

EXTENSION EVALUATION PROTOCOL
– ADDENDUM

**Extension Evaluation of the 'SAFE'
(Support, Attend, Fulfil, Exceed)
Taskforces**

RAND Europe, University of Westminster,
FFT Education Datalab

Principal investigator: Dr Ana FitzSimons

Extension Evaluation of the 'SAFE' (Support, Attend, Fulfil, Exceed) Taskforces



Addendum to the original evaluation protocol

Evaluating institution: RAND Europe, University of
Westminster, FFT Education Datalab

Principal investigator: Dr Ana FitzSimons

Project title	Evaluation of the 'SAFE' (Support, Attend, Fulfil, Exceed) Taskforces
Developer (Institution)	Department for Education
Evaluator (Institution)	RAND Europe, University of Westminster, FFT Education Datalab
Principal investigator(s)	Dr Ana FitzSimons
Protocol author(s)	Dr Ana FitzSimons, Martha Aitken, Dr Veruska Oppedisano, Prof Richard Dorsett, Dave Thomson
Study design	Quasi-experimental design: difference-in-differences
Study type	Efficacy
Evaluation setting	Selected secondary schools in 10 local authority areas
Target group	Pupils in Years 7 to 9 who are at considered at risk of exclusion or disengagement from education
Number of participants	10 local authority areas are participating in SAFE and the evaluation. A further 12 local authority areas will be included as comparison sites for the evaluation. 10,066 pupils in the 10 local authority areas in which SAFE operates were recorded as having had referrals to SAFE

	accepted. The final number of comparison group pupils included in the evaluation will be the same as (if one-to-one matching is employed) or more than (if many-to-one matching is employed) the final number of pupils who participated in SAFE.
Primary outcome and data source	<u>For pupils in years 7-9 during the initial 3-year pilot period:</u> Suspensions from school (source: National Pupil Database data) (Endline date: end of Spring term 2026)
Secondary outcome and data source	<u>For pupils in years 7-9 during the initial 3-year pilot period:</u> Overall absence (source: National Pupil Database data) Unauthorised absence (source: National Pupil Database data) Permanent exclusion from school (source: National Pupil Database data) (Endline date: end of Spring term 2026) <u>For pupils born between 01/09/08 and 31/08/10</u> Initial (Year 12) post-16 destinations (source: National Pupil Database linked to Individual Learner Record data) (Endline date: October 2026) <u>At the local authority level:</u> Serious violence offences (source: Police Recorded Crime and Outcomes Open Data Tables) (Endline data: end of the Financial Year 2026)

Protocol version history

Version	Date	Reason for revision
1.0 [original]	23.10.2025	

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Background

This evaluation is an extension to the existing YEF-funded evaluation of the SAFE Taskforces programme launched by the Department for Education in 2022 in 10 Local Authority areas. The existing evaluation includes a quasi-experimental impact evaluation that uses a difference-in-differences approach to estimate the causal effect of SAFE on a range of outcomes for pupils and local authorities (LAs) during the programme pilot period (from academic year 2022/23 to 2024/25); a mixed methods process evaluation that aims to understand how the SAFE Taskforces programme is delivered and the experiences of those involved; and a cost evaluation that describes the costs associated with delivery of SAFE.

Full details about the SAFE Taskforces programme and existing evaluation can be found within the original SAFE Evaluation [Study Protocol](#).

This extension evaluation seeks to provide added value by estimating longer term causal effects of SAFE on the pupils and LAs that participated in the programme during the initial pilot period. It explores the causal effect of SAFE on the full range of outcomes included in the original evaluation, plus an additional secondary outcome of pupils' initial post-16 destinations (i.e. Year 12), during the year following of the programme pilot (academic year 2025/26). (Initial post-16 destinations refer to pupil enrolment in publicly funded education, training or apprenticeships on the 31st October in the year when they are aged 16.)

