauma informed practice on young people's behavioural and wellbeing outcomes.

How will the study be conducted?

The aim of this study is to assess the impact of the TISUK intervention by comparing pupil and staff outcomes in schools that have received the intervention (referred to as the intervention group) and schools that have not (referred to as the control group).

To do this, the study will collect survey data in all schools in both the control and intervention groups at two points in time:

- In October/November 2023: before the implementation of the intervention in intervention schools
- In March-April 2025: after the intervention has been delivered in intervention schools.

Although the intervention is 'whole school', the focal year group for the evaluation are pupils who begin **Year 8 in September 2023**.

What are schools being asked to do?

All schools in both the intervention and control group are required to complete the following activities to ensure satisfactory administration of the study:

- Assign a key contact to liaise with the evaluation team (Ipsos/TONIC/University of Kent) and the delivery team (TISUK).
- Distribute privacy notices, parental opt-out forms and pupil information sheets as directed by the evaluation team.
- Administer surveys to all school staff and all pupils presently in Year 8 at agreed time points.

- Share school information about pupil exclusions, attendance, eligibility for Free School Meals as well as data on staff retention and absence, Name/class information, date of birth and sex will be used to match baseline and follow-up data and identify Unique Pupil Number. Please note that we will be arranging for each school to receive and sign a data sharing agreement with the research team in late 2023.

This guide provides further information and administration guidance on the above activities.

Key contacts

Our Schools Liaison Officer will be the key point of contact for schools and will be available to support school staff by providing further information on study procedure, responding to questions and providing technical support as required. Please direct all study enquiries to our School Liaison Officer, Luisa Gomes (tisuk@ipsosresearch.com)

Each school is asked to identify an equivalent contact who will be overseeing the administration of the trial within the school. By now the contact details of this person has been provided to the School Liaison Officer (details above).

Schools will also need to identify a member of staff to supervise administration of the survey to pupils and a member of staff with pastoral responsibility who will provide support to pupils after they have completed the survey, should that be needed.

These can be the same or different members of staff, but all involved must have a copy of this guide and be familiar with the study procedures. Please let our School Liaison Officer know who will be responsible for each of these roles within your school.

Informing parents and pupils

Parents and pupils must be fully informed about the study and given the opportunity to ask questions before data collection begins.

We will provide schools with an information sheet and opt-out form and privacy notice for parents. Schools are free to adapt these to use school headed paper but are asked not to change any text. These can be distributed electronically to parents using your schools parent communication system and should be sent at least one week in advance of any survey administration.

The information sheet will include details about who to contact should parents have questions about the study. Schools must provide parents with a date by which they must return opt-out forms should they wish their child **not to take part** in the study.

Schools should keep a record of any pupils who do not have parental consent to take part, and will be asked to share this with the evaluation team. Please ensure, any pupils who have been withdrawn do not take part in data collection. Pupils can withdraw on their own remit, and do not require a parental signature to do so.

An information sheet for pupils will also be provided at the same time and should also be distributed one week in advance of the start of data collection. Pupils will be asked to give their assent on the day of data collection (see below).

Administering the pupil surveys

Schools are asked to administer an online survey to all pupils in a single year group at two timepoints during the study:

- October/November 2023 (Year 8)
- March/April 2025 (same year group now in Year 9)

A link to the survey will be provided to schools in advance of data collection. Please note, the survey uses Ipsos's survey software (Dimensions), and the link will direct pupils to the Ipsos server. Schools need to ensure in advance that any network permissions are set to allow the external link to be accessed. Please refer to the Privacy Notice for more information on data protection and security.

The survey should be administered either in whole class or smaller groups in ICT or computer rooms. It is essential for confidentiality and privacy that **one child is allocated per computer**. Pupils must not share computers or must not be able to see each other's responses. Depending, on how many computers are available, smaller group administration may be more appropriate.

A member of school staff who is familiar with the details and procedure of the study should be present throughout the survey administration and must provide short instructions before and after the surveys are complete. Please use the scripts we have provided for these (see below) to ensure administration is consistent across all schools.

Further instructions will be provided for pupils on the introductory page of the survey. These include information on confidentiality, the right to withdraw and data protection procedures. These are written in accessible and child-friendly language, but pupils should also be given the opportunity to ask questions if there is anything they do not understand.

At the end of the information page each pupil will be asked for their assent to take part by providing their name, class name, date of birth, before clicking through to the start of the survey. Staff supervising the survey administration should be alert to anyone who does not want to take part and take a note of their name.

Once started, the survey will take approximately 10-15 minutes to complete.

Accessibility and capacity to take part

All pupils are eligible to take part in the study and no pupil should not be excluded on the basis of Special Educational Needs or Disability. However, schools should decide one a case-by-case basis if pupils are able to understand the nature of the study, what they are being asked to do, and whether they have the capacity to give informed assent to take part. In addition, any pupils with access arrangements for assessment, should be given the same support to complete the survey. That may mean some pupils need smaller groups or a one-to-one setting to complete the survey.

We can also be provided with paper copies of the survey with enlarged text and a version of the online survey compatible with audio narration. These may be used to support pupils with visual impairments, dyslexia or other reading difficulties, or those with English as an Additional Language.

Please make our school liaison officer aware of any specific accessibility or inclusion needs we may need to address.

Aftercare and support

The questionnaires included in this survey ask young people to reflect on their behaviour, emotions, wellbeing and sense of belonging to their school community. All the questions are validated for young people and have been tested and used extensively in research in schools. This means they have been constructed in an age-appropriate and sensitive manner that enable young people to take part safely. However, for some young people reflecting on behavioural and emotional aspects of their wellbeing may be upsetting or raise worries or concerns. It is therefore essential that pupils are given the opportunity to speak to someone in school after completing the questionnaire should they feel they need to.

