Ministry of Housing, Communities & Local Government

Towns Fund Evaluation

Programme-level Impact Evaluation Annexes

July 2026

frontier economics

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July 2026
Ministry of Housing, Communities & Local Government

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Annex A Programme-level impact evaluation

The programme-level evaluation aims to test whether Towns Fund investments are associated with measurable changes in outcomes associated with the key evaluation questions, relative to appropriate comparators.

These evaluation questions are set out in Section 2.3.1 of the Towns Fund Evaluation Main Report. Those questions more relevant for the programme-level evaluation relate to sustainable economic growth, employment and skills, local wellbeing, digital connectivity, and local authority capability.

The following sections detail the quasi-experimental methodology underlying the programme-level impact evaluation.

This methodology consists of 2 stages:

- **impact assessment:** to quantify whether and how much impact was generated as a result of the Towns Fund investments
- **displacement assessment:** to identify whether displacement effects are likely to have materialised in the areas surrounding the Towns Fund investments

A.1 Impact assessment

The programme-level impact evaluation aims to isolate and quantify the overall impact of the projects supported by the Towns Fund.

A difference-in-differences (DiD) regression approach was identified as the most suitable method for this analysis (more information is available in the Feasibility report).[1] To achieve this, the analysis compares changes in outcomes for recipient towns between receiving funding and after the funded projects were completed, with changes over the same period in similar towns which were candidates for investment but did not receive it.

The key features of the DiD analysis are as follows, with further detail provided in the subsequent sections:

1. **Fund-specific analysis:** Separate analyses are undertaken for the Town Deals and Future High Streets Fund to reflect the differences in the nature of the funds and their allocation approaches.
2. **Outcome-specific and indicator-specific analysis:** Separate analyses are undertaken for each intended outcome, measured by a series of indicators (a separate analysis is conducted for each outcome indicator).
3. **Geographic scope of analysis:** Impacts are assessed at the town level, to reflect the programme's intended impact geography, and to capture any overlapping, interacting effects of multiple projects within towns. The only exceptions are:

[1] This method was considered feasible for a subset of short- and medium-term outcomes expected to be realised and for which data would be available in the timeframe for the evaluation (Q1 2026).

a. **Future High Streets Fund:** Footfall and associated indicators are assessed at the high street level.
b. **Town Deals:** local authority capabilities are assessed by the local authority district.

4. **Time period for analysis:** A systematic definition of the pre- and post- treatment time periods for each indicator, by selecting an indicator-specific measurement cut-off date. This cut-off date is defined by setting a time lag between project completion and measurement, allowing for the longest possible time for impacts to materialise while retaining a sufficiently large number of completed projects for quantitative analysis. In practice, longer lags increase the time available for impacts to emerge but reduce the number of projects that can be included. In contrast, shorter lags increase coverage but may capture impacts before they have fully materialised.
5. **Data available for this analysis:** Indicators were selected for this analysis based on available data with sufficient coverage, appropriate geographic granularity, and suitable publication frequency to support adequate sample sizes for analysis.

A.1.1 Fund-specific analysis

The Town Deals and Future High Streets Fund were analysed separately to account for differences in recipient selection approaches. See Annex E of the Feasibility Report for further details on the selection process for each fund:

1. **Town Deals:** This fund aimed to support broad, town-wide economic regeneration and longer-term growth, with investments spanning themes such as regeneration, transport, digital connectivity, skills and culture. Towns across England were identified and shortlisted based on 10 key metrics, and were invited to develop proposals for how funding might be allocated. Read Annex A.2 for more details on the key metrics. The final selection of towns was subject to Ministerial approval. Investment was ultimately awarded to 101 towns selected from a shortlist of 541 of the most deprived English towns.[2]
2. **Future High Streets Fund:** This fund focused on the renewal of high streets and town centres to support their long-term vitality and sustainability. Local authorities applied for funding in a competitive process. Expressions of Interest were shortlisted based on minimum eligibility criteria and business case feasibility. Investment was ultimately awarded to 72 high streets, shortlisted from over 200 proactive submissions from local councils.[3]

Similarly, the identification of comparator towns or places for each fund differs as follows:

- for **Town Deals**, comparator towns were selected from the 452 candidate towns that were shortlisted but not selected for Town Deals
- for the **Future High Streets Fund**, comparator towns were selected from the 158 candidate high streets that submitted Expressions of Interest but were not selected[4]

[2] Of the 101 initially selected for investment, 89 remain in the final sample for analysis. Projects which were cancelled, indicated as feasibility studies (intended to review the feasibility of a project and therefore not fully fledged projects in their own right), or that did not complete in time for this evaluation have been excluded.
[3] Of the 72 initially selected for investment, 60 remain in the sample. Projects which were cancelled, indicated as feasibility studies, or that were not completed in time for this evaluation have been excluded.
[4] In practice, it has not been possible to identify the geographic locations for 3 of these high streets. There were, therefore, effectively 155 comparator towns available for the Future High Streets Fund analysis

Read Annexes A.2 and A.3 for more detail on the selection of these comparators and their use in the DiD regression analysis.

A.1.2 Outcome-specific analysis

The Towns Fund projects are diverse in their activities and outcomes. The evaluation, therefore, analyses each outcome separately. To support this, projects have been suitably categorised by their intended outcomes, to ensure that relevant projects are within the scope of each analysis.

Project categorisation also mitigates the risk of multiple hypothesis testing. This refers to the risk of identifying statistically significant impacts purely by chance when conducting multiple tests and variations of the analysis. Careful categorisation of projects prior to analysis and ensuring they align with project objectives reduces the risk of selective emphasis of results. Since the analysis for each outcome is run on a different group of projects each time, with separate testing, this reduces the risk of multiple hypothesis testing as several outcomes are not assessed simultaneously across the wider sample.[5]

Local authorities that received the funding grouped their projects under 4 overarching themes based on intended outcomes to meet their Monitoring and Evaluation (M&E) reporting requirements.[6] However, the feasibility review noted that while the primary theme categorisations were a helpful starting point, substantial diversity of outcomes remained within each theme. In particular, several outcomes could be identified for projects within a single group.

The analysis, therefore, included a re-categorisation step to group interventions with similar outcomes more consistently, to ensure a robust impact evaluation. Projects were grouped under 10 new outcome categories, agreed with MHCLG during a workshop in August 2023.

The re-categorisation exercise applied machine-learning techniques, and the resulting categories were subject to extended validation with MHCLG and local authorities. The results of the validation demonstrated that the preferred model offered a robust, well-performing categorisation approach.[7] Read more details on this re-categorisation exercise in Annex C of the Interim report.

An 11th outcome, local authority capabilities, was defined only for Town Deals projects. This outcome aims to capture any improvements in the ability of local authorities to plan programmes, monitor and deliver services effectively following the training that was offered to local authorities to support the effective management and deployment of allocated

[5] In principle, the analysis could address the multiple testing issue by applying a Bonferroni adjustment. However, the analysis runs only a small number of regressions within each outcome group, and indicator-specific cut-off dates mean that tests often draw on different samples rather than repeatedly testing the same underlying data. On this basis, the analysis does not apply Bonferroni adjustments.
[6] These themes are: urban regeneration, skills and enterprise, transport, and digital connectivity.
[7] A comprehensive validation exercise was completed with local authorities from 38 towns for a sample of 124 projects across both the Town Deals and Future High Streets Fund (representing c.15% of the projects). Categorisations from the preferred model were validated in 2 ways: (i) by benchmarking them against 2 human classifications, to confirm alignment with human judgment, and (ii) by asking local authorities to review the assigned outcomes directly, to confirm whether these were correctly captured and to identify whether any relevant outcomes were omitted. The preferred categorisation model was found to perform materially better than a single informed economist, and broadly in line with an ensemble of 15 economists. Performance remained stable across the larger validation sample.

funds. This outcome is therefore assessed only for the Town Deals projects, and all projects are within the scope of this analysis.

Table A.1 presents the 11 new outcomes alongside the results of the project categorisation exercise for 799 projects.[8]

Table A1 Number of Towns Fund projects by intended outcomes

Outcome	Description	Number of projects categorised – Town Deals	Number of projects categorised – Future High Streets Fund
Business outcomes	Relating to increased turnover, investment, business creation and productivity.	251	152
Property and land use	Relating to changes in land use, reductions in vacancy rates, and increases in property prices and rents.	113	85
Business decarbonisation	Relating to reducing the intensity of GHG emissions and promoting more sustainable energy usage.	43	< 10
Job creation	Relating to the safeguarding of existing jobs and the creation of new jobs.	339	101
Employment mix	Relating to skills development and the creation of new employment types.	170	16
Local wellbeing	Relating to resident happiness or life satisfaction, and social improvements (eg crime reduction).	224	79
Social mobility	Relating to the average income of residents and the reduction in any income or social disparities.	25	< 10
Pride in place	Relating to perceptions of the local area by residents or visitors.	274	149
Physical connectivity	Relating to improvements in physical infrastructure.	133	65
Digital connectivity	Relating to improvements in digital infrastructure.	45	< 10
Local authority capabilities	Relating to changes in local strategic management and governance.	571	Not applicable

Source: *Frontier Economics, based on Towns Fund monitoring data (Round 7, available July 2025).*

Note: *A total of 799 projects were categorised for this analysis (excluding those that were cancelled or designated as feasibility studies). Since projects could be categorised against up to 3 outcomes, column totals exceed the total number of projects.*

[8] Funding was originally allocated for 863 projects (based on Towns Fund monitoring data [Round 7, available July 2025]). Of these, 64 projects were excluded from this analysis, as they were cancelled or designated as feasibility studies.

Read Annex D for more information on the detailed exercise that was conducted to identify available metrics to measure these outcomes.

A.1.3 Geographic level of analysis

The programme-level impact evaluation focused on assessing impacts at the town level, rather than at the project level. There are 2 reasons for this.

First, combining investments within towns reflects the intended outcomes of the local councils. Multiple Towns Fund projects delivered within the same town are typically intended to generate benefits for the same local area. As explained in the Feasibility report:

- Town Deals were granted at the town level, and this is the scope of impact that has been targeted by the funding[9]
- Future High Streets Fund projects mainly focused on high street improvements, although bids were assessed with wider town impacts in mind[10]

Second, within-town effects cannot clearly be separated for analysis. The impacts of projects in close proximity are likely to overlap, interact, and generate synergies. A project-level approach would therefore make it difficult to isolate and attribute changes in outcomes to individual projects. A town-level analysis provides a more robust and comprehensive assessment of the combined impacts of the Towns Fund portfolio within each town, including interdependencies between closely located initiatives.

While the analysis assesses all outcomes and their underlying indicators at the town level, some indicators were evaluated at a different geographical level. The exceptions are:

1. **Footfall and dwell time (the length of time spent by a visitor on a given high street):** These leading indicators of business outcomes are assessed for the Future High Streets Fund at the high street level. This is because these impacts are highly local and have a narrower geographic focus than the town as a whole.
2. **Local authority capabilities:** These indicators are associated with understanding potential improvements to local authority capabilities. They are assessed at the local authority level rather than at the town level, given the nature of the outcome of interest.

Read Annex B.1 for details on the geographic definition applied to these areas.

[9] Each selected town developed a Town Investment Plan (TIP) that outlined 'A vision for the town, complementing agreed or emerging local economic strategies' (Ministry of Housing, Communities and Local Government, 2019, p. 22).
[10] Future High Streets Fund investment was awarded to local authorities with the aim of 'helping local high streets evolve and adapt to these changes' (Ministry of Housing, Communities and Local Government, 2018). One local authority was also able to apply and receive funding for several high streets. The bids were assessed for funding based on the 'geographical spread of impact and wider economic considerations' (Ministry of Housing, Communities and Local Government, 2019, p. 2) among other factors.

A.1.4 Time period of analysis

DiD analysis estimates the impact of an intervention by comparing the pre-post change in outcomes for a treatment group against the pre-post change for a comparator group.

This analysis applies the following definitions:

1. **Pre-treatment period:** The latest available data point for the outcome indicator prior to March 2020 (to avoid any distortionary effects resulting from the COVID-19 period). Where monthly data are available, an additional rule is applied to ensure that the same calendar months are compared in the pre- and post-treatment periods. This ensures a like-for-like comparison and mitigates the influence of seasonal variation in the data.
2. **Post-treatment period:** The latest recorded data point for a given outcome indicator.

For example, changes in local wellbeing with respect to the number of total crime offences are assessed using Police UK data, which is published in September each year for the preceding 12 months. This analysis defines the September 2019 data set (covering the period from September 2018 to August 2019) as the pre-treatment period and the September 2025 data set (covering the period from September 2024 to August 2025) as the post-treatment period.

A.1.5 Data available for this analysis

Data availability, data quality, and treatment group size have constrained the scope of the analysis in practice. The expected timeframe for impacts to materialise has also limited the scope of the analysis.

A.1.6 Data availability and data quality

The analysis requires highly granular geographic data to identify outcomes at the town or high street level, which has posed a significant constraint. The lack of sufficiently granular data on a consistent basis, or the prohibitive cost of such data, has limited the extent of quantitative analysis possible. Read Annex D for more about the list of data sources and indicators explored for this evaluation.

TREATMENT SAMPLE SIZE

The analysis included projects based on a cut-off date for project completion. The cut-off date is set as the date that yields the longest lag between the project completion date and the latest available post-treatment observation for a given indicator.[11]

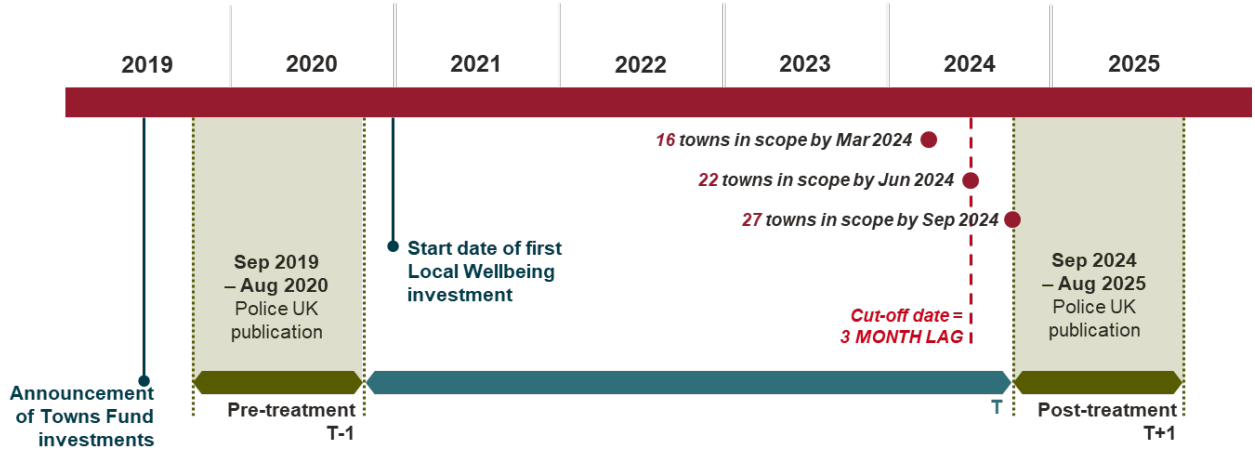
The analysis aims to apply a 6-month lag between project completion and outcome measurement where possible. Where an indicator yields fewer than 20 observations after a 6-month lag, the analysis relaxes the requirement to shorter lags in order to ensure a minimum sample size of 20 observations for estimation, decreasing to an absolute minimum of 0 months. Any samples which remain below 20 observations at this stage are excluded from the analysis. This approach reflects a trade-off between maximising

[11] While project funding may have been allocated to local authorities in 2019, many projects have taken several months to implement. This approach implies that impacts only begin to materialise once a project has been completed, and not at any point during implementation.

statistical power (through a larger sample size) and allowing sufficient time for impacts to materialise. Results based on shorter lags may therefore understate effects that emerge more gradually and should be interpreted with this limitation in mind.

For instance, Figure A.1 illustrates an example for crime indicators to measure the impacts of the programme on local wellbeing. This shows that the most recent Police UK data is available for the period September 2024 to August 2025. By this date, 16 towns had completed at least one local wellbeing project within 6 months (by March 2024), 22 towns had completed at least one local wellbeing project within 3 months (by June 2024), and 27 towns had completed at least one local wellbeing project within 0 months. The analysis, therefore, defines June 2024 as the treatment cut-off date, allowing a 3-month lag between project completion and the recording of the outcome indicator.

Figure A1 Illustrative example: Selection of cut-off date for Police UK data



Source: Frontier Economics.

Under this approach, cut-off dates for the central analysis range from 1 October 2023 to 1 May 2025. The robustness checks test the sensitivity of results to these cut-off assumptions.

TIMEFRAME FOR IMPACTS TO MATERIALISE

In practice, the elapsed time between project completion and the post-treatment observation varies across projects and indicators, reflecting differences in completion dates and data publication frequency.

In all cases, however, the cut-off dates for project completion allow a few months for impacts to materialise and become observable in the data. While this is a suitable timeframe for measuring immediate changes, the data are less likely to detect longer-term impacts, which materialise over an extended period. For instance, improvements in social mobility typically materialise through a sequence of steps (changes in skills, qualifications, work experience, job progression and earnings growth), many of which become visible over several years rather than months.

As a result, evaluations with short analysis windows can underestimate the full extent of longer-term impacts, as some effects will become observable at a later stage. Findings should therefore be interpreted as evidence on impacts observable within the available

post-completion window for each indicator, rather than as definitive evidence of longer-term effects.

Table A.2 summarises the outcomes and indicators selected for quantitative assessment, based on available indicators and a sufficient sample size to support quantitative assessment. This review identified 5 of the 11 outcomes as suitable for DiD analysis, and a further 2 outcomes as suitable for descriptive analysis only.

While these outcomes meet the minimum thresholds for inclusion, several have very few treated towns within scope. In these cases, the statistical analysis has limited power to detect effects and produces less precise estimates. Findings for these outcomes should therefore be interpreted carefully and may be less generalisable across the full set of funded places and projects.

Table A2 Preliminary review of sample sizes for quantitative analysis

Outcome	Indicator	Geographic unit of analysis	Preliminary number of treatment units available for analysis [A] – Town Deals	Quantitative analysis conducted – Town Deals	Preliminary number of treatment units available for analysis [A] – Future High Streets Fund	Quantitative analysis conducted – Future High Streets Fund
Business outcomes	Change in total number of businesses, 2019 to 2025	Town	23	DiD regression	28	DiD regression
Business outcomes	Change in business birth rate, 2019 to 2025	Town	23	DiD regression	28	DiD regression
Business outcomes	Change in total employment, 2019 to 2025	Town	23	DiD regression	28	DiD regression
Business outcomes	Change in average monthly footfall, Q3 2019 to Q3 2025 (Future High Streets Fund only)	High street	Not applicable	None	42	DiD regression
Business outcomes	Change in average monthly dwell time, 2019 to 2025 (Future High Streets Fund only)	High street	Not applicable	None	42	DiD regression
Business outcomes	Change in total monthly dwell time, 2019 to 2025 (Future High Streets Fund only)	High street	Not applicable	None	42	DiD regression
Property and land use	Average property price (12 months), 2019/20 to 2024/25	Town	<10	None	11	Descriptive analysis
Property and land use	Commercial vacancy rates	Town	16	Descriptive analysis	30	Descriptive analysis [12]

[12] In principle, it would be possible to apply DiD regression analysis for this indicator. However, the required data was made available too late for full regression analysis to be undertaken. The analysis therefore reports descriptive analysis instead.

Outcome	Indicator	Geographic unit of analysis	Preliminary number of treatment units available for analysis [A] – Town Deals	Quantitative analysis conducted – Town Deals	Preliminary number of treatment units available for analysis [A] – Future High Streets Fund	Quantitative analysis conducted – Future High Streets Fund
Job creation	Change in total employment, 2019 to 2025	Town	41	DiD regression	26	DiD regression
Job creation	Change in unemployment benefit claimant rate, Nov 2019 to Nov 2025	Town	46	DiD regression	27	DiD regression
Job creation	Change in share of part-time jobs, 2019 to 2025	Town	21	DiD regression	15	Descriptive analysis
Employment mix	Change in share of part-time jobs, 2019 to 2025	Town	20	DiD regression	<10	None
Employment mix	Change in the share of employees in the given sectors, 2019 to 2025 [13, 14]	Town	20	DiD regression	<10	None
Employment mix	Change in the concentration of sector employment shares, 2019 to 2025 [15]	Town	20	DiD regression	<10	None
Local wellbeing	Change in crime rate per 1,000 capita, 2018/19 to 2024/25 [16]	Town	22	DiD regression	16	Descriptive analysis
Digital connectivity	Percentage of residential premises with access to superfast broadband, Sept 2019 to Jan 2025	Town	18	Descriptive analysis	<10	None

[13] The sectors assessed are Retail, Accommodation, Food and beverage, Activities typically associated with economic decline (including the tobacco and vape stores, charity shops, takeaways and gambling stores) and Activities typically associated with economic improvement (including libraries, museums and sports centres).
[14] Businesses in each sector have been identified using Standard Industrial Classification (SIC) codes from Companies House. Further details on the specific classification used for each of these categories are set out in Annex E.5.
[15] Based on the Herfindahl–Hirschman Index (HHI), a common measure of market concentration. This is calculated by taking the sum of the squared employment shares, where the shares are expressed as fractions. The result is proportional to the average employment share, weighted by employment share. As such, it can range from 0 to 1, where the former represents employment spread across several sectors, and the latter a single monopolistic employer. For the purpose of this analysis, businesses have been grouped into sectors using 2-digit SIC codes.
[16] The categories of crime assessed are Total crime, Shoplifting, Theft from the person, Robbery, Burglary and Antisocial behaviour. These low-level crime categories have been selected as most likely to be affected by local improvements to towns or the high streets.

Outcome	Indicator	Geographic unit of analysis	Preliminary number of treatment units available for analysis [A] – Town Deals	Quantitative analysis conducted – Town Deals	Preliminary number of treatment units available for analysis [A] – Future High Streets Fund	Quantitative analysis conducted – Future High Streets Fund
Local authority capabilities (Town Deals only)	Change in proportion of major planning applications decided in time, Q2/Q3 2019 to Q2/Q3 2025 (Town Deals only)	Local authority district	29	DiD regression	Not applicable	Not applicable

Source: *Frontier Economics, based on Towns Fund monitoring submissions (Round 7, available in July 2025).*

Note: *[A] Counts are preliminary and do not reflect final sample sizes. Final sample sizes could at times be smaller due to additional data cleaning. See Annex E.1 for the information on the final sample sizes used for this analysis.*
[B] Preliminary counts were calculated as the number of treated units associated with each outcome that contains at least one project within the scope of this evaluation. Projects are considered in scope if they have not been cancelled, are not indicated as feasibility studies, and have been completed before the cut-off date for the relevant metric.
[C] Preliminary counts of 20 or above were considered suitable for DiD analysis. Preliminary counts above 10 but below 20 were considered suitable for descriptive analysis.

Note that the analysis assesses footfall and dwell-time indicators for the Future High Streets Fund only. Future High Streets Fund investments target high streets, which allows the analysis to define consistent impact zones and select credible comparators. By contrast, Town Deals support a diverse mix of projects with impacts that can extend across different parts of a town and do not map consistently to a single, well-defined 'impact zone' without bespoke town-by-town assessment. Comparator selection would be similarly challenging. The analysis, therefore, excludes footfall and dwell time analysis for Town Deals towns.

A.2 Impact assessment: Town Deals

This section sets out the approach used to identify and quantify the impacts of the Town Deals investments.

Read Annex A.7 for more details on the diagnostic checks used to assess the validity of the approach, and Annex A.8 for the sensitivity testing undertaken to examine how results vary under key assumptions.

A.2.1 Identifying suitable comparators

As the Town Deals were not allocated randomly, treated and non-treated towns may differ systematically in town selection or outcomes. To improve the comparability between the treated and comparison groups, the analysis uses matching and weighting to balance observed pre-treatment characteristics between the 2 groups. This allows the comparison of treated towns with comparators on a like-for-like basis, improving the credibility of estimated treatment effects.

Propensity score matching (PSM) is the primary matching approach used for this analysis. PSM matches each treated town to one or more comparator towns with a similar estimated probability of receiving Towns Fund investment, conditional on observed pre-treatment characteristics. These characteristics affect both selection into the programme and the outcome of interest.

Covariate-balancing propensity scores (CBPS) are used as a robustness check. CBPS constructs weights based on propensity scores that balance observed covariates between treated and comparator towns. Therefore, comparing PSM and CBPS results provides an assessment of whether the estimated impacts are robust to the matching method.

Annex C presents a discussion of the matching and weighting approaches considered for this analysis, alongside the rationale for the chosen methods above.

A.2.2 Identifying relevant covariates

For most regressions, there are several plausible covariates which may be useful for the matching analysis.[17] Given the relatively small number of observations relative to the number of covariates, including too many variables risks overfitting and can inflate the

[17] This is the case for all Town Deals impact analysis, excluding Local wellbeing and Local authority capabilities. LASSO-based covariate selection was not required for these regressions, as the set of candidate covariates identified for these outcomes was already sufficiently small to include directly in the specification.

standard errors of the variables of interest. To avoid this, the analysis reduces the covariate set by removing variables which add little predictive value to the model.

The analysis adopts a principled approach to covariate selection:

1. First, a shortlist of candidate covariates is identified based on economic insights and an understanding of the programme context.
2. Second, the Least Absolute Shrinkage and Selection Operator (LASSO) is applied to further reduce the shortlist to a smaller set of covariates. This improves predictive performance while limiting overfitting.

This approach provides a systematic and transparent method for covariate selection and avoids the time and subjectivity involved in iterating over multiple manual, theory-led specifications.

LASSO is a machine learning method that identifies the most relevant covariates to achieve the best predictive performance. It builds on Ordinary Least Squares (OLS) regression, a standard method that estimates coefficients by minimising the sum of squared errors by minimising the sum of squared errors. LASSO extends this approach by adding a penalty proportional to the sum of absolute values of the covariate coefficients. This penalty shrinks coefficients towards or exactly to 0, providing an objective way to select a smaller set of predictors.[18] The variables selected and excluded by LASSO are reviewed to ensure that the final selection aligns with reasonable expectations from economic theory.

The analysis applies LASSO at the first stage of the PSM, where a logistic (logit) model is used to estimate the predicted treatment propensity for both treated and comparator units.[19] In this case, LASSO applies a penalty term to the logit regression, yielding the predicted propensity scores.

The resulting coefficients from the shrinkage are systematically biased. As such, conventional standard errors and p-values from the penalised fit are not valid for inference in the usual way. Results from the LASSO-based estimation should therefore be considered purely as a predictive step in constructing the propensity score, rather than as estimates of effect sizes for individual drivers.

[18] There are several regularisation methods which might have been applied for this analysis, including ridge regression and elastic net, which apply alternative penalty structures to select covariates. LASSO was selected for pragmatic reasons; in particular, it is transparent, computationally efficient, and typically yields a relatively interpretable set of retained covariates, which supports sense-checking and explainability.

[19] Logistic (or logit) regression is a statistical technique used to estimate the likelihood of a binary outcome (in this case for example, whether or not a town is treated), as a function of one or more explanatory variables. It applies a logistic transformation so that all predicted probabilities fall between 0 and 100%.

A.2.3 Regression approach

The impact assessment for the Town Deals comprises 2 steps:

1. **Conducting the PSM:** This step identifies a suitable comparator group by first estimating propensity scores and then matching treatment and comparator units on that basis. An alternate matching approach, CBPS, is used as a robustness check.
2. **Estimating the ATT:** Comparing the pre-post change in the indicator of interest between the matched treated and comparator towns, to estimate the weighted Average Treatment Effect on the Treated (ATT). This is interpreted as the average difference in outcomes for the treated towns relative to what would have been expected for those towns in the absence of the Towns Fund.

STEP 1A: ESTIMATING PROPENSITY SCORES

In the first step of analysis, the propensity score is estimated following the equation:

$$\boldsymbol{D_i} = \boldsymbol{\alpha} + \boldsymbol{\eta X_i} + \boldsymbol{\varepsilon_i}$$

- $\boldsymbol{D_i}$**:** a binary variable which equals 1 if there is at least one Town Deals project associated with the outcome of interest in town i
- $\boldsymbol{X_i}$**:** a set of covariates which are either relevant in explaining the treatment selection D_i, or likely to influence both treatment selection and the indicator of interest Y_i. For instance, when assessing crime impacts, these may include indicators of pre-intervention labour market conditions, such as the unemployment rate
- $\boldsymbol{\varepsilon_i}$**:** heteroskedasticity-robust standard errors, to allow for non-constant variance in the error term across towns

This regression predicts each treated and comparator town's propensity to receive treatment.

STEP 1B: MATCHING BASED ON PROPENSITY SCORES

The analysis then uses matching to select one or more comparator towns for each treated town on the basis of similarity in propensity scores. The central analysis applies the following matching rules:

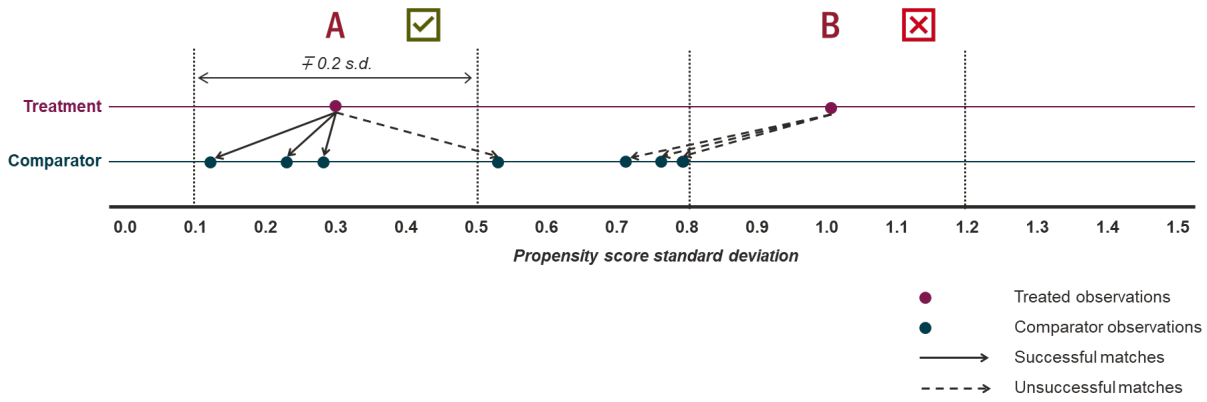
1. **1:5 'nearest neighbour' matching:** The matching allows up to 5 comparators to be matched to a given treated town based on the closeness of their propensity scores.
2. **Calliper = 0.2 standard deviations (s.d.):** The matching allows for a maximum difference of 0.2 s.d. (a commonly used value in econometric analysis) in propensity scores between the treated and comparator towns.
3. **Matching with replacement:** The matching allows one comparator town to be matched to several treated towns.

The analysis assigns weights to comparator towns based on this matching procedure. The weight reflects how many times a comparator town is matched with a treated town, and

therefore its contribution to the weighted comparison group. Unmatched comparator towns receive a weight of 0.[20]

Figure A.2 illustrates the PSM approach described. In the example, a hypothetical Town A is matched with 3 comparator towns with similar propensity scores (within 0.2 s.d.). By contrast, Town B has no suitable comparator towns within this threshold and is excluded from the matched sample.

Figure A2 Illustration of PSM matching of treated and comparator units



Source: Frontier Economics.

STEP 2: ESTIMATING THE ATT

The analysis estimates the treatment effect as the difference between the outcomes of the treated towns and the weighted average outcomes of the matched comparator towns.

In the second stage of the analysis, the ATT is estimated using the following equation:

$$\Delta \boldsymbol{Y_i} = \boldsymbol{\alpha} + \boldsymbol{\beta} \boldsymbol{D_i} + \boldsymbol{\varepsilon_i}$$

- $\Delta \boldsymbol{Y_i}$**:** the change observed in the indicator of interest for town i between the pre- and post- treatment periods
- $\boldsymbol{D_i}$**:** a binary variable which equals 1 if there is at least 1 Town Deals project associated with the outcome of interest in town i
- $\boldsymbol{\varepsilon_i}$**:** heteroskedasticity-robust standard errors, to allow for non-constant variance in the error term across towns
- $\boldsymbol{\beta}$**:** the coefficient of interest, which represents the average impact observed across towns receiving Town Deals investments, relative to the weighted comparator towns. When this coefficient is statistically significant, evidence of impact may be inferred

[20] In CBPS, comparator towns are weighted on the basis of their similarity to the treated group across the covariates included in the model. Comparator towns that more closely resemble treated towns receive greater weight, while less comparable towns receive lower weight.

The interpretation of $\boldsymbol{\beta}$, the estimated ATT, differs depending on how the outcome is defined:

1. **Where the outcome is measured in levels (ΔY_i is a change in levels):** the coefficient $\boldsymbol{\beta}$ can be directly interpreted as the additional change in the outcome level for the treatment group relative to the comparator group.
2. **Where outcome is measured in log levels (ΔY_i is a change in log levels):** the coefficient $\boldsymbol{\beta}$ may be transformed to be interpreted as a relative percentage change (growth rate) between the treatment group and the comparator group. This is given by the transformation: $e^{\boldsymbol{\beta}} - 1$.

A MODIFIED APPROACH TO ADDRESS LOW SAMPLE SIZES

The sample sizes for this analysis are relatively small because the scope is limited to towns with at least one relevant project completed within the evaluation timeframe. Read Annex A.1 for the definition of pre- and post- treatment periods.

