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The short- and long-run effects of paying disadvantaged teenagers to go to school

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Abstract

We evaluate the short- and long-run effects of a large conditional cash transfer program that paid students to remain in full-time education beyond the compulsory school-leaving age. The Education Maintenance Allowance paid teenagers from low-income families in the United Kingdom up to £30 per week ($70 in 2024 prices). Exploiting the programme's staggered rollout across local areas in England, we find that participation in full-time education increased by two percentage points among the poorest students, and that the programme lowered crime amongst pupils with the lowest prior attainment. However, we find no improvements in test scores, no effect on qualifications beyond the lowest level, and a small negative effect on the labour market outcomes of eligible young people in their twenties. While the reductions in crime may have generated some social benefits, these are small relative to the programme's substantial costs.

JEL codes: I28, J24, H52

Keywords: conditional cash transfer, long-term effects, earnings

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1 Introduction

Children born into poverty face stark barriers to economic success. In the United Kingdom, among those born in the mid-1980s, the 15% students poor enough to qualify for free school meals were 40% more likely to leave school at age 16, over twice as likely to be convicted of a crime, half as likely to leave school with good qualifications, and 30% more likely to earn below median wage in adulthood.

Improving disadvantaged students' upward social mobility through increased educational attainment was a key priority for the 'New Labour' government that took office in 1997, with a key policy being the introduction of the Education Maintenance Allowance (EMA).[1] The EMA was one of the largest ever education-related conditional cash transfer scheme in a high-income country. It provided weekly payments of up to £30 (around $70 in 2024 prices), or £1,400 ($3,200 in 2024) per year, to all 16–19-year-olds from low-income households who remained in full-time education beyond the minimum school leaving age of 16 and regularly attended classes. The program cost £900 million ($1.1 billion) a year at its peak in 2010 (2024 prices), and effectively increased government funding for the poorest 16-18 year old students by 20-25% - on top of existing school funding and other relevant welfare payments.

Conditional cash transfer programs have shown substantial positive effects in developing countries, but evidence from high-income settings is limited. The EMA's large scale and comprehensive implementation make it an ideal case study for assessing the effects of these initiatives in richer nations. The programme was extensively advertised and, notably, paid benefits directly to students rather than parents, helping to achieve take-up of well over 90% amongst eligible students.

This paper examines the short- and long-run effects of the EMA on education participation, qualifications, criminality and earnings among low-income pupils using linked administrative data for England with near-universal coverage. Our empirical strategy leverages the staggered rollout of the programme at different times in different areas. We use a range of difference-in-differences strategies, with our main approach

[1] In his party conference speech in October 1996, the Labour party leader and future prime minister Tony Blair famously declared that his three main priorities for government were "education, education and education", with an explicit focus on making educational opportunities available to all.

treating areas that received the EMA early as a control for areas that received it later. Our preferred estimates use data from 50 local authorities (one-third of the total number in England), which is the subgroup that is most observably similar between treated and control – half of which received the EMA before 2004, and half in 2004. Our analysis focuses on students who were eligible for free school meals when they were 16, virtually all of whom were also eligible for the full EMA award.

We find that despite high take-up amongst those in education, the EMA failed to improve upward social mobility among children from the poorest families. If anything, earnings through people's twenties fell slightly as a result of the policy. Consistent with this, we find reductions in employment and increases in out-of-work benefit claims (primarily job-seeker's allowance) during people's twenties. We do not observe any effects on criminal convictions on average.

Several mechanisms explain these disappointing results. First, the EMA's effect on education participation was small, increasing full-time participation by only two percentage points among low-income students at ages 16/17 and by one percentage point at ages 17/18. These modest effects are not simply due to the focus on the poorest students as estimates for the full student population suggest the impacts were similarly small across all eligible groups. Second, the majority of students who were induced to stay in full-time education because of the policy would have done something valuable anyway, such as part-time education or work-based training. For individuals who were pulled out of inactivity into full-time education, it appears that the EMA simply delayed, rather than prevented, the period of inactivity. Third, while the EMA might have been expected to improve academic performance by reducing financial constraints and part-time work among enrolled students, we find no impact on qualifications beyond the most basic level. The programme likely reduced part-time work among students who would have remained in education anyway. Hence, it may have crowded out valuable early labour market experience without offsetting human capital benefits. Fourth, even among groups where the programme had larger effects on participation, such as students with special educational needs and students with low prior attainment, we still observe negative effects on later earnings, indicating that the additional education received by these individuals as a result of the EMA may have been of low value.

A more encouraging finding is that pupils with low prior attainment did experience a fall in the probability of criminal conviction due to the EMA, both at age 16–18 and in young people's twenties. The magnitude is a 7% decrease in the share convicted per year. However, the associated financial benefits are likely to be small relative to the costs of the policy.

Our work contributes, first, to the existing literature on the effects of the EMA. Based on surveys of around 5,000 individuals and a cross-sectional matching approach, Dearden et al. (2009) found education participation effects that were around three times larger than ours.[2] Sabates and Feinstein (2008) explored the effects of the EMA on crime using aggregated local crime data and found that EMA pilot areas experienced drops of 1-1.5 convictions per 1,000 individuals. Compared to these studies, our estimates rely on weaker identification assumptions and are more precise due to our much larger sample sizes.[3] This has become possible thanks to newly-linked administrative data covering nearly the whole population of young people in England across several cohorts (around 600,000 individuals). The individual-level panel allow us to estimate the long-run effect of EMA eligibility on qualifications, criminality and earnings (up to young people's mid-twenties), which the previous evaluations of the EMA were unable to consider, and has not previously been possible for any conditional cash transfer in a high-income country.[4]

Our second contribution is to the general literature on the effect of cash transfers. Comparable programs in high income areas, including Australia (Dearden and Heath, 1996) and New York City (Riccio et al., 2013) have similarly shown statistically significant but economically modest effects on full-time participation and attainment. With regard to crime, Watson et al. (2020) finds a small reduction in crime as a result of an unconditional

[2]We attribute this difference to methodological factors (cross-sectional vs. difference-in-differences estimation), the declining real value of EMA over time, and differences in how participation was measured in survey versus administrative data. See Section 6.1.1 for a more detailed discussion.

[3]For example, we assume common trends between treatment and control areas, whereas the cross-sectional approach in Dearden et al. (2009) assumes common trends and levels between treatment and control areas.

[4]Jiang (2024) uses data from the NextSteps survey to study the effect of EMA *receipt* amongst lower-income students on longer-term outcomes, within a single birth cohort. However, because receipt was directly linked to school attendance, this approach largely captures differences in enrolment rather than the causal impact of the policy itself. In contrast, we estimate the effect of EMA *eligibility*, exploiting variation across cohorts.

cash transfer in Alaska. The findings contrast with evidence from low- and middle-income countries, which have shown substantial positive effects of conditional cash transfers on education participation and academic performance (Barrera-Osorio et al., 2011; Attanasio et al., 2012; Glewwe and Kassouf, 2012; Galiani and McEwan, 2013), large decreases in crime (Chioda et al., 2016), and improvements in long-run outcomes such as university attendance (Barrera-Osorio et al., 2019) and earnings (Barham et al., 2013). We show how these contrasting results between high and low income areas can be rationalised in a Roy (1951) model of human capital investment.

Given the encouraging findings from the EMA pilot on enrolment, it was not obvious that the EMA would fail to achieve its aim of enabling social mobility through educational attainment. Since the design of the EMA rewarded regular attendance, one might have expected the program to improve educational attainment through increased instruction hours (Harmon and Walker, 1995; Oreopoulos, 2006; Fryer Jr, 2014; Guryan et al., 2023). Similarly, the incentive to attend schooling may have reduced idle time available to get involved in crime (Jacob and Lefgren, 2003; Machin and Meghir, 2004)) and the higher income could have diminished incentives for crime through wealth effects (Foley, 2011; Blattman et al., 2017; Watson et al., 2020; Chioda et al., 2016). The EMA could also have affected students who would have enrolled anyway due to additional time to focus on their studies since they may have needed to work fewer hours to support themselves financially. Besides, the EMA might have also reduced spells of youth unemployment after leaving education, which have been shown to be particularly damaging (Gregg and Tominey, 2005).

Our findings instead support Heckman's (2013) contention that impacting long-run outcomes through educational interventions becomes increasingly challenging as children age. They are consistent with more recent evidence showing that raising the compulsory school leaving age in the UK had no long-run effect on earnings (Clark, 2023) and evidence that high-school dropout in Norway had no negative long-run consequences (Andresen and Løkken, 2024).[5] Taken together, these findings suggest that disadvantaged students who choose to drop out of school or attend irregularly may not benefit from additional

[5] Pischke and von Wachter (2008) also found no effect of increasing the school-leaving age in Germany, but this result has recently been challenged by Cygan-Rehm (2022).

education in a traditional school setting.[6]

The remainder of this paper is structured as follows. Section 2 outlines the institutional background of the Education Maintenance Allowance and the post-16 education landscape in England. Section 3 introduces a theoretical framework for thinking about the likely long-run impact of the EMA and the mechanisms through which those impacts can occur. Sections 4 and 5 describe the data and the methodology, respectively. Section 6 reports the short and long-run impacts of the policy, and analyses heterogeneity in effects across groups. Section 7 discusses the robustness of the results to changes in assumptions and specifications. The results are discussed in Section 8 before Section 9 concludes.

2 Institutional background

2.1 Post-16 options in England in the early 2000s

In England during the early 2000s, compulsory education ended after Year 11 (the academic year in which students typically turn 16). Nearly all students took nationally-standardised General Certificate of Secondary Education (GCSE) examinations at this stage. GCSEs typically covered 10-12 subject areas, with letter grades assigned from A* through to G, based on performance. All grades through to G are considered a pass, with fails graded as "Unclassified". For some of our analysis, we divide people up based on whether they had low prior attainment (meaning they did not achieve at least five GCSEs with grade G or above); medium prior attainment (meaning they achieved five GCSEs with at least a G but did not achieve five GCSEs with a grade C or above); or high prior attainment (meaning they achieved at least five GCSEs with at least a grade C).[7]

Students who achieved at least five A*-C grades at GCSE level could progress to Advanced-Level qualifications (A-Levels) in Year 12. These two-year courses,

[6]While the long-run effects of job training programs targeting disadvantaged young people have been similarly disappointing (Schochet et al., 2008; Alzúa et al., 2016), the evidence presented in Cavaglia et al. (2020) suggests that in England, apprenticeships may offer substantial positive returns in some fields.

[7]The use of five A*-C GCSE grades as a measure of achievement is common in the UK literature. Our use of five A*-G is less common but is intended to divide up the 75% of free-school-meal-eligible students who did not achieve five A*-C grades. Our classification of prior attainment also includes grades from the vocational "GNVQs", which were considered equivalent to GCSEs.

culminating in examinations at the end of Year 13 (typically age 18), were the primary route to university admission. Students could pursue A-Levels either in schools or Further Education (FE) colleges.

Alternative post-16 options included lower-level academic courses, such as GCSE retakes, but also vocational qualifications. From 16-18, vocational qualifications could be done at Level 1 (basic), Level 2 (intermediate) or Level 3 (advanced), depending on the individual's prior attainment, with Level 2 being the most common. These programs were typically delivered in FE colleges and offered both full-time and part-time study options.

Students who did not continue in education after Year 11 had three main alternatives. First, they could enter the labour market without any formal training. Second, they could participate in (work-based) training, which combined employment with structured instruction. Third, they could become neither employed nor engaged in education or training (NEET) – though this may in many cases have reflected difficulty securing work or a training placement rather than an active choice.