We ask each school to nominate a member of staff with pastoral responsibility to be available for pupils after they have completed the questionnaire. The name of the staff member and how children can access them should be inserted into the debriefing script below.

Confidential support helplines will be provided at the end of the pupil survey. These will include:

CHILDLINE: 0800 11 11

A confidential and free helpline for young people 18 and under. The number will not appear on a phone bill. You can also go online for a 1-2-1 instant messenger chat with someone who is there to listen and support you at www.childline.org.uk.

Text "SHOUT" to 85258

This is a free, confidential, 24/7 text messaging mental health support service run by a charity called Mental Health Innovations. You can text free from all major UK mobile network and will be connected to a volunteer for an anonymous conversation by text.

SCRIPTS FOR SURVEY ADMINISTRATION

Introduction script: Before the survey is begun, use teachers are to use this script:

Our school is taking part in an important piece of research about how schools can support young people's behaviour and wellbeing by using an approach called trauma informed practice.

Year 8s from 100 schools across the England are taking part.

You and your parents have been given information about the study and they have agreed for you to take part.

This involves filling in a short survey today and another survey again next year.

The survey asks a range of questions about how you feel, your behaviour and your wellbeing.

You will be given all the instructions for filling in the survey once you click on the link, but a couple of important things to remember:

- *There are no right or wrong answers*
- *All your answers and data are confidential, this means that only the researchers see them and no information will be shared with school or with your parents*
- *You do not have to take part if you don't want to and you can stop at any time.*
- *When you are asked to do so please make sure you write your name, class, and date of birth carefully and accurately.*

- *Please do not talk to your neighbour or look at anyone else's answers*

Are there any questions?

Debriefing script: After surveys have been complete

Thank you for completing this survey. Your responses will contribute to this important research about how to support young people.

If there is anything in the survey that has worried or upset you and you want to talk to someone about it, you can come and see XXX (for schools to complete) at XXX (schools to identify). You can do this today, or any other time if you think about something that you want to talk about later. There are also details on your information sheet of confidential support helplines.

You will be asked to fill in a similar survey again in a years' time. But you can ask any questions in the meantime either for us or for the researchers whose contact details are also on your information sheets.

Appendix 5: Memorandum of Understanding

For schools participating in the Trauma Informed Schools UK trial.

Once completed, this document should be returned to support@traumainformedschools.co.uk

Introduction

This information is for schools wishing to take part in the Trauma Informed Schools UK (TISUK) trial in the academic years 23-24 and 24-25. **The aim of this study is to understand the impact of trauma informed practice on young people's outcomes related to behaviour and wellbeing.**

The intervention will be delivered by TISUK; an international provider of training to schools and communities, working to date with over 5000 organisations. The study will be funded by the Youth Endowment fund (YEF) and the independent evaluation will be undertaken by IPSOS in partnership with the University of Kent and TONIC.

The intervention will consist of training courses and support for school staff carried out by TISUK trainers and consultants. The evaluation team will measure the impact of TISUK training on:

- Externalising behaviours of young people
- Psychological distress
- Wellbeing and school connectedness in staff and pupils
- Exclusions and attendance
- Retention, absences and attitudes to Trauma Informed Practice among staff

Schools that agree to participate in the study are being asked to sign this Memorandum of Understanding (MoU) to ensure that the roles and responsibilities of the delivery and evaluation teams are clear to all partners and that schools understand what involvement in the study means for them. **In signing the MoU, schools are confirming that they have read and understood the requirements set out in this document.** Further details about the intervention and associated training can be found here.

The intervention is 'whole school' but the focal year group for the evaluation will be pupils who begin Year 8 in September 2023.

The study is a 'randomised controlled trial' (RCT)

In October 2023, eligible schools that have completed the pre-requisites will be **randomly assigned to either a 'control' or 'intervention' group (akin to a 'coin flip')**. This means that each school has a 50:50 chance of being in the intervention condition, but it is the *comparison* between pupils in intervention and control schools that makes the study possible and worthwhile.

Between January 2024 and March 2025, schools assigned to the **intervention** group will be provided with:

- 2x3 hours of training for the entire staff
- 11 hours of training for 6 senior leaders
- 7.5 hours of network consultancy meetings
- 20 hours of reflective supervision for 2 staff members
- 6 hours of webinar input and discussion for staff and pupils
- 60.5 hours of training for 6 diploma practitioners

Control schools will be expected to facilitate the collection of data on outcomes which can be compared with outcomes in intervention schools. Schools in both the control and intervention groups will receive a financial incentive of £500 upon completing all data collection along with a results pack outlining the findings. In addition, five schools within the intervention group will be selected for a case study and will receive a further £500.

The RCT aims to measure the impact of TISUK by comparing pupil and staff outcomes within schools that have received the intervention and control schools that have not. The trial will provide robust evidence informing the education sector about the impact of trauma informed practice and how best to implement it in schools. The study will collect survey data in all schools in the control and intervention groups at two points in time; in October-November 2023 (before the TISUK training is delivered in intervention schools) and in March-April 2025 (after TISUK training has been delivered in intervention schools). **It is integral that all schools that choose to participate in the study commit to involvement for the duration of the study.**

This study is currently being reviewed by University of Kent ethics board. No data collection will take place until the application has been approved.

