Education Subsidies and School Drop-Out Rates

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Abstract

This paper evaluates whether means-tested grants paid to 16- to 18-year-olds are an effective way of reducing the proportion of school drop-outs. We look at this problem using matching techniques on a pilot study carried out in England using a specially designed panel data-set that ensures that valid comparisons between our pilot and control areas are made. The impact of the subsidy is quite substantial, with initial participation rates (at age 16/17) being around 4.5 percentage points higher. Full-time participation rates one year later are found to have increased by around 7.0 percentage points, which is largely due to the grant having a significant effect on retention within post-compulsory education. These effects vary by eligibility group, with those receiving the full payment having the largest initial increase in participation. The extent to which the impact of the policy is due to credit constraints, rather than an unconstrained price effect is unclear. Among those eligible for a full grant, the point estimate of the effect of the policy is larger for renters than for owner-occupiers, but this difference is not statistically different from zero at conventional levels of significance.

JEL Codes: I21, I22

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1. Introduction

Education has been at the centre of policies intended to promote growth both in the developing world and in wealthier countries. It is seen as a key to development and to the ability of a country to keep up with the fast-moving technological change.[1] The recent increase in the returns to education in the US[2] and the UK[3] has reinforced this view. Education is also seen as a way for individuals to escape poverty and welfare (and possibly crime) dependency and this perception has motivated numerous policies worldwide that promote education as a long-run solution to these problems.

The most recent figures (OECD, 2006) show just 70 per cent of 25- to 34-year-olds with upper-secondary education in the UK as of 2003, which is 17 percentage points lower than the corresponding figure for the US despite continuing problems with drop-out rates in some US cities.[4] This compares with over 95 per cent in countries such as Korea and Norway and largely reflects the fact that the proportion of youngsters dropping out of school at the age of 16 and failing to obtain upper-secondary education qualifications in the UK is very high compared with most developed countries.

There has been worldwide focus on school drop-out rates and a number of policies have been devised to help reduce them. One of the key policy changes in most OECD countries after the Second World War was to introduce free secondary-school

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[1] See, among many others, Benhabib and Spiegel (1994), Krueger and Lindahl (2001) and Vandenbussche, Aghion and Meghir (2004).

[2] See Juhn, Murphy and Pierce (1993).

[3] See Gosling, Machin and Meghir (2000).

[4] See OECD (2006), table A1.2a, page 38. In the US, students may drop out of school if they have reached the age set in their state's law for the end of compulsory schooling, which ranges between 16 and 18, but drop-outs are not considered to have completed school and no certificate or award is issued at this stage. The US drop-out rate is just over 11 per cent of secondary-level students aged 16 and older according to the latest US Department of Education figures (see http://www.ed.gov).

education and to increase the compulsory school-leaving age. The timing and pace of these reforms varied tremendously across countries, and in the US the most important reforms actually occurred before the Second World War (see Goldin (1999)). In the UK, fees for state secondary schools were abolished by the Education Act 1944 (the Butler Act) and the compulsory school-leaving age was increased from 14 to 15 in 1946 and then from 15 to 16 in 1974, where it remains today. In the US today, the compulsory school-leaving age ranges from 16 to 18;[5] for the remaining 29 OECD countries, it ranges from 14 to 18.[6]

Making secondary education free and increasing the compulsory school-leaving age had an effect on school drop-out and completion rates, and a number of these reforms have been analysed in previous research.[7] In recent years, a number of countries, both developed and developing, have introduced means-tested grants in an attempt to encourage students to stay in school, rather than simply raising the compulsory school-leaving age.[8] This seems to have become an important policy option that may well become a permanent feature in a number of countries. In the UK, these conditional cash transfers are now an official policy implemented on a national basis since September 2004, following the evaluation results partly reported here.

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[5] Compulsory schooling ends by law at age 16 in 30 states, at age 17 in nine states and at age 18 in 11 states plus the District of Columbia. Source: US Department for Education.

[6] See OECD (2006), table C.1.2, page 266.

[7] See, for example, Goldin (1999), who examines the 1910 to 1940 reforms in the US, Harmon and Walker (1995), who exploit the changes in the compulsory school-leaving age in Britain to estimate the returns to schooling, and Meghir and Palme (2005), who exploit changes in the Swedish secondary education system to estimate the returns to education.

[8] Prominent examples are the AUSTUDY programme introduced in Australia in 1988 for children in their last two years of secondary school (now called Youth Allowance) (see Dearden and Heath (1996)), the PROGRESA programme in Mexico, which covers children from primary school to the end of high school (see Schultz (2004) and Attanasio, Meghir and Santiago (2005)), and the recently introduced Familias en Acción programme in Colombia modelled on PROGRESA (see Attanasio et al. (2005)).

The available evidence on the importance of monetary incentives for educational participation originates mainly from direct modelling of individual choices in the presence of alternative tuition levels, as in Heckman, Lochner and Taber (1999), who also consider the general equilibrium effects of varying such subsidies. Dynarski (2003) examines the impact of incentives for college attendance and completion in the US. There is, however, little direct evidence on the importance of monetary incentives for school participation. The most prominent large-scale example in this line of work is the PROGRESA experiment in Mexico (see Schultz (2004)), which established significant effects particularly for 12- to 14-year-olds. Todd and Wolpin (2003) use the PROGRESA data to test whether using a model with the wage as the opportunity cost of schooling is capable of predicting the impact of a schooling subsidy; they get mixed results. Attanasio, Meghir and Santiago (2005) test directly whether the impact of a wage reduction is equal to that of introducing the subsidy; they reject the hypothesis. These results highlight the importance of direct evidence of the impact of subsidies for policy evaluation.[9]

This paper examines the impact of a programme that subsidises children to remain in school for up to two years beyond the statutory age in the UK – the Education Maintenance Allowance (EMA). The programme was first piloted in a number of areas in England from September 1999. Evaluating such interventions is, of course, critical to the shaping of education policy and the effectiveness or otherwise of a conditional cash transfer to 16- and 17-year-olds in reducing school drop-out rates is of general policy interest to policy-makers worldwide.[10] The evaluation of this

[9] Another related example is by Angrist and Lavy (2004), who use a randomised experiment to assess the sensitivity to monetary incentives for obtaining a high-school graduation certificate in Israel.

[10] There is already evidence that financial aid paid to college students has a significant impact on college attendance and completion. See, for example, Dynarski (2003).

programme provides valuable information on whether such subsidies, which effectively reduce the cost of education, actually reduce school drop-out rates, which at present is the central policy concern.[11]

We find that the impact of the subsidy is quite substantial, especially for those who receive the maximum payment. The subsidy increases the initial education participation of eligible males by 5.0 percentage points and of eligible females by 4.0 percentage points. In the second year, this increases to 6.8 percentage points for eligible males and 7.1 percentage points for eligible females, suggesting that the effect of the policy is to increase not only initial participation but also retention within full-time education. The initial effects are largest for those who receive the maximum payment. We estimate that around a third of individuals who stayed in education were drawn from inactivity rather than work. We also find that the effect of EMA is largest for children with lower levels of prior educational achievement.