Training encompassed several programs, primarily delivered directly by employers or in partnership with Further Education colleges. The main routes were Advanced Modern Apprenticeships (Level 3) and Foundation Modern Apprenticeships (Level 2). Most training participants were employed and received wages. Those in unpaid training positions received a Minimum Training Allowance of at least £40 per week. There were also unpaid preparatory programs such as Entry to Employment and Provider-led Apprenticeships, though these were later reclassified as full-time education in April 2006.

2.2 The Education Maintenance Allowance

The EMA provided weekly payments to 16-19-year-old students from low-income backgrounds who remained in full-time education. Most students could claim the allowance for up to two years starting from Year 12 (the academic year in which they turned 17), with the possibility of extending to Year 14.[8] To be eligible, students had to be above the compulsory school leaving age and enrolled in full-time academic or further

[8] Students receiving support for Special Educational Needs could claim the allowance for up to three years.

education courses. While both vocational and academic courses were eligible, more advanced courses (above Level 3 - most notably, higher education) and work-based training were excluded.

The payment operated on a sliding scale based on parental income (Ashworth et al., 2001). Students with parents earning less than £19,630 in the preceding tax year (approximately 40% of the population) received £30 per week during term time, totalling £1,200 annually.[9] This represented a substantial sum, equivalent to a quarter of full-time minimum wage earnings for a 16-year-old, or equivalent to 20-25% of per-pupil funding for 16-18 education in schools and colleges.[10] Students from households earning up to £24,030 (next 10% of the population) received £20 per week, while those with parental income up to £30,000 (following 10% of the population) received £10 per week. Additional incentives included £50 bonuses for completing each school term and for good examination performance, allowing students to receive up to a maximum of £1,400 per year.[11]

Official government statistics suggest that take-up rates of the EMA were high. Table A1 in the Appendix gives official estimates of the number of EMA recipients by year, alongside total numbers of students in full-time education who were potentially eligible for the award. Just over half of all students claimed the EMA within each academic year, which is close to 100% of the number of eligible students.[12]

2.3 The staggered rollout of the EMA

First announced in spring 1999, the EMA was initially rolled out as pilot schemes in 15 out of 150 English Local Authorities (LAs) in September 1999. Students living in these LAs were eligible for up to £30 or £40 per week, depending on their LA's pilot scheme variant

[9] For more detail on the policy see Middleton et al. (2005) and Ashworth et al. (2001).

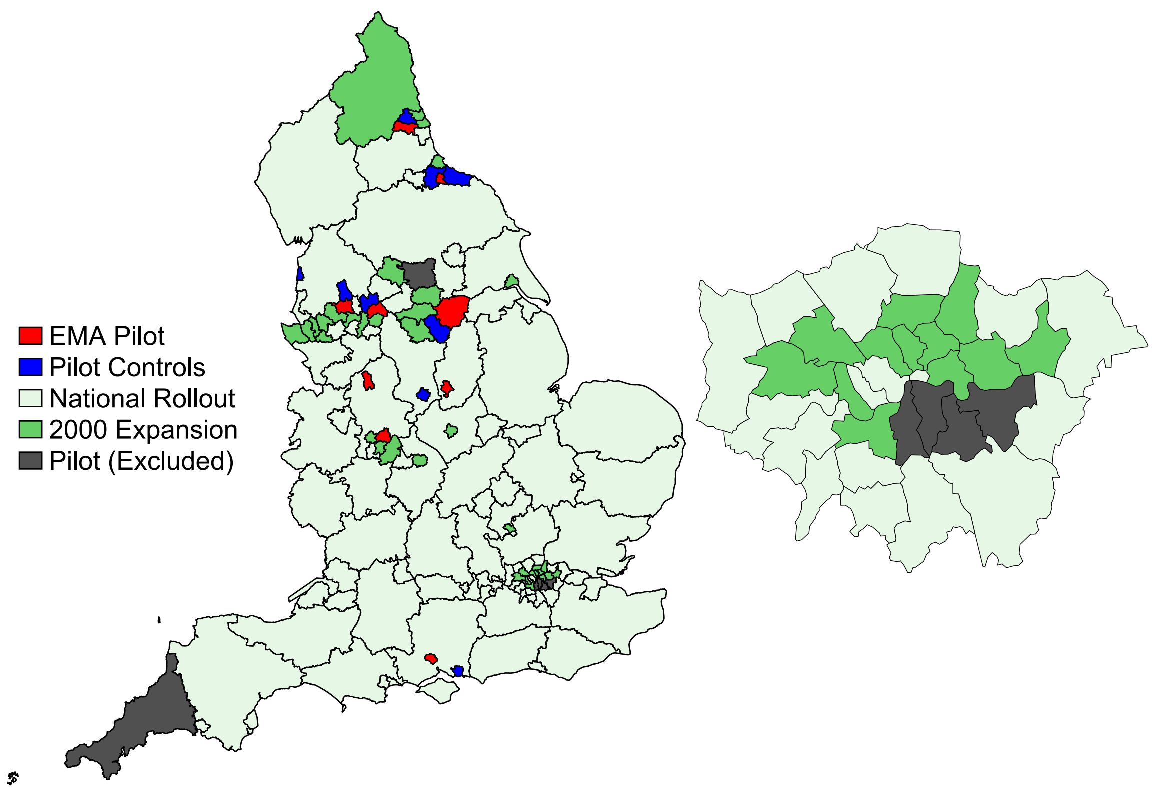
[10] At the time of the national rollout of the EMA, per-student funding ranged from £4,000 to £4,500 (Britton et al., 2020). Additional education-related welfare payments (see Appendix Section A for more detail) take this figure up to around £6,000-£6,500 per student.

[11] All the numbers in this section are in 2004 prices unless stated otherwise. The parental income thresholds were periodically increased, but went up more slowly than inflation. The amount of weekly cash support available was frozen in nominal terms until the EMA was abolished in England in 2011.

[12] Britton and Waltmann (2019) use data from the Next Steps survey to show that 60% of the population were eligible for the EMA, but participation was around 10 percentage points lower amongst this population. This suggests that around 56% of those in education were eligible for the EMA. Table A1 therefore suggests uptake of between 93% and 100%.

and their parental income. Eleven LAs were designated as control areas, chosen for their similar characteristics to the pilot LAs - these were predominantly deprived, urban areas outside London. In September 2000, the pilots expanded to another 41 LAs, though the original control LAs remained excluded. For the next four years, LAs containing about one-third of England's population - mostly urban areas with higher deprivation levels - had access to the EMA, while the other two-thirds - predominantly rural and suburban areas with lower deprivation - did not. Although students could participate in education in a different LA to the one they lived in, eligibility was based entirely on their home LA, meaning they could not cross LA borders to receive it.

Figure 1: Education Maintenance Allowance Rollout



Note: Local Authorities in England, with London expanded to the right, by EMA rollout group. The pilot (excluded) areas received the EMA in the 1999 pilot but were excluded from the Dearden et al. (2009) analysis. The pilot controls received the EMA in 2004 along with the national rollout areas.

Past work on the EMA (Dearden et al., 2009; Sabates and Feinstein, 2008) exploited differences between the 1999 pilot and control areas to estimate the short-term effects of the policy. Dearden et al. (2009)'s main analysis focuses on nine pilot and nine control

LAs, selecting an urban non-London subset from the pilot and control areas for better comparability. We also use this matched subset of LAs in some of our analyses.

Figure 1 shows the EMA rollout across areas. The nine EMA pilot and nine Pilot control areas used in Dearden et al. (2009) were similar in terms of geography and size. The 2000 expansion included more urban areas, namely in the North West and in London. In 2004, the EMA was extended nationwide. This rollout was typically in more rural areas, although it also included urban areas in London and cities such as Bristol, Plymouth and York. The 2004 rollout also included the pilot control LAs, which were typically more deprived, and more urban.[13]

While different LA variants of the EMA had existed from 1999-2004 (varying in payment amounts, income thresholds, achievement bonuses, and payment recipients), a uniform version paying students directly was implemented in the 2004 national rollout.

3 Conceptual framework

This section introduces a simple framework to illustrate how conditional cash transfers might influence post-16 education choices and later-life outcomes. Consider a two-period model where individuals choose between staying in school (S) or dropping out (D) in period 1. The value of each choice includes both monetary returns (like earnings) and non-monetary factors (like enjoyment of school or work). Let π_{1A} represent the utility from option A in period 1, incorporating both monetary returns (including any earned income or transfers) and non-monetary factors (like enjoyment or distaste for school), where $A \in \{S, D\}$. Let y_{2A} represent the monetary returns (earnings) in period 2 from option A.[14] Suppose there is no borrowing or saving. With discount factor δ, individuals choose to stay in school if and only if:

$$\pi_{1S} + \delta y_{2S} > \pi_{1D} + \delta y_{2D}. \tag{1}$$

[13] The EMA was also introduced in Scotland, Wales and Northern Ireland in 2004 and continues to operate there. Wales significantly increased the scheme's generosity in 2023.

[14] While period 1 utility captures both monetary and non-monetary factors, we abstract from non-monetary factors in period 2 to simplify the discussion and connect the framework more clearly to our main object of interest: the long-run earnings effects of the EMA.

3.1 Factors affecting the education decision

This simple framework is sufficient to highlight several channels through which the EMA might affect education choices and subsequent outcomes. Even when staying in school offers substantial long-run returns ($y_{2S} >> y_{2D}$), students might still drop out if the immediate costs are high enough ($\pi_{1S} << \pi_{1D}$). These immediate costs can be particularly important. Some students' families may be unable to provide sufficient financial support during further education, making even modest direct costs of attendance prohibitive regardless of the potential returns.[15] Others may have a strong dislike for school or face attractive immediate labour market opportunities. The EMA directly increases π_{1S} by providing resources to overcome financial barriers, compensating for a dislike of school, and offsetting foregone earnings. Additionally, students may prioritise immediate consumption over future earnings due to high discount rates (low δ). The EMA reduces this trade-off by providing current income while in education.

Students also face imperfect information about the returns to education. They must make their education choice based on their perceived returns ($\tilde{y}_{2S} - \tilde{y}_{2D}$), which may differ substantially from the true returns ($y_{2S} - y_{2D}$). In communities where few adults have stayed in education beyond the minimum leaving age, students may systematically underestimate the true returns due to limited exposure to successful examples. Students from these backgrounds may also face greater uncertainty about the returns to staying in education. Risk aversion could lead them to favour the more familiar path of leaving school, even when expected returns to education are positive.

However, the effectiveness of the EMA in improving long-run outcomes depends crucially on whether students who are induced to stay in school actually experience higher second-period returns ($y_{2S} > y_{2D}$). If students rationally drop out because they expect low returns from additional education - rather than due to inability to finance education costs or imperfect information about returns - then increasing participation

[15] Dearden et al. (2004) find that a large fraction of poorer students in the UK face prohibitive financial barriers that prevent them from remaining in education, based on data on a cohort born in 1970. Dearden et al. (2009) find larger impacts of the EMA among individuals who they argue are more likely to face these financial barriers.

through financial incentives may not improve second-period earnings.

3.2 Factors affecting educational achievement

The EMA was contingent not only on signing up for an education course but also on attending classes: to receive the weekly payment, attendance had to be above 90%, and all absences had to be authorised. This additional time in the classroom may be valuable for improving test scores and subsequent earnings outcomes. The attendance requirements combined with the additional cash resources may also impact engagement with part-time work alongside study. This could also boost the amount of time individuals can spend focussing on their studies and may also improve test scores and long-run outcomes (y_{2S}). On the other hand, less time accumulating work experience and a weaker attachment to the labour market could result in worse labour market outcomes. Importantly, these channels can potentially impact all students, not just the marginal students who are induced to stay in education as a result of the EMA, who are a minority group in our context.