To address this constraint, the analysis applies a modified 2-step approach in which the first and second stages are estimated on different samples:

1. **Propensity score estimation:** Uses the full sample of treated towns which received Town Deals investment, regardless of project completion dates.
2. **ATT estimation:** Uses the restricted sample of treated towns with at least one relevant project completed within the evaluation window, and compares them with their matched comparator towns over the defined pre- and post- treatment periods.

This distinction reflects the different purposes of the 2 stages:

1. **Step 1 is a selection model**, which aims to predict each town's probability of receiving treatment based on pre-treatment characteristics. As these characteristics are defined prior to treatment, they should not depend on whether projects were completed early enough to fall within the impact evaluation window. As such, using the full sample of relevant treated towns when estimating treatment propensity is appropriate.
2. **Step 2 is an impact model**, which aims to estimate the effect of treatment on observed outcomes. This requires the analysis to be restricted to towns in which at least one relevant project had been completed early enough for impacts to have had a reasonable chance to materialise in the available post-treatment data.

Removing treatment towns with project completion dates outside of the evaluation window in Step 2 also removes their matched comparator towns. As some comparator towns may have been matched to more than one treated town remaining in the sample, the comparator sample must be reweighted so that the weights correctly reflect the restricted treatment group used in the ATT estimation.[21]

This modified approach relies on an important assumption that the subset of projects completed by the cut-off date does not materially differ from the broader set of projects

[21] In PSM, matching weights reflect how much each matched comparator town contributes to the counterfactual for treated towns. Matching in this analysis takes place with replacement, such that the comparator town's total weight increases in proportion to the frequency that it is selected. When treated towns are dropped between stages 1 and 2 of the PSM, weights on the comparator towns must be updated to reflect the frequency that each comparator town is used in the restricted sample.

used to estimate propensity scores, with respect to the characteristics relevant to treatment selection and expected outcomes.[22] Subject to this assumption, estimating propensity scores on the full treated sample improves the statistical power of the first step by using a larger set of treated towns.

In general, restricting the sample between Step 1 and Step 2 can weaken balance, as original matches and weights are no longer optimised for the narrower sample. For this reason, the analysis tests the credibility of this assumption by also reviewing balance at Step 2, to compare the covariate profiles of treated towns which remain in scope of the second-stage analysis with their associated matched comparator towns. The results of these tests show that while balance may be slightly weaker for individual covariates than after Step 1, matching remains valuable. This is because the wider sample remains more balanced than the unmatched comparator pool, and the model continues to deliver materially better balance in the propensity score and covariates than the unmatched comparator pool.

A.3 Impact assessment: Future High Streets Fund

This section sets out the approach used to identify and quantify the impacts of the Future High Streets Fund investments.

Read Annex A.7 for more details on the diagnostic checks used to assess the validity of the approach, and Annex A.8 for information on the sensitivity testing undertaken to examine how results vary under key assumptions.

A.3.1 Identifying suitable comparators

As the Future High Streets Fund was not allocated randomly, treated and non-treated towns may differ systematically in both town selection and subsequent outcomes. It would therefore be reasonable to consider applying matching or weighting methods.

Future High Streets Fund allocations were based on a qualitative assessment of Expressions of Interest submitted by local authorities, rather than specified selection criteria. Implementing PSM for this fund, therefore, risks omitting important selection drivers not captured in the available data. This weakens the ability of any matching or weighting technique to mitigate selection bias.

In addition, matching and weighting methods rely on having a sufficiently large and diverse pool of comparator towns to construct credible counterfactuals for the treated towns. As only 155 comparator towns are available (compared to 452 for Town Deals), the comparator pool is significantly limited. This can make it harder to find close matches and achieve adequate overlap in observed characteristics (or in propensity scores) between treated and comparator groups.

[22] Individual projects may differ in the scale of impacts they generate. Projects completed by the cut-off date may on average be smaller or quicker to deliver, and may therefore generate smaller impacts than projects which are due to complete later. However, these differences relate to post-award delivery and impact intensity, rather than to pre-treatment characteristics used to estimate propensity scores. They should therefore not, in themselves, bias the propensity score model, although they may affect the interpretation and generalisability of estimated impacts.

For these reasons, a direct DiD approach that uses the full pool of comparator towns is the favoured method.

By using all available comparator units, this specification maximises sample size and coverage, thereby supporting more precise estimation and strengthening the extent to which the findings may be generalised for the Towns Fund. However, this approach relies on the parallel trends assumption holding across a broad and potentially heterogeneous set of comparator towns. By contrast, the Town Deals analysis, which applies PSM before assessing impact, prioritises comparability by improving covariate balance between the treated and comparator towns. This can reduce bias arising from observable differences, but can also reduce the effective sample size, which can limit the ability to generalise findings based on the subset of towns which are matched. Differences in estimated impacts between the 2 funds may therefore reflect genuine programme effects, or they may arise from these methodological differences. As such, results from the 2 analyses are not directly comparable and should be considered with this caveat in mind.

A.3.2 Identifying relevant covariates

The analysis identified a large set of plausible covariates for the Future High Streets Fund regressions. To reduce this set to a smaller set of relevant comparators, the analysis used a double-selection (double-LASSO) method, which involves 2 rounds of variable selection.

The following steps are taken:

1. A LASSO regression of the treatment indicator on the full set of covariates, to identify covariates that are important treatment predictors.
2. A LASSO regression of the indicator of interest on the same set of covariates, to identify those covariates which are important predictors of the outcome.
3. A DiD outcome regression on the treatment indicator and the union of covariates selected in steps 1 and 2.

Applying the LASSO approach directly for covariate selection would not be appropriate for inferring causality.[23] This is because penalised selection may exclude the most important confounders, which are associated with both treatment assignment and the outcome, or the treatment dummy, creating omitted variable bias.

The double-selection approach, therefore, helps to reduce dimensionality and lowers the risk of inadvertently omitting variables relevant to either the outcome or treatment assignment.

[23] For instance, see Belloni, Chernozhukov and Hansen (2014) Inference on Treatment Effects after Selection among High-Dimensional Controls, and Urminsky, Hansen and Chernozhukov (2016) Using double-lasso regression for principled variable selection.

A.3.3 Regression approach

The DiD equation for the Future High Streets Fund analysis is:

$$\Delta \boldsymbol{Y_i} = \boldsymbol{\alpha} + \boldsymbol{\beta D_i} + \boldsymbol{\eta X_i} + \boldsymbol{\varepsilon_i}$$

- $\Delta \boldsymbol{Y_i}$**:** the observed change in the indicator of interest for town i between the pre- and post- treatment periods
- $\boldsymbol{D_i}$**:** a binary variable which equals 1 if there is at least 1 Future High Streets Fund project associated with the outcome of interest in town i
- $\boldsymbol{X_i}$**:** the union of covariates identified from the double-LASSO approach
- $\boldsymbol{\varepsilon_i}$**:** heteroskedasticity-robust standard errors, to allow for non-constant variance in the error term across high streets
- $\boldsymbol{\beta}$**:** the coefficient of interest, which gives the average impact observed across towns receiving the Future High Streets Fund investments, relative to all comparator towns. Evidence of impact can be inferred when this coefficient is statistically significant.

The interpretation of $\boldsymbol{\beta}$, the estimated ATT, differs depending on how the outcome is defined:

1. **Where outcome is measured in levels (**$\Delta \boldsymbol{Y_i}$ **is a change in levels)**, the coefficient $\boldsymbol{\beta}$ can be directly interpreted as the additional change in the outcome level for the treatment group relative to the comparator group.
2. **Where outcome is measured in log levels (**$\Delta \boldsymbol{Y_i}$ **is a change in log levels)**, the coefficient $\boldsymbol{\beta}$ may be transformed to be interpreted as a relative percentage change (growth rate) between the treatment group and the comparator group. This is given by the transformation: $e^{\boldsymbol{\beta}} - 1$.

A.4 Displacement assessment

Observed changes in activity at Towns Fund locations may have resulted from displacement from other locations rather than from the activity itself. If this occurs, any estimated impacts might be overestimated. It is therefore important to assess displacement effects to evaluate the additionality of any observed changes and to identify possible distributional consequences.

This analysis does not seek to quantify the extent of displacement, but intends to test whether displacement effects are likely to have occurred. A full quantification of displacement would require a substantially more detailed modelling exercise and is beyond the scope of this evaluation. The purpose of this analysis is therefore narrower: to determine whether the observed pattern of changes is consistent with possible displacement effects, and hence whether displacement should be considered when interpreting the impact findings.

The analysis is applied only where the economic rationale suggests displacement is plausible. It is most relevant if funded projects change the relative attractiveness of the area, such that gains in one area may be offset by losses elsewhere. Some outcomes of interest in this analysis are not expected to generate meaningful displacement, because they primarily increase underlying capacity or service quality without drawing activity away from other areas. For instance, improvements in local authority capabilities (eg approvals

of major planning applications) in one area are unlikely to affect the capabilities of neighbouring local authorities.

For outcomes where displacement is considered plausible, displacement is assessed using one of 2 approaches, depending on the geographic level of the indicator of interest:

1. **Concentric rings:** Displacement is assessed for most indicators based on proximity to the funded project locations within the treated town or high street.
2. **Neighbouring high streets:** Footfall displacement for the Future High Streets Fund is assessed for up to 3 neighbouring high streets around the treated high street, selected based on the level of co-visitation observed with the treated high street.[24]

Read Annex E for the full list of outcomes for which displacement analysis has been conducted for this evaluation.

A.4.1 Concentric rings

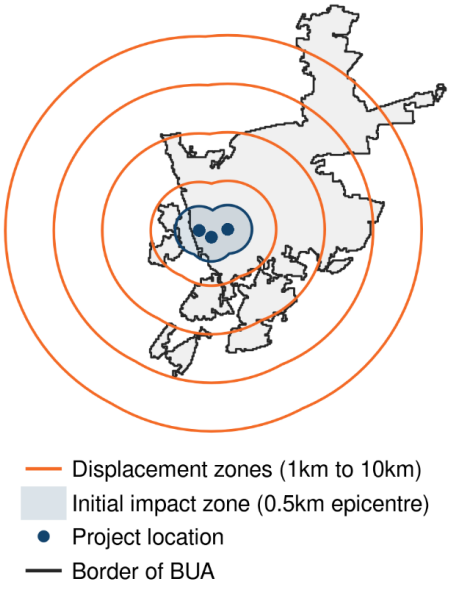
The concentric rings approach is a common method of investigating displacement (and wider spatial spillovers).[25] This approach estimates impacts across a series of distance bands or concentric rings around the intervention area. It assesses how the impact profile varies with proximity to the impact location and changes in the centre.

For this evaluation, 11 shapes are drawn, spanning a total of 10km surrounding the project locations. In particular, a central impact zone with a width of 0.5km is drawn around the project locations. This is followed by 10 further concentric rings, where the first ring is 0.5km wide, and the following 9 rings are each 1km wide.

Figure A.3 uses the example of King's Lynn to illustrate the concentric rings approach for 3 Town Deals local wellbeing project locations within a single town, comprising a single impact zone. Four concentric rings are drawn around the project locations, for illustrative purposes.

[24] Place Informatics defines the co-visitation between 2 high streets i and j as the proportion of high street i's primary catchment population that also visits high street j.
[25] For example, this approach was used in Gibbons et al (2021), which tested impacts on local regeneration projects using the distance from the nearest site.

Figure A3 Illustrative concentric rings for displacement analysis: Town Deals projects (local wellbeing) in King's Lynn



Source: Frontier Economics, based on project locations available in Towns Fund monitoring data (Round 7, available July 2025).

Annex B provides further details on the definition of these impact and displacement boundaries.

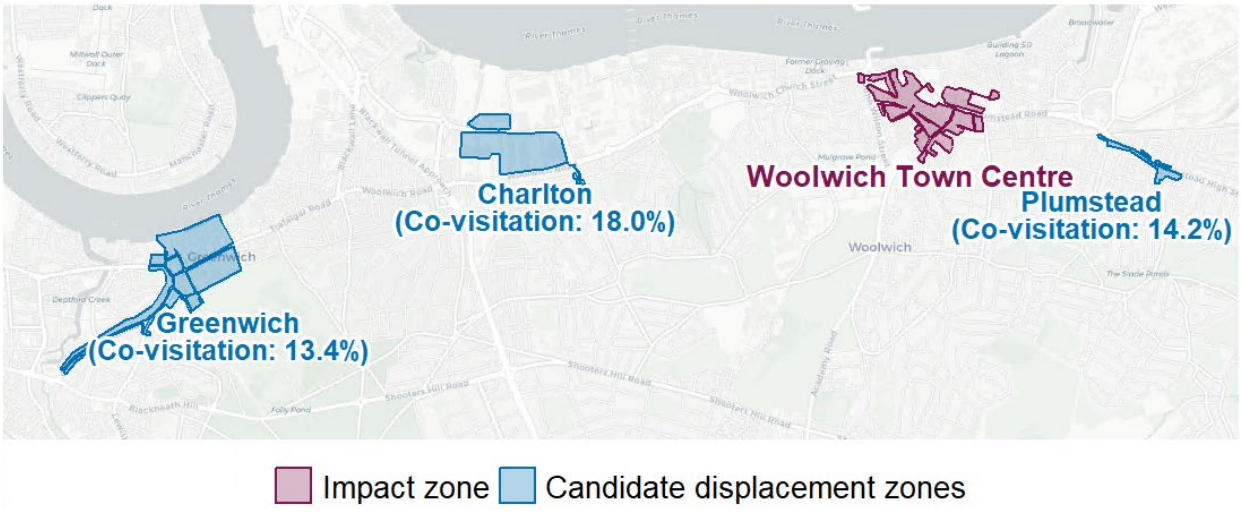
A.4.2 Neighbouring high streets

The availability of highly granular data on high street footfall (and related statistics) has enabled a more focused, localised assessment of displacement.

In this analysis, footfall displacement has been tested directly by assessing footfall data for up to 3 candidate displacement high streets surrounding a treated high street.

Figure A.4 uses the example of Woolwich Town Centre to illustrate the neighbouring high streets approach. The candidate displacement zones identified for Woolwich Town Centre are those with the highest co-visitation rates: Charlton (18.0%), Plumstead (14.2%), and Greenwich (13.4%).

Figure A4 Illustrative candidate high street approach for displacement analysis



Source: Frontier Economics, based on shapefiles and co-visitation information provided by Place Informatics.
Note: Based on co-visitation data calculated over the period March 2019 to December 2019.

Annex B.1 provides further details on the selection of these displacement high streets.

A.5 Displacement assessment: Concentric rings

This section outlines the approach for identifying correlated outcomes between the central impact zone and surrounding areas, as defined by the concentric rings approach.

This approach applies to all indicators assessed for the Town Deals and Future High Streets Fund analysis, except footfall.

A.5.1 Establishing correlation

This approach aims to identify correlations between any changes observed in the central impact zone and in the surrounding concentric rings.

A statistically significant positive correlation between outcomes in the central impact zone and those in the outer rings indicates a positive spillover effect from the central impact zone into the surrounding rings. A statistically significant negative correlation indicates that changes in the central impact zone are associated with displacement effects in the surrounding area.

A.5.2 Identifying relevant covariates

For the majority of regressions, the analysis identified a large set of plausible covariates for the concentric rings analysis. [26] To reduce this to a smaller set of relevant controls, the analysis applies a double-selection (double-LASSO) method, which involves 2 rounds of variable selection (applied in the same way as for the Future High Streets Fund footfall impact analysis; read Annex A.3 for details).

This method is more suitable than a direct LASSO approach since the estimation method consists of a single-stage regression to estimate the correlation between the central impact zone and the surrounding concentric rings.

The following steps are taken:

1. A LASSO regression of the distance variable on the full set of covariates, to identify covariates that change with proximity to the central impact zone. This measure captures the correlation between each surrounding concentric ring and the central impact zone.
2. A LASSO regression of the indicator of interest on the same set of covariates, to identify those covariates which are important predictors of the outcome.
3. A correlation regression (estimated by OLS), including the distance variable and the union of covariates selected in steps 1 and 2.

This aims to reduce the risk of omitting important confounders that are associated with both the outcome and the distance variable.

[26] This is the case for all concentric circles analysis, excluding local wellbeing and local authority capabilities. LASSO-based covariate selection was not required for these regressions, as the set of candidate covariates identified for these outcomes was already sufficiently small to include directly in the specification.

A.5.3 Regression approach

The displacement analysis regresses changes observed in the outer rings on changes observed in the central impact zone.

This is estimated using the following specification:

$$\Delta \boldsymbol{Y_{ir}} = \boldsymbol{\alpha} + \boldsymbol{\beta dist_i} + \boldsymbol{\gamma}(\boldsymbol{dist_i} * \Delta \boldsymbol{Y_{i0}}) + \boldsymbol{\eta X_i} + \boldsymbol{\varepsilon_{ir}}$$

- $\Delta \boldsymbol{Y_{ir}}$**:** the change observed in the indicator of interest for town i in radius r between the pre- and post- treatment periods, where $r = 0$ reflects the central impact zone, and r values between 1 and 10 reflect each successive ring surrounding this central impact zone
- $\boldsymbol{dist_i}$**:** a discrete variable reflecting the inverse linear distance from the project location, taking integer values from 1 to 10, in intervals of 1km. That is, the first concentric ring outside of the central impact zone is given a value of 10, while the last concentric ring is given a value of 1
- $\boldsymbol{X_i}$**:** the union of covariates identified from the double-LASSO approach
- $\boldsymbol{\varepsilon_{ir}}$**:** clustered standard errors, to account for correlation across concentric zones within the same town
- $\boldsymbol{\beta}$**:** the marginal effect of an additional unit of distance from the impact centre
- $\boldsymbol{\gamma}$**:** the coefficient of interest, which captures the marginal effect on the surrounding rings associated with changes in the outcome observed at the impact zone

LINEAR SPATIAL DECAY

This specification imposes a linear relationship between the centre and outer bands. $\boldsymbol{\beta}$ captures how average changes vary with distance from the centre, and the coefficient on the interaction term allows the association between changes in the impact zone and those in surrounding rings to vary in proportion to distance. The specification, therefore, relies on the assumption that any displacement or spillover effect changes in proportion with distance from the centre. This means that, for example, the ring closest to the centre is assumed to experience 10 times the effect of the outermost ring.

This keeps the model straightforward and reduces the risk of overfitting, given the limited number of rings and observations per town. However, it does not allow for more complex patterns, such as spillover effects that may be strongest at intermediate distances or that fade unevenly. This means that the results should be read as an average effect that fades proportionally with distance, rather than a detailed characterisation of how effects vary at each distance.

EXOGENEITY ACROSS RINGS

It is possible that exogenous factors such as local economic conditions, policing activity, or seasonal changes may affect both the centre and surrounding areas differently. Movements in outer rings may also influence changes in the centre. As a result, $\boldsymbol{\gamma}$ should be interpreted as capturing co-movement consistent with displacement or spillovers, but cannot be taken as definitive causal evidence of displacement between the impact zone and surrounding rings.

A.6 Displacement assessment: Neighbouring high streets

This section sets out the approach used to identify correlated outcomes between the central impact zone and surrounding areas, as defined by highly co-visited high streets.

This approach is relevant only for footfall analysis in the Future High Streets Fund.

A.6.1 Establishing correlation

This analysis examines the correlation between changes observed in Future High Streets Fund high streets and those in their neighbouring, highly co-visited high streets.

A statistically significant positive correlation between outcomes observed in the treated high street and those observed in neighbouring high streets indicates a positive spillover effect, while a statistically significant negative correlation indicates that changes in the treated high street are associated with displacement effects on neighbouring high streets.

A.6.2 Identifying relevant covariates

The analysis identified a large set of plausible covariates for the Future High Streets Fund regressions. To reduce this to a smaller set of relevant controls, the analysis used a double-selection (double-LASSO) method, which involves 2 rounds of variable selection.

The following steps are taken:

1. A LASSO regression of the co-visitation variable on the full set of covariates, to identify covariates that change with co-visitation with the central impact zone. This measure can be considered a proxy for the exposure of the candidate displacement high street to the treated high street.[27]
2. A LASSO regression of the indicator of interest on the same set of covariates, to identify those covariates which are important predictors of the outcome.
3. A correlation regression (estimated by OLS), including the co-visitation metric and the union of covariates selected in steps 1 and 2.

This approach aims to reduce the risk of omitting important confounders which are associated with both the outcome and the co-visitation metric.

[27] Annex B provides more information on co-visitation and how it is measured.

A.6.3 Regression approach

To identify the correlation between footfall changes observed on the treated Future High Streets Fund high street and its neighbouring high streets, the analysis uses the following equation:

$$\Delta \boldsymbol{Y_j} = \boldsymbol{\alpha} + \boldsymbol{\beta covisit_{ij}} + \boldsymbol{\gamma}\left(\boldsymbol{covisit_{ij}} * \Delta \boldsymbol{Y_i}\right) + \boldsymbol{\eta X_j} + \boldsymbol{\varepsilon_j}$$

- $\Delta \boldsymbol{Y_i}$**:** the change in the average monthly footfall observed on the Future High Streets Fund high street
- $\Delta \boldsymbol{Y_j}$**:** the change in the average monthly footfall observed on a neighbouring high street or candidate displacement zone
- $\boldsymbol{covisit_{ij}}$**:** the proportion of co-visitation observed between high streets i and j, which is the proportion of the catchment population for the Future High Streets Fund high street i which also visits high street j
- $\boldsymbol{X_j}$**:** the union of covariates identified from the double-LASSO approach
- $\boldsymbol{\varepsilon_j}$**:** clustered standard errors, to account for correlation across displacement high streets potentially affected by the same treated high street
- $\boldsymbol{\gamma}$**:** the coefficient of interest, which reflects the marginal impact of footfall changes in the treated town on the footfall observed on neighbouring high streets surrounding the treated high street

A.7 Model diagnostics and tests

A.7.1 Checking coverage of the analysis

The final analysis covers only a subset of funded projects for each outcome. This is because it includes only projects completed by the relevant cut-off dates and, where PSM is applied, further excludes treated towns without suitable comparators. Reporting the number of projects and places retained at each stage is therefore essential for understanding the coverage of the analysis and the extent to which the results may be generalised to the wider programme.

For the Town Deals impact assessment, these checks capture:

1. **Total:** The total number of treated towns (and corresponding number of projects) available in the Towns Fund monitoring data (Round 7). The total number of available comparator towns for the analysis is also reported.
2. **Total available for Stage 1:** The total number of treated towns (and corresponding number of projects) available in the Towns Fund monitoring data (Round 7), after data cleaning. The count of available comparator towns after data cleaning is also reported. These are the towns which are in the scope of the Stage 1 analysis.
3. **Total available for Stage 2:** The total number of treated towns (and corresponding number of projects) available in the Towns Fund monitoring data (Round 7), after the exclusion of cancelled projects, incomplete projects, and feasibility studies. The total number of available comparator towns for the analysis is also reported. These are the towns which are in the scope of the Stage 2 analysis.

4. **Stage 1 (PSM) counts:** The total number of towns (and corresponding number of projects) available for PSM analysis, with at least one matched comparator town.[28] The number of comparator towns remaining after matching is also reported.
5. **Stage 2 (ATT) counts:** The total number of matched treated towns (and corresponding number of projects) after the exclusion of cancelled projects, incomplete projects and feasibility studies, that include at least one project that was completed within the relevant cut-off date. The count of matched comparator towns for this retained sample of treated towns is also reported.

For the Future High Streets Fund impact assessment, these checks capture:

1. **Total:** The total number of treated towns or high streets (and corresponding number of projects) available in the Towns Fund monitoring data (Round 7). The total number of available comparator towns or high streets for the analysis is also reported.
2. **Total available for DiD:** The total number of treated towns (and corresponding number of projects) available in the Towns Fund monitoring data (Round 7), after the exclusion of cancelled projects, incomplete projects, and feasibility studies. The total number of available comparator towns or high streets for the analysis is also reported. These are the towns which are in the scope of the DiD analysis.
3. **DiD counts:** The total number of treated towns or high streets (and corresponding number of projects) with at least one project that was completed within the relevant cut-off date. The total number of comparator towns or high streets is also reported.

For the Town Deals displacement analysis, the number of towns used for the analysis is the same as the ATT counts from the impact assessment.

For the Future High Streets Fund displacement analysis, the number of towns used for the analysis are the same as the DiD counts from the impact assessment.

A.7.2 Testing the credibility of the counterfactual

The impact analysis set out in this annex relies on a credible counterfactual to ensure that any results may be interpreted as directly attributable to the Towns Fund investments. Estimated impacts are only meaningful if the comparator group provides a plausible representation of what would have happened in the absence of the intervention.

The following checks help validate the chosen analytical approach and support robust interpretation of the results.

COVARIATE BALANCE: ASSESSING SIMILARITY BASED ON PRE-TREATMENT CHARACTERISTICS

This analysis uses balancing tests to assess the statistical difference in the covariate means of pre-treatment characteristics between the treated and comparator towns. Good balance is achieved when the absolute standardised mean difference between the 2 groups is not statistically significant.

[28] In this analysis, treated towns may be matched with up to 5 comparator towns with propensity scores within 0.2 s.d. of each other. Matching may take place with replacement, where one comparator town may be matched with several treated towns.

While relevant to both impact analyses, this test is particularly helpful for assessing the quality of the matched/weighted model in the Town Deals impact analysis. In successful matching, the matched comparator group closely resembles the treated group.

PROPENSITY SCORE BALANCE: ASSESSING SIMILARITY IN THE PROPENSITY OF TREATMENT SELECTION (PRIOR TO TREATMENT ASSIGNMENT)

Balancing tests may also be used to assess statistical differences in propensity scores between treated and matched comparator towns.

This applies only to analyses that include matching or weighting. Propensity score distributions for both treatment towns and the matched comparators are visually assessed to confirm that the matching has improved comparability between the groups.

PARALLEL TRENDS: ASSESSING SIMILARITY BASED ON TRENDS IN PRE-TREATMENT CHARACTERISTICS

Parallel trends test the core DiD assumption that, absent the intervention, outcomes in the treated towns would have followed the same trajectory as outcomes in the comparator towns. If the treatment and comparator areas display similar pre-intervention trends, it is reasonable to assume that these trends would have continued without the intervention. Under this assumption, any divergence observed in the trajectories of the 2 groups following the intervention may be attributed to the programme. In this evaluation, the pre-treatment period is 2019, the time before the selection of places for the funds. The post-treatment period is 2024 or 2025 (depending on data availability). This means there is a substantial gap of at least 4 years between the pre- and post-treatment periods.

In this evaluation, therefore, the credibility of this assumption is materially weakened by this substantial gap. This interval includes various external events which affected local communities and economies. Notably, the COVID-19 pandemic and recovery period occurred between 2020 and 2023. It imposed significant, long-lasting shocks on local economies and communities due to the health implications it caused and the extreme economic and social restrictions it imposed. This gap also included the UK’s exit from the European Union, following a long transition period which concluded on 31 December 2020. Additional unobserved shocks may also have occurred over this period. Outcomes may therefore have moved substantially between 2019 and the point of intervention.

The effects of these events are unlikely to have been uniform across places. Different towns and high streets were exposed in different ways, depending on factors such as sectoral composition, pre-existing deprivation, reliance on town-centre activity, or local response patterns. As such, any pre-treatment parallel trends are likely to provide weak evidence on how treated and comparator towns would have evolved over the post-treatment period in the absence of investment.

The parallel trends evidence for this evaluation should therefore be interpreted as supportive, but not conclusive, since it cannot rule out the possibility that treated and comparator areas experienced different shocks or recovery paths during the intervening period. Estimated impacts should be interpreted cautiously, particularly where there are strong reasons to think that pandemic effects may have varied systematically across

places relevant to the outcome being assessed. This limitation applies regardless of whether the statistical tests formally confirm or reject the existence of parallel trends.[29]

The evaluation uses an event-study regression to assess whether treated and comparator areas exhibited similar trends before the intervention. This approach estimates difference-in-differences effects between the 2 groups at several points in time leading up to the intervention. If the estimated differences before the intervention are small and jointly insignificant, this supports the view that the groups were on broadly parallel trends prior to investment.

This test is performed as part of the main analysis for the Future High Streets Fund when at least 2 pre-treatment data points are available for the outcome variable. This test is not conducted for the Town Deals, as comparability between the treated and comparator towns is already improved through the matching stage, and parallel trends testing would provide limited additional assurance, given the limitations set out above. Read Annex E for a full list of technical specifications and related testing.

A.7.3 Testing match quality

The credibility of any matching approach depends on whether the matched comparator provides a plausible counterfactual for the treated towns. Poor-quality matches can lead to biased effect estimates if important differences between treated and comparator towns are not controlled for. Further, low overlaps can mean that a small number of comparator towns disproportionately influence the results.

The following checks help ensure the analysis is based on credible like-for-like comparisons and make clear when a small, unrepresentative subset of covariates is driving results.

These tests have been run only for the Town Deals impact analysis, where matching is a feature of the quasi-experimental approach.

COMMON SUPPORT: TO ENSURE THERE ARE SUITABLE MATCHES AVAILABLE FOR TREATED TOWNS

Credible estimates require sufficient overlap in the treatment propensity of both treated and comparator towns. Checking common support is therefore essential when applying matching or weighting methods. Where overlap is low, weighting techniques may assign extreme weights to some comparator towns. In this case, a small number of comparator towns may influence the analysis, reducing the credibility of the estimated effects.

For the Town Deals analysis, overlap can be tested by plotting the distribution of propensity scores.

[29] This approach is consistent with wider evaluation practice; other post-COVID-19 evaluations commonly treat the period between 2020 and 2021 as a structural break and avoid using the pandemic period to validate pre-trends. See for instance Fend and Bilinski (2024) Parallel Trends in an Unparalleled Pandemic Difference-in-differences for infectious disease policy evaluation and Callaway and Li (2023) Policy evaluation during a pandemic.

This analysis tests overlap in 2 ways:

1. **Before and after matching:** to assess the overlap between treated and comparator towns. Limited overlap in parts of the distribution indicates that like-for-like comparisons may not be feasible for some treated towns.
2. **Applying restrictions:** to assess whether imposing common support restrictions and project completion date restrictions have caused changes in the composition of the treated group systematically, for observations remaining in scope after the project completion date.

MATCH STATISTICS: TO MEASURE MATCH EFFICIENCY AND STATISTICAL POWER

Several simple statistics can inform match quality and provide supplemental insights into overlap, such as the match rate (the proportion of treatment towns successfully matched) and the drop rate (the proportion of treated towns that are not matched).

OFF-SUPPORT CHECKS: TO REVIEW POTENTIAL SELECTION BIAS

The characteristics of off-support treated observations should also be assessed. This helps verify that there are no systematic differences between the off-support-treated observations and the treated towns retained in the analysis, as such differences may introduce bias.

The Town Deals impact analysis conducts these checks at 2 points:

- after the Stage 1 PSM, to assess any treated towns that fall outside common support and are excluded
- during the Stage 2 ATT, to assess the treated towns remaining in scope of the analysis after excluding towns in which projects were completed too late for impacts to be observed in the available data

A.8 Sensitivity testing

Sensitivity testing assesses the robustness of the central specification by varying key parameters or the scope of the treated sample. These tests indicate the extent to which estimated impacts are sensitive to model design and underlying assumptions.

Where appropriate, the analysis triangulates evidence across a range of reasonable specifications to assess whether treatment effects remain consistent.

Tests for Town Deals impact assessment include:

1. **Testing the matching parameters:** a more stringent calliper of 0.1 s.d. instead of the 0.2 s.d. used in the central model. This imposes closer matches between treated and comparator towns, but recognises that this may increase off-support and reduce the size of the matched sample.
2. **Testing the matching approach:** using CBPS weighting instead of the PSM, to test the robustness of the analysis to the choice of balancing method.
3. **Testing outlier treatments:** the central approach caps values below the 0.5th and above the 99.5th percentiles, to retain all observations while limiting the influence of extreme values. Sensitivity checks test the following alternate treatments:

    a. **No outlier treatment:** retains all values, uncapped, assuming that extreme values reflect true changes in the indicator.
    b. **Dropping outliers:** removes extreme values entirely, to assess impact under a more conservative assumption.
4. **Testing the scope of the sample:** later cut-off by applying shorter lags between project completion and outcome measurement than those used in the central approach. The central approach uses a cut-off date that aims to apply a 6-month lag (where this is not possible, it uses the largest achievable lag to retain a sufficient sample size for analysis).

Tests for Future High Streets Fund impact assessment and Future High Streets Fund footfall displacement assessment (based on the neighbouring areas approach) include:

1. **Testing outlier treatments:** the central approach caps values below the 0.5th and above the 99.5th percentiles, to retain all observations while limiting the influence of extreme values. Sensitivity checks test the following alternate treatments:
    a. **No outlier treatment:** retains all values, uncapped, assuming that extreme values reflect true changes in the indicator.
    b. **Dropping outliers:** removes extreme values entirely, to assess impact under a more conservative assumption.
2. **Testing the scope of the sample:** later cut-off by applying shorter lags between project completion and outcome measurement than those used in the central approach. The central approach uses a cut-off date that aims to apply a 6-month lag (where this is not possible, it uses the largest achievable lag to retain a sufficient sample size for analysis).

Tests for the Town Deals and Future High Streets Fund non-footfall displacement assessments (based on the concentric rings approach) include:

1. **Testing the scope of the sample:** later cut-off by applying shorter lags between project completion and outcome measurement than those used in the central approach. The central approach uses a cut-off date that aims to apply a 6-month lag (where this is not possible, it uses the largest achievable lag to retain a sufficient sample size for analysis).
2. **Testing the extent of exposure:** smaller radius by reducing the span of concentric rings to 3km or 5km. The central approach tests displacement over a total area of 10km around the project locations (where this is not possible due to poor data quality, the central model tests displacement over a 5km area).