As specified in the original SAFE Evaluation [Study Protocol](#), this outcome was not included in the original impact evaluation, as it lay outside the SAFE programme theory of change, some SAFE interventions did not target it, and the smaller eligible sample within the original evaluation period limited power to detect effect. We noted, however, that it would be possible to look at this outcome in a future study. We include initial post-16 destinations in this extension evaluation as, over the extension period, more pupils will reach age 16, increasing power, and because it is an outcome of interest within the implementation and process evaluation.

The extension evaluation does not include any further process or cost evaluation. Analysis and reporting are planned to take place in 2027.

Impact evaluation

Summary of the approach to assessing further impact on outcomes included in the original evaluation

This extension evaluation will use the same methodology as set out in the original SAFE Evaluation [Study Protocol](#), to answer the same set of impact evaluation questions. See the original SAFE Evaluation [Study Protocol](#) for further details on our methodology and original impact evaluation questions.

The evaluation of impact on educational outcomes will use an individual-level analysis, comparing differences in changes in outcomes between pupils participating in the SAFE Taskforces programme and a comparison sample of matched pupils in matched schools in non-SAFE areas that are delivering the DfE's Alternative Provision Specialist Taskforce (APST) programme¹. The comparison pupil sample will be selected through a two-step propensity score matching procedure: first, we will match schools participating in SAFE to similar schools in non-SAFE APST areas; second, we will match pupils participating in SAFE to similar pupils within the sample of matched comparison schools (see Analysis section below for further details of the matching process). The estimation of impact on pupil-level outcomes will be based on a difference-in-differences methodology applied to this sample of treatment pupils and comparison pupils. The analysis will compare the difference in outcomes between treatment and comparison pupils before the interventions start, to the difference in outcomes following the interventions. It will control for differences in pupil- and school-related characteristics (listed in the Analysis section of this Study Protocol). The preliminary analysis undertaken to inform the matching and methodology designs can be found in the original [evaluation protocol appendix A](#).

The evaluation of impact on the serious violence outcome will use an LA-level analysis, comparing differences in changes in outcomes between LAs participating in SAFE and LAs that are participating in APST but not SAFE. The estimation of impact on LA-level outcomes will be based on a difference-in-differences methodology applied to this sample of SAFE LAs and non-SAFE APST LAs. The analysis will compare the difference in outcomes between treatment and comparison LAs before the interventions start, to the difference in outcomes following the interventions.

¹ The APST programme was delivered in 22 LAs, of which 10 also delivered the SAFE Taskforce programme. APST and SAFE areas were selected on the same metrics of serious violence but over a different time period

The definitions and rationale for each outcome included in the original evaluation will be the same for this extension evaluation. The key difference in the analysis of these outcomes in this extension evaluation is that it will use data from one additional year following the end of the initial SAFE programme pilot period (i.e. 2025/26 data). Table 2 summarises the design of this strand of the extension evaluation.

Table 1: Design of the extension impact evaluation: education and serious violence outcomes

Design		QED (treatment/comparison group pre/post-test using difference-in-differences methodology)
Unit of analysis		For educational outcomes: pupils For the serious violence outcome: LAs
Number of LAs to be included in the analysis		Treatment group: 10 SAFE LAs Comparison group: 12 non-SAFE APST LAs
Number of schools to be included in the analysis		Treatment group (schools in SAFE LAs): 581 Comparison group (schools in non-SAFE APST LAs): TBC, once propensity score matching has been conducted to identify secondary schools for inclusion in the comparison group
Number of pupils included in the analysis		Treatment group (pupils participating in SAFE): 10,066 Comparison group (matched pupils in matched schools in non-SAFE APST LAs): TBC, once propensity score matching has been conducted to identify pupils for inclusion in the comparison group
Primary outcome	variable	Suspension from school
	measure (instrument, scale, source)	Number of suspensions per pupil Measured for treatment and matched comparison pupils, termly, starting from the term in which the treatment pupil had a referral to SAFE accepted and ending at the end of Spring Term 2026 Source: NPD
Secondary outcome(s)	variable(s)	Overall absence from school
	measure(s) (instrument, scale, source)	Rate of overall absence, measured as the percentage of all possible attendance sessions missed due to overall absence Measured for treatment and matched comparison pupils, termly, starting from the term in which the