3.3 The role of the outside option

The simple framework features two post-16 options: education (S) and dropping out (D). As discussed in Section 2, teenagers face several different post-16 options outside of full-time education, including part-time education work, different forms of work-based training (such as apprenticeships) and doing nothing. One way to accommodate this variety in the framework above would be to reframe the work options as an individual's best outside option. Intuitively, if individuals are drawn from inactivity, one might expect the long-run earnings returns to be positive. Inactivity has been shown to have long-run scarring effects (Gregg and Tominey, 2005), while staying in education might provide an opportunity to gain some basic skills that have value in the labour market. On the other hand, if individuals come from options that have positive long-run effects which boost y_{2D} - such as work-based training - then the positive long-run effects of the EMA are less assured.

3.4 The EMA and crime

Another channel and set of outcomes not captured by the simple framework above is criminal activity, which could be affected by a cash transfer in an immediate and lasting manner.

Economic possibilities affect crime, as shown by a large literature that began with the theoretical foundations in (Becker, 1968) – who posits that individuals rationally choose between crime and legal activities by considering the returns to each of these options and the likelihood and severity of punishment. In the short-term, cash transfers might decrease crimes by increasing the returns to the legal option (in this context, studying).[16] This model also accommodates other incentives for crime - like financial need - which might increase an individual's willingness to bear the possible risks associated with being caught. Cash transfers might thus reduce crime by decreasing financial stress (Watson et al., 2020).

Falls in short-term crime could also affect later-life criminality through their direct effects on human capital, later-life wages and employment; and by affecting employers' search process. Time spent incarcerated or convicted can decrease human capital accumulation and savings (Mueller-Smith, 2015; Garin et al., 2024). Besides, a fall in crime engagement earlier in life might also reduce opportunities to accumulate criminal capital, decreasing future expected crime returns.[17] Beyond these direct effects, having a criminal record can also induce discrimination or selective screening in the labour market (Pager, 2003).

A special case which might be differently affected by a cash transfer is drug crime. If illegal drugs are normal goods, one would expect that additional available income could induce drug buying. Watson et al. (2020) finds empirical evidence in favour of this mechanism for a universal transfer. If drug consumption leads to addiction patterns, these early purchases could also affect later-life consumption, and hence later-life crime.

[16] While the Becker model is most applicable to offences where there is a material gain – like thefts, robbery or burglary – the empirical literature has consistently found similar effects of unemployment and low wages on property and violent crime (Gould et al., 2002; Bell et al., 2022).

[17] See Mocan et al. (2005); Arora (2023) and Bell et al. (2022) for evidence on the theoretical foundations and empirical importance of this mechanism.

4 Data

We use several administrative datasets to estimate the effect of the EMA in the short run and long run on educational outcomes, labour markets, and crime. To assess the effects on education and the labour market, we use the Longitudinal Educational Outcomes dataset, which links school records on the universe of state-educated children in England with separate administrative datasets from Further Education colleges and universities. Finally, it is linked to administrative tax and benefit data.

School records from the National Pupil Database (NPD) include data on results at nationally standardised academic exams. These include GCSEs, which as described above are taken at age 16 by most students, and A-Levels, which are taken at age 18 by around one-fifth of students eligible for free school meals. The NPD incorporates data on whether students remained in school after age 16. It also includes some demographic information on students such as month and year of birth, gender, ethnicity, first language, and special educational needs status.

The NPD also includes an indicator of whether a student is eligible for free school meals, which is based on access to certain state benefits.[18] Recipients are typically within the bottom 15% of the population by parental income. The school records do not have any marker of eligibility for the EMA, so we focus our estimation on those who were eligible for free school meals. Using the Family Resources Survey, we estimate that over 99% of those who were eligible for free school meals would also have been eligible for the full EMA award in the following academic year.[19]

Since data on free school meals comes from school records, we only have this information up to the last year of compulsory schooling, age 16. We restrict our sample to those who were eligible for free school meals at age 16. This means that some would not have been eligible for the EMA at ages 18 or 19 if their parents had a sudden large increase in their income, but we expect this to be rare. Therefore, we expect that when we restrict our sample to those who were eligible for free school meals, we capture a population that would be almost surely eligible for the full EMA (those at the lower end

[18] For some of these benefits, such as Child Tax Credit, eligibility for free school meals also required recipients' household income to fall below a certain threshold.

[19] Eligibility for the EMA was based on parental taxable income in the previous tax year.

of the income distribution). The results of this paper should be interpreted as the effect of the EMA on those from the bottom 15% of the household income distribution.

Data on further education comes from the Individualised Learner Record (ILR). This has details on whether a student was attending a Further Education college in each academic year. It also provides records on all of the courses they studied, from which we can derive whether they were vocational or academic, and whether they successfully completed them, giving us the level of the qualification achieved. We also use data from the Young Person's Matched Administrative Dataset (YPMAD), a derived dataset from the NPD and ILR which provides simplified information on student's activities in a given school year.

Data on university education comes from the Higher Education Statistics Authority (HESA). It includes details on what university students attended, what degree classification they achieved, and at what age they attended.

Administrative data from HMRC, the tax authority, enables us to link educational data with long-run earnings and benefit receipts. Tax data from HMRC reports total earnings in each tax year for all individuals who are successully matched to the NPD, allowing us to observe student's earnings both during and after leaving education. It does not include data on other aspects of employment, such as hours or occupation. However, we use the tax data to derive a measure of employment which is based on whether individuals earned above a the lower earnings limit in a given tax year.[20]

The tax data also includes information from the Department of Work and Pensions (DWP) on individual benefit spells. This includes whether benefits were received, for how long they were received, and whether these were out-of-work benefits, such as unemployment benefits or income support for those unable to work. This allows us to estimate the effect of the EMA on different types of benefits and gives us an additional indicator of unemployment.

For the evaluation of the effect of the EMA on criminal outcomes, we use the Ministry

[20] This was £4,004 in 2004. During this period, employers are legally only legally required to declare earnings of employees earning above the lower earnings limit in a given tax year. We therefore think this is a more useful measure of employment than whether an individual is earning above £0 per year. Of course, our cumulative earnings measures may miss some earnings below this level that are not reported to HMRC. However, the fact that there is no discontinuity at the lower earnings limit in the data suggests this is unlikely to be a major issue.

of Justice NPD-PNC database. This source links the NPD dataset described above with criminal records from the Police National Computer (PNC). This includes individual-level records on offences for which an individual receives a caution or conviction in court. We focus on convictions, which are more severe offences. For each offence, the PNC includes details on the type (for example, drug offences, thefts, and other categorisations), the day, and the sentence received.

For each individual and academic year, we compute the probability of being convicted in a given year, and, separately, the probability of being convicted for two major crime categories: thefts, and drug offences. We focus on whether crimes were committed during the period in which an individual might have received the EMA (in years 12 to 13, at ages 16 to 18), or after year 14 (at ages 19 to up to 29). For individuals eligible for free school meals, the likelihood of yearly conviction was 4.3% at ages 16 to 18, and 3.5% at ages 19 to 29. These are relatively high numbers when compared to non-FSM-eligible pupils for whom the likelihood of yearly conviction was 1.8% at ages 16 to 19, and 1.6% at ages 19 to 29.

The earliest cohort for whom we have information on all our key outcomes (education, earnings, and crime) are those who finished their last compulsory year of schooling in the summer of 2002 (the 2002 GCSE cohort). We follow cohorts between the 2002 and 2009 GCSE cohorts. In addition to data on education participation, we use data on qualifications obtained up to age 23 (except for university degrees, where we use data up to age 26). This allows us to evaluate the effect of the EMA on qualifications obtained during and after students' period of eligibility. We use data on earnings up to age 28, and data on criminal convictions up to age 29.

5 Methodology

5.1 Estimation samples

We estimate the effect of the EMA within three samples of Local Authorities (LAs): the pilot treatment and control areas from Dearden et al. (2009), which were selected specifically for their similarity to each other; an expanded sample including both the pilot treatment and control areas and an additional matched sample of areas with similar observable characteristics; and all areas in England.

Table 1: Sample descriptives for FSM-eligible pupils, by area

Sample	Pilot areas		Pilot + matched		Whole of England	
Rollout	1999	2004	1999/00	2004	1999/00	2004
A. Student characteristics (at baseline)						
% White	71.6	79.2	75.8	79.0	60.1	79.3
% 5 A*-C at GCSE	24.8	25.0	25.3	24.1	30.0	26.0
% 5 A*-G at GCSE	72.3	73.3	74.2	74.6	77.3	76.0
% Special Educational Needs	29.8	30.6	30.3	30.4	29.9	32.2
% Deprived neighbourhood	63.3	59.3	54.2	45.1	66.0	33.0
% Crime by age 16	4.9	4.9	4.9	5.0	4.2	4.2
B. Student outcomes						
% Full-time Ed., age 16-17	54.7	54.3	55.9	55.6	60.9	57.9
% NET, age 16-17	22.9	22.1	21.8	21.7	19.2	22.8
% Earnings $\geq$ LEL, age 17	10.3	10.0	10.8	11.0	8.6	12.7
% Crime age 16-18	7.1	7.2	7.1	7.6	6.4	6.7
% University attendance	16.4	17.0	17.9	17.2	25.0	18.2
Obs.	10,115	7,890	30,075	23,495	80,705	73,525
LAs	9	9	25	25	53	94

Note: Data covers cohorts turning 16 in the 2003/2004 and 2004/2005 academic years. ”LEL” = Lower Earnings Limit of approx. £4,000 (2004 prices). “NEET”= Not in Education Employment or Training. Suffolk, Sunderland, and Lancashire are excluded from this analysis, as they received non-standard versions of the EMA. The total number of LAs in the final two columns (147) differs from the number of clusters used in subsequent regressions (149) due to some reclassification of LAs during our observation period. Different choices about how to treat this reclassification do not affect our results.

The nine pilot and nine control areas used in Dearden et al. (2009) either received the EMA pilot in 1999 or were deliberately held back from receiving the EMA until 2004.[21] Table 1 highlights the similarities between pilot treated and pilot control areas, and the differences relative to the rest of the country. These similarities might make the identifying assumptions more plausible, but the small number of areas limits precision. The FSM-eligible population of pilot treated or control areas is relatively small, with less than 10,000 students per cohort across treatment and control groups, compared to over 75,000 across the whole population.

We therefore also include an expanded sample of LAs in our analysis. In addition to the

[21] Six additional LAs also first received the EMA in 1999, but these areas were not assigned similar enough controls for inclusion in Dearden et al. (2009).

nine pilot and nine control areas used in Dearden et al. (2009), this sample also includes a set of "Matched" areas selected using nearest-neighbour matching to identify the LAs that are observably similar to each other.[22] The matching was done using a probit model to calculate a propensity score for receiving the EMA prior to 2004, rather than first receiving it in 2004. Then, the most similar areas in their propensity scores were matched together one-to-one. Using this matching process, we selected an additional 16 LAs for both early and late rollout areas. The full list of areas can be found in Table A3 in the Appendix. As shown in the third and fourth columns of Table 1, students in the early and late rollout areas in this "pilot + matched" sample had similar demographic backgrounds and later-life outcomes, though fewer children lived in deprived neighbourhoods in the national rollout areas.

Finally, we also present results for the near-universe of LAs in England, dividing the whole country into pre- and post national rollout areas.[23] This sample provides an estimate of the average effect of the EMA on all FSM-eligible students across England and, due to the large sample size, provides the most precise estimates of the effect of the EMA on eligible students. However, as the rightmost columns of Table 1 show, FSM-eligible students in early rollout areas were less likely to be white and were from more deprived neighbourhoods, but performed better academically. These differences make differential trends more of a concern, so it is plausible that greater precision comes at the cost of some bias. We consider the post-trends for the different samples in Section 5.4.

5.2 Education and earnings

To assess the effects of the EMA on education and earnings, we use a difference-in-differences specification that takes differences at the area-by-cohort level. We compare the cohort-by-cohort change in outcomes for pupils who lived in areas that first received the EMA in 1999 or 2000 to pupils who lived in areas that only received the

[22]The area characteristics used for this matching process can be found in Table A2 in the Appendix.

