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Does tackling poverty related barriers to education improve school outcomes? Evidence from the North East of England.

Morgan Beeson[a,*], Josephine M. Wildman[b], John Wildman[a]

[a] Newcastle University Business School, Newcastle University, Frederick Douglass Centre, Helix Science Square, Newcastle upon Tyne NE4 5TG, United Kingdom
[b] Scottish Centre for Social Research (ScotCen), Scotiabank House, 2nd floor, 6 South Charlotte Street, Edinburgh EH2 4AW, United Kingdom

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ABSTRACT

Poverty related barriers to education perpetuate inequalities in educational attainment which lead to inequalities in income, health, and happiness in later life. While schools cannot tackle poverty directly, they can implement policies that tackle the stigma of poverty and ensure that the school day is more equitable. This study estimates the effect of a programme delivered to schools in North East England that is designed to remove barriers to education by reducing the stigma of poverty which impacts pupils' educational attainment and school absences. Since the roll-out of the programme was staggered, we apply the Callaway–Sant'Anna time-varying-treatment difference-in-differences approach. The results show that tackling the obstacles to learning that arise from children being in poverty can improve their educational attainment and the attainment for all children, particularly in math and English reading.

1. Introduction

Attending school can be expensive, creating barriers to education for pupils from lower income families. The stigma associated with poverty can lead pupils to disengage with school and classroom activities and with initiatives aimed at helping lower income pupils, such as Free School Meals (FSM) (Yang et al., 2022). Since "Educational inequalities are both a cause and a consequence of the wider gaps we see in society – whether in income, health or happiness" (Farquharson et al., 2022, pg. 102), tackling poverty is crucial. While schools cannot tackle poverty directly, they can remove inequitable school policies, and implement policies that ensure that the school day is more equitable (Harms and Garrett-Ruffin, 2023). We investigate whether making the school day more equitable improves school outcomes.

Identifying and removing the barriers to accessing schooling faced by children living in poverty may make schools more inclusive and improve behaviour and learning outcomes. We investigate Poverty Proofing©, a programme delivered by Children North East in England. The programme consists of a school audit engaging directly with students, staff, parents, and governors to draw up a set of school- specific recommendations to remove barriers to learning.[1] While qualitative evidence of a pilot suggests that the programme reduces the stigma of poverty (Mazzoli Smith and Todd, 2016), there has been no quantitative evaluation.

We investigate the effect of the roll-out of the programme between the school years 2016/17 and 2018/19 on a range of outcomes in schools in North East England, one of England's most deprived areas. Using a staggered differences-in-differences (DiD) approach, we find that undertaking poverty proofing significantly improves standardised test scores in math and English reading. Children receiving FSM and children not receiving FSM both benefit from the programme. Our results suggest that improved attainment is not caused by improved attendance.

2. Data and empirical strategy

2.1. Data

We use annual, school level data from the Compare School Performance service (DfE, 2020) covering the 2015/16 to 2018/19 school years.[2] We focus on school level outcomes because information on which pupils live in poverty is not available, and it is schools that are treated rather than pupils. We obtain school information, age 11

* Corresponding author.
E-mail address: morgan.beeson@ncl.ac.uk (M. Beeson).

[1] See Mazzoli Smith & Todd (2016) for a detailed description of the intervention.

[2] While data is available for years prior and after these dates, the measurement methodology of key dependent variables was altered after 2014/15 and data after 2018/19 is affected by the COVID-19 pandemic.

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standardised test results (Key Stage (KS) 2 exams), and pupil absence. School level deprivation is recorded as the local census area's (lower layer super output area (LSOA)) index of multiple deprivation, 2015 (IMD) decile. A description of the variables used is provided in the online appendix, Table A1.

We create a balanced panel of schools that were open throughout the period, located in North East England and with complete data. Treatment timings are reported in the online appendix, Table A2. The sample of primary schools in North East England is 330 of which 9 were treated in 2016/17, 20 in 2017/18, and 19 in 2018/19. A school is considered as treated in the academic year the intervention occurs. Treatment is considered as an absorbing state. We consider schools to be treated when they have undergone the intervention audit, rather than when they have implemented the audit's recommendations. This is because the recommendations are school-specific and non-binding.