Eligibility criteria

To be eligible for inclusion in the study, schools must meet all of the following criteria:

Eligibility criteria	Check (√) if condition met
Schools should have pupils in both Year 8 and Year 9	
Schools should not have a staff member who has completed the TISUK 11-day diploma	
Schools should not have received TISUK whole staff training in the last three years	
If a member of staff has completed TISUK SLT or SMHL training, then they should not have made changes to policy or wider staff practice within the school (e.g., staff training around PACE)	
Schools should not be a fee-paying school	
Schools should not be alternative provision or special schools with 'SEMH' focus	
Schools should not be part of an existing randomised controlled trial	

If your school has received training in trauma informed approaches from an alternative provider within the last three years, then please give details below:

If you are not sure if your school fulfils these criteria, then please contact a member of the evaluation or delivery team to discuss (contact details at the end of this form). If your school is eligible and wishes to participate, **the first step is to get this MoU signed by the Headteacher and another contact who will lead the project within the school** and returned via email to the delivery team.

For a school to be officially part of the study and considered for randomisation there are further requirements that must also be fulfilled (detailed below). **Owing to high levels of interest in the trial, schools will be enrolled in the study on a first-come, first-served basis**. (Note that pre-requisites relate *solely* to inclusion in the study, not getting the TISUK training since the intervention is allocated via 'coin flip' as noted above.)

If your school is eligible…read on…

Roles and responsibilities

There are three 'actors' involved in making this study successful: **schools**, the **delivery team** and the **evaluation team**. For the study to work, each actor must understand their responsibilities and undertake their tasks as required. Schools are, of course, crucial to all of this so we need to be as clear and up-front as possible about what schools need to do in the lead-up to, and when participating in, the study.

The school's responsibilities along with the roles and responsibilities of the delivery and evaluation teams are detailed below. Following the description of the schools' role, we set out the pre-requisites for inclusion in the study – tasks that schools need to complete before they can be part of the project.

Roles and responsibilities for all schools (both those in intervention and control group to enable the study activities to take place:

Schools in both the intervention and control group will be required to complete the following activities to ensure satisfactory administration of the study:

- Assign a key contact to liaise with the evaluation team and TISUK delivery team.
- Distribute privacy notices and parental opt-out forms as directed by the evaluation team.
- Administer surveys to all school staff and all pupils presently in Year 8 at agreed time points (see table below).
- Agree to share school and pupil level information about demographics, SEND, exclusions, attendance, eligibility for Free School Meals and number of looked after children.
- Agree to share school level data regarding staff retention and absence numbers.
- Notify the delivery and evaluation teams in a timely manner if the school needs to withdraw from the trial for operational or other unavoidable reasons. Wherever possible, evaluation data should still be provided.

- One key staff member within schools in the control group only will be asked to complete a BAU survey to gather essential information regarding mental health, wellbeing, and trauma-informed training available to school staff in the control group.
- Schools within the **control group** will agree not to undertake training from Trauma Informed Schools UK for the duration of the study (until June 2025).

Additional roles and responsibilities for schools allocated to the intervention group to enable the TISUK activities to take place:

It is important to keep in mind that the intervention consists of six different training elements. If your school is allocated to the **intervention group**, you will be expected to support the intervention in the following ways:

- Ensure that all staff attend the 3 hour online whole staff trainings in Spring 2024 and Autumn 2025. There will be a minimum of ten different dates available for each and so how this is facilitated will be up to each school.
- Consider carefully TISUK guidance about choosing staff to undertake the 11-day practitioner diploma and support at least 5 to do this (this number may be less for a particularly small school).
- Ensure that at least 4 members of SLT attend the 2-day Senior Lead Training in Spring 24 (there will be multiple dates available and members of SLT do not all need to attend the same training).
- Ensure that SLT attend three termly network consultancy meetings and complete school-based audits/checklist regarding implementation of trauma informed practice.
- Ensure that all staff and pupils have the opportunity to watch three webinars provided by TISUK and all staff watch the webinar: *Conversation that matter: From Racial trauma and discrimination in schools and communities, to respecting and celebrating difference and diversity.*
- Enable at least one member of staff to complete the 4-day reflective supervision workshop and implement a reflective model across your setting.
- Provide a list of pupils who have received targeted support from staff who have completed the TISUK 11-day practitioner diploma.

Appendix 6: Roles & responsibilities of the delivery team: Trauma Informed Schools UK

As part of the delivery of the intervention, the delivery team is responsible for the actions below to allow activities to take place:

- Recruiting schools.
- Distributing and collecting participation documents from schools (EOIs, MoUs)
- Providing schools with information to explain the purpose/design of the intervention. This will include holding information sessions for interested schools and providing guidance on which staff should attend which training.
- Providing whole staff, SLT, diploma and reflective supervision trainings to school staff as well as facilitating network consultancy meetings and providing schools with webinars.
- Liaising with any school staff who were unable to attend a training due to sickness and where possible, facilitating them accessing the missed content on a different course.
- Ensuring that all TISUK trainers and consultants follow guidance set out in our safeguarding policy and that any serious safeguarding concerns/incidents that arise during project delivery will be reported to YEF as per their safeguarding policy.

- Providing a contact for all participating schools staff for a support desk service at any time [support@traumainformedschools.co.uk].