[23]We exclude three LAs: Suffolk and Sunderland, which received a variant of the EMA that included free bus travel but no weekly payment, and Lancashire, in which different sub-areas of the LA received the EMA at different times.

EMA in 2004 (the National Rollout areas).[24] The specification is:

$$y_{ijc} = \alpha_j + \nu_c + \beta Post_c \times NR_j + \delta X_{ijc} + \epsilon_{ijc} \quad (2)$$

where $\hat{\beta}$ is interpreted as the causal effect of the National Rollout of the EMA on the outcome of interest. Local authority fixed effects are denoted by α_j and capture time-invariant attributes of each local authority. Cohort-level fixed effects denoted by ν_c capture cohort-level attributes that do not change across areas. We also include a set of individual and neighbourhood-level controls in vector X.[25] We run this specification on two cohorts either side of the national rollout – those finishing compulsory education between 2002 and 2005 – and we compute standard errors clustering at the LA level.

This specification differs from the traditional difference-in-differences in that the "control" areas (the pre-National Rollout areas) had already received the EMA. Therefore, instead of one group switching from untreated to treated over time and the other group being never-treated, we have a group which is always-treated within our framework and another that switches. Slightly different assumptions are required for $\hat{\beta}$ to identify a causal effect compared to the standard set-up.[26]

We assume that the effect of the EMA stays constant across the cohorts we study. Then, the change in the outcome variable for the always-treated group relative to the later-treated group is the same as if the same group had never been treated, and inference can proceed as in the standard difference-in-differences case. This assumption is plausible in our setting as the treatment is assigned by cohort, so that observations at different times relate to different individuals and fade-out and duration-of-treatment effects do not apply. We are also reassured by the fact that education participation rates were quite stable during the period we are studying, and not close to 100% for the population of interest (in which case, increases in participation would not be possible).

One key threat to identification is implementation lags: cohorts soon after the formal

[24]Some areas which first received the EMA in 1999 or 2000 changed between different variants of the EMA in 2004, as discussed in Section 2. We show that our results are robust to excluding these areas in Section 7

[25]A full list of controls can be found in Table A4 in the Appendix.

[26]Other empirical papers to have used similar reverse difference-in-differences approach to identify effects include, Kim and Lee (2019), Sawada et al. (2022) and von Hinke and Sørensen (2023) (though in the latter case, the treated group goes from treated to untreated, while the control group is always untreated).

rollout of the EMA to a given area may have been only partially treated due to bureaucratic delays or low awareness of the programme. In our case, this would only be a threat to identification if full implementation took more than two years, as the last control LAs received the EMA starting with the 2000 cohort and we only use data from the 2002 cohort onwards. Furthermore, as shown in Table A1 in the Appendix, we observe high and constant EMA take-up rates in different years, suggesting substantial implementation lags were unlikely.

While our data does not allow us to examine pre-trends, in our setting it is *post*-trends that are more informative about the plausibility of the underlying parallel trends assumption (Kim and Lee, 2019). In order to examine the post-trends, we estimate event studies of the form:

$$y_{ijc} = \alpha_j + \nu_c + \sum_{c \neq 2004}^{2009} \beta_c \left(\mathbb{1}[cohort = c] * ER_j \right) + \delta X_{ijc} + \epsilon_{ijc} \tag{3}$$

where $ER_j = 1$ in early rollout areas, and $ER_j = 0$ if area j is a national rollout area. The objects of interest are the coefficients $\hat{\beta}_c$ from the 2005 to 2009 cohorts.

5.3 Crime

In addition to estimating equation 2, which exploits variation across areas and cohorts in the reverse difference-in-differences specification (referred to as "Reverse DD" in the results section), we complement the estimates for crime with an alternative identification strategy.

The alternative strategy is a triple difference-in-differences specification (referred to as "Triple DDD" in Section 6). Crucially, the first difference exploits changes in crime before and after individuals reach age 16, when they become eligible for the EMA.[27] The second difference is across areas (some areas were receiving the EMA in 2002 and 2003 while others were not), while the third difference is by cohort (in areas which received the EMA in 2004, older cohorts did not receive it, while younger ones (those who finished compulsory schooling in 2004 and 2005) did.

[27] This is possible because crime participation is observed annually from age 14 onwards. It is not possible to use this approach for human capital outcomes: education participation is compulsory until age 16, so there is no pre-treatment variation, and we do not observe tax records before age 16.

The estimating equation is as follows:

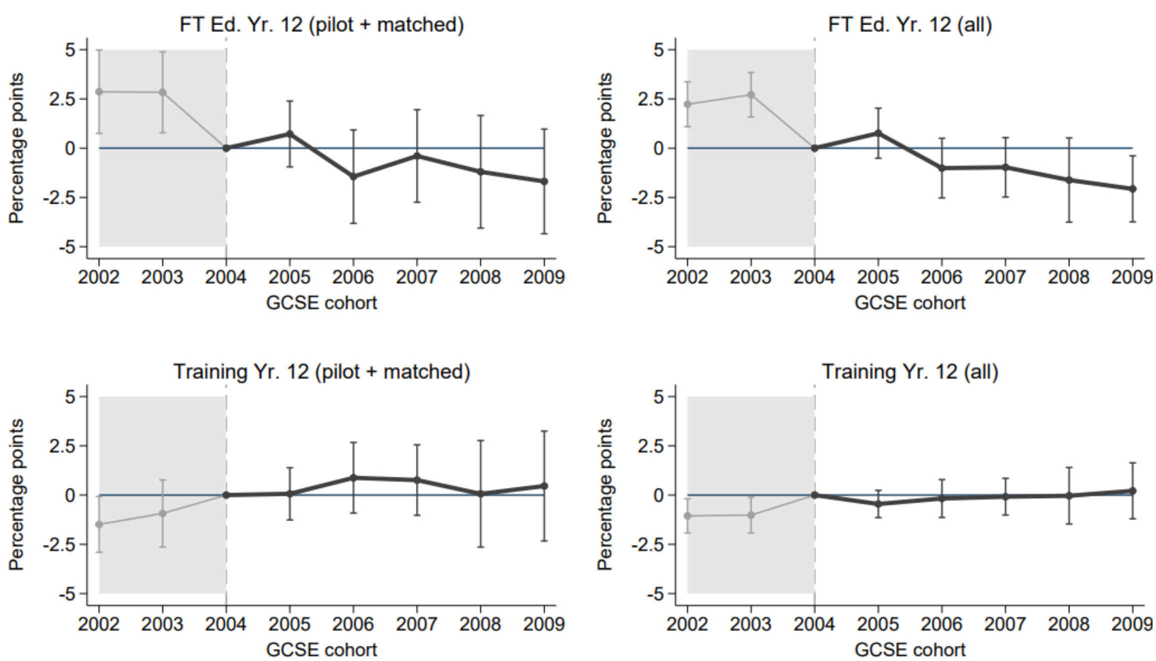
$$y_{ijca} = \psi_i + \lambda^1_{ac} + \lambda^2_{aj} + \beta Post_c \times NR_j \times \mathbb{1}[a \geq 16] + \delta X_{ij} + \epsilon_{ijca} \tag{4}$$

where $\mathbb{1}[a \geq 16]$ indicates that individuals have finished compulsory schooling, and β represents the parameter of interest. We include individual fixed effects ψ_i, which incorporate area-, cohort- and area-by-cohort fixed effects, as well as interactions of age and cohort and age and area in λ^1_{ac} and λ^2_{aj}. ER_j indicates whether an individual's area was in the early EMA rollout.

This specification is robust to additional differences across areas, including differential changes in outcomes upon finishing compulsory schooling across areas. However, estimates from this specification are less precise.

5.4 Trends in outcomes

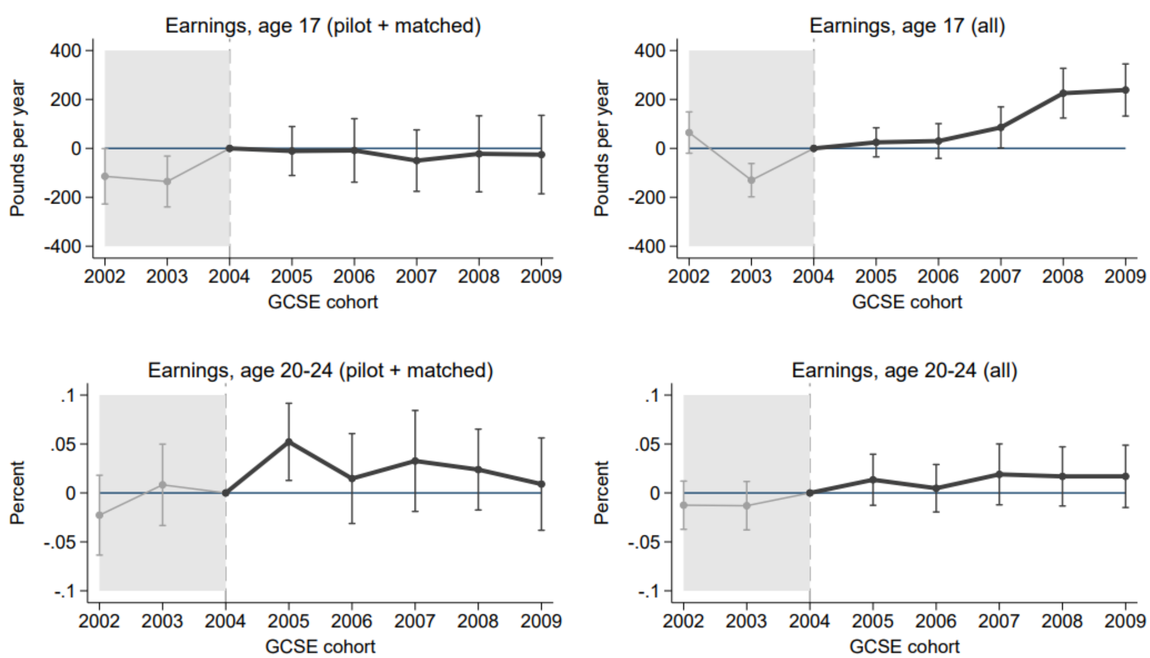
Figure 2: Reverse difference-in-differences event studies (education and training)



Note: The solid black line is used to assess common trends between treatment and control areas (post-treatment, due to our reverse difference-in-differences design). The bars show confidence intervals at the 95% confidence levels computed clustering standard errors at the LA level.

Figures 2 and 3 show event studies in key outcomes of interest around the implementation of the national rollout, for education and earnings outcomes respectively. In each case, event studies are presented for two of the samples discussed in Section 5.1: the combined "pilot + matched areas" and the whole of England ("all" areas). In most cases, the post trends look reassuringly parallel for both samples. However, the charts suggest that in some cases, such as earnings at age 17, the smaller samples in the pilot + matched areas may provide a more reliable estimates than the whole of England. For this outcome, there is evidence of a differential trend in the post-period across all areas, but not for the pilot + matched areas.[28]

Figure 3: Reverse difference-in-differences event studies (earnings)

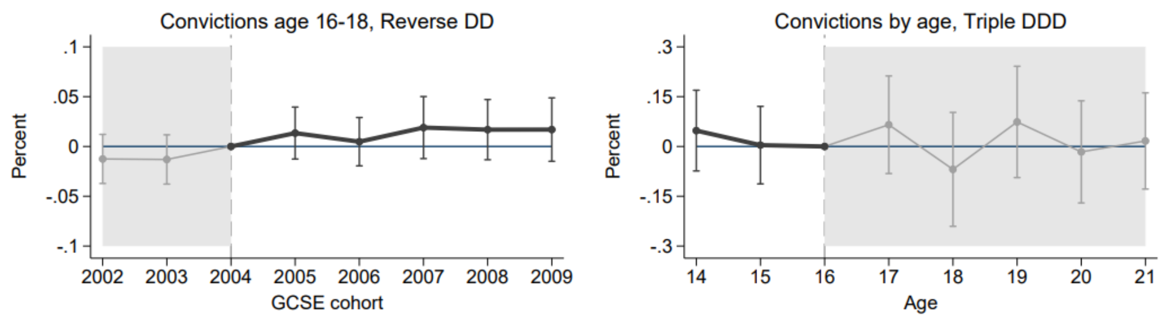


Note: The solid black line is used to assess common trends between treatment and control areas (post-treatment, due to our reverse difference-in-differences design). The bars show confidence intervals at the 95% confidence levels computed clustering standard errors at the LA level.

