Department for Education

The 16 to 19 bursary fund: impact evaluation

Research report

June 2015

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Background

The Education Maintenance Allowance (EMA) was a cash transfer worth up to £1,200 per year, paid to 16- to 19-year-olds from low-income households conditional on post-compulsory education participation. The EMA was intended to help achieve the government's priority of closing the gap in attainment between those from poorer and more affluent backgrounds, and ensuring 16- to 19-year-olds participate in and benefit from a place in education or training. It was introduced in 2004 and was found to be reasonably successful in raising participation amongst those eligible for the award; Dearden et al. (2009) estimate that the EMA increased Year 12 participation by 4.5 percentage points amongst this group.

However, the EMA was considered to have a high deadweight cost, meaning that a large number of EMA recipients would have stayed in education even without it. In 2010/11, an estimated 57% of 560,000 individuals participating in education England were eligible for some form of the award, which meant its total cost was high: around £564 million in 2010/11. Despite the findings in Deaden et al. (2009) that expected longer-term savings through higher tax revenues and lower benefit payments outweighed the up-front costs, the EMA was abolished in England and replaced with the "16 to 19 Bursary Fund" in September 2011. The 16 to 19 Bursary Fund differed from the EMA in two key ways. First, it had a significantly lower budget of £180 million in 2011/12, £384 million less than the 2010/11 EMA in nominal terms. Second, whereas for the EMA grant amounts were clearly defined and related to parental income, under the 16 to 19 Bursary the majority of allocation was essentially discretionary.[1] Schools, colleges and training providers would receive an overall budget for spending on Bursaries but could choose their own eligibility schemes in terms of the amount per award, how they were paid (cash or in kind) and how frequently they were paid (e.g. weekly, termly or yearly).

The Institute of Education and the Institute for Fiscal Studies were commissioned by the Department for Education (DfE) to undertake a statistical impact analysis of this reform. This report provides estimates of the policy's impact on participation and attainment during the 2011/12 and 2012/13 academic years, and provides a cost–benefit analysis of the policy taking into account effects on lifetime earnings and exchequer tax receipts and benefit payments. The study is part of the Department for Education's wider research on the 16 to 19 Bursary Fund, which includes a separately commissioned process evaluation investigating the characteristics of pupils receiving the Bursary, its administration and perceived impact.

[1] A number of "Defined Vulnerable Group" Bursaries were also made available, which were subject to strict eligibility criteria and not awarded on a discretionary basis. These account for a small fraction of the overall number of bursaries awarded, however.

Methods

The primary outcomes analysed in this report are full-time (FT) participation[2] in post-16 education and whether learners had achieved the Level 2 or 3 attainment[3] threshold by the end of the academic year in which they turned 18. The estimated impacts should be interpreted as the changes in participation and attainment rates compared to a hypothetical no-reform scenario where the EMA had remained in place and unchanged in 2011/12 and 2012/13. It is not an estimate of its effect compared to no financial support for learners aged 16 to 19. We do not observe whether or not individuals received a Bursary in our data. Our estimates are therefore reported at the population level and separately for the group of individuals who would have been eligible for the EMA if it had continued.

Measuring the impacts of the policy reform is complicated by the fact that it was implemented for all young people in England at the same time. With no true control group, it is challenging to identify a baseline against which outcomes in 2011/12 and 2012/13 can be compared. In order to estimate the causal impact we need to estimate what participation and attainment rates would have been in 2011/12 and 2012/13 *had there been no reform to EMA*. We do this by using groups that were never eligible for the EMA as the control group.

This analysis is based on the administrative data for state school pupils who were in Year 11 between 2005/06 and 2011/12. It tracks post-16 education outcomes for lower-income pupils – whom it is believed would have been eligible for EMA had it been retained – against the same outcomes for a group of pupils whose family income was just too high for them to have ever been eligible for EMA. The latter group is assumed to have been unaffected by the reform and therefore the *change* in their observed post-16 education outcomes after the reform is taken to be a reliable guide to the changes that would have occurred to our groups who are affected by the policy change. We show that prior to the reform, all our groups did move in parallel in terms of participation and attainment. We assume that this would have continued if there had been no reform. Using this assumption (the so-called common trends assumption) we can identify the causal impact of the introduction of the 16 to 19 Bursary using a 'difference in difference' (DiD) methodology. We do a number of robustness checks to check our methodological approach.

[2] Participation indicators come from two sources: the school census and the Individualised Learner Record (ILR) data. If an individual is present in the school census and not the ILR, they are classified as participating full time. If they are in the ILR, they are classified as being in full-time participation if their mode of study indicator in the ILR is set equal to 1 (full time, full-year) or 2 (full time, part-year). The alternative for this variable is 3, which indicates part-time study.

[3] Level 2 attainment is qualifications of at least five A*-C GCSEs or equivalent, while Level 3 attainment is at least two A Levels or equivalent via non-academic qualifications.

An important feature of the analysis is that it is performed without the direct observation of income in the available data. EMA eligibility is estimated through family income figures imputed on the basis of other socio-economic characteristics, such as Free School Meals and local neighbourhood characteristics,[4] and their relationship with income observed in the nationally representative Family Resources Survey (FRS). The result of this necessary but imperfect imputation is that the results almost certainly underestimate the true effect of the policy change. Under the old EMA policy, children could get some EMA if their family income was £30,810 or below. The adopted approach attempts to minimise the underestimation by including individuals with incomes up to £35,000 in our 'EMA-eligible' group, reducing the extent to which our control group contains EMA-eligible individuals. This means that our estimates of impact are for a group slightly larger than the true EMA-eligible group,[5] but ensures that our control group is unlikely to contain individuals who would have been eligible for the EMA. It also means that our estimate of the overall impact on the whole cohort should be robust, as long as our modified DiD assumptions hold.

Impact on participation and attainment

The headline participation and attainment effects are shown in Table i.[6] Since we have two post-reform years of data, these impacts are the average across the two years and this analysis supersedes that of the interim report (Britton et al., 2014). The first row shows that the estimated effect of the implementation of the policy led to a 1.6 percentage points (ppts) fall in FT participation amongst Year 12 students who would otherwise have been eligible for the full EMA award. This translates into a fall from 82.1% to 80.5% for this group. In other words, their participation rate in FT education in Year 12 would have been 1.6 ppts higher on average in 2011/12 and 2012/13 had there been no reform to EMA. The impact among the wider group of pupils who would have been eligible for any level of EMA support is 1.4 ppts, which translates into a fall amongst the cohort as a whole of 0.9 ppts, from 85.0% to 84.1%. For the Year 13 transition cohort, there was a 2.0 ppt fall in FT participation among the poorest students, who would have previously been eligible for the full EMA, and a 1.7 ppt fall among pupils who would previously have been eligible for any EMA. This translates into a reduction of 1.0 ppt across the entire cohort, from 71.6% to 70.6%.

[4] The local neighbourhood characteristics are: Index of Multiple Deprivation (IMD) and Income Deprivation Affecting Children Index (IDACI) scores, the ACORN socio-economic classification and the proportion of households that are owner-occupiers.

[5] For the remainder of the report, when we refer to the EMA-eligible group we are referring to this slightly larger group and hence are likely to be underestimating the impact for those who are eligible for the EMA. Our overall estimate of income for the whole cohort will not suffer from this problem.

[6] Participation and attainment figures in Table i may differ from official government statistics due to slight differences in methodologies used.

Table i: Summary of estimated impacts on attainment and participation

	Impact on lowest-income pupils (who would have been eligible for maximum EMA support)	Impact across all pupils who would have been eligible for any EMA support	Impact across cohort as a whole
Y12 FT participation	-1.56ppts (80.5%)	-1.44ppts (81.4%)	-0.87ppts (84.1%)
Y13 FT participation	-2.02ppts (65.2%)	-1.69ppts (66.5%)	-1.02ppts (70.6%)
L2 by 18 attainment	-2.25ppts (80.2%)	-1.83ppts (82.4%)	-1.11ppts (87.2%)
L3 by 18 attainment	-0.45ppts (34.3%)	-0.27ppts[ᵻ] (38.1%)	-0.16ppts[ᵻ] (48.4%)

The figures in the parentheses give the actual average post-reform percentages. ᵻ Indicates not statistically significant (at the 5% level).

The impacts on attainment are largest for the poorest students, defined as those who would have been eligible for the full EMA award. We estimate that there was a 2.3 ppt fall in the Level 2 achievement rate by age 18, leading to a 1.1 ppt fall across the whole Year 13 cohort, from 88.3% to 87.2%. For Level 3 achievement, the effect is much smaller at 0.5 ppts for those who would have been eligible for the full grant, with no statistically significant impact across the whole eligible group or the cohort as a whole.

An important benchmark with which to compare the participation results is the effect of the introduction of the EMA on participation rates estimated by Dearden et al. (2009). They estimate that participation amongst Year 12s increased by 4.5 percentage points amongst those eligible for some form of the EMA. There are two important caveats to this estimate, however. First, it may have overestimated the true effect as the estimate was taken from the EMA pilot, which targeted deprived areas of England, which may consist of individuals more responsive to the grant. Second, the real value of the EMA was considerably lower in 2010/11 than during the pilot, in 1999: in fact the maximum grant available did not increase in nominal terms at all, remaining constant at £30 per week.

There is some evidence of a stronger effect – in particular, for Level 2 attainment – in the second year of the reform, suggesting that the true effects are muted slightly by the transitional year and that longer-term impacts might be greater than those found on average across the period here.[7]

Due to methodological concerns, we are unable to estimate the effects of the policy on part-time participation rates and therefore overall participation rates. If some individuals' response to the reform were to substitute into part-time education from full-time

[7] This is consistent with the estimates in the 2011/12 interim report (Britton et al., 2014) being smaller in magnitude.

education, then our estimated impact on full-time participation rates would be unaffected. However, because we cannot estimate the impact on part-time education, for cost–benefit analysis we have to assume that this remains unchanged. If the reform actually increased part-time education then this would result in us overstating the overall negative participation effects. Due to insufficient years of observations we are also unable to investigate the extent to which people have changed their behaviour so that they take time away then return to education as a result of the reform.

Finally, and again due to methodological concerns, we are unable to obtain robust estimates of subgroup effects other than by gender, where we find no evidence of significant differences in impacts. We were not able to investigate whether the effect was larger for certain ethnic groups or for different regions or other groups of interest as the common trends assumption does not appear realistic for these groups individually.

Cost–benefit analysis

To assess the value for money of the policy change, the short-run savings from a reduction in financial support and the funding of education places of £459 million (in September 2014 prices) can be compared to the impacts on participation. Using the estimates discussed above, we estimate that on average in 2011/12 and 2012/13, there were 23 fewer individuals in full-time education, 14 fewer individuals achieving Level 2 qualifications and 2 fewer individuals achieving Level 3 qualifications per £1 million saved. This does not take into account any part-time participation effect. Due to the misclassification error discussed above, these estimates are likely to underestimate the true effect. In our alternative specification using individuals with very high parental income as the control group, the equivalent figures are 34 fewer individuals participating in education, 19 fewer Level 2 achievers and 7 fewer Level 3 achievers per £1 million saved. As discussed above the figures using this control group are likely to be less affected by the misclassification bias, but may not be a good representation of the changes in participation that would have occurred in the absence of the policy reform for our EMA-eligible groups.

However, this does not take into account the long-run costs associated with the policy or the consequences of alternative uses of the immediate cash savings. To the extent that individuals become less educated as a result of the policy, lifetime earnings and hence exchequer tax receipts will be affected. Through a simulated model of lifetime earnings we find that the average discounted present value cost of lost earnings is £6,735 per household affected by the policy change. This translates into an estimated loss to the exchequer of £2,762 per affected household in discounted present value terms, through reduced tax receipts and increased benefit payments, offsetting the immediate savings.

Overall, on central estimates using this methodology the short-run savings associated with the policy are in aggregate outweighed by the long-run losses: we estimate an overall loss to the exchequer of £84 million, modelling earnings at the household level including dynamics in family formation. There is a very high degree of uncertainty associated with these figures due to the difficulties associated with modelling lifetime earnings effects for the future and having to produce models using proxy variables. As such, our estimated confidence intervals span zero and when we do not account for household-level effects we estimate a small net saving to government.

We believe these estimates are likely to understate the true impact, for several reasons. First, the misclassification error associated with our estimates suggests we are underestimating the true effect on attainment – for example, in the extreme case of using the highest income group as a comparison group, the loss to the government increases from £84 million to £466 million. Second, there appears to be evidence of a stronger effect in the second year, meaning the average impact across the two years is muted by the transitional phase between policies. This appears to be particularly important for Level 2 attainment, which is a key part of our cost–benefit analysis. Third, we look only at the wage effect and do not account for wider productivity effects, which are likely to be larger (e.g. see Dearden, Reed and van Reenen, 2006), or the effect on other outcomes such as health or crime.

Conclusion

The estimates presented here suggest that abolishing EMA and introducing the 16 to 19 Bursary Fund had a relatively modest effect on participation and attainment in the first two years of implementation, but that this disproportionately affected low-income young people.

Importantly, it is likely that the overall impact estimates presented in this report underestimate the true impacts of the policy reform in question. Nevertheless, our cost–benefit analysis estimates that the long-run costs from the policy outweigh the short-run savings, and that is without taking into account wider impacts of the policy, including the effects on productivity, crime or health.

Of course, the analysis does not account for the economic benefits that accrue from alternative investments of the short-run savings. Those alternatives should be examined with a similar approach to that used here in order to assess their relative cost-effectiveness.

1. Introduction

The 16 to 19 Bursary Fund was implemented nationally in September 2011 as the replacement for the previous Education Maintenance Allowance (EMA). The policy intention was to preserve participation amongst individuals from low income backgrounds, but to reduce the associated deadweight cost. This involved a budget cut – from £564 million in England in 2010/11 to £180 million in 2011/12 – and a change in the allocation of funds. With the intention of providing more efficient and better targeted support for post-16 learning, allocation was delegated to sixth forms and colleges, allowing them to allocate most of the funding on a discretionary basis to the learners they deemed most in need of additional support.

Prior to this, EMA eligibility had been dictated by clear guidelines: individuals were eligible for £30 per week if their parental income was below £20,818; £20 per week if parental income was between £20,818 and £25,521; and £10 per week if parental income was between £25,522 and £30,810. Individuals would receive their weekly payments so long as their attendance was sufficiently high. The Institute of Education and Institute for Fiscal Studies were commissioned by the Department for Education (DfE) to undertake an impact analysis of the policy change on participation and attainment as part of a wider evaluation including a separate process evaluation study (Lloyd et al., 2015). This report provides findings on the impacts of the policy on participation and attainment rates in 2011/12 and 2012/13.

The EMA was closed to new applicants on 1 January 2011, and the new 16 to 19 Bursary Fund arrangements were in place from September 2011 onwards. Pupils who started Year 13 in September 2011 were subject to transitional arrangements, whereby those who had previously successfully applied for a full EMA award of £30 a week were to receive a reduced EMA of £20 a week, while those who had previously claimed a partial EMA award of £10 or £20 a week would no longer receive EMA but could apply for support from the 16 to 19 Bursary Fund. Meanwhile, pupils starting Year 12 in September 2011 faced the new bursary arrangements only.

The new bursary arrangements consist of two types of award. The first is defined vulnerable group bursaries, which have clear eligibility criteria for post-16 providers to follow and are worth up to £1,200 per year (this is equal to the maximum EMA award). Only a small number of individuals are eligible for these – approximately 2% of the cohort in education and work based learning received them in 2013/14 according to the process evaluation. The second type is discretionary bursaries, with allocations determined by colleges and schools. Eligibility (based on whether the individual needs financial support), distribution (weekly, monthly or termly), amount (individuals can receive more than they would under the EMA) and conditions (dependent on attendance, attainment or behaviour) are all at their discretion. Any individual can apply for a discretionary bursary, though in practice it is unlikely to be awarded without evidence of financial need.

The analysis in this research briefing uses statistical techniques to provide estimates of the impact of replacing EMA with the 16 to 19 Bursary Fund. The outcomes analysed are full-time (FT) participation[8] in post-16 education at school, college or training providers, and whether individuals had achieved the Level 2 or 3 attainment threshold by the end of the academic year in which they turned 18[9]. The estimated impacts in this research briefing should be interpreted as the changes in participation and attainment rates compared to a hypothetical no-reform scenario where the EMA had been retained. They are not the impacts compared to a scenario of no 16 to 19 financial support.

Measuring the impacts of the policy reform in question is complicated by the fact that the reform was implemented across England at the same time. With no true control group it is challenging to identify what participation and attainment rates would have been in 2011/12 and 2012/13 *had there been no reform to EMA*. Reliably measuring this quantity is crucial, as it is the baseline to which the actual post-reform levels of participation and attainment should be compared, in order to isolate impacts that can be attributed to the policy reform itself.

Briefly, this analysis compares the post-16 education outcomes for lower-income pupils – who would have been eligible for EMA in 2011/12 and 2012/13 had it been retained – against the same outcomes for pupils whose family income was slightly too high for them to have been eligible for EMA had it been retained. The latter group is assumed to have been unaffected by the reform, since they would not have been eligible for EMA anyway. Furthermore, it is assumed that the change in their observed post-16 education outcomes between ages 16 and 19 is a reliable guide to the change in education outcomes that would have been seen among lower-income pupils had there been no policy reform, despite them having different *levels* in these outcomes controlling for other factors. Pupils with family incomes just above the EMA eligibility income threshold are used as the basis for comparison in order to maximise the validity of this assumption, which is crucial to the 'difference-in-difference' (DiD) approach used here.

The report also assesses the costs and benefits of the policy. The savings are in the short run, through the £384 million budget cut in 2011/12 and through fewer people being in education. The costs are generally in the long run, as individuals become less educated under the new policy and hence earn less through their lifetime. We estimate these long-run costs by estimating a simulated model of lifetime earnings that is

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[8] Participation indicators come from two sources: the school census and the Individualised Learner Record (ILR) data. If an individual is present in the school census and not the ILR, they are classified as participating full time. If they are in the ILR, they are classified as being in full time participation if their mode of study indicator in the ILR is set equal to 1 (full-time, full-year) or 2 (full-time, part-year). The alternatives (modes 3-6) in the ILR are all classified as part-time study.

[9] Level 2 attainment is qualifications of at least 5 A*-C GCSEs or equivalent, while Level 3 attainment is at least two A Levels or equivalent. In both cases we include non-academic qualifications with the academic qualifications. Unfortunately we are unable to obtain reliable estimates of the two separately.

dependent on highest education qualification, then running the simulated earnings through a model of taxes and benefits to estimate the total cost to the exchequer in lost tax receipts and increased benefit spending of having a slightly less well educated population. These costs are put in discounted present value terms for comparison with the short-run savings. It is important to recognise that estimating these long-run costs is a speculative exercise and comes with a high degree of uncertainty.