Appendix 7: Roles & responsibilities of the evaluation team: Ipsos UK, TONIC and the University of Kent

The evaluation team will design and conduct the study and collect all relevant data and is thus responsible for the actions below:

- The random assignment of schools to intervention and control groups.
- Acting as the first point of contact for any questions about the evaluation and providing ongoing guidance to schools via a dedicated School Liaison Officer (Ipsos), to answer questions from schools and support the administration of data collection activities.
- Providing information about the trial for parents of participating pupils.
- Developing primary data collection tools (pupil survey & staff survey), administration instructions and other guidance.
- Conducting baseline and end of study surveys with school staff.
- Conducting case study visits to 5 schools during the trial.
- Monitoring changes in policy and school operating environment to identify whether external factors may influence schools' capacity to engage with the study or influence outcomes between baseline and follow-up periods.
- Collect school and pupil level data including names, demographics, SEND, exclusions, attendance, eligibility for Free School Meals, staff retention and staff absence.
- Providing financial incentives to schools that complete evaluation activities.
- Organising a Data Sharing Agreement to be put in place with schools.
- Leading data analysis and disseminating research findings through YEF report and conferences and academic papers.

Appendix 8: Overview and timings for the Evaluation and Delivery of the Intervention

Appendix table 2: Overview and timings for the Evaluation and Delivery of the Intervention

Note: I= only intervention schools, I/C=both intervention and control schools **Year→**	**Academic year 2023/24**			**Academic year 2024/25**			**Responsibility: Evaluation Team (E) or Delivery**
Term →	T1	T2	T3	T1	T2	T3	
Recruitment of schools	✓						**D**
Randomisation	✓						**E**
BAU survey (C)			✓	✓			**E**
"Baseline" pupil survey (I/C)	✓						**E**
"Follow-up" pupil survey (I/C)					✓		**E**
"Baseline" staff survey (I/C)	✓						**E**
"Follow-up" staff survey (I/C)					✓		**E**
Staff interviews (I)*			✓	✓			**E**
Case studies (I) *					✓	✓	**E**
Whole school training (I)		✓		✓			**D**
11-day practitioner diploma (I)		✓	✓	✓			**D**
SLT 2-day training (I)		✓	✓				**D**
Network consultancy meetings (I)			✓	✓	✓		**D**
Webinar content (I)				✓	✓		**D**
Reflective supervision training (I)				✓	✓		**D**

**IPE activities consist of qualitative data collection in schools. Case studies will be done on 5 schools selected within schools in the intervention arm.*

Research activity Programme activity

Throughout the study, we will collect data for the evaluation. It is important to keep in mind that **both intervention and control schools will need to engage with the pupil and staff surveys.**

Only **intervention schools** will be invited to participate in an Implementation and Process Evaluation (IPE) undertaken in parallel to the trial (the RCT). The IPE aims to assess how well trauma-informed practice is implemented throughout schools. This will involve a series of longitudinal interviews with two staff members from 10 schools, alongside case studies with 5 schools, with each case study comprising interviews and focus groups with pupils and interviews with staff who have taken part in the programme. It is not compulsory for schools to participate in the IPE, individual participants must opt-in to the qualitative activities part of the IPE.

Pre-requisites to be considered for randomisation

This table shows the steps a school needs to take before they can be included in the randomisation (which is the point at which the school is officially 'in' the study).

Pre-requisites to be considered for randomisation:
MoU signed by Head + at least 1 other staff member who will lead administration of data collection activities and be the key contact for evaluation team
School staff informed about the study
Parental opt-out forms distributed (and collected where applicable)
"Baseline" staff online survey administered in October
"Baseline" pupil survey administered in October

Informing parents. Schools will need to distribute **opt-out consent letters**, drafted by the evaluation team, to parents of children in participating schools e.g. via Parentmail (schools are free to send out on their own headed paper). Note that in nearly all YEF-funded school-based projects, pupils are automatically enrolled in the study so their parent/guardian has to return the opt-out letter with an indication if they would like their child withdrawn from the study. Two weeks before any data collection, schools will confirm that the opt-out letter has been sent to parents. Once the study is underway, a list of opted-out pupils (if there are any) will need to be returned to the evaluation team, and these pupils will not be asked to complete any surveys.

School staff 'baseline' survey. All staff will need to be informed about the study in advance and will be given the option to withdraw from the dataset. However, schools are asked to encourage all staff to complete the anonymous surveys as these will be used for the trial. ***Before*** **a school can be randomised, at least 80% of school staff must have completed the baseline staff survey.** Participating schools are expected to support these surveys by distributing the survey link assigned to each school.

Schools will be asked to provide administrative data to support the study. Data Sharing Agreements between Ipsos UK, TISUK, TONIC, and the University of Kent are in place to ensure that this happens in a way that maintains security of data. Providing data, therefore, relies on schools consenting to the sharing of data with the intervention and the research team. The evaluation team will develop data sharing agreements (DSAs) will schools to enable sharing of school administration data including pupil level data including names, demographics, SEND, exclusions, attendance, eligibility for Free School Meals, staff retention and staff absence will. This will be provided to schools in late 2023.

Data protection

The evaluation team will collect and store all study data in accordance with the Data Protection Act (1998). All child data and any other personal data used for the project will be treated with the strictest confidence and will be used and stored in accordance with the General Data Protection Regulation (GDPR) (2018) and the Data Protection Act (2018). Study data will be stored on secure servers. Data transferred between the

delivery and evaluation teams containing any identifying information will be passphrase encrypted. Data transferred between schools and the evaluation team will be via encrypted files or secure file transfer.

No-one who participates in the study will be named in any publications arising from the study and results will be presented on aggregate. Schools will have the option of allowing their participation in the study to be known.