		treatment pupil had a referral to SAFE accepted and ending in at the end of Spring Term 2026 Source: NPD
	variable(s)	Unauthorised absence from school
	measure(s) (instrument, scale, source)	Rate of unauthorised absence, measured as the percentage of all possible attendance sessions missed due to unauthorised absence Measured for treatment and matched comparison pupils, termly, starting from the term in which the treatment pupil had a referral to SAFE accepted and ending in at the end of Spring Term 2026 Source: NPD
	variable	Permanent exclusion from school
	measure (instrument, scale, source)	Number of pupils with permanent exclusions Measured for treatment and matched comparison pupils, termly, starting from the term in which the treatment pupil had a referral to SAFE accepted and ending at the end of Spring Term 2026. Source: NPD
	variable	Serious violence offences
	measure (instrument, scale, source)	Annual number of offences that fall under the serious violence definition in a year per 10,000 in the whole area population Measured for treatment and comparison areas, yearly, starting the financial year in which SAFE interventions began (2022/23) and ending the financial year 2025/26 Source: Police Recorded Crime and Outcomes Open Data Tables
	variable	Suspension from school
Baseline for primary outcome	measure (instrument, scale, source)	Number of suspensions per pupil Measured for treatment and matched comparison pupils, termly, for the 12 terms prior to the treatment pupil having a referral to SAFE accepted Source: NPD
	variable	Overall absence from school
Baseline for secondary outcome	measure (instrument, scale, source)	Rate of overall absence, measured as the percentage of all possible attendance sessions missed due to overall absence

		Measured for treatment and matched comparison pupils, termly, for the 12 terms prior to the treatment pupil having a referral to SAFE accepted Source: NPD
	variable	Unauthorised absence from school
	measure (instrument, scale, source)	Rate of unauthorised absence, measured as the percentage of all possible attendance sessions missed due to unauthorised absence Measured for treatment and matched comparison pupils, termly, for the 12 terms prior to the treatment pupil having a referral to SAFE accepted Source: NPD
	variable	Permanent exclusion from school
	measure (instrument, scale, source)	Number of pupils with permanent exclusions Measured for treatment and matched comparison pupils, termly, for the 12 terms prior to the treatment pupil having a referral to SAFE accepted Source: NPD
	variable	Serious violence offences
	measure (instrument, scale, source)	Annual number of offences that fall under the serious violence definition in a year per 10,000 in the whole area population Measured for treatment and comparison areas, yearly, in the four financial years prior to SAFE interventions beginning (2018/19, 2019/20, 2020/21, 2021/22) Source: Police Recorded Crime and Outcomes Open Data Tables

The approach to assessing impact on the additional secondary outcome of pupils' initial post-16 destinations

Research question

This extension evaluation includes an additional research question, set out in the table below.

Table 2: Additional research question included in this extension evaluation

Impact at the individual pupil level	Impact EQ6 – Secondary outcome What is the impact of the SAFE Taskforces programme on pupil-level initial post-16 destinations (i.e. Year 12)?
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Additional secondary outcome:

Post-16 destinations

Measure: Enrolment at a school or college in Year 12

Definition of the measure: The number of pupils enrolled in learning at a school or Further Education (FE) provider on 31st October in the year when they are aged 16, using a flag (1/0) to indicate pupils who are enrolled.

Annual data available from/to: All years up to 2025/26.

Treatment group: Pupils who have a referral to a SAFE intervention accepted who were born between 1st September 2008 and 31st August 2010. These are the two oldest age cohorts exposed to SAFE. Younger cohorts will not have turned 16 during the lifetime of the evaluation.