Beyond estimating the effects of incentives on educational participation, another key issue is understanding the mechanism by which subsidies operate. A subsidy changes the opportunity cost of education. Thus, even if it is optimal not to participate under the pre-policy environment, it may become optimal to do so post-policy. However, the main mechanism that motivates policy, and indeed the presumption of policy-makers, has been that these low levels of education participation are due to financial constraints rather than being the outcome of an informed choice in an unconstrained environment.[12]

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[11] With respect to dropping out at 16, following the GCSE qualification which is obtained at that age, the then Minister for Lifelong Learning, Margaret Hodge, stated in Parliament: 'The real challenge is to increase the number of young people achieving two A-levels. That comes under our schools agenda – our 14 to 19 agenda. A particular problem is the haemorrhaging of young people, who achieve five A to Cs at GCSE level and then do not stay on to do further education full time' (*House of Commons Hansard Debates* for 5 July 2001, column 391). A recent survey of government policy by Johnson (2004) also highlights this concern.

[12] 'We recognise that for some young people there are financial barriers to participating in education, particularly for those from lower income households' (Department for Education and Skills, *General Information about EMA*, http://info.emasys1.dfes.gov.uk/control.asp?region=partners&page=general).

The desirability of a school subsidy in this case would be much greater because it could improve efficiency. To say something about this, we compare the impact of the grant on those living in owner-occupied housing with the impact on those living in rented accommodation. The parents of the former are unlikely to be liquidity constrained because they are relatively more likely to have access to either financial assets or credit, not least because it is relatively straightforward for them to borrow against the house; whether they are willing to provide the funds to their children, of course, is another matter.[13] We find a larger (9.1 percentage points) and statistically significant impact of the policy on participation in education among those in rented accommodation compared with a smaller (3.8 percentage points) and not statistically significant impact on those in owner-occupied accommodation. However the 4.3 percentage point difference between these two estimates is not statistically different from zero (p value = 12%). Therefore the extent to which the impact of the policy is due to credit constraints, rather than an unconstrained price effect, remains unclear.

The paper proceeds as follows. In section 2, we describe the programme and its variants and describe the data we use to evaluate the programme. In section 3, we discuss the evaluation methodology. We discuss the results in section 4 and offer some concluding remarks in section 5.

2. Background and Data

The Education Maintenance Allowance pilots were launched in September 1999 in 10 local education authorities (LEAs). The scheme paid a means-tested benefit to 16- to

[13] Papers exploring whether or not liquidity constraints are important for educational outcomes include Cameron and Heckman (1998), Carneiro and Heckman (2002), Cameron and Taber (2000), Dale and Krueger (1999) and Keane and Wolpin (1997).

18-year-olds who remained in full-time education after Year 11[14], when education ceases to be compulsory (i.e. after 16 years of age approximately). The benefit could be claimed for up to two years (or three for young people with special educational needs) and could be used to attend any form of full-time post-16 education, whether academic or vocational. In this paper, we look at the effects of the EMA on individuals who first became eligible for the EMA in September 1999.[15]

Four different variants of the EMA were piloted and these are outlined in Table 2.1. The basic EMA (variant 1) was piloted in three urban areas and one rural area. Variants 2, 3 and 4 were all piloted in two urban areas. In each area, the maximum EMA weekly payment (£30 or £40, which was disregarded for the purposes of both income tax and welfare payments) could be received by young people whose parents' incomes were £13,000 or below.[16] The benefit was tapered linearly for family incomes between £13,000 and £30,000, with those from families earning £30,000 receiving £5 per week. No payment was made for families with income in excess of £30,000. In addition, at the end of a term of regular attendance, the child would receive a non-means-tested retention bonus (£50 or £80).[17] The children also received an achievement bonus on successful completion of their course examination. To put these amounts in context, the median net wage among those who opted for full-time work in our sample was £100 per week, corresponding to less than 40 hours' work a

[14] UK compulsory schooling system is based on 12 years: age 4 (reception) through to age 16 (Year 11). Participation at ages 17 and 18 (years 12 and 13) are currently voluntary but are also provided free at the point of use in state institutions and are generally necessary for immediate entry into Higher Education.

[15] We also have data on a second cohort, who became eligible for the payment from September 2000. We have not included this cohort in our analysis as there is a chance that their academic outcomes in Year 11 may have been influenced by the announcement of the programme, whereas this was not true for the first cohort because of the timing of the announcement. We concentrate on urban areas only as it was only in urban areas that all four variants were piloted. Full results for all cohorts and individuals who participated in the pilots are available from the authors.

[16] Income is defined as the taxable income of the biological parents in the previous tax year.

[17] This bonus was paid to the child in all variants (including variant 3).

week. Thus the maximum eligibility for the EMA, depending on the variant, replaces around a third of post-tax earnings.

Table 2.1. The four variants of EMA

Variant	Maximum weekly EMA award	Weekly payment paid to:	Retention bonus (per term)	Achievement bonus
1	£30	Young person	£50	£50
2	£40	Young person	£50	£50
3	£30	Primary carer[a]	£50	£50
4	£30	Young person	£80	£140

[a] Usually the mother.

The programme was announced in the spring of 1999, just before the end of the school year, and the lateness of the announcement means that it could not have impacted on a child's Year 11 examination results.[18] The data used to evaluate the programme are based on initial face-to-face interviews with both the parents and the children and on follow-up annual telephone interviews with the children. The data-set was constructed so as to include both eligible and ineligible individuals in pilot and control areas.[19] The first interview was conducted at the beginning of the school year in which the subsidy became available. In the following year, the same students (but not parents) were followed up using a telephone interview.

We collected a wealth of variables relating to family income and background, childhood events (such as ill health and mobility) and prior school achievement as well as administrative data on the quality of schooling in the child's neighbourhood

[18] This was not true for our second cohort and for this reason they are excluded from the analysis. We feel that it is important to control for student ability and the only measures we have relate to school outcomes in Year 11.

[19] We used data from the British Youth Cohort Studies to choose our control areas so as to ensure that the background characteristics of the control areas in terms of historical education participation, background characteristics of parents and neighbourhood characteristics were as similar as possible to those of the selected pilot areas which we knew in advance.

and other measures of neighbourhood quality measured prior to the introduction of the EMA.[20]

Table 2.2 provides some pre-reform neighbourhood statistics for our pilot and control areas, while Appendix 1 provides definitions of each of these neighbourhood variables.[21] Larger values of these indices point to a greater level of deprivation. For the sake of comparison, we also show the average indices and their standard deviation for the whole of England. Based on this, it is clear that the pilots and controls are in more deprived areas and remarkably close to each other relative to the overall variation in England. The control areas were selected on the basis of similar socioeconomic characteristics, and similar levels and trends of education participation for the 16-18 age group[22]. As can be seen from the table, the characteristics of the treatment and control areas are very similar indeed, with pilot areas tending to be slightly more disadvantaged. Indeed, the (proxy for the) aggregate non-school-participation rate pre-reform is just less than 3 percentage points higher in our pilot areas than in our control areas. This highlights the importance of appropriately weighting our control group because if we do not take this pre-reform difference into account, we are likely to underestimate the EMA impact.

[20] The neighbourhood data we used were based at ward level, which can cover as few as 400 people to as many as 30,000 people, but usually between 5,000 and 7,000 individuals. There are 8,414 wards in England.

[21] These are based on government benefit figures and are produced annually by Oxford University.

[22] The initial choices for the control areas were made by policy makers, but were subsequently changed following analysis using various years of the Youth Cohort Study.