The selected legal bases for processing personal and special data align with the public task basis under the UK General Data Protection Regulation (GDPR). The Evaluation team (Ipsos UK, Tonic and the University of Kent) is the **joint data controller** and is committed to conducting the evaluation in the public interest and exercising official authority vested in the controller. The collection and processing of personal and special data are essential for this trial's research and statistical purposes. The overarching goal is to contribute to children's and young people's and school staff's well- being

All respondents to trial surveys and questionnaires will be provided with an option to withdraw personal data from the study up until August 2025. This will be outlined within the Privacy Notices and information sheets. Please refer to the Privacy Notice, which outlines how personal data will be collected, processed and secured.

In addition to assessing the impact of our projects over this evaluation period, YEF also wish to evaluate the long-impact of TISUK in the future. At the end of the study, data collected will have any identifiable information removed and stored in a secure Data Archive. This data might be analysed in the future along with data from other sources. This may include, for example, assessing whether children and young people who took part in YEF- funded projects were less likely to be excluded from school or get involved in crime in the future.

This process involves sharing the data with the Department for Education who replace all personal data with their unique Pupil Matching Reference Number, and all personal data is then deleted. The YEF Data Archive is held in the Office for National Statistics Secure Research Service. Further information on the Data Archive can be found here: https://youthendowmentfund.org.uk/wp-content/uploads/2021/07/YEF-Data-Guidance-Participants.pdf

Signature – Agreement for school to participate

In order to participate in this study, schools must ensure that this MoU is signed by the Headteacher and an additional member of staff who will be the lead contact for the trial. This document should then be emailed to the delivery team (support@traumainformedschools.co.uk)

By signing this memorandum of understanding we:

- **(i) confirm that we have read and understood the eligibility requirements for this study and that we fulfil all applicable eligibility requirements;**
- **(ii) confirm that we have read and understood the pre-requisites for participating in this study and that we commit to completing these in order to be included in the study;**
- **(iii) confirm that we have read this document and the privacy notice, understand and agree that pupil data will be processed under the legal basis of public task**
- **(iv) confirm that we have read and understood the section above that sets out 'roles and responsibilities' for the evaluation team, the delivery team and schools;**
- **(v) agree to engage with the evaluation and delivery teams to facilitate the study tasks;**

School Name and Address:	

Number of pupils in school:			
Number of staff:			
Number of staff in SLT:			
Is your school part of a Multi-Academy Trust?			
Head Teacher Name:			
Head Teacher Signature:		Date	__ / __ / __
Head Teacher Email Address:			
School Lead Contact Name:			
School Lead Signature:		Date	__ / __ / __
School Lead Contact Email Address:			
School Telephone Number:			

Space for notes about the study/questions/reminders:

Evaluation Team (IPSOS) contact:	**Lottie Hayes –** Lottie.Hayes@ipsos.com
Delivery Team (TISUK) contact:	**Rowan Jones 01752 278477** **support@traumainformedschools.co.uk**

Appendix 9: Strengths and Difficulties Questionnaire - baseline

S 11-17

For each item, please mark the box for Not True, Somewhat True or Certainly True. It would help us if you answered all items as best you can even if you are not absolutely certain or the item seems daft! Please give your answers on the basis of how things have been for you over the last six months.

Your Name .....Male/Female

Date of Birth.....

	Not True	Somewhat True	Certainly True
I try to be nice to other people. I care about their feelings	☐	☐	☐
I am restless, I cannot stay still for long	☐	☐	☐
I get a lot of headaches, stomach-aches or sickness	☐	☐	☐
I usually share with others (food, games, pens etc.)	☐	☐	☐
I get very angry and often lose my temper	☐	☐	☐
I am usually on my own. I generally play alone or keep to myself	☐	☐	☐
I usually do as I am told	☐	☐	☐
I worry a lot	☐	☐	☐
I am helpful if someone is hurt, upset or feeling ill	☐	☐	☐
I am constantly fidgeting or squirming	☐	☐	☐
I have one good friend or more	☐	☐	☐
I fight a lot. I can make other people do what I want	☐	☐	☐
I am often unhappy, down-hearted or tearful	☐	☐	☐
Other people my age generally like me	☐	☐	☐
I am easily distracted, I find it difficult to concentrate	☐	☐	☐
I am nervous in new situations. I easily lose confidence	☐	☐	☐
I am kind to younger children	☐	☐	☐
I am often accused of lying or cheating	☐	☐	☐
Other children or young people pick on me or bully me	☐	☐	☐
I often volunteer to help others (parents, teachers, children)	☐	☐	☐
I think before I do things	☐	☐	☐
I take things that are not mine from home, school or elsewhere	☐	☐	☐
I get on better with adults than with people my own age	☐	☐	☐
I have many fears, I am easily scared	☐	☐	☐
I finish the work I'm doing. My attention is good	☐	☐	☐

Appendix 10: Strengths and Difficulties Questionnaire – follow up

S 11-17

FOLLOW-UP

For each item, please mark the box for Not True, Somewhat True or Certainly True. It would help us if you answered all items as best you can even if you are not absolutely certain or the item seems daft! Please give your answers on the basis of how things have been for you over **the last month**.

Your Name .....Male/Female

Date of Birth.....