Figure 4 shows trends for crime outcomes across the two different estimation strategies we use. The first, in the left-hand panel, uses the same methodology as those in Figures 2 and 3, with the outcome of interest being the probability of receiving a conviction between the ages of 16 and 18. for crimeDue to a lack of statistical power for criminal outcomes, we

[28]One potential driver of this is the introduction of the National Minimum Wage for 16/17 year-olds in 2004. Although this was a national policy, the minimum wage may have had different bite in different areas.

Figure 4: Conviction event studies



Note: The solid black line is used to assess common trends between treatment and control areas (post-treatment in the left hand plot; pre-treatment in the right hand plot). Both plots use data from the "all areas" sample. The bars show confidence intervals at the 95% confidence levels computed clustering standard errors at the LA level.

only use the sample with all areas for crime, but the post-trends appear to be parallel.

The right-hand panel shows some suggestion of non-parallel trends, with teenagers in early rollout areas having different trends in their probability of conviction before age 16 than those in later rollout areas. This disappears in the triple difference-in-differences, where changes across cohort are also taken into account. The estimates from the triple difference-in-differences may therefore be more reliable, though they are less precise.

6 Results

6.1 Education participation

Table 2 presents estimates of the impact of the EMA on education participation and related outcomes in Year 12 and Year 13, the first and second years of post-compulsory education respectively. We estimate effects in three samples: (i) the original pilot areas from the 1999 evaluation, (ii) an expanded sample including pilot areas plus matched comparison areas with similar characteristics, and (iii) the full set of areas across England.

The results indicate moderate positive effects on full-time education participation, particularly in Year 12. The estimates suggest a statistically significant increase of between 2 and 2.5 percentage points in Year 12 participation, representing a 4-5% increase. The Year 13 effects are positive but somewhat smaller in magnitude, of around 1.1-1.3 percentage points. The increased participation in full-time education comes mainly through enrolment in Further Education colleges, with coefficients of 1.8-2.1 percentage points in Year 12, while school enrolment changes are close to zero.

Table 2: Impact of the EMA on the main economic activities of 16-18 year olds

	Year 12			Year 13		
	(1) Pilot Areas	(2) Pilot + Matched	(3) All Areas	(4) Pilot Areas	(5) Pilot + Matched	(6) All Areas
Full-time Education	1.61	2.45**	2.06***	1.21	1.11	1.26***
	(1.36)	(0.97)	(0.52)	(1.26)	(0.81)	(0.48)
Mean of dep. var.	49.9	50.7	53.5	37.4	38.0	39.4
School	-0.31	0.37	0.29	-0.14	0.19	0.40
	(1.00)	(0.53)	(0.45)	(0.72)	(0.41)	(0.38)
Mean of dep. var.	9.9	13.0	17.3	5.8	7.9	10.8
Further Education	1.94	2.09**	1.78***	1.35	0.92	0.85
	(1.38)	(0.87)	(0.58)	(1.40)	(0.83)	(0.54)
Mean of dep. var.	40.0	37.7	36.1	31.5	30.1	28.7
Part-time Education	-0.77	-0.64	-0.11	-0.66	-0.56	-0.33
	(0.49)	(0.58)	(0.29)	(0.65)	(0.34)	(0.30)
Mean of dep. var.	6.1	6.4	5.6	7.3	7.8	7.3
Training	0.13	-1.26*	-0.82**	-0.47	-1.54**	-1.06***
	(0.98)	(0.70)	(0.37)	(0.87)	(0.63)	(0.32)
Mean of dep. var.	19.1	18.3	14.9	19.1	18.6	15.9
No. of obs.	35,940	105,546	302,735	35,940	105,546	302,735
No. of clusters	18	50	149	18	50	149
NEET	-1.45	0.06	-1.08**	0.93	1.72**	0.23
	(1.58)	(0.83)	(0.49)	(0.91)	(0.74)	(0.42)
Mean of dep. var.	21.6	20.3	21.5	27.5	26.5	27.5
Full-time work	-0.38	-0.18	-0.13	-1.68***	-0.66*	-0.12
	(0.31)	(0.18)	(0.11)	(0.57)	(0.36)	(0.20)
Mean of dep. var.	1.2	1.4	1.8	6.0	6.2	7.0
No. of obs.	24,295	71,660	204,575	31,915	94,080	267,980
No. of clusters	18	50	149	18	50	149
FSM-eligible only	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
LA FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: All values are multiplied by 100 so they can be interpreted as percentage point changes. Year 12 is the first post-compulsory academic year (when most students turn 17). NEET = Not in Employment, Education or Training. Full-time work applies only to those who are not in education or training, and earn over the LEL, but it is constructed using LEO instead of YPMAD, and so is not constructed using the exact same population. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

As outlined in Section 3, if marginal students are drawn from potentially productive alternatives like training rather than inactivity, we might not expect positive long-term effects even if participation increases. The estimates in column (2) suggest that of the marginal individuals induced to stay in full-time education in Year 12, around 50% would have done work-based training had they not been eligible for the EMA, and roughly a quarter each would have been in part-time education and full-time work, with no effects on the number of NEETs in Year 12. Columns (1) and (3) suggest slightly higher shares drawn into full-time education from NEET. However, estimates from all three samples show an *increase* in NEET shares in Year 13, which suggests at best no overall improvement in the share of NEETs across both years.[29] For Year 13, the small increases in full-time education also appear to come primarily from individuals who would otherwise have been in training.

6.1.1 National rollout versus pilot estimates

Our point estimates for the education participation effects of the EMA are considerably smaller than estimates from evaluations of the 1999 pilot phase of the programme (Dearden et al., 2009; Middleton et al., 2005). For example, Dearden et al. (2009) find a 6.7 percentage point increase in full-time Year 12 participation among those eligible for the full EMA, and evidence of even larger effects in Year 13. In both cases, the difference with our results is statistically significant at the 5% level (assuming independent samples).

This does not seem to be explained by the specific areas the pilot was conducted in: we actually get slightly smaller effects on full-time participation in Table 2 when we use the same set of areas as used by Dearden et al. (2009) as we get in the other areas. However, there are several other potential explanations. First, while we employ a difference-in-differences approach, the pilot evaluations used cross-sectional comparisons, which may be subject to bias. Second, the pilot evaluations were in 1999, five years before the national rollout of the policy in 2004, which is the variation that we are using. During that period, the amount of cash support available was not increased,

[29] This is consistent with national statistics, which suggest a high and stagnant NEET rate amongst 16-18 year olds throughout the 2000s (Public Health England, 2014). The increase in Year 13 is plausibly driven by the increase in full-time education participation, suggesting the policy may have delayed, rather than prevented periods of inactivity.

meaning it declined in real value and may have impacted its efficacy.

Third, the survey data used in pilot evaluations may capture participation differently to our administrative data. Part of this could be error in survey responses; in particular, responses may have been influenced by the existence of the EMA itself, which may have biased estimates in Dearden et al. (2009) upward.[30] However, surveys might also partially capture actual engagement in education rather than just enrolment. For example, a student who enrolled on an education course but stopped attending midway through may appear as in education in the administrative data but as NEET in the survey data. If the EMA reduced instances of enrolling and then dropping out, it would be detectable in the survey data but not the administrative data. This would be consistent with Dearden et al.'s (2009) finding that around three-quarters of the increase in participation came from the NEET population, while our estimates suggest much smaller reductions in NEET shares. Importantly, when we look at qualifications attempted (see Appendix Table A5) we find only a small impact on basic qualification attempts. This suggests that any additional participation effects of the EMA that we are missing did not translate into more people attempting the exams which may lead to higher qualifications.

6.2 Short-term earnings effects

Before turning to attainment and longer-term outcomes, we consider short-term effects of the policy on employment. As argued in Section 3, this is a potentially important mechanism through which the EMA could affect attainment and longer-term outcomes. Table 3 examines the short-run impact of the EMA on employment outcomes in the tax year in which those in a given school year group turn 17. We analyse two measures of employment: annual earnings (including zeros) and the proportion of individuals earning above the Lower Earnings Limit (LEL), the threshold at which employees were legally required to be registered in the tax system. While it is more conventional to study employment directly, our data only contains information on annual earnings. At age 17, differences in earnings likely primarily reflect differences in hours worked, so earnings

[30] In practice, many "full-time" courses for 16-18 year-olds in this period were less intensive than compulsory schooling and included several free periods per week. This may have led some students in full-time education to mistakenly report that they were in part-time education. Since the EMA was only paid to individuals in full-time education, this misreporting was plausibly less likely among EMA recipients.

can serve as a proxy measure of employment.

Table 3: Impact of the EMA on employment of 16-18 year olds

Outcome	(1) Pilot Areas	(2) Pilot + Matched	(3) All Areas
Annual earnings	-206**	-152***	-61
	(95)	(55)	(39)
Mean of dep. var.	£2,118	£2,198	£2,416
% earning above LEL	-1.86*	-1.08**	-0.80***
	(0.88)	(0.47)	(0.27)
Mean of dep. var.	12.47	12.72	14.07
No. of obs.	31,915	94,080	297,575
No. of clusters	18	50	149
Annual earnings (if in FT Ed.)	-10	-146***	-113***
	(85)	(47)	(34)
Mean of dep. var.	£1291	£1,332	£1,539
No. of obs.	13,500	40,640	126,755
No. of clusters	18	50	149
FSM-eligible only	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
LA FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes

Note: Effects of eligibility for the EMA on labour market outcomes between April of Year 12 and April of Year 13 (when students are between ages 16 and 18, with all students aged 17 on 31 August of that year). Earnings conditional on FT Education are based on Educational status in Year 13, covering the last seven months of the earnings period. Monetary values expressed in 2023/24 prices. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

The results show that the EMA reduced annual earnings by around £130 in the pilot and matched areas, representing approximately an 8% reduction. Effects sizes are larger (but less precise) for the pilot areas only and smaller for the whole of England. We include individuals with zero earnings in this analysis, which is important given that only around one in eight individuals in our sample have earnings above the LEL. This approach allows us to capture both intensive and extensive margin responses to the programme.

We also examine the impact on the proportion of young people earning above the LEL. The estimates indicate that the EMA reduced the proportion earning above the LEL by 1-2 percentage points, from a baseline of 12-14%.

Finally, we consider the impacts on the earnings of individuals alongside studying. Our theoretical framework suggests that this is a potentially important channel for future outcomes, due to two competing effects: reduced work could allow more time for studying and improve educational and hence labour market outcomes, but less early work experience could harm later labour market prospects. The estimates in columns (2) and (3) suggest that the EMA caused people to work less alongside their studying, as average earnings among full-time students dropped by around 10%.[31]

Unfortunately, these estimates are not pure causal effects of the EMA on those who would have stayed in education even without it, but also incorporate the compositional effect of the programme by encouraging more young people to remain in full-time education. However, since we estimate that only around 5% of those in education were induced to stay by the EMA, the compositional effect could only account for half the estimated effect even in the extreme case where all of those induced into full-time education by the EMA had zero earnings. It thus seems likely that the observed reduction in part-time earnings was primarily driven by fewer part-time hours worked by those who would have remained in full-time education even in the absence of the EMA.