In our main analysis, we estimate the effect on the average scores for pupils at KS2 in [1] Math, [2] English Reading (hereafter Reading), and [3] English Grammar, Punctuation, and Spelling (GPS); the average progress for pupils between KS1 and KS2 in [4] Math, [5] Reading, and [6] Writing; [7] the overall proportion of sessions missed by pupils; and, [8] the proportion of pupils missing 10 % or more possible sessions – a measure of persistent absence. In a secondary analysis, we estimate the effect on the test scores and progress of pupils on FSM as opposed to all pupils.

2.2. Empirical strategy

We estimate a dynamic staggered DiD model to account for the differential timing of treatment, where treatment is an absorbing state and not yet treated units are the controls. We estimate:

$$y_{its} = a_s + b_t + \sum_{p=-3}^{-2} \beta_p D_{sp} + \sum_{p=0}^{2} \gamma_p D_{sp} + x_{its} + e_{its},$$

where y is the outcome variable. Subscripts i, t, and s denote the school, year, and treatment group respectively. Parameters a and b capture school and time (school-year) fixed effects. The subscript p denotes the period relative to the year of treatment. We estimate 3 leads and 2 post-treatment effects (lags) around the treatment period, $p = 0$.[3] D is a dummy variable taking a value of 1 if the school has been treated in or before period p, and x is a vector of controls. The average treatment effect on the treated (ATT) is calculated from aggregating the β_p and γ_p estimates according to Callaway & Sant'Anna (2021) as an event study.[4]

For many schools, treatment is determined exogenously. For example, some schools are part of wider groups of schools where the decision to engage with the intervention is taken centrally. For other schools, the senior leadership team selects into treatment, leading to potential selection bias.[5] Summary statistics by treatment status are reported in Table 1 and in the online appendix Table A3. Schools receiving the intervention generally perform worse academically, are larger, are more likely to be Academy or Foundation schools, and are less likely to be faith schools.[6] Intervention schools also have more pupils on FSM and more pupils with English not as their first language. To mitigate possible selection bias we use the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on stabilized inverse probability weighting and ordinary least squares. This estimator allows for matching based on observable characteristics.[7] Additional bias may arise if schools share knowledge of the successful recommendations to untreated schools, which would result in the underestimation of the treatment effect. Potential spill-overs are limited as recommendations vary by school.

Table 1
Summary statistics by treatment status. Means with standard deviations in parentheses.

Variable	Not Treated	Treated
KS2 Score – Math	104.238 (2.406)	103.395 (2.704)
KS2 Score – Reading	103.783 (2.850)	102.632 (2.965)
KS2 Score – GPS	105.424 (2.767)	104.724 (2.990)
Progress – Math	0.860 (2.329)	0.542 (2.172)
Progress – Reading	0.515 (2.455)	−0.059 (2.322)
Progress – Writing	1.044 (2.075)	1.114 (2.261)
Overall Absence	4.358 (0.756)	4.464 (0.747)
Persistent Absence	9.482 (4.029)	10.221 (4.014)
Schools (School-years)	292 (1168)	38 (152)

3. Results

Fig. 1 plots the estimated ATTs aggregated as an event study for each outcome measure. Period 0 is the instantaneous effect of the intervention. Pre-treatment differences are shown in blue and post-treatment differences are shown in red. The pre-treatment differences are insignificantly different from zero after matching and including control variables suggesting that pre-trends violations are not a concern. There are statistically significant improvements in Math and Reading test scores, but not for GPS. The effect is not immediate but lagged. Test scores are normalised so that the national average is roughly 100. Therefore, the effect sizes of approximately 5 in the second year after treatment suggest that the intervention improved scores by approximately 5% over two years. There are statistically significant improvements in Math and Reading progress scores, but not in Writing. The effect on Math is lagged, whilst the effect on Reading progress is significant from the year of treatment. There are no effects on either measure of absence.

The online appendix, Figure A3 presents the results for the educational outcomes for pupils on FSM. The effect sizes are similar to the main results. This suggests that treatment, on average, affects both pupils on FSM and not on FSM to a similar degree.

4. Discussion and conclusion

The results demonstrate that removing poverty related barriers to education can improve school outcomes in terms of average grades and progress. Our analyses of pupils on FSM and not on FSM suggest the whole school benefits from tackling poverty, and not only those children who are potentially most affected by poverty. Such benefits may stem from a reduction in disruption that arises from greater engagement from all pupils. One mechanism that we can investigate is absenteeism, but we find no evidence that this drives the results.