	Not True	Somewhat True	Certainly True
I try to be nice to other people. I care about their feelings	☐	☐	☐
I am restless, I cannot stay still for long	☐	☐	☐
I get a lot of headaches, stomach-aches or sickness	☐	☐	☐
I usually share with others (food, games, pens etc.)	☐	☐	☐
I get very angry and often lose my temper	☐	☐	☐
I am usually on my own. I generally play alone or keep to myself	☐	☐	☐
I usually do as I am told	☐	☐	☐
I worry a lot	☐	☐	☐
I am helpful if someone is hurt, upset or feeling ill	☐	☐	☐
I am constantly fidgeting or squirming	☐	☐	☐
I have one good friend or more	☐	☐	☐
I fight a lot. I can make other people do what I want	☐	☐	☐
I am often unhappy, down-hearted or tearful	☐	☐	☐
Other people my age generally like me	☐	☐	☐
I am easily distracted, I find it difficult to concentrate	☐	☐	☐
I am nervous in new situations. I easily lose confidence	☐	☐	☐
I am kind to younger children	☐	☐	☐
I am often accused of lying or cheating	☐	☐	☐
Other children or young people pick on me or bully me	☐	☐	☐
I often volunteer to help others (parents, teachers, children)	☐	☐	☐
I think before I do things	☐	☐	☐
I take things that are not mine from home, school or elsewhere	☐	☐	☐
I get on better with adults than with people my own age	☐	☐	☐
I have many fears, I am easily scared	☐	☐	☐
I finish the work I'm doing. My attention is good	☐	☐	☐

Appendix 11: School Connectedness Scale questionnaire

Each assessment item was scaled using a 3-point Likert scale with the available selections being:

(a) True
(b) Somewhat true
(c) Not true.

The first construct, Teacher Bonding and Attachment refers to a student's ability to create and maintain positive relationships with their teachers and as they are successful in developing these relationships, their values, beliefs, attitudes, and involvement in school improve. Questionnaire items measuring the Teacher Bonding and Attachment construct are:

(a) I like my teachers
(b) My teachers help me
(c) If I have a problem, my teacher helps me

The second construct, Peer Bonding and Attachment refers to a student's ability to create and maintain positive relationships with their peers and as they are successful in developing these relationships, their values, beliefs, attitudes, and involvement in school improve. Questionnaire items measuring the Peer Bonding and Attachment construct are:

(a) People are happy when I come to school
(b) I have a friend at school
(c) I have more than one friend at school
(d) My friends are happy when I'm at school

The third construct, School Engagement, maintains its current definition. Questionnaire items measuring the school engagement construct are:

(a) I do my classwork,
(b) I do my homework
(c) I come to school every day

Appendix 12: The Short Warwick–Edinburgh Mental Well-being Scale (WEMWBS)

Below are some statements about feelings and thoughts.

Please tick the box that best describes your experience of each over the last 2 weeks

STATEMENTS	None of the time	Rarely	Some of the time	Often	All of the time
I've been feeling optimistic about the future	1	2	3	4	5
I've been feeling useful	1	2	3	4	5
I've been feeling relaxed	1	2	3	4	5
I've been dealing with problems well	1	2	3	4	5
I've been thinking clearly	1	2	3	4	5
I've been feeling close to other people	1	2	3	4	5
I've been able to make up my own mind about things	1	2	3	4	5

Warwick–Edinburgh Mental Well-being Scale (WEMWBS)

Appendix 13: General Health Questionnaire

We should like to know if you have had any medical complaints, and how your health has been in general, *over the past few weeks.* Please answer ALL the questions on the following pages simply by underlining the answer which you think most nearly applies to you. Remember that we want to know about present and recent complaints, not those that you had in the past.

It is important that you try to answer ALL the questions.

Have you recently:

1. *been able to concentrate on whatever you're doing?*	Better than usual	Same as usual	Less than usual	Much less than usual
2. *lost much sleep over worry?*	Not at all	No more than usual	Rather more than usual	Much more than usual
3. *felt that you are playing a useful part in things?*	More so than usual	Same as usual	Less useful than usual	Much less useful
4. *felt capable of making decisions about things?*	More so than usual	Same as usual	Less so than usual	Much less capable
5. *felt constantly under strain?*	Not at all	No more than usual	Rather more than usual	Much more than usual
6. *felt you couldn't overcome your difficulties*	Not at all	No more than usual	Rather more than usual	Much more than usual
7. *been able to enjoy your normal day-to-day activities*	More so than usual	Same as usual	Less so than usual	Much less than usual
8. *been able to face up to your problems*	More so than usual	Same as usual	Less able than usual	Much less able
9. *been feeling unhappy and depressed*	Not at all	No more than usual	Rather more than usual	Much more than usual
10. *been losing confidence in yourself?*	Not at all	No more than usual	Rather more than usual	Much more than usual
11. *been thinking of yourself as a worthless person?*	Not at all	No more than usual	Rather more than usual	Much more than usual
12. *been feeling reasonably happy, all things considered?*	Not at all	No more than usual	Rather more than usual	Much more than usual

13. How well would you say you are managing financially these days?	Living comfortably or doing alright	Just about by	Finding it difficult or very difficult

Any previous treatment for mental health problems?
Have you ever received treatment for depression, anxiety, or other mental health problem in the past? *(Treatment might be tablets, or counselling, or seeing a psychiatrist or other mental health professional)*
(Please tick one)

Yes No

Appendix 14: Engagement Tool

Appendix table 3: Engagement Tool School Scores

School	Adherence score	Diploma	Reflective supervision	Consultancy	Webinar	Whole staff training	Rating
1	**100%**	45	10	15	5	25	Excellent
2	**100%**	45	10	15	5	25	Excellent
3	**98%**	45	10	15	3	25	Excellent
4	**98%**	45	10	15	3	25	Excellent
5	**98%**	45	10	15	3	25	Excellent
6	**98%**	45	10	15	3	25	Excellent
7	**98%**	45	10	15	3	25	Excellent
8	**98%**	45	10	15	3	25	Excellent
9	**95%**	45	10	15	0	25	Excellent
10	**95%**	45	10	10	5	25	Excellent
11	**95%**	45	10	15	0	25	Excellent