6.3 Attainment

Given the positive effects on participation and reductions in employment, there are several channels through which the EMA might have improved qualifications obtained or – for those students remaining on an academic track – improved performance in nationally standardised exams. Table 4 examines the impact of the EMA on various levels of educational attainment.

[31] We observe no significant effect in the pilot areas, suggesting that the estimated effects on earnings in those areas were driven by those not working alongside study. This is consistent with the result that more people were substituting away from work into education in these areas.

Table 4: Effect of the EMA on qualifications

	(1) Pilot Areas	(2) Pilot + Matched	(3) All Areas
A. Qualifications obtained from age 16/17 to age 22/23			
Basic education	-0.26	0.56	1.03***
	(0.87)	(0.51)	(0.32)
Mean of dep. var.	14.85	13.91	12.68
Intermediate level	0.51	-0.03	0.62*
	(0.91)	(0.61)	(0.35)
Mean of dep. var.	23.11	23.28	21.68
High school (or equiv.)	0.81	-0.31	-0.44
	(0.65)	(0.62)	(0.42)
Mean of dep. var.	21.99	21.11	19.51
Passing academic track	0.21	0.03	-0.25
	(0.52)	(0.40)	(0.28)
Mean of dep. var.	7.12	8.04	9.18
University enrolment	1.03*	0.10	-0.24
	(0.54)	(0.40)	(0.33)
Mean of dep. var.	14.32	15.18	15.87
University degree	0.17	-0.51	-0.61*
	(0.44)	(0.46)	(0.31)
Mean of dep. var.	10.87	11.79	12.29
No. of obs.	35,925	105,535	302,700
No. of clusters	18	50	149
B. Age 18 attainment (for those on academic track)			
Standardised score	8.45	0.81	-1.11
	(6.38)	(3.36)	(1.72)
No. of obs.	6,110	19,480	65,720
No. of clusters	18	50	149
FSM-eligible only	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
LEA FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes

Note: All values are multiplied by 100 so can be interpreted as percentage point changes. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

The first three sets of estimates consider the shares of individuals achieving “basic”, “intermediate” and “high school (or equivalent)” qualifications by age 23. Basic qualifications are referred to as “Level 1” in England and are considered equivalent to receiving a basic pass at GCSE or equivalent vocational qualifications. Intermediate qualifications are referred to as "Level 2" in England and are equivalent to a good pass (grade C or above) at GCSE or equivalent vocational qualifications. High school level qualifications are called "Level 3" qualifications in England and consist of having passed A-Levels or an equivalent vocational qualification. Passing the academic track requires achieving two A-Levels or equivalent Level 3 qualifications.

The results suggest that if the EMA had any positive effect on qualifications, it was concentrated in basic qualifications. The fact that we only see significant effects on low-level qualifications in the full sample could simply be due to a lack of statistical power in the matched samples, or it could indicate a spurious result due to differential trends across areas.[32] We find no consistent evidence of impacts on intermediate or higher-level qualifications. As seen in the bottom panel, we also find no statistically significant impact on nationally standardised school-leaving exams (A-Levels) among the roughly one fifth of individuals in our sample who continued in academic education, suggesting there was no impact on individuals who would have stayed in education regardless of the EMA.[33]

Finally, we estimate whether the EMA had an impact on progression to and graduation from university. Again, we find no evidence of a systematic impact. This is consistent with our other findings: the increased participation we documented earlier was concentrated in Level 1 courses, which do not typically lead to university entry. Moreover, the lack of improvement in A-Level performance means there was also no improvement in university-readiness as a result of the EMA.

6.4 Medium and long-term labour market outcomes

We evaluate medium and long-term labour market outcomes. Table 5 presents estimates of the impact of the EMA on earnings and benefit receipt up to age 28. Looking first at earnings, we find negative effects on cumulative earnings of around 3% in both the

[32]Table A5 of the Appendix shows that the effect on attempted qualifications is very similar to the effect on obtained qualifications, suggesting the increase in obtained qualifications is primarily a result of the increase in participation in low-level courses, rather than an increase in attainment conditional on participation.

[33]One might be concerned that these results might be distorted by composition effects, but we see no change in the share of people taking A-level exams. Another potential concern is that standardising exam results within cohort would have attenuated the estimated effect. Given that the EMA was rolled out to areas home to around half of all students in 2004, and around half of those were in fact eligible for the EMA, our estimates for the full sample are likely attenuated by around a quarter.

medium-term (ages 20-24) and longer-term (ages 24-28). These effects are statistically significant in the medium-term case, and when the two periods are aggregated together. We also find a small increase in the proportion of individuals claiming out-of-work benefits in both periods, of around one percentage point in the matched and full samples. These results are surprising, as they suggest the EMA may have *worsened* long-run labour market outcomes among eligible students, despite increasing their participation in post-compulsory education.

6.5 The effects of the EMA on crime

Table 6 estimates the short- and long-run impact of the EMA on criminal convictions. We present two sets of estimates: a ″Reverse DD″ using all areas in England, consistent with the difference-in-difference estimates in previous tables; and a ″Triple DDD″ which exploits variation across area, age and cohort (exploiting the fact that earlier cohorts were ineligible in some areas). Overall, the two identification strategies give reassuringly similar results.

Columns (1) and (2) estimate a negative effect of the EMA on crime amongst 16-18 year-olds, which is not statistically significant. The magnitude is a drop of 0.1 percentage points (2% of the baseline mean) per year. Columns (3) and (4) suggest slightly larger effects of the EMA at later ages with falls in convictions of around 0.15 percentage points (4% of the baseline mean). Using our Reverse DD estimation the effect on the long-term is significant at the 90% confidence level. We do not find statistically significant effects on drugs or thefts.

Table 5: Effect of the EMA on labour market outcomes

Outcome	(1) Pilot Areas	(2) Pilot + Matched	(3) All Areas
log(Total earnings), age 20-24	-3.18	-3.21*	-1.97**
	(2.69)	(1.64)	(0.96)
Mean of dep. var.	£40,237	£43,339	£46,757
No. of obs.	25,100	75,225	217,275
log(Total earnings), age 25-28	-3.20	-2.56	-1.58
	(2.03)	(1.89)	(1.02)
Mean of dep. var.	£45,271	£48,208	£51,932
No. of obs.	24,200	72,825	210,650
log(Total earnings), age 20-28	-3.87	-3.58*	-2.67**
	(2.81)	(1.97)	(1.03)
Mean of dep. var.	£85,259	£91,034	£98,011
No. of obs.	27,735	82,655	238,220
Any out-of-work benefit, ages 20-24	0.28	1.31*	0.72*
	(1.00)	(0.68)	(0.37)
Mean of dep. var.	48.16	45.41	41.17
No. of obs.	35,925	105,535	302,700
Any out-of-work benefit, ages 25-28	0.84	1.43**	0.58
	(0.95)	(0.64)	(0.40)
Mean of dep. var.	39.08	36.66	33.14
No. of obs.	35,925	105,535	302,700
Any out-of-work benefit, ages 20-28	0.63	1.43**	0.85**
	(1.07)	(0.64)	(0.39)
Mean of dep. var.	53.52	50.91	46.53
No. of obs.	35,925	105,535	302,700
No. of clusters	18	50	149
FSM-eligible only	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
LA FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes

Note: Sample for log(Total earnings) is all individuals earning over £1,000 in the given period. Ages refer to the fiscal year (April-April) in which the individual is the given age on 31st August. Monetary values expressed in 2023/24 prices. Individuals are counted as having an out-of-work benefit spell if they received out-of-work benefits for more than six months of any given fiscal year. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

Table 6: Impact of the EMA on criminal convictions

	Ages 16-18		Ages 19-28	
	(1) Reverse DD	(2) Triple DDD	(3) Reverse DD	(4) Triple DDD
% Convicted	-0.097	-0.109	-0.145*	-0.164
	(0.137)	(0.143)	(0.078)	(0.119)
Mean of dep. var.	4.290	4.290	3.525	3.525
% Drug conviction	0.016	0.036	-0.005	0.021
	(0.044)	(0.048)	(0.023)	(0.033)
Mean of dep. var.	0.435	0.435	0.515	0.515
% Theft conviction	-0.020	0.024	-0.025	0.036
	(0.068)	(0.070)	(0.030)	(0.061)
Mean of dep. var.	1.692	1.692	0.735	0.735
No. of obs.	605,478	1,210,956	3,027,390	3,632,868
No. of clusters	149	149	149	149
FSM-eligible only	Yes	Yes	Yes	Yes
Post-16 ages only	Yes	No	Yes	No
Age FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	No	Yes	No
LA FE	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Individual controls	Yes	No	Yes	No
Age × Cohort FE	No	Yes	No	Yes
Age × LA FE	No	Yes	No	Yes

Note: All values are multiplied by 100 so they can be interpreted as percentage point changes. Figures represent the average effect on the outcome in a given academic year within the age period, rather than the cumulative effect across the whole period. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

6.6 Heterogeneity

Table 8 examines heterogeneity in the impact of the EMA across gender, special educational needs (SEN) status, and prior educational attainment using the "pilot + matched" sample.[34] The participation effects are notably larger among boys, students with SEN, and those with low prior attainment. For boys and SEN students, the increased participation appears to come primarily from reduced participation in training programmes. In contrast, those with low prior attainment seem to have been drawn from

[34]More detailed heterogeneity can be found in Appendix Tables A6 and A7.

not being in education, employment or training.

Table 7: Effect of the EMA on education and labour market outcomes by gender, SEN and prior attainment

			Prior Attainment		
	(1)	(2)	(3)	(4)	(5)
	Boys	SEN	Low	Medium	High
FT Education, Year 12	3.11***	4.22**	4.12***	2.68**	2.35**
	(1.06)	(1.55)	(1.52)	(1.34)	(1.09)
Mean of dep. var.	46.1	38.1	25.4	50.3	82.4
No. of obs.	52,990	32,020	25,515	53,776	24,387
Training, Year 12	-1.83**	-1.78	-0.79	-1.60*	-1.15*
	(0.82)	(1.23)	(1.55)	(0.87)	(0.64)
Mean of dep. var.	20.5	22.0	24.7	20.0	7.0
No. of obs.	52,990	32,020	25,515	53,776	24,387
NEET, Year 12	0.47	-0.74	-0.47	0.16	-0.62
	(0.96)	(1.28)	(1.42)	(1.03)	(0.66)
Mean of dep. var.	21.6	25.9	35.5	19.0	5.9
No. of obs.	36,765	20,850	17,695	36,095	17,765
Basic education qualifications	0.84	0.35	1.55	0.44	-0.50
	(0.64)	(0.94)	(1.24)	(0.68)	(0.56)
Mean of dep. var.	15.9	22.0	23.3	13.3	4.3
No. of obs.	52,985	32,020	25,510	53,775	24,385
Earnings, age 17 (£)	-217**	-53	-121	-70	-382***
	(96)	(83)	(95)	(73)	(107)
Mean of dep. var.	£2,460	£1,771	£1,532	£2,400	£2,503
No. of obs.	48,500	27,970	21,725	48,285	22,580
log(Total earnings), age 20-28	-1.77	-4.97	-3.67	-1.76	-2.90
	(2.28)	(3.82)	(4.46)	(2.15)	(2.58)
Mean of dep. var.	£102,374	£64,829	£50,037	£90,180	£138,657
No. of obs.	43,685	22,370	16,325	43,380	21,835
No. of clusters	50	50	50	50	50
FSM-eligible only	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
LA FE	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes

Note: Sample for log(Total earnings) is all individuals earning over £1,000 in the given period. All values except for those in £ are multiplied by 100 so they can be interpreted as percentage point changes. Monetary values expressed in 2023/24 prices. Year 12 is the academic year in which nearly all students turn 17. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

The short-run earnings effects are more pronounced for boys and high-achievers, though in the longer term, the negative effects on earnings are similar across groups. For crime, we find statistically significant and large reductions in convictions of around 7% per year among those with low prior attainment.