This research briefing is structured as follows. Section 2 sets out in detail the challenges involved in reliably measuring the impacts of the reform on participation and attainment rates, and the approach proposed by this research to deal with them. It describes the data sources used and the information on outcomes and pupil characteristics that will frame the impact analysis. It also tests the likely validity of the proposed empirical approach and identifies the particular outcomes for which the proposed approach is most likely to provide reliable estimates. Section 3 then provides the estimated impacts of the reform on participation and attainment, both across the cohort as a whole and for specific groups of pupils. In Section 4 the robustness of the results to various checks is tested. Section 5 provides a cost–benefit analysis by evaluating the effect of the policy on lifetime earnings and the discounted present value of tax receipts to the exchequer. Section 6 concludes.

2. Impact analysis methodology

This section describes the empirical challenges and approaches involved in robustly measuring the impact of the 16 to 19 Bursary Fund on post-16 participation and attainment rates. It also discusses the data, provides evidence on the likely suitability of the proposed approach and identifies the outcomes examined in the impact analysis. A description of the methodology for the cost–benefit analysis is provided in Section 5 alongside the results.

2.1 Evaluation design and methods

The aim of this report is to measure the impact of the policy reform as the difference between the actual participation and attainment outcomes that were observed in 2011/12-2012/13, and the participation and attainment outcomes that would have been observed in 2011/12-2012/13 had there been no policy reform. The latter cannot be measured directly as they relate to a hypothetical – often referred to as 'counterfactual' – scenario. The evaluation strategy must therefore use statistical techniques to best approximate the participation and attainment outcomes that would have been observed in the counterfactual scenario. A major challenge here is that the policy reform in question was implemented across the whole of England at the start of the 2011/12 academic year. This means there are no easily identified areas or groups of learners still eligible for the scheme's predecessor (EMA) which could serve as potential comparators.[10]

Since EMA eligibility was determined on the basis of family income, a potential comparison group is pupils from higher-income backgrounds who would not have been eligible for EMA even if it had been left unchanged. However, it is well known that pupils from higher-income backgrounds generally have higher post-16 participation and attainment rates. We therefore compare the change in participation from before and after the reform between our treatment and control groups, thus removing the effect of levels.

A further issue arises when attempting to make comparisons over time between education outcomes in 2011/12-2012/13, when the 16 to 19 Bursary Fund was in operation, and education outcomes in previous years. In particular, other factors could have caused a rise or fall in participation or attainment rates over this period; examples might be the broader economic environment, the characteristics of young people, the

[10] Pupils in Year 14 in 2011/12 were still eligible for EMA. However, such pupils would have been a poor comparator because of their age difference (compared with pupils in Year 12 and Year 13) and because there are few of them, thereby preventing precise statistical analysis. An alternative approach might be to use Scotland or Wales as a control group, as the EMA was preserved in its original form there. However data constraints (the National Pupil Database does not include pupils in these areas) mean that this is not possible to do in a robust way.

state of the youth labour market or reforms to the higher education finance regime.[11] To the extent that these factors differentially affect the treatment and control groups, it is necessary to strip out these factors in order to be confident that any changes in participation and attainment only reflect the introduction of the 16 to 19 Bursary Fund.

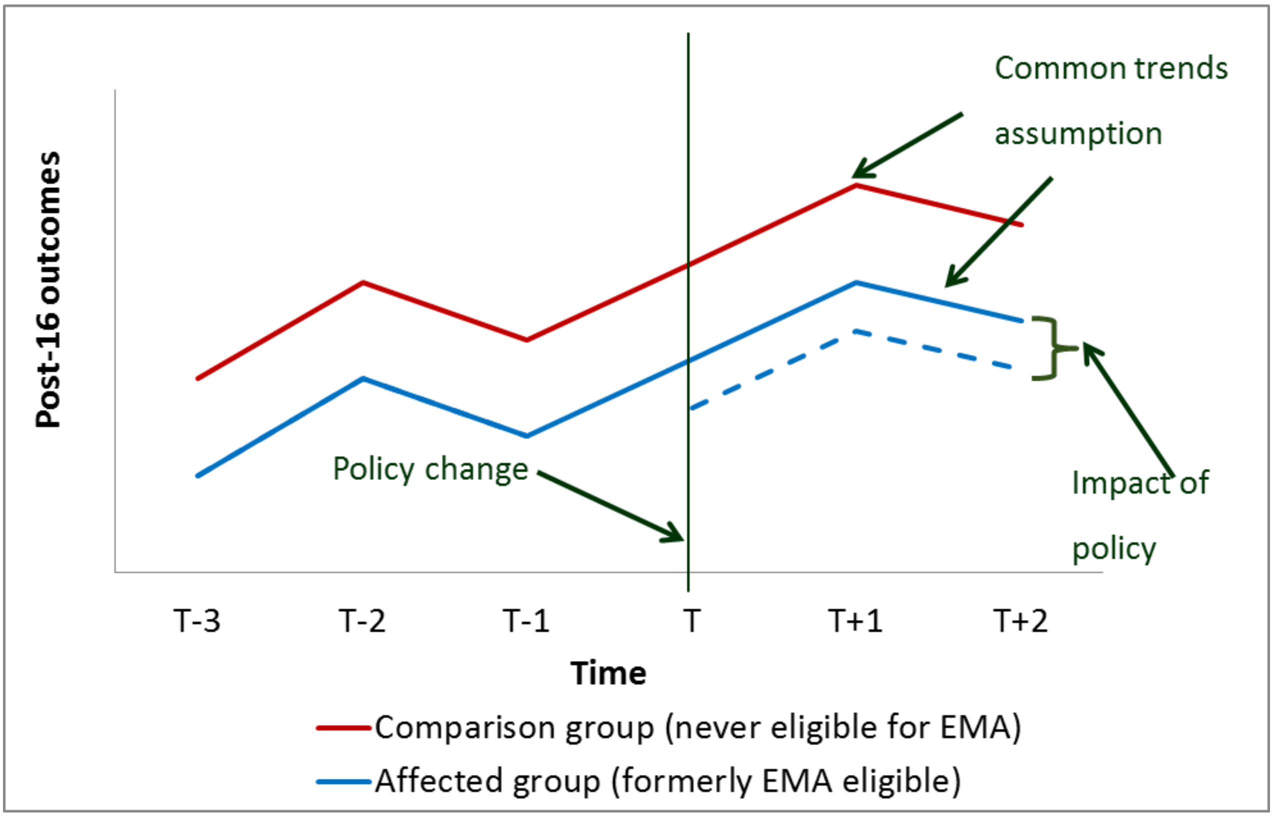
In an attempt to circumvent this issue, the empirical analysis in this report compares the *trends over time* in participation and attainment outcomes for lower-income pupils against the *trends over time* in the same outcomes for higher-income pupils. Specifically, the analysis tracks successive cohorts of young people and examines the change in lower-income pupils' outcomes after the reform; this is then compared against the change in higher-income pupils' outcomes after the reform. The *difference* between these two changes is then estimated as the impact of the policy reform; as a result, this methodology is referred to as a 'difference-in-difference' (DiD) approach.[12]

Figure 2.1.1 illustrates the DiD approach schematically. The crucial assumption that underpins the DiD approach is that of common underlying trends after the policy reform: while the levels of outcomes are different, the trends in outcomes between different groups follow a parallel trajectory and would have continued to follow a parallel trajectory had there been no policy reform. In other words, the assumption is that there is no convergence or divergence between the education outcomes of the affected and comparison groups; if the assumption holds, then any convergence or divergence that takes place after the policy reform occurs can be attributed to the policy reform itself.

[11] The cohort in Year 13 in 2011/12 would have been the first cohort to face the new higher education finance regime, involving maximum tuition fees of £9,000 per year.

[12] This approach has been used before to evaluate the quantitative impact of education programmes and reforms. See, for example, Kendall et al. (2005) and Tanner et al. (2011).

Figure 2.11: Illustration of the Difference-in-Difference Approach



The common trends assumption cannot be assessed using data following the policy reform because that would involve outcomes that may be affected by it. Instead, the validity of this approach is assessed by examining whether the affected and comparison groups exhibited common trends in their outcomes before the policy reform. If this is true then, while it does not prove conclusively that the two groups' outcomes would have continued to follow parallel trends after the policy reform (had the reform not happened), it does provide some confidence in the common trends assumption.

The empirical analysis used in this report goes a step further and uses a modified version of a DiD model to provide greater statistical robustness. As the data sources on which the analysis is based cover many time periods, it is possible to estimate a long-run underlying linear trend in education outcomes for each income group as well as the effects of observed characteristics. To conduct an assessment of whether the common trends assumption holds, we remove the underlying trends and the effect of background characteristics from the data for each income group and investigate whether the remaining trends are parallel. Using this approach we find evidence that the common trends assumption holds for our main outcome measures, but not for part-time participation or non-academic qualifications. We therefore use this modified DiD approach to examine the impact of the reforms on full-time[13] participation in Y12 and Y13 as well as the proportion obtaining L2 and L3 qualifications.[14]

Section 2.4 provides graphical evidence on whether the parallel trajectories assumption holds for our outcomes for different income groups once we strip out the effects of linear trends and changes in background characteristics. The impact analysis will then focus on those outcomes for which this appears to hold, since the empirical approach is most valid for them. However it is first necessary to describe in more detail the data that is used in the empirical analysis and the methods for constructing the different income groups.

2.2 Data and policy definitions

This analysis uses National Pupil Database (NPD) records on state school pupils who were in Year 11 from 2005/06 to 2011/12. Table 2.2.1 provides a breakdown of the number of pupil records analysed, by the academic year (cohort) in which pupils were in Year 11.

Table 2..2.1: Number of pupil records by year

Year 11 cohort	Number of records
2005/06	591,499
2006/07	598,949
2007/08	595,974
2008/09	577,260
2009/10	576,180
2010/11	564,584
2011/12	542,748
Total	4,047,194

For each pupil, the following information is available on their demographic and socio-economic characteristics from the School Census:

- Gender;
- Ethnicity;
- Free School Meals (FSM) eligibility;

[13] Participation indicators come from two sources: the school census and the Individualised Learner Record (ILR) data. If an individual is present in the school census and not the ILR, they are classified as participating full time. If they are in the ILR, they are classified as being in full time participation if their mode of study indicator in the ILR is set equal to 1 (full-time, full-year) or 2 (full-time, part-year). The alternatives (modes 3-6) in the ILR are all classified as part-time study.

[14] Technically, this approach of estimating separate trends for each group can be thought of as running a before–after analysis for each group and then examining the differences between each affected group's before–after estimates and the comparison group's before–after estimate.

- English as an Additional Language (EAL) status;
- Special Education Needs (SEN) status;
- Index of Multiple Deprivation (IMD) and Income Deprivation Affecting Children Index (IDACI) scores.[15]

Linked information on each pupil's average point score at Key Stage 2 (KS2) and total capped point score[16] at Key Stage 4 (KS4) is also available from within the NPD data.

Finally, information on each pupil's post-16 participation and attainment is also linked in. Participation records come either from School Census data one (two) years later to capture participation in school sixth forms in Year 12 (13),[17] or from Individualised Learner Record (ILR) data to capture participation in other sixth form and further education institutions. Post-16 attainment data is taken from the Level 2/3 indicators data provided by the DfE: for each pupil, indicators for whether they achieved the Level 2/3 threshold by the end of the academic year in which they turned 18 – and if so, whether through the academic or vocational route – are linked in. The participation and attainment outcomes are recorded for 2005/06[18] to 2012/13, which was the latest year available at the time of analysis.

The cohorts who were in Year 11 up to 2008/09 faced the previous EMA regime: these are referred to as EMA cohorts. The Year 11 cohort in 2009/10 is the EMA transition cohort: such pupils would have faced the EMA regime in Year 12 in 2010/11, but would have then faced the transitional arrangements in Year 13 in 2011/12. Finally, the cohorts in Year 11 in 2010/11 and 2011/12 are the first bursary cohorts: such pupils will have faced the new bursary arrangements in both Year 12 and Year 13. This means that the analysis of Year 12 participation outcomes in this report focuses on the two bursary cohorts, while the analysis of Year 13 participation and attainment outcomes focuses on a combination of one transition cohort and one bursary cohort who have been exposed to the bursary for two years.[19]

The analysis of common trends and the impact analysis both take into account pupil characteristics in order to control for them to the extent that they might influence the participation and attainment outcomes of interest. In particular, there may be changes over time in the relevant characteristics of different income groups: for example, while higher-income pupils tend to have higher prior attainment, it may be that the prior attainment of lower-income pupils has caught up over time, thereby narrowing the

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[15] For more information on these indices, see http://data.gov.uk/dataset/index-of-multiple-deprivation.

[16] This is GCSE points taking scores from the best 8 exams only.

[17] Individuals are defined as being in Year 12 in the first academic year they are observed in education after Year 11 and in Year 13 in the second academic year they are observed in education after Year 11.

[18] This is one year after the EMA was implemented nationally. The analysis does not use any data on post-16 outcomes preceding this point in order to avoid having any policy changes during the pre-reform window (which could compromise the assessment of the trends in outcomes) and to increase the validity of the use of a linear trend.

[19] In the interim report (see Britton et al., 2014), we were able to investigate the transitory Year 13 cohort only.

attainment gap. Any analysis would need to take *relative* changes such as these into account to avoid confounding the estimated impacts of the policy reform. The following characteristics are controlled for, noting that the analysis is split by gender so that is not included as a control:

- Ethnicity;
- EAL status;
- SEN status;
- Attainment at KS2 and KS4;
- IDACI quintile and IMD quintile.

2.3 EMA eligibility imputation

The next step is to construct the different income groups in order to identify pupils who would have been affected by the reforms and pupils who were not. However, the data used in this analysis do not contain any measures of family income that could be used to define whether a pupil would have been eligible on EMA grounds. We therefore have to impute EMA eligibility based on a set of the socio-economic characteristics available. These are an individual's FSM status in Year 11, their neighbourhood's IMD and IDACI scores, their postcode's 'ACORN' socio-economic classification[20] and the proportion of households in the pupil's neighbourhood that are owner-occupiers (taken from the 2001 Census). Finally, KS2 attainment is also included as this is known to be well correlated with parental income. All of this information is combined into an index which serves as a proxy for family income.[21]

To map this index to levels of actual family income, it is combined with information on the distribution of family income taken from the Family Resources Survey (FRS) for 2003/04 to 2010/11. In particular, the percentile points for the distribution of gross family income among households with at least one child aged 14–16 are used. Each pupil's percentile in the socio-economic index is then mapped to the same percentile in the income distribution: for example, if a pupil's score on the socio-economic index is at the 39th percentile of that index, they are given the income corresponding to the 39th percentile of the family income distribution according to the FRS. The assumption underpinning this is that a pupil's ranking in the socio-economic index is the same as their ranking in the distribution of family income.

With a level of family income assigned to each pupil, they are then classified to a particular level of potential EMA eligibility using the known income thresholds for EMA, including for cohorts subject to the transitional or bursary arrangements who might

[20] For more information on the ACORN index, see http://acorn.caci.co.uk/.

[21] This is done using principal components analysis. More information can be found in Appendix B.

otherwise have been eligible for EMA. The aim is to identify groups of pupils with different levels of exposure to the policy reform in 2011/12 and 2012/13.[22] Each income group corresponds to a particular level of EMA eligibility, as shown in Table 2.3.1. We define those eligible for any form of the grants as Groups 1-3, with Group 1 consisting of individuals eligible for the full grant (see Table 2.3.2 later).

Table 2.31: EMA eligibility

EMA eligibility	Gross family income (before 2005/06)	Gross family income (from 2005/06)
Full (£30)	£19,630 or less	£20,817 or less
Partial (£20)	£19,631–£24,030	£20,818–£25,521
Partial (£10)	£24,031–£30,000	£25,522–£30,810
None	£30,001+	£30,811+

Our control group – henceforth “Group 4” – consists of pupils whose imputed family income was slightly too high for them to be potentially eligible for EMA if they stayed in education or training. This group serves as main the comparison group in the following analysis.[23] The income band used for this group was chosen specifically to ensure Group 4 would be as ‘similar’ as possible to Groups 1–3 in terms of pupil characteristics and education outcomes, while also being likely to be ineligible for EMA and therefore likely to be unaffected by the 16 to 19 Bursary Fund. Using this tightly-defined comparison group (Group 4) in order to measure the impacts of the reform maximises the likely validity of the assumptions that are made regarding the linearity of the trend in each income group’s outcomes (relative to the comparison group). Section 2.4 uses these group definitions to assess the validity of these assumptions.

An unfortunate consequence of the fact that we do not observe income is that individuals will inevitably be inaccurately allocated to groups and hence EMA eligibility. Consequently, of those who were truly ineligible for the EMA, we will assign a fraction to be eligible, and vice versa. In Appendix B, we test the accuracy of our prediction method by comparing our predicted eligibility with actual EMA eligibility using a small subset of 10,700 individuals who are in both the NPD and the Longitudinal Survey of Young People

[22] The analysis assumes that the EMA eligibility income thresholds would have been held constant in the absence of the policy reform. This seems reasonable as the thresholds were unchanged in nominal terms from 2005/06.

[23] As seen in Table 2.3.2, Groups 1 to 4 cover 72% of the individuals in Y12 or Y13 in 2011/12 (this proportion declines over time from 81% of individuals in Y12 and Y13 in 2005/06). The Table shows that we estimate 60% of individuals as being eligible for some form of EMA. This is only broadly consistent with the 45% take-up figure estimated by the Department for Education, for two reasons. First, Group 3 is extended in Table 2.3.2 to £35,000. If it were set to £30,810, we would get only 56.7% of individuals being eligible. Second, the 45% is the percentage of those in education rather than of the whole population and participation rates are lower amongst the EMA eligible.

in England (LSYPE),[24] which includes a measure of household income, from which we can infer eligibility. We find that of those individuals we assign as ineligible, 34.4% actually receive the grant, according to their responses. Of those we assign as eligible, 75.2% actually receive the grant.

This result strongly suggests that our overall analysis will underestimate the true effect of the policy. This is because if participation were to drop amongst the truly eligible and remain constant amongst the truly ineligible, participation will drop in our control group by more than it should and will not drop by as much as it should in the treated group.

It important to treat these findings with caution, however, as the LSYPE income data is known to be unreliable, and the subset of individuals who we are able to run the test on is a tiny fraction of the overall sample size. We therefore choose not to use these results to adjust our estimates based on the misclassification bias. Instead, we take two precautions. First, we vary the EMA eligibility threshold to minimise the misclassification, finding that extending the income eligibility threshold to £35,000 minimises the misclassification in the LSYPE.[25] This results in the contraction of our control group (Group 4) so that it consists of individuals with household incomes between £35,000 and £45,000.

Second, we provide results with Group 5 – those individuals with the very highest incomes (i.e. those with income above £45,000) – as a control variable. Individuals in Group 5 should be less susceptible to the misclassification bias, meaning that if misclassification is important, the estimated effect of the policy with Group 5 as the control group should be larger. Our main headline results do not use this group as the control group, however, due to concerns that there are too many systematic differences between individuals in this group and individuals eligible for the EMA that we cannot control for. Instead, we treat the results with Group 5 as the control as an upper bound for our estimates. The five groups described above are summarised in Table 2.3.2.