Table 2.2. Pre-reform neighbourhood characteristics of pilot and control areas

	Pilot areas		*Control areas*		*All England*[a]	
	Mean	Std dev.	Mean	Std dev.	Mean	Std dev.
Number of observations	*4,518*		*2,320*		*8,414*	
Measures of local deprivation (index)						
Multiple deprivation score[b]	38.36	17.00	37.05	18.64	21.70	15.39
Income[b]	30.14	11.79	29.93	13.62	18.86	11.31
Employment[b]	16.66	6.23	16.38	7.14	10.19	6.49
Health deprivation and disability[b]	1.04	0.58	0.97	0.68	0.00	0.92
Education, skills and training[b]	0.78	0.98	0.70	0.82	0.00	0.87
Housing[b]	0.47	0.83	0.34	1.00	0.00	0.92
Geographical access to services[b]	–0.53	0.46	–0.60	0.54	0.00	0.87
Child poverty[b]	43.78	17.12	42.70	19.61	26.74	17.02
Education drop-out rates in 1998						
% dropping out of education at 16[c]	38.9		35.8		30.0	
% staying on in education[d]	69.15	8.99	66.63	10.50		
Nearest-school data						
Class sizes in 1999	21.43	2.29	21.41	2.23		
Average authorised absences (days per year)	8.69	1.99	8.86	2.63		
% getting 5 GCSEs[e] A*–C in 1999	35.35	17.72	35.48	15.82		
% getting 0 GCSEs[e] A*–G in 1999	6.93	5.09	6.54	6.08		
School has 6th form[f]	0.45	0.50	0.34	0.47		
Distance to nearest Year 12 provider (metres)	1,630.7	1,051.1	1,951.6	1,480.8		

[a] The all-England data are calculated on the basis of ward-level data (small subdivisions of municipalities). There are 8,414 wards in England.

[b] A higher score indicates a higher incidence of deprivation. Scores across different measures are not comparable.

[c] These data are taken from official LEA-based calculations of 16-year-old stay-on rates in 1998 (see Department for Education and Skills (2005)), weighted by our sample populations. (This is necessary, as in two of our control LEAs, we sampled half as many individuals as in our other control LEAs.)

[d] These data are calculated by looking at the number of 17-, 18- and 19-year-olds in receipt of Child Benefit divided by the number of 13-, 14- and 15-year-olds receiving the benefit in the local area (ward). Child Benefit is payable for all children under 16 and all those over 16 in secondary education. It has nearly 100 per cent take-up. As very few 19-year-olds are in secondary – rather than tertiary – education, this figure is an underestimate (by about a third) of the proportion of young people staying in post-compulsory education and should be understood as a proxy for this figure.

[e] GCSE exams are taken in the last year of compulsory education (Year 11) and are graded A* to G. The government has a target for at least 60 per cent of 16-year-olds nationwide, and at least 30 per cent of pupils in all schools, to achieve five GCSEs at grades A* to C by 2008.

[f] The 6th form is the two years of post-compulsory schooling, Years 12 and 13.

To control for differences between pilot and control areas, we use individual-level data from our survey as well as these administrative and local area data. The variables

we use include individual-based characteristics on prior achievement, household income, parental occupation and education, household composition and ethnicity; and childhood variables on early health problems, early childcare and grandparental inputs, special needs, and geographic mobility in early life. We have also controlled for publicly available data on the pre-reform quality of the child's nearest Year 11 state school[23] and distance to the nearest Year 12 state educational provider (post-16 education).[24] Summary statistics for our remaining variables used in the analysis are provided in Appendix 2.

3. The Evaluation Methodology

The outcome of interest in this paper will be participation in post-compulsory school, i.e. in Years 12 and 13. As we discuss in the results section below, we are interested in the impact of financial incentives on the entire target population, on the population of those fully or partially eligible for the subsidy and on the ineligible population. In each case, we will be comparing the outcomes relative to the appropriate comparison group.

Although the treatment and control areas are very well matched, the distribution of characteristics is not identical, which it might have been after a successful and large-scale randomisation. To allow for the fact that this was not going to be a randomised experiment, we designed the panel to include a large array of individual and local area

[23] We have address grid references for every child in our survey as well as for every Year 11 school in the country. These allowed us to identify the nearest (as the crow flies) comprehensive Year 11 school for every child in our survey. Once we identified the school, we mapped in publicly available pre-reform quality measures for that school.

[24] A number of studies have shown that distance to school is an important determinant of educational decisions (see Card (1995, 1999)).

characteristics, which should control for any relevant differences in the treatment and control areas before the programme was introduced.

Our approach to estimation is to use matching which we implement as follows: first we estimate the probability of an individual receiving the Education Maintenance Allowance (the treatment group) versus belonging to the comparison group. Based on the propensity score we check for common support across treatment and control observations. Because of the way we designed the data collection; in practice we have no problems of common support. Having kept (almost all) the observations that are in the common support we then run an OLS regression where school participation is regressed on individual characteristics (discussed above) with interactions and on individual characteristics interacted with a dummy variable for belonging to the EMA group. The effects we report are averaged over characteristics; the weights used for averaging are the observed distribution of characteristics among the treated groups and thus we interpret the results as the impact of treatment on the treated. This approach turned out to be particularly efficient giving the most precise results. [25], [26] The key to the credibility of this approach is the large number of individual characteristics that we use to control for any differences in composition between the pilot and comparison areas. The characteristics we use include variables relating to family income and background, childhood events (such as ill health and mobility) and prior school achievement as well as administrative data on the quality of schooling in the child's neighbourhood and other measures of neighbourhood quality and

[25] Because we allow for a large number of interactions, this approach is conceptually equivalent to using a probit or logit model: The interactions allow for the non-linearity implied by the probability function.

[26] See the work of Blundell, Dearden and Sianesi (2005) on this issue.

performance at the school level measured prior to the introduction of the EMA. The characteristics are all described in the Appendix

As a final step, we also carry out some sensitivity analysis. In one experiment, we consider aggregate school participation data for 16-year-olds including eligible and ineligible pupils because on aggregate we do not measure outcomes for the eligible only.[27] In the second experiment, we compare the change in school participation between the younger and the older sibling in pilot and control areas. In doing this, we also control for a number of characteristics. The reasons this is not our main evaluation method are that not all children have older siblings of the same sex and that the time-varying covariates we measure, including income, relate to the date of the survey, i.e. when the younger sibling was deciding whether to continue in education or drop out. Nevertheless, this sensitivity analysis confirms the results we find with matching.

In all cases, the standard errors are calculated analytically and allow for clustering at the local education authority level, which is the unit of treatment.[28]

4. The Results

4.1 Impact of the EMA on Year 12 destinations

Table 4.1 shows estimates of the impact of the EMA (overall and by sex) on young people's initial decisions to remain in full-time education, to move into employment or training, or to be inactive (NEET – Not in Education, Employment or Training).

[27] See Department for Education and Skills (2005).

[28] For the retention rate between Year 12 and Year 13 we use the block bootstrap with 1,000 replications, using again the LEA as the definition of the block/cluster.

We compare the results obtained when we control for characteristics and those obtained by a simple comparison of means.