A.9 Limitations

This section sets out the main limitations of the programme-level impact evaluation and the implications for interpreting the findings. These limitations arise from both the design and data constraints of the current evaluation and from broader challenges inherent in place-based impact analysis.

Taken together, they do not invalidate the analytical approach. They do, however, affect the strength of causal interpretation, the coverage of the analysis, and the extent to which findings can be generalised across the full set of Towns Fund projects and places.

A.9.1 Technical limitations

LIMITED ANALYTICAL COVERAGE

The coverage of the programme-level evaluation across the 11 outcomes of interest has been limited. This constrains the strength and generalisability of the findings of this analysis.

There are 4 drivers for this:

- limited sample size for analysis
- incomplete coverage of funded projects
- lack of data availability
- low visibility of long-term impacts

Limited sample size for analysis
The analysis necessarily focuses on the subset of places where at least one relevant project was completed within the evaluation window. Towns without a completed project were therefore excluded, reducing the number of treated places available for quantitative analysis.

Across all outcomes, the resulting smaller sample sizes reduce statistical power and, therefore, limit the ability of the analysis to detect statistically significant impacts, particularly where effects are modest, variable across places, or still emerging. As a result, the lack of statistically significant findings in this evaluation should not be interpreted as evidence of no impact. Smaller regression samples require larger effect sizes to be detected as statistically significant by the analysis.

In addition, to maximise the available sample for analysis, the analysis applies relatively short lags between project completion and indicator measurement in some cases. This reduces the time available for impacts to materialise and be reflected in the data, and may therefore understate true effects, particularly where outcomes materialise gradually. Smaller observed effect sizes also reduce the likelihood of detecting statistically significant effects, which is especially relevant given the relatively small sample sizes for several outcomes.

Incomplete coverage of funded projects
Approximately two-thirds of projects were not completed in time for this evaluation. As a result, the evidence base is drawn from an incomplete subset of the overall programme, which limits the extent to which any findings from this evaluation may be generalised to the full set of funded projects and places. In particular, projects with longer delivery timelines, which are more likely to fall outside the current evaluation window, may generate different, potentially larger, more complex, or more long-lasting impacts.

For some outcomes (eg Business decarbonisation and Social mobility), sample sizes were too small to support quantitative analysis, which has resulted in the exclusion of these outcomes from the analysis. This means that this evaluation provides only a partial view of potential short-term impacts.

Overall, the available results are likely to be more informative about the shorter-term impacts of projects that were completed earlier than about the full programme-wide impacts of the Towns Fund.

Lack of data availability
Place-based evaluation requires granular outcome data that align with town and high street interventions and are collected on a consistent basis over time. In practice, many relevant indicators were not available at the required geographic level, frequency, or completeness (read Annex D for details of this limitation for each outcome). This has restricted the scope of the quantitative assessment, either by removing the outcomes from the scope of the analysis or by capturing only a partial view of the intended outcome. For example, the evaluation has assessed changes in recorded crime as one aspect of local wellbeing, but this alone cannot provide a comprehensive measure of the wellbeing impacts generated by the programme.

The findings presented from this evaluation should therefore be interpreted as evidence on those outcome dimensions that can be measured reliably, rather than as a complete assessment of all impacts the programme may have generated.

Low visibility of medium- and long-term impacts
The short evaluation timeframe has limited the visibility of longer-term impacts that may result from the Towns Fund investments. Many of the funded projects are expected to generate impacts over a longer time horizon, including changes in productivity, skills, social mobility, business composition, land use and wider perceptions of place. These effects may only become observable several years after project completion.

As such, the current findings are more likely to capture near-term or leading effects, rather than the full range of longer-term programme benefits, and should be interpreted as indicative only. Where positive effects are identified, these are unlikely to capture the full benefits that may accrue in the long term.

ATTRIBUTION

The Towns Fund investments were diverse and, in many cases, funded only one component of a larger co-funded package. These overlapping interventions make it difficult to isolate the Towns Fund's contribution from the effects of other funding sources. The programme-level analysis, therefore, focuses on assessing the overall impacts observed in places supported by the Towns Fund, regardless of whether they received additional funding, rather than identifying a precise effect attributable solely to the Towns Fund.

An additional limitation is that the analysis can only partially account for the wider local funding environment. In principle, it would be appropriate to control for other funding in the programme-level analysis if:

- other funds targeted similar outcomes to the Towns Fund
- those local funds were distributed unevenly between the treated and comparator towns, such that they may have affected outcomes differently across the 2 groups

Consistent with this approach, the analysis does not exclude Town Deals locations that also received Future High Streets Fund support (or vice versa), as doing so would further reduce already limited sample sizes.

Similarly, the analysis does not exclude comparator towns that received the other fund (Future High Streets Fund comparators that received Town Deals support, or Town Deals comparators that received Future High Streets Fund support). This reflects the broader treatment of the local funding environment in this evaluation. Given incomplete coverage of other local funding, applying cross-fund exclusions selectively would not provide a consistent control for co-funding and would simply serve to weaken statistical power.

MHCLG have provided data on some additional local funding awarded to areas around the same time as the Towns Fund.[30] However, this is not a complete record of all local funds awarded. Testing for differences in non-Towns Fund funding between treated and comparator towns may only be conducted for the subset of funding that is observable. It cannot fully rule out the possibility that other unobserved funding streams, or differences in the timing, scale or purpose of local funding, may also have influenced outcomes.

Table A.3 and Table A.4 present the results of these tests for projects associated with business outcomes.

Table A.3 sets out the local funding received by Town Deals towns and their comparator towns (including funding associated with the Future High Streets Fund). Funding information provided by MHCLG was attributed to towns by mapping postcode data for project locations to the respective Town Deals and Future High Streets Fund towns.

[30] The funds reviewed are: Coastal Community Fund, Community Ownership Fund, European Regional Development Fund (ERDF), Getting Building Fund, Levelling Up Fund (LUF), Levelling Up Parks Fund, Local Growth Fund, National Lottery Heritage Fund, and UK Shared Prosperity Fund.

Table A.3 Non-Town Deals local funding received by Town Deals towns and comparator towns (business outcomes) and comparator towns

	Town Deals: Treated – within evaluation sample	**Town Deals: Treated – full sample**	**Town Deals: Comparator**
Total number of Town Deals towns	23	38	452
Towns which received non-Town Deals local funding	23	37	346
Average funding per capita (mean): Difference between treated and comparator towns	Not statistically significant	Statistically significant*	Not applicable
Distribution of funding per capita (Wilcox test): Difference between treated and comparator towns	Statistically significant***	Statistically significant***	Not applicable

Source: *Frontier Economics, based on project locations in Towns Fund monitoring data (Round 7, available July 2025) and local funding information compiled by MHCLG. See Footnote 21.*

Note: *[A] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*
[B] 2 Town Deals towns were found to have received non-Towns Fund funding for amounts of magnitude much larger than all other Town Deals towns. Results remain reasonably similar if these observations are excluded.
[C] For completeness, any Future High Streets Fund investment received by Town Deals towns is also included (since these funds are assessed separately in this analysis).

Table A.4 sets out the local funding received by Future High Streets Fund towns and their comparator towns (including funding associated with the Town Deals).

Table A4 Non-Future High Streets Fund local funding received by Future High Streets Fund towns (business outcomes) and comparator towns

	Future High Streets Fund: Treated – within the evaluation sample	Town Deals: Treated – full sample	Town Deals: Comparator
Total number of Future High Streets Fund towns	28	48	156
Towns which received non-Future High Streets Fund local funding	28	47	152
Average funding per capita (mean): Difference between treated and comparator towns	Not statistically significant	Not statistically significant	Not applicable
Distribution of funding per capita (Wilcox test): Difference between treated and comparator towns	Not statistically significant	Not statistically significant	Not applicable

Source: *Frontier Economics, based on project locations in Towns Fund monitoring data (Round 7, available July 2025) and local funding information compiled by MHCLG. See Footnote 21.*

Note: *[A] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*
[B] 2 Town Deals towns were found to have received non-Towns Fund funding for amounts of magnitude much larger than all other Town Deals towns. Results remain reasonably similar if these observations are excluded.
[C] For completeness, any Future High Streets Fund investment received by Town Deals towns is also included (since these funds are assessed separately in this analysis).

At this stage of the analysis, there is limited evidence of meaningful differences in observed non-Future High Streets Fund funding received by treated and comparator towns within the evaluation sample. In particular, the difference in per capita funding between treated towns included in this analysis (those towns with projects that were completed on time for this evaluation) and non-treated towns is not statistically significant. On this basis, controlling for the observed local funding captured in the available data is not expected to materially affect the regression results for the current evaluation sample.

The same comparison for the Town Deals analysis suggests that the observed non-Town Deals funding received by treated towns in the evaluation sample differs significantly from that received by comparator towns. This makes the attribution more challenging and supports a more cautious interpretation of the results. It is therefore sensible, in principle, to control for local funding in this case. However, for the reasons explained above, isolating the impact of Town Deals funding relative to the wider funding environment is complex, and in many cases, not feasible with the available data (limitations include coverage, timing, and comparability across funds). Any impacts identified in this analysis should, therefore, be interpreted as reflecting the combined influence of Town Deals funding and the wider funding environment, rather than a precise net effect of the Town Deals alone.

A.9.2 General limitations associated with place-based analysis

The evaluation approach provides a structured way to estimate the impact of the Towns Fund investments. However, the inherent qualities of place-based analysis pose specific challenges for evaluation and can undermine some of the assumptions required for robust estimation and inference.

This section sets out the key limitations to bear in mind when interpreting the results.

SPATIAL DEPENDENCE

Spatial dependence is an inherent feature of place-based analysis, as places are often interconnected through geography and economic linkages. As a result, outcomes in one area can be influenced by what happens in nearby or related areas.

This represents an important limitation of this evaluation, as a DiD design requires that comparator towns be unaffected by Towns Fund investments. Spatial dependence implies that neighbouring towns or high streets may be exposed to spillovers or displacement activity, or experience shared local shocks, which means that comparator areas may be indirectly affected by the investments. These effects can weaken their suitability as comparators, and estimated impacts may partly reflect displacement or broader general equilibrium effects rather than changes within the treated area alone.

In principle, the analysis could mitigate this by excluding comparators located close to treated areas. The analysis accommodates this to some extent by ensuring that within each fund, comparator Built-up areas (BUAs) do not overlap with treated BUAs. However, the analysis does not apply further restrictions to exclude neighbouring, non-overlapping BUAs, as limited sample sizes mean that such exclusions would materially reduce the comparator pool and weaken statistical power. As a result, estimates should be interpreted with this limitation in mind.

UNOBSERVABLE PLACE EFFECTS

Places differ in ways which are difficult to measure, such as local governance, geography, historical investment, local amenities, and public perceptions. These factors can influence both the likelihood of receiving investment and the outcomes being evaluated. The available data rarely capture these important drivers of place-based change.[31]

While this evaluation design may account for differences in observable characteristics between places, bias may still arise. This can happen if there are meaningful differences in unobservable characteristics, and if these factors change over time in ways which are correlated with the delivery of the investment.

[31] Evidence from MHCLG's High Street Taskforce finds that local authority characteristics such as governance, leadership and delivery capacity strongly condition on the delivery and impact of local investments.

DATA LIMITATIONS

Measurement is a material constraint for place-based analysis, as it requires highly granular data to identify outcomes that align with the geographic scale of local investments such as the Towns Fund.

Place-based interventions operate at very fine geographies, while many standard data sets are only available at broader administrative levels. Furthermore, consistent time series are often lacking because local data sets may be short, contain gaps, or be affected by changes in boundary definitions over time.

These issues can limit the value of quantitative analysis for generating robust place-based insights.

Annex B Mapping impact, comparator and displacement zones

This annex sets out the definitions and methodology used to construct the impact, comparator, and displacement zones for this analysis.

The annex is structured as follows:

- Annex B.1 describes the methodology used to map the geographic shapes for each zone
- Annex B.2 explains how the analysis aggregates metrics reported for standard administrative areas to align with these zones

Read Annex A.1 for details on the geographic definitions the analysis uses.

B.1 Defining the geographic shapes of each zone

B.1.1 Impact zones

Impact zones are the local areas which Towns Fund investments aim to directly affect.

Each impact zone contains all projects affecting a given outcome. Where there are multiple projects within the same geographic area intending to affect the same outcome, this analysis groups them into a single 'treatment' area. In a small number of cases, funding was allocated jointly to 2 towns; in these cases, the impact zone is defined as the union of those towns, to reflect the intended scope of the allocated funding.

As set out in Annex A.1, this impact analysis assesses 3 geographic levels depending on the indicator being tested:

- **Towns:** for the majority of metrics of interest
- **High streets:** for footfall analysis
- **Local authority districts (LADs):** for indicators associated with understanding potential improvements to local authority capabilities

The analysis identified the impact zones in 2 steps:

- **step 1:** Mapping locations of projects focusing on the same outcomes using coordinates from the Towns Fund monitoring data
- **step 2:** Identifying the relevant impact zones surrounding these locations

The analysis applies consistent geographic definitions to map the spatial boundaries for these impact zones: towns, high streets, and local authority districts.

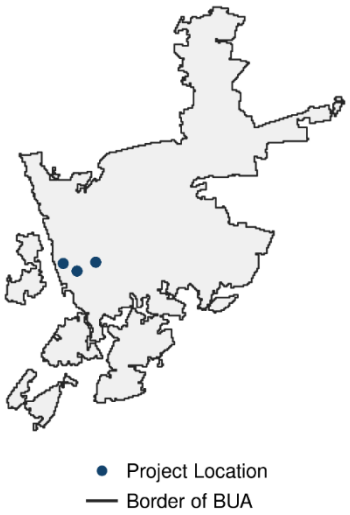
TOWNS

This analysis defines the boundaries for towns using the ONS classification of Built-Up Areas (BUAs). These boundaries are based on the physical built environment and do not

follow administrative area classifications. They define the urban extent of a town and not the surrounding areas.

Figure B.1 illustrates an example mapping of 3 Town Deal projects focused on local wellbeing within the BUA of King's Lynn.

Figure B.1 Example of town mapping: Town Deal projects (local wellbeing) in King's Lynn



Source: Frontier Economics, based on project locations available in Towns Fund monitoring data (Round 7, available July 2025) and 2021 LAD shapefiles from the ONS.

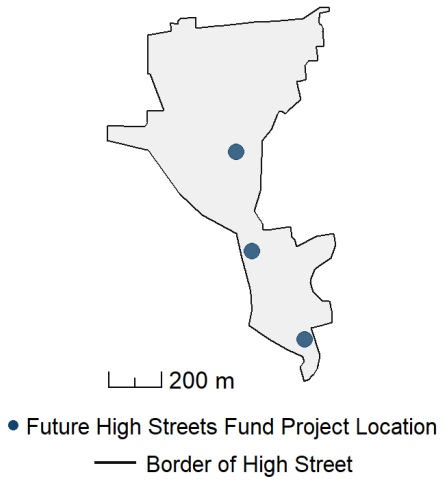
HIGH STREETS

This analysis defines high street boundaries using proprietary definitions from the third-party provider, Place Informatics (as there was no formal or unified classification for high streets at the time of this analysis).[32] These definitions were cross-checked against other available classifications and, together with MHCLG, validated as suitable for this analysis based on local authority insights for a sample of high street locations.

Figure B.2 illustrates an example mapping of 3 Future High Streets Fund projects focused on business outcomes in the high street of Leamington Town Centre.

[32] The Ordnance Survey Functional Economic Market Area (FEMA) data set was considered as an alternative, which sets out definitions for high street regions based on a collection of retail addresses along a single road or street. The Place Informatics shapes were selected because they are produced using a broadly similar methodology and offer the practical advantage of consistency, with footfall data readily available from the same provider for this analysis. Based on the sample reviewed, both definitions were broadly consistent with local authority views of their local high streets.

Figure B.2 Example of high street mapping: Future High Streets Fund projects (business outcomes) in Leamington Town Centre



Source: Frontier Economics, based on project locations available in Towns Fund monitoring data (Round 7, available July 2025) and high street shapefiles from Place Informatics.

LOCAL AUTHORITY DISTRICTS (LADS)

This analysis defines the boundaries of local authority areas using the ONS classification of LADs. LADs are primary administrative geographic units which represent the areas of responsibility for local councils in England.

Figure B.3 illustrates an example mapping of 3 Town Deal projects in Ipswich Council.

Figure B.3 Example of LADs mapping: Town Deal projects (local authority capabilities) in Ipswich council

Source: Frontier Economics, based on project locations available in Towns Fund monitoring data (Round 7, available July 2025) and 2021 LAD 2021 shapefiles from the ONS.

B.1.2 Comparator zones

Comparator zones are local areas that closely match the characteristics of impact zones. The analysis uses these areas as comparators for observed changes in the impact zones.

As set out in Annex A.1, the analysis identifies comparator locations for the Town Deals and Future High Streets Fund separately.

TOWN DEALS COMPARATOR AREAS

The analysis selects comparator towns from the 452 shortlisted towns which did not receive funding:

1. **For the 10 main outcomes of interest:** Comparator areas were identified by matching the names of comparator towns to the corresponding BUAs in the Ordnance Survey data.
2. **For the assessment of local authority capabilities:** Comparator LADs were identified as the relevant LAD areas, which include only comparator towns.

FUTURE HIGH STREETS FUND COMPARATOR AREAS

The analysis uses the 158 comparator high streets that submitted Expressions of Interest (EoI) for Future High Streets Fund funding but were not selected:

1. **For the footfall and dwell time indicators (used as short-term indicators of changes in business outcomes):** Comparator areas were identified as the high streets within the EoIs. Boundaries of these areas provided in the EoIs were mapped with the closest high street boundaries identified by Place Informatics.
2. **For the non-footfall analysis:** Comparator areas were identified by identifying the corresponding BUAs in the Ordnance Survey data in which the high streets fell.

B.1.3 Displacement zones

Displacement zones are local areas near impact zones where displacement effects may occur.

As set out in Annex A.4, the analysis assesses displacement only where economic logic suggests that it would be meaningful (when one activity might cause another activity from a different location to relocate).

Table B.1 presents the subset of outcomes and metrics evaluated for displacement activity.

Table B.1 Indicators for displacement activity

Outcome	Metric for assessment	Geographic level of assessment
Business outcomes	Change in total number of businesses, 2019 to 2025.	Town
Business outcomes	Change in business birth rate, 2019 to 2025.	Town
Business outcomes	Change in total employment, 2019 to 2025.	Town
Business outcomes	Change in average monthly footfall, Q3 2019 to Q3 2025 (Future High Streets Fund only)	High street
Business outcomes	Change in average monthly dwell time, 2019 to 2025 (Future High Streets Fund only)	High street
Business outcomes	Change in total monthly dwell time, 2019 to 2025 (Future High Streets Fund only)	High street
Job creation	Change in total employment, 2019 to 2025.	Town
Job creation	Change in share of part-time jobs, 2019 to 2025.	Town
Employment mix	Change in share of part-time jobs, 2019 to 2025.	Town
Local wellbeing	Change in crime rate per 1,000 capita, 2018/19 to 2024/25.[33]	Town

Source: Frontier Economics.

The analysis assesses displacement differently depending on the geographic level of the indicator of interest:

- analysis of displacement for indicators at the town level uses the **concentric rings** approach
- analysis of displacement for footfall and dwell time uses the **neighbouring high streets** approach

The following sections detail how these shapes are defined.

DISPLACEMENT ANALYSIS USING CONCENTRIC RINGS

A concentric ring approach provides a practical and transparent way to analyse spatial displacement patterns within a town.

This approach recognises that:

- multiple projects that affect the same outcome may be dispersed across different locations within a town
- spillover or displacement effects may take place at varying distances from the intervention, depending on the outcome and metric

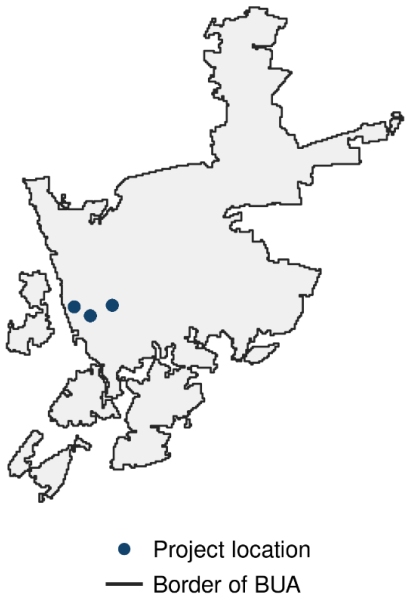
[33] The categories of crime assessed are total crime, theft from the person, robbery, burglary, shoplifting, and antisocial behaviour.

The analysis defines 11 concentric rings surrounding the impact zone. The first is a circle of 0.5km width surrounding the project locations, followed by one concentric ring of 0.5km width and 9 rings of 1km width each. The rings have been defined in 4 steps, as follows.

Step 1: Mapping project locations
The analysis maps the projects in scope for each town and outcome by postcode.

Figure B.4 illustrates the mapping of 3 local wellbeing projects in King's Lynn.

Figure B.4 Concentric rings: Step 1 – Mapping project locations: Town Deal projects (local wellbeing) in King's Lynn

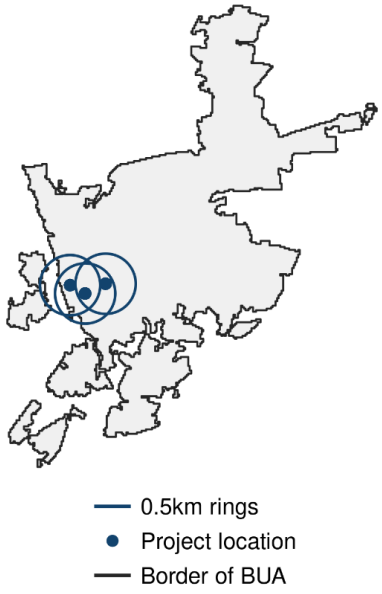


Source: Frontier Economics, based on project locations available in Towns Fund monitoring data (Round 7, available July 2025) and BUA shapefiles from the ONS.

Step 2: Drawing initial project-level impact zones
Circles with a 0.5km radius are drawn around each project location. These initial zones are narrow to provide fine resolution near the intervention site, where effects are most likely to be strongest and most likely to vary with distance.

Figure B.5 illustrates the initial 0.5km impact circles for 3 local wellbeing projects in King's Lynn.

Figure B.5 Concentric rings: Step 2 – Drawing initial project-level impact zones: Town Deal projects (local wellbeing) in King's Lynn



Source: Frontier Economics, based on project locations available in Towns Fund monitoring data (Round 7, available July 2025) and BUA shapefiles from the ONS.

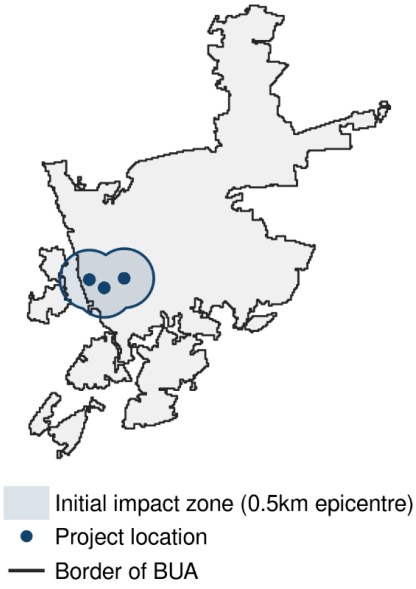
Step 3: Defining town-level impact zones
The analysis combines the areas of all initial impact zones within a given town boundary into a single, unified area of impact.

There are several considerations for defining areas in this way:

1. **Where initial impact zones overlap:** It is intuitive to consider these as part of the same zone
2. **Where initial impact zones are distinct and non-overlapping, but are within 1km of each other (this happens in the majority of cases):** It is assumed that these are all focused on impacting the same local area
3. **Where initial impact zones are distinct and non-overlapping, but are further than 1km from each other:** Each case was reviewed individually, and it was concluded that these zones could also be considered as part of the same impact zone

Figure B.6 illustrates the first town-level impact zone for 3 local wellbeing projects in King's Lynn. It is the union of the 0.5km circles drawn for each project in Step 2.

Figure B.6 Concentric rings: Step 3 – Drawing town-level impact zones: Town Deal projects (local wellbeing) in King's Lynn

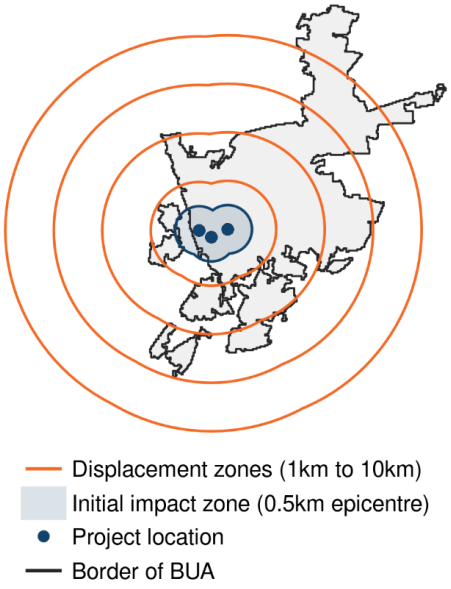


Source: Frontier Economics, based on project locations available in Towns Fund monitoring data (Round 7, available July 2025) and BUA shapefiles from the ONS.

Step 4: Drawing concentric rings for potential displacement zones
The analysis draws 10 successive concentric rings around the town-level impact zone identified in Step 3. The first ring drawn around the central impact zone is 0.5km wide, and each of the 9 subsequent rings is 1km wide.

Figure B.7 illustrates the concentric rings around the local wellbeing projects in King's Lynn.

Figure B.7 Concentric rings: Step 4 – Drawing town-level impact zones: Town Deal projects (local wellbeing) in King's Lynn



Source: Frontier Economics, based on project locations available in Towns Fund monitoring data (Round 7, available July 2025) and BUA shapefiles from the ONS.

DISPLACEMENT ANALYSIS USING NEIGHBOURING HIGH STREETS

The analysis identified up to 3 neighbouring high streets for the footfall and dwell time displacement analysis. This analysis only applies to the Future High Streets Fund.

These neighbouring high streets have been selected based on their level of co-visitation, which measures the extent to which 2 high streets share visitors.

This evaluation uses co-visitation data from Place Informatics, the third-party provider of footfall and dwell-time data. Place Informatics define co-visitation between 2 high streets i and j as the proportion of high street i's primary catchment population that also visits high street j in a given period. The primary catchment of high street i is the set of postcodes within 3 miles (in linear distance) where more than 5% of the postcode population visits high street i. This co-visitation measure is not symmetric (the co-visitation of high street i's catchment population with high street j is not the same as the co-visitation of high street j's population with high street i), since the primary catchment area of high street i is not the same as the primary catchment area of high street j.

A high co-visitation indicates that a large share of people who visit the treated high street also visit another high street. This may be consistent with several mechanisms influenced by visitor behaviour that this analysis aims to test:

1. **Displacement:** If visitors use the locations interchangeably, a positive change in the treated high street might reduce visitation to the co-visited high street (and vice versa).
2. **Spillovers:** If visitors use the locations in complementary ways, a positive change in the treated high street might be associated with an increase in visits to the co-visited high street.
3. **No correlation:** If the high streets serve completely different purposes, a positive change in the treated high street would not affect visitation to the neighbouring high street.

Note that candidate displacement zones selected for this analysis include other high streets, but exclude other shopping formats such as retail parks. The available evidence suggests that findings are mixed on the extent to which retail parks compete with, or complement, high streets, and therefore on the extent to which they should be included in the displacement analysis. Given this uncertainty, an objective approach would involve testing the sensitivity of any results to the inclusion of retail parks. In practice, however, retail park footfall data provided by Place Informatics is only available from 2022. As such, there are no pre-treatment observations available prior to 2019. As a result of this limitation, retail parks have been excluded from the analysis. Future evaluation of footfall displacement should consider including retail parks to address the current gap in the literature and evidence on the level of substitution between high streets and retail parks.

The analysis selected up to 3 candidate displacement zones for each treated high street. This selection process followed these principles:

1. **Highest possible co-visitation:** Selecting neighbouring high streets with the highest co-visitation, to ensure any marginal effects could be clearly identified.
2. **Uniqueness of high streets:** Selecting unique displacement high streets across all treated high streets, to maximise the scope of the analysis. Where a candidate displacement zone appeared in the top 3 for more than one treated high street, it was allocated to the treated high street with which it had the highest co-visitation, and the next highest-ranked candidate was selected for the remaining treated high street(s).
3. **Minimal spatial overlap:** Selecting displacement high streets that do not overlap with any treated high streets and minimising overlaps between comparator displacement high streets, to minimise contamination and avoid double-counting of footfall across zones.

B.2 Calculating indicators for each zone

The analysis aggregates data for all relevant indicators using the mapping of impact, comparator, and displacement zones defined in Annex A.1.

The aggregation approach is the same across all zone types (towns, high streets or LADs), but varies by indicator type. In particular, the method differs depending on whether the indicator is expressed as a count or a proportion. These aggregations are the sums or averages of the relevant variables reported for statistical geographies defined by the Office for National Statistics (ONS).

The following sections describe the aggregation methodologies.

B.2.1 Aggregating indicators for count variables

Count variables (such as the number of businesses or employees) are calculated for each impact, comparator, or displacement zone by taking the sum of counts across lower-level statistical geographies that intersect the zone.

As these boundaries rarely align with statistical geographies (such as postcodes), the analysis allocates an appropriate share of each geography's count to the impact zone. This share is the proportion of the statistical geography which overlaps with the impact zone.[34]

Figure B.8 illustrates the following steps for a hypothetical impact zone A:

- **step 1:** calculating the proportion of each statistical geography which overlaps with the impact zone
- **step 2:** calculating the weighted count of each statistical geographical area using these proportions
- **step 3:** summing all the weighted counts

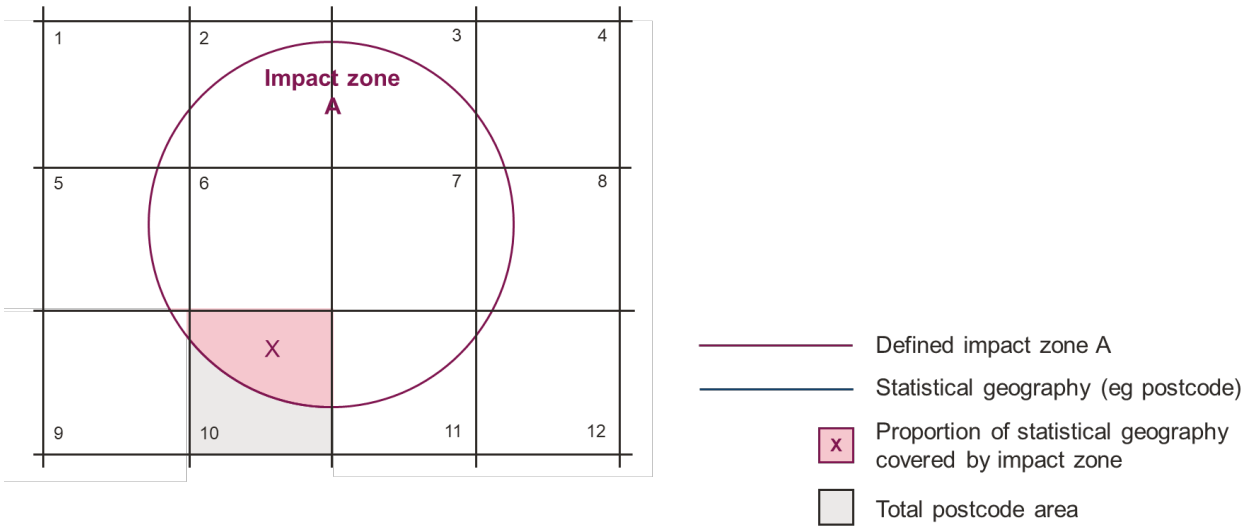
In Step 1, the analysis divides the overlap area (marked by 'X') by the total postcode area (the grey area) to calculate the proportion of the postcode area that lies within Impact zone A.

Next, the analysis multiplies this proportion for each area by the count associated with that statistical geographical area. For example, multiplying the number of businesses attributable to postcode 10 by the proportion calculated in Step 1.

Finally, the analysis takes the sum of the weighted counts of all statistical geographies that overlap with the impact zone.

[34] This approach assumes that the recorded metric is evenly distributed across the statistical geography.

Figure B.8 Aggregating indicators for count variables



Source: Frontier Economics.

B.2.2 Aggregating indicators for rate or proportion variables

Rate or proportion variables (such as crimes per 1000 inhabitants or the proportion of part-time workers) are calculated for each impact, comparator, or displacement zone by averaging values across lower-level statistical geographies that intersect the zone.

Because the boundaries of an impact, comparator, or displacement zone rarely align with statistical geographies (such as postcodes), the analysis needs to allocate an appropriate share of each statistical geography's rate to the final aggregation. In particular, the appropriate share is the proportion of the overlapping area relative to the total impact zone area.