Comparison group: Matched pupils of the same age, who are enrolled in matched mainstream schools in comparator local authorities at the time intervention begins for their matched treatment group counterpart.

Rationale: Reducing the number of young people not in education, employment and training (NEET) is an outcome identified in the SAFE Taskforces theory of change. This measure focuses on those who make an initial transition into post-16 study. Data on longer-term outcomes would not be available within the evaluation timescales.

Design

The evaluation of impact on initial post-16 destinations (i.e. Year 12) will use an individual-level analysis, comparing differences in outcomes between pupils participating in the SAFE Taskforces programme (i.e. those who had a referral to a SAFE intervention accepted between September 2022 and March 2025, regardless of whether or not they attended a SAFE intervention) and a comparison sample of matched pupils in matched schools in non-SAFE APST areas.

Similarly to the design outlined for educational outcomes, the comparison pupil sample for the initial post-16 destinations outcome will be selected through a two-step propensity score

matching procedure: first, we will match schools participating in SAFE to similar schools in non-SAFE APST areas; second, we will match pupils participating in SAFE to similar pupils within the sample of matched comparison schools (see Analysis section below for further details of the matching process).

The estimation of impact on pupil-level outcomes will be based on a regression analysis assessing differences in outcomes between this sample of treatment pupils and comparison pupils. The analysis will compare differences in the level of outcomes between treatment and comparison pupils. It will control for differences in pupil- and school-related characteristics (listed in the Analysis section of this Study Protocol).

To give credible impact estimates, relevant differences between the treatment and comparison groups should be controlled for. It is, however, possible that limitations in the available data mean that unobservable differences (i.e. those on which we cannot gather data) remain uncontrolled for. A simple comparison of outcomes between treatment and comparison groups would then not capture solely the impact of participation in SAFE, but would also partly reflect those unobserved differences. A key challenge is that the outcome is observed in Year 12 only, preventing us from using a difference-in-differences strategy. While our initial matching approach is designed to eliminate observable, pre-existing differences, we acknowledge that our estimates could still be biased by unobservable pupil- and school-level characteristics that are not captured in our data.

To address this limitation, we will also present a robust difference-in-differences specification that leverages the school level panel data structure. This specification will use all pupils in both treated and matched comparison schools, comparing their initial Post-16 outcomes across different cohorts both before and after the introduction of the SAFE program. This difference-in-differences approach is particularly powerful because it controls for time-invariant unobserved characteristics that are constant within each school over time. The primary challenge with this specification, however, is that the treatment effect may be diluted since it includes all pupils, not just those directly targeted by the programme. Consequently, there is a risk with this technique that, due to dilution, we may not be able to identify a statistically significant or meaningful effect, even if one exists.

Table 3: Design of the extension impact evaluation: initial post-16 destinations outcome

Design	QED (treatment/comparison group post-test using matching methodology)
Unit of analysis	Pupils

Number of LAs to be included in the analysis		Treatment group: 10 SAFE LAs Comparison group: 12 non-SAFE APST LAs
Number of schools to be included in the analysis		Treatment group (schools in SAFE LAs, with pupils for whom initial post-16 outcomes can be measured): 349. Comparison group (schools in non-SAFE APST LAs): TBC, once propensity score matching has been conducted to identify secondary schools for inclusion in the comparison group.
Number of pupils included in the analysis		Treatment group (pupils participating in SAFE born between 01/09/08 and 31/08/10, i.e. those aged 16 on 31 st October of any year within the measurement period): 3,466. Comparison group (matched pupils in matched schools in non-SAFE APST LAs): TBC, once propensity score matching has been conducted to identify pupils for inclusion in the comparison group.
Secondary outcome(s)	variable(s)	Initial post-16 destinations (i.e. Year 12)
	measure(s) (instrument, scale, source)	1/0 flag to indicate pupils who are enrolled in learning at a school or Further Education (FE) provider on the 31 st October in the year when they are aged 16. Source: NPD, linked to Individual Learner Record data
Baseline for secondary outcome	variable	n/a
	measure (instrument, scale, source)	n/a

Participants

Our proposed intervention and comparison sample for the impact evaluation includes:

- All pupils participating in SAFE interventions (understood as those who had a referral to a SAFE intervention accepted) up until March 2025, who were born between 01/09/08 and 31/08/10; and a comparison group of matched pupils in matched schools in non-SAFE APST areas.