Table 4.1. Impact of EMA on Year 12 destinations of eligibles

	Participation in pilot areas	Unmatched	Fully interacted OLS
All			
FT education	69.2	3.9	4.5
(s.e.)	(0.8)	(1.4)	(1.3)
Work/Training	16.4	–0.4	–1.5
(s.e.)	(0.6)	(1.1)	(1.2)
NEET	14.5	–3.5	–3.0
(s.e.)	(0.6)	(1.1)	(0.8)
Pilot sample size	*3,524*	*3,524*	*3,518*
Control sample size	*n/a*	*1,791*	*1,791*
Total sample size	*3,524*	*5,315*	*5,309*
Males			
FT education	66.4	5.3	5.0
(s.e.)	(1.1)	(2.0)	(2.0)
Work/Training	19.7	–1.5	–2.5
(s.e.)	(1.0)	(1.6)	(2.0)
NEET	13.9	–3.8	–2.4
(s.e.)	(0.8)	(1.5)	(1.4)
Pilot sample size	*1,753*	*1,753*	*1,747*
Control sample size	*n/a*	*900*	*900*
Total sample size	*1,753*	*2,653*	*2,647*
Females			
FT education	71.9	2.5	4.0
(s.e.)	(1.1)	(1.9)	(1.7)
Work/Training	13.0	0.7	–0.4
(s.e.)	(0.8)	(1.4)	(1.4)
NEET	15.1	–3.2	–3.6
(s.e.)	(0.9)	(1.5)	(0.9)
Pilot sample size	*1,771*	*1,771*	*1,771*
Control sample size	*n/a*	*891*	*891*
Total sample size	*1,771*	*2,662*	*2,662*

Notes: All standard errors allow for clustering at local education authority level. Our fully interacted OLS model imposes common support for males and females. Overall estimates are the appropriately weighted estimates for males and females.

The EMA has had a positive and significant effect on post-compulsory education participation among eligible young people. The overall estimate is 4.5 percentage

points from a baseline of 64.7 per cent in our matched sample of controls.[29] Considering the third and fifth rows of the table, we see that as a result of the policy, inactivity declined by 3.0 percentage points and work by 1.5 percentage points. Although we should regard this last distinction with some caution, given the standard errors, these point estimates indicate that a large proportion of the increased school participation originates from those who are otherwise not working. This is significant because it shows that to a large extent the policy is displacing individuals not from work but from unproductive activities, thus implying an overall lower cost of providing this incentive to education. This does raise the issue of the quality of individuals attracted to education by the subsidy, since they seem to be largely those with little opportunity cost. However, as we shall see, they tend to stay in full-time education for the whole two years of the subsidy. Moreover, given the regulated nature of the education institutions they have to attend, one can hypothesise they are receiving valuable training. Ultimately, however, this can only be evaluated using eventual labour market outcomes, not available to us.

The effects are higher for males, who have lower participation rates, than for females. However, the difference is not significant.

4.2 The impact of the EMA in the second year (year 13)

So far, the analysis has concentrated on the impact of the EMA on initial destinations in Year 12, the first post-compulsory year. However, the EMA is available for two years. Thus an important question is whether the impact of the EMA persists in the

[29] Our baseline figure is different from the aggregate figure for a number of reasons. First, the population is different. Second, the age window that the aggregate figure looks at is different since the aggregate figure works with age and not with school years as we do. Thus the aggregate figure relates to slightly older persons. Finally, we may have had differential non-response between participants and non-participants. Note, however, that there is no evidence that the

second year, altering significantly the entire path post-16. Education (whether academic or vocational) at Years 12 and 13 typically consists of courses lasting 2 years and both years usually need to be completed before qualifications are obtained. It is therefore interesting to see whether individuals, having sampled post-compulsory schooling as a result of the EMA, may have subsequently decided to drop out before Year 13. To answer this question, we focus on individuals who we observe for a second year, and examine their destinations in Year 13, one year after the introduction of EMA.

When considering whether the policy has led to longer-term increases in participation, we will have to use the second wave of data for our cohort. However, there has been some attrition: about 25 per cent of the original sample was lost in the follow-up. Appendix 3 shows that the likelihood of remaining in the sample is higher for those with incomes that would make them eligible for the EMA relative to the rest. However, the pattern of attrition is the same for the treatment and control areas, possibly implying that attrition has changed mainly the overall population composition rather than led to biases for the population we are considering. In Appendix 3, we report the results of running a probit on the determinants of attrition. We see that those who come from families earning less than £13,000 per annum (i.e. those in our pilot and control groups who we define as fully eligible) are slightly less likely to drop out of the panel but there is no difference conditional on this eligibility between pilot and control areas. These results suggest that attrition was not directly related to the EMA. When we re-estimate the impact of EMA in the first year only on the sample of those who do not drop out of the panel, we obtain very similar estimates

non-response is different between pilots and controls. In fact, the results on attrition in Appendix 3 imply that any non-response will be balanced between pilots and controls.

of the overall impact of EMA on full-time education participation.[30] Whilst this is reassuring, it is also clear that the distribution of observable characteristics has changed as a result of attrition in the second wave. In particular, the individuals who did not drop out of the sample tended to originate from a better family background and were more likely to be in school in wave 1 of the data (see Appendix 3). In this sense, the population for which we will be looking at the longer-term outcomes is different from the one for which we can look at the shorter-term outcomes. However, it should be stressed that issues relating to the impact of attrition are only relevant when we look at the longer-term effects of the programme.

We define the potential outcomes that could occur two years after the introduction of the programme as: education in Year 12 and education in Year 13; education in Year 12 and other activity in Year 13; other activity in Year 12 and education in Year 13; and, finally, other activities in both Year 12 and Year 13. Hence the overall impact on full-time education in Year 12 for this second wave can be found by comparing the outcomes of those in our first two groups with those in our second two groups in the first year.

Table 4.2 shows the impact of EMA based on the division of the population into the four mutually exclusive groups described above using our preferred fully interacted linear matching technique. The important conclusion that comes from the table is that where the EMA has been effective, it has led to an increase in both Year 12 and Year 13 attendance, and thus it is shown to have long-term effects. This is important because it indicates that those drawn into education due to the EMA are committed to

[30] For males, we find 5.1 percentage points with a standard error of 1.8, compared to our estimate of 5.0 percentage points for the full sample. For females, we find 4.3 percentage points with a standard error of 1.5, compared to our estimate of 4.0 percentage points for the full sample.

it. They do not just sample it only to find that it is not for them and drop out a few months later. Table 4.2 also shows that the EMA has increased average education retention rates, defined as the proportion of those in full-time education in Year 12 who were still in full-time education in Year 13. The EMA increased average retention rates by 4.1 percentage points (from 77.6 per cent to 81.7 per cent).