Figure B.9 illustrates the following steps for a hypothetical impact zone B:

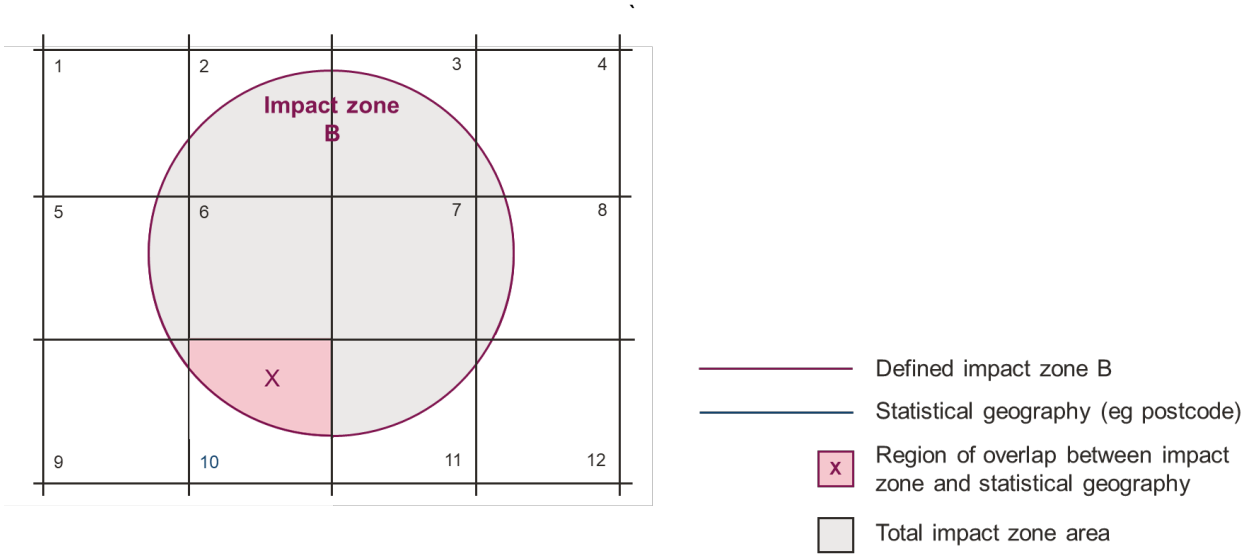
- **step 1:** calculating the proportion of overlap divided by the total area of each impact zone
- **step 2:** calculating the weighted rate of each statistical geographical area using these overlap proportions
- **step 3:** summing all the weighted rates to arrive at the weighted average

In Step 1, the analysis divides the overlap area (marked by 'X') by the total impact area (the grey area) to calculate the weight that the rate of postcode 10 has in impact zone B.

Next, the analysis multiplies the statistical geography area's weight by the rate associated with that statistical geography area. For example, multiplying the rate of part-time jobs that contribute to the total rate in impact zone B.

Finally, the analysis takes the sum of the weighted rates of all statistical geographies that overlap with the impact zone.

Figure B.9 Aggregating indicators for rate or proportion variables



Source: Frontier Economics.

B.2.3 Implications for interpretation

These methods assume that outcome variables are distributed uniformly within each statistical geography when allocating values by overlapping area.

In practice, outcome variables may be distributed unevenly across a statistical geography. For example, crime may cluster in town centres, or employment may cluster around business parks. As a result, this area-based allocation can introduce measurement error where the overlap does not reflect where the activity actually occurs. This limitation is most likely to matter for highly localised outcomes and should be considered when interpreting results.

The analysis mitigates this risk by aggregating statistics for each zone from the lowest-level statistical geography available for each indicator. This reduces the scope for any concentrated activity to be masked by averages across large areas.

Annex C Matching and weighting techniques

This annex sets out the matching and weighting techniques considered for constructing the comparator sample for the difference-in-differences (DiD) regression set out in Annex A.

The most suitable methods identified for this evaluation are propensity score matching (PSM) and covariate-balancing propensity scores (CBPS).

The annex is structured as follows:

- Annex C.1 explains the purpose of matching and weighting in this analysis
- Annex C.2 summarises the matching and weighting methods considered and assesses their suitability for this evaluation

C.1 Purpose of matching and weighting

A key assumption underpinning a DiD design is the existence of parallel trends. It assumes that the treated and comparison groups would have followed similar trends in their characteristics and outcome of interest absent the intervention. If parallel trends hold, divergence in treatment group outcomes can be attributed to the intervention.

As selection into the Towns Fund was not random, and the comparison group comprises the remaining shortlisted towns, there are likely systematic differences between the 2 groups that may undermine the parallel trends assumption and risk biasing the DiD estimates.

Matching and weighting methods can help to construct comparison samples that more closely mirror the treated group. These methods rely on the following assumptions:

- conditional independence
- common support
- Stable Unit Treatment Value Assumption (SUTVA)

Conditional independence assumes that controlling for relevant observable town characteristics creates treatment and comparator groups that are similar, as if treatment were allocated at random. The results are valid if the model includes all relevant observable confounders. However, the model may still be biased if it does not control for unobservable factors that influence treatment selection and outcomes.

Common support requires a sufficient overlap in the characteristics of treated and comparator towns. Limited overlap can result in the excessive exclusion of observations from the analysis (in the case of matching) or the assignment of extreme weights (in the case of reweighting). In these cases, results may not reflect the true effect of the intervention.

SUTVA assumes that the treatment status of one town does not affect the outcomes of another, and that there are no different forms or versions of treatment. In other words, towns receiving investment do not affect the outcomes of the comparator towns.

C.2 Review of techniques

This analysis reviewed a range of matching and weighting techniques. The methods considered, together with their advantages and disadvantages for this analysis, are summarised below.

The assessment identified PSM and CBPS as the most suitable methods for the analysis. Both methods have been implemented to help validate and cross-check the final results.

C.2.1 Entropy Balancing

Entropy balancing is a reweighting method that produces a weighted comparison group where the pre-treatment covariates match those of the treated group. It determines weights by solving an optimisation problem that:

- imposes covariate balance constraints (typically on means, and optionally on higher moments such as variances)
- keeps the weights as close as possible to a set of base weights, subject to those constraints

Treatment effects are estimated by comparing outcomes for treated towns with those for the reweighted comparator group.

SUITABILITY FOR TOWNS FUND ANALYSIS: LOW

This method is highly sensitive to the choice of covariates. Given the complexity of the Towns Fund analysis, determining suitable covariates requires several rounds of refinement. In addition, the need for strict balance in covariate moments, at the expense of retaining outlier variables, is unhelpful for this analysis.

Given the availability of more suitable alternatives, the Towns Fund analysis does not use this method.

Advantages of Entropy balancing for the Towns Fund analysis

1. Good balancing properties and low covariate bias, which achieve exact balance as a result of explicitly balancing on moments.
2. Transparent methodology, which lends itself to simple inference.

Disadvantages of Entropy balancing for the Towns Fund analysis

1. Sensitivity to covariate selection. Relevant covariates are not privileged over those which are irrelevant. Balance is therefore enforced on covariate moments even when not strictly necessary.
2. Extreme weights. If there is low common support, precision may be compromised when extreme weights are present.

C.2.2 Mahalanobis distance matching

This approach pairs each treated unit with one or more comparator units based on the Mahalanobis distance. This metric rescales differences in the observed pre-treatment covariates by their variances, accounting for covariate correlations. It standardises all covariates, avoiding matches which are disproportionately affected by variables measured on larger scales, and provides a single summary measure of multivariate closeness.

The Mahalanobis distance is computed for each treated unit and each potential comparator, using the distance of the comparator covariates to the treatment.

The analysis then estimates treatment effects by comparing outcomes between treated units and their matched comparators, applying appropriate weighting when comparators are matched to multiple treated units.

SUITABILITY FOR TOWNS FUND ANALYSIS: LOW

This method is most appropriate for cases with few, continuous covariates that are well-measured. As the Towns Fund analysis relies on a large number of covariates with mixed data types, this method was not suitable.

Advantages of Mahalanobis distance matching for the Towns Fund analysis

1. Direct multivariate matching using a single summary measure, which naturally accounts for scale and correlation.

Disadvantages of Mahalanobis distance matching for the Towns Fund analysis

1. Sufficient observations are required for analysis. Common support considerations require sufficiently large numbers of observations to use this method.
2. Sensitive to covariate selection. Relevant covariates are not privileged over those which are irrelevant. As a result, careful selection of covariates is required.
3. Less suited to high-dimensional analysis. Balance improves implicitly through covariate closeness, but is not guaranteed. Typically works better in instances with few covariates, but degrades in higher dimensions.
4. Limited interpretability. The Mahalanobis metric (distance scaled by the covariance matrix) is not straightforward to interpret and communicate.

C.2.3 Coarsened Exact Matching (CEM)

This matching technique groups towns into categories based on observable characteristics and then matches treated and comparator towns within the same categories.

In practice, the researcher chooses a set of covariates and defines suitable categorisations. For example, grouping deprivation into bands or population into size categories. Treatment towns are matched to comparator towns that share the same combination of categories (eg high deprivation and large size in the West Midlands). The analysis excludes towns without comparable matches, restricting comparisons to areas of common support.

The assessment estimates treatment effects by comparing outcomes between treated units and matched comparators within each category, typically using CEM weights to account for differences in the numbers of treated and comparator units across categories.

SUITABILITY FOR TOWNS FUND ANALYSIS: LOW

This method is suited to cases with large amounts of data and meaningful groupings based on a few key covariates. As this is not the case for the Towns Fund analysis, this method was not suitable.

Advantages of CEM for the Towns Fund analysis

1. Reasonably good balance may be achieved by grouping towns based on covariate similarity across a range of covariates.
2. Highly transparent and intuitive covariate groupings, which are simple to communicate.

Disadvantages of CEM for the Towns Fund analysis

1. Sufficient observations are required for analysis. Common support considerations require sufficiently large numbers of observations to use this method.
2. Subject to a trade-off between refinement and coarsening. The algorithm may discard many units if the bins are too fine, and the overlap within them is limited. However, coarser bins retain information at the expense of bias.

C.2.4 Propensity score matching (PSM)

This technique compares treated towns with similar untreated towns based on an estimated propensity score, or the estimated probability of receiving Towns Fund investment based on their observable characteristics.

Propensity scores are estimated using a statistical model, often a logistic regression, that predicts treatment status based on a set of pre-treatment covariates (eg baseline deprivation, population, geography, and local economic indicators). Covariate selection is based on the plausibility of receiving treatment and the outcomes of interest. This aims to improve comparability between treated and comparator towns in observed characteristics and, in doing so, reduce confounding due to pre-existing differences.

Each treated town is matched to one or more comparator towns with similar propensity scores. Outcomes are compared between treated towns and their matched comparators. The analysis applies weights to reflect the matching approach; for example, if a comparator town is matched twice to 2 different treated towns, then it will be assigned twice the weight of a comparator town that is only matched once to one treated town.

SUITABILITY FOR TOWNS FUND ANALYSIS: HIGH

This method is most suitable for cases with a reasonable number of treated units and a large pool of comparators with good overlap.

There is a sizable pool of 452 comparator towns available for Town Deals analysis, whereas only 155 are available for the Future High Streets Fund analysis. As a result, PSM is only appropriate for the Town Deals analysis.

Advantages of PSM for the Towns Fund analysis

1. Less sensitive to covariate selection. Irrelevant covariates included within the PSM estimation have smaller coefficients in the estimation equation.
2. PSM produces a single likelihood score that is intuitive and easy to explain.
3. Restricting to common support can drop outlier observations and improve comparability.

Disadvantages of PSM for the Towns Fund analysis

1. Sufficient observations are required for analysis. Common support considerations require sufficiently large numbers of observations to use this method.
2. Covariate balance is not guaranteed. Balance improves implicitly through alignment on the propensity score, rather than on the covariates. Moment balance must, therefore, be checked and tuned. Residual covariate imbalance may remain in the final match.
3. Restricting to common support can risk high variance and reduced precision if too many units are discarded.

C.2.5 Inverse probability weighting (IPW)

This technique uses estimated propensity scores to construct a weighted sample that balances observed pre-treatment covariates across both treated and comparator groups. Weights for treated towns are inversely proportional to their probability of being treated ($1/p(X)$), while weights for comparator towns are inversely proportional to their probability of not being treated ($1/(1-p(X))$). This results in under-represented treatment observations receiving greater weight, thereby making the treated sample more balanced across groups.

The treatment effects are estimated by comparing weighted outcomes between treated and comparator units.

SUITABILITY FOR TOWNS FUND ANALYSIS: MEDIUM

This method is most suitable when there is good overlap and propensity scores are not too close to 0 or 1.

The Towns Fund analysis is diverse and comprises multiple regressions, for which this condition may not always be satisfied. As a result, the overlap condition required for stable IPW is unlikely to hold consistently across all regressions, which increases the risk that a small number of heavily weighted towns will affect some estimates.

For this reason, this analysis favoured alternative approaches that are typically less volatile under imperfect overlap, allowing like-for-like comparisons without relying heavily on extreme weighting. IPW may be a reasonable robustness check in cases where the condition above applies.

Advantages of IPW for the Towns Fund analysis

1. Performs better on covariate balance than PSM. IPW reweights the sample to create a 'pseudo-population' where covariates are balanced. This often yields a stronger balance on average across many covariates when the propensity model is well-behaved.

Disadvantages of IPW for the Towns Fund analysis

1. Highly sensitive to poor overlap. This method is prone to extreme weights when propensity scores are near 0 or 1. This can make estimates unstable and sensitive to small model errors.
2. Sensitive to the propensity score specification, as this feeds directly into weights. Small misspecifications can lead to large weight distortions in the tails of the distribution.

C.2.6 Covariate-balancing propensity scores (CBPS)

This method extends the propensity score method by incorporating covariate-balancing conditions into the estimation process, ensuring that the weights achieve better balance by construction.

In practice, CBPS estimates propensity scores using a set of pre-treatment covariates while simultaneously enforcing alignment of covariate moments between the treated and comparator towns. The analysis adjusts the weights for the comparator towns to closely resemble those of the treated towns, both in terms of the propensity score and the chosen set of covariates.

Treatment effects are estimated by comparing outcomes between treated units and the resulting weighted comparator group.

SUITABILITY FOR TOWNS FUND ANALYSIS: HIGH

Relative to PSM, CBPS places greater weight on achieving covariate balance. It typically uses weighting rather than selecting a small set of matches, allowing more observations to remain in the model. This can improve balance across observed covariates and reduce sensitivity to misspecification in the propensity model.

Since PSM is suitable for this analysis, this method is also naturally suitable. In this evaluation, CBPS serves as a reasonable cross-check for PSM.

Advantages of CBPS for the Towns Fund analysis

1. Good balancing properties and low covariate bias, as explicitly balancing on moments is one of the objective functions for this method.
2. Straightforward weighted estimation and inference.

Disadvantages of CBPS for the Towns Fund analysis

1. Highly sensitive to poor overlap. As this method relies on the inverse probability score for balancing, similar to IPW, it is also susceptible to extreme weights.

C.2.7 Synthetic controls

This method uses historical data to construct a 'synthetic' comparator group. A synthetic control is created by using a weighted average of comparator towns to closely approximate the treated town's pre-intervention characteristics and outcomes. This weighted combination of comparator towns represents what would have happened to the treated town in the absence of the intervention.

The treatment effect is estimated as the difference between the treated town's observed outcomes and the synthetic control's outcomes after the intervention, interpreted as the estimated counterfactual gap.

This method is most credible when the synthetic control closely matches the treated units before the intervention and when the intervention is the main structural change affecting outcomes at that time.

SUITABILITY FOR TOWNS FUND ANALYSIS: MEDIUM

This method is most helpful when there are a small number of treated units and insufficient comparator towns for analysis. This makes this approach suitable for some outcomes.

However, the synthetic control method requires a relatively long pre-outcome history. This makes it unsuitable for much of the Towns Fund analysis, as most of the assessed indicators show a short pre-trend.

Advantages of Synthetic controls for the Towns Fund analysis

1. Useful for low sample sizes. This method may be used in cases where there are few treated units and sufficient comparator towns are unavailable.
2. An explicit targeting of pre-period fit yields a tight balance, including on-time-invariant unobserved covariates.

Disadvantages of Synthetic controls for the Towns Fund analysis

1. Requires good historical time-series data on pre-intervention outcomes, relative to the number of cross-sectional data points.

Annex D Data sources

This annex summarises the data sources used to support the quasi-experimental analysis undertaken as part of the programme-level evaluation.

The annex is structured as follows:

1. Annex D.1 sets out the full range of data sources reviewed and describes the process used to assess their suitability and select those taken forward for analysis.
2. Annex D.2 presents the final set of indicators included in the evaluation, explains how these were used across the different elements of the analysis, and outlines any data cleaning and transformations undertaken to prepare them for the final econometrics assessment.

D.1 Data sources explored for this evaluation

An extended review of the available metrics and indicators was conducted at the feasibility stage of this evaluation. This was subsequently refreshed several times during the implementation stage to ensure that the final analysis reflected the most up-to-date data.

The analysis assessed the suitability of these indicators against a range of criteria:

1. Whether the indicator is available at a suitable geographic granularity.
2. Whether the indicator is published with sufficient frequency to support the pre- and post-intervention time series required for the difference-in-differences analysis.
3. Whether there are material time lags in publication that would limit the ability to detect impacts within the evaluation period.
4. Whether the definition and methodology for the indicator are consistent over time.
5. Whether the indicator has suitable coverage, including consistent availability across treatment and comparison areas, low levels of missing data, and a sufficiently representative sample.
6. The sample size available for analysis, given the latest publication date and the resulting number of usable observations.[35, 36]
7. Other considerations relevant to their suitability for robust quantitative analysis.

[35] See Annex A for a description of how a sufficient treatment sample size is assessed based on project completion dates and available data.
[36] The rule of thumb for statistical analysis suggests that sample sizes of 30 and above are ‘sufficiently large’ for the Central Limit Theorem to hold. The analysis relaxes this assumption, given the low sample sizes available, and applies a threshold of at least 20 treated observations.

The following quantitative analyses were considered feasible for at least one of the funds:

1. **Difference-in-differences (DiD) regression analysis** for a subset of 5 outcomes (business outcomes, job creation, employment mix, local wellbeing, and local authority capabilities).
2. **Descriptive analysis** for a further 2 outcomes, where relevant indicators are available but the sample size is insufficient for full regression analysis (digital connectivity and property and land use).
3. **No quantitative analysis** is possible for the remaining outcomes (business decarbonisation, pride in place, physical connectivity, and social mobility).

Table D.1, Table D.2, Table D.3, Table D.4, Table D.5, Table D.6, and Table D.7 present the candidate indicators identified for each outcome, for which some quantitative analysis was considered feasible. The tables present the results of the assessment against the criteria set out above, by outcome and outcome metric (OM).

Table D.1 presents the candidate indicators identified for business outcomes. The table presents the results of the assessment against the criteria set out above, by outcome and outcome metric (OM).

Table D.1 Data sources available to assess business outcomes

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM1.1.1	Proportion of businesses which increase their turnover	Secure data – Inter-departmental Business Register (IDBR): Turnover data by sector	Unsuitable	Data source not available at sufficient granularity (available at the 'Enterprise' level but not at a local business level).[37]
OM1.1.2	Change in overall local business turnover	Proprietary data – Transaction data from grocery stores in England	Unsuitable	Comprehensive coverage across all grocery stores is not possible at a reasonable cost.
OM1.1.2	Change in overall local business turnover	Secure data – Annual Business Survey: Turnover data by sector	Unsuitable	Lack of sufficient time series (data available annually until 2021 only).
OM1.1.2	Change in overall local business turnover	Secure data – Business Structure Database (BSD): Turnover data by sector	Unsuitable	Data based on snapshots of the IDBR. Discarded as more frequent publications and more recent data were available directly from the IDBR.
OM1.1.2	Change in overall local business turnover	Secure data – IDBR: Turnover data by sector	Unsuitable	Data source not available at sufficient granularity (available at the 'Enterprise' level but not at a local business level).[38]
OM1.1.3	Change in local labour productivity based on Gross Value Added (GVA)	Appropriate data source not identified	Not applicable	Not applicable.
OM1.1.4	Change in high street footfall	Proprietary data – Footfall data based on GPS signals from mobile network providers	Unsuitable	Comprehensive coverage across all mobile network providers is not possible at a reasonable cost.
OM1.1.4	Change in high street footfall	Proprietary data – MRI OnLocation: Footfall data based on surveillance footage and AI image recognition	Unsuitable	Partial geographic coverage – data from this provider is only available for c. 70% of Towns Fund towns. In addition, a risk of double-counting was identified in the provider's methodology.

[37] For instance, turnover associated with a supermarket chain will be presented at an aggregate level for the entire supermarket chain, rather than at a local level for a specific location.
[38] For instance, turnover associated with a supermarket chain will be presented at an aggregate level for the entire supermarket chain, rather than at a local level for a specific location.

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM1.1.4	Change in high street footfall	Proprietary data – Place Informatics: Footfall data based on GPS signals from mobile apps	Suitable for Future High Streets Fund only	Assessed as suitable for Future High Streets Fund, but disproportionate cost required for Town Deals, given the lack of defined geographic shapes for impact zones. Read Annex C for more information.
OM1.1.5	Change in net business creation weighted by business longevity	Secure data – BSD: Birth and death rates for businesses	Suitable but not preferred	Data based on snapshots of the IDBR. Discarded as more frequent publications and more recent data were available directly from the IDBR.
OM1.1.5	Change in net business creation weighted by business longevity	Published data – Business Demography: Birth and death rates for businesses	Unsuitable	Lack of sufficient geographic granularity (data available at Unitary Authority level only).
OM1.1.5	Change in net business creation weighted by business longevity	Secure data – IDBR: Business births each year	Suitable and preferred	Sufficient geographic granularity, frequency, time series and sample size for regression for both Future High Streets Fund and Town Deals (Table D.11 provides more details).
OM1.1.6	Change in annual private sector investment in the town	Proprietary data – Beauhurst: Private sector investment	Unsuitable	Proprietary data – too costly to obtain for the purpose of this evaluation.
OM1.1.6	Change in annual private sector investment in the town	Proprietary data – Data City: Private sector investment	Unsuitable	Proprietary data – too costly to obtain for the purpose of this evaluation.
OM1.1.7	Change in local economic activity on high streets	Proprietary data – Fable: Card transactions data	Unsuitable	Proprietary data – too costly to obtain for the purpose of this evaluation.
OM1.1.7	Change in local economic activity on high streets	Proprietary data – Kantar: Card transactions data	Unsuitable	Proprietary data – too costly to obtain for the purpose of this evaluation.

Source: Frontier Economics.

Table D.2 presents the candidate indicators identified for property and land use outcomes. The table presents the results of the assessment against the criteria set out above, by outcome and OM.

Table D.2 Data sources available to assess property and land use

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM1.3.1	Change in residential house prices and rents over time	Published data – Land Registry: transaction price	Suitable if grouped with OM1.3.2 (the indicator does not distinguish between residential and commercial properties)	Assessed suitable for descriptive statistics for the Future High Streets Fund only due to insufficient sample size for regression analysis (Table D.12 provides more details).
OM1.3.2	Change in commercial property prices and rents over time	Published data – Land Registry: transaction price	Suitable if grouped with OM1.3.1 (the indicator does not distinguish between residential and commercial properties)	As for OM1.3.1.
OM1.3.3	Change in commercial vacancy rates	Proprietary data – Local Data Company (LDC): Vacancy rates and retail use	Suitable	Sufficient geographic granularity, frequency, time series and sample size for regression for both Future High Streets Fund and Town Deals (Table D.12 provides more details).
OM1.3.3	Change in commercial vacancy rates	Proprietary data – Geolytix: Retail use data	Unsuitable	Discarded as equivalent data readily available from Green Street under MHCLG licence.
OM1.3.4	Change in land use over time	Appropriate data source not identified	Not applicable	Not applicable.

Source: Frontier Economics.

Table D.3 presents the candidate indicators identified for job creation outcomes. The table presents the results of the assessment against the criteria set out above, by outcome and OM.

Table D.3 Data sources available to assess job creation

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM2.1.1	Number of jobs directly created and/or safeguarded by the Towns Fund	Appropriate data source not identified.	Not applicable	Not applicable.
OM2.1.2	Number of jobs created and/or safeguarded overall	Appropriate data source not identified.	Not applicable	Not applicable.
OM2.1.3	Change in net local unemployment levels (Total employment)	Secure data – IDBR: Total employment	Suitable	Sufficient geographic granularity, frequency, time series and sample size for regression for both Future High Streets Fund and Town Deals (Table D.11 provides more details).
OM2.1.3	Change in net local unemployment levels (Total employment)	Secure data – Annual Population Survey: Total employment	Unsuitable	Lack of suitable coverage at the local level (reliance on few units in each Output area [OA]).
OM2.1.3	Change in net local unemployment levels (Benefits claimant rate)	Published data – Department for Work and Pensions (DWP) Benefits information on Jobseekers Allowance (JSA) and Universal Credit (UC)	Suitable	Sufficient geographic granularity, frequency, time series and sample size for regression for both Future High Streets Fund and Town Deals (Table D.13 provides more details).
OM2.1.3	Change in net local unemployment levels (Benefits claimant rate)	Secure data – Annual Population Survey: Unemployment benefits rate	Unsuitable	Lack of suitable coverage at the local level (reliance on few units in each OA).
OM2.1.3	Change in net local unemployment levels (Benefits claimant rate)	Secure data – Understanding Society: Unemployment benefits rate	Unsuitable	Lack of sufficient time series (data available annually until 2018 only).
OM2.1.4	Change in job density	Secure data – Business Register and Employment Survey (BRES): Proportion of full-time and part-time jobs	Suitable but not preferred	Data available annually until 2023. Discarded as more recent data were available from the Annual Survey of Hours and Earnings (ASHE).

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM2.1.4	Change in job density	Secure data – ASHE: Proportion of part-time jobs	Suitable and preferred	Assessed suitable for regression analysis for the Town Deals (Table D.11 provides more details), but descriptive statistics only for the Future High Streets Fund due to insufficient sample size.

Source: Frontier Economics.

Table D.4 presents the candidate indicators identified for employment mix outcomes. The table presents the results of the assessment against the criteria set out above, by outcome and OM.

Table D.4 Data sources available to assess employment mix

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM2.2.1	Change in composition of employment by business SIC codes in the local area	Published data – Census 2021: Proportion of people employed by industry	Unsuitable	Lack of sufficient time series (data available once every ten years, last available in 2021).
OM2.2.1	Change in composition of employment by business SIC codes in the local area	Secure data – ASHE: Number of employees by SIC code	Suitable	Assessed suitable for regression analysis for the Town Deals only (Table D.11 provides more details). Quantitative analysis not possible for the Future High Streets Fund due to insufficient sample size.
OM2.2.2	Change in the number of individuals trained and/or upskilled	Appropriate data source not identified.	Not applicable	Not applicable.
OM2.2.3	Change in economic activity by employment status (Employment status)	Secure data – Annual Population Survey: Employment status	Unsuitable	Lack of suitable coverage at the local level (reliance on few units in each OA).
OM2.2.3	Change in economic activity by employment status (Employment status)	Secure data – Understanding Society: Employment status	Unsuitable	Lack of sufficient time series (data available annually until 2018 only).
OM2.2.3	Change in economic activity by employment status (Proportion of part-time employment)	Secure data – ASHE: Proportion of full-time and part-time jobs	Suitable	Assessed suitable for regression analysis for the Town Deals only (Table D.11 provides more details). Quantitative analysis not possible for the Future High Streets Fund due to insufficient sample size.
OM2.2.3	Change in economic activity by employment status (Proportion of part-time employment)	Secure data – BRES: Proportion of full-time and part-time jobs	Suitable but not preferred	Data available annually until 2023; discarded as more recent data were available from the ASHE.
OM2.2.4	Change in low-pay employment	Appropriate data source not identified.	Not applicable	Not applicable.

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM2.2.5	Skills and training	Published data – Census 2021: Highest level of qualification held	Unsuitable	Lack of sufficient time series (data available once every 10 years, last available in 2021).
OM2.2.5	Skills and training	Secure data – Annual Population Survey: Highest level of qualification held	Unsuitable	Lack of suitable coverage at the local level (reliance on few units in each OA).

Source: Frontier Economics.

Table D.5 presents the candidate indicators identified for local wellbeing outcomes. The table presents the results of the assessment against the criteria set out above, by outcome and OM.

Table D.5 Data sources available to assess local wellbeing

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM3.1.1	Change in reported resident happiness and life satisfaction	Appropriate data source not identified.	Not applicable	Not applicable.
OM3.1.2	Health improvements over time	Published data – Sport England: Density of sport/leisure assets	Unsuitable	Insufficient sample size for both the Town Deals and Future High Streets Fund based on the latest publication date available (Feb 2023).
OM3.1.2	Health improvements over time	Secure data – Annual Population Survey: Long-term sickness and health problems in the last 12 months	Unsuitable	Lack of suitable coverage at the local level (reliance on few units in each OA).
OM3.1.3	Change in reported crime	Published data – Police UK: Crime rates over 12 months	Suitable	Assessed suitable for regression analysis for the Town Deals (Table D.13 provides more details), but descriptive statistics only for the Future High Streets Fund due to insufficient sample size.

Source: Frontier Economics.

Table D.6 presents the candidate indicators identified for digital connectivity outcomes. The table presents the results of the assessment against the criteria set out above, by outcome and OM.

Table D.6 Data sources available to assess digital connectivity

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM5.2.1	Number of residential premises with access to superfast broadband	Published data – Ofcom: Superfast broadband availability for residential properties	Suitable for Town Deals only	Assessed suitable for descriptive statistics for the Town Deals only due to insufficient sample size for regression analysis (Table D.13 provides more details). Quantitative analysis not possible for the Future High Streets Fund due to insufficient sample size.
OM5.2.2	Number of businesses with access to superfast broadband	Appropriate data source not identified.	Not applicable	Not applicable.
OM5.2.3	Number of wifi hotspots in the area	Appropriate data source not identified.	Not applicable	Not applicable.
OM5.2.4	Change in average broadband speeds	Published data – Ofcom: Broadband performance data	Unsuitable	Lack of consistent reporting over the required period for analysis. Data on average broadband speeds is reported as an average across all connection types until 2023, and as an average by banded broadband capacity after this date. Although some average speed information is available by banded broadband capacity prior to 2023, these band definitions differ from those used after 2023. This, therefore, limits the comparability of this data over time.
OM5.2.5	Digital exclusion	Secure data – Understanding Society: Indicator of computer availability at home	Unsuitable	Lack of sufficient time series (data available annually until 2018 only).
OM5.2.5	Digital exclusion	Published data – Greater Manchester Office of Data Analytics: Digital Exclusion Index	Unsuitable	Lack of sufficient time series (data available for 2021 and 2025 only; a suitable pre-trend data point is missing).
OM5.2.6	Digital user classification	Appropriate data source not identified.	Not applicable	Not applicable.

Source: Frontier Economics.

Table D.7 presents the candidate indicators identified for local authority capabilities (Town Deals only) outcomes. The table presents the results of the assessment against the criteria set out above, by outcome and OM.

Table D.7 Data sources available to assess local authority capabilities (relevant for Town Deals only)

Outcome metric	Outcome of interest	Indicators identified	Assessment: Suitable for analysis?	Assessment reasoning
OM6.1.1	Change in the proportion of major planning applications decided in time	Published data – LGInform: Major decisions within 13 weeks	Suitable	Sufficient geographic granularity, frequency, time series and sample size for regression for the Town Deals (Table D.13 provides more details).
OM6.1.2	Change in current expenditure on cultural services	Published data – LGInform: Net expenditure on cultural services	Unsuitable	A further assessment of this OM concluded that it was not a suitable measure of the outcome. Expenditure primarily reflects budget choices and fiscal pressures rather than underlying delivery capacity.
OM6.1.3	Change in current expenditure on conservation and listed buildings planning policy	Published data – LGInform: Total expenditure on conservation and listed buildings planning	Unsuitable	A further assessment of this OM concluded that it was not a suitable measure of the outcome. Expenditure primarily reflects budget choices and fiscal pressures rather than underlying delivery capacity.
OM6.1.4	Change in collections of non-domestic rates	Published data – LGInform: Collection rate	Unsuitable	A further assessment of this OM concluded that it was not a suitable measure of the outcome. Collection rates depend heavily on wider economic conditions and tax policy, and so changes related to these factors may mask collection efficiency.
OM6.1.5	Change in total dwellings on the valuation list	Published data – LGInform: Total value of social rent dwelling stock	Unsuitable	A further assessment of this OM concluded that it was not a suitable measure of the outcome. Availability of total dwellings mainly reflects housing development and demographic trends, rather than an authority's underlying capacity or service quality.

Source: Frontier Economics.

D.2 Data sources used for this evaluation

This section summarises the full list of data sets used in the final quasi-experimental analysis.

Broadly, the analysis required 3 main types of data sets:

- **project information:** collated and provided by MHCLG
- **geographic shapefiles:** used to define the impact and displacement zones
- **variables used in the regression analysis:** including both outcome variables and covariates

D.2.1 Data processing and cleaning

The data were aggregated and cleaned using a consistent set of principles to ensure the final data set was suitable for robust analysis.

Aggregations of the raw data were performed at the town, concentric-circle, and high street levels, depending on the treatment unit used in the analysis. Read Annex A for details on the relevant level of aggregation for each regression, and Annex C for information on the aggregation methodology.

The cleaning process involved removing variables and observations where data quality was insufficient (missing, error or 0 values – while retaining measures where 0 values were considered 'valid'), reducing the influence of extreme values (capping outlier values), and standardising any covariates prior to modelling (such as de-meaning covariates).

Table D.8, Table D.9, Table D.10, Table D.11, Table D.12, and Table D.13 provide an overview of the data sets used in the final quantitative analysis, including key information on each data set and a summary of how each was used.

D.2.2 Data sources

PROJECT INFORMATION

Table D.8 sets out the variables used to identify the project information required for the regression analysis. This included project locations, project durations, delivery status and associated economic outcomes relevant to the programme-level evaluation. These data provide the basis for defining the treatment and timing of projects within the analysis.