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[24] See https://www.education.gov.uk/ilsype/workspaces/public/wiki/Welcome/LSYPE for details on the LSYPE. Although there were more than 15,000 individuals in the initial dataset, we are only able to use 10,700 because we need a number of years of data on those individuals in order to run the test, and there was some attrition from LSYPE sample.

[25] In practice the results are not sensitive to this research decision.

Table 2.3.2: EMA groups

Group	Notional EMA eligibility	Gross family income (before 2005/06)	Gross family income (from 2005/06)	Proportion in each group (in Y12 or Y13 in 2011/12)*
1	Full (£30)	£19,630 or less	£20,817 or less	44%
2	Partial (£20)	£19,631–£24,030	£20,818–£25,521	5%
3	Partial (£10)	£24,031–£35,000	£25,522–£35,000	11%
4	Control Group	£35,001–£45,000	£35,001–£45,000	12%
5	Rich Group	£45,001+	£45,001+	28%

*The proportion in each group changes with income in each year as eligibility thresholds did not move in line with wages.

2.4 Appropriateness of estimation strategy

As stated above, the DiD approach is valid under the assumption of common trends across groups. We assess this assumption graphically in this section by investigating the pre-reform years of 2005/06-2010/11. For each outcome of interest, a graph is constructed by taking the mean participation/attainment rate across the six pre-reform years for each group and adding to that the residuals from a regression of participation/attainment on a linear trend (which is allowed to vary by group) and background characteristics. This is of interest because we are interested in common trends once these underlying trends and background characteristics have been allowed for.

Figure 2.4.1 shows Year 12 full-time participation of males for Groups 1 to 5, after controlling for the characteristics described above and stripping out linear trends. Under the common trends assumption these lines would be parallel, maintaining the same difference with the comparison group over time. We see that all groups, even Group 5, appear to move in parallel over the entire period.[26] Group 4 – our main control group – appears to follow common trends with all of our groups of interest. Figure 2.4.2 shows the equivalent for females. Again the common trends assumption appears to hold.

Figures 2.4.3 and 2.4.4 look at Year 13 participation of males and females respectively. Figures 2.4.5 and 2.4.6 look at Level 2 attainment through either academic or vocational routes, while Figures 2.4.7 and 2.4.8 look at Level 3 attainment, again for males and females respectively. In all cases there appears to be good evidence of the common trends assumption holding.

[26] We also repeated the analysis using quadratic trends, but these were rarely significant and did not perform as well as simple linear trends.

In all of the participation graphs, there is a jump in 2009/10. This is likely to be due to the increase in participation observed after the recession.[27] The jump is significant enough to not be picked up by the underlying trends. It does not represent a concern for our analysis, as it appears to be common for all groups. This pattern does not appear to hold for attainment, suggesting that participation temporarily increased after the recession, but that those individuals did not acquire any additional qualifications as a result. Indeed the attainment results appear to be less variable each year, suggesting they are following a steady underlying trend and are not as susceptible to economic conditions.

Figure 2.4.1: Pre-reform common trends in male full-time Y12 participation

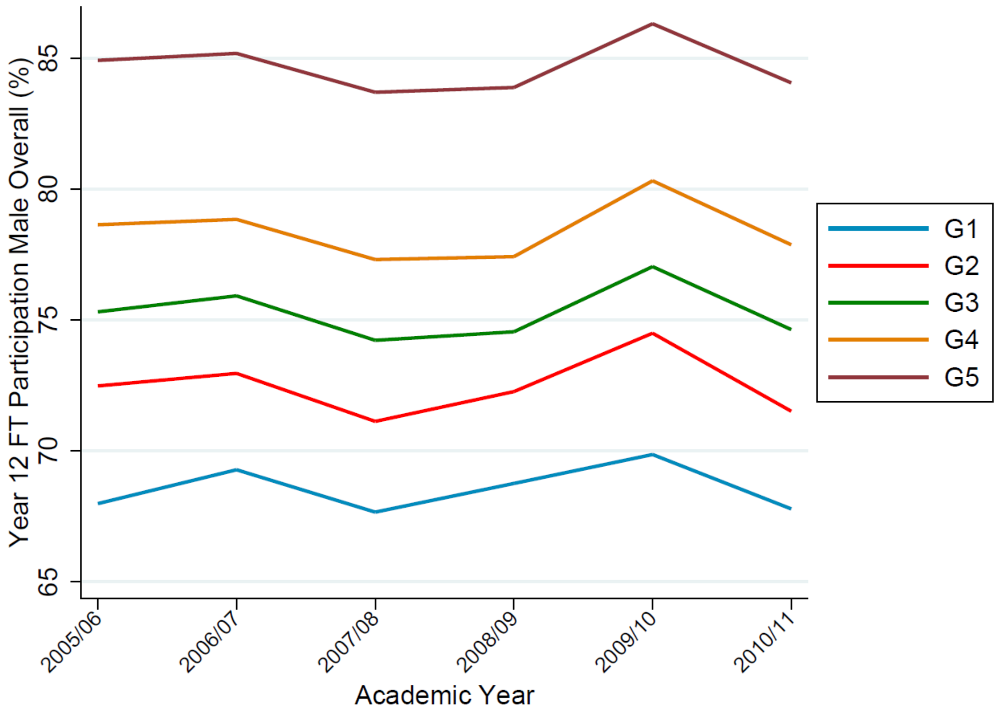

Figure consists of the mean Y12 participation rate for each group in the 6 pre-reform years added to the residuals from a regression of a Y12 participation dummy on a set of background characteristics and a linear trend for that group.

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[27] See http://www.cesi.org.uk/sites/default/files/event_downloads/ACEVO_report.pdf

Figure 2.4.2: Pre-reform common trends in female full-time Y12 participation

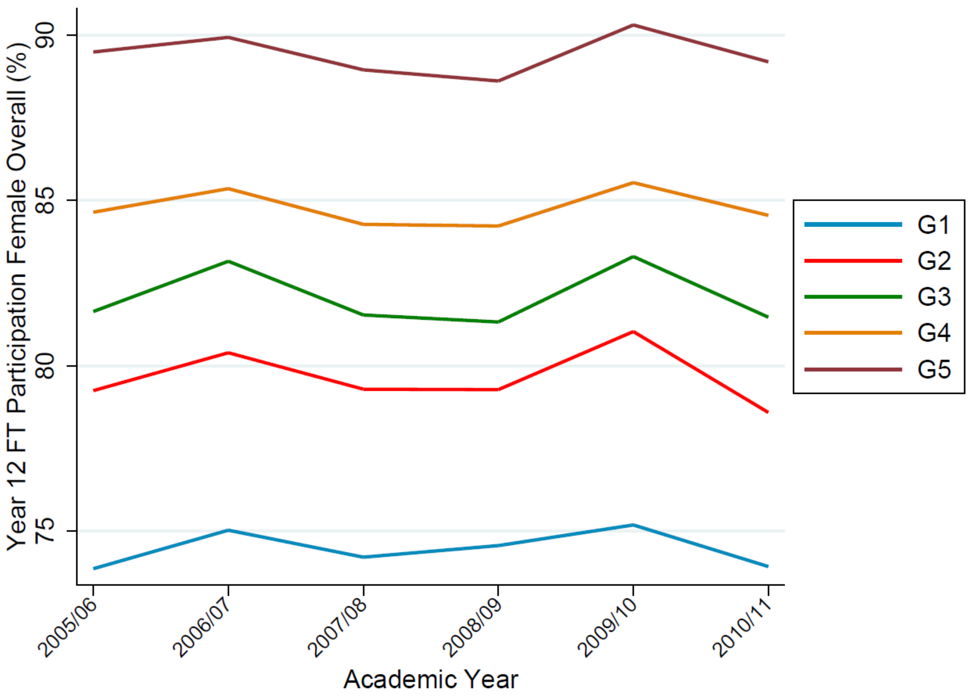

Figure consists of the mean Y12 participation rate for each group in the 6 pre-reform years added to the residuals from a regression of a Y12 participation dummy on a set of background characteristics and a linear trend for that group.

Figure 2.4.3: Pre-reform common trends in male full-time Y13 participation

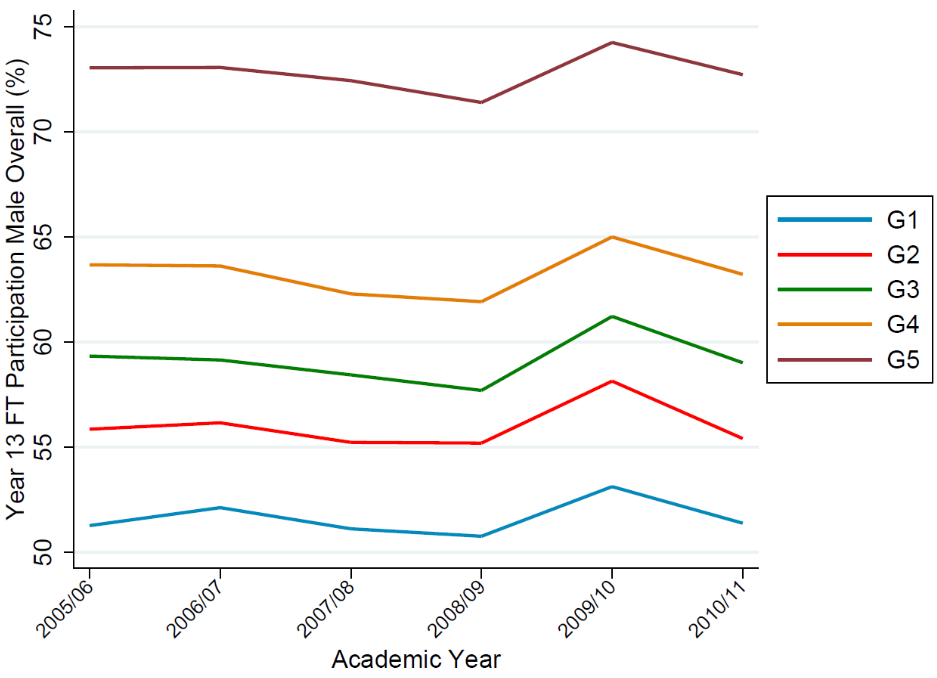

Figure consists of the mean Y13 participation rate for each group in the 6 pre-reform years added to the residuals from a regression of a Y13 participation dummy on a set of background characteristics and a linear trend for that group.

Figure 2.4.4: Pre-reform common trends in female full-time Y13 participation

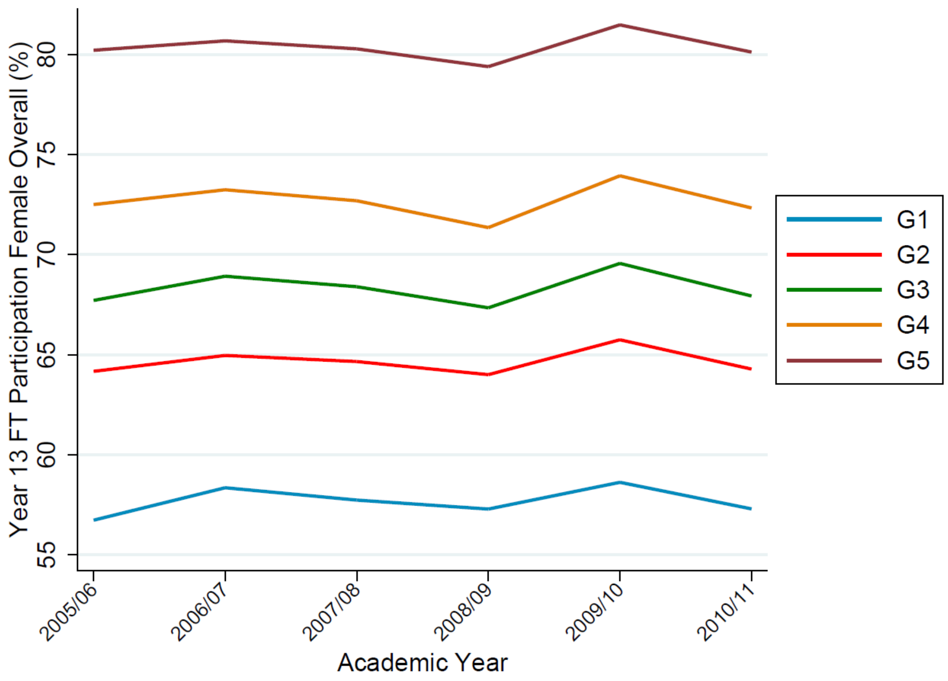

Figure consists of the mean Y13 participation rate for each group in the 6 pre-reform years added to the residuals from a regression of a Y13 participation dummy on a set of background characteristics and a linear trend for that group.

Figure 2.4.5: Pre-reform common trends in male Level 2 attainment

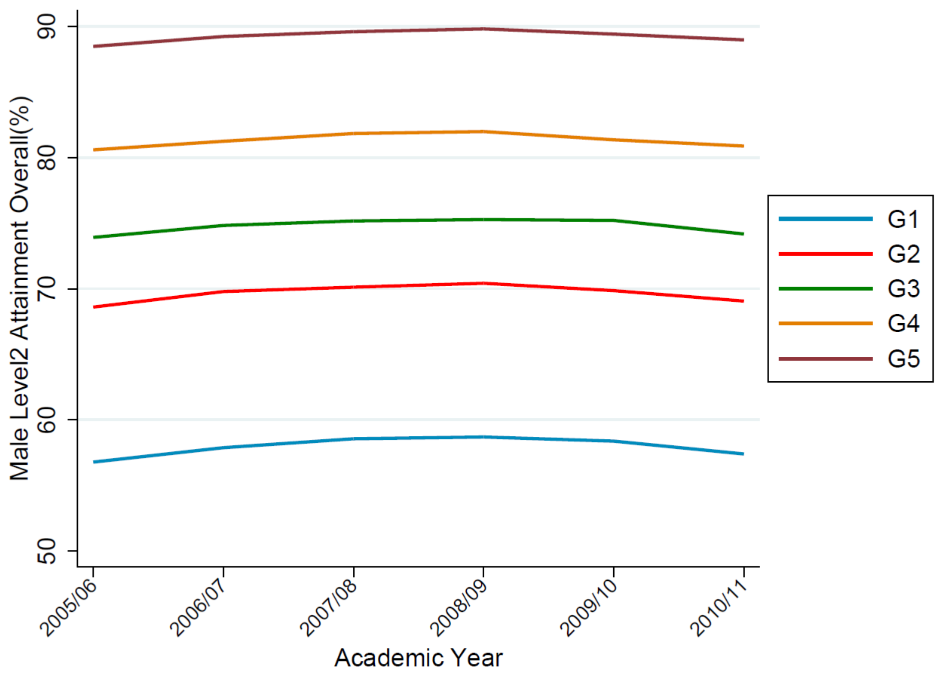

Figure consists of the mean Level 2 attainment rate for each group in the 6 pre-reform years added to the residuals from a regression of a Level 2 attainment dummy on a set of background characteristics and a linear trend for that group.

Figure 2.4.6: Pre-reform common trends in female Level 2 attainment

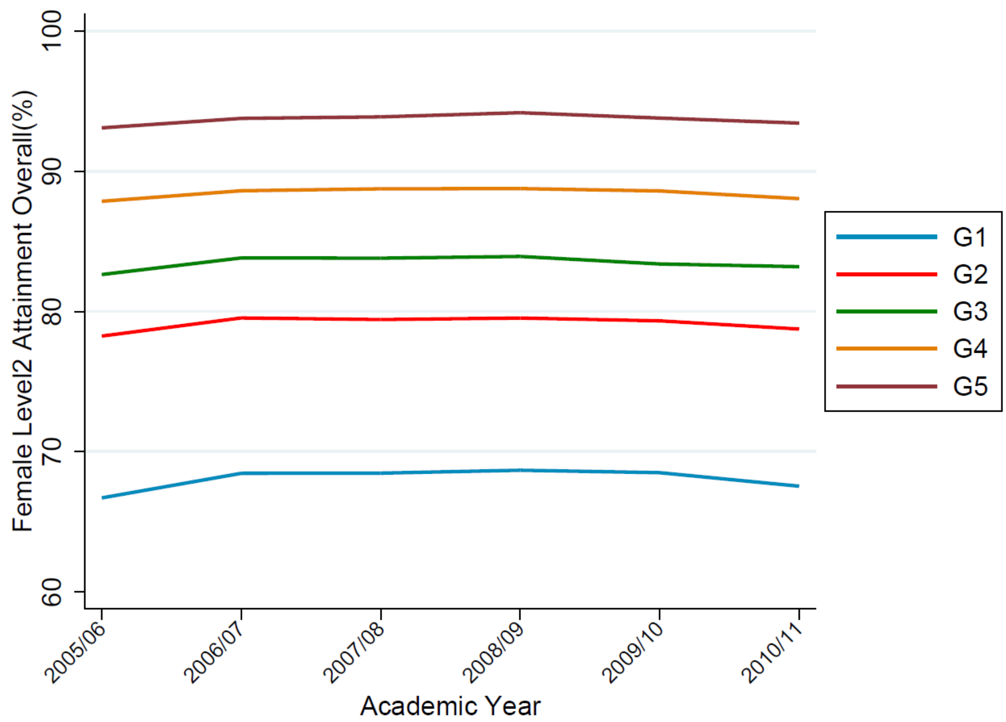

Figure consists of the mean Level 2 attainment rate for each group in the 6 pre-reform years added to the residuals from a regression of a Level 2 attainment dummy on a set of background characteristics and a linear trend for that group.

Figure 2.4.7: Pre-reform common trends in male Level 3 attainment

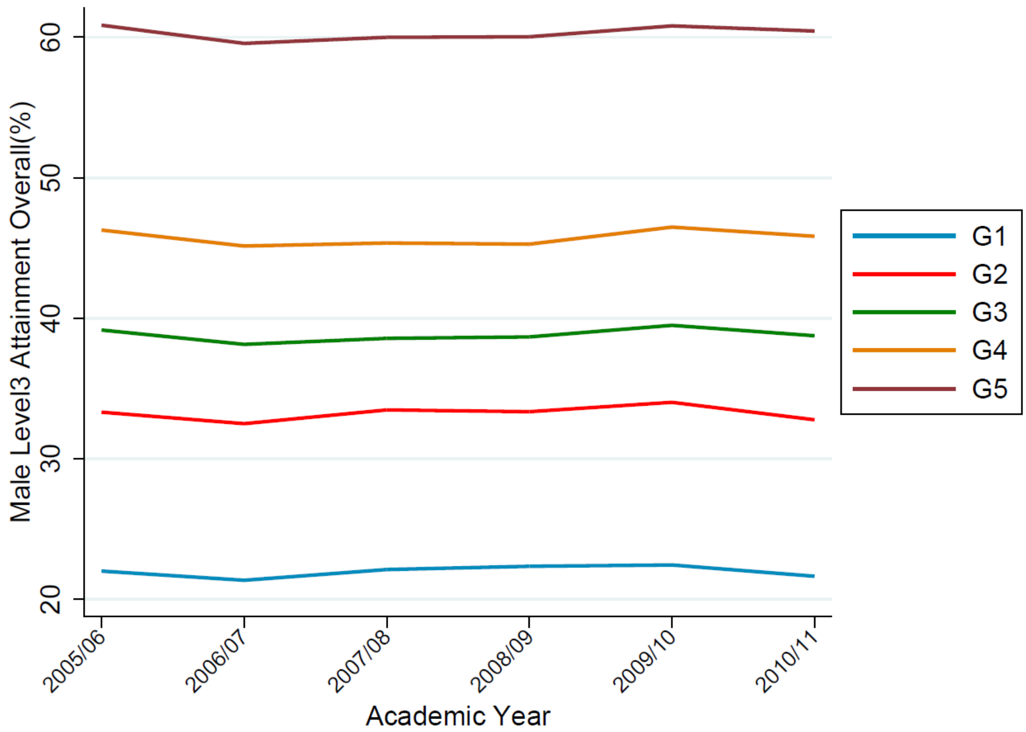

Figure consists of the mean Level 3 attainment rate for each group in the 6 pre-reform years added to the residuals from a regression of a Level 3 attainment dummy on a set of background characteristics and a linear trend for that group.