The intervention sample includes pupils who participated in SAFE when in Years 8 and 9 in mainstream secondary schools, as well as pupils who had a referral to a SAFE intervention accepted in the summer holidays (pupils participating in SAFE interventions in the summer before joining Year 8 are treated as being in Year 8, and so on for Year 9).

Selection of comparison group and identification of assumptions

The selection of the comparison sample will be achieved through propensity score matching in a two-step procedure, as described in the original SAFE Evaluation [Study Protocol](#) for the primary outcome.

Effect size calculations

We present MDES estimates for initial post-16 outcomes in Table 2 below. Estimates were calculated using PowerUp software. The number of pupils for whom initial post-16 outcomes can be measured (i.e. those born between 01/09/08 and 31/08/10), who had referrals into SAFE interventions accepted by March 2025, is 3,466. Based on this, we calculate MDES assuming an average sample size of 3.3 pupils per year group in two year-groups, resulting in 10 pupils per school. We calculate MDES based on 349 schools being allocated to the treatment arm and 349 to the control arm (which reflects the number of schools with pupils participating in SAFE for whom initial post-16 outcomes can be measured, as of March 2025).

As the analysis will use administrative data sources, we expect attrition to be low. We therefore estimate MDES on the basis of no attrition or drop out.

Based on 2021/22 NPD data collected and analysed by FFT, the intraclass correlation coefficient (ICC, i.e. the variation between schools divided by the total variation within and between schools) is less than 0.1 for post-16 outcomes. We therefore report MDES under two assumptions for the ICC: 0.05 and 0.1. Calculations assume an alpha of 0.05, power of 0.8, and the inclusion of seven school-level pre-intervention covariates: four ethnic composition groups (White, Black, Asian, and Other), the percentage of pupils eligible for Free School Meals (FSM), the percentage of pupils with English as an Additional Language (EAL), and the percentage of pupils with Special Educational Needs (SEN). We present all scenarios under the worst-case assumption that the explanatory power of the level 1 (individual) covariates is 0.1, while the level 2 (schools) covariates have zero explanatory power. These scenarios all suggest that the evaluation will be well-powered to capture effects, and meet YEF's expectation that impact evaluations should be powered to have an MDES of 0.2 or lower.

Table 2: Estimated MDES for initial post-16 outcomes

		Scenario 1: 0% attrition, $\rho=0.1$, $r_{12}=0.1$; $r_{22}=0$	Scenario 3: 0% attrition, $\rho=0.05$, $r_{12}=0.1$; $r_{22}=0$
Minimum Detectable Effect Size (MDES)		0.09	0.079
Explanatory power of covariates	level 1 (participant)	0.1	0.1
	level 2 (schools)	0.0	0.0
Intracluster correlations (ICCs)	level 2 (schools)	0.1	0.2
Alpha		0.05	0.05
Power		0.8	0.8
Average cluster size (if clustered)		10 per school	10 per school
Number of settings	Intervention	349	349
	comparison	349	349
	Total	698	698
Number of pupils	Intervention	3490	3490
	comparison	3490	3490
	total	6980	6980

Analysis

Our analysis will estimate the impact of participation in SAFE on initial post-16 destinations. All analyses will be conducted in Stata. The outlined plan refers to initial post-16 outcomes (analysis for all other outcomes will be consistent with the original [Evaluation Protocol](#)).