Table 4.2. Impact of EMA on Year 12 and Year 13 destinations of eligibles

	Participation in pilot areas	Unmatched difference	Fully interacted OLS
All			
Educ. Y12, educ. Y13	61.0	5.2	6.7
(s.e.)	(1.0)	(1.7)	(1.3)
Educ. Y12, not Y13	13.7	–1.4	–2.0
(s.e.)	(0.7)	(1.2)	(1.1)
Not Y12, educ. Y13	2.4	0.2	0.3
(s.e.)	(0.3)	(0.5)	(0.6)
Not Y12, not Y13	23.0	–4.1	–5.0
(s.e.)	(0.8)	(1.5)	(1.2)
Educ. retention rate	81.7	3.1	4.1
(s.e.)	(0.8)	(1.6)	(1.9)
Pilot sample size	*2,537*	*2,537*	*2,506*
Control sample size	*n/a*	*1,297*	*1,297*
Total sample size	*2,537*	*3,834*	*3,803*
Males			
Educ. Y12, educ. Y13	58.6	8.7	7.4
(s.e.)	(1.4)	(2.4)	(2.1)
Educ. Y12, not Y13	13.4	–2.8	–2.3
(s.e.)	(1.0)	(1.7)	(1.5)
Not Y12, educ. Y13	1.8	–0.6	–0.6
(s.e.)	(0.4)	(0.7)	(0.8)
Not Y12, not Y13	26.3	–5.3	–4.6
(s.e.)	(1.3)	(2.2)	(1.7)
Educ. retention rate	81.4	5.9	4.8
(s.e.)	(1.4)	(2.5)	(2.8)
Pilot sample size	*1,234*	*1,234*	*1,211*
Control sample size	*n/a*	*636*	*636*
Total sample size	*1,234*	*1,870*	*1,847*
Females			
Educ. Y12, educ. Y13	63.2	1.8	5.9
(s.e.)	(1.3)	(2.3)	(1.5)
Educ. Y12, not Y13	14.0	0.0	–1.7
(s.e.)	(1.0)	(1.7)	(1.6)
Not Y12, educ. Y13	2.9	0.9	1.2
(s.e.)	(0.5)	(0.8)	(0.8)
Not Y12, not Y13	19.9	–2.8	–5.4
(s.e.)	(1.1)	(1.9)	(1.6)
Educ. retention rate	81.9	0.4	3.4
(s.e.)	(1.3)	(2.0)	(2.6)
Pilot sample size	*1,303*	*1,303*	*1,295*
Control sample size	*n/a*	*661*	*661*
Total sample size	*1,303*	*1,964*	*1,956*

Notes: As Table 4.1 except that standard errors on estimated impact on retention rates are calculated by bootstrapping with 1,000 repetitions (allowing for stratification at the LEA level).

4.3 Impact of EMA in Year 12 by eligibility group

We now turn to comparing the impact of the policy separately for those who are eligible for the full amount of the EMA and those who are only eligible for a fraction because their parents have an income higher than £13,000. The impact may be different between the two groups for a number of conflicting reasons. First, if the subsidy is lower, it is likely to have a smaller effect. Second, the individuals who receive a lower subsidy do so because they come from a better-off background; this may make them more likely to go to school in the first place and thus may also affect their sensitivity to monetary incentives. With this design, we cannot distinguish one effect from the other. Thus, in Table 4.3, we distinguish between full eligibility, partial eligibility and ineligibility to see if the impact of the EMA differs by whether a person was fully or only partially eligible and to see if there were any spillovers to those in the ineligible group.

Table 4.3. Impact of EMA on Year 12 destinations: all young people by eligibility

	Fully eligible		*Partially eligible*		*Ineligible*	
	Participation in pilot area	Fully interacted OLS	Participation in pilot area	Fully interacted OLS	Participation in pilot area	Fully interacted OLS
All						
FT education	67.2	6.7	72.1	1.2	57.3	0.7
(s.e.)	(1.0)	(1.7)	(1.2)	(1.8)	(1.2)	(1.7)
Work/Training	15.5	–1.5	17.7	–1.4	13.5	1.3
(s.e.)	(0.8)	(1.5)	(1.0)	(1.8)	(1.1)	(1.9)
NEET	17.3	–5.2	10.1	0.2	5.7	–1.9
(s.e.)	(0.8)	(1.3)	(0.8)	(1.2)	(0.7)	(1.2)
Pilot sample size	*2,131*	*2,122*	*1,393*	*1,372*	*994*	*927*
Control sample size	*n/a*	*1,080*	*n/a*	*711*	*n/a*	*529*
Total sample size	*2,131*	*3,202*	*1,393*	*2,083*	*994*	*1,456*
Males						
FT education	66.2	6.7	66.7	4.0	75.2	3.2
(s.e.)	(1.5)	(2.5)	(1.8)	(2.9)	(1.9)	(3.0)
Work/Training	18.5	–0.7	21.6	–6.5	18.4	0.1
(s.e.)	(1.2)	(2.2)	(1.6)	(2.7)	(1.7)	(3.6)
NEET	15.3	–6.0	11.7	2.5	6.3	–3.4
(s.e.)	(1.1)	(1.7)	(1.2)	(1.5)	(1.1)	(1.9)
Pilot sample size	*1,060*	*1,051*	*693*	*683*	*521*	*494*
Control sample size	*n/a*	*539*	*n/a*	*361*	*n/a*	*270*
Total sample size	*1,060*	*1,590*	*693*	*1,044*	*521*	*764*
Females						
FT education	68.2	6.8	77.6	–1.6	86.9	–2.2
(s.e.)	(1.4)	(2.3)	(1.6)	(2.2)	(1.6)	(1.8)
Work/Training	12.5	–2.3	13.9	3.6	8.0	2.5
(s.e.)	(1.0)	(1.5)	(1.3)	(2.4)	(1.3)	(1.8)
NEET	19.3	–4.4	8.6	–2.1	5.1	–0.3
(s.e.)	(1.2)	(1.9)	(1.1)	(1.9)	(1.0)	(1.4)
Pilot sample size	*1,071*	*1,071*	*700*	*689*	*473*	*433*
Control sample size	*n/a*	*541*	*n/a*	*350*	*n/a*	*259*
Total sample size	*1,071*	*1,612*	*700*	*1,039*	*473*	*692*

Notes: As Table 4.1.

In Cohort 1, only just over 47 per cent of individuals were eligible for the maximum EMA payment, around 31 per cent for partial payment and 22 per cent were not eligible. All eligible individuals were entitled to the bonuses, which were not means-tested.

Among those who were estimated to be eligible for a full EMA award, EMA increased full-time education participation in Year 12 by 6.7 percentage points. For those estimated to be eligible for only a partial award, the corresponding figure is 1.2 percentage points (and not statistically significant at conventional levels). The p-value for their difference is 2.5 per cent. Thus we can say with reasonable confidence that the response of those fully eligible is larger than the response of those on the taper. A recent survey of education policy in England by Johnson (2004) has highlighted that one of the key aims of policies such as EMA is to improve post-compulsory staying-on rates for children from deprived social backgrounds. The combination of a more generous payment and possibly their greater responsiveness to the payment points to a success of the policy in this dimension.[31]

Similarly, for ineligible individuals, the overall effect is very small (+0.7 percentage points) and not statistically significant at conventional levels, indicating that the spillover effects in the short run are not important and reinforcing our confidence in the results, i.e. there is no evidence that an unobservable area effect is driving the results.