Table D.8 Data sources used: Towns Fund project information

Variable	Data source	Version used	Granularity	How it is used in the analysis
Towns Fund Performance monitoring data	MHCLG (Towns Fund monitoring data)	Round 7 (available July 2025)	Projects	**Postcodes:** To map project locations to statistical geographies and define impact zones. **Project start and end dates:** To derive sample sizes used for regression analysis.
Towns Fund project descriptions	MHCLG (Towns Fund monitoring data)	Collated from April 2024 to August 2025	Projects	**Project descriptions:** To re-categorise projects into the relevant economic outcomes for regression analysis.
Future High Streets Fund Expressions of Interest (EoI)	MHCLG	December 2023	Projects	**Location descriptions:** To identify locations of Future High Streets Fund comparator high streets and map these to corresponding latitudes and longitudes.
Town Deals selection criteria (Table D.10 provides more details)	MHCLG	June 2023	Towns	**Selection criteria:** As a covariate in the PSM stage of the Town Deals impact regression analysis.

Source: *Frontier Economics.*

Note: *[A] Town Deals locations were selected based on a broader set of 10 variables. Selection variables based on RAG scoring (in particular, the Economic shock score, Investment Opportunities score, and Alignment with Government Interventions score) or categorisation (in particular, the Rurality score) were excluded because they did not provide sufficient variation to support econometric analysis.*

GEOGRAPHIC SHAPEFILES

Table D.9 sets out the polygon shapes used to define the geographic boundaries applied in the analysis. The analysis uses these shapes to define the relevant areas for the analysis, and to aggregate metrics at the appropriate geographic level when constructing the impact, comparator and displacement zones used in the evaluation. Read Annex B for details on these zones.

Table D.9 Data sources used: Geographic shapefiles

Variable	Data source	Publication version used	How it is used in the analysis
Lower layer Super Output Area (LSOA) and Output Area (OA) shapefiles	Published data – Office for National Statistics (ONS)	Data based on 2011 and 2021 publications (to align with the relevant geographic definitions for each data set; for instance, the Annual Population Survey uses 2011 boundary definitions)	To support metric aggregation for the Town Deals and Future High Streets Fund non-footfall impact assessments.
Built-up area (BUA) shapefiles	Published data – ONS	December 2022	To support metric aggregation for the Town Deals and Future High Streets Fund non-footfall impact assessments.
Local authority district (LAD) shapefiles	Published data – ONS	December 2021	To support metric aggregation for the Town Deals impact assessment of local authority capabilities.
Postcode shapefiles	Published data – Ordnance Survey	April 2024	To support metric aggregation for the Town Deals and Future High Streets Fund impact and displacement assessments.
High street shapefiles	Proprietary data – Place Informatics	July 2025	To support metric aggregation for the Future High Streets Fund footfall impact and displacement assessments.

Source: Frontier Economics.

DATA FOR REGRESSION ANALYSIS

Table D.10, Table D.11, Table D.12, and Table D.13 set out the outcome variables and covariates used in the impact and displacement regressions conducted for the programme-level evaluation. These variables provide the basis for the DiD analysis and are presented by data source.

Table D.10 presents the outcome variables and covariates for the Town Deals selection variables used in the impact and displacement regressions conducted for the programme-level evaluation.

Table D.10 Data sources used: Town Deals selection variables

Variable	Data source	Frequency of publication	Time period(s) used	Granularity	How it is used in the analysis	Transformations applied
Indices of Multiple Deprivation (IMD): Proportion of LSOAs in the most deprived decile in England	Department for Communities and Local Government (DCLG)	Irregular	2015	Town	As a covariate in the PSM stage of the Town Deals impact regression analysis, to capture pre-treatment deprivation levels.	None
Income deprivation – Income component of the IMD	DCLG / ONS	Irregular	2015	Town	As a covariate in the PSM stage of the Town Deals impact regression analysis, to capture pre-treatment deprivation levels.	None
Gross domestic household income (GDHI) per capita	ONS	Annual	2017	Town	As a covariate in the PSM stage of the Town Deals impact regression analysis, to capture pre-treatment income levels.	None
Productivity – GVA per hour worked	ONS	Annual	2017	NUTS3	As a covariate in the PSM stage of the Town Deals impact regression analysis, to capture pre-treatment productivity.	None

Variable	Data source	Frequency of publication	Time period(s) used	Granularity	How it is used in the analysis	Transformations applied
Skills deprivation – Proportion of working-age population (those aged 16 to 64) with no qualifications at National Vocational Qualification (NVQ) level	ONS	Annual	2018	Local authority	As a covariate in the PSM stage of the Town Deals impact regression analysis, to capture pre-treatment skills levels.	None
EU Exit exposure – GVA of sectors identified as 'at risk' with respect to a 'no-deal, no-transition' EU exit	MHCLG / ONS / Bank of England	Single publication	2017	NUTS3	As a covariate in the PSM stage of the Town Deals impact regression analysis, to capture the estimated exposure of local areas to the EU Exit.	None

Source: *MHCLG.*

Note: *[A] Town Deals locations were selected based on a broader set of 10 variables. Selection variables based on RAG scoring (in particular, the Economic shock score, Investment Opportunities score, and Alignment with Government Interventions score) or categorisation (in particular, the Rurality score) were excluded from the analysis as they did not provide sufficient variation to support econometric analysis. Read Annex E of the Feasibility Report for more details.*
[B] Selection for the Future High Streets Fund was not based on systematic scoring but on a subjective assessment of the submitted EoI. For this reason, similar selection variables have not been identified for this fund. Read Annex E of the Feasibility Report for more details.

Table D.11 presents secure data sources used in the impact and displacement regressions conducted for the programme-level evaluation.

Table D.11 Data sources used: Secure data

Variable	Data source	Frequency of publication	Time period(s) used	Granularity	How it is used in the analysis	Transformations applied
Business birth rate	IDBR	Rolling 12-month snapshots available annually for 2019 to 2022, and quarterly for Q1 2023 to Q2 2025	2019, Q2 2025 (12-month snapshot)	Individual businesses	As an outcome indicator for business outcomes for the Town Deals and Future High Streets Fund impact and displacement regression analyses. As a covariate for all other business outcomes regressions.	Calculated by taking the number of businesses born in a given year and dividing by the number of active businesses in the same year.[39]
Total employment	IDBR	Rolling 12-month snapshots available annually for 2019 to 2022, and quarterly for Q1 2023 to Q2 2025	2019, Q2 2025 (12-month snapshot)	Individual businesses	As an outcome indicator for business outcomes and job creation for the Town Deals and Future High Streets Fund impact and displacement regression analyses. As a covariate for all other business outcomes and job creation regressions.	Log transformation.

[39] Active businesses are defined as those with recorded turnover greater than 0 or a recorded number of employees greater than 0. This aligns with the approach taken by the Department for Business, Energy and Industrial Strategy in March 2018 to identify the number of canonical enterprises in the IDBR data set.

Variable	Data source	Frequency of publication	Time period(s) used	Granularity	How it is used in the analysis	Transformations applied
Concentration of sector employment shares, 2019 to 2025	ONS: ASHE	Annual	2019, 2025	Individuals	As an outcome indicator for employment mix for the Town Deals and Future High Streets Fund impact regression analyses. As a covariate for all other employment mix regressions.	Based on the Herfindahl–Hirschman Index (HHI), a common measure of market concentration. This is calculated by taking the sum of squared employment shares for each sector, where the shares are expressed as fractions. This provides a measure of sector concentration in the area. For the purpose of this analysis, businesses have been grouped into sectors using 2-digit SIC codes.
Proportion of part-time employees	ONS: ASHE	Annual	2019, 2025	Individuals	As an outcome indicator for job creation and employment mix for the Town Deals and Future High Streets Fund impact and displacement regression analyses. As a covariate for all other job creation and employment mix regressions.	Calculated as the number of part-time employees in a given period divided by the total number of employees in the same period.
Highest qualification	ONS: Annual Population Survey (APS)	Quarterly	2019	Individuals	As a covariate for Future High Streets Fund footfall impact and displacement regressions. This aims to capture differences in the socioeconomic status of residents in the area, which can influence consumer behaviour and, in turn, footfall patterns.	Calculated as the percentage of all adults with higher education (defined as having at least a GCSE, A-level or equivalent qualification).

Source: Frontier Economics.

Table D.12 presents the proprietary data used in the impact and displacement regressions conducted for the programme-level evaluation.

Table D.12 Data sources used for regression: Proprietary data

Source	Data source	Frequency of publication	Time period(s) used	Granularity	How it is used in the analysis	Transformations applied
Total monthly footfall	Place Informatics	Monthly	Q3 2019, Q3 2025 (3-month moving averages). Parallel trend test: Q2 2019, Q3 2019,[40] Q2 2019, Q3 2019	High street	As an outcome and covariate indicator for business outcomes in the Future High Streets Fund, footfall-related impact and displacement regression analyses.	Values are first normalised by population in each catchment area (Table D.13 provides variable details), then a 3-month moving average is taken. This value is then transformed into logs. When used as a covariate, the standard deviation of the logged pre-treatment level is used to capture differences in volatility across high streets. Changes in footfall-related metrics can be very volatile, so controlling for this measure helps isolate the treatment effect by accounting for the variation driven by underlying volatility.
Average monthly dwell time	Place Informatics	Monthly	Q3 2019, Q3 2025 (3-month moving averages). Parallel trend test: Q2 2019, Q3 2019,[41] Q2 2019, Q3 2019	High street	As an outcome and covariate indicator for business outcomes in the Future High Streets Fund, dwell time-related impact and displacement regression analyses.	A 3-month moving average is taken and then transformed into logs. When used as a covariate, the s.d. of the logged pre-treatment observation is taken (for the same reason as for 'Total footfall').

[40] Only 2 data points are available for parallel trend testing, due to limited data availability.
[41] Only 2 data points are available for parallel trend testing, due to limited data availability.

Source	Data source	Frequency of publication	Time period(s) used	Granularity	How it is used in the analysis	Transformations applied
Total monthly dwell time	Place Informatics	Monthly	Q3 2019, Q3 2025 (3-month moving averages). Parallel trend test: Q2 2019, Q3 2019,[42] Q2 2019, Q3 2019	High street	As an outcome and covariate indicator for business outcomes in the Future High Streets Fund, total time-related impact and displacement regression analyses.	Calculated as the product of average monthly footfall and average dwell time for a given month. This value is then transformed into logs.
Co-visitation (read Annex C for a description of this variable)	Place Informatics	Irregular	Computed for this analysis for the period Mar to Dec 2019	High street	As a covariate for the business outcomes in the Future High Streets Fund displacement regressions for all footfall-related indicators.	None.

Source: Frontier Economics.

Table D.13 presents the published data used in the impact and displacement regressions conducted for the programme-level evaluation.

[42] Only 2 data points are available for parallel trend testing, due to limited data availability.

Table D.13 Data sources used for regression: Published data

Variable	Data source	Frequency of publication	Time period(s) used	Granularity	How it is used in the analysis	Transformations applied
Access to Healthy Assets and Hazards (AHAH): Retail Environment domain	Consumer Data Research Centre (CDRC)	Irregular	2016	LSOA	As a covariate for the local wellbeing Town Deals impact and displacement regressions.	None.
Crime in England and Wales: Total crime offences (12-month total); Antisocial behaviour (12-month total); Burglary (12-month total); Robbery (12-month total); Shoplifting (12-month total); Theft (12-month total)	Police UK	Annual (publications run cover the 12 months from Sep to Aug each year)	2018 to 2019, 2024 to 2025	LSOA	As an outcome indicator for local wellbeing for the Town Deals impact and displacement regression analyses. As a covariate for all other local wellbeing regressions.	Log transformation.
Children aged 0 to 19 in relatively low-income out-of-work families	DWP	Annual	2019	OA	As a covariate for the business outcomes, Future High Streets Fund impact regressions for footfall-related indicators.	None.
Density of sport and leisure assets	Sport England	Irregular	April 2020	LSOA	As a covariate for local wellbeing, Town Deals impact regressions.	Log transformation.
Economic activity: Economically active: Full-time student; Economically active: Unemployed; Economically inactive	Census 2021	Every 10 Years	2021	OA	As a covariate for business outcomes, job creation and employment mix regressions for the Town Deals and Future High Streets Fund impact and displacement analyses.	Calculated as a percentage of the working-age population.
GVA per head	ONS	Annual	2019	LSOA	As a covariate for the business outcomes, Future High Streets Fund impact regressions for all footfall-related indicators.	None.

Variable	Data source	Frequency of publication	Time period(s) used	Granularity	How it is used in the analysis	Transformations applied
IMD: Overall score	MHCLG	Irregular	2019	LSOA	As a covariate for the business outcomes Future High Streets Fund impact and displacement regressions for all footfall-related indicators. As a covariate for the Local authorities' capabilities impact regression for the Town Deals.	None.
Number of jobs by sector[43]	BRES	Annual	2019	LSOA	As covariates for the business outcomes, job creation and employment mix, Town Deals and Future High Streets Fund impact and displacement regressions. These variables are expected to capture the jobs available in the area, and therefore the types of business operating there.	Calculated as a percentage of total employment.
Occupation group[44]	Census 2021	Every 10 Years	2021	OA	As covariates for the business outcomes, job creation and employment mix, Town Deals and Future High Streets Fund impact and displacement regressions. These variables provide information on the occupations of the area's inhabitants. This is likely to reflect different purchasing and spending habits.	Calculated as a percentage of total employment among the population aged 16 to 74.
Public sector employees	BRES	Annual	2019	LSOA	As covariates for the business outcomes, job creation and employment mix, Town Deals and Future High Streets Fund impact and displacement regressions.	Calculated as a percentage of total employment.

[43] The sectors reviewed include: Jobs in accommodation and food services (hospitality); Jobs in agriculture, forestry and fishing; Jobs in arts, entertainment, recreation and other services; Jobs in business administration and support services; Jobs in construction; Jobs in education; Jobs in financial and insurance; Jobs in health; Jobs in information and communication; Jobs in manufacturing; Jobs in mining, quarrying and utilities; Jobs in motor trades; Jobs in professional, scientific and technical; Jobs in property; Jobs in public administration and defence; Jobs in retail; Jobs in transport and storage (including postal); Jobs in wholesale.

[44] The occupations reviewed include: Administrative and secretarial occupations; Associate professional and technical occupations; Caring, leisure and other service occupations; Elementary occupations; Managers, directors and senior officials; Process, plant and machine operatives; Professional occupations; Sales and customer service occupations; Skilled trades occupations.

Variable	Data source	Frequency of publication	Time period(s) used	Granularity	How it is used in the analysis	Transformations applied
Population aged under 16; Population aged 16 to 64; Population aged over 65	Census 2021	Every 10 years	2021	OA	As a covariate for the business outcomes, Future High Streets Fund impact and displacement regressions for all footfall-related indicators.	None.
Population density (persons per sq/km)	ONS	Irregular	2018	OA	As a covariate for all Town Deals and Future High Streets Fund displacement regression analysis.	None.
Total population (estimate)	ONS	Irregular	2022 and 2024	OA	For normalising total footfall into per-capita terms for all Future High Streets Fund regression analysis.	None.
Unemployment benefit claimants (Jobseekers Allowance and out-of-work Universal Credit claimants)	DWP	Monthly[45]	November 2019 and November 2025. Parallel trend test: November 2018 and November 2019	LSOA	As an outcome indicator for job creation in the Town Deals and Future High Streets Fund, impact and displacement regression analyses. As a covariate for all other Future High Streets Fund regressions for non-footfall-related indicators.	Calculated in percentage terms, as a proportion of the adult population aged 16 to 64.
Major decisions made within 13 weeks by the Local authority	LGInform	Quarterly	2019 Q1 to Q4 and 2025 Q1	Local Authority District	As an outcome indicator for local authority capabilities for the Town Deals impact analysis.	Calculated as a share of total decisions taken.
2011 LADs Area Classification	ONS	Irregular	2011	Local Authority District	As a covariate for all local authority capabilities, Town Deals regressions.	None.

Source: *Frontier Economics.*

Note: *With the exception of data from LGInform, all data sets in this table were downloaded from public sources, collated and processed by OCSI Local Insight. Frontier Economics has used these data as provided.*

[45] Only 6-monthly snapshots were available for the analysis: November snapshots were available for the years 2018, 2019, 2020, 2023, 2024, and 2025.

Annex E Technical specifications

This annex presents the technical specifications used for the programme-level analysis, by outcome and indicator. The analysis in this annex underpins the evidence base for the evaluation findings.

The annex is structured as follows:

1. Annex E.1 provides an overview of the sample sizes available for each analysis.
2. Sections E.2 to E.8 set out the technical specifications for each of the outcomes in the final scope of the programme-level quantitatively assessed in this evaluation: business outcomes, job creation, property and land use, employment mix, local wellbeing, digital connectivity and local authority capabilities.

Read Annex A for more details on the quasi-experimental approach, and Annex D for more information on the data sources and indicators for this analysis.

E.1 Sample sizes available for analysis

This section provides an overview of the sample sizes available for analysis for each outcome.

These sample sizes represent the number of towns that had at least one relevant project completed during the evaluation period. For Future High Streets Fund footfall analysis, sample sizes refer to the number of high streets.

To arrive at the final sample sizes, the analysis:

- identifies the projects relevant to each outcome for each fund
- excludes cancelled projects and feasibility studies
- retains only projects completed by the relevant project cut-off date (see Annex A for more details)
- counts the number of towns or high streets that have at least one remaining project after these steps

Regression analysis was conducted, where the sample at this stage comprised at least 20 units.[46] Where samples fell between 10 and 20 units, descriptive analysis was used. Further data cleaning steps were then applied to remove towns with missing or poor data coverage.

Table E.1 and Table E.2 present the resulting sample sizes for the econometric analysis for each fund.

[46] The rule of thumb for statistical analysis suggests that sample sizes of 30 and above are 'sufficiently large' for the Central Limit Theorem to hold. The analysis relaxes this assumption, given the low sample sizes available, and applies a threshold of at least 20 treated observations.

Table E.1 presents the resulting sample sizes at each stage of the process for the Town Deals analysis. Regression methods were applied to 5 outcomes (business outcomes, job creation, employment mix, local wellbeing, and local authority capabilities), whereas the analysis was only able to report descriptive statistics for a further 2 outcomes (property and land use and digital connectivity). Quantitative analysis was not possible for the remaining outcomes.

Table E.1 Final sample sizes per outcome for Town Deals econometrics analyses

Outcome	Sample size: Following project categorisation	Sample size: After excluding cancelled projects and feasibility studies	Sample size: After imposing lag time constraints[A]	Quantitative analysis conducted	Sample size: After data cleaning
Business outcomes	83	83	23	Difference-in-differences (DiD) regression	22 to 23[B]
Property and land use	64	63	< 10 to 16[B]	Varies across indicators - descriptive analysis where sample size is >10	< 10 to 13[B]
Business decarbonisation	35	35	No metric available for analysis	None	Not applicable
Job creation	85	85	21 to 46[B]	DiD regression	16 to 43[B]
Employment mix	79	79	20	DiD regression	17 to 20[B]
Local wellbeing	81	80	22	DiD regression	13 to 20[B]
Social mobility	22	22	No metric available for analysis	None	Not applicable
Pride in place	86	86	No metric available for analysis	None	Not applicable
Physical connectivity	79	76	No metric available for analysis	None	Not applicable
Digital connectivity	38	37	18	Descriptive analysis	18
Local authority capabilities	58	58	29	DiD regression	29

Source: Frontier Economics, based on Towns Fund monitoring data (Round 7, available July 2025).

Note: [A] The sample sizes presented reflect the indicator-specific cut-off dates applied for each outcome. For each indicator, the analysis sets the cut-off date to maximise the lag between project completion and the latest available post-treatment observation. Read Annex A for more details.
[B] Sample sizes may vary across indicators for a given outcome. Ranges are presented to reflect the minimum and maximum sample size ranges across indicators for each outcome.

Table E.2 presents the resulting sample sizes at each stage of the process for the Future High Streets Fund analysis. This demonstrates that regression methods may be applied for only 2 outcomes (business outcomes, job creation), while the analysis can only report descriptive statistics for a further 2 outcomes (property and land use, local wellbeing). Quantitative analysis is not possible for the remaining outcomes.

Table E.2 Final sample sizes per outcome for Future High Streets Fund econometrics analyses

Outcome	Sample size: Following project categorisation	Sample size: After excluding cancelled projects and feasibility studies	Sample size: After imposing lag time constraints[A]	Quantitative analysis conducted	Sample size: After data cleaning
Business outcomes	59 to 60	57 to 60	28 to 42[B]	DiD regression	28 to 38[B]
Property and land use	46	44	11 to 30	Descriptive analysis[47]	11 to 29[B]
Business decarbonisation	< 10	< 10	No metric available for analysis	None	Not applicable
Job creation	53	52	15 to 27[A]	Varies across indicators – descriptive analysis and DiD regression	14 to 27[B]
Employment mix	17	16	< 10	None	Not applicable
Local wellbeing	48	47	16	Descriptive analysis	15 to 16[B]
Social mobility	< 10	< 10	No metric available for analysis	None	Not applicable
Pride in place	57	57	No metric available for analysis	None	Not applicable
Physical connectivity	40	39	No metric available for analysis	None	Not applicable
Digital connectivity	< 10	< 10	< 10	None	Not applicable

Source: *Frontier Economics, based on Towns Fund monitoring data (Round 7, available July 2025).*

Note: *[A] The sample sizes presented reflect the indicator-specific cut-off dates applied for each outcome. For each indicator, the analysis sets the cut-off date to maximise the lag between project completion and the latest available post-treatment observation. See Annex A for further details.*
[B] Sample sizes may vary across indicators for a given outcome. Ranges are therefore presented to reflect the min-max sample size across indicators for each outcome.

[47] In principle, it would be possible to apply DiD regression analysis for some indicators. However, the required data was made available too late for full regression analysis to be undertaken. The analysis therefore reports descriptive analysis instead.

E.2 Business outcomes

Table E.3 presents the indicators used by the programme-level analysis for business outcomes.

The analysis considered these indicators based on their relevance to measuring changes in business outcomes and the availability of suitable data. Read Annex D for more information on the variables explored and used in this analysis.

Regression analysis for impact is feasible for all selected indicators, although footfall-related indicators are assessed only for the Town Deals. Displacement analysis is relevant for all indicators. Read Annex A for more information on the rationale for excluding footfall indicators from the scope of impact analysis for the Future High Streets Fund.

Table E.3 Quantitative analysis conducted for business outcomes

Item	Impact assessment – Town Deals	Impact assessment – Future High Streets Fund	Displacement assessment – Town Deals	Displacement assessment – Future High Streets Fund
Change in total number of businesses, 2019 to 2025	DiD regression analysis	DiD regression analysis	Concentric circles regression analysis	Concentric circles regression analysis
Change in business birth rate, 2019 to 2025	DiD regression analysis	DiD regression analysis	Concentric circles regression analysis	Concentric circles regression analysis
Change in total employment, 2019 to 2025	DiD regression analysis	DiD regression analysis	Concentric circles regression analysis	Concentric circles regression analysis
Change in average monthly footfall, Q3 2019 to Q3 2025 (Future High Streets Fund only)	None – Not applicable to Town Deals analysis	DiD regression analysis	None – Not applicable to Town Deals analysis	Neighbouring areas regression analysis
Change in average monthly dwell time, 2019 to 2025 (Future High Streets Fund only)	None – Not applicable to Town Deals analysis	DiD regression analysis	None – Not applicable to Town Deals analysis	Neighbouring areas regression analysis
Change in total monthly dwell time, 2019 to 2025 (Future High Streets Fund only)	None – Not applicable to Town Deals analysis	DiD regression analysis	None – Not applicable to Town Deals analysis	Neighbouring areas regression analysis

Source: Frontier Economics.

The following sections set out the technical specification and results for these indicators for both the Town Deals and Future High Streets Fund.

E.2.1 Town Deals business outcomes: Impact assessment

The Town Deals programme-level impact assessment uses a difference-in-differences (DiD) approach with propensity score matching (PSM). This entails the following steps:

1. **PSM** to identify a suitable comparator population. Covariate balancing propensity scores (CBPS) is conducted as a cross-check.
2. **Weighted average means comparison** of the indicator of interest, before and after the treatment period and between the treatment and matched comparator groups, to estimate the Average Treatment Effect on the Treated (ATT).

This section is structured as follows:

1. **Detailed results:** Presents the regression results for changes in the Total number of businesses. The report places greater emphasis on this indicator because the regression identifies statistically significant effects.
2. **Additional results:** Summarises the findings for the remaining 2 Business outcome indicators tested for the Town Deals, for which the estimated impacts are not statistically significant. These null results do not necessarily imply no impact; limited sample sizes, incomplete project delivery within the evaluation window, and short post-completion periods may reduce the ability to detect effects, particularly where impacts materialise gradually.
3. **Model diagnostics and tests:** Sets out the validation diagnostic checks underpinning the reported findings. This includes coverage checks, a review of the covariates used and tests to assess the credibility of the counterfactual and the match quality.

DETAILED RESULTS: TOTAL NUMBER OF BUSINESSES

Table E.4 presents the central model and sensitivity testing for the impact analysis of Town Deals on the number of businesses.

This indicates that the Towns Fund has had an estimated positive effect on the change in the total number of businesses between 2019 and 2025, of between 0.9% to 2.4%.

The central specification shows a relatively small, non-statistically significant effect. However, the estimated impact size varies with the specification applied; for example, when outlier treatment is changed. As the impact size increases when outliers are dropped, this indicates that the central estimate may be reduced by the presence of extreme values. In addition, the CBPS matching approach yields a larger and more statistically significant estimate than the central model. Together, these sensitivity results point to a consistent positive effect, although the magnitude depends on the matching assumptions used.

Table E.4 Town Deals – Impact analysis: business outcomes – Change in the number of businesses – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	85	23	0.011	0.016	1.06%
Testing parameters for matching: PSM – More stringent calliper	84	21	-0.001	0.0163	-0.13%
Testing matching approach: CBPS	441	23	0.023*	0.0138	2.36%
Testing outlier treatment: PSM – Dropping outliers	83	23	0.014	0.0152	1.37%
Testing outlier treatment: PSM – No outlier treatment	85	23	0.009	0.0165	0.94%
Testing the scope of the sample: PSM – 3-month lag	98	27	0.01	0.0147	1.02%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

ADDITIONAL RESULTS: BUSINESS BIRTH RATE, TOTAL EMPLOYMENT

Table E.5 presents the central model and sensitivity testing for the impact analysis of Town Deals on **business birth rates**. It shows that the central model estimated treatment effects are small and not statistically significant. These results are consistent across variations in matching parameters, matching approaches, and sample scope.

Table E.5 Town Deals – Impact analysis: business outcomes - Change in business birth rate – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	85	23	-0.002	0.0071	-0.16%
Testing parameters for matching: PSM – More stringent calliper	84	21	-0.002	0.0077	-0.24%
Testing matching approach: CBPS	441	23	-0.003	0.0069	-0.35%
Testing outlier treatment: PSM – Dropping outliers	84	22	0.004	0.0053	0.41%
Testing outlier treatment: PSM – No outlier treatment	85	23	-0.002	0.0075	-0.21%
Testing the scope of the sample: PSM – 3-month lag	98	27	-0.001	0.0062	-0.14%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.6 presents the central model and sensitivity testing for the impact analysis of Town Deals on **total employment**. It shows that the central model estimated treatment effects are small and not statistically significant. These results are consistent across variations in matching parameters, matching approaches, and sample scope.

Table E.6 Town Deals – Impact analysis: business outcomes – Change in total employment – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	70	22	-0.026	0.0348	-2.58%
Testing parameters for matching: PSM – More stringent calliper	73	21	-0.023	0.0351	-2.28%
Testing matching approach: CBPS	441	23	-0.021	0.0205	-2.03%
Testing outlier treatment: PSM – Dropping outliers	68	22	0.001	0.0200	0.09%
Testing outlier treatment: PSM – No outlier treatment	70	22	-0.033	0.0406	-3.24%
Testing the scope of the sample: PSM – 3-month lag	85	26	-0.025	0.0307	-2.49%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Overall, the analysis cannot rule out 0 impact based on these results. At this stage, any impacts are likely to be modest or may materialise over a longer timeframe.

MODEL DIAGNOSTICS AND TESTS

This section presents the empirical results of the testing and diagnostics for the impact analysis of the Town Deals on business outcomes.

These tests check the performance of the PSM stage of the analysis and are common across the business outcomes indicators.

The relevant checks include:

- the coverage of treated observations available for analysis
- the covariates included for the PSM specification
- the diagnostic tests used to assess counterfactual credibility, including covariate selection and final balance checks
- the diagnostic tests used to assess match quality, including common support checks, match statistics and off-support checks

Checking coverage of the analysis

Table E.7, Table E.8, and Table E.9 present the sample sizes at each stage of the Town Deals business outcomes impact analysis for each indicator.

Table E.7 presents the sample sizes at each stage of the Town Deals number of businesses impact analysis. It shows that the matching stage retains nearly all treated towns. Applying the project completion cut-off restrictions reduces the final sample to around one-third of its original size. This corresponds to only 10.4% on all the Town Deals-funded projects. This implies a limited coverage of the treatment in the final impact analysis. As projects that take longer to complete may have different levels of impact on changes in business outcomes, results should therefore be interpreted conservatively, and the findings should not be generalised to the full sample at this stage.

Table E.7 Town Deals – Impact analysis: business outcomes – Number of businesses – Coverage checks: Sample sizes at each stage

Stage of the analysis	Number of projects	Proportion of projects remaining in the sample	Number of treated towns	Proportion of treated towns remaining in the sample	Number of comparator towns
Total: All projects or towns	263	100%	83	100%	452
Total available for Stage 1: All projects or towns, after data cleaning	255	97% of Total	79	95% of Total	441
Total available for Stage 2: All projects or towns, excluding cancelled projects, incomplete projects and feasibility studies	251	95% of Total	83	100% of Total	441
Stage 1 PSM: Stage 1 sample after matching	255	97% of the Stage 1 sample	79	99% of the Stage 1 sample	197
Stage 2 ATT: Stage 2 sample after applying project completion cut-offs	26	10% of the Stage 2 sample	23	28% of the Stage 2 sample	85

Source: Frontier Economics.

Table E.8 presents the sample sizes at each stage of the Town Deals business birth rate impact analysis. It shows that the matching stage retains all treated towns. Applying the project completion cut-off restrictions reduces the final sample to around one-third of its original size. This corresponds to only 10.4% on all the Town Deals-funded projects.

Table E.8 Town Deals – Impact analysis: business outcomes – Business birth rate – Coverage checks: Sample sizes at each stage

Stage of the analysis	Number of projects	Proportion of projects remaining in the sample	Number of treated towns	Proportion of treated towns remaining in the sample	Number of comparator towns
Total: All projects/towns	263	100%	83	100%	452
Total available for Stage 1: Total after data cleaning	255	97% of Total	79	95% of Total	441
Total available for Stage 2: All projects or towns, excluding cancelled projects, incomplete projects and feasibility studies	251	95% of Total	83	100% of Total	441
Stage 1 PSM: Stage 1 sample after matching	255	100% of the Stage 1 sample	79	100% of the Stage 1 sample	197
Stage 2 ATT: Stage 2 sample after applying project completion cut-offs	26	10% of the Stage 2 sample	23	28% of the Stage 2 sample	85

Source: Frontier Economics.

Table E.9 presents the sample sizes at each stage of the Town Deals total employment analysis. It shows that the matching stage retains nearly all treated towns. Applying the project completion cut-off restrictions reduces the final sample to around one-third of its original size. This corresponds to only 10.4% on all the Town Deals-funded projects.

Table E.9 Town Deals – Impact analysis: business outcomes – Total employment – Coverage checks: Sample sizes at each stage

Stage of the analysis	Number of projects	Proportion of projects remaining in the sample	Number of treated towns	Proportion of treated towns remaining in the sample	Number of comparator towns
Total: All projects or towns	263	100%	83	100%	452
Total available for Stage 1: All projects or towns, after data cleaning	255	97% of Total	79	95% of Total	441
Total available for Stage 2: All projects or towns, excluding cancelled projects, incomplete projects and feasibility studies	251	95% of Total	83	100% of Total	441
Stage 1 PSM: Stage 1 sample after matching	247	97% of the Stage 1 sample	77	93% of the Stage 1 sample	156
Stage 2 ATT: Stage 2 sample after applying project completion cut-offs	23	9% of the Stage 2 sample	22	27% of the Stage 2 sample	70

Source: Frontier Economics.

Testing the credibility of the comparator group

Covariate selection

The Town Deals impact assessment applies covariate selection using the Least Absolute Shrinkage and Selection Operator (LASSO) method within the PSM stage of the analysis. Read Annex A for more details on the LASSO approach.

A long list of potential covariates was specified a priori for this analysis, based on economic reasoning and data availability, with the objective of capturing pre-treatment factors that plausibly influence both the likelihood of receiving the intervention and the outcomes of interest. The long list was restricted to variables measured prior to the intervention and available consistently across locations, to avoid conditioning on post-treatment changes and to support comparability.

This list comprised 38 variables capturing:

1. **Town Deals model selection variables:** variables capturing key local characteristics (including measures of local productivity, skills, and deprivation), which were used to inform the selection of towns for the Town Deals.[48]
2. **Business environment:** pre-treatment business demographics (such as sector composition, business births, and turnover per employee), as well as pre-treatment levels of the indicators of interest.
3. **Local socioeconomic demographics:** population characteristics (such as age structure, education, occupational mix, and unemployment) were included to reflect local consumer behaviour that can shape high-street activity.