Our main analysis will estimate this impact by comparing outcomes between pupils who participated in SAFE and a matched comparison sample of pupils in matched schools in non-SAFE APST local authorities. We use an intention-to-treat approach, such that pupils are treated as having participated in SAFE if they had a referral to a SAFE intervention accepted between September 2022 and March 2025, regardless of whether or not they attended a SAFE intervention. Because individual-level outcomes are observed only once, at age 16, we cannot use a difference-in-differences design as we do for the primary educational outcomes, nor can we account for unobserved time-invariant pupil or school characteristics through fixed effects. Instead, this approach relies on cross-sectional comparisons of post-16 destinations between the treatment group and a matched comparator group.

The matching procedure follows the same two-step propensity score matching process we defined for the educational outcomes. First, we match schools in SAFE areas to similar schools in non-SAFE APST areas. Second, within those matched schools, we match pupils who participated in SAFE and are eligible for our analysis of post-16 outcomes (i.e. those born between 1st September 2008 and 31st August 2010) to pupils with similar observable characteristics in the comparator schools. Matching is performed separately for each outcome using the set of variables outlined in Table 3 below (these are the same as those used in the original evaluation). As a result, the comparator sample may differ across outcomes to ensure the best possible match for each specific analysis.

Table 3: NPD data used for school- and pupil-level matching, and as control variables

Variables included in school-level matching, and as controls in estimating impact on education outcomes	Variables included in pupil-level matching, and as controls in estimating impact on education outcomes	Variables included in pupil-level matching, but not as controls (as they are outcomes)	Variables included as controls in estimating impact on the serious violence outcome
• Total pupils aged 11-14 in latest available year • Percentage of pupils aged 11-14 identifying as white	• Month of birth • Age in months • Attainment at Key Stage 2 in reading and maths	• Absence rates (calculated for each year from Reception upwards): used in	• LA total population • Total pupils in LA aged 11-14 in latest available year

Variables included in school-level matching, and as controls in estimating impact on education outcomes	Variables included in pupil-level matching, and as controls in estimating impact on education outcomes	Variables included in pupil-level matching, but not as controls (as they are outcomes)	Variables included as controls in estimating impact on the serious violence outcome
• Percentage of pupils aged 11-14 who are female • Percentage of pupils aged 11-14 who are eligible for free school meals (FSM) • Percentage of pupils in state-funded education aged 11-14 with special educational needs (SEN) met by an education, health and care (EHC) plan • Percentage of pupils in state-funded education aged 11-14 with SEN not met by an EHC plan • Percentage of pupils aged 11-14 with a history of involvement in social care • Mean local authority IDACI score for pupils aged 11-14, latest available year (2019) • Variables summarising mean outcomes for each of the four pre-treatment years • Serious violence at the LA level (sourced from NPC data)	• Ethnicity • First language (English/ other) • Gender • Percentage of terms eligible for FSM • Age first identified with SEN • Percentage of terms on SEN register • Primary SEN type • Secondary SEN type • Ever in care • Ever in need • IDACI score of home postcode	• matching for the absences outcomes • Number of suspensions (calculated for each year from Reception upwards): used in matching for the suspensions outcome • Ever excluded (calculated from Reception upwards): used in matching for the exclusions outcome	• Percentage of pupils aged 11-14 identifying as white in LA • Percentage of pupils aged 11-14 who are female in LA • Percentage of pupils aged 11-14 who are eligible for free school meals (FSM) in LA • Percentage of pupils in state-funded education aged 11-14 with special educational needs (SEN) met by an education, health and care (EHC) plan in LA • Percentage of pupils in state-funded education aged 11-14 with SEN not met by an EHC plan in LA • Percentage of pupils aged 11-14 with a history of involvement in social care in LA • Mean local authority IDACI score for pupils aged 11-14 in LA, latest available year (2019)

Although this matching approach helps to reduce observable differences between the treatment and comparison groups, we acknowledge that unobserved factors at both the pupil and school level remain a source of potential bias. Referral decisions for SAFE participation are influenced by professional judgments, pupil and family attitudes, and behavioural or

relational factors not captured in the NPD. These unobservable components mean that estimated impacts may partly reflect differences unrelated to the intervention itself.