4.4 Does it matter who receives the payment?

Our analysis suggests that there are no significant differences in outcomes for variants where the child receives the payment (variants 1, 2 and 4). Increasing the generosity of the payment (variant 2) or the level of retention bonuses (variant 4) does not result

[31] He says, 'The UK has a relatively low staying-on rate in full time education after age 16. Given high returns this is, perhaps, surprising and probably economically inefficient. Given very substantial differences in staying-on rates by social background, it is also of concern from an equity point of view' (pages 177–178).

in any significant impact on measured outcomes in Year 12 or Year 13 compared with those in variant 1.[32]

In one of the EMA variants piloted (variant 3), the payment was made to the primary carer (usually the mother) instead of the child. There are many reasons why paying the mother could have a different effect. In one extreme, if the mother is not expected to pass on the benefit to the child, then the child will have a lower incentive to attend school. On the other hand, since transfers are already taking place from the parents to the child, one can argue that even if the benefit is given to the child, it can be clawed back by the parents, and hence whether it is paid to the child or to the parents should not make much difference.

In order to investigate this, we compare outcomes in variants 1 and 3, where the only difference in the scheme is who received the weekly payment. In order to ensure we are comparing like with like, we use the same sample of individuals from the control group in assessing each variant and only include those who satisfy the common support restrictions for both variants. If we do not distinguish by eligibility, the impacts of the variants are almost identical. When we do distinguish by eligibility, though, there are differences in the effect depending on whether the payment is made the mother or the child. When the payment is made to the child, the impact appears to be concentrated solely on those eligible for a full award, whereas when the payment is made to the mother, the impact appears to be more evenly spread among those eligible for either full or partial EMA awards. These findings have obvious policy interest and suggest that if the key interest is in increasing participation among those from the poorest backgrounds (those from families earning less than £13,000 per

[32] All of the results cited in this section are available from the authors on request.

annum) then payment to the child may be preferred, whereas if the government is keen to have an effect across the whole eligibility distribution then payment to the mother may be more effective – at least in terms of initial staying-on decisions and at a cost of a lower effect amongst those from the poorest backgrounds.[33]

4.5 Does the impact vary by prior academic achievement?

We have already seen that the EMA has its largest impact on children from relatively lower-income families who are able to qualify for the maximum award (Table 4.3). Another key question is whether children with low prior academic achievement can be encouraged to stay in school longer, possibly improving their skills before labour market entry. Thus, in Table 4.4, we present results where the sample is those eligible for a full EMA award only, split into low and high prior achievement.[34] The EMA seems to affect primarily those with low prior achievement. However, this is perhaps not so surprising, given that the post-compulsory school participation rate is much higher for those with high prior achievement. It does point out, however, that the increase in participation comes primarily from the lower ability group and is consistent with the earlier result showing that a large proportion of the increase in participation comes from those who would not otherwise be employed.

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[33] The EMA since September 2004 has been rolled out nationally and all payments are made to the child.

[34] Prior achievement is based on grades obtained in GCSE Maths and English exams that all students had to sit in Year 11. Each grade in these exams was given a score of 0 to 8 and then they were added together to obtain a score out of 16. Our high-ability children had a score of 9 or above, and this was just under half our sample of those eligible for a full EMA award.

Table 4.4. Impact of EMA on Year 12 destinations of those fully eligible for the EMA: by prior academic achievement

	Low prior academic achievement		*High prior academic achievement*	
	Participation in pilot area	Fully interacted OLS	Participation in pilot area	Fully interacted OLS
All				
FT education	65.2	9.2	85.1	1.0
(s.e.)	(1.6)	(2.6)	(1.3)	(1.9)
Work/Training	19.5	–2.2	9.5	–1.3
(s.e.)	(1.3)	(2.9)	(1.0)	(1.6)
NEET	15.3	–7.0	5.5	0.4
(s.e.)	(1.2)	(3.0)	(0.8)	(1.3)
Pilot sample size	*909*	*876*	*803*	*747*
Control sample size	*n/a*	*435*	*n/a*	*421*
Total sample size	*909*	*1,311*	*803*	*1,168*
Males				
FT education	62.9	6.0	86.1	1.0
(s.e.)	(2.2)	(3.4)	(1.8)	(2.3)
Work/Training	23.7	–0.3	10.1	–1.0
(s.e.)	(2.0)	(3.1)	(1.6)	(2.4)
NEET	13.4	–5.7	3.8	0.0
(s.e.)	(1.6)	(2.6)	(1.0)	(1.5)
Pilot sample size	*464*	*437*	*366*	*315*
Control sample size	*n/a*	*218*	*n/a*	*196*
Total sample size	*464*	*655*	*366*	*511*
Females				
FT education	67.6	12.3	84.2	1.0
(s.e.)	(2.2)	(3.9)	(1.7)	(2.9)
Work/Training	15.1	–4.0	8.9	–1.6
(s.e.)	(1.7)	(4.9)	(1.4)	(2.1)
NEET	17.3	–8.3	6.9	0.6
(s.e.)	(1.8)	(5.3)	(1.2)	(2.1)
Pilot sample size	*445*	*439*	*437*	*432*
Control sample size	*n/a*	*217*	*n/a*	*225*
Total sample size	*445*	*656*	*437*	*657*

Notes: As Table 4.1.

4.6 Credit constraints?

The previous five sections have shown that the EMA grant appears to have a relatively large impact on the education participation decisions in both Year 12 and Year 13. A key question, which is directly relevant for understanding the way the policy works and for evaluating its merits, is whether the effect we estimate is due to

liquidity constraints. In a standard education model, the individual needs to borrow to fund education, if anything because living expenses have to be covered. In practice, such funding typically comes from the parents. Part of the funding will be motivated by straight altruism: if the parents value the child's utility, they will be willing to fund education that will improve the life-cycle welfare of the child (see Becker (1991)). If the child can commit to repay some of the funds, the parents may be willing to advance even more. However, the practical difficulty is that parents with little or no assets and low income may in fact be unable to provide adequate funding, leading the child to work instead of attending school. This is the concern of policy-makers. Thus there are two alternative ways that the policy can have led to the increases in participation. One is a simple price distortion: by subsidising education, its market price has been artificially lowered and children who would not otherwise (optimally) attend school do so. This can be shown to be the case in a household that is altruistically linked or if the child is acting as an individual (Becker, 1991, Altonji, Hayashi and Kotlikoff, 1992). In this case, their returns to education (net of costs) will be low. The other mechanism is that the subsidy alleviates a liquidity constraint and children obtain more education now that a market distortion against education has been (at least partially) corrected.

To get a handle on this issue, we follow a long tradition in the consumption literature (see Zeldes (1989)) and split the sample by assets, the idea being that those with assets are not liquidity constrained. Our measure of assets is house ownership by the family; families that own a house are relatively more likely to have access to financial assets or credit – not least as it would be relatively straightforward for them to release equity by borrowing against the house. Under the null hypothesis, we also need to assume that once we have conditioned on our plethora of socio-economic

characteristics, ability and local variables, house ownership is not correlated with costs of or returns to schooling.

Given this assumption, we compare the impact of the policy on those living in an owner-occupied house and the impact on those living in rented accommodation (pre-policy). The key assumption here is that house ownership in itself does not lead to different responses to financial incentives, other than because it implies different access to funds. Under the null, those in rented accommodation will react in the same way as those in owner-occupied housing because in both cases they were able to obtain the optimal amount of schooling pre-policy. Post-policy, the price of education is distorted and this affects both groups in the same way. Under the alternative, however, those in rented accommodation will have two reasons to increase education: first, they will now have the opportunity to fund education when before they could not; second, they will also face the price distortion. Overall, those in rented accommodation should respond more to the subsidy.