Table 8: Effect of the EMA on crime by gender, SEN and prior attainment

			Prior Attainment		
	(1)	(2)	(3)	(4)	(5)
	Boys	SEN	Low	Medium	High
% Convicted, age 16-18	-0.145	-0.304	-0.665*	0.098	-0.041
	(0.218)	(0.340)	(0.397)	(0.139)	(0.095)
Mean of dep. var.	7.069	7.426	10.149	2.999	0.729
No. of obs.	305,408	187,754	132,996	304,068	158,164
% Convicted, age 19-29	-0.184	-0.139	-0.495**	-0.099	-0.071
	(0.139)	(0.187)	(0.229)	(0.087)	(0.055)
Mean of dep. var.	5.822	5.923	7.273	2.951	0.813
No. of obs.	1,527,040	938,770	664,980	1,520,340	790,820
No. of clusters	149	149	149	149	149
FSM-eligible only	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
LA FE	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes

Note: All values are multiplied by 100 so they can be interpreted as percentage point changes. Figures represent the average effect on the outcome in a given academic year within the age period, rather than the cumulative effect across the whole period. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

7 Robustness

We begin by estimating the effects of the introduction of the EMA on all students - including those who were ineligible for the EMA - rather than only those eligible for free school meals. These additional results enable us to assess whether our results are specific to the most disadvantaged subset of those eligible for the EMA. The estimates are in column (1) of Tables 9 and 10. The coefficients for education participation and for earnings at age 17 are the same sign and around half the size of the equivalent estimates for the FSM-eligible subpopulation. This suggests that the effects for those eligible for the EMA but *not* eligible for FSM were similar to the effects for those eligible for the EMA *and* FSM.[35] However, this does not hold for the longer-run effects on earnings and criminal

[35]For education participation, the headline estimates for the FSM-eligible, which account for around 15% of the overall population are 2.5ppts. Given the effect for all students is 1.5ppts, we can back-out an estimate for

convictions. In those cases, the effects become much smaller and insignificantly different to zero for the whole sample. The implications are that while the initial effects on participation and working at age 17 hold up for the whole EMA cohort, the small negative effects on longer-run earnings and on criminal convictions are specific to the more deprived subset of FSM-eligible individuals that we focus on for our headline results.

Next, we assess whether our results are sensitive to the choice of pre- and post-treatment cohorts. In column (2) of Tables 9 and 10 we show that the results are not affected by the exclusion of the 2004 cohort, the first post-treatment cohort (which we replace with the 2006 cohort in this column). This set of results suggest that dynamic treatment effects - which are a potential challenge to identification in the reverse difference-in-difference setting - are not a major concern in our context. This is probably because by 2004, the EMA was a well-established policy, meaning the administration of the award was relatively seamless in the national rollout, while students were also very likely to be aware of its availability to them.[36]

In column (3), we assess the robustness of our results to different assumptions on parallel trends. While we do not find much evidence of non-parallel post-trends in our outcomes of interest, tests for post-trends often have limited power. To that end, we run a synthetic version of the reverse difference-in-differences estimation, following Arkhangelsky et al. (2021). In this specification, we apply different weights to different areas across the whole country, such that cohort-by-cohort trends in the outcome of interest in the post-treatment period are parallel by construction. Results for this specification are very similar to the headline results, suggesting that our results are unlikely to be driven by differential trends in outcomes.

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the 45% of individuals who were eligible for the EMA but not FSM under the assumption of no impact of the policy on those who are ineligible for the EMA. Assuming effects are half as large for the 20% of students who were eligible for the partial EMA award, the weighted average effect would be $(2.5 * 0.15) + (x * 0.25) + (0.5 * x * 0.2) = 1.5 \Rightarrow x = ((0.015 - (0.025 * 0.15))/0.35 = 3.2$, where x is the implied effect for those eligible for the EMA but not FSM.

[36]The take-up figures appear to align with this observation (see Table A1 in the appendix). In particular, there is no drop in uptake numbers coinciding with the national rollout, suggesting that students in national rollout areas were well informed about the EMA's existence.

Table 9: Robustness: education and labour market

	(1) All students	(2) 2004 excluded	(3) Synthetic DiD	(4) More generous variants excluded
FT Education, Year 12	1.48**	3.18***	2.40***	2.63**
	(0.65)	(1.08)	(0.65)	(1.07)
Mean of dep. var.	65.5	54.2	50.7	50.7
Training, Year 12	-0.65*	-1.71**	-0.80**	-1.57**
	(0.35)	(0.78)	(0.33)	(0.78)
Mean of dep. var.	14.5	18.3	14.3	18.3
No. of obs.	663,468	104,410	594,181	90,393
No. of clusters	50	50	143	42
NEET, Year 12	-0.52	-0.08	-0.93*	0.45
	(0.35)	(0.90)	(0.39)	(0.50)
Mean of dep. var.	12.5	20.3	21.4	20.3
No. of obs.	472,855	70,865	670,570	61,255
Earnings, age 17 (£)	-89*	-150**	-182***	-134**
	(51)	(69)	(44)	(59)
Mean of dep. var.	2,728	2,198	2,454	2,198
No. of obs.	617,820	93,285	743,105	80,560
log(Total Earnings), age 20-24	-0.24	-4.08**	-1.21	-1.59
	(0.49)	(1.99)	(1.06)	(1.70)
Mean of total earnings (£)	61,289	43,339	47,138	43,339
No. of obs.	556,775	74,840	667,380	64,640
No. of clusters	50	50	143	42

Note: All values except for those in £ are multiplied by 100 so they can be interpreted as percentage point changes. Monetary values expressed in 2023/24 prices. Earnings from 20-24 used no post-trends available to reweight on when earnings up to 28 used. Year 12 is the academic year in which nearly all students turn 17. Areas omitted in column (4) are Oldham, Nottingham, Gateshead, Stoke-on-Trent, Birmingham, Leicester, South Tyneside and Wigan. The 4 smallest LAs are dropped from the synthetic DiD. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

Table 10: Robustness: crime

	(1) All students	(2) 2004 excluded	(3) Synthetic DiD	(4) More generous variants excluded
% Convicted, age 16-18	0.002	-0.128	-0.104	-0.133
	(0.039)	(0.138)	(0.168)	(0.141)
Mean of dep. var.	1.777	4.290	4.26	4.290
No. of obs.	4,299,952	599,484	1,188,362	540,174
% Convicted, age 19-29	0.025	-0.105		-0.184**
	(0.026)	(0.081)		(0.081)
Mean of dep. var.	1.568	3.525		3.525
No. of obs.	21,499,760	2,922,577		2,700,870
No. of clusters	149	149	146	141
FSM-eligible only	No	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
LA FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	No	Yes

Note: All values are multiplied by 100 so they can be interpreted as percentage point changes. Convictions results represent the average effect on the outcome in a given academic year within the age period, rather than the cumulative effect across the whole period - age 19-29 results are missing for the synthetic DiD due to a lack of post-trends to reweight on. Areas omitted in column (4) are Oldham, Nottingham, Gateshead, Stoke-on-Trent, Birmingham, Leicester, South Tyneside and Wigan. The 4 smallest LAs are dropped from the synthetic DiD. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

Finally, in column (4) we test the robustness of our estimates to the exclusion of areas where different variants of the EMA were implemented prior to 2004. Before 2004, some areas differed in the size of their weekly payment, the size of their attainment bonuses and their eligibility thresholds. In 2004, at the same time as the national rollout of the EMA, the variants were standardised into one version of the programme. In column (4), we exclude the eight areas that differed in the size of their weekly payment or bonuses from the standard £30 per week in years prior to 2004. Again, the estimates are very similar to our headline specification, suggesting that the small changes in the treatment intensity was not an important driver of our results.

8 Discussion

8.1 Participation effects of the EMA

Our findings indicate modest effects of the EMA on education participation, suggesting the program did not relieve binding constraints for most students or substantially alter education decisions at the margin. The relatively small participation response could reflect several factors. First, parents may have reduced their financial support in response to the EMA, partially crowding out the transfer. Second, and perhaps more importantly, the UK already provided substantial support for students remaining in post-16 education during this period. Education was free, travel costs were often subsidised, and low-income families with children in full-time education were eligible for various benefits including Child Benefit, Child Tax Credit, and Income Support. This existing support system meant many students who wanted to continue their education likely could already do so without major hardship for them or their families. This may help explain why similar conditional cash transfer programs in developing countries, where there is typically no or little state financial support available for families with children in education, typically show much larger participation effects.

8.2 Attainment effects

Despite increasing participation and reducing part-time work, we find no meaningful impact of the EMA on educational attainment. We find no evidence of an improvement in educational outcomes among students who would have stayed in education even without the EMA, suggesting the program's attendance requirements and the associated reduction in part-time work did not boost academic performance. The lack of effect on attainment suggests that modest amounts of part-time work do not significantly detract from academic achievement.

There is tentative evidence of a positive effect on basic qualifications, consistent with the larger participation effects we observe among students without these qualifications (Table 8). However, these gains are modest and do not appear to translate into improved outcomes at higher qualification levels.

8.3 Long-run labour market effects

Perhaps most strikingly, we find evidence of small negative effects of the EMA on earnings through individuals' twenties. The most compelling explanation for this pattern is that the EMA crowded out valuable early labour market experience. The broad scope of this effect - impacting both marginal and infra-marginal students - helps explain the size of the overall earnings penalty we observe. Our findings suggest that even modest amounts of teenage employment may help some pupils develop workplace skills, connections and attachment to the labour market that boost long-run earnings. The negative effect on longer-term labour market outcomes is notable because it does not appear to extend to the slightly less deprived group of students who were eligible for the EMA but not for FSM. This suggests that workplace skills are particularly valuable for students coming from the most deprived backgrounds.

8.4 Cost effectiveness

To assess cost-effectiveness, we first consider the direct programme costs: the government spent approximately £2,200 in cash transfers per eligible student, based on observed education participation rates. However, the full cost-benefit calculation must account for several additional factors. The government faced further costs from providing additional education, administration costs, lost tax receipts, and increased welfare payments, though likely realised some savings from reduced criminal activity. Students benefited directly from the cash transfers and increased welfare receipts, while society benefited from reduced crime.

The programme's impact on earnings requires careful interpretation. Reduced earnings during the program period might represent students choosing more leisure time due to the income effect of the transfer. However, reduced earnings in later years more likely reflect lower productivity, possibly due to lost early work experience.

Table 11 presents our cost-benefit calculations under three scenarios, varying by how we treat reduced earnings. It shows our estimates of government costs, private benefits for individuals and an estimate of the Marginal Value of Public Funds (MVPF), which is equal to the cost-benefit ratio. A pure transfer programme with no other effects would generate an MVPF of one - each pound spent by the government creates one pound of private benefits. Our estimates fall well below this benchmark. If we count all earnings reductions as costs, each pound of government spending generated only 22 pence in private benefits (MVPF = 0.22). Under our preferred interpretation, which counts only post-programme earnings reductions as costs, the MVPF rises to 0.39. Even in the most optimistic scenario, where we ignore all earnings reductions, the MVPF reaches only 0.86, reflecting the additional costs of administration and providing education without corresponding increases in private benefits.[37]

Table 11: Changes in government costs and benefits from the EMA

	Government Costs	Private Benefits	MVPF
All earnings reductions included	£3,080	£670	0.22
Only post-EMA reductions included	£3,080	£1,195	0.39
No earnings reductions included	£3,080	£2,660	0.86

Note: Changes to government costs include the cost of the EMA itself, reductions in tax receipts, increases in welfare spending, and reductions in the cost of processing crime. Private benefits include the transfer itself, increases in welfare spending, reductions in victimhood of crimes, and in some cases, reductions in long-run earnings. Costs and benefits are expressed in 2024 prices.