Across the PSM specifications for the business outcomes indicators, the LASSO selection retained several useful pre-treatment indicators from the long list of potential covariates. Annex D provides a description of each variable and its source.

For the **number of businesses**, selected covariates were:

- Economically active: Full-time student (Census 2021)
- Economically active: Unemployed (Census 2021)
- Jobs in construction (2019)
- Jobs in financial and insurance (2019)
- Occupation group: Administrative and secretarial occupations (Census 2021)
- Occupation group: Elementary occupations (Census 2021)
- Occupation group: Skilled trades occupations (Census 2021)
- Index of multiple deprivation rank (2015)

For the **business birth rate**, selected covariates were:

- Economically active: Full-time student (Census 2021)
- Economically active: Unemployed (Census 2021)
- Jobs in construction (2019)
- Jobs in financial and insurance (2019)
- Occupation group: Administrative and secretarial occupations (Census 2021)
- Occupation group: Elementary occupations (Census 2021)
- Occupation group: Skilled trades occupations (Census 2021)
- Index of multiple deprivation rank (2015)

For **total employment**, selected covariates were:

- EU Exit exposure index (2017)
- Jobs in construction (2019)
- Occupation group: Administrative and secretarial occupations (Census 2021)
- Occupation group: Elementary occupations (Census 2021)
- Occupation group: Managers, directors and senior officials (Census 2021)

[48] The full list of selection variables are: Gross value added (GVA) per hour (2017), Proportion of working age population (those aged 16 to 64) with no qualifications (2018), Indices of Multiple Deprivation (IMD): Proportion of LSOAs in the most deprived decile in England (2015), Income component of the Index of Multiple Deprivation (2015), GVA of sectors at risk identified by the Bank of England with respect to a no-deal, no-transition EU exit (2017), as well as qualitatively assessed scores to measure town exposure to economic shocks, investment opportunities and alignment with wider government interventions.

- Occupation group: Sales and customer service occupations (Census 2021)
- Gross Value Added (GVA) per head (2019)
- Total employment (2019)
- Index of multiple deprivation rank (2015)

Several indicators of business activity were retained, while most demographic variables were discarded. This is consistent with economic expectations, as indicators of business activity are likely to be stronger predictors of business-related outcomes, and were likely also correlated with selection for funding.

Balancing checks

Figure E.1 shows the absolute standardised mean differences (ASMD) for the propensity score and covariates before matching (blue markers), after matching in Stage 1 (orange markers), and after the application of the project completion cut-offs and removal of any cancelled and non-feasible projects in Stage 2 (green markers). A dashed line marks the conventional balance threshold of 0.1.[49]

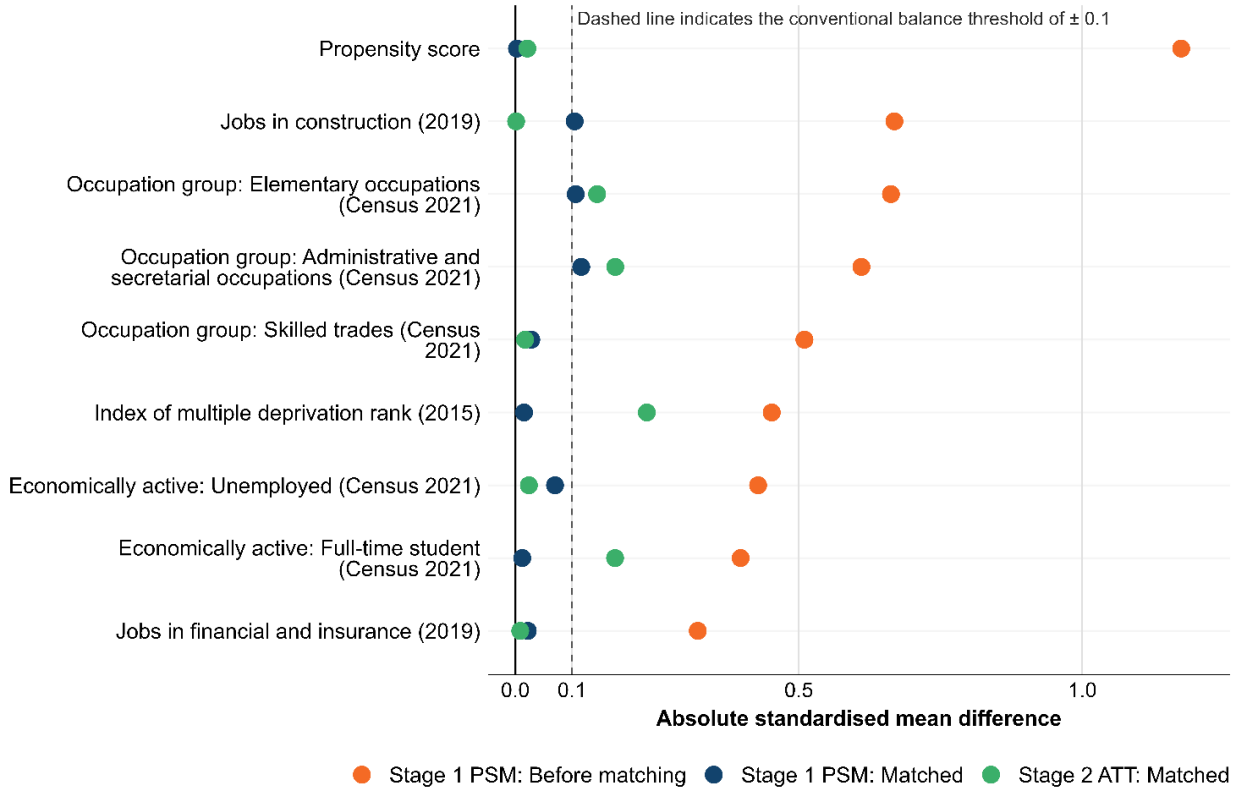
The figure illustrates the results for the number of businesses, although the balance diagnostics and findings are very similar for the business birth rate and total employment indicators. Read more about the 2 stages of the PSM approach used in this analysis in Annex A.2.

As indicated in the figure, prior to matching, there is a material imbalance between the treated and comparator towns on the propensity score and several covariates, with many ASMDs well above 0.1 and significantly different from 0. Stage 1 matching substantially improves balance, with the propensity score moving close to 0 and the ASMD for most covariates seeing large reductions (although some covariates remain above the 0.1 threshold). After applying the project completion cut-offs and moving to the Stage 2 ATT sample, the propensity score remains well-balanced and within the conventional threshold. Covariate balance weakens slightly for a small number of variables, although it remains markedly better than in the unmatched sample.

Overall, Figure E.1 demonstrates that matching substantially improves covariate balance and increases comparability between treated and comparator towns with respect to observed pre-treatment characteristics, relative to the unmatched sample.

[49] The conventional balance threshold is an ASMD of less than 0.1. A value below 0.1 indicates good balance, meaning the matching or weighting successfully reduced differences between the groups. See Belitser and others (2011), Ali and others (2014), and Stuart and others (2013).

Figure E.1 Town Deals – Impact analysis: business outcomes – Number of businesses – Propensity score and covariate balance



Source: Frontier Economics.

Testing match quality

Common support

The analysis reviews the estimated propensity score distributions for treated and comparator towns at 3 stages, to assess overlap and confirm the plausibility of the matched comparators:

1. **Before matching**, there is good overlap in general between the treated and comparator distributions for low-to-mid propensity scores. The overlap is weaker, however, in the upper range, where treated towns are more concentrated. Like-for-like comparisons may therefore be limited for treated towns with the highest propensity scores.
2. **After matching**, all treated towns are successfully matched through the PSM. The comparator distribution shifts towards the treated distribution and becomes more concentrated in the same propensity score range, improving the similarity of the matched comparator and the treated group in the upper tail. This reflects the fact that the matching procedure selects and places greater weight on comparator towns whose propensity scores are most similar to those of the treated group.

3. **When the analysis is restricted to the ATT sample**, the treated distribution narrows, removing observations with weaker support at the extremes of the propensity score distribution. This implies that the estimated ATT applies to the subset of treated towns with suitable matches. The treated and comparator distributions are not perfectly aligned, but remain broadly similar across the main area of common support. This suggests that the comparators remain plausible for the retained sample.

Match statistics and off-support check

There are fewer than 10 treated towns which remain unmatched at the PSM stage for the **number of businesses** and **total employment**. All treated towns are matched in the PSM stage for **business birth rate**.

Matching has therefore been successful for the majority of treated towns in the analysis.

Off-support tests are nevertheless conducted to assess whether systematic differences between matched and unmatched observations may introduce bias. However, as counts are below 10, reporting on these unmatched observations is not possible to avoid possible secondary disclosure. Read the UKDS Research data handling and security guide for users (2025) for more information.

E.2.2 Town Deals business outcomes: Displacement assessment

For the business outcome indicators, the analysis assesses potential displacement for all indicators:

1. For the **number of businesses** and the **business birth rate**, displacement is relevant, as regeneration activity may encourage firms to relocate or concentrate in treated areas, thereby redistributing activity from nearby centres rather than generating additional business activity.
2. For **total employment**, displacement analysis is relevant as employment activity could plausibly shift across neighbouring areas (for example, through firm relocation or rebalancing staff across sites).

The displacement assessment for the business outcomes indicators estimates impacts across a series of concentric rings around the Town Deals project locations. This approach assesses how effects vary with proximity and helps identify potential displacement or spillover effects. Read Annex A.4 for more details on the concentric rings methodology.

DETAILED RESULTS: TOTAL NUMBER OF BUSINESSES

Table E.10 presents the central model and sensitivity testing of the displacement analysis for changes in the **number of businesses**.

The central specification shows a small, positive, but not statistically significant, displacement effect within a town. Sensitivity checks produce similar estimates and are statistically insignificant in most cases, indicating limited evidence of potential spillover or displacement effects.

Table E.10 Town Deals – Displacement analysis: business outcomes – Change in the number of businesses – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	23	229	0.007	0.0089
Testing the extent of exposure: 3km radius	23	68	0.013	0.0091
Testing the extent of exposure: 5km radius	23	114	0.010	0.0119
Testing the scope of the sample: 3-month lag	27	269	0.010	0.0093

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

When applying a later project cut-off date to broaden the sample scope, the treatment term is larger and statistically significant (at the 5% level). While this is consistent with potential spillover effects from the central impact zone into the surrounding rings, taken together with results from the remaining sensitivities, it can only be interpreted as indicative.

ADDITIONAL RESULTS: BUSINESS BIRTH RATE, TOTAL EMPLOYMENT

Table E.11 and Table E.12 present the central model and sensitivity testing of the displacement analysis for changes in the **business birth rate** and in **total employment**.

Table E.11 shows that the coefficient of interest is negative but very close to 0 and not statistically significant in the central specification. These results provide no evidence of systematic co-movement in business birth rate between the central impact zone and the outer rings in employment outcomes over the period assessed.

Note that this finding is consistent with the earlier result of no detectable programme impact for the business birth rate. Evidence of positive co-movement between the centre and surrounding areas reflects that business births in the centre and surrounding areas tend to rise and fall together over time. This suggests that if any future programme effect is identified, it is likely that changes may also be seen in nearby areas. The analysis, therefore, provides contextual, indicative evidence about potential spillovers rather than causal evidence of displacement.

Table E.11 Town Deals – Displacement analysis: business outcomes – Change in business birth rate – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	22	219	0.007*	0.0036
Testing the extent of exposure: 3km radius	22	65	0.008***	0.0023
Testing the extent of exposure: 5km radius	22	109	0.005	0.0036
Testing the scope of the sample: 3-month lag	26	259	0.011**	0.0045

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.12 shows that the coefficient is negative, not statistically significant, but very close to 0 in the central specification. Most sensitivity checks report similarly close to 0 interaction terms. This indicates no evidence of potential spillover or displacement effects for changes in total employment.

Table E.12 Town Deals – Displacement analysis: business outcomes – Change in total employment – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	23	229	-0.003	0.0058
Testing the extent of exposure: 3km radius	23	68	-0.012	0.0082
Testing the extent of exposure: 5km radius	23	114	0.007	0.0059
Testing the scope of the sample: 3-month lag	27	269	-0.001	0.0067

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

E.2.3 Future High Streets Fund business outcomes: Impact assessment

The Future High Streets Fund impact assessment is based on a DiD approach. Read Annex A.3 for more details on this approach.

This section is structured as follows:

1. **Detailed results:** Presents the regression results for changes in the average footfall, average dwell time, and total dwell time on high streets. The report places greater emphasis on these indicators because the regression identifies statistically significant effects.
2. **Additional results:** Summarises the findings for the remaining non-footfall business outcomes indicators tested for the Town Deals, for which the estimated impacts are not statistically significant. These null results do not necessarily imply no impact; limited sample sizes, incomplete project delivery within the evaluation window, and short post-completion periods may reduce the ability to detect effects, particularly where impacts materialise gradually.
3. **Model diagnostics and tests:** Sets out the validation diagnostic checks underpinning the reported findings. This includes coverage checks, a review of the covariates used and tests to assess the credibility of the counterfactual and the match quality.

DETAILED RESULTS: AVERAGE FOOTFALL, AVERAGE DWELL TIME, AND TOTAL DWELL TIME

Annex A.3 presents the central specification that the analysis uses and Annex A.7 provides details on the sensitivity testing.

Table E.13, Table E.14 and Table E.15 present the main regression results on the impacts of the Future High Streets Fund on **average footfall**, **average monthly dwell time**, and **total monthly time spent on high streets**.

Table E.13 shows that the central model shows a statistically significant increase in footfall of 6.7% relative to the comparator towns. This positive impact is found to be stable across the sensitivity checks. This provides good evidence that the programme is likely to have generated a genuine uplift in high street footfall.

Table E.13 Future High Streets Fund – Impact analysis: business outcomes – Change in average monthly footfall – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	147	38	0.065***	0.0227	6.73%
Testing outlier treatment: DiD – dropping outliers	146	36	0.039**	0.0166	3.94%
Testing outlier treatment: DiD – no outlier treatment	147	38	0.077***	0.0290	8.06%
Testing the scope of the sample: DiD – 2-month lag	147	47	0.055***	0.0185	5.70%

Source: *Frontier Economics.*

Note: *[A] Annex A.3 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.14 shows that the central model indicates a 3% treatment effect on dwell time, but it is not statistically significant. The estimated effect size varies slightly across sensitivity checks and becomes statistically significant under some tests. This pattern suggests a possible positive effect on dwell time, although the evidence is not conclusive at this stage.

Table E.14 Future High Streets Fund – Impact analysis: business outcomes – Change in average monthly dwell time – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	147	38	0.030*	0.0180	3.03%
Testing outlier treatment: DiD – dropping outliers	145	36	0.020	0.0178	2.05%
Testing outlier treatment: DiD – no outlier treatment	147	38	0.036*	0.0190	3.71%
Testing the scope of the sample: DiD – 2-month lag	147	47	0.034**	0.0160	3.44%

Source: *Frontier Economics.*

Note: *[A] Annex A.3 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.15 shows that the central model shows that the treatment increased the total time spent on the high streets by 17%. The impact size is stable and statistically significant across the sensitivity tests. This consistent pattern suggests that the programme likely increased overall time spent on high streets.

Table E.15 Future High Streets Fund – Impact analysis: business outcomes – Change in total monthly dwell time – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	147	38	0.157***	0.0400	17.0%
Testing outlier treatment: DiD – dropping outliers	146	36	0.109***	0.0302	11.5%
Testing outlier treatment: DiD – no outlier treatment	147	38	0.181***	0.0494	19.8%
Testing the scope of the sample: DiD – 2-month lag	147	47	0.144***	0.0318	15.5%

Source: *Frontier Economics.*

Note: *[A] Annex A.3 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

ADDITIONAL RESULTS: TOTAL NUMBER OF BUSINESSES, BUSINESS BIRTH RATE, TOTAL EMPLOYMENT

The regression analysis generally finds no consistent, statistically significant impact of the Future High Streets Fund on the remaining business outcomes indicators. This is likely because the sample is too small and the post-completion analysis window is too short, which limits the ability of the analysis to identify longer-lived impacts.

Table E.16, Table E.17 and Table E.18 report the estimated impact of the Future High Streets Fund on the change in the **total number of businesses**, **business birth rate**, and **total employment**.

Table E.16 shows that the analysis estimates a small negative effect on the number of businesses across all specifications. Across most specifications, this effect is statistically significant. This suggests a robust short-run reduction in the number of businesses relative to the comparator towns, which may reflect transitional disruption during delivery rather than longer-term effects. In addition, 72% of the relevant projects in the towns included in the analysis were not completed before the cutoff. As a consequence, these might have caused further disruptions in these towns. As such, these findings should be interpreted as evidence of early post-completion impacts only, rather than the programme's long-run impacts.

Table E.16 Future High Streets Fund – Impact analysis: business outcomes – Change in the number of businesses – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	154	28	-0.029*	0.0152	-2.84%
Testing outlier treatment: DiD – dropping outliers	152	28	-0.031**	0.0148	-3.01%
Testing outlier treatment: DiD – no outlier treatment	154	28	-0.023	0.0174	-2.28%
Testing the scope of the sample: DiD – 3-month lag	154	36	-0.032**	0.0151	-3.19%

Source: *Frontier Economics.*
Note: *[A] Annex A.3 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.17 shows that the treatment effect estimated by the central model is positive, small and insignificant. This result is stable across the sensitivity checks. This suggests that there is no measurable short-run effect of the Future High Streets Fund on business formation at this stage.

Table E.17 Future High Streets Fund – Impact analysis: business outcomes – Change in business birth rate – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	154	28	0.002	0.0019	0.17%
Testing outlier treatment: DiD – dropping outliers	152	28	0.001	0.0019	0.13%
Testing outlier treatment: DiD – no outlier treatment	154	28	0.001	0.0019	0.13%
Testing the scope of the sample: DiD – 3-month lag	154	36	0.001	0.0017	0.13%

Source: *Frontier Economics.*
Note: *[A] Annex A.3 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.18 shows that estimated impacts on total employment are small, negative and statistically insignificant across the central model and sensitivity specifications. There is no clear evidence, therefore, at this stage of the analysis, that the Future High Streets Fund has had any clear impact on employment.

Table E.18 Future High Streets Fund – Impact analysis: business outcomes – Change in total employment –Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	154	28	0.005	0.0160	0.54%
Testing outlier treatment: DiD – dropping outliers	153	27	-0.009	0.0144	-0.85%
Testing outlier treatment: DiD – no outlier treatment	154	28	0.017	0.0174	1.70%
Testing the scope of the sample: DiD – 3-month lag	154	36	-0.008	0.0136	-0.81%

Source: Frontier Economics.

Note: [A] Annex A.3 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

MODEL DIAGNOSTICS AND TESTS

This section presents the empirical results of the testing and diagnostics for the impact analysis of the Future High Streets Fund on business outcomes.

The relevant checks include the:

- coverage of treated observations available for analysis
- diagnostic tests used to assess counterfactual credibility, including covariate selection and parallel trends

Checking coverage of the analysis

Table E.19 presents the sample sizes at each stage of the Future High Streets Fund business outcomes impact analysis for each of the indicators being assessed.

It shows that the footfall analysis is based on a relatively small sample size (38 treated towns). While this exceeds conventional minimum thresholds for quantitative analysis, the sample is relatively small, which limits statistical power and increases uncertainty around the estimates.[50] Results should therefore be interpreted conservatively, and the findings at this stage should not be generalised to the full sample.

[50] The rule of thumb for statistical analysis suggests that sample sizes of 30 and above are 'sufficiently large' for the Central Limit Theorem to hold. We have relaxed this assumption, given the low sample sizes available for this analysis, and ensured that a threshold of at least 20 treated observations is met.

Table E.19 Future High Streets Fund– Impact analysis: business outcomes – Footfall-related metrics – Coverage checks – Sample sizes at each stage

Indicator	Number of projects	Proportion of projects remaining in the sample	Number of treated towns	Proportion of treated towns remaining in the sample	Number of comparator towns
Total: All projects and towns	245	100%	60	100%	155
Total available for DiD: All projects and towns, excluding cancelled projects, incomplete projects and feasibility studies	228	93% of Total	60	100% of Total	147
DiD: Change in average monthly footfall, 2019 to 2025	75	33% of DiD sample	38	63% of DiD sample	147
DiD: Change in average monthly dwell time, 2019 to 2025	75	33% of DiD sample	38	63% of DiD sample	147
DiD: Change in total monthly dwell time, 2019 to 2025	75	33% of DiD sample	38	63% of DiD sample	147

Source: Frontier Economics.

Testing the credibility of the comparator group

Covariate selection

The Future High Streets Fund impact assessment applies a double-LASSO method for covariate selection. Read Annex A.3 for more details on the double-LASSO method.

A long list of potential covariates was specified a priori for this analysis, based on economic reasoning and data availability, with the objective of capturing pre-treatment factors that plausibly influence both the likelihood of receiving the intervention and the outcomes of interest. The long list was restricted to variables measured prior to the intervention and available consistently across locations, to avoid conditioning on post-treatment changes and to support comparability.

This list comprised 25 variables capturing:

1. **Baseline footfall conditions:** pre-treatment footfall levels and related metrics were included because they are typically strong predictors of future footfall and help to account for baseline differences across locations.
2. **Business environment:** pre-treatment business demographics (such as sector composition, business births, and turnover per employee) were included to represent high-street structure and local economic activity (likely determinants of footfall dynamics).
3. **Local socioeconomic demographics:** population characteristics (such as age structure, education, occupational mix, and unemployment) were included to reflect local consumer behaviour that can shape high-street activity.

4. **Local economic conditions:** deprivation and poverty measures (such as the Index of Multiple Deprivation [IMD] and children in low-income households) were included to capture underlying economic constraints that may affect spending patterns and footfall. These characteristics were also likely to be associated with the initial allocation of the Future High Streets Fund.

Across the central specifications for the business outcome indicators, the double-LASSO process consistently retained pre-treatment measures that reflect the key factors outlined above and that plausibly influence both footfall dynamics and programme selection.

Annex D provides a description of each variable and its source.

For **average monthly footfall**, selected covariates were as follows:

- Children aged 0 to 19 in relatively low-income out-of-work families (2019)
- Business births vs business deaths ratio (2019)
- Footfall (3-month moving average) (Q3 2019)
- Occupation group: Caring, leisure and other service occupations (Census 2021)
- Occupation group: Managers, directors and senior officials (Census 2021)
- Occupation group: Skilled trades occupations (Census 2021)
- Percentage of people with qualification Level 3+ (2019)
- Population aged over 65 (Census 2021)

For **average monthly dwell time**, selected covariates were as follows:

- Change in Unemployment benefit claimants (Jobseekers Allowance [JSA] and out-of-work Universal Credit [UC] claimants) (2019 vs 2025)
- Dwell-time (3-month moving average) (2019)
- Occupation group: Administrative and secretarial occupations (Census 2021)
- Occupation group: Caring, leisure and other service occupations (Census 2021)
- Occupation group: Managers, directors and senior officials (Census 2021)
- Occupation group: Process, plant and machine operatives (Census 2021)
- Percentage of people with qualification Level 3+ (2019)
- Turnover per employee (2019)
- Unemployment benefit claimants (JSA and out-of-work UC claimants) (2019)

For **total monthly dwell time**, selected covariates were as follows:

- Children aged 0 to 19 in relatively low-income out-of-work families (2019)
- Business births vs business deaths ratio (2019)
- Change in Unemployment benefit claimants (JSA and out-of-work UC claimants) (2019 vs 2025)
- Total time (3-month moving average) (Q3 2019)
- Occupation group: Caring, leisure and other service occupations (Census 2021)
- Occupation group: Managers, directors and senior officials (Census 2021)
- Occupation group: Skilled trades occupations (Census 2021)
- Population aged over 65 (Census 2021)
- Turnover per employee (2019)

Parallel trends

Parallel-trend testing is a key diagnostic tool for the DiD analysis of footfall indicators.

Table E.20 presents the results of the parallel-trend testing for all 3 indicators.

The analysis conducted limited parallel-trend testing for the footfall metrics (average monthly footfall, average monthly dwell time, and total monthly dwell time), using available monthly data from early 2019. However, due to the small number of pre-intervention data points and the long interval between pre- and post-treatment, insights from parallel testing are limited. Read Annex A.8 for more information about these limitations.

The analysis does not reject the assumption of parallel trends for average footfall, and this result remains unchanged under additional sensitivity specifications. These results provide further evidence for the robustness of the DiD analysis of changes in monthly footfall. The analysis rejects the parallel trends assumption for both average and total dwell times, with sensitivity tests confirming this. This weakens the impact assessment results to some degree but does not completely invalidate them, given the limitations of parallel-trends analysis.

Table E.20 Future High Streets Fund – Impact analysis: business outcomes – Parallel trends testing

Indicator	Number of comparator towns	Number of treated towns	Parallel trends test[A]
Change in average monthly footfall, 2019 to 2025	147	38	NOT REJECTED: No significant difference found between the parallel trends for treated and comparator groups
Change in average monthly dwell time, 2019 to 2025	147	38	REJECTED: Significant difference found in the parallel trends for the treated and comparator groups
Change in total monthly dwell time, 2019 to 2025	147	38	REJECTED: Significant difference found in the parallel trends for the treated and comparator groups

Source: Frontier Economics.
Note: [A] Parallel trends are assessed on pre-treatment observations for Q2 and Q3 2019 only.

E.2.4 Future High Streets Fund business outcomes – Displacement assessment

For the business outcomes, the analysis assesses potential displacement for all indicators.

For the footfall-related indicators, displacement is particularly relevant as local improvements may change where people choose to visit, shifting trips from nearby centres into the treated area rather than increasing overall activity.

For the **total number of businesses** and the **business birth rate**, displacement is relevant, as regeneration activity may encourage firms to relocate or concentrate in treated areas, thereby redistributing activity from nearby centres rather than generating additional business activity.

For **total employment**, displacement is relevant, as employment activity could plausibly shift across neighbouring areas (for example, through firm relocation or rebalancing staff across sites).

The Future High Streets displacement assessment consists of 2 methodologies. These methodologies differ based on the indicator assessed:

1. **Displacement analysis for footfall indicators** estimates the impact that changes to the treated high street had on up to 3 co-visited neighbouring high streets. Read Annex A.6 for more information on the neighbouring areas methodology.
2. **Displacement analysis for all other business outcomes indicators** estimates impacts across a series of concentric rings around the Future High Streets Fund project locations. This approach assesses how effects vary with proximity and helps identify potential displacement or spillover effects. Read Annex A.5 for more information on the concentric-rings methodology.

DETAILED RESULTS: AVERAGE FOOTFALL, AVERAGE DWELL TIME, AND TOTAL DWELL TIME

Table E.21, Table E.22 and Table E.23 present the central model and sensitivity testing of the displacement analysis for changes in **monthly average footfall**, **monthly average dwell time**, and **monthly time spent on high streets**.

The central specification reported in Table E.21 shows a small, positive, but not statistically significant, displacement effect with neighbouring towns. Sensitivity checks produce estimates of similar magnitude, and most are statistically insignificant, indicating limited evidence of potential spillover or displacement effects.

Table E.21 Future High Streets Fund – Displacement analysis: business outcomes – Change in average monthly footfall – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Robust standard error
Central model	38	85	0.318	0.2460
Testing outlier treatment: DiD – dropping outliers	36	79	0.903***	0.2891
Testing outlier treatment: DiD – no outlier treatment	38	85	0.269	0.2390
Testing the scope of the sample: DiD – 2-month lag	47	100	0.273	0.2499

Source: *Frontier Economics.*

Note: *[A] Annex A.6 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

The central specification reported in Table E.22 shows a small, non-statistically significant impact. Sensitivity checks produce estimates of similar magnitude and are statistically insignificant, indicating no evidence of potential spillover or displacement effects.

Table E.22 Future High Streets Fund – Displacement analysis: business outcomes – Change in average monthly dwell time – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Robust standard error
Central model	38	85	0.027	0.1299
Testing outlier treatment: DiD – dropping outliers	36	80	0.218	0.3437
Testing outlier treatment: DiD – no outlier treatment	38	85	0.027	0.1296
Testing the scope of the sample: DiD – 2-month lag	47	100	-0.119	0.1341

Source: *Frontier Economics.*

Note: *[A] Annex A.6 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

The central specification presented in Table E.23 shows a positive and non-statistically significant impact. Sensitivity checks produce estimates of varying magnitudes that are statistically insignificant, indicating no evidence of potential spillover or displacement effects.

Table E.23 Future High Streets Fund – Displacement analysis: business outcomes – Change in monthly time spent – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	38	85	0.166	0.1362
Testing outlier treatment: DiD – dropping outliers	36	79	0.191	0.2331
Testing outlier treatment: DiD – no outlier treatment	38	85	0.155	0.1230
Testing the scope of the sample: DiD – 2-month lag	47	100	0.117	0.1403

Source: *Frontier Economics.*

Note: *[A] Annex A.6 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

ADDITIONAL RESULTS: TOTAL NUMBER OF BUSINESSES, BUSINESS BIRTH RATE, TOTAL EMPLOYMENT

The regression analysis finds no statistically significant displacement or spillover effects of the Future High Streets Fund on the remaining business outcomes indicators.

For the **total number of businesses**, the central analysis finds a positive but insignificant coefficient of interest. Sensitivity tests yield similar results for the magnitude and significance of the coefficient of interest.

For the **business birth rate**, the central analysis finds a small, negative coefficient of interest that is not statistically significant. Sensitivity tests similarly report statistically insignificant coefficients, which are close to 0.

Finally, for **total employment**, the central analysis and all sensitivity specifications report negative coefficients which are close to 0 and statistically insignificant.

E.3 Property and land use

Table E.24 presents the indicators used by the programme-level analysis for property and land use.

The analysis considered these indicators based on their relevance to measuring changes in property and land use outcomes and the availability of suitable data. Read Annex D for more information on the variables explored and used in this analysis.

The analysis reports these indicators descriptively for the Future High Streets Fund, as available sample sizes are insufficient to support regression analysis for impact. Displacement analysis, which relies solely on regression methods, cannot be undertaken. For the Town Deals, sample sizes are too small to support any quantitative analysis. Table E.1 and Table E.2 provide more details on the available sample sizes by fund and outcome.

Table E.24 Quantitative analysis conducted for property and land use

Item	Impact assessment – Town Deals	Impact assessment – Future High Streets Fund	Displacement assessment – Town Deals	Displacement assessment – Future High Streets Fund
Average property price (12 months), 2019/20 to 2024/25	None – Insufficient sample size	Descriptive analysis	None – Insufficient sample size	None – Not applicable
Commercial vacancy rates	Descriptive analysis	Descriptive analysis	None – Not applicable	None – Not applicable

Source: Frontier Economics.

The descriptive analysis calculates a relative change between the treatment and comparator groups. This provides a single percentage that summarises the treatment group's improvement relative to the comparator trend between the pre- and post-treatment periods.

This is calculated by:

1. Calculating the log difference between the outcomes in the pre- and post- treatment periods for each observation in the treatment and comparator groups, $\Delta \ln Y_i$.
2. Calculating the mean log difference observed across each of the treatment and comparator groups, where:
   a. $\Delta T = E[\Delta \ln(y_i) \mid treated_i = 1]$
   b. $\Delta C = E[\Delta \ln(y_i) \mid treated_i = 0]$
3. Taking the difference between the group means and converting this into a relative percentage change by applying the exponential: $e^{(\Delta T - \Delta C)} - 1$.

This relative change cannot be interpreted as a causal impact attributable to the Towns Fund, as it reflects only a difference in observed trends. Other factors, such as changes in population or local conditions that may affect treated and comparator areas differently,

may drive the observed differences in ways unrelated to the funded projects. Therefore, the descriptive analysis can only provide indicative evidence of potential change, which should be tested at a later stage using more robust impact evaluation methods.

The following sections present the results of this analysis for both Towns Deal and Future High Streets Fund, where applicable.

E.3.1 Town Deals property and land use: Impact assessment

RESULTS: COMMERCIAL VACANCY RATES

Table E.25 shows the descriptive analysis conducted on the **change in commercial vacancy rates** between the pre- and post-treatment periods, where data is available.

The analysis draws on a small treated sample of 13 towns, and the findings should be interpreted with caution. This shows that the overall relative change for the treatment group compared with the comparator group is 5%, indicating that commercial vacancy rates in treated towns grew marginally more than in comparator towns over the pre- and post-treatment periods.

Table E.25 Town Deals – Impact analysis: property and land use – Commercial vacancy rates – Descriptive statistics

Specification	Number of comparator towns	Number of treated towns	Mean log difference for comparator towns (ΔC)	Mean log difference for treated towns (ΔT)	Relative percentage change, $e^{(\Delta T - \Delta C)} - 1$
Change in Commercial vacancy rates	363	13	0.153	0.202	5%

Source: Frontier Economics.

E.3.2 Future High Streets Fund property and land use: Impact assessment

RESULTS: PROPERTY PRICES – COMMERCIAL VACANCY RATES

Table E.26 and Table E.27 show the descriptive analysis conducted on the **change in average property prices** and the **change in commercial vacancy rates** between the pre- and post-treatment periods.

As these analyses draw on small treated samples (11 and 29 towns), the findings should be interpreted with caution.

Table E.26 presents the descriptive analysis for the Future High Streets Fund impact analysis of the **change in average property prices.** This shows that the overall relative change for the treatment group compared with the comparator group is -1%. This indicates that average property prices in treated towns grew marginally less than in comparator towns over the pre- and post-treatment periods.

Table E.26 Future High Streets Fund – Impact analysis: property and land use – Average property price (12 months) – Descriptive statistics

Specification	Number of comparator towns	Number of treated towns	Mean log difference for comparator towns (ΔC)	Mean log difference for treated towns (ΔT)	Relative percentage change, $e^{(\Delta T - \Delta C)} - 1$
Change in Average property price (12 months)	155	11	0.165	0.159	-1%

Source: Frontier Economics.