The results of this test are presented in Table 4.5. Overall, we find that the point estimate of the impact of a full EMA award is 9.1 percentage points for non-owner-occupiers. The estimated impact for owner-occupiers is substantially smaller (3.8 percentage points), but the difference between the two coefficients is not significant at conventional levels (p-value of 12 per cent). Therefore the extent to which the impact of the policy is due to credit constraints, rather than an unconstrained price effect, remains unclear.

Table 4.5. Impact of EMA on Year 12 and Year 13 destinations of those fully eligible for the EMA: by housing tenure

	Not owner-occupiers		*Owner-occupiers*	
	Participation in pilot area	Fully interacted OLS	Participation in pilot area	Fully interacted OLS
All				
FT education	58.0	9.1	77.5	3.8
(s.e.)	(1.5)	(3.0)	(1.3)	(2.2)
Work/Training	17.5	–2.6	13.3	–2.0
(s.e.)	(1.1)	(2.2)	(1.1)	(1.9)
NEET	24.6	–6.5	9.2	–1.8
(s.e.)	(1.3)	(2.0)	(0.9)	(1.4)
Pilot sample size	*1,123*	*1,096*	*1,008*	*984*
Control sample size	*n/a*	*512*	*n/a*	*518*
Total sample size	*1,123*	*1,608*	*1,008*	*1,502*

Notes: As Table 4.1.

These estimates are consistent with the hypothesis that the EMA has a larger impact on renters than owner-occupiers. This hypothesis, if true, would have two implications for policy. First, were the difference in outcomes due to the presence of credit constraints, then the policy would have much stronger grounds for support as it would suggest that lack of finances leads to under-education of children from low-income families. Second, irrespective of the presence of credit constraints, the policy could be more effectively targeted although not without introducing further distortions.

4.7 Sensitivity analysis

Before we conclude, we proceed to carry out a number of sensitivity tests that demonstrate the robustness of our results. We thus consider results based on aggregate data and results based on comparing the behaviour of these children with that of their siblings who did not face the policy.

Aggregate data

We now present simple difference-in-differences estimates based on aggregate school participation data for 16-year-olds. We use three post-policy periods compared with the one pre-policy period (1998) where we have a complete set of data. In reading these results, note that the proportion of fully eligible individuals is about 47 per cent. If we include those partly eligible (i.e. on the taper), the proportion rises to 78 per cent. So if we assume the policy had no effect on ineligible individuals, we need to multiply the estimated effect on fully eligible individuals by 2.1 and on partially eligible individuals by 1.3.

The three difference-in-differences estimates for 1999, 2000 and 2001 are respectively 2.7, 2.3 and 4.7 percentage points, always with 1998 as the baseline.[35] If we multiply these by 1.3, we obtain effects of 3.5, 3.0 and 6.1 percentage points respectively, which are remarkably close to the effect we obtained from the individual data of 4.5 percentage points, and certainly within the 95 per cent confidence interval.

Using older siblings

An alternative approach, which allows us to focus more closely on the group of interest and at the same time to control for characteristics as in our main analysis, is to use difference-in-differences using as a comparison group the older siblings of the children in our pilot and control areas. We thus compare the change in participation between the current cohort and the older siblings in the pilot and comparison areas, controlling for observable characteristics. We include a full set of cohort and area dummies. We find an EMA effect of 8.4 percentage points (with a standard error of 2.6), which is larger than the effect we reported above. The difference is not

[35] All of the results cited in this section are available from the authors on request.

significant at conventional levels.[36] The smaller sample has made the estimate less precise, but offers support for the significant effect of the EMA.

Finally, we also carry out successive difference-in-differences across siblings reaching the statutory school-leaving age before the period when the policy was in place. We find that in all previous periods, this dummy 'effect' is not significant. In the final period, when the policy was in place, we obtain a positive and significant effect, again corroborating and strengthening our results.

4.8 A back-of-the-envelope cost–benefit calculation

If the strong impact of the EMA is due to liquidity constraints, then even in the absence of any externalities to education, we could expect a positive welfare effect of the policy. However, this is hard to measure because we have no measure of the individual costs of education and we cannot easily measure the distortionary effects of raising funds for the subsidy. In any case, we now carry out a simple, back-of-the-envelope calculation.

Based on our results that the EMA increased the percentage of individuals from income-eligible families completing two years of post-compulsory education by 6.7 percentage points, from 54.3 per cent to 61.0 per cent, and that in the first year (second year) one-third (two-thirds) of this increase is from individuals who would otherwise have been in paid employment, we estimate that those brought into education would need to experience a real increase in future earnings of 6.2 per cent as a result of the additional two years of education for the programme to break even,

[36] The standard error allows for clustering at the family level.

allowing for the opportunity cost of education.[37] If we also allow £3,000 for the annual extra cost of educating those who stay on in secondary education,[38] the required return to education for the two years is 7.7 per cent.[39] Research into the returns from staying on in post-compulsory education suggests that the returns are in fact 11 per cent for males and 18 per cent for females (Dearden, McGranahan and Sianesi, 2004). There may well be other benefits of the policy: the government might value the redistribution to lower-income families with children; infra-marginal individuals may reduce hours of work and increase effort put into education; there may be crime reductions. Many of these benefits are hard to evaluate but they should not be discounted without further research.[40]

5. Conclusions

Conditional cash transfers have become very popular as a way of improving participation in schools. One such policy intervention, and probably unique in a rich industrialised country, is the Education Maintenance Allowance in the UK. Despite a steady increase, the participation in education following completion of compulsory schooling in England remained relatively low before its introduction. We collected

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[37] To do this calculation, we find the rate of return to education, r, that solves $\sum_{t=0}^{1} \frac{pEMA_t + (\alpha_t \lambda) w_t + \lambda C_t}{(1+R)^t} = \sum_{2}^{47} \frac{\lambda r w_t}{(1+R)^t}$ where EMA_t is the annual average EMA transfer payment allowing for the fact that not all those eligible receive a full award (this average is estimated to be £900 a year – £25 a week for 30 weeks plus £150 in bonuses) and p is the proportion in full-time education eligible for the EMA (estimated to be 75.2 per cent). α_t is the proportion drawn from paid employment at time t which is estimated to be one-third for $t=0$ (Year 12) and two-thirds for $t=1$ (Year 13). λ is the increase in participation in education (estimated to be 6.7 percentage points from Table 4.2).C_t is the marginal cost of those brought into education as a result of the EMA and w_t represents the estimated life-cycle wage profile based on the 2002–03 *Family Resources Survey*. We assume 2 per cent a year real growth in future wages. R is the discount rate, which is assumed to be 3½ per cent, which is the recommended discount rate in the UK HM Treasury Green Book (http://greenbook.treasury.gov.uk/).

[38] See Department for Education and Skills (2003), table 7.

[39] The precise marginal cost is hard to quantify since one would want to keep quality constant. We have taken the average expenditure per pupil as our measure.

[40] Feinstein and Sabates (2005) find some evidence that the EMA led to a reduction in the number of convictions of young men for both burglary and theft.

panel data on three cohorts of individuals during the time the policy was being piloted in England and we used these data to measure the effect of the intervention on participation. Since September 2004, the EMA programme has been rolled out nationally.