Even with our use of matching techniques, the results could be affected by sample selection problems. That is, school leaders who are most concerned about poverty related learning barriers may be more likely to engage with the treatment. If sample selection is present, our

[3] The number of leads and lags is determined by the data available.

[4] As a sensitivity analysis, we re-run the analysis using a leads and lags specification and the Chaisemartin and D'Haultfoeuille (2020) estimators, with the same controls as the main estimates. The results, presented in the online appendix, Figures A1 and A2, are similar to the main results.

[5] Our data provide no information on the treatment allocation mechanism, so it is not possible to enumerate those that self-selected in and those for whom it was determined exogenously.

[6] Academy and foundation schools have no/less oversight from their Local Authority (LA) compared to community schools. See the online appendix, Table A1 for a full explanation of school types.

[7] The matching control variables are school size, OFSTED rating, IMD decile, religious status, school type, student-teacher ratio, and the proportion of students that: don't have English as a first language, have SEN support, have a SEN statement, and are female. OFSTED rating, IMD decile, religious status are time invariant.

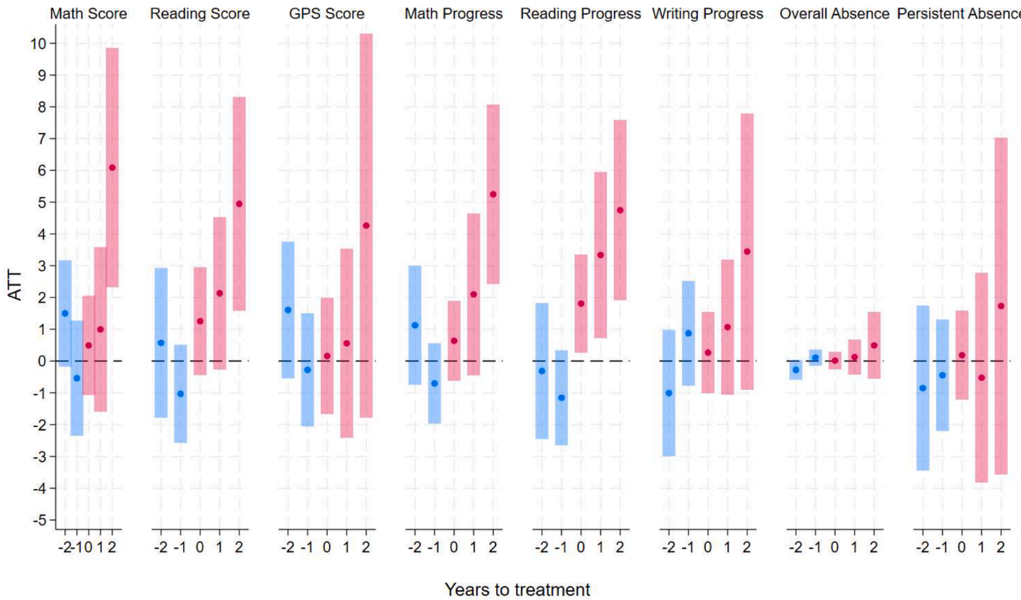

Fig. 1. The effect of the intervention on educational outcomes of pupils (KS2 Scores and Progress) and absences presented as an event study.
The estimation procedure follows Callaway and Sant'Anna (2021) using the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on stabilized inverse probability weighting and ordinary least squares. ATTs are aggregated as an event study with 2 leads and 3 lags. Circles are the point estimates and the vertical bars are the 95 % confidence intervals. Blue points and bars are the lead school years and the red points and bars are the lag school years.

results show that when school leaders are interested in tackling poverty-based obstacles to learning, they can improve school outcomes across all children. These results demonstrate the importance of tacking poverty-based obstacles to learning.

There are several potential limitations to our results. The analysis is restricted to schools in North East England and may not be generalisable. North East England is the most deprived region of England and so the benefits may potentially be larger here than in other regions.

Furthermore, the instantaneous effects reported in Year 0 (Fig. 1) are based on the school year the intervention occurred, though the actual audit and recommendations may have taken place later in the school year. There may be some non-compliance as schools choose which recommendations from the audit to enact. There are also possible spill-over of effects from treated to not treated schools if knowledge was shared. These issues would likely bias the estimates towards zero.

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Data availability

The authors do not have permission to share data.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econlet.2024.111614.

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