12	**95%**	45	10	15	0	25	Excellent
13	**93%**	45	10	10	3	25	Excellent
14	**93%**	40	10	15	3	25	Excellent
15	**93%**	40	10	15	3	25	Excellent
16	**91%**	45	8	10	3	25	Excellent
17	**90%**	45	10	10	0	25	Excellent
18	**90%**	45	10	10	0	25	Excellent
19	**90%**	45	10	15	0	20	Excellent
20	**88%**	35	10	15	3	25	Good
21	**88%**	35	10	15	3	25	Good
22	**85%**	45	10	5	0	25	Good
23	**85%**	45	10	15	0	15	Good
24	**85%**	35	10	15	0	25	Good
25	**85%**	45	10	5	0	25	Good
26	**78%**	30	10	10	3	25	Good
27	**78%**	25	10	15	3	25	Good
28	**75%**	30	10	15	0	20	Moderate
29	**73%**	25	10	10	3	25	Moderate
30	**70%**	35	10	10	0	15	Moderate
31	**68%**	35	0	15	3	15	Moderate

32	**63%**	35	10	5	3	10	Moderate
33	**60%**	30	0	5	0	25	Moderate
34	**54%**	35	4	0	0	15	Moderate
35	**50%**	20	10	5	0	15	Poor
36	**40%**	25	0	10	0	5	Poor

Appendix 15: Data protection detailed

This section outlines the measures and considerations undertaken to ensure compliance with data protection regulations for this trial. This statement framed the Memorandum of Understanding, (see Appendix 5) information sheets, and privacy notice provided to potential participants within schools.

Legal Basis for Processing Personal Data

The processing of personal data in the context of the trial was conducted under the legal basis of the UK General Data Protection Regulation (UK GDPR), specifically Article 6(1)(e). Article 6(1)(e) pertains to the processing of personal data necessary for performing a task in the public interest or exercising official authority vested in the controller.

Legal Basis for Processing Special Data

The processing of special category personal data within this trial was justified under the UK GDPR, specifically Article 9(2)(j). Article 9(2)(j) permits processing for archiving purposes in the public interest, scientific or historical research or statistical purposes.

The rationale for Selected Legal Bases

The selected legal bases for processing personal and special data align with the public task basis under the UK GDPR – 6(1)(e) and 9(2)(j). The Evaluation team (Ipsos UK, Tonic and the University of Kent) is committed to conducting the evaluation in the public interest and exercising official authority vested in the controller. The collection and processing of personal and special data were essential for this trial's research and statistical purposes. The overarching goal is to contribute to children's and young people's and school staff's well-being.

GDPR Compliance

To ensure compliance with the GDPR, the Evaluation team implemented the measures below:

1. Protecting Individual Data Subjects' Rights: Data subjects were informed of their rights regarding their personal data, including the right to access, rectification, erasure, restriction of processing, and objection. Mechanisms for exercising these rights were provided.

2. Purposes for Data Processing: The trial-specific privacy notice provided to potential participants (CYP, parents/guardians, and school staff) clearly outlined the purposes for which their data was collected and processed.

3. Parties with Access to Data: Access to personal data was limited to authorised personnel involved in the Ipsos team. Access was granted on a need-to-know basis and in adherence to data protection principles. Data was held securely on the UK servers at Ipsos UK and all personal/sensitive information was stored in secure folders, encrypted/password protected, and only accessible by the Ipsos team working on the trial.

4. Retention Periods: The retention of personal data was limited to the duration necessary for the purposes outlined in the trial-specific privacy notice. Once the data is no longer required, it will be securely deleted from all locations by the evaluator and/or delivery team.

5. Information Sharing Agreement: The evaluation and intervention delivery teams established and sign an information sharing agreement that clearly outlined what information will be shared, the reasons for sharing, and the means of sharing. This agreement ensured that data was shared securely and in compliance with data protection regulations. We used Ipsos Transfer for the secure transfer of files containing personal/sensitive information encrypted to a minimum standard of AES 256.

6. Secure Communication: All communication between the intervention and evaluation teams occurred through encrypted channels secured using a virtual private network (VPN). This approach protected the confidentiality and integrity of the data during transmission.

Data Processing Roles

During the evaluation process of the trial, the roles of the data controller and any processors were as follows:

Data Controller: Ipsos UK assumed the role of the data controller and held the responsibility for determining the purposes and means of processing personal data within the scope of the RCT.

Processors: The evaluation team, comprised of Ipsos UK, Tonic, and the University of Kent, as well as the intervention delivery team at TISUK, acted as processors who process personal data under the instructions and on behalf of the data controller. Their involvement was essential for the evaluation tasks outlined in the RCT.

Consent collection

The privacy notice provided to potential participants clearly outlined the parties from whom consent were obtained, ensuring transparency and informed decision-making.

For surveys with CYP and schools staff, all respondents (CYP and school staff) were given the chance to opt-out from data collection activities though opt-out letters before baseline testing (for CYP this opt-out option will be offered to their parent/guardians given the age of the CYP). Assent from CYP and school staff were obtained immediately before surveys start. This was outlined within the privacy notice as well as within introductory text prior to respondents commencing survey questionnaires. Information sheets specified the aims of the study, what the data was being used for (including a description of the Data Archive) as well as the legal basis for processing personal data.