Figure 2.4.8: Pre-reform common trends in female Level 3 attainment

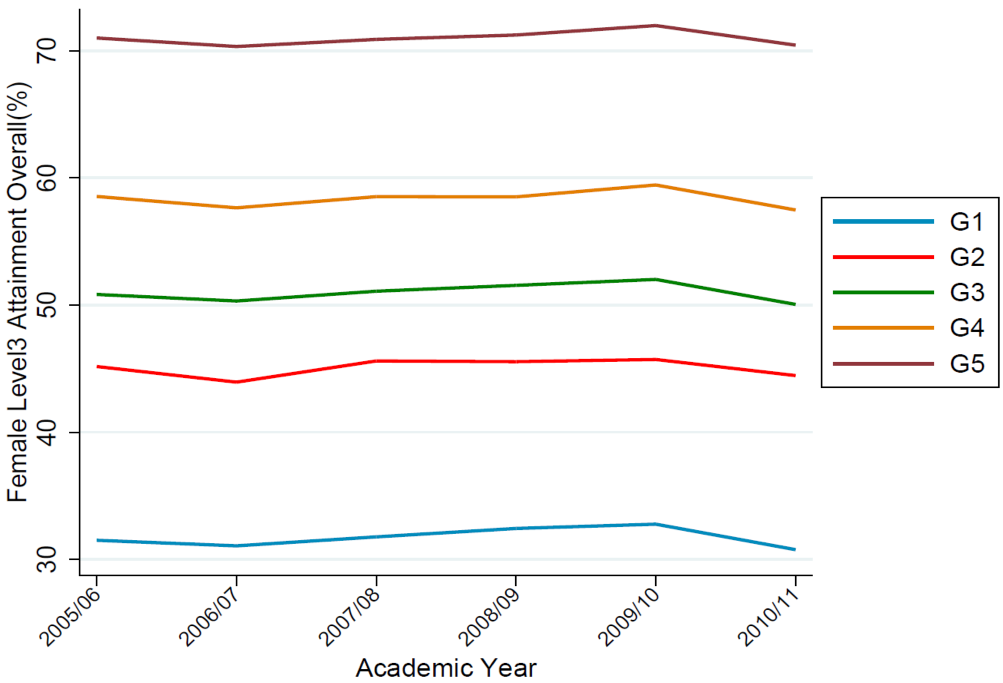

Figure consists of the mean Level 3 attainment rate for each group in the 6 pre-reform years added to the residuals from a regression of a Level 3 attainment dummy on a set of background characteristics and a linear trend for that group.

This analysis suggests that the proposed methodology is appropriate in order for us to assess the following key outcomes:

- Full-time (FT) participation in Year 12;
- Full-time (FT) participation in Year 13;
- Achievement of the Level 2 (L2) threshold by 18;[28]
- Achievement of the Level 3 (L3) threshold by 18.

In addition to these key outcomes, part-time education participation in Year 12 and Year 13, and specifically non-academic Level 2 and Level 3 attainment, are also of interest.[29] However, as seen in figures 2.4.9, 2.4.10, 2.4.11 and 2.4.12, the argument that the common trends assumption holds for each of these cases is weak. The graphs for males are presented, though the graphs for females are similar (they are available from the authors on request). Unfortunately these figures suggest that we could not be confident

[28] Each individual in the dataset (excluding the 2012/13 Year 12 cohort, who are excluded from the attainment analysis as they are not observed up to age 18) has a dummy variable set equal to one if they have achieved Level 2 by age 18 and zero if not. That includes individuals not in any form of education or in part-time education. The same applies for Level 3 attainment.

[29] The effect on part-time participation is ambiguous: it may decrease due to the reduced financial incentives, or it may increase if individuals substitute into part-time study in order to work at the same time to finance their studies. Meanwhile, non-academic qualifications are of interest because one might expect the effect to be stronger for this group than for individuals with academic qualifications. Individuals partaking in non-academic qualifications may be closer to the margin of accepting work in the first place. They are also likely to be lower income and therefore potentially more responsive to changes in financial incentives.

about the validity of our results if we were to use the DiD strategy to analyse these outcomes.

Figure 2.4.9: Pre-reform common trends in male part-time Y12 participation

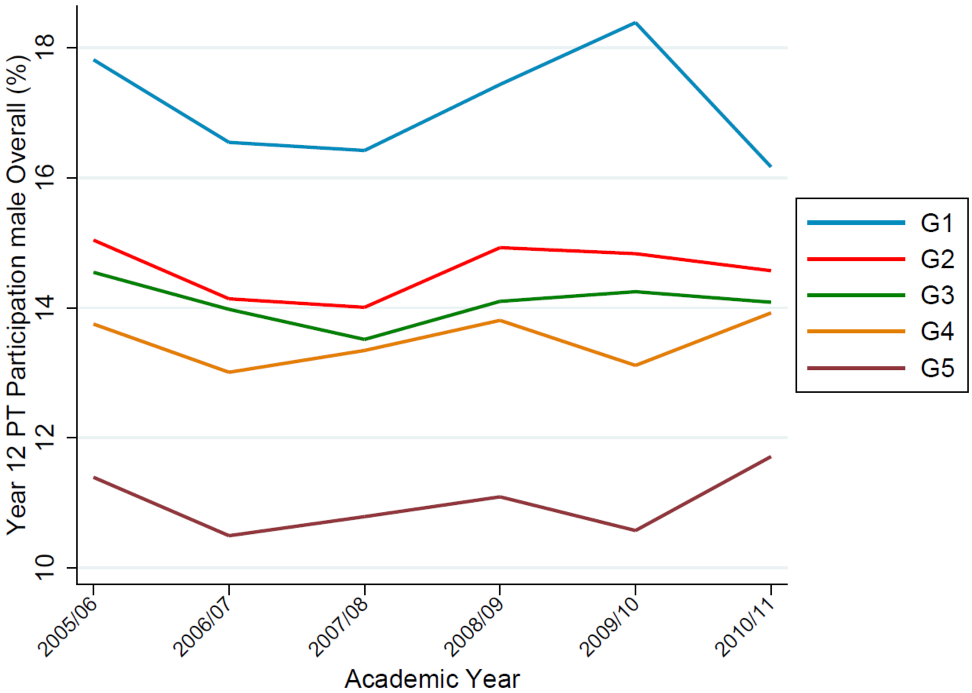

Figure consists of the mean Y12 PT participation rate for each group in the 6 pre-reform years added to the residuals from a regression of a Y12 PT participation dummy on a set of background characteristics and a linear trend for that group.

Figure 2.4.10: Pre-reform common trends in male part-time Y13 participation

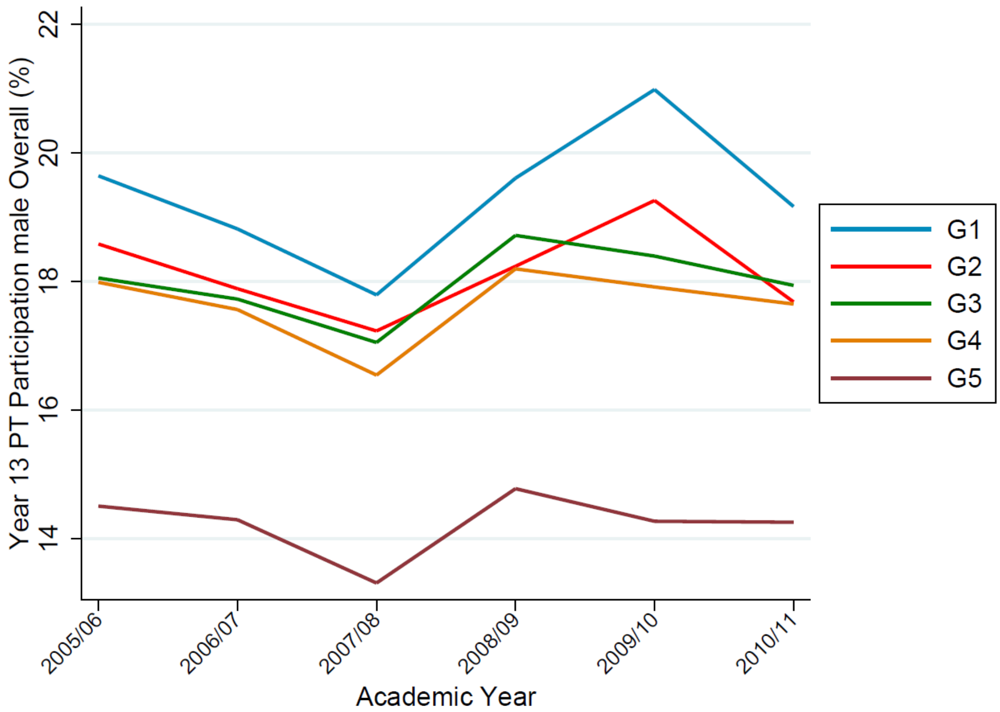

Figure consists of the mean Y13 PT participation rate for each group in the 6 pre-reform years added to the residuals from a regression of a Y13 PT participation dummy on a set of background characteristics and a linear trend for that group.

Figure 2.4.11: Pre-reform common trends in male Level 2 non-academic attainment

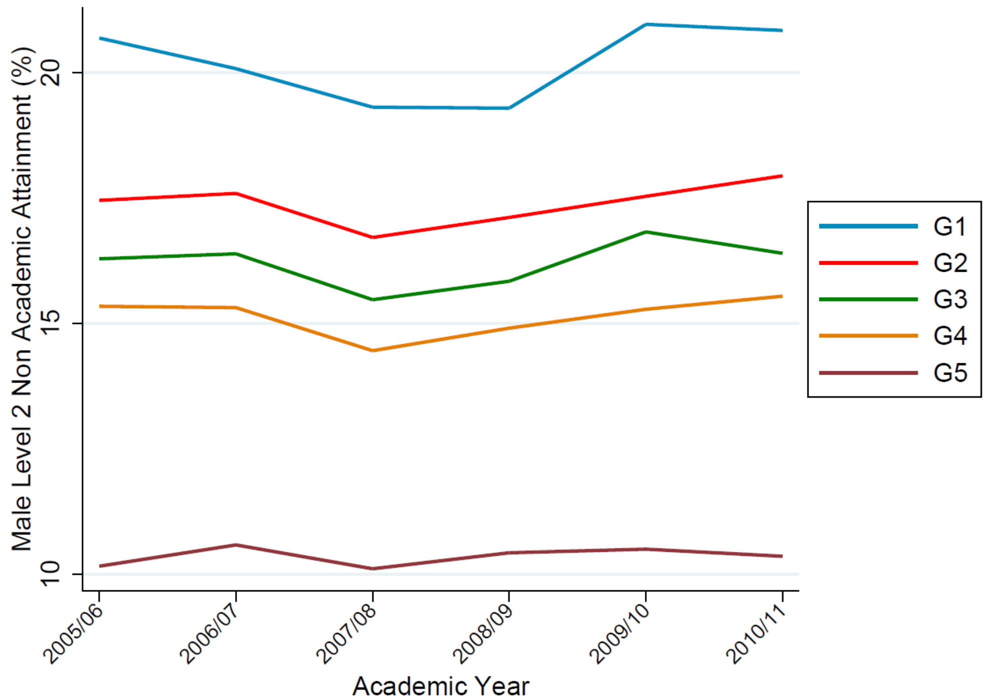

Figure consists of the mean Level 2 non-academic attainment rate for each group in the 6 pre-reform years added to the residuals from a regression of a Level 2 non-academic attainment dummy on a set of background characteristics and a linear trend for that group.

Figure 2.4.12: Pre-reform common trends in male Level 3 non-academic attainment

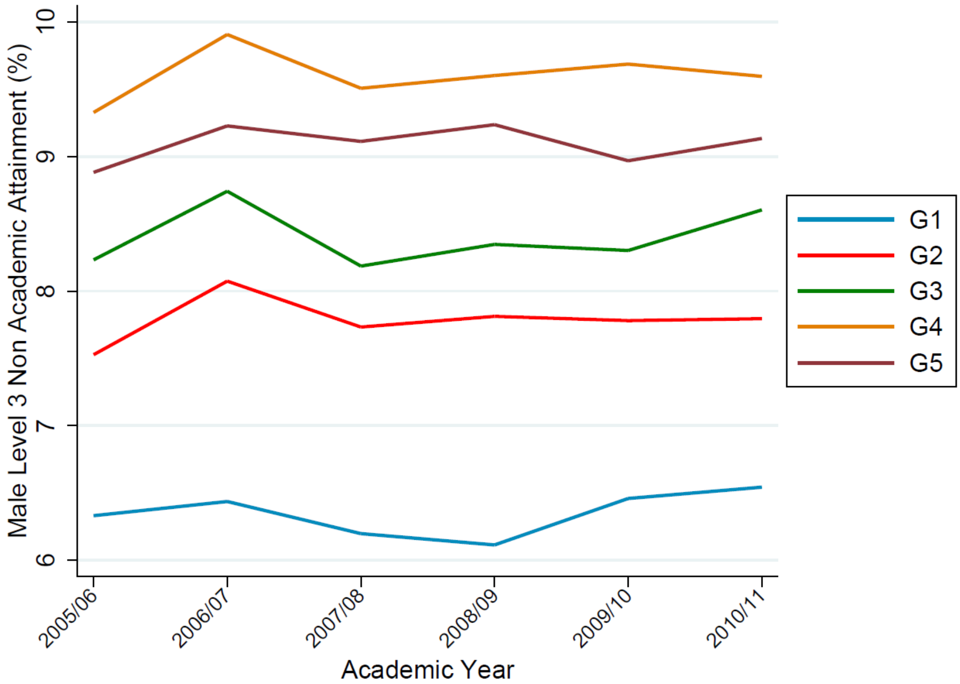

Figure consists of the mean Level 3 non-academic attainment rate for each group in the 6 pre-reform years added to the residuals from a regression of a Level 3 non-academic attainment dummy on a set of background characteristics and a linear trend for that group.

3. Research analysis findings

This section presents the estimates of the impacts of the 16 to 19 Bursary Fund, based on the empirical approach described previously. The impacts are averaged across the 2011/12 and 2012/13 academic years and are presented in percentage point terms. They should be interpreted as the effect on participation or attainment of replacing EMA with the 16 to 19 Bursary Fund, not the actual change in outcomes that happened over the period. For example, an impact of -0.5 percentage points (ppts) on FT participation means that FT participation was 0.5 ppts lower as a result of the reform; in absence of the reform to the EMA, participation would have been 0.5 ppts higher on average in 2011/12 and 2012/13. Following the main headline impacts, subgroup analysis is discussed. Finally, the possibility of different impacts in each of the two post-reform years is investigated.

3.1 Headline impact results on participation and attainment

The headline estimates are presented in Table 3.1.1. These figures show, for each outcome, the impact on each EMA eligibility group (1, 2 or 3), the average impact across Groups 1–3 and the average impact across the cohort as a whole. We are slightly concerned that our comparison group, Group 4, may be contaminated and include people who were receiving EMA. As a way of checking this, the table includes the impact of the change in policy on Group 5 compared with Group 4. Our hypothesis would be that this group should follow a similar trend to Group 4 after the policy reform, and therefore should not have differential post-reform effects to Group 4. This does not hold true in any of the four cases, however, as we observe significant positive coefficients on the Group 5 dummy. This suggests we may be underestimating the effects of changes in the policy for Groups 1, 2 and 3 using Group 4 as the control, as the use of Group 5 is likely to be less affected by the misclassification bias.

Table 3.1.1: Impact on participation and attainment

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
Group 1	-1.562**	-2.021**	-2.254**	-0.445*
	(0.213)	(0.245)	(0.142)	(0.223)
Group 2	-1.580**	-1.698**	-1.457**	-0.285
	(0.315)	(0.365)	(0.209)	(0.336)
Group 3	-0.937**	-0.556*	-0.571**	0.333
	(0.241)	(0.284)	(0.160)	(0.269)
Group 5	1.040**	0.772**	0.903**	0.741**
	(0.195)	(0.238)	(0.132)	(0.226)
Groups 1-3	-1.439**	-1.689**	-1.831**	-0.269
	(0.197)	(0.231)	(0.132)	(0.216)
Overall	-0.872**	-1.019**	-1.105**	-0.163
	(0.119)	(0.139)	(0.080)	(0.130)
Actual (%)	84.115	70.605	87.248	48.431
Counterfactual (%)	84.988	71.624	88.352	48.594
Sample size	4,616,181	4,655,863	4,655,863	4,655,863

Standard errors are given in the parentheses and are clustered at school level. *Indicates significant at 5%, ** indicates significant at 1%.

These results are derived from separate regressions for males and females, presented in Tables 3.2.1 and 3.2.2 below. According to our estimates, the reform reduced the Year 12 FT participation rate of pupils in Group 1 – those who would have been eligible for the full EMA award had it been available – by 1.6ppts, and this estimate is statistically significant at the 1% level.[30] The effect for Group 2 is also 1.6ppts, while the effect for Group 3 is about 0.9ppts. These are the groups who would have been eligible for the partial EMA. These estimates are statistically significantly different from zero, but not from each other (i.e. we would reject the hypothesis that the effects on Group 2 and Group 3 are different from each other).[31] Consequently the average impact across the three groups is estimated at -1.4ppts, which is statistically significant.[32]

An important point to consider when looking at the Y12 participation results is the positive, statistically significant coefficient on Group 5.[33] Were this group to be used

[30] This means that if the analysis were repeated many times with different populations, there would be less than a 1% chance of finding an impact of at least this magnitude if the 'true' effect were actually zero.

[31] It is also worth noting that allocation to Groups 1, 2 and 3 is also subject to the misclassification error.

[32] Since the data in question cover the full population of state school pupils, the interpretation of a standard error and associated confidence interval is more nuanced than it would be in a study involving a sample of students (e.g. from a survey). In this analysis there is no immediate sampling error since the data cover the full population of interest. However, the population that is observed could be thought of as drawn from a broader set of potential populations over a longer time period; in future years, the relevant population may look different. Moreover, even if the full population is observed, the standard errors around the point estimates also reflect the presence of an error term in the model. To the extent that it is impossible for these statistical models to perfectly fit the data, the impacts they provide are estimated with a certain amount of error.