To estimate impacts, we will use the following cross-sectional model on the sample of pupils participating in SAFE and their matched comparators:

$$Y_{ist} = \beta_0 + \beta_1 SAFE_{is} + \beta_2 X_s + \beta_3 X_{it} + \tau_t + u_{ist}$$

Where Y_{ist} is the initial post-16 destination outcome for pupil i in school s in cohort t , observed once at age 16. $SAFE_{is}$ is an indicator that equals 1 if the pupil participated in SAFE, and 0 otherwise. X_s and X_{it} represent observable school-level and pupil-level characteristics used in the matching procedure and included here as controls (as per Table 3), and τ_t is the cohort fixed effect. β_1 is the coefficient of interest, capturing the estimated intention-to-treat (ITT) effect of participation in SAFE. To account for heterogeneity on the number of pupils participating in SAFE across different schools, we will use weighted least squares with weights proportional to the number of pupils in each school, so that schools with more pupils contribute appropriately. The pupil-level regressions will report standard errors clustered at the school level to account for within-school correlation of residuals and to produce valid inference when pupils per school vary.

Compared to the specification in the original evaluation protocol for educational outcomes, this specification no longer includes pupil-, school-, or term-level fixed effects, as repeated observations of outcomes are not available. Likewise, there is no baseline or follow-up period for outcome data, meaning that we cannot assess pre-intervention trends as in a panel setting.

Given these constraints, the estimate of β_1 should be interpreted with caution, as it may be influenced by unobserved pupil- or school-level factors that are not accounted for in the model.

To assess the sensitivity of our matching estimates to unobserved confounders in the initial post-16 outcome analysis, we will use Rosenbaum Bounds analysis.² This method quantifies how strongly an unobserved variable would have to influence the treatment assignment to invalidate our study's findings, providing a measure of the robustness of our results.

To address further the limitation of the cross-sectional analysis, we will also present a supplementary analysis: a robust difference-in-differences specification that leverages the

² Rosenbaum, Paul R. 2005. "Observational Study." In Encyclopedia of Statistics in Behavioral Science, ed. Brian S. Everitt and David C. Howell. Vol. 3 John Wiley and Sons

school level panel data structure. This specification will use all pupils in both treated and matched comparison schools, comparing their outcomes across different cohorts both before and after the introduction of the SAFE program. Our pre-treatment cohorts will be pupils born between 01/09/03 and 31/08/08, while our post-treatment cohorts will be those born between 01/09/08 and 31/08/10.

This difference-in-differences approach is particularly powerful because it controls for time-invariant unobserved characteristics that are constant within each pupil and school over time. The specification is defined as follows:

$$Y_{st} = \beta_0 + \beta_1 SAFE_s + \beta_2 X_s + \tau_t + u_{st}$$

Where Y_{st} is the initial post-16 destination outcome for all pupils in school s in cohort t , observed once at age 16. $SAFE_s$ is an indicator that equals 1 if the school had pupils who participated in SAFE, and 0 otherwise. X_s represents observable school-level characteristics used in the matching procedure and included here as controls, and τ_t is the cohort fixed effect. β_1 is the coefficient of interest, capturing the estimated intention-to-treat (ITT) effect of participation in SAFE. The challenge with this specification, however, is that the treatment effect may be diluted since it includes all pupils of eligible age in the intervention and comparison schools, not just those directly targeted by the programme. Consequently, due to this dilution, we may not be able to identify a statistically significant or meaningful effect, even if one exists.

Sub-group analyses

We will follow the same strategy outlined for educational outcomes in the original SAFE Evaluation Protocol, and produce sub-group analyses only for the primary outcome.