For interviews and focus groups with pupils, obtaining consent occurred via two steps. Schools were asked by the school liaison officers to provide consent forms to parents of identified pupils eligible to participate

together with information sheets. Pupils participated in the data collection after giving assent and once the school had received a signed consent from the pupil's parent/guardian. Privacy Notices as well as information and consent/assent forms had full details of the research as well as contact details of key evaluation research staff should parents or pupils require further information. All pupils participating in interviews and focus groups were required to provide written assent prior to the activity taking place.

For all other qualitative data collection with TISUK trainers, network consultants and school staff, consent was obtained either in writing or verbally prior to commencement of the interview. All participants were sent an information sheet as well as a Privacy Notice in advance of any fieldwork activity.

Data Sharing Agreements (DSAs) and Data Protection Impact Assessments (DPIAs)

For schools collaborating with TISUK and the evaluation team, the following measures were implemented:

Data Sharing Agreements (DSAs): A comprehensive data sharing agreement was established between the schools and the relevant parties involved, including TISUK and the evaluation team. This agreement outlined the specific information to be shared, the purposes of sharing, and the means of sharing. It ensured that all parties involved were aware of their responsibilities and obligations regarding data protection and confidentiality.

Data Protection Impact Assessments (DPIAs): A DPIA was conducted for the schools involved in this trial. This assessment identified and evaluated any potential risks and impacts on the privacy and rights of individuals whose data was processed within the school setting. The DPIA assessed the necessity and proportionality of the data processing activities, as well as the measures in place to mitigate any identified risks. The evaluation team, in collaboration with TISUK and relevant school authorities, ensured that appropriate DPIAs are conducted in accordance with the requirements of the UK GDPR.

Appendix 16: Observation Performance

ervation framework

Date:	**Researcher:**
Session:	**Type of Attendees**

Activities	**Materials**	**Interactions / Discussions**	**Objects**	**Users**
Activities taking place, exercises carried out by staff/volunteers was the activity/session delivered as planned, the length of the session, whether the activity/session is regular or a one-off, are different elements received less well	*What materials are required, what the format of the activity/session is (small groups, breakout, etc.)*	*What were the key topics of discussion, whether young people contribute/level of contribution, any group/power dynamics at play*	*What kinds of objects are required for staff/volunteers and young people to carry out activities and session, how do objects relate to actions etc*	*Who is in attendance/how many are involved*

Our standards and accreditations

Ipsos' standards and accreditations provide our clients with the peace of mind that they can always depend on us to deliver reliable, sustainable findings. Our focus on quality and continuous improvement means we have embedded a "right first time" approach throughout our organisation.

ISO 20252

This is the international specific standard for market, opinion and social research, including insights and data analytics. Ipsos UK was the first company in the world to gain this accreditation.

Market Research Society (MRS) Company Partnership

By being an MRS Company Partner, Ipsos UK endorse and support the core MRS brand values of professionalism, research excellence and business effectiveness, and commit to comply with the MRS Code of Conduct throughout the organisation & we were the first company to sign our organisation up to the requirements & self-regulation of the MRS Code; more than 350 companies have followed our lead.

ISO 9001

International general company standard with a focus on continual improvement through quality management systems. In 1994 we became one of the early adopters of the ISO 9001 business standard.

ISO 27001

International standard for information security designed to ensure the selection of adequate and proportionate security controls. Ipsos UK was the first research company in the UK to be awarded this in August 2008.

The UK General Data Protection Regulation (UK GDPR) and the UK Data Protection Act 2018 (DPA)

Ipsos UK is required to comply with the UK General Data Protection Regulation (GDPR) and the UK Data Protection Act (DPA). These cover the processing of personal data and the protection of privacy.

HMG Cyber Essentials

Cyber Essentials defines a set of controls which, when properly implemented, provide organisations with basic protection from the most prevalent forms of threat coming from the internet. This is a government-backed, key deliverable of the UK's National Cyber Security Programme. Ipsos UK was assessed and validated for certification in 2016.

Fair Data

Ipsos UK is signed up as a “Fair Data” company by agreeing to adhere to twelve core principles. The principles support and complement other standards such as ISOs, and the requirements of data protection legislation. .

Appendix 17. YEF Security Rating

Rating	Design	MDES Outcome: Threshold*	Attrition	Initial score		Adjustments		Final score
5	Randomised design	Offending: <=0.1 SDQ tot: <= 0.3 Other: <= 0.2	0-10%	0		+1		
4	Design for comparison that considers some type of selection on unobservable characteristics (e.g. RDD, Diff-in-Diffs, Matched Diff-in-Diffs)	Offending: 0.11 – 0.19 SDQ tot: 0.31 – 0.39 Other: 0.21 – 0.29	11-20%					
3	Design for comparison that considers selection on all relevant observable confounders (e.g. Matching or Regression Analysis with variables descriptive of the selection mechanism)	Offending: 0.2 – 0.29 SDQ tot: 0.4 – 0.49 Other: 0.3 – 0.39	21-30%					
2	Design for comparison that considers selection only on some relevant confounders	Offending: 0.3 – 0.39 SDQ tot: 0.5 – 0.59 Other: 0.4 – 0.49	31-40%					
1	Design for comparison that does not consider selection on any relevant confounders	Offending: 0.4 – 0.49 SDQ tot: 0.6 – 0.69 Other: 0.5 – 0.59	41-50%					1
0	No comparator	Offending: >= 0.5 SDQ tot: >= 0.7 Other: >= 0.6	>50%					

*MDES requirements vary by outcome measurement. Offending: Offending data collected through self-report or admin data; SDQ tot = SDQ total difficulties score; Other: all other outcomes, incl. SDQ externalising and internalising