[33] Given the suggestive evidence of common trends between Groups 4 and 5 presented in Figures 2.4.1 and 2.4.2, this finding is unexpected. There are two plausible explanations. First, we could be badly misallocating income, and Group 4 could include a number of individuals affected by the reform. This would suggest our estimated impact is conservative, as participation within our control group might have been negatively affected by the reform. Second, common trends between Groups 4 and 5 might have broken down between 2010/11 and 2011/12 due to a factor that differentially affected Groups 4 and 5. If this factor also differentially impacted Groups 1, 2 and 3, this represents a cause for concern. One candidate factor is the increase in University tuition fees from £3,000 to £9,000 per year that affected students starting University in 2012 onwards. For this to explain the observed effect, students would have to have opted out of education between ages 16 and 18 due to the increased cost of

instead of Group 4 as our control group, the magnitudes of the estimates for all groups would have increased by 1.0 ppt. This would involve an estimate of closer to -2.4 ppts for all three groups.

For Year 13 FT participation the estimated impact of the policy change (using Group 4 as the control group) is stronger and more concentrated on Groups 1 and 2; amongst Group 1, participation is estimated to have dropped by 2 ppts as a result of the policy change. This suggests that the low levels of EMA funding have less of an impact in the second year of study. The average impact across Groups 1-3 is -1.7 ppts, while the overall effect on the cohort is -1.0 ppts. All of these estimates are statistically significant. Again, the coefficient on Group 5 is positive and statistically significant, although it is smaller than for the Year 12 results. This suggests that the Year 12 and Year 13 impacts would be extremely similar in magnitude, were Group 5 used as the control group rather than Group 4.

It is important to note that we are unable to obtain reliable estimates of the part-time participation effect. To the extent that individuals might be substituting full-time participation for part-time participation after the policy change, this might reduce the overall negative participation effect. However, it is also possible that part-time participation decreased after the policy change and we are underestimating the true participation effect. This part-time mechanism does not affect the attainment regressions, however.

For attainment, the analysis estimates that among Group 1, the proportion who had achieved at least the L2 threshold by 18 fell by 2.3 ppts, with a corresponding fall across all groups potentially eligible for EMA of 1.8 ppts. As a result, the overall proportion of the cohort achieving the L2 threshold by 18 is estimated to be around 1.1 ppts below the counterfactual level, at 88.4%. If Group 5 rather than Group 4 were used as the control group, all these estimates would have increased by more than 0.9 ppts, again suggesting that the effects we are estimating may be conservative.

The overall impact on the L3 attainment rate is smaller and, for the most part, statistically insignificant. This suggests that individuals induced into non-participation (either not starting courses or dropping out) under the new policy were more likely to have otherwise had L2 study aims, rather than to have been the L3 margin. However the effect on Group 1 is an estimated drop of 0.4 ppts, which is significant at the 5% level. Further, were Group 5 used as the control group, our estimated effects would be 0.7 ppts larger, suggesting there is some impact at Level 3. It should be noted, though, that this latter

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University. This seems less likely considering the high labour market returns to post-16, pre-University qualifications (e.g see Conlon and Patrignani (2010)). Further, for the effect of this to bias the coefficient negatively, it would require participation to be more negatively affected in Group 4 than in Groups 1, 2 and 3 by the tuition change. This seems unlikely, suggesting that any bias on the estimates caused by the change in tuition fees would be towards zero. Thus in both cases, the positive coefficient on Group 5 suggests the effect on participation is being underestimated in this setting.

observation appears to be driven almost entirely by girls, as the male Group 5 coefficient is effectively zero (see Tables 3.2.1 and 3.2.2 below).

3.2 Subgroup analysis

The results in Table 3.1.1 are derived from separate regressions by gender. Splitting the main analysis by gender in this way is common in this area, and follows the approach used in Dearden et al., 2008. It was considered necessary not only because the underlying participation levels for males and females are known to be different, but also because it was deemed likely that there might be different underlying trends and different effects of other background characteristics. The separate regression results by gender are given in Tables 3.2.1 and 3.2.2. In practice, the tables show that the effects are very similar for males and for female, in terms of both participation and attainment, suggesting the reform had little impact on the gender participation gap (participation is lower amongst men).

Table 3.2.1: Impact on participation and attainment, males

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
Group 1	-1.557**	-2.134**	-2.376**	-0.497
	(0.31)	(0.36)	(0.21)	(0.31)
Group 2	-1.445**	-1.510**	-1.473**	0.124
	(0.46)	(0.54)	(0.31)	(0.48)
Group 3	-0.871*	-0.870*	-0.578*	0.103
	(0.35)	(0.42)	(0.24)	(0.38)
Group 5	1.468**	0.794*	1.170**	0.108
	(0.29)	(0.35)	(0.20)	(0.32)
Groups 1-3	-1.410**	-1.814**	-1.919**	-0.313
	(0.29)	(0.34)	(0.20)	(0.30)
Overall	-0.857**	-1.099**	-1.163**	-0.190
	(0.17)	(0.21)	(0.12)	(0.18)
Actual (%)	82.207	67.731	84.607	43.322
Counterfactual (%)	83.064	68.830	85.769	43.512
Sample Size	2,349,739	2,367,316	2,367,316	2,367,316

Standard errors are given in the parentheses and are clustered at school level. *Indicates significant at 5%, ** indicates significant at 1%.

Table 3.2.2: Impact on participation and attainment, females

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
Group 1	-1.567**	-1.904**	-2.128**	-0.391
	(0.29)	(0.33)	(0.19)	(0.32)
Group 2	-1.720**	-1.892**	-1.440**	-0.708
	(0.43)	(0.49)	(0.28)	(0.47)
Group 3	-1.006**	-0.232	-0.563**	0.570
	(0.33)	(0.38)	(0.21)	(0.38)
Group 5	0.596*	0.749*	0.626**	1.395**
	(0.26)	(0.32)	(0.17)	(0.32)
Groups 1-3	-1.470**	-1.559**	-1.739**	-0.224
	(0.27)	(0.31)	(0.18)	(0.31)
Overall	-0.888**	-0.937**	-1.045**	-0.135
	(0.16)	(0.19)	(0.11)	(0.19)
Actual (%)	86.093	73.578	89.979	53.715
Counterfactual (%)	86.982	74.514	91.024	53.850
Sample Size	2,266,442	2,288,547	2,288,547	2,288,547

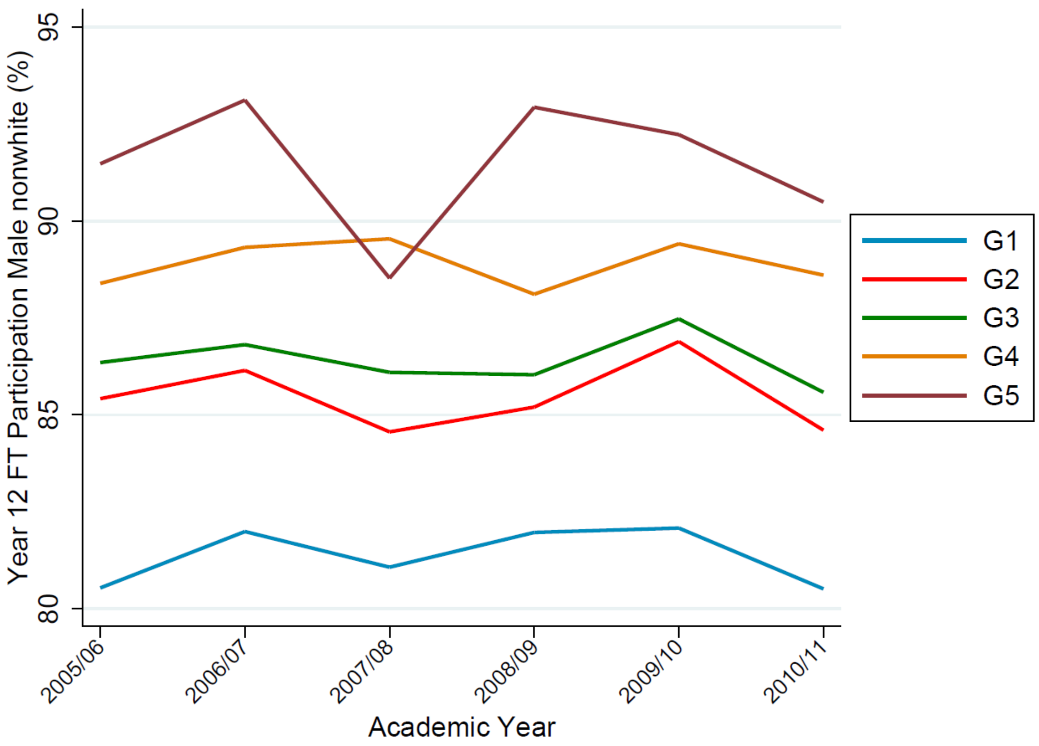
Standard errors are given in the parentheses and are clustered at school level. *Indicates significant at 5%, ** indicates significant at 1%.

There are a number of additional sub-populations of interest, such as various ethnic groups, on students with statements of Special Educational Needs, students whose first language is not English, or on individuals from more deprived backgrounds. In this section we investigate these subgroups. Before estimating the effects on participation or attainment it is again necessary to investigate the validity of the common trends assumption in each case.

Figure 3.2.1 does this for 'non-white' students in isolation. Though this is a very broad definition of ethnicity, we observe that the common trends assumption does not hold for this group when looking at male Year 12 participation. This observation holds for females and for other outcomes of interest.[34] For the finer ethnic groups, the lack of common trends is even starker. This finding is consistent with the subgroup analysis in the interim report (Britton et al., 2014), which found inconsistent effects in terms of both magnitude and sign.

[34] Figures 3.2.1–3.2.5 are constructed in the same way as the figures presented in Section 2.4.

Figure 3.2.1: Pre-reform common trends in male non-white Y12 participation



Figures 3.2.2 - 3.2.5 investigate the common trends assumption for English as Additional Language students, students with statements of Special Educational needs, “deprived” (where a deprived student is someone in the bottom quintile of the IDACI distribution - though a similar result holds using IMD as the deprivation index) and individuals from London. In each of these cases there is insufficient evidence that the common trends result holds.[35] This means that we are unable to obtain robust estimates of the effect of the policy on these subgroups.

[35] Again, the underlying trends for males in Year 12 are presented, though the conculsions hold for other outcomes of interest and for females. Further common trends graphs are available on request from the authors.

Figure 3.2.2: Pre-reform common trends in male EAL students' Y12 participation

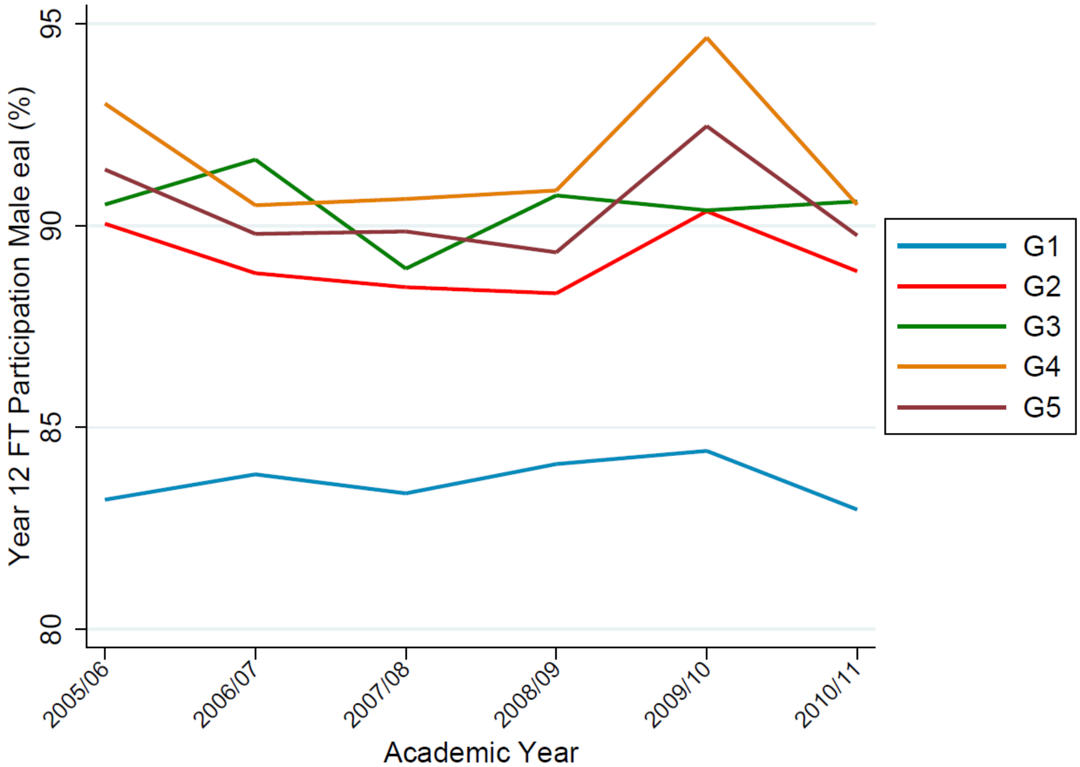


Figure 3.2.3: Pre-reform common trends in male SEN students' Y12 participation

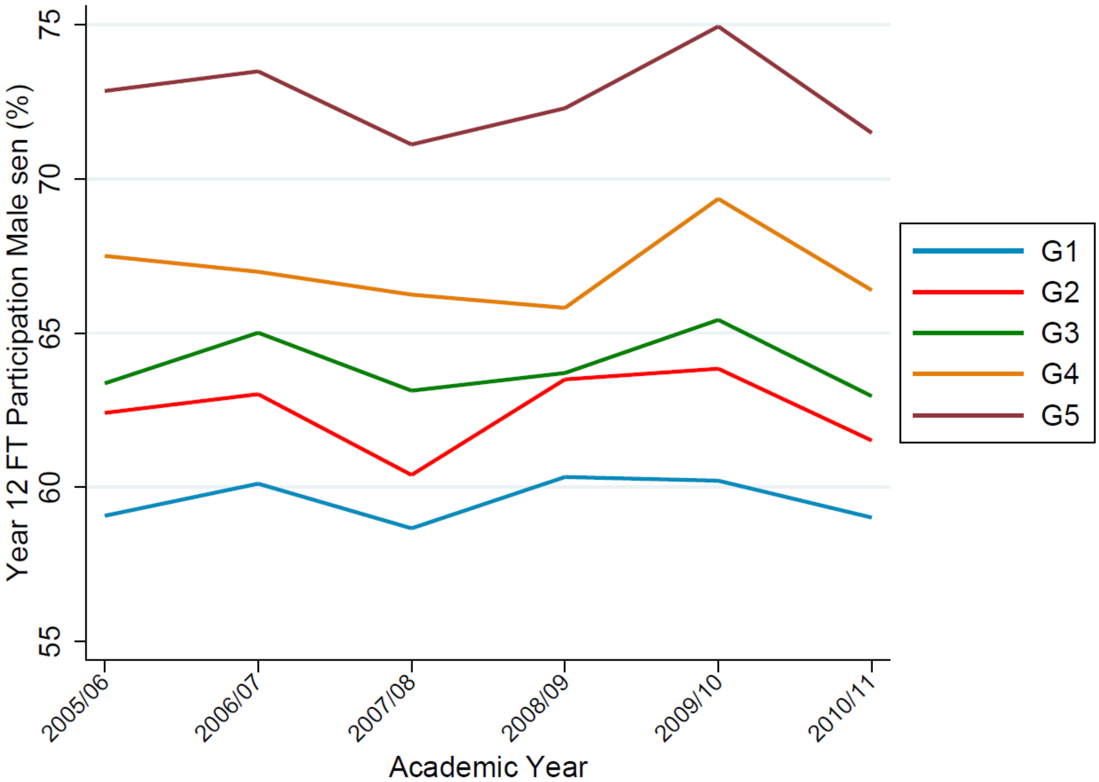


Figure 3.2.4: Pre-reform common trends in male low IDACI students' Y12 participation

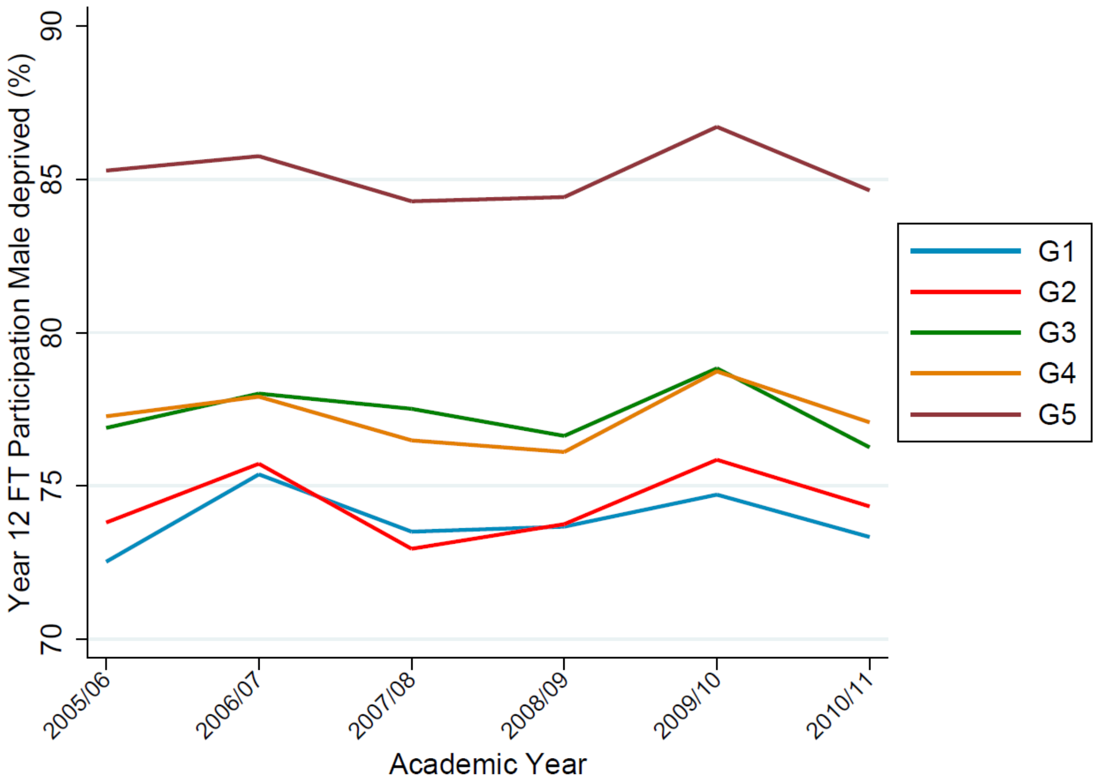
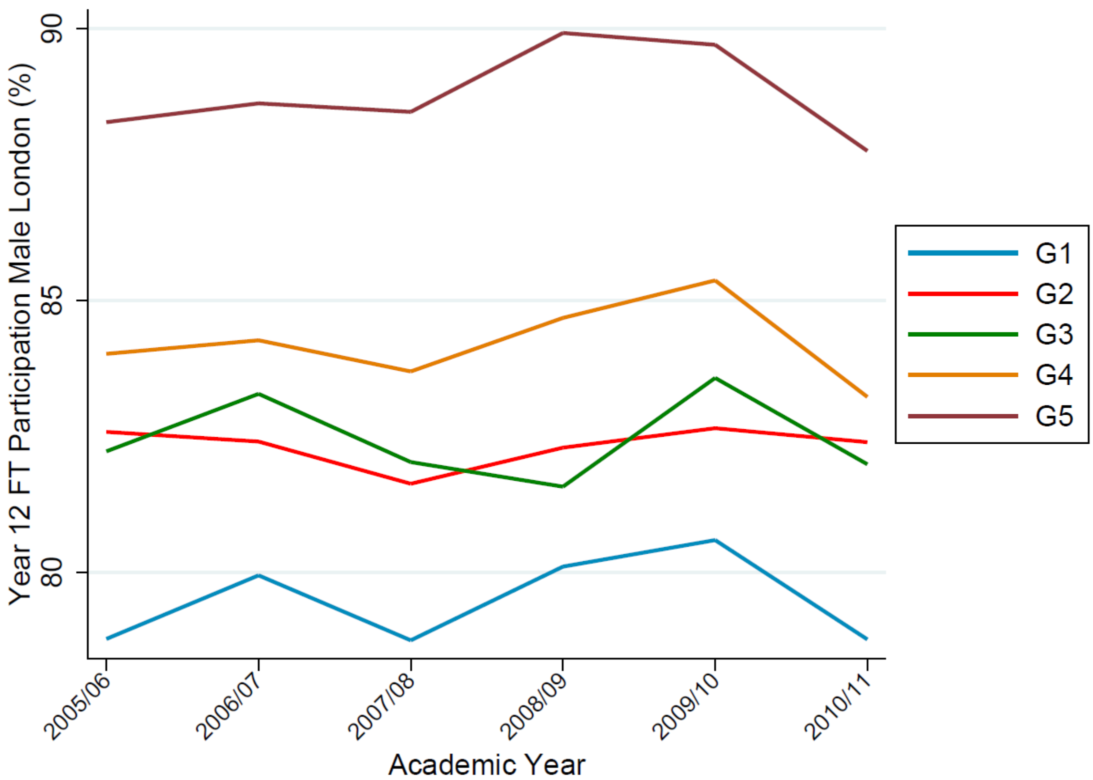