9 Conclusion

This paper evaluates a large conditional cash transfer program in the United Kingdom, exploiting its staggered rollout across local areas in England. Despite the substantial value of the transfer – around one-third of per-student education spending for the poorest students – we find only modest effects on education participation. These participation effects did not translate into improved qualifications or earnings, implying that the EMA failed to improve upward social mobility for children from the poorest families. Indeed, earnings through people's twenties appear to have fallen slightly as a result of the policy. Although we find evidence that the policy reduced youth crime among individuals with low prior attainment, these effects are small compared to the program's considerable costs.

Our analysis yields three broader insights for education policy. First, conditional cash

[37]These calculations assume benefits and costs cease at the end of participants' twenties and are adjusted for inflation and discounting.

transfers may have limited effectiveness in settings where existing support for education is substantial. Second, such programs can inadvertently crowd out valuable activities such as training or part-time work. Third, the effects of educational interventions may differ substantially between pilot programs and a national rollout, highlighting the importance of evaluation at scale.

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Appendix

A Additional institutional detail

A.1 Other financial support for post-16 education

Young people aged between 16 and 18 in full-time education were classified as dependents, making their parents eligible for various benefits. Child Benefit (£16.50 per week in 2004/05) was a universal payment for parents of children in full-time education, with no means testing. Additionally, low-income families could receive Child Tax Credit, worth up to £41.70 per week per child in education in 2004/05. Together with the EMA, this meant families of eligible students could receive up to around £90 extra per week if a child remained in education, of which the EMA comprised about one-third (although the EMA was the only scheme under which payments were made directly to the student, rather than their parents).[38]

Parents of young people in work-based learning (WBL) or other training were not eligible for these benefits, as their children were classified as independent. However, unpaid WBL participants received a minimum training allowance of at least £40 per week. The reclassification of some forms of WBL as full-time education in 2006 meant that students lost access to the allowance but gained access to the EMA plus the additional family-level support, such as the Child Benefit and Child Tax Credit.

A.2 Crime policy

From 1999 to 2002 the government announced a Crime Reduction Programme which allocated £250 million to a range of crime policies across England and Wales. Several initiatives were implemented across the two nations, like the expansion of CCTV or changes in the processing of young offenders which favoured restorative justice. Two – the Reducing Burglary Initiative (RBI) and the Targeted Policing Initiative (TPI) – were

[38] There were some national reforms to the benefit system in April 2003 that included the introduction of the Child Tax Credit. However, these changes had almost no impact on the additional financial support available for families with children aged 16-18 in full-time education. There was also a notable change to youth labour market regulation during this period with the introduction of a national minimum wage of £3 per hour for 16-17-year-olds in April 2004.

implemented in specific neighbourhoods to tackle high-incidence crime areas. In some cases, the area-specific policies could have coincided with the rollout of the EMA, particularly as the EMA was piloted earlier in more deprived areas, which also tend to have higher crime.

The RBI aimed at lowering domestic burglary rates. It first launched in 1999 in 63 selected high-risk areas and was rolled out until 2002 (Hamilton-Smith, 2004). [39] The grants were provided based on competitive bidding and the projects covered increased police patrols, enhanced street lighting, improved home security, and community engagement programs. Prior work has suggested that the RBI was not effective on its own, but that in conjunction with the EMA there were some declines in local crime (Sabates and Feinstein, 2008).

The TPI provided grants to tackle a variety of crimes. It first launched in 1999 in 10 areas and was later rolled out to reach 59 projects. The grants were provided based on competitive bidding where bidders had to prove they were able to implement targeted problem-solving strategies. Several focused on anti-social behaviour, and others focused on specific crimes, like vehicle offences or hate crime (Bullock and Tilley, 2003).

B Additional tables

Table A1: EMA takeup relative to number of students

School Year	2004/05	2005/06	2006/07	2007/08	2008/09
EMA recipients	297	430	527	546	576
Total student numbers	570	831	942	975	1025
% of students claiming EMA	52%	52%	56%	56%	56%

Note: Numbers expressed in thousands. *Source for recipients*: House of Commons Library (Bolton, 2011)

[39] The areas were meant to cover 3,000 to 5,000 residents – much smaller than LAs – and were defined to cover specific neighbourhoods or police beats (Bowles and Pradiptyo, 2004).

Table A2: List of LA characteristics used for matching

Control
% Black
% Asian
% Not White, Black, or Asian
% First language other than English
% Students eligible for Free School Meals
Mean Key Stage 2 overall points score, standardised
Mean GCSE overall points score, standardised
% Achieving 5 Good Passes at GCSE Income Deprivation Affecting Children Index (IDACI)
% of pop. living in urban areas
% Students staying in education past age 16
% of pop. in higher managerial occupations
% of pop. in lower managerial occupations
% of pop. in routine occupations
% of pop. long-term unemployed
% of pop. in owner-occupied housing
% of pop. in social housing
% of pop. with no qualifications
% of pop. with degree-level qualifications

Note: Key Stage 2 exams are taken at the end of primary school, at age 11.

Table A3: List of LAs in Pilot and Matched areas

Pilot	Pre-2004 areas	National roll-out areas
	Walsall	Redcar and Cleveland
	Doncaster	Rotherham
	Gateshead	Newcastle upon Tyne
	Bolton	Rochdale
	Oldham	Blackburn with Darwen
	Middlesbrough	Stockton on Tees
	Southampton	Portsmouth
	Stoke-on-Trent	Blackpool
	Nottingham	Derby
Matched	**Pre-2004 areas**	**National roll-out areas**
	Ealing	City of London
	Coventry	Croydon
	St Helens	Enfield
	Wirral	Dudley
	Salford	Stockport
	Tameside	Kirklees
	Wigan	Derbyshire
	Leeds	Durham
	Wakefield	Darlington
	North Tyneside	Slough
	Luton	Plymouth
	Leicester	Torbay
	Halton	Thurrock
	Worcestershire	Nottinghamshire
	Cornwall	Cumbria
	Northumberland	Isle of Wight

Table A4: List of characteristics used as controls

Control
White
Black
Asian
Male
First language other than English
Special Educational Needs without statement
Special Educational Needs with statement
Income Deprivation Affecting Children Index (IDACI) quintile
Key Stage 2, English standardised score
Key Stage 2, Maths standardised score
Key Stage 2, Science standardised score

Note: Special Educational Needs statements were given to children with the most severe needs. Other children with special educational needs were known as "without statement". Key Stage 2 exams are taken at the end of primary school, at age 11.

Table A5: Effect of the EMA on highest qualification attempted from age 16/17 to age 22/23

	(1) Pilot	(2) Pilot + Matched	(3) All
Basic education	-0.31	0.49	1.04***
	(0.67)	(0.44)	(0.30)
Mean of dep. var.	11.81	11.17	10.12
Intermediate level	-1.05	0.23	0.35
	(0.93)	(0.60)	(0.37)
Mean of dep. var.	29.93	29.52	28.32
High school (or equiv.)	0.43	-0.75	-0.69
	(1.08)	(0.66)	(0.43)
Mean of dep. var.	32.82	31.97	30.37
No. of obs.	35,925	105,535	302,700
No. of clusters	18	50	149
FSM-eligible only	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
LA FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes

Note: Effects of eligibility for the EMA on labour market outcomes. Earnings at a given age refer to earnings in the fiscal year in which almost all students of a cohort reach that age at the end of an academic year. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

Table A6: Effect of the EMA by gender and SEN

	Gender		SEN	
	(1) Boys	(2) Girls	(3) SEN	(4) No SEN
A. Education and labour market				
Full Time Education, Year 12	3.11***	1.81	4.22**	1.64
	(1.06)	(1.25)	(1.55)	(1.06)
Mean of dep. var.	46.1	55.3	38.1	56.2
Training, Year 12	-1.83**	-0.65	-1.78	-0.96
	(0.82)	(0.89)	(1.23)	(0.68)
Mean of dep. var.	20.5	16.0	22.0	16.6
NEET, Year 12	0.47	-0.39	-0.74	0.43
	(0.96)	(1.11)	(1.28)	(0.88)
Mean of dep. var.	21.6	18.8	24.9	18.0
Earnings, age 17	217**	-79	-53	-198***
	(96)	(55)	(83)	(68)
Mean of dep. var.	£2,460	£1,916	£1,771	£2,382
log(Total Earnings), age 20-28	-1.77	-5.51**	-4.97	-3.45
	(2.28)	(2.26)	(3.82)	(2.29)
Mean of dep. var.	£102,374	£78,844	£64,829	£102,331
No. of obs.	52,990	52,556	23,414	73,523
No. of clusters	50	50	50	50
B. Crime				
% Convicted, age 16-18	-0.145	-0.024	-0.304	0.005
	(0.218)	(0.115)	(0.340)	(0.113)
Mean of dep. var.	7.069	1.440	7.426	2.791
No. of obs.	305,408	300,070	139,316	417,720
% Convicted, age 19-29	-0.184	-0.088*	-0.139	-0.130*
	(0.139)	(0.050)	(0.187)	(0.077)
Mean of dep. var.	5.822	1.168	5.923	2.378
No. of obs.	1,527,040	1,500,350	696,580	2,088,600
No. of clusters	149	149	149	149
FSM-eligible only	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
LA FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes

Note: Sample for log(Total earnings) is all individuals earning over £1,000 in the given period. All values except for those in £ are multiplied by 100 so they can be interpreted as percentage point changes. Monetary values expressed in 2023/24 prices. Year 12 is the academic year in which nearly all students turn 17. Convictions figures represent the average effect on the outcome in a given academic year within the age period, rather than the cumulative effect across the whole period. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.

Table A7: Effect of the EMA by ethnicity

	Ethnicity	
	(1)	(2)
	White	Non-white
Full Time Education, Year 12	2.89**	1.10
	(1.150)	(1.076)
Mean of dep. var.	46.8	66.8
No. of obs.	82,090	23,456
Training, Year 12	-1.53*	-0.39
	(0.85)	(0.99)
Mean of dep. var.	20.1	10.5
No. of obs.	82,090	23,456
NEET, Year 12	0.31	0.03
	(1.04)	(0.22)
Mean of dep. var.	22.2	12.9
No. of obs.	55,420	16,240
Earnings, age 17 (£)	-100**	-185**
	(49)	(70)
Mean of dep. var.	1,799	1,211
No. of obs.	73,575	20,505
log(Total Earnings), age 20-28	-3.58	-2.76
	(2.24)	(3.85)
Mean of dep. var. (£)	69,290	72,496
No. of obs.	64,015	18,645
No. of clusters	50	50
% Convicted, age 16-18	-0.146	-0.018
	(0.170)	(0.201)
Mean of dep. var.	4.485	3.515
No. of obs.	420,534	184,944
% Convicted, age 19-29	-0.113	-0.157
	(0.091)	(0.145)
Mean of dep. var.	3.586	3.279
No. of obs.	2,106,170	924,720
No. of clusters	149	149
FSM-eligible only	Yes	Yes
Cohort FE	Yes	Yes
LA FE	Yes	Yes
Individual controls	Yes	Yes

Note: Sample for log(Total earnings) is all individuals earning over £1,000 in the given period. All values except for those in £ are multiplied by 100 so they can be interpreted as percentage point changes. Monetary values expressed in 2023/24 prices. Year 12 is the academic year in which nearly all students turn 17. Standard errors clustered at the LA level in parenthesis; *,** and *** indicate significance at the 10%, 5%, and 1% level respectively.