Treatment effects in the presence of non-compliance

We will follow the same strategy regarding treatment effects in the presence of non-compliance that is outlined in the original SAFE Evaluation Study Protocol. For the additional initial post-16 destination outcome, we will follow the strategy outlined there for educational outcomes and estimate an intention to treat effect on those pupils within the eligible age range who were referred to SAFE and had their referral accepted.

Missing data

As the analysis will utilise administrative data sources for primary and secondary outcomes and relevant covariates, we expect attrition to be low.

Effect size calculation

The effect size for the initial post-16 destination outcome is the cross-sectional estimate of the treatment effect divided by the population standard deviation in the outcome for all pre-treatment years (i.e. years unaffected by the treatment) in schools in the dataset combined.

$$ES = \frac{\textit{Treatment eff}}{\sigma_{pop}}$$

The lower and upper confidence intervals for the treatment effect will also be divided by the same standard deviation to produce confidence intervals for the effect size. As this is a binary outcome, risk ratios will additionally be reported to provide an intuitive measure of relative differences in probabilities.

Ethics and registration

Ethical approval for the original evaluation of the SAFE Taskforces programme was sought by the evaluation team from, and granted by, the RAND U.S. Human Subjects Protection Committee (HSPC). The HSPC ID is: 2022-N0243.

Since that time, RAND Europe has established the RAND Europe Research Ethics Committee. The evaluation team completed an Ethics Application with that Committee for this extension evaluation, and the Committee confirmed that no further ethics approval was required for this additional analysis.

Data protection

Our data protection arrangements for this extension evaluation will follow those used within the original evaluation, which are fully set out in the original SAFE Evaluation Study Protocol.

The data controllers are the DfE and RAND Europe. RAND Europe is registered with the Information Commissioner's Office (ICO), with registration number Z6947026, and is certified for adhering to ISO 9001:2015 quality management practices. University of Westminster and FFT Education Datalab are data processors. Once data are archived at the end of the study, Youth Endowment Fund will become the data controller of personal data.

The legal basis for processing special category personal data is public interest, as detailed in Article 6(1)(e) of the UK GDPR. The legal basis for processing other personal data is legitimate interest, as detailed in Article 6(1)(f) of the UK GDPR. These legitimate interests are the broader societal benefits of conducting high quality evaluation to expand the evidence base on what works to address youth violence, which may then be used to inform policy and practice. The study team processes only what is required to meet these legal bases and ensures security and safeguards are in place to protect the information.

To ensure the privacy and protection of the collected data, rigorous data protection procedures will be implemented, including:

- The data sharing protocols used for the original evaluation will be amended as needed to cover this extension period, to ensure that any sharing of data follows strict guidelines and processing is conducted in line with the agreed protocols.
- Any data transfers will be conducted using secure and encrypted channels to maintain confidentiality.
- Regular monitoring and audits will be conducted to assess the compliance of data protection procedures throughout the study. Any potential risks are promptly addressed in consultation with RAND Europe's Data Protection Officer, and necessary actions taken to mitigate them.

Timeline

Dates	Activity	Staff responsible/leading
Sep 2026 – Oct 2027	Evaluation management: Regular meetings with YEF	RAND Europe University of Westminster FFT Education Datalab
Feb 2026	Preparatory activity: Application for NPD and ILR data	FFT Education Datalab
Jan 2027- May 2027	Construction of analytical datasets: (dependent on timelines of NPD release and loading of this into SRS)	FFT Education Datalab
May 2027 – Jul 2027	Summative analysis and reporting: Impact evaluation summative analysis and report writing (dependent on timelines of NPD release and loading of this into SRS)	RAND Europe University of Westminster

		FFT Education Datalab
Aug-Oct 2027	Submission of draft evaluation report (dependent on data availability in SRS for impact analysis)	RAND Europe



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