Figure 3.2.5: Pre-reform common trends in London male students' Y12 participation



3.3 Investigating the post-reform years separately

We have two years of post-reform data available, from the 2011/12 and 2012/13 academic years. This allows the possibility of investigating separate post-reform effects. It might be the case, for example, that the 16 to 19 Bursary had a different effect in year two than in year one on Year 13 participation because 2011/12 was a transition year where eligibility was slightly different for Year 13s than it would be in subsequent years. Year two is also the first year when a cohort will have been exposed to the Bursary policy for both Year 12 and Year 13; there may be some knock-on impacts for later study from non-participation in the first non-compulsory year. Alternatively there might be a rebound effect as individuals initially take a year out to save money before returning, or attempt to find employment and return upon failing to do so. We investigate this by modifying the regression specification used in Sections 3.1 and 3.2 by including two separate post-reform dummies instead of having one pooled post-reform dummy. This is again done separately by gender, with the results presented for males in Tables 3.3.1 and for females in Table 3.3.2.

Table 3.3.1: Male participation and attainment, separate post-reform year dummies

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
Group 1 Y1	-1.176**	-2.293**	**-1.952****	-0.760*
	(0.33)	(0.39)	**(0.22)**	(0.33)
Group 1 Y2	-2.207**	-1.721**	**-2.944****	0.079
	(0.40)	(0.45)	**(0.26)**	(0.40)
Group 2 Y1	-0.868	-1.236*	-1.107**	0.096
	(0.50)	(0.60)	(0.35)	(0.54)
Group 2 Y2	-2.717**	-1.277*	-1.415**	0.639
	(0.58)	(0.64)	(0.36)	(0.57)
Group 3 Y1	-0.755	-1.108*	-0.475	-0.205
	(0.39)	(0.47)	(0.26)	(0.42)
Group 3 Y2	-1.091*	-0.273	-0.535	0.789
	(0.44)	(0.51)	(0.28)	(0.48)
Group 5 Y1	1.535**	0.344	0.974**	-0.028
	(0.32)	(0.38)	(0.21)	(0.35)
Group 5 Y2	1.327**	1.696**	1.632**	0.469
	(0.37)	(0.44)	(0.23)	(0.41)
Sample Size	2,349,739	2,367,316	2,367,316	2,367,316

Standard errors are given in the parentheses and are clustered at school level. *Indicates significantly different from zero at 5%, ** at 1%. Bold indicates the statistically significantly different effects.

Table 3.3.2: Female participation and attainment, separate post-reform year dummies

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
Group 1 Y1	-1.539**	-1.675**	**-1.759****	-0.314
	(0.31)	(0.36)	**(0.20)**	(0.35)
Group 1 Y2	-1.652**	-2.244**	**-2.638****	-0.422
	(0.36)	(0.42)	**(0.23)**	(0.41)
Group 2 Y1	-1.626**	-1.283*	-0.913**	-0.072
	(0.46)	(0.55)	(0.30)	(0.53)
Group 2 Y2	-1.899**	-2.423**	-1.686**	-1.077
	(0.54)	(0.59)	(0.33)	(0.58)
Group 3 Y1	-1.039**	-0.132	-0.411	0.685
	(0.37)	(0.42)	(0.22)	(0.42)
Group 3 Y2	-1.010*	-0.327	-0.647*	0.539
	(0.41)	(0.48)	(0.26)	(0.48)
Group 5 Y1	0.627*	0.863*	0.541**	1.524**
	(0.29)	(0.35)	(0.18)	(0.34)
Group 5 Y2	0.503	0.599	0.870**	1.277**
	(0.33)	(0.39)	(0.20)	(0.41)
Sample Size	2,266,442	2,288,547	2,288,547	2,288,547

Standard errors are given in the parentheses and are clustered at school level. *Indicates significantly different from zero at 5%, ** at 1%. Bold indicates the statistically significantly different effects.

There is some evidence of a differential effect across the two years. However, although there appears to be a stronger participation effect in the second year in many cases, the vast majority of the effects are not statistically significantly different from one another. The exceptions (both highlighted in bold in the tables) are Level 2 attainment amongst the most deprived groups, for both males and females. In both cases the Level 2 attainment effect is smaller in the first, transitional year, which suggests the true effect of the 16 to 19 Bursary on attainment may be larger than our reported average effects.

4. Robustness

In this section we run three tests of the robustness of our results. In Section 4.1 we run a more formal test of the common trends assumption by testing for an effect using 2010/11 as the placebo treatment year. In Section 4.2, we test the sensitivity of our results to the exclusion of data from 2005/06. Finally, we investigate the sensitivity of our results to changing the definition of our income groups. In all three cases, the results of these tests support our overall findings.

4.1 DiD analysis on a placebo treatment year

As discussed previously, for a difference-in-difference analysis to be valid, the common trends assumption must hold. In this case, the key identifying assumption is that trends in education participation would be the same in Groups 1, 2 and 3 as in Group 4, were it not for the policy change, once underlying trends and group composition are taken into account. The figures presented in Section 2.4 provide suggestive evidence that this assumption does indeed hold. However in this section we investigate the common trends assumption more formally by running a placebo difference-in-difference test with 2010/11 as the treatment year. This means using exactly the same specification as in the main results in Section 3, but with 2011/12 and 2012/13 dropped from the analysis, and 2010/11 treated as the post-reform year. The linear trend is therefore estimated using the 2005/06–2009/10 period in this case. If the common trends assumption holds, we would not expect to get significant estimates in this test. This is because a significant result indicates a divergence of the trend. This would be reasonable after the reform – we interpret significant differences in this case as the effect of the reform – but not prior to the reform, when we assume that the trends are parallel once underlying linear trends and the effects of background characteristics are removed.

The results from this test are presented in Table 4.1.1 for males, and Table 4.1.2 for females. The tables show that the coefficients are considerably smaller in magnitude than the corresponding effects reported in Tables 3.2.1 and 3.2.2, and that of the 32 estimated coefficients, only six are significantly different from zero. We feel this is sufficient evidence in favour of our assumption of common trends,[36] though we acknowledge that these results highlight that there is probably additional uncertainty in the estimates that is not accounted for in the standard errors (which are constructed on the assumption that the common trends assumption holds).

[36] Uncertainty in the estimation process means that in expectation, 1 in 20 zero coefficients would be estimated as being significantly different from zero. Although six coefficients is perhaps more than one would expect through natural variation, the overall results are still favourable to our assumptions.

Table 4.1.1: Effect on participation and attainment with 2010/11 as placebo treatment year, males

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
Group 1	-0.631*	-0.415	-0.276	-0.622*
	(0.25)	(0.30)	(0.20)	(0.24)
Group 2	-0.684	-0.706	-0.262	-0.713
	(0.42)	(0.45)	(0.31)	(0.37)
Group 3	-0.325	-0.066	-0.183	-0.173
	(0.32)	(0.36)	(0.25)	(0.30)
Group 5	-0.163	0.011	0.184	0.081
	(0.27)	(0.31)	(0.22)	(0.26)
Sample Size	1,787,577	1,788,360	1,788,360	1,788,360

Standard errors are given in the parentheses and are clustered at school level. *Indicates significant at 5%, ** indicates significant at 1%.

Table 4.1.2: Effect on participation and attainment with 2010/11 as placebo treatment year, females

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
Group 1	-0.675**	-0.310	-0.379*	-0.202
	(0.23)	(0.29)	(0.19)	(0.26)
Group 2	-1.104**	-0.059	-0.034	0.341
	(0.39)	(0.44)	(0.29)	(0.39)
Group 3	-0.596*	-0.085	0.094	0.004
	(0.30)	(0.35)	(0.23)	(0.31)
Group 5	-0.036	0.136	0.151	0.388
	(0.25)	(0.30)	(0.20)	(0.27)
Sample Size	1,726,729	1,731,331	1,731,331	1,731,331

Standard errors are given in the parentheses and are clustered at school level. *Indicates significant at 5%, ** indicates significant at 1%.

An additional check using 2009/10 as the placebo treatment year is found to yield very similar results (these results are available from the authors).

4.2 Exclusion of 2005/06

In this section we test the robustness of our results to the exclusion of 2005/06 from the estimation. Results that are highly sensitive to the removal of years from the analysis would not be very reassuring about the assumptions regarding the underlying linear trend in participation. Results with male and female effects pooled together are presented in Table 4.2.1. The results are generally smaller in magnitude than those in Table 3.1.1, though they are not substantively different. This again is favourable for the reliability of our overall results.

Table 4.2.1: Overall effect on participation and attainment, 2005/06 excluded

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
Group 1	-1.131**	-1.623**	-1.973**	-0.094
	(0.23)	(0.27)	(0.16)	(0.24)
Group 2	-1.311**	-1.489**	-1.249**	-0.160
	(0.34)	(0.40)	(0.23)	(0.37)
Group 3	-0.612*	-0.437	-0.482**	0.410
	(0.26)	(0.31)	(0.18)	(0.30)
Group 5	1.018**	0.825**	0.792**	0.842**
	(0.21)	(0.26)	(0.14)	(0.25)
Groups 1-3	-1.048**	-1.373**	-1.606**	0.000
	(0.21)	(0.25)	(0.14)	(0.24)
Overall	-0.635**	-0.829**	-0.969**	0.000
	(0.13)	(0.15)	(0.10)	(0.14)
Actual (%)	84.114	70.603	87.246	48.427
Counterfactual (%)	84.749	71.432	88.215	48.427
Sample Size	4,035,779	4,075,221	4,075,221	4,075,221

Standard errors are given in the parentheses and are clustered at school level. *Indicates significant at 5%, ** indicates significant at 1%.

4.3 Sensitivity to group bandwidths

In this section we investigate the sensitivity of our results to the income band definitions. Previously, Group 3 was defined to include individual with incomes between £25,522 and £35,000. Here, the £30,000–£35,000 group is dropped from Group 3 and excluded from the analysis altogether. The results are presented in Table 4.3.1, which shows that the main results are highly insensitive to this assumption: the estimates are very similar to those in Table 3.1.1.

Table 4.3.1: Overall effect on participation and attainment, £30k-£35k excluded

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
Group 1	-1.568**	-2.015**	-2.249**	-0.444*
	(0.21)	(0.25)	(0.14)	(0.22)
Group 2	-1.588**	-1.696**	-1.456**	-0.290
	(0.32)	(0.37)	(0.21)	(0.34)
Group 3	-1.223**	-0.516	-0.600**	0.268
	(0.27)	(0.32)	(0.19)	(0.30)
Group 5	1.035**	0.768**	0.904**	0.730**
	(0.20)	(0.24)	(0.13)	(0.23)
Groups 1-3	-1.522**	-1.768**	-1.931**	-0.327
	(0.20)	(0.23)	(0.13)	(0.22)
Overall	-0.899**	-1.040**	-1.136**	-0.192
	(0.12)	(0.14)	(0.08)	(0.13)
Actual (%)	84.096	70.584	87.141	48.311
Counterfactual (%)	84.995	71.624	88.277	48.503
Sample Size	4,415,717	4,435,289	4,435,289	4,435,289

Standard errors are given in the parentheses and are clustered at school level. *Indicates significant at 5%, ** indicates significant at 1%.

5. Cost–benefit analysis

In this section we assess the costs and benefits of the policy, primarily for the exchequer, of replacing the EMA with the 16 to 19 Bursary. The benefits are generally in the short run, as the government accrues savings today through the budget reduction. In section 5.1 the participation and attainment effects estimated in section 3 are used frame those short-run savings in terms of savings per student dropping out of education and per student attaining a given qualification level. In section 5.2 the long-run costs of the policy are assessed through the investigation of the long-run earnings impact for individuals, which may imply an effect on economic output, and the subsequent loss to the exchequer through reduced tax receipts and increased benefit expenditure.

5.1 The short-run savings from replacing the EMA with the 16 to 19 Bursary

Government savings associated with the policy change are in the short run, through expenditure savings, and through the reduced cost of education as fewer people attend. In 2010/11, spending on the EMA in England was an estimated £564 million,[37] while spending on the 16 to 19 Bursary in 2011/12 was £180 million, according to figures provided by the Department for Education. Assuming the same amount would have been spent on the EMA in 2011/12 had it not been replaced[38] this is a saving of £384 million to the government (in 2011 prices).

In addition to this, as seen in Section 3, education participation in Year 12 and Year 13 dropped as a result of the policy. Since there is a cost associated with educating each participating individual, this will result in an additional saving to the government under the 16 to 19 Bursary. The magnitude of this saving is calculated below. First, Table 5.1.1 shows the estimated effect on Year 12 and Year 13 participation, as well as the L2 and L3 attainment effects. The central estimates are repeated from Table 3.1.1 while the upper and lower bounds represent each end of the 95% confidence intervals around those estimates. The estimated impacts using Group 5 as the control group are also given.

[37] See www.parliament.uk/briefing-papers/SN05778.pdf

[38] This seems a reasonable assumption given that EMA grants were not linked to inflation.

Table 5.1.1: Range of percentage point impacts across all EMA-eligible individuals

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
MAIN SPECIFICATION: Group 4 as control				
Upper bound	-1.826	-2.140	-2.089	-0.693
Central estimate	-1.439	-1.689	-1.831	-0.269
Lower bound	-1.053	-1.237	-1.572	0.154
ALTERNATIVE SPECIFICATION: Group 5 as control				
Upper bound	-3.906	-3.684	-3.894	-2.174
Central estimate	-2.479	-2.461	-2.733	-1.010
Lower bound	-2.093	-2.009	-2.475	-0.586

Second, the change in the number of individuals participating in Year 12 and Year 13 as a result of the policy is given in Table 5.1.2, alongside the change in the number of individuals achieving L2 and L3 qualifications. The central, main specification estimates 9,867 fewer individuals participating in Year 12 or Year 13, 5,898 fewer individuals achieving L2, and 868 fewer individuals achieving L3. With the alternative specification, using Group 5 as the control group, the central estimates are 15,551 fewer individuals participating, 8,807 fewer individuals attaining L2, and 3,254 fewer individuals achieving L3.

Table 5.1.2: Headcount impacts on participation and attainment

	Full Time Y12 Participation	FT Year 13 Participation	Level 2 Attainment	Level 3 Attainment
MAIN SPECIFICATION: Group 4 as control				
Upper bound	-5,614	-6,897	-6,730	-2,213
Central estimate	-4,425	-5,441	-5,899	-868
Lower bound	-3,237	-3,986	-5,067	497
ALTERNATIVE SPECIFICATION: Group 5 as control				
Upper bound	-12,008	-11,879	-12,547	-7,005
Central estimate	-7,623	-7,929	-8,807	-3,254
Lower bound	-6,434	-6,473	-7,976	-1,890

Headcount impacts are the estimated number of individuals not participating, or not achieving L2/L3 qualifications as a result of the policy. They are equal to the corresponding EMA-eligible percentage point impact multiplied by the EMA-eligible cohort size. Because our headline impacts are the average effect across two years of the policy, we use average EMA-eligible cohort size across the two years as the cohort size.

Third, these participation estimates are multiplied by the estimated average cost of educating each individual, per year. Our best estimate of this is £4,975 per pupil.[39] This figure is multiplied by the total participation effect (for Y12 and Y13 combined)[40] and added to the estimated expenditure savings in Table 5.1.3 to give the total estimated savings to the government associated with the policy.

[39] Estimate supplied by DfE based on assumption-based modelling of full-time funding costs. This is the average of their 2011/12 and 2012/13 estimates (which are themselves very similar).

[40] The cost per pupil is multiplied by 0.965 in the second year to reflect the discounting of the future by 3.5%.

Table 5.1.3: Government savings (£m)

	Financial Support	Cost of Education Provision	Total
MAIN SPECIFICATION: Group 4 as control			
Upper bound	384	61.0	445.0
Central estimate	384	48.1	432.1
Lower bound	384	35.2	419.2
ALTERNATIVE SPECIFICATION: Group 5 as control			
Upper bound	384	116.7	500.7
Central estimate	384	76.0	460.0
Lower bound	384	63.1	447.1

Savings are in £ millions and are reported in 2011 prices. These are the estimated average savings to the government per year associated with the 16 to 19 Bursary being in place instead of the EMA.

For our main specification, total government savings associated with having the 16 to 19 Bursary in place instead of the EMA are therefore estimated to be £432 million per year. The upper and lower bounds on this estimate are £445 million and £419 million respectively. Under the alternative specification with Group 5 as the control, government savings are estimated to be £460 million per year, with a lower bound of £447 million, and an upper bound of £501 million. The savings are larger under the alternative specification because more people dropped out of education as a result of the policy under this scenario.