Table E.27 presents the descriptive analysis for the Future High Streets Fund impact analysis of the **change in commercial vacancy rates**. It shows that the overall relative change for the treatment group compared with the comparator group is 2%. This indicates that commercial vacancy rates in treated towns grew marginally more than in comparator towns over the pre- and post-treatment periods.

Table E.27 Future High Streets Fund – Impact analysis: property and land use – Commercial vacancy rates – Descriptive statistics

Specification	Number of comparator towns	Number of treated towns	Mean log difference for comparator towns (ΔC)	Mean log difference for treated towns (ΔT)	Relative percentage change, $e^{(\Delta T - \Delta C)} - 1$
Change in Commercial vacancy rates	153	29	0.126	0.149	2%

Source: Frontier Economics.

E.4 Job creation

Table E.28 presents the indicators used by the programme-level analysis for job creation.

The analysis considered these indicators based on their relevance to measuring changes in job creation outcomes and the availability of suitable data. Read Annex D for more information on the variables explored and used in this analysis.

Regression analysis for impact is feasible for all indicators except the share of part-time jobs for the Future High Streets Fund, which has a small sample size available to support descriptive analysis only. Displacement analysis is only undertaken for total employment.

Table E.28 Quantitative analysis conducted for job creation

	Impact assessment – Town Deals	Impact assessment – Future High Streets Fund	Displacement assessment – Town Deals	Displacement assessment – Future High Streets Fund
Change in total employment, 2019 to 2025	DiD regression analysis	DiD regression analysis	Concentric circles regression analysis	Concentric circles regression analysis
Change in unemployment benefit claimant rate, Nov 2019 to Nov 2025	DiD regression analysis	DiD regression analysis	None – Not considered relevant for displacement analysis	None – Not considered relevant for displacement analysis
Change in share of part-time jobs, 2019 to 2025[51]	DiD regression analysis	Descriptive analysis	Concentric circles regression analysis	None – Not applicable

Source: Frontier Economics.

The following sections set out the technical specification and results for these indicators for both the Town Deals and Future High Streets Fund.

E.4.1 Town Deals job creation: Impact assessment

The Town Deals programme-level impact assessment uses a DiD approach with PSM. This entails the following steps:

1. **PSM** to identify a suitable comparator population. An alternate matching approach, CBPS, is conducted as a cross-check.
2. **Weighted average means comparison** of the indicator of interest, before and after the treatment period and between the treatment and matched comparator groups, to estimate the average treatment effect.

This section is structured as follows:

1. **Results:** Summarises the findings for the job creation indicators tested for the Town Deals, for which the estimated impacts are not statistically significant. These null results do not necessarily imply no impact; limited sample sizes, incomplete project delivery within the evaluation window, and short post-completion periods may reduce the ability to detect effects, particularly where impacts materialise gradually.
2. **Model diagnostics and tests:** Sets out the validation diagnostic checks underpinning the reported findings. This includes coverage checks, a review of the covariates used, and tests to assess the credibility of the counterfactual and the match quality.

[51] This indicator was considered relevant as a proxy for job intensity and job quality. If economic activity increases but the share of part-time work also rises, this may indicate that employment growth is concentrated in low-hours roles. By contrast, if economic activity is associated coincides with a falling share of part-time work, this may indicate that growth is supporting more full-time roles or higher average hours per worker.

RESULTS: TOTAL EMPLOYMENT, UNEMPLOYMENT BENEFITS, SHARE OF PART-TIME JOBS

Table E.29, Table E.30, and Table E.31 present the central model and sensitivity testing for the impact analysis of Town Deals on **total employment**, **unemployment benefits claimant rate** and the **share of part-time jobs**.

The analysis does not detect a consistent, measurable programme-level effect across the job creation indicators for the Town Deals at this stage.

This should not be interpreted as evidence that the funds had no impact. The relatively small sample sizes available for this analysis reduce statistical power and make it harder to detect statistically significant effects. Furthermore, job creation is a longer-term outcome that is more likely to materialise (and become visible in the data) a few years after project completion.

Table E.29 presents the central model and sensitivity testing results for the impact analysis of Town Deals on **total employment**. It shows that the central estimate is negative but not statistically significant. Sensitivity tests produce results consistent with the baseline.

Table E.29 Town Deals – Impact analysis: job creation – Total employment – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	87	39	-0.040	0.0319	-3.91%
Testing parameters for matching: PSM – More stringent calliper	87	37	-0.037	0.0299	-3.60%
Testing matching approach: CBPS	441	39	-0.005	0.0217	-0.47%
Testing outlier treatment: PSM – Dropping outliers	85	39	-0.021	0.0251	-2.10%
Testing outlier treatment: PSM – No outlier treatment	87	39	-0.042	0.0343	-4.08%
Testing the scope of the sample: PSM – 3-month lag	91	42	-0.054	0.0368	-5.25%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.30 presents the central model and sensitivity testing for the impact analysis of Town Deals on the **unemployment benefits claimant rate**. This shows that the central estimate is positive and not statistically significant, with most sensitivities consistent with this. As significance arises only under the CBPS specification, this should be treated as potential rather than conclusive evidence of impact.

Table E.30 Town Deals – Impact analysis: job creation – Unemployment benefits claimant rate – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	112	44	0.038	0.0335	3.90%
Testing parameters for matching: PSM – More stringent calliper	115	42	0.041	0.0340	4.20%
Testing matching approach: CBPS	441	44	0.067**	0.0302	6.94%
Testing outlier treatment: PSM – Dropping outliers	110	44	0.040	0.0334	4.06%
Testing outlier treatment: PSM – No outlier treatment	112	44	0.037	0.0337	3.80%
Testing the scope of the sample: PSM – 7-month lag	113	47	0.034	0.0336	3.50%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

Table E.31 presents the central model and sensitivity testing results for the impact analysis of Town Deals on the **share of part-time jobs**. This shows that the central estimate is negative and not statistically significant. Only the 3-month lag analysis yields significance at the 10% level. As significance arises only under this sensitivity, it is best interpreted as indicative of potential, not conclusive, evidence of impact.

Table E.31 Town Deals – Impact analysis: job creation – Share of part-time jobs – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	71	16	-0.025	0.0197	-2.45%
Testing parameters for matching: PSM – More stringent calliper	71	16	-0.029	0.0187	-2.83%
Testing matching approach: CBPS	287	16	-0.018	0.0173	-1.77%
Testing outlier treatment: PSM – Dropping outliers	69	16	-0.023	0.0192	-2.27%
Testing outlier treatment: PSM – No outlier treatment	71	16	-0.025	0.0198	-2.50%
Testing the scope of the sample: PSM – 4-month lag	82	20	-0.025	0.0174	-2.51%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

MODEL DIAGNOSTICS AND TESTS

This section presents the empirical results of the testing and diagnostics for the impact analysis of the Town Deals on job creation. These tests check the performance of the PSM stage of the analysis.

The relevant checks include the:

- coverage of treated observations available for analysis
- covariates included for the PSM specification
- diagnostic tests used to assess counterfactual credibility, including covariate selection and final balance checks
- diagnostic tests used to assess match quality, including common support checks, match statistics and off-support checks

Table E.32 summarises the results of these checks. Overall, this provides evidence that the PSM is effective at improving comparability between treated and comparator towns. However, the coverage of the final analysis is low, likely leading to low analysis power and capturing impacts that are shorter-term, based on smaller-scale projects that are more likely to have been completed and included in the analysis.

Table E.32 Town Deals – Impact analysis: job creation – Model diagnostics and tests

Test	Total employment	Unemployment benefit claimant rates	Share of part-time jobs
Coverage	2 treated towns are not matched at the PSM stage, but all remaining treated observations are retained at the ATT stage. The final sample at the ATT stage includes 39 towns, which corresponds to 46% of all Town Deals towns with at least one job creation project.	2 treated towns are not matched at the PSM stage, but all remaining treated observations are retained at the ATT stage. The final sample includes 44 towns, which corresponds to 52% of all Town Deals towns with at least one job creation project.	3 treated towns are not matched at the PSM stage, but all remaining treated observations are retained at the ATT stage. The final sample at the ATT stage includes 16 towns, which corresponds to 19% of all Town Deals towns with at least one job creation project.
Covariate selection	16 pre-treatment indicators retained from the original long list of 21. These covariates relate to economic conditions, labour market structure, deprivation, income, productivity and exposure to EU exit.	17 pre-treatment indicators retained from the original long list of 20. These covariates relate to economic conditions, labour market structure, deprivation, income, productivity and exposure to EU exit.	7 pre-treatment indicators retained from the original long list of 20. These covariates relate to indicators of economic activity and occupation groups.
Balancing checks	After Stage 1 PSM, absolute standardised mean differences for most covariates are brought within 0.5 s.d. In Stage 2, the balance of all but 3 covariates is still better than that of the unmatched control group.	After Stage 1 PSM, absolute standardised mean differences of all covariates are brought within 0.1 s.d. In Stage 2, the balance of all but 3 covariates is still better than that of the unmatched control group.	After Stage 1 PSM, absolute standardised mean differences of most covariates are brought within 0.1 s.d. In Stage 2, the balance of all but 1 covariate is still better than that of the unmatched control group.
Common support checks	Although the distributions are not perfectly aligned, they remain similar across the main area of common support.	Although the distributions are not perfectly aligned, they remain similar across the main area of common support.	Although the distributions are not perfectly aligned, they remain similar across the main area of common support.
Off-support checks	As counts are <10, reporting is not possible; to avoid possible secondary disclosure. See the UKDS Research data handling and security guide for users (2025).	As counts are <10, reporting is not possible; to avoid possible secondary disclosure. See the UKDS Research data handling and security guide for users (2025).	As counts are <10, reporting is not possible; to avoid possible secondary disclosure. See the UKDS Research data handling and security guide for users (2025).

Source: Frontier Economics.

E.4.2 Town Deals job creation: Displacement assessment

For the job creation indicators, the analysis assesses potential displacement for **total employment** and the **share of part-time jobs** only.

For **total employment** and the **share of part-time jobs**, displacement may be relevant as employment activity could plausibly shift across neighbouring areas (for example, through firm relocation or rebalancing staff across sites).

For the **unemployment benefit claimant rate**, the rationale is weaker, as these indicators reflect broader characteristics of the local workforce, rather than activity concentrated in a specific town-centre location.

The displacement assessment for the job creation indicators estimates impacts across concentric rings surrounding the Town Deals project locations. This approach assesses how effects vary with proximity and helps identify potential displacement or spillover effects. Read Annex A.5 for more information on the concentric rings methodology.

RESULTS: TOTAL EMPLOYMENT, SHARE OF PART-TIME JOBS

Table E.33 and Table E.34 present the central model and sensitivity testing for the displacement analysis of the Town Deals, focusing on changes in total employment and the change in the share of part-time jobs.

Overall, the displacement analysis finds no evidence of employment or employment type shifting between the central impact zone and the surrounding rings.

The results suggest that the programme has not measurably redistributed employment across neighbouring areas over the evaluation period. However, the analysis cannot rule out small or longer-term displacement effects that may emerge over a longer time horizon or remain undetected in the available data. In addition, displacement from more locations further than 10km from the central impact zone, which may be occurring, may not be captured by the concentric ring approach of this analysis.

Table E.33 presents the central model and sensitivity testing for the Town Deals displacement analysis of **total employment**. The central estimate is close to 0, showing no evidence of displacement. Sensitivity tests produce results consistent with this finding.

Table E.33 Town Deals – Displacement analysis: job creation – Change in total employment – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	41	286	0.006	0.0089
Testing the extent of exposure: 3km radius	38	87	-0.001	0.0128
Testing the extent of exposure: 5km radius	41	150	0.003	0.0089
Testing the scope of the sample: 3-month lag	46	326	0.011	0.0083

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.34 presents the central model and sensitivity testing for the Town Deals displacement analysis of the **share of part-time jobs**. The central estimate shows small, positive coefficients across all specifications. However, none of these estimates is statistically significant.

Table E.34 Town Deals – Displacement analysis: job creation – Change in the share of part-time jobs – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	20	119	-0.003	0.0086
Testing the extent of exposure: 3km radius	17	36	-0.004	0.0065
Testing the extent of exposure: 5km radius	20	63	-0.003	0.0073
Testing the scope of the sample: 4-month lag	26	157	0.002	0.0094

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

E.4.3 Future High Streets Fund job creation: Impact assessment

The Future High Streets Fund impact assessment is based on a DiD approach. Annex A.3 provides more details on the DiD approach.

This section is structured as follows:

1. **Results:** Summarises the findings for the job creation indicators tested for the Future High Streets Fund, for which the estimated impacts are not statistically significant. These null results do not necessarily imply no impact; limited sample sizes, incomplete project delivery within the evaluation window, and short post-completion periods may reduce the ability to detect effects, particularly where impacts materialise gradually.
2. **Model diagnostics and tests:** Sets out the validation diagnostic checks underpinning the reported findings. This includes coverage checks, review of the covariates used and tests to assess the credibility of the counterfactual and match quality.

RESULTS: TOTAL EMPLOYMENT, UNEMPLOYMENT BENEFITS, SHARE OF PART-TIME JOBS

Table E.35 and Table E.36 present the central model and sensitivity testing for the impact analysis of the Future High Streets Fund on **total employment** and **unemployment benefit claimant rate**. Table E.37 shows the descriptive analysis conducted on the change in the **share of part-time jobs** between the pre-treatment and post-treatment periods.

The regression analysis finds no evidence of a measurable programme-level effect on either total employment or the unemployment benefit claimant rate for the Future High Streets Fund. Descriptive statistics for the share of part-time jobs indicate a similar directional effect for this indicator.

Overall, these results imply that the analysis does not detect short-run changes in any of the job creation indicators attributable to the Future High Streets Fund. This suggests that any impacts are either too small to detect using the available data or that impacts may not have materialised at this stage of the analysis.

Table E.35 presents the central model and sensitivity testing for the Future High Streets Fund impact analysis of **total employment**. The central model's coefficient is positive but extremely small and not statistically significant. This estimate remains close to 0, but the direction varies under alternative outlier treatments and lag choices.

Table E.35 Future High Streets Fund – Impact analysis: job creation – Total employment – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	154	26	0.006	0.0212	0.61%
Testing outlier treatment: DiD – dropping outliers	153	25	-0.018	0.0157	-1.77%
Testing outlier treatment: DiD – no outlier treatment	154	26	0.014	0.0256	1.41%
Testing the scope of the sample: DiD – 0-month lag	154	36	-0.003	0.0162	-0.31%

Source: *Frontier Economics.*

Note: *[A] Annex A.3 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.36 presents the central model and sensitivity testing for the Future High Streets Fund impact analysis of the **unemployment benefit claimant rate**. The central model's coefficient is similarly near-0 and not statistically significant. Estimates remain close to 0 under various sensitivities, with minor sign changes and no statistical significance.

Table E.36 Future High Streets Fund – Impact analysis: job creation – Unemployment benefits claimant rate – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	154	27	0.019	0.0113	1.87%
Testing outlier treatment: DiD – dropping outliers	153	26	0.014	0.0112	1.42%
Testing outlier treatment: DiD – no outlier treatment	154	27	0.017	0.0112	1.77%
Testing the scope of the sample: DiD – 7-month lag	154	36	0.015	0.0098	1.48%

Source: *Frontier Economics.*

Note: *[A] Annex A.3 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.37 presents the descriptive analysis for the Future High Streets Fund impact analysis of the **share of part-time jobs.** The relative change observed for the treatment group relative to the comparator group is -2%, indicating that the treated group grew slightly less than the comparator group over the pre- and post- treatment periods.

Table E.37 Future High Streets Fund – Impact analysis: job creation – Share of part-time jobs – Descriptive statistics

Specification	Number of comparator towns	Number of treated towns	Mean log difference for comparator towns (ΔC)	Mean log difference for treated towns (ΔT)	Relative percentage change, $e^{(\Delta T - \Delta C)} - 1$
Change in the share of part-time jobs	155	14	-0.057	-0.074	-2%

Source: Frontier Economics.

MODEL DIAGNOSTICS AND TESTS

This section presents the empirical results of the testing and diagnostics for the impact analysis of the Future High Streets Fund on job creation.

The relevant checks include:

- The coverage of treated observations available for analysis
- The diagnostic tests used to assess counterfactual credibility, including covariate selection and parallel trends

Table E.38 summarises the results of these checks. Overall, the coverage of the final analysis is low, likely leading to low analytical power and capturing only short-term impacts, based on smaller-scale projects that are more likely to have been completed and included in the analysis.

Table E.38 Future High Streets Fund – Impact analysis: job creation – Model diagnostics and tests

Test	Total employment	Unemployment benefit claimant rates
Coverage	The final analysis is based on 26 towns. This corresponds to 28% of all Future High Streets Fund projects and 50% of all Future High Streets Fund towns with at least one job creation project.	The final analysis is based on 27 towns. This corresponds to 30% of all Future High Streets Fund projects and 52% of all Future High Streets Fund towns with at least one job creation project.
Covariate selection	9 pre-treatment indicators were retained from the original long list of 17. These covariates relate to labour market conditions and dynamics, occupational structure, deprivation, and public sector employment.	8 pre-treatment indicators were retained from the original long list of 16. These covariates relate to labour market conditions and dynamics, occupational structure, deprivation, and public sector employment.
Parallel trends	Insufficient data available to conduct this test.	No significant difference in parallel trends was found between the treated and comparator groups.

Source: Frontier Economics.

E.4.4 Future High Streets Fund job creation: Displacement assessment

For the job creation indicators, the analysis assesses potential displacement for **total employment** only.

For **total employment**, displacement may be relevant as employment activity could plausibly shift across neighbouring areas (for example, through firm relocation or rebalancing staff across sites).

For the **share of part-time jobs**, it is relevant to conduct a displacement analysis for the same reason. However, low sample sizes for the Future High Streets Fund prevent this assessment from being conducted in practice.

The rationale for the **unemployment benefit claimant rate** is weaker, as these indicators reflect broader characteristics of the local workforce rather than activity concentrated in a specific town-centre location.

The assessment for the job creation indicators estimates impacts across a series of concentric rings around the Future High Streets Fund project locations. This approach assesses how effects vary with proximity and helps identify potential displacement or spillover effects. Read Annex A.5 for more information on the concentric rings methodology.

RESULTS: TOTAL EMPLOYMENT

Table E.39 presents the central model and sensitivity testing for the displacement analysis of the Future High Streets Fund for changes in total employment. The displacement analysis for total employment finds no clear evidence of employment shifting between the central impact zone and the surrounding rings.

The central model's coefficient is positive and statistically significant at the 10% level, indicating modest co-movement between employment changes in the centre and surrounding areas. While the positive estimate increases when testing displacement up to 3km from the central impact zone, it becomes statistically insignificant and remains so for other sensitivities.

The results suggest that the programme has not measurably redistributed employment across neighbouring areas over the evaluation period. However, the analysis cannot rule out small or longer-term displacement effects that may emerge over a longer time horizon or remain undetected in the available data. In addition, displacement from locations more than 10km from the central impact zone, which may be occurring, may not be captured by the concentric ring approach used in this analysis.

Table E.39 Future High Streets Fund – Displacement analysis: job creation – Change in the total employment – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	26	204	0.015*	0.0076
Testing the extent of exposure: 3km radius	25	64	0.020	0.0122
Testing the extent of exposure: 5km radius	26	102	0.013	0.0093
Testing the scope of the sample: 0-month lag	36	280	0.001	0.0104

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

E.5 Employment mix

Table E.40 presents the indicators used in the employment mix analysis.

The analysis considered these indicators based on their relevance to measuring changes in employment mix outcomes and the availability of suitable data. Read Annex D for more information on the variables explored and used in this analysis.

Regression analysis for impact is feasible for all indicators for the Town Deals, although displacement analysis is undertaken only for total employment.

For the Future High Streets Fund, the available sample size is insufficient to support any quantitative analysis. Table E.1 and Table E.2 provide further detail on available sample sizes by fund and outcome.

Table E.40 Quantitative analysis conducted for employment mix

Item	Impact assessment – Town Deals	Impact assessment – Future High Streets Fund	Displacement assessment – Town Deals	Displacement assessment – Future High Streets Fund
Change in share of part-time jobs, 2019 to 2025	DiD regression analysis	None – Insufficient sample size	Concentric circles regression analysis	None – Insufficient sample size
Change in the share of employees in given sectors, 2019 to 2025[52, 53]	DiD regression analysis	None – Insufficient sample size	None – Not considered relevant for displacement analysis	None – Not considered relevant for displacement analysis
Change in the concentration of sector employment shares, 2019 to 2025[54]	DiD regression analysis	None – Insufficient sample size	None – Not considered relevant for displacement analysis	None – Not considered relevant for displacement analysis

Source: Frontier Economics.

The following sections present the results of the analysis for the Town Deals only.

[52] The sectors assessed are Retail, Accommodation, Food and beverage, Activities typically associated with economic decline (including the tobacco and vape stores, charity shops, takeaways and gambling stores) and Activities typically associated with economic improvement (including libraries, museums and sports centres).
[53] Businesses in each sector have been identified using Standard Industrial Classification (SIC) codes from Companies House.
Each sector in this analysis was identified as follows:
(i) **Retail** includes businesses in Division 47 SIC codes
(ii) **Accommodation** corresponds to Division 55 SIC codes
(iii) **Food and beverage** corresponds to Division 56 SIC codes
(iv) **Sectors associated with economic decline** are defined by Frontier Economics as those businesses whose presence may indicate underlying economic distress of an area. These have been identified using SIC codes 4726 – Retail sale of tobacco products in specialised stores, 47250 – Retail sale of beverages in specialised stores, 47799 – Retail sale of other second hand goods in stores, 56103 – Take away food shops and mobile food stands, 64921 – Credit granting by non deposit taking finance houses and other Specialist consumer credit grantors, 92 – Gambling and betting activities, 95 – Repair of communication equipment, 96020 – Hairdressing and other beauty treatment.
(v) **Sectors associated with economic improvement** are defined by Frontier Economics as those activities whose presence typically signals rising economic vitality. These have been identified using SIC codes 90 – Creative, arts and entertainment activities, 91 – Libraries, archives, museums and other cultural activities, and 93 – Sports activities and amusement and recreation activities.
[54] Based on the Herfindahl–Hirschman Index (HHI), a common measure of market concentration. This is calculated by taking the sum of the squared employment shares, where the shares are expressed as fractions. The result is proportional to the average employment share, weighted by employment share. As such, it can range from 0 to 1, where the former represents employment spread across several sectors, and the latter a single monopolistic employer. For the purpose of this analysis, businesses have been grouped into sectors using 2-digit SIC codes.

E.5.1 Town Deals employment mix: Impact assessment

The Town Deals programme-level impact assessment uses a DiD approach with PSM. This entails the following steps:

1. **PSM** to identify a suitable comparator population. An alternate matching approach, CBPS, is conducted as a cross-check.
2. **Weighted average means comparison** of the indicator of interest, before and after the treatment period and between the treatment and matched comparator groups, to estimate the average treatment effect.

This section is structured as follows:

1. **Results:** Summarises the findings for the employment mix indicators tested for the Town Deals, for which the estimated impacts are not statistically significant. These null results do not necessarily imply no impact; limited sample sizes, incomplete project delivery within the evaluation window, and short post-completion periods may reduce the ability to detect effects, particularly where impacts materialise gradually.
2. **Model diagnostics and tests:** Sets out the validation diagnostic checks underpinning the reported findings. This includes coverage checks, a review of the covariates used and tests to assess the credibility of the counterfactual and the match quality.

RESULTS: SHARE OF PART-TIME JOBS, EMPLOYMENT SHARES BY SECTOR, EMPLOYMENT SHARE CONCENTRATION

The analysis provides limited evidence of changes in employment composition during the evaluation period.

Table E.41 presents the central model and sensitivity testing for the impact analysis of the Town Deals on the share of part-time jobs. The central model's estimate is negative and statistically significant at the 10% level. This result remains broadly consistent across most sensitivity tests, although the estimate loses statistical significance only under the most conservative specifications (dropping outliers and applying shorter lags).

Table E.41 Town Deals – Impact analysis: employment mix – Share of part-time jobs – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	62	17	-0.027	0.0172	-2.66%
Testing parameters for matching: PSM – More stringent calliper	60	17	-0.029	0.0187	-2.83%
Testing matching approach: CBPS	287	17	-0.043**	0.0169	-4.24%
Testing outlier treatment: PSM – Dropping outliers	60	17	-0.026	0.0168	-2.54%
Testing outlier treatment: PSM – No outlier treatment	62	17	-0.027	0.0173	-2.7%
Testing the scope of the sample: PSM – 4-month lag	68	21	-0.037**	0.0162	-3.67%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.42 presents the central model and sensitivity testing for the impact analysis of the Town Deals on employment shares in the accommodation sector. It shows that all estimates are near-0 and not statistically significant in the central model. Sensitivity tests produce similarly small estimates that switch signs across specifications.

Table E.42 Town Deals – Impact analysis: employment mix – Accommodation sector employment shares – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	75	20	-0.005	0.0175	-0.51%
Testing parameters for matching: PSM – More stringent calliper	63	19	0.008	0.0141	0.78%
Testing matching approach: CBPS	438	20	-0.000	0.0157	-0.02%
Testing outlier treatment: PSM – Dropping outliers	73	20	0.001	0.0156	0.09%
Testing outlier treatment: PSM – No outlier treatment	75	20	-0.006	0.0182	-0.6%
Testing the scope of the sample: PSM – 4-month lag	85	25	-0.004	0.0140	-0.37%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

Table E.43 presents the central model and sensitivity testing for the impact analysis of the Town Deals on employment shares in the retail sector. It shows that all estimates are near-0 and not statistically significant in the central model. Sensitivity tests produce similarly small estimates that switch signs across specifications.

Table E.43 Town Deals – Impact analysis: employment mix – Retail sector employment shares – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	75	20	0.004	0.0134	0.43%
Testing parameters for matching: PSM – More stringent calliper	63	19	0.014	0.0142	1.37%
Testing matching approach: CBPS	438	20	-0.008	0.0121	-0.81%
Testing outlier treatment: PSM – Dropping outliers	73	20	0.003	0.0118	0.3%
Testing outlier treatment: PSM – No outlier treatment	75	20	0.003	0.0140	0.27%
Testing the scope of the sample: PSM – 4-month lag	85	25	0.003	0.0133	0.26%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.44 presents the central model and sensitivity testing for the impact analysis of the Town Deals on employment share in sectors associated with declining economic activity. It shows that all estimates are near-0 and not statistically significant in the central model. Sensitivity tests produce similarly small estimates that switch signs across specifications.

Table E.44 Town Deals – Impact analysis: employment mix – Food and beverage sector employment shares – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	75	20	0.005	0.0103	0.45%
Testing parameters for matching: PSM – More stringent calliper	63	19	-0.004	0.0076	-0.37%
Testing matching approach: CBPS	438	20	0.004	0.0071	0.44%
Testing outlier treatment: PSM – Dropping outliers	73	20	0.003	0.0094	0.28%
Testing outlier treatment: PSM – No outlier treatment	75	20	0.005	0.0103	0.46%
Testing the scope of the sample: PSM – 4-month lag	85	25	-0.005	0.0098	-0.55%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.45 presents the central model and sensitivity testing for the impact analysis of the Town Deals on employment shares in the food and beverage sector. It shows that all estimates are near-0 and not statistically significant in the central model. Sensitivity tests produce similarly small estimates that switch signs across specifications.

Table E.45 Town Deals – Impact analysis: employment mix – Employment shares in economic decline sectors – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	75	20	0.001	0.0041	0.05%
Testing parameters for matching: PSM – More stringent calliper	63	19	-0.003	0.0036	-0.25%
Testing matching approach: CBPS	438	20	-0.003	0.0030	-0.26%
Testing outlier treatment: PSM – Dropping outliers	73	20	0.001	0.0032	0.13%
Testing outlier treatment: PSM – No outlier treatment	75	20	-0.000	0.0046	-0.02%
Testing the scope of the sample: PSM – 4-month lag	85	25	0.001	0.0041	0.1%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.46 presents the central model and sensitivity testing for the impact analysis of the Town Deals on employment share in sectors associated with improving economic activity. It shows that all estimates are near-0 and not statistically significant in the central model. Sensitivity tests produce similarly small estimates that switch signs across specifications.

Table E.46 Town Deals – Impact analysis: employment mix – Employment shares in economic improvement sectors – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	75	20	-0.007	0.0083	-0.72%
Testing parameters for matching: PSM – More stringent calliper	63	19	-0.010	0.0087	-1.02%
Testing matching approach: CBPS	438	20	-0.005	0.0079	-0.45%
Testing outlier treatment: PSM – Dropping outliers	73	20	-0.008	0.0079	-0.85%
Testing outlier treatment: PSM – No outlier treatment	75	20	-0.006	0.0087	-0.61%
Testing the scope of the sample: PSM – 4-month lag	85	25	-0.011	0.0083	-1.11%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

Table E.47 presents the central model and sensitivity testing for the impact analysis of the Town Deals on employment sector concentration. It shows that all estimates are near-0 and not statistically significant in the central model. Sensitivity tests produce similarly small estimates that switch signs across specifications.

Table E.47 Town Deals – Impact analysis: employment mix – Employment sector concentration – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	74	19	0.005	0.0108	0.53%
Testing parameters for matching: PSM – More stringent calliper	74	20	0.012	0.0111	1.22%
Testing matching approach: CBPS	416	20	0.005	0.0097	0.55%
Testing outlier treatment: PSM – Dropping outliers	72	19	0.004	0.0106	0.43%
Testing outlier treatment: PSM – No outlier treatment	74	19	0.006	0.0109	0.58%
Testing the scope of the sample: PSM – 4-month lag	84	23	0.004	0.0100	0.43%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

Overall, the evidence indicates a modest reduction in the share of part-time jobs associated with the Town Deals. All other indicators produce near-0, non-significant estimates, which are sensitive to the specification. Taken together, this provides no clear evidence of sectoral rebalancing over the evaluation period. This is plausible, given that changes in sectoral composition typically emerge over longer time horizons and may vary substantially across places and projects.

MODEL DIAGNOSTICS AND TESTS

This section presents the empirical results of the testing and diagnostics for the impact analysis of the Town Deals on employment mix.

These tests check the performance of the PSM stage of the analysis and are common across the employment mix indicators.

The relevant checks include the:

- coverage of treated observations available for analysis
- covariates included for the PSM specification
- diagnostic tests used to assess counterfactual credibility, including covariate selection and final balance checks
- diagnostic tests used to assess match quality, including common support checks, match statistics and off-support checks

Table E.48 summarises the results of these checks. Overall, this provides evidence that the PSM is effective at improving comparability between treated and comparator towns. However, the coverage of the final analysis is low, likely leading to low analytical power and capturing only short-term impacts, based on smaller-scale projects that are more likely to have been completed and included in the analysis.

Table E.48 Town Deals – Impact analysis: employment mix – Model diagnostics and tests

Test	Share of part-time jobs	Sector concentration	Employment shares (accommodation, retail, food and beverage, sectors associated with economic decline, sectors associated with economic improvement)
Coverage	5 treated towns are not matched at the PSM stage, but all remaining treated observations are retained at the ATT stage. The final sample at the ATT stage included 17 towns, which correspond to 22% of all Town Deals towns with at least one employment mix project.	6 treated towns are not matched at the PSM stage, but all remaining treated observations are retained at the ATT stage. The final sample at the ATT stage included 20 towns, which correspond to 25% of all Town Deals towns with at least one employment mix project.	2 treated towns are not matched at the PSM stage, but all remaining treated observations are retained at the ATT stage. The final sample at the ATT stage included 20 towns, which correspond to 25% of all Town Deals towns with at least one employment mix project.
Covariate selection	24 pre-treatment indicators retained from the original long list of 38. These covariates capture local economic structure, sectoral composition, labour market participation and occupations, skills and deprivation, as well as income, productivity, business dynamism, and exposure to EU exit.	20 pre-treatment indicators retained from the original long list of 39. These covariates capture local economic structure, sectoral composition, labour market participation and occupations, skills and deprivation, as well as income, productivity, business dynamism, and exposure to EU exit.	7 pre-treatment indicators retained from the original long list of 38. These covariates relate to economic activity indicators, occupational structure and deprivation.
Balancing checks	After Stage 1 PSM, absolute standardised mean differences of most covariates are brought within 0.1 or 0.5 s.d. In Stage 2, the balance of all but 6 covariates is still better than that of the unmatched control group.	After Stage 1 PSM, absolute standardised mean differences of most covariates are brought within 0.1 s.d. In Stage 2, the balance of all but 6 covariates is still better than that of the unmatched control group.	After Stage 1 PSM, absolute standardised mean differences of most covariates are brought within 0.1 s.d. In Stage 2, the balance of all covariates is still better than that of the unmatched control group.
Common support checks	Although the distributions are not perfectly aligned, they remain similar across the main area of common support.	Although the distributions are not perfectly aligned, they remain similar across the main area of common support.	Although the distributions are not perfectly aligned, they remain similar across the main area of common support.

Test	Share of part-time jobs	Sector concentration	Employment shares (accommodation, retail, food and beverage, sectors associated with economic decline, sectors associated with economic improvement)
Off-support checks	As counts are <10, reporting is not possible: to avoid possible secondary disclosure. See the UKDS Research data handling and security guide for users (2025).	As counts are <10, reporting is not possible: to avoid possible secondary disclosure. See the UKDS Research data handling and security guide for users (2025).	As counts are <10, reporting is not possible: to avoid possible secondary disclosure. See the UKDS Research data handling and security guide for users (2025).