It is important to note, however, that part-time figures are excluded from this analysis because we do not have a reliable estimate of the part-time participation effect. Since the part-time effect is ambiguous – lost financial support might mean fewer participants through the same mechanism as the effect on full-time participation, or it might mean more participants as people switch to part-time in order to work whilst studying – it is not possible to say whether this contributes to us underestimating or overestimating the true effect.

Using the figures given in Tables 5.1.2 and 5.1.3, it is possible to estimate changes in the total number of individuals participating and the total number of individuals attaining L2 or L3 qualifications per £1 million saved by the government. These estimates are given in Table 5.1.4.

Table 5.1.4: Headcount impacts per £1m saved

	Y12 + Y13 Participation	L2 Overall	Level 3 Overall
MAIN SPECIFICATION: Group 4 as control			
Upper bound	-28.1	-15.1	-5.0
Central estimate	-22.8	-13.6	-2.0
Lower bound	-17.2	-12.1	1.2
ALTERNATIVE SPECIFICATION: Group 5 as control			
Upper bound	-47.7	-25.1	-14.0
Central estimate	-33.8	-19.1	-7.1
Lower bound	-28.9	-17.8	-4.2

Using 2011 prices.

For our main specification, the central impact estimate is of 23 fewer individuals participating in education, 14 fewer L2 achievers and 2 fewer L3 achievers per £1 million saved. Under the alternative specification, the central estimates are of 34 fewer individuals in education, 19 fewer L2 achievers and 7 fewer L3 achievers per £1 million saved. These figures do not take into account the long-run costs associated with the policy, however. These are investigated in the following section.

5.2 The long-run costs of replacing the EMA with the 16 to 19 Bursary

The analysis in Section 3 indicated a negative impact of switching from the EMA to the 16 to 19 Bursary on both participation and attainment. In this section we consider how the effect on attainment might translate into long-term economic losses for the individual and for the government.

Our methodology closely follows Cattan, Crawford and Dearden (2014). First, we estimate the highest education level an individual will achieve by age 21 under the policy (the 16 to 19 Bursary) and under the policy counterfactual (keeping the EMA). There are four possible education levels: less than Level 2, Level 2 (at least 5 A*-C GCSEs or equivalent), Level 3 (at least 2 A Levels or equivalent) and Level 4 (University degree). Second, we simulate 10,000 lifetime earnings paths for each highest education qualification level and gender. For each path, we also simulate cohabitation and childbirth, as well as additional household income. Third, we run the 10,000 simulated profiles through the Institute for Fiscal Studies’s model of taxes and benefits (henceforth, “TAXBEN”) in each period, taking into account other household income, marital status and number of children present, converting gross earnings into net earnings. Fourth, we randomly assign an earnings path to every individual in the NPD sample in Year 13 in 2011/12 or 2012/13. Using this, we report (the discounted present value of) average individual lifetime earnings and average individual tax contributions under both policies. Because earnings are dependent on the highest education qualification of the individual, a negative shift in the highest-education distribution caused by moving from the EMA to the 16 to 19 Bursary should therefore translate into lower lifetime earnings on average, and lower tax receipts for the exchequer.

5.2.1 Estimating the highest education qualification

As outlined above, the first step in the process is to estimate the highest-education distribution for individuals under the EMA and under the 16 to 19 Bursary. In practice this involves estimating for each individual a probability that they will achieve a certain level by age 21, and taking the average probability for each level across all individuals under each scenario.

The probabilities are estimated in two stages. In the first, we replicate the approach used in Section 3 and use a difference-in-difference approach to estimate regression models looking at the effect of moving from the EMA to the Bursary on Level 2 and Level 3 attainment separately. For the cohort of individuals who would have been in Year 13 in 2012/13, we use these models to predict the probability of the individual achieving Level 2 or Level 3 by age 18 under both policy scenarios. The residual is the probability of achieving Level 1.[41]

In the second stage, we use the LSYPE to estimate the probability of individuals achieving a particular level at 21, given their education level at 18.[42] We do this by merging the LSYPE to the NPD so that we have individuals' qualifications at age 18 (from the NPD) and at age 21 (from the LSYPE). We then take the probability of an individual transitioning from a given level at 18 to any other at 21 as equal to the proportion of individuals in the LSYPE sample who made that transition. We fragment the sample by FSM eligibility, gender and ethnicity (white versus non-white) so that the transition probabilities vary across these groups. We then multiply the probability of an individual achieving a given level by 18 by the transition probability and sum across all possible routes to get the final probability. So, for example the probability of an individual achieving Level 4 by 21 is equal to:

$$P(L4_{21}) = P(L1_{18}) * P(L4_{21}|L1_{18}) + P(L2_{18}) * P(L4_{21}|L2_{18}) + P(L3_{18}) * P(L4_{21}|L3_{18})$$

The average of these probabilities across all individuals for each level gives us our highest-education distributions. These are shown under both the EMA and the 16 to 19 Bursary in Table 5.2.1. The education distributions are shifted to the left under the 16 to 19 Bursary compared to the EMA, in that a higher proportion are at Level 1, and a lower proportion are at all of the other three levels. This is true both for boys and for girls, although girls on average achieve better qualifications than boys under both scenarios.

We estimate the distribution both with Group 4 as a control group (as in the main specification regressions) and with Group 5 as a control group, which we treat as an upper bound on our estimates. With Group 5 the pattern in the differences between the policies remains the same, though the difference is now more pronounced. This aligns with the regression results in Section 3, where we observed a significant positive coefficient on the Group 5 dummy in the Level 2 regressions. The estimates in Table 5.2.1 all suggest that moving from the EMA to the 16 to 19 Bursary would be associated with a small drop in the proportion of individuals attaining Level 3 and Level 4 qualifications by age 21. However, the biggest changes are at the Level 1/Level 2 threshold, which again aligns with our previous findings.

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[41] This approach is simply an alternative way of estimating the model used in Section 3, the main results, and the results are exactly equivalent: it is simply a way of using the analysis to get a probability distribution of attaining different education qualifications for each individual under both policies.

[42] This assumes the relationship between qualifications by 18 and qualifications at age 21 is not affected by the policy. This appears to be the safest assumption in this context.

Table 5.2.1: Estimated highest-education distribution by age 21 under the 16 to 19 Bursary and under the EMA for individuals in Groups 1-3

	(A) EMA	(B) 16 to 19 Bursary	(C) Difference between (A) and (B)
MAIN SPECIFICATION: GROUP 4 AS THE CONTROL GROUP			
Probability for males of attaining each final educational level			
Level 1 (< 5 GCSEs A*–C or equivalent)	0.155	0.174	0.019
Level 2 (5 GCSEs A*–C or equivalent)	0.316	0.306	-0.011
Level 3 (A levels)	0.197	0.193	-0.004
Level 4 (University degree)	0.331	0.327	-0.004
Probability for females of attaining each final educational level			
Level 1	0.098	0.116	0.017
Level 2	0.286	0.276	-0.010
Level 3	0.211	0.208	-0.003
Level 4	0.405	0.401	-0.004
ALTERNATIVE SPECIFICATION: GROUP 5 AS THE CONTROL GROUP			
Probability for males of attaining each final educational level			
Level 1	0.145	0.175	0.030
Level 2	0.326	0.308	-0.018
Level 3	0.194	0.188	-0.005
Level 4	0.334	0.328	-0.006
Probability for females of attaining each final educational level			
Level 1	0.093	0.116	0.023
Level 2	0.282	0.277	-0.005
Level 3	0.210	0.205	-0.006
Level 4	0.415	0.402	-0.013

Individuals in Groups 1, 2 and 3 would have been eligible for EMA had it not been abolished.

5.2.2 Simulating lifetime earnings

To estimate the long-run impact of the policy on individuals' lifetime earnings and on the exchequer, we use a model of employment and earnings to simulate lifetime earnings of the individual. The model uses a combination of the British Household Panel Survey (BHPS) and the Labour Force Survey (LFS) to estimate the dynamics of earnings and the probability of employment and unemployment. This model has been used by researchers at the Institute for Fiscal Studies in a number of contexts, including the analysis of Higher Education finance (Crawford and Jin, 2014) and of a pre-school education intervention (Cattan, Crawford and Dearden, 2014). It is described in full detail in Appendix A.3.

In the model, earnings and employment through the life cycle are dependent on the highest education qualification (Level 1, 2, 3 or 4) and gender of the individual.

Individuals in Levels 1-3 start earning at age 19, whilst individuals with Level 4 start earning at age 22. All individuals can work until age 60.[43] In general, the earnings trajectories are steeper and reach a higher level for higher-educated individuals, and are higher on average for males than for females.

In Table 5.2.2 we present the discounted present value (DPV) of lifetime earnings under the EMA, and under the 16 to 19 Bursary. This value is averaged across all individuals in Groups 1-3 (i.e. individuals who would have been eligible for the EMA) and across all education groups. We follow the Green Book[44] and set the discount rate at 3.5% for the first 30 years, and at 3% for the next 25 years. Although earnings do not begin until age 19 at the earliest in the model, we start discounting from age 16, as this is the age at which the government would have to make the funding available for the EMA.

We model earnings at both the individual and the household level. The latter requires us to include in the model a probability (conditional on age and education) that an individual will become married, and a probability that an individual will have a child (conditional on age, education and marital status) in each period. These probabilities are taken from the BHPS. Our estimated net present value of earnings at the household level thus includes all net earnings of the household of the individual over the life cycle; i.e. there is no division of household income between spouses. For this reason the DPV of lifetime earnings is larger at the household level than at the individual level in Table 5.2.2.

The advantages of modelling at the household level are twofold. First, it incorporates the fact that changes to individuals' education may influence their marital and fertility decisions into the model. Second, it improves our estimate of exchequer returns (see Section 5.2.3), as the tax and benefit system is in practice highly dependent on family situation. In our calculations below, we allow for the fact that two individuals from the same household may both be affected by the policy.

Finally, as in the previous section, we repeat the analysis using Group 5 as the control group rather than Group 4. As seen in Table 5.2.2, with Group 4 as the control group the loss in DPV of lifetime earnings associated with moving from the EMA to the 16 to 19 Bursary is £4,097 when estimated at the individual level and £6,735 when estimated at the household level. Using Group 5 as the control group, the equivalent figures are £7,042 and £11,330. These larger figures are a consequence of the larger shift in the education distribution associated with the policy change when Group 5 is used as the control group.

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[43] This will potentially produce conservative impacts for this cohort, although earnings at this point are heavily discounted, thus muting the impact of this somewhat.

[44] HM Treasury (2011), *The Green Book: Appraisal and Evaluation in Central Government* (https://www.gov.uk/government/publications/the-green-book-appraisal-and-evaluation-in-central-governent).

Table 5.2.2: Estimated average discounted present value (DPV) of gross lifetime earnings under the 16 to 19 Bursary and under the EMA for individuals in Groups 1-3

	(A) EMA	(B) 16 to 19 Bursary	(C) Difference between (A) and (B)
MAIN SPECIFICATION: GROUP 4 AS THE CONTROL GROUP			
Discounted present value of gross lifetime earnings at the individual level			
Individual-level average DPV of lifetime earnings (£)	£638,463 (1,540)	£634,367 (1,556)	-£4,097 (509.8)
Discounted present value of gross lifetime earnings at the household level			
Household-level average DPV of lifetime earnings (£)	£1,391,497 (1,548)	£1,384,761 (1,372)	-£6,735 (721.9)
ALTERNATIVE SPECIFICATION: GROUP 5 AS THE CONTROL GROUP			
Discounted present value of gross lifetime earnings at the individual level			
Individual-level average DPV of lifetime earnings (£)	£641,204 (1,610)	£634,162 (1,556)	-£7,042 (401.4)
Discounted present value of gross lifetime earnings at the household level			
Household-level average DPV of lifetime earnings (£)	£1,395,815 (1,493)	£1,384,485 (1,374)	-£11,330 (565.1)

Individuals in Groups 1, 2 and 3 would have been eligible for EMA had it not been abolished. Figures in the parentheses are bootstrapped standard errors, from 1000 repetitions. Figures are in September 2014 prices.

5.2.3 Estimating the exchequer's tax receipts

In this section we calculate the NPV of average tax receipts per individual in Groups 1-3 accrued by the exchequer under the EMA and under the 16 to 19 Bursary. This exercise is essential for calculating the costs and benefits associated with the policy. We do this by running the earnings profiles estimated for each individual through TAXBEN, a model of the 2012/13 UK tax and benefit system.[45] In our individual-level model, we assume that the individual never marries or has children, and lives on his/her own throughout the lifecycle. In the household-level model we allow individuals to marry and to have children, as described in the previous section. We again estimate the average NPV of lifetime tax receipts using both Group 4 and Group 5 as the control group separately.

Table 5.2.3 shows that at the individual level, with Group 4 as the control group, the average tax receipts per individual are £1,334 lower under the 16 to 19 Bursary than under the EMA. At the household level this figure is £2,762. When Group 5 is used as the control group these figures are increased to £2,367 and £4,857 per individual respectively.

[45] We assume that the tax and benefit system stays unchanged (in real terms) through the lifecycle. While this is clearly unrealistic, attempting to predict future changes to the system would be of limited value.

Table 5.2.3: Estimated discounted present value (DPV) of exchequer receipts under the 16 to 19 Bursary and under the EMA for those in Groups 1-3

	(A) EMA	(B) 16 to 19 Bursary	(C) Difference between (A) and (B)
MAIN SPECIFICATION: GROUP 4 AS THE CONTROL GROUP			
Discounted present value of exchequer receipts at the individual level			
Average DPV of exchequer receipts per individual in £	£279,325 (645.6)	£277,991 (610.4)	-£1,334 (212.2)
Discounted present value of exchequer receipts at the household level			
Average DPV of exchequer receipts per individual in £	£696,226 (859.7)	£693,464 (789.18)	-£2,762 (351.1)
ALTERNATIVE SPECIFICATION: GROUP 5 AS THE CONTROL GROUP			
Discounted present value of exchequer receipts at the individual level			
Average DPV of exchequer receipts per individual in £	£280,270 (630.5)	£277,903 (610.4)	-£2,367 (167.3)
Discounted present value of exchequer receipts at the household level			
Average DPV of exchequer receipts per individual in £	£698,175 (841.2)	£693,319 (789.7)	-£4,857 (276.7)

Individuals in Groups 1, 2 and 3 would have been eligible for EMA had it not been abolished. Figures in the parentheses are bootstrapped standard errors, from 1000 repetitions. Figures are in September 2014 prices.

5.3 Summarising the costs and benefits

The loss in DPV of lifetime tax receipts associated with moving from the EMA to the 16 to 19 Bursary should be compared to the savings associated with the policy. As seen in Table 5.1.3, the estimated total annual savings associated with the move were £432 million, in 2011 prices. This is £459 million in September 2014 prices, which we use for the comparison with the costs. The total costs equal the loss to the exchequer per individual multiplied by the number of affected individuals, of which there are 322,237.[46]

The savings and costs are summarised in Table 5.3.1. We again produce results from calculations at both the individual and the household level. To calculate the population-level long-run costs for the individual level, we simply multiply the average individual effect by the number of EMA-eligible individuals in the cohort. This approach is inappropriate when looking at the household level, however, as if two affected individuals were to cohabit, that household would be counted twice. To avoid this, we have to make an assumption about the proportion of individuals who cohabit with other individuals

[46] This is the average number of individuals in Year 13 across 2011/12 and 2012/13. This figure is appropriate because the assessment of the costs involves looking at Level 2 and Level 3 attainment by the end of Year 13. It is appropriate to use the average of the two cohort sizes because we pool the post-reform years to get an average effect across the two years in the analysis.

affected by the policy. In our model, individuals spend 64% of the time married or cohabiting. Assuming all cohabitations are between individuals affected by the policy,[47] the average household-level cost should be multiplied by 0.61.[48] Calculations in Table 5.3.1 use this scaling factor.

Table 5.3.1: Estimated overall exchequer costs and benefits of the policy (£m)

	Savings (£m)	Costs (£m)	Savings – Costs (£m)
MAIN SPECIFICATION: GROUP 4 AS THE CONTROL GROUP			
Individual Level	£458.9	£429.9	£29.1
Household Level	£458.9	£542.6	-£83.7
ALTERNATIVE SPECIFICATION: GROUP 5 AS THE CONTROL GROUP			
Individual Level	£488.5	£762.8	-£274.2
Household Level	£488.5	£954.3	-£465.8

Using central estimates of savings. All figures are in September 2014 prices.

Using Group 4 as the control group, the individual-level simulations result in an estimated saving to the government of approximately £29 million. However, our best estimate is to consider the household-level calculations, which result in a loss of approximately £84 million, in September 2014 prices. This means that the discounted present value of lost tax receipts under the 16 to 19 Bursary caused by reduced lifetime earnings exceeds the short-run savings associated with the policy. When Group 5 is used as the control group, we estimate a loss to the exchequer of £274 million at the individual level and £466 million at the household level. This highlights the sensitivity of the results to a small change in the estimated impact on attainment, and suggests that the underestimation of the effect due to misclassification error is likely to lead to a large underestimation of the true long-run costs to the government.

As discussed above, omitting the analysis of part-time participation does affect the benefits side of our calculation. However it does not affect the costs, as our analysis of the costs is driven entirely by the attainment regression results, for which part-time individuals are included. This means that the scope for part-time participation to affect the overall conclusions of the cost–benefit analysis is limited.

[47] This is a conservative assumption, as one would expect at least some of the cohabiting to be with individuals unaffected by the policy.

[48] If 100% were cohabiting 100% of the time, we should multiply the average household level cost by 1/(1+1) = 0.5. If individuals are cohabiting 64% of the time, we should multiply the average household level cost by 1/(1+0.64) = 0.61.

6. Conclusion

The analysis in this report uses statistical techniques to provide estimates of the impact of replacing EMA with the 16 to 19 Bursary Fund. The headline impacts indicate that the reform led to an average 1.6 ppt fall across the 2011/12-2012/13 academic years in FT participation amongst Year 12 students who would otherwise have been eligible for the full EMA award. The effect on those eligible for a partial EMA are smaller, resulting in an estimated overall effect on those eligible for any form of EMA of -1.4 ppts, or -0.9 ppts across the whole cohort. For Year 13 participation, there was an average 2.0 ppt fall in FT participation among the poorest students who would have previously been eligible for the full EMA. Overall, there was a statistically significant fall in FT participation of 1.7 ppts across all EMA-eligible Year 13 pupils and 1.0 ppts across the whole cohort.

The findings for attainment suggest that the impacts were the most negative among the poorest students: among those who would have been eligible for the full EMA award, there was a 2.3 ppts fall in the L2 achievement rate, leading to a 1.1 ppts fall across the whole Year 13 cohort. This is a perhaps surprising finding as the intention of the Bursary was to more effectively target those most in need of the award. The effects on L3 attainment were considerably smaller, and with the exception of the group who would have been eligible for the full grant, insignificantly different from zero. There was no evidence of a strong differential effect for girls and for boys, and no clear evidence of differing effects by other sub-groups.

Importantly, it is likely that the overall impact estimates presented in this report underestimate the true impacts of the policy reform in question. Nevertheless, our cost–benefit analysis estimates that the long-run costs from the policy because of reduced tax receipts outweigh the short-run savings from expenditure cuts, and that is without taking into account wider impacts including the effects on productivity, crime or health.

Of course, the analysis does not account for the economic benefits that accrue from alternative investments of the short-run savings. Those alternatives should be examined with a similar approach to that used here in order to assess their relative cost-effectiveness.

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Appendix A: Econometric methodology

A.1 Regression specifications

A.1.1 Main results

The statistical models used in this analysis are all least squares regression models (since the outcome indicators are all binary, they are also linear probability models). The estimating equation for the headline impact analysis is as follows:

$$y_{it} = \alpha + v_s + \beta X_{it} + \gamma_g D_g + \lambda P_t + \delta_g(D_g \cdot P_t) + \eta t + \theta_g(D_g \cdot t) + \varepsilon_{it} (1)$$

- y_{it} is the participation or attainment outcome for pupil i at time t;
- α is an intercept;
- v_s is the school fixed effect;
- X_{it} is the set of characteristics for pupil i at time t controlled for in the model. These are gender, ethnicity, SEN status, EAL status, IDACI quintile, IMD quintile, KS2 average point score (entered linearly), KS4 capped point score (entered linearly), a dummy for whether the individual had achieved Level 2 by age 16 and the neighbourhood proportion of residents with a Level 4/5 qualification, obtained from the 2001 Census (entered linearly);
- D_g is a set of income group indicators (the comparison group, Group 4, being the omitted category);
- P_t is an indicator for the post-reform period, i.e. 2011/12-2012/13;
- $D_g \cdot P_t$ is an interaction term which identifies the outcomes observed among Group g in the post-reform period 2011/12-2012/13. The coefficients on this interaction term, δ_g, are the impact estimates;
- t is an aggregate linear trend in the outcome (effectively the trend for the comparison group);
- $D_g \cdot t$ is a separate linear trend for Group g;
- ε_{it} is an error term representing all other unmeasured influences on y_{it} for pupil i at time t.

For the gender-specific regressions, the above specification is used for each gender separately, with the gender dummy variable omitted from the right-hand side. In all the models, the standard errors are clustered at the school (in Year 11) level. This assumes that ε_{it} is independent across schools, but can be correlated across different pupils in the same school, and can also be correlated over time within the same school.

A.1.2 Investigating the post-reform years separately

In Section 3.3 we investigate the effects of the post-reform years separately. The specification used is as follows:

$$y_{it} = \alpha + v_s + \beta X_{it} + \gamma_g D_g + \sum_{j=1}^{2} \left(\lambda_j P_t^j + \delta_{jg} \left(D_g \cdot P_t^j \right) \right) + \eta t + \theta_g \left(D_g \cdot t \right) + \varepsilon_{it} \qquad (2)$$

This is exactly as in (1), but there are now two post-reform dummies, P_t^1 and P_t^2, and the coefficients δ_{1g} and δ_{2g} are the coefficients of interest.

A.1.3 Difference-in-difference placebo tests

In section 4.1, as a test of our common trends assumption, we investigate using a pre-treatment year as the placebo year. For this, the specification is exactly as in (1), except that P_t is now a dummy variable set equal to one for 2010/11. The 2011/12-2012/13 data is not used in these regressions.

A.2 Bootstrapped standard errors

A potential area of concern is the fact that an individual's income group is only estimated in our process as we do not directly observe income. The standard errors from the main specification regressions assume that individuals are accurately allocated to income groups and therefore do not incorporate this additional degree of uncertainty. We investigate the validity of the standard errors (which will affect the statistical significance of our findings) by recalculating them using a bootstrap technique. It is important to note that this does not deal with the misclassification bias, but instead should in theory incorporate the additional uncertainty into the standard errors.

The technique involves taking a subset of all individuals included in the estimation and re-estimating the entire model on that subset alone. This process includes the re-estimation of an individual's income group using their percentile rank in the socio-economic status (SES) distribution created using principal components analysis. Note that an individual may be allocated to two different income groups in consecutive iterations of the bootstrap despite having identical characteristics, purely due to a change in the composition of the subsample for the principal components analysis, which is used to allocate individuals an income (and hence income group). The impact regressions are each re-estimated with each repetition, with variation across subsamples creating variation in the regression coefficients. The standard errors are then equal to the standard deviations of these regression coefficients.

This process is found to make extremely little difference to our results. Due to the fact that the process is so highly computationally burdensome, bootstrapped standard errors are not reported in the tables given in this report (Tables 5.2.2 and 5.2.3 excepted).

A.3 Earnings and employment dynamics model

A.3.1 Earnings model

The model for log earnings (y_{iat}) for individual i at age a in year t is:

$$y_{iat} = \beta X_{iat} + \hat{y}_{iat}$$

$$\hat{y}_{iat} = \alpha_i + \gamma_i a + u_{iat} + z_{iat}$$

$$u_{iat} = \epsilon_{iat} + \theta \epsilon_{i,a-1,t-1}$$

$$z_{iat} = \rho z_{i,a-1,t-1} + \eta_{iat}$$

$$z_{i0t} = 0$$

$$\epsilon_{i0t} = 0$$

where X_{iat} is a vector of observable characteristics for individual i that includes a quartic polynomial in age, a full set of year dummies, and dummies for region and ethnicity. α_i is an individual-specific fixed effect and γ_i is an individual-specific deterministic linear trend in age. Together, α_i and γ_i allow for cross-sectional heterogeneity in both the level and age-profile of the deterministic component of earnings. The idiosyncratic stochastic component comprises two parts: z_{iat} is a first-order autoregressive persistent shock and u_{iat} is a first-order moving-average transitory shock. We allow the variances of both shocks, ϵ_{iat} and η_{iat}, to be quadratic functions of age and we allow the autoregressive parameter, ρ, to be a cubic function of age. The moving-average parameter, θ, is assumed to be fixed across ages.

The model parameters are estimated separately for male and for female graduates using the British Household Panel Survey (BHPS). Estimation takes place in three stages:

1. Regress log earnings on the observed characteristics X_{iat} and store the residuals as $\hat{y}_{iat}$.
2. Calculate the sample auto-covariance function of the residuals $\hat{y}_{iat}$ at each age for up to 10 lags. This generates a set of estimated auto-covariances, $Cov(y_a, y_{a-d})$ for $d = 0, \dots, 10$.
3. Choose the parameters of the earnings model to minimise the distance between the sample auto-covariance function and the theoretical auto-covariance function implied by the model. Each element of the auto-covariance function is weighted by $n_{a,d}^{0.5}$, where $n_{a,d}^{0.5}$ is the number of observations that were used in the construction of the sample auto-covariance at age a and lag d.

In total, 374 moments were used in the estimation for university graduates and 407 moments were used in the estimation for non university graduates.

A.3.2 Models for annual employment

We define an individual to be non-employed in year t if they are observed to have annual earnings less than £1,000 in that year. We estimate three models for employment dynamics: the probability of moving from employment to non-employment, the probability of moving from non-employment to employment, and the annual earnings of re-employed workers.

Exit to non-employment

The probability of a currently employed worker becoming non-employed is assumed to be a probit model with age and log earnings as independent variables. Age enters as a quartic polynomial. Log earnings enter as a quadratic polynomial.

Entry to employment

The probability of a previously non-employed worker becoming employed is assumed to be a probit model with age and duration of non-employment as independent variables. Age enters as a quartic polynomial. Duration enters as dummy variables for up to one year, one to two years and more than two years.

Re-entry earnings

Log earnings of a previously non-employed worker are assumed to be a function of age, duration of non-employment and last log annual earnings before becoming non-employed. Age enters as a quartic polynomial. Duration enters as dummy variables for up to one year and more than one year. Last log annual earnings enter linearly.

A.3.3 Simulating the BHPS model for earnings and employment

The estimated earnings and employment models are simulated alongside each other, using the simulated earnings as inputs to determine both the probability of becoming non-employed and the re-entry earnings upon re-employment. The only thing that remains to be specified is how the stochastic (random) component of earnings upon re-employment is divided between the persistent and transitory components. This is done differently for males and females. For males, it is assumed that the transitory component is equal to the stochastic component of the re-entry earnings equation; the persistent component is equal to the remainder. For females, it is assumed that the persistent component is a weighted average of the persistent component as just described for males, and a random draw from the unconditional distribution of the persistent component (assuming full employment) at the relevant age; the weights used are 0.35 on the former and 0.65 on the latter. These specifications were chosen because they were found to generate employment patterns and re-entry earnings distributions that match the BHPS well at each age.

To generate a simulated series for raw earnings from the simulated series for logs, we first add back the estimated quartic age profile from the first-stage regression. Next we randomly assign each simulated individual to a region–ethnicity group, according to the observed region–ethnicity distribution. We then add back the relevant region–ethnicity constants. Finally, we add back the intercept term that corresponds to the year effect for the most recent year (2008) and exponentiate log earnings to obtain raw earnings.

A.3.4 Adjusting for consistency with the LFS

The final step is to adjust the cross-sectional distributions of non-zero earnings to be consistent with the observed cross-sectional distributions of non-zero earnings in the Labour Force Survey (LFS). To do this, we calculate the following percentiles of the log-earnings distribution in the LFS at each age:, 1, 2, 3, 4, 5, 10, 15, 20, 25, 30, 35, 40, 45, 50, 55, 60, 65, 70, 75, 80, 85, 90, 95 and 99. Each percentile is smoothed across ages using a five-point moving average.

For each simulated log-earnings realisation, we calculate its rank in the simulated distribution at that age. We then re-assign it the corresponding log earnings from the smoothed percentiles in the LFS, using linear interpolation to evaluate ranks that lie between the percentiles listed above. Two things should be noted. First, non-employed simulations (i.e. those with zero earnings) are not affected by this transformation; hence, the fraction of people employed at each age is left unchanged. Second, since annual earnings in the LFS are calculated as weekly earnings multiplied by 52, it is likely that the LFS overstates earnings in the bottom parts of the distribution, due to the presence of part-year workers.

A.3.5 Goodness-of-fit of the earnings and employment model

It is very important that the model delivers a good fit of the data since we rely heavily on its predictions to compute the effect of pre-school quality on lifetime earnings. Figures A.3.1 and A.3.2 compare the data with the predictions of the model on several dimensions of earnings and employment. In the interests of space, we only report such goodness-of-fit exercises for male and female university graduates, but the patterns are similar for the other educational categories.

Figure A.3.1: Goodness-of-fit of the earnings and employment model for male university graduates

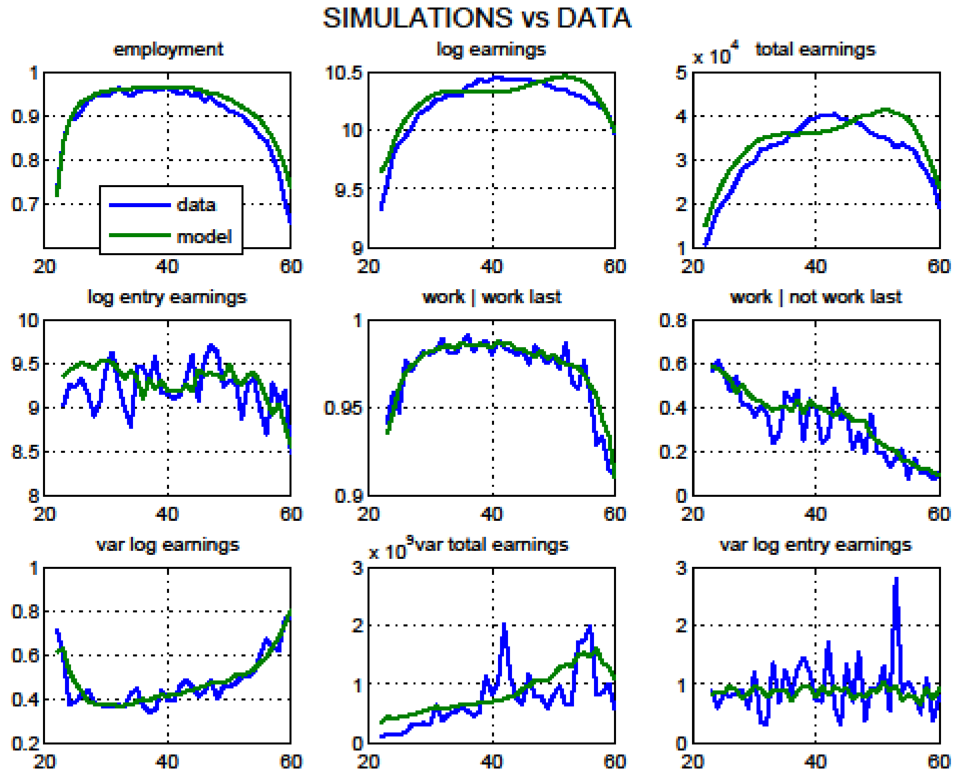
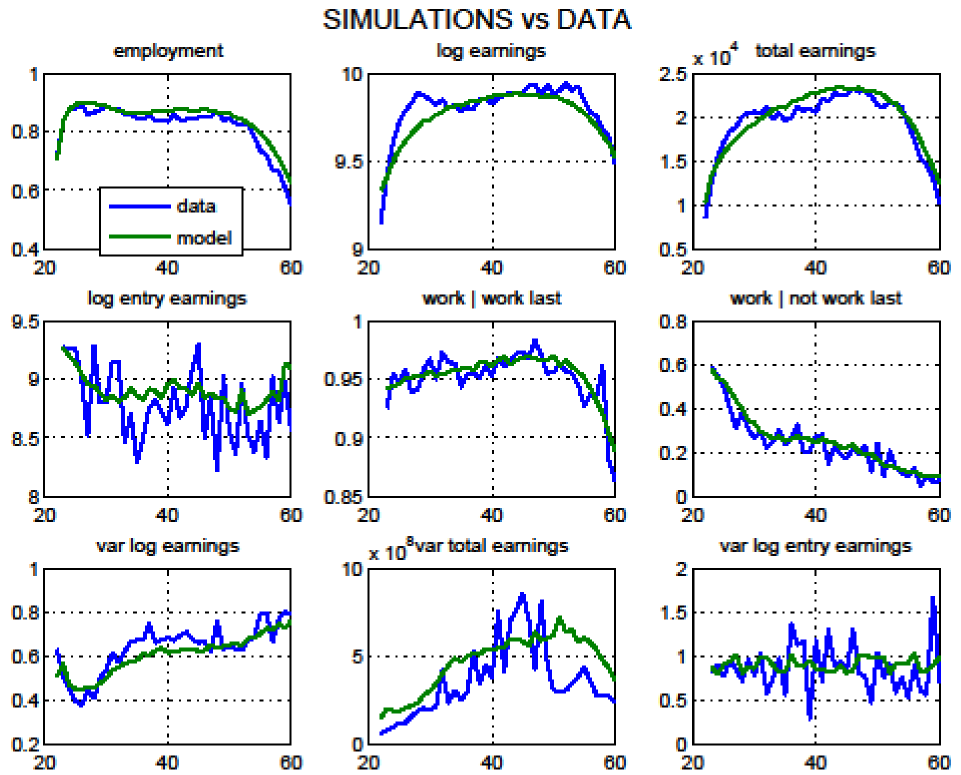


Figure A.3.2: Goodness-of-fit of the earnings and employment model for female university graduates



Appendix B: Income imputation

Parental income is required for determining EMA eligibility. However it is not observed in the data, and is instead estimated using background characteristics of individuals in the National Pupil Database (NPD) dataset. The estimation process is described in more detail in Section B.1, while the accuracy of prediction is discussed in Section B.2.

B.1 Creation of the socio-economic index

Since parental income is not observed, it has to be estimated based on background characteristics of the individuals. The background characteristics are combined using principal components analysis to create one continuous socio-economic status (SES) index. A summary of the variables included in the principal components, and their respective factor loadings, is given in Table B.1.1. Income is assigned using the SES index by matching individuals by their percentile rank in the SES index to the income at the respective percentile rank in the income distribution taken from the Family Resources Survey (FRS).

Table B.1.1: Creation of the SES index

Factor	Factor Loading	Factor	Factor Loading
FSM	0.26	KS2 Score	-0.23
IMD Score	0.45	KS2 Missing	0.18
IDACI Score	0.46	Asian	0.14
ACORN Score	0.38	Black	0.16
ACORN Missing	-0.43	Other/Mixed	0.09
% of Households Owner-Occupied	-0.01	White	-0.23

B.2 Assessment of the validity of the income imputation

In this section, we use the Longitudinal Survey of Young People in England (LSYPE) to assess the validity of our income imputation. We merge individuals in the NPD with their background characteristics to the LSYPE, which includes parental income and responses to questions about EMA receipt. There are 10,708 individuals who appear in both datasets.

Using this subsample of the main dataset, we can investigate how well we assign EMA eligibility to individuals. Table B.2.1 shows a comparison of actual EMA eligibility according to parental earnings in the LSYPE,[49] and predicted EMA eligibility based on

[49] In fact we rank parental income and assign eligibility based on the same percentile ranks as we use to allocate eligibility using the SES measure. We do this to keep the overall eligible proportion fixed, and because we do not think parental income in the LSYPE is completely reliable.

our income imputation technique. The table shows that of those individuals we assign as being ineligible, 65.6% are ineligible according to their parental income. Of those we assign as eligible, 75.2% are actually eligible.

Table B.2.1: Predicted versus actual EMA eligibility using LSYPE income

		Predicted EMA (%)		N
		0	1	
Actual EMA (%)	0	**65.6**	**24.8**	4,497
	1	**34.4**	**75.2**	6,211
N		4,510	6,198	10,708

0 indicates ineligible, 1 indicates eligible.

This shows that the misclassification error is quite high. Misclassification will result in an underestimate of the true effect, as participation may not drop as much in our treated group if it includes ineligible individuals, while participation may drop in our control group due to the presence of eligible individuals.

We investigate the possibility that these misclassifications are driven by poor responses to the parental income question in Table B.2.2. In this, we use questions about EMA receipt in the LSYPE to determine eligibility instead of income. The question is only answered by individuals in education, which explains the reduced sample size in the table. The implications for the success of our EMA allocation for this table are similar to those for Table B.2.1.

Table B.2.2: Predicted versus actual EMA eligibility using LSYPE EMA receipt questions

		Predicted EMA (%)		N
		0	1	
Actual EMA (%)	0	**72.1**	**31.3**	4,095
	1	**28.0**	**68.7**	4,050
N		3,681	4,464	8,145

0 indicates ineligible, 1 indicates eligible.

To try to improve the success of the allocation mechanism, we tried changing the set of background characteristics used to generate the SES index, and we tried to predict income through a regression-based approach. However neither approach was able to improve our prediction accuracy.

Department for Education

Reference: DFE-RR414

ISBN: 978-1-78105-456-7

This research was commissioned under the 2010 to 2015 Conservative and Liberal Democrat coalition government. Views expressed in this report are those of the authors. They do not necessarily reflect government policy.

The views expressed in this report are the authors' and do not necessarily reflect those of the Department for Education.

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