Source: Frontier Economics.

E.5.2 Town Deals employment mix: Displacement assessment

For the employment mix indicators, the analysis assesses potential displacement for the **share of part-time jobs** only.

For the **share of part-time jobs**, displacement may be relevant as employment activity could plausibly shift across neighbouring areas (for example, through firm relocation or rebalancing staff across sites).

For **sector concentration** and **employment sector shares**, displacement analysis is unlikely to be informative, as these measures reflect broader structural change and so any local redistribution across nearby areas may be difficult to isolate.

The assessment for the job creation indicators estimates impacts across a series of concentric rings around the Future High Streets Fund project locations. This approach assesses how the effects vary with proximity and helps identify potential displacement or spillover effects. Read Annex A.5 for more details on the concentric rings methodology.

RESULTS: SHARE OF PART-TIME JOBS

Table E.49 presents the central model and sensitivity testing for the displacement analysis of the Town Deals for changes in the **share of part-time jobs**.

The displacement analysis of the share of part-time jobs finds no clear evidence of a shift in employment type between the central impact zone and the surrounding rings.

The central model's coefficient is close to 0 but not statistically significant. This pattern is consistent across all sensitivities.

Overall, the analysis finds no measurable evidence that the programme has shifted the share of part-time jobs between neighbouring areas over the evaluation period.

Nevertheless, small or longer-term displacement effects may still arise and may not yet be detectable in the available data.

Table E.49 Town Deals – Displacement analysis: employment mix – share of part-time jobs – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	19	105	0.013	0.0107
Testing the scope of the sample: 3-month lag	25	144	0.016*	0.0089
Testing the extent of exposure: 3km radius	17	35	0.011	0.0151
Testing the extent of exposure: 5km radius	19	58	0.012	0.0086

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

E.6 Local wellbeing

Table E.50 presents the indicators used by the programme-level analysis for local wellbeing uses.

The available indicators relate to crime rates. The analysis considers the total crime rate and selected subcategories to provide a more granular view of offences most plausibly linked to town-centre activity and the local environment.

While these indicators capture one important dimension of local wellbeing, public safety, they do not provide a comprehensive measure of wellbeing, which in a broader sense also reflects wider economic, social, and environmental factors. The analysis therefore provides only a partial view of potential local wellbeing impacts. Additional indicators to capture these broader dimensions were unavailable or deemed unsuitable for inclusion in this analysis. Read Annex D for more information on the variables explored and used in this analysis.

Impact regression analysis is feasible for all crime indicators for the Town Deals, and displacement analysis is also considered relevant for this outcome. Sample sizes are insufficient to support an analysis for the Future High Streets Fund. See Table E.1 and Table E.2 for more details on the available sample sizes by fund and outcome.

Table E.50 Quantitative analysis conducted for local wellbeing

	Impact assessment – Town Deals	Impact assessment – Future High Streets Fund	Displacement assessment – Town Deals	Displacement assessment – Future High Streets Fund
Change in crime rate per 1,000 capita, 2018/19 to 2024/25[55]	DiD regression analysis	Descriptive analysis	Concentric circles regression analysis	None – Not applicable

Source: Frontier Economics.

The following sections present the results of the analysis for the Town Deals only.

E.6.1 Town Deals local wellbeing: Impact assessment

The Town Deals programme-level impact assessment uses a DiD approach with PSM. This entails the following steps:

1. **PSM** to identify a suitable comparator population. An alternate matching approach, CBPS, is conducted as a cross-check.
2. **Weighted average means comparison** of the indicator of interest, before and after the treatment period and between the treatment and matched comparator groups, to estimate the average treatment effect.

This section is structured as follows:

1. **Results:** Summarises the findings for the local wellbeing indicators tested for the Town Deals, for which the estimated impacts are not statistically significant. These null results do not necessarily imply no impact; limited sample sizes, incomplete project delivery within the evaluation window, and short post-completion periods may reduce the ability to detect effects, particularly where impacts materialise gradually.
2. **Model diagnostics and tests:** Sets out the validation diagnostic checks underpinning the reported findings. This includes coverage checks, a review of the covariates used and tests to assess the credibility of the counterfactual and the match quality.

RESULTS: CRIME RATES PER 1,000 CAPITA

Table E.51, Table E.52, Table E.53, Table E.54, Table E.55, and Table E.56 respectively present the central model and sensitivity testing for the impact analysis of the Town Deals on total crime offences, shoplifting offences, theft from the person offences, robbery offences, burglary offences, and antisocial behaviour incidents. These metrics are all estimated as rates per 1,000 capita.

Overall, the analysis does not provide robust evidence of programme-level impacts on crime outcomes over the evaluation period.

[55] The categories of crime assessed are total crime, theft from the person, robbery, burglary, shoplifting, and antisocial behaviour. These low-level crime categories have been selected as most likely to be affected by local improvements to towns or the high streets.

Table E.51 presents the central model and sensitivity testing for the impact analysis of the Town Deals on total crime offences. The central model's coefficient is positive and not statistically significant. Sensitivity tests produce very similar results, although the coefficient becomes statistically significant under some alternative specifications.

Table E.51 Town Deals – Impact analysis: local wellbeing – Total crime rate per 1,000 capita – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	72	19	0.231	0.1518	26.03%
Testing parameters for matching: PSM – More stringent calliper	70	18	0.250	0.1598	28.4%
Testing matching approach: CBPS	413	20	0.182**	0.0757	19.95%
Testing outlier treatment: PSM – Dropping outliers	71	18	0.072	0.0663	7.5%
Testing outlier treatment: PSM – No outlier treatment	72	19	0.241	0.1589	27.24%
Testing the scope of the sample: PSM – 2-month lag	92	23	0.222*	0.1300	24.84%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.52 presents the central model and sensitivity testing for the impact analysis of the Town Deals on total shoplifting offences. The central model's coefficient is positive and not statistically significant. Sensitivities broadly with the baseline, although the sign turns negative (and remains non-significant) when dropping outliers or using a shorter lag.

Table E.52 Town Deals – Impact analysis: local wellbeing – Shoplifting offences per 1,000 capita – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	70	20	0.033	0.1083	3.32%
Testing parameters for matching: PSM – More stringent calliper	68	19	0.037	0.1133	3.81%
Testing matching approach: CBPS	370	20	0.077	0.0932	8.05%
Testing outlier treatment: PSM – Dropping outliers	69	19	-0.043	0.0965	-4.24%
Testing outlier treatment: PSM – No outlier treatment	70	20	0.035	0.1089	3.56%
Testing the scope of the sample: PSM – 2-month lag	86	24	-0.081	0.1140	-7.8%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted.*
[B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

Table E.53 presents the central model and sensitivity testing for the impact analysis of the Town Deals on theft from the person offences. The central model's coefficient is negative and not statistically significant. Sensitivity tests show very similar results.

Table E.53 Town Deals – Impact analysis: local wellbeing – Theft from the person offences per 1,000 capita – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	43	12	-0.090	0.1143	-8.56%
Testing parameters for matching: PSM – More stringent calliper	41	10	-0.142	0.1320	-13.2%
Testing matching approach: CBPS	184	13	-0.135	0.1011	-12.7%
Testing outlier treatment: PSM – Dropping outliers	42	11	-0.019	0.1041	-1.87%
Testing outlier treatment: PSM – No outlier treatment	43	12	-0.092	0.1151	-8.77%
Testing the scope of the sample: PSM – 2-month lag	55	15	-0.089	0.0941	-8.50%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.54 presents the central model and sensitivity testing for the impact analysis of the Town Deals on robbery offences. The central model's coefficient is positive but not statistically significant. Most sensitivities reduce the magnitude of the effect, but the estimate becomes larger and statistically significant at the 1% level when dropping outliers.

Table E.54 Town Deals – Impact analysis: local wellbeing – Robbery offences per 1,000 capita – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	56	15	0.088	0.0848	9.19%
Testing parameters for matching: PSM – More stringent calliper	53	14	0.049	0.0895	5.00%
Testing matching approach: CBPS	263	15	0.065	0.0640	6.67%
Testing outlier treatment: PSM – Dropping outliers	55	14	0.161***	0.0583	17.5%
Testing outlier treatment: PSM – No outlier treatment	56	15	0.081	0.0896	8.40%
Testing the scope of the sample: PSM – 2-month lag	71	18	0.047	0.0731	4.84%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.55 presents the central model and sensitivity testing for the impact analysis of the Town Deals on burglary offences. The central model's coefficient is positive but not statistically significant. Sensitivity results show very similar results.

Table E.55 Town Deals – Impact analysis: local wellbeing – Burglary offences per 1,000 capita – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	66	18	0.062	0.0807	6.36%
Testing parameters for matching: PSM – More stringent calliper	66	18	0.062	0.0807	6.36%
Testing matching approach: CBPS	400	20	0.073	0.0584	7.6%
Testing outlier treatment: PSM – Dropping outliers	64	18	0.054	0.0797	5.52%
Testing outlier treatment: PSM – No outlier treatment	66	18	0.062	0.0809	6.43%
Testing the scope of the sample: PSM – 2-month lag	86	22	0.023	0.0700	2.31%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.56 presents the central model and sensitivity testing for the impact analysis of the Town Deals on antisocial behaviour incidents. The central model's coefficient is positive and not statistically significant. Sensitivity tests remain consistent with the central model and typically reduce the estimated magnitude when applying CBPS, dropping outliers, or shortening the lag.

Table E.56 Town Deals – Impact analysis: local wellbeing – Antisocial behaviour incidents per 1,000 capita – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	74	18	0.075	0.1139	7.74%
Testing parameters for matching: PSM – More stringent calliper	73	17	0.068	0.1192	7.02%
Testing matching approach: CBPS	400	20	0.030	0.1001	3.04%
Testing outlier treatment: PSM – Dropping outliers	73	17	0.015	0.1105	1.47%
Testing outlier treatment: PSM – No outlier treatment	74	18	0.080	0.1157	8.3%
Testing the scope of the sample: PSM – 2-month lag	87	22	0.093	0.1005	9.79%

Source: *Frontier Economics.*

Note: *[A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

At this stage of the analysis, there is limited evidence that the Town Deals had any meaningful impact on crime rates. This finding should be interpreted as indicative only and should be subject to further testing as more post-completion data become available.

MODEL DIAGNOSTICS AND TESTS

This section presents the empirical results of the testing and diagnostics for the impact analysis of the Town Deals on local wellbeing.

These tests check the performance of the PSM stage of the analysis, and are common across the local wellbeing indicators.

The relevant checks include the:

- coverage of treated observations available for analysis
- covariates included for the PSM specification
- diagnostic tests used to assess counterfactual credibility, including covariate selection and final balance checks
- diagnostic tests used to assess match quality, including common support checks, match statistics and off-support checks

Table E.57 summarises the results of these checks. Overall, this provides evidence that the PSM is effective, to an extent, at improving comparability between treated and comparator towns. However, the coverage of the final analysis is extremely low, likely leading to low analytical power and capturing only short-term impacts, based on smaller-scale projects that are more likely to have been completed and included in the analysis.

Table E.57 Town Deals – Impact analysis: local wellbeing – Model diagnostics and tests

Test	Total crime offences	Subcategories of crime (shoplifting offences, theft from the person offences, robbery offences, burglary offences, antisocial behaviour incidents)
Coverage	3 treated towns are not matched at the PSM stage, but all remaining treated observations are retained at the ATT stage. The final sample at the ATT stage included 19 towns, which corresponds to 24% of all Town Deals towns with at least one local wellbeing project.	0 to 3 treated towns are not matched at the PSM stage, but all remaining treated observations are retained at the ATT stage. The final sample at the ATT stage included 12 to 20 towns, which corresponds to 16% to 25% of all Town Deals towns with at least one local wellbeing project.
Covariate selection	Not applicable – see Annex A.1, Footnote 17 for details.	Not applicable – see Annex A.1, Footnote 17 for details.
Balancing checks	After Stage 1 PSM, absolute standardised mean differences of all covariates are brought within 0.1 s.d. In Stage 2, the balance of all but for covariates is still better than that of the unmatched control group.	After Stage 1 PSM, absolute standardised mean differences of all covariates are brought within 0.1 s.d. In Stage 2, the balance of all but 2 to 4 covariates is still better than that of the unmatched control group.
Common support checks	Although the distributions are not perfectly aligned, they remain similar across the main area of common support.	Although the distributions are not perfectly aligned, they remain similar across the main area of common support.
Off-support checks	As counts are <10, reporting is not possible: to avoid possible secondary disclosure. See the UKDS Research data handling and security guide for users (2025).	As counts are <10, reporting is not possible: to avoid possible secondary disclosure. See the UKDS Research data handling and security guide for users (2025).

Source: Frontier Economics.

E.6.2 Town Deals local wellbeing: Displacement assessment

The analysis assesses potential displacement for **all crime indicators** for local wellbeing. These indicators are total crime offences and the subcategories: theft from the person offences, robbery offences, burglary offences, and antisocial behaviour incidents.

This is because improvements in the treated areas may change behaviours and shift the opportunities for certain offences across nearby areas (for example, between neighbouring town centres and public spaces).

The displacement assessment for the local wellbeing indicators estimates impacts across concentric rings surrounding the Town Deals project locations. This approach assesses how effects vary with proximity and helps identify potential displacement or spillover effects. Read Annex A for more information on the concentric-rings methodology.

Given that any impacts on crime are expected to be highly localised, the displacement analysis focuses on 3km and 5km concentric rings around the project locations (rather than the 10km rings used for all other analyses).

RESULTS: CRIME RATES PER 1,000 CAPITA

Table E.58, Table E.59, Table E.60, Table E.61, Table E.62, and Table E.63 present the central model and sensitivity testing for the displacement analysis of the Town Deals on total crime offences, shoplifting offences, theft from the person offences, robbery offences, burglary offences, and antisocial behaviour incidents. These metrics are all estimated as rates per 1,000 capita.

The displacement regressions indicate strong co-movement in crime outcomes between the central impact zones and the surrounding rings.

Table E.58 presents the central model and sensitivity testing for the displacement analysis of the Town Deals on total crime offences. The central model's coefficient is positive and statistically significant at the 1% level. The sensitivity results are closely aligned with the central model.

Table E.58 Town Deals – Displacement analysis: local wellbeing – Total crime rate per 1,000 capita – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	21	63	0.062***	0.0159
Testing the extent of exposure: 5km radius	22	106	0.051***	0.0066
Testing the scope of the sample: 2-month lag	25	75	0.075***	0.0093

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.59 presents the central model and sensitivity testing for the displacement analysis of the Town Deals on shoplifting offences. The central model's coefficients are positive, close to 0, and significant at the 10% and 5% level, respectively. Sensitivity results remain similar in magnitude, although statistical significance varies with lag definitions and distance specification.

Table E.59 Town Deals – Displacement analysis: local wellbeing – Shoplifting offences per 1,000 capita – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	21	54	0.016*	0.0086
Testing the extent of exposure: 5km radius	21	80	0.015**	0.0063
Testing the scope of the sample: 2-month lag	25	64	0.028*	0.0141

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.60 presents the central model and sensitivity testing for the displacement analysis of the Town Deals on theft from the person offences. The central model's coefficients are positive, close to 0, and significant at the 10% and 5% level, respectively. Sensitivity results remain similar in magnitude, although statistical significance varies with lag definitions and distance specification.

Table E.60 Town Deals – Displacement analysis: local wellbeing – Theft from the person offences per 1,000 capita – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	17	36	0.022**	0.0086
Testing the extent of exposure: 5km radius	17	46	0.015*	0.0078
Testing the scope of the sample: 2-month lag	20	41	0.026***	0.0074

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.61 presents the central model and sensitivity testing for the displacement analysis of the Town Deals on robbery offences. The central model coefficients are positive and statistically significant at the 1% level. Sensitivity tests confirm these patterns, with robbery in particular remaining significant at the 1% level.

Table E.61 Town Deals – Displacement analysis: local wellbeing – Robbery offences per 1,000 capita – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	19	49	0.051***	0.0060
Testing the extent of exposure: 5km radius	19	64	0.049***	0.0059
Testing the scope of the sample: 2-month lag	23	57	0.042***	0.0098

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.62 presents the central model and sensitivity testing for the displacement analysis of the Town Deals on burglary offences. The central model coefficients are positive and statistically significant at the 1% level. Sensitivity tests confirm these patterns, with robbery in particular remaining significant at the 1% level.

Table E.62 Town Deals – Displacement analysis: local wellbeing – Burglary offences per 1,000 capita – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	21	63	0.044***	0.0100
Testing the extent of exposure: 5km radius	21	105	0.031***	0.0067
Testing the scope of the sample: 2-month lag	25	75	0.054***	0.0086

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

Table E.63 presents the central model and sensitivity testing for the displacement analysis of the Town Deals on antisocial behaviour incidents. The central model coefficients are positive and statistically significant at the 1% level. Sensitivity tests confirm these patterns, with robbery in particular remaining significant at the 1% level.

Table E.63 Town Deals – Displacement analysis: local wellbeing – Antisocial behaviour incidents per 1,000 capita – Central model and sensitivity tests

Specification	Number of treated towns	Number of displacement zones	Coefficient of interest	Clustered standard error
Central model	21	63	0.080***	0.0181
Testing the extent of exposure: 5km radius	21	105	0.081***	0.0160
Testing the extent of exposure: 3km radius	25	75	0.084***	0.0090

Source: *Frontier Economics.*

Note: *[A] Annex A.5 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.*

These positive coefficients suggest that changes in crime in the central impact zones tend to move in tandem with changes in surrounding areas, rather than indicating clear displacement from the centre into neighbouring rings (or vice versa). This suggests that if any local wellbeing impact is attributed to the Town Deals intervention, these effects would likely extend further than the centre.

E.6.3 Future High Streets Fund local wellbeing: Impact assessment

The descriptive analysis calculates a relative change percentage between the treatment and comparator groups, which provides a single percentage that summarises the treatment group's improvement relative to the comparator trend.

This is calculated by:

1. Calculating the log difference between the outcomes observed in the pre- and post-treatment periods for each observation in the treatment and comparator groups, $\Delta \ln Y_i$.
2. Calculating the mean log difference observed across each of the treatment and comparator groups, where:
   a. $\Delta T = E[\Delta \ln(y_i) \mid treated_i = 1]$
   b. $\Delta C = E[\Delta \ln(y_i) \mid treated_i = 0]$
3. Taking the difference between the group means and converting this into a relative percentage change by applying the exponential: $e^{(\Delta T - \Delta C)} - 1$.

This uplift cannot be interpreted as a causal impact attributable to the Towns Fund, as it reflects only a difference in observed trends. As such, at best, it may provide only indicative evidence of potential impact, which should be tested at a later stage using more robust impact evaluation methods.

RESULTS: CRIME RATES PER 1,000 CAPITA

Table E.64 shows the descriptive analysis conducted on the change in crime rates per 1,000 capita between the pre-treatment and post-treatment periods.

The descriptive results for the Future High Streets Fund show broadly similar movements in most crime indicators between treated and comparator towns, with a small relative change of 1% observed for total crime.

Relative changes for shoplifting offences and antisocial behaviours are similarly low, while burglary reports a fall in offences, with trends in the treated towns declining by slightly less than in comparator towns.

Two indicators diverge more strongly. Theft from the person falls in comparator towns but rises in treated towns, producing a large relative change of 74%. Robbery rises in both groups, giving a relative change of 23%. This pattern may be consistent with increased investment and economic activity in town centres, providing increased opportunities for crime, even when overall conditions improve.

However, these figures are descriptive statistics and do not provide causal evidence of programme impact. As such, no firm conclusions may be drawn. These observed differences may reflect wider factors (such as changes in reporting, enforcement, local shocks, or underlying trends) rather than effects attributable to the intervention.

Furthermore, as this descriptive analysis draws on a relatively small treated sample (15 to 16 towns, depending on the indicator), a small number of observations may disproportionately influence the results. Findings should therefore be interpreted with caution.

Table E.64 Future High Streets Fund – Impact analysis: local wellbeing – Crime rates per 1,000 capita – Descriptive statistics

Specification	Number of comparator towns	Number of treated towns	Mean log difference for comparator towns (ΔC)	Mean log difference for treated towns (ΔT)	Relative percentage change, $e^{(\Delta T - \Delta C)} - 1$
Change in Total crime rate per 1,000 capita	154	16	-0.176	-0.136	4%
Change in Shoplifting offences per 1,000 capita	152	16	0.374	0.403	3%
Change in Theft from the person offences per 1,000 capita	146	15	-0.456	0.096	74%
Change in Robbery offences per 1,000 capita	147	15	-0.003	0.204	23%
Change in Burglary offences per 1,000 capita	153	16	-0.558	-0.495	6%
Change in Antisocial behaviour incidents per 1,000 capita	153	16	-0.452	-0.439	1%

Source: Frontier Economics.

E.7 Digital connectivity

Table E.65 presents the indicators used by the programme-level analysis for digital connectivity.

The analysis considered this indicator based on its relevance to measuring changes in digital connectivity outcomes and the availability of suitable data. Read Annex D for more information on the variables explored and used in this analysis.

The analysis reports these indicators descriptively for the Town Deals, as the available sample sizes are insufficient to support impact regression analysis. For the Future High Streets Fund, sample sizes are too small to support any quantitative analysis. Displacement analysis was not considered relevant for this indicator. Table E.1 and Table E.2 provide more details on available sample sizes by fund and outcome.

Table E.65 Quantitative analysis conducted for digital connectivity

Item	Impact assessment – Town Deals	Impact assessment – Future High Streets Fund	Displacement assessment – Town Deals	Displacement assessment – Future High Streets Fund
Percentage of residential premises with access to superfast broadband,[56] Sept 2019 to Jan 2025	Descriptive analysis	None – Insufficient sample size	None – Not considered relevant for displacement analysis	None – Not considered relevant for displacement analysis

Source: Frontier Economics.

The following sections present the results of this analysis for the Town Deals only.

E.7.1 Town Deals digital connectivity: Impact assessment

The descriptive analysis calculates a relative change percentage between the treatment and comparator groups, which provides a single percentage that summarises the treatment group's improvement relative to the comparator trend between the pre- and post- treatment periods.

This is calculated by:

1. Calculating the log difference between the outcomes observed in the pre- and post-treatment periods for each observation in the treatment and comparator groups, $\Delta \ln Y_i$.
2. Calculating the mean log difference observed across each of the treatment and comparator groups, where:
   a. $\Delta T = E[\Delta \ln(y_i) \mid treated_i = 1]$
   b. $\Delta C = E[\Delta \ln(y_i) \mid treated_i = 0]$
3. Taking the difference between the group means and converting this into a relative percentage change by applying the exponential: $e^{(\Delta T - \Delta C)} - 1$.

This uplift cannot be interpreted as a causal impact attributable to the Towns Fund, as it reflects only a difference in observed trends. As such, at best it may provide only indicative evidence of potential impact, which should be tested at a later stage using more robust impact evaluation methods.

RESULTS: ACCESS TO SUPERFAST BROADBAND

Table E.66 shows the descriptive analysis conducted on the change in the percentage of homes with access to superfast broadband between the pre-treatment and post-treatment periods.

Overall, the uplift in the percentage of homes with access to superfast broadband for the treatment group relative to the comparator group is 16%, indicating that the treated group grew more than the comparator group over the pre- and post-treatment periods.

[56] Superfast broadband refers to broadband speeds of 30Mbit/s or greater.

As this analysis draws on a small treated sample (18 towns), the findings should be interpreted with caution.

Table E.66 Town Deals – Impact assessment: digital connectivity – Access to superfast broadband – Descriptive statistics

Specification	Number of comparator towns	Number of treated towns	Mean log difference for comparator towns (ΔC)	Mean log difference for treated towns (ΔT)	Relative percentage change, $e^{(\Delta T - \Delta C)} - 1$
Change in the percentage of residential premises with access to superfast broadband	452	18	0.819	0.967	16%

Source: Frontier Economics.

E.8 Local authority capabilities

Table E.67 presents the indicators used by programme-level analysis for the Town Deals for local authority capabilities.

The available indicator relates to the proportion of major planning application decisions made in time. In principle, stronger capabilities, such as clearer processes and better programme management, could improve a local authority's ability to process and determine applications within statutory timeframes.

However, at best, this indicator may only provide an indirect proxy for local authority capability. It captures one operational aspect of local authority capability, in terms of planning performance, but does not provide a comprehensive measure of wider capability, which is inherently difficult to quantify in a single metric. The analysis, therefore, provides a partial view and does not capture broader dimensions such as skills capacity, governance, programme management capability, or financial resilience. Additional indicators to capture these broader dimensions were unavailable or deemed unsuitable for inclusion in this analysis. Read Annex D for more information on the variables explored and used in this analysis.

An analysis of local authority capabilities is only relevant for the Town Deals (read Annex A.1 for more information). Impact regression analysis was feasible, although displacement analysis was deemed irrelevant for this outcome. The rationale for this approach is presented in the subsections that follow in this Annex.

Table E.67 Quantitative analysis conducted for local authority capabilities

Item	Impact assessment – Town Deals	Impact assessment – Future High Streets Fund	Displacement assessment – Town Deals	Displacement assessment – Future High Streets Fund
Change in proportion of major planning applications decided in time, Q2/Q3 2019 to Q2/Q3 2025 (Town Deals only)	DiD regression analysis	None – Not applicable to the Future High Streets Fund analysis	None – Not considered relevant for displacement analysis	None – Not applicable to the Future High Streets Fund analysis

Source: Frontier Economics.

The following sections present the results of the analysis for the Town Deals.

E.8.1 Town Deals local authority capabilities: Impact assessment

The Town Deals programme-level impact assessment uses a DiD approach with PSM. This entails the following steps:

1. **PSM** to identify a suitable comparator population. An alternate matching approach, CBPS, is conducted as a cross-check.
2. **Weighted average means comparison** of the indicator of interest, before and after the treatment period and between the treatment and matched comparator groups, to estimate the average treatment effect.

This section is structured as follows:

1. **Results:** Summarises the findings for the local authority capabilities indicators tested for the Town Deals, for which the estimated impacts are not statistically significant. These null results do not necessarily imply no impact; limited sample sizes, incomplete project delivery within the evaluation window, and short post-completion periods may reduce the ability to detect effects, particularly where impacts materialise gradually.
2. **Model diagnostics and tests:** Sets out the validation diagnostic checks underpinning the reported findings. This includes coverage checks, a review of the covariates used and tests to assess the credibility of the counterfactual and the match quality.

RESULTS: PLANNING APPLICATION DECISIONS

Table E.68 presents the central model and sensitivity testing for the impact analysis of the Town Deals on the **proportion of major planning applications decided in time**.

The regression analysis finds no statistically significant impact on the proportion of major planning applications decided in time.

The central model's estimate is positive, close to 0, and not statistically significant. Sensitivity tests with varying lag assumptions and outlier treatments produce results consistent with the central model. The sensitivity in which outliers are dropped yields a positive coefficient, but it remains not significant.

Overall, most results are not indicative of potential improvements in planning decisions made in time. Impacts may materialise over a longer time horizon.

Table E.68 Town Deals – Impact analysis: local authority capabilities – Proportion of major planning applications decided in time – Central model and sensitivity tests

Specification	Number of comparator towns	Number of treated towns	Coefficient of interest	Robust standard error	Estimated treatment effect
Central model	69	29	0.001	0.1507	0.12%
Testing parameters for matching: PSM – More stringent calliper	69	29	0.001	0.1507	0.12%
Testing outlier treatment: PSM – Dropping outliers	67	29	-0.036	0.1449	-3.51%
Testing outlier treatment: PSM – No outlier treatment	69	29	0.004	0.1517	0.44%
Testing the scope of the sample: PSM – 3-month lag	69	33	0.030	0.1445	3.07%

Source: Frontier Economics.

Note: [A] Annex A.2 details the specifications for the central model, and Annex A.8 sets out the sensitivity testing conducted. [B] p-value significance is denoted as follows: (*) significant at the 10% level, (**) significant at the 5% level, (***) significant at the 1% level.

MODEL DIAGNOSTICS AND TESTS

This section presents the empirical results of the testing and diagnostics for the impact analysis of the Town Deals on local authority capabilities.

These tests check the performance of the PSM stage of the analysis.

The relevant checks include:

- The coverage of treated observations available for analysis
- The covariates included for the PSM specification
- The diagnostic tests used to assess counterfactual credibility, including covariate selection and final balance checks
- The diagnostic tests used to assess match quality, including common support checks, match statistics and off-support checks

Table E.69 summarises the results of these checks. Overall, this provides evidence that the PSM is effective, to some extent, in improving comparability between treated and comparator towns. However, the coverage of the final analysis is limited, likely leading to low analytical power and capturing only short-term impacts, based on smaller-scale projects that are more likely to have been completed and included in the analysis.

Table E.69 Town Deals – Impact analysis: local authority capabilities – Model diagnostics and tests

Test	Proportion of major planning applications decided in time
Coverage	All 42 treated local authorities matched at the PSM stage. The final sample at the ATT stage included 29 local authorities, representing 69% of all Town Deals projects.
Covariate selection	Not applicable – see Annex A.1, Footnote 17 for details.
Balancing checks	After matching, absolute standardised mean differences for most covariates are brought within conventional balance thresholds. The absolute standardised mean difference for the 2 local authority categories indicator is slightly above the balance threshold.
Common support checks	Although the distributions are not perfectly aligned, they remain similar across the main area of common support.
Off-support checks	Not applicable - all treated local authorities are matched at the PSM stage.

Source: Frontier Economics.

E.8.2 Town Deals local authority capabilities: Displacement assessment

The analysis does not assess potential displacement for the local authority capabilities indicator. This is because improvements in local authority capabilities primarily reflect increases in underlying capacity or service quality within the authority, and are unlikely to affect the capabilities of neighbouring authorities. Displacement effects are therefore not expected to be material for this outcome.

Annex F Quality assurance

All the work undertaken by Frontier Economics has been underpinned by a rigorous, best-practice approach to quality management and assurance, with clear lines of responsibility across the team for its delivery.

This annex outlines the key principles of quality assurance (QA) that have guided this evaluation and summarises, at a high level, the different types of QA undertaken throughout the work.

F.1 Principles and approach to QA

Quality assurance has been a core part of the Towns Fund evaluation, largely due to the scale and complexity of the analysis.

The final evaluation has brought together evidence collected over 3 years and combines different forms of evidence, each with its own sources of uncertainty. The programme-level analysis has involved extensive data preparation, a layered analytical design and a range of econometric methods to provide an overarching assessment of the programme's overall impact. The intervention-level impact evaluation has required careful case study design, evidence collection, synthesis, and interpretation to bring out insights on the mechanisms by which projects created impacts and how local communities experienced interventions.

Together, these workstreams have required consistent, structured QA to ensure that the analytical approach is appropriate and that the conclusions drawn from the evaluation are credible and proportionate.

F.2 Implementation of QA for this evaluation

The QA approach for this evaluation was designed in line with the principles set out in the government's AQuA Book.

F.2.1 Embedded throughout the analytical cycle

QA was built into each phase of the work, from the evaluation design at the feasibility stage to implementation, interpretation and reporting. These checks have helped to ensure that issues identified at one stage could be addressed and, where needed, fed back into earlier methodological choices.

F.2.2 Proportionate and tailored

The level of QA applied has reflected both the importance of the analysis to the overall findings and the degree of uncertainty in the underlying data. Greater scrutiny was focused on the components of the analysis with the greatest potential to affect the robustness of the findings or the credibility of the conclusions. As such, detailed QA has been applied to key methodological choices, including project categorisation, the definition of high street and town centre boundaries, the econometric approach used in the programme-level analysis, and the selection of case studies and survey tools for the intervention-level evaluation. The team also undertook more extensive sensitivity testing where findings

were more exposed to assumptions, modelling choices or sample composition. Read Annex A for a full list of the tests conducted.

F.2.3 Independent and objective

Review and challenge have been provided by independent experts at a range of levels, within Frontier Economics, by MHCLG, and externally. This has included oversight and QA by Frontier staff outside the core delivery team, challenge from Steering Group members and technical advisors, and targeted peer review by academic experts on specific methodological components such as displacement, econometric design and variable selection methods. Drawing on this breadth of expertise has helped ensure that the analysis was subject to diverse perspectives and that key assumptions, methods, and interpretations were tested objectively throughout the evaluation.

F.2.4 Transparent and repeatable

A core part of the approach has been to work transparently with all stakeholders involved in the Towns Fund evaluation. All quality assurance was supported by a clear decision-making trail through Steering Group and core team methodology discussions, with slides and key discussion points recorded in each case. The evaluation team also maintained clear version control and assumption logs for the technical analysis. This has ensured that changes could be traced over time and that the basis for key judgements is transparent and, where required, repeatable.

F.2.5 Robust to uncertainty

This work has been subject to many sources of uncertainty, including limited data availability, missing information, measurement issues, modelling assumptions, attribution challenges, and the sensitivity of results to methodological choices. These sources of uncertainty have been tested through sensitivity analysis and documented in the technical annexes to this report. This has included testing alternative benchmarks for project categorisation, cross-checking geographic boundary definitions against alternative sources, and conducting extended sensitivity analysis of both the econometric and survey-based analyses. Where findings were subject to particular limitations or uncertainty, the report has made this clear and evidence has been interpreted accordingly.

F.2.6 Validation, verification and communication

This work has addressed the key components of QA: validation, verification, and communication. Validation checks ensured that the methods, assumptions and outputs were appropriate to the evaluation questions and intended use, while verification checks ensured that the analysis was implemented correctly through technical review of code, calculations, and data processing. Communication also played an important role in the QA process, helping to ensure that limitations and caveats were clearly explained and that findings were interpreted appropriately.