Our results imply that the scope for affecting education decisions using subsidies to education can be substantial. More specifically, they imply that the EMA has significantly raised the stay-on rates past the age of 16. The initial impact is around 4.5 percentage points while having no effect on ineligibles. Taking into account that this was a time when the labour market was particularly buoyant, these seem to be quite large effects, although they were achieved with a replacement rate of 33–40 per cent of average net earnings for the age-group.

The results also suggest that the impact of the EMA on participation actually increases in the following year. This result is important because it suggests that those who are induced into extra education do not find the courses unexpectedly difficult or uninteresting and are willing to stay for the full two years of the programme. Importantly, about two-thirds of the increase in school participation is due to a decline in inactivity rather than work. This reduces the implicit costs of the programme because the forgone earnings for these individuals are zero. However, this may also mean that the programme is attracting those with few other opportunities, as also demonstrated by the fact that the largest effect is among those with low prior achievement. The key policy question here is the extent to which this extra education is valuable to them. If the main mechanism by which the policy works is by alleviating liquidity constraints then it would reinforce the view that those attracted into education by this policy would enjoy positive net returns. Among those eligible for a full grant, the point estimate of the effect of the policy is larger for renters than

for owner-occupiers. While this is consistent with some families facing credit constraints, the difference in the estimated impact of the policy is not statistically different from zero at conventional levels of significance. Therefore the extent to which the impact of the policy is due to credit constraints, rather than an unconstrained price effect, remains unclear.

We also do not know what returns those induced into staying on by the subsidy will realise. Furthermore, we really have very little idea of how these returns and the future supply of educated workers may change now that the programme has been rolled out nationally. This of course depends on many factors, not least the nature of the production function. These are all-important research and policy questions that we will be pursuing in the future

Appendix 1: Indicators used in each deprivation score

Income	Adults in Income Support households (DSS) for 1998
	Children in Income Support households (DSS) for 1998
	Adults in Income-Based Jobseeker's Allowance households (DSS) for 1998
	Children in Income-Based Jobseeker's Allowance households (DSS) for 1998
	Adults in Family Credit households (DSS) for 1999
	Children in Family Credit households (DSS) for 1999
	Adults in Disability Working Allowance households (DSS) for 1999
	Children in Disability Working Allowance households (DSS) for 1999
	Non-earning, non-IS pensioner and disabled Council Tax Benefit recipients (DSS) for 1998 apportioned to wards
Employment	Unemployment claimant counts (JUVOS, ONS) average of May 1998, August 1998, November 1998 and February 1999
	People out of work but in TEC delivered government supported training (DfEE)
	People aged 18–24 on New Deal options (Employment Survey)
	Incapacity Benefit recipients aged 16–59 (DSS) for 1998
	Severe Disablement Allowance claimants aged 16–59 (DSS) for 1999
Health deprivation and disability	Comparative mortality ratios for males and females at ages under 65; district-level figures for 1997 and 1998 applied to constituent wards (ONS)
	People receiving Attendance Allowance or Disability Living Allowance (DSS) in 1998 as a proportion of all people
	Proportion of people of working age (16–59) receiving Incapacity Benefit or Severe Disablement Allowance (DSS) for 1998 and 1999 respectively
	Age- and sex-standardised ratio of limiting long-term illness (1991 Census)
	Proportion of births of low birth weight (<2,500g) for 1993–97 (ONS)
Education, skills and training	Working-age adults with no qualifications (3 years' aggregated LFS data at district level, modelled to ward level) for 1995–98
	Children aged 16 and over who are not in full-time education (Child Benefit data – DSS) for 1999
	Proportions of 17- to 19-year-old population who have not successfully applied for HE (UCAS data) for 1997 and 1998
	KS2 primary school performance data (DfEE, converted to ward-level estimates) for 1998
	Primary school children with English as an additional language (DfEE) for 1998
	Absenteeism at primary level (all absences, not just unauthorised) (DfEE) for 1998
Housing	Homeless households in temporary accommodation (Local Authority HIP Returns) for 1997–98
	Household overcrowding (1991 Census)
	Poor private sector housing (modelled from 1996 English House Condition Survey and RESIDATA)
Geographical access to services	Access to a post office (General Post Office Counters) for April 1998
	Access to food shops (Data Consultancy) for 1998
	Access to a GP (NHS, BMA, Scottish Health Service) for October 1997
	Access to a primary school (DfEE) for 1999
Child poverty	Percentage of children that live in families that claim means-tested benefits (Income Support, Jobseeker's Allowance (Income-Based), Family Credit and Disability Working Allowance)

Source: Department of the Environment, Transport and the Regions (2001), *Regeneration Research Summary: Indices of Deprivation 2000*, (Number 31, 2000) (www.urban.odpm.gov.uk/research/summaries/03100/index.htm).

Appendix 2: Sample characteristics

	Whole sample	Pilot areas	Control areas
Male	0.504	0.503	0.504
Pilot area	0.661	1.000	0.000
Fully eligible for EMA	0.470	0.472	0.466
Partially eligible for EMA	0.308	0.308	0.306
Ineligible for EMA	0.223	0.220	0.228
In full-time education Year 12	0.709	0.717	0.694
In work Year 12	0.156	0.157	0.154
Characteristics used in matching			
Weekly family income	389.01	387.50	391.95
Family receives means-tested benefit	0.263	0.268	0.253
Mother and father figure present	0.623	0.626	0.617
Father figure present	0.753	0.753	0.753
Father's age (= 0 if absent)	30.096	30.301	29.696
Mother's age	39.859	39.867	39.843
Owner-occupier	0.693	0.686	0.709
Council or Housing Association	0.253	0.266	0.226
Has statemented special needs	0.092	0.093	0.090
Mother has A levels or higher	0.245	0.237	0.259
Mother has O levels or equivalent	0.246	0.245	0.247
Father has A levels or higher	0.221	0.220	0.223
Father has O levels or equivalent	0.171	0.168	0.177
Father manager or professional	0.166	0.163	0.172
Father clerical or similar	0.243	0.246	0.238
Mother manager or professional	0.129	0.121	0.144
Mother clerical or similar	0.294	0.300	0.282
Father variables missing	0.363	0.362	0.366
1 or 2 parents in work when born	0.831	0.825	0.843
Attended 2 primary schools	0.254	0.256	0.251
Attended more than 2 primary schools	0.076	0.077	0.073
Received childcare as a child	0.911	0.915	0.903
1 set of grandparents around when child	0.326	0.320	0.337
2 sets of grandparents around when child	0.448	0.466	0.413
Grandparents provided care when child	0.316	0.307	0.332
Ill between 0 and 1	0.223	0.225	0.219
Number of older siblings	0.941	0.928	0.968
Number of younger siblings	0.975	0.979	0.968
Older sibling educated to 18	0.291	0.286	0.299
White	0.896	0.892	0.903
Father in full-time work	0.503	0.504	0.502
Father in part-time work	0.021	0.019	0.025
Mother in full-time work	0.335	0.327	0.350
Mother in part-time work	0.309	0.312	0.304
Maths GCSE score	4.233	4.232	4.235
English GCSE score	3.810	3.798	3.834
GCSE score missing	0.129	0.131	0.126
Number of observations	6,838	4,518	2,320

Appendix 3: Probability of attrition between wave 1 and wave 2

	Marginal effect	Standard error
Partially eligible for EMA	–0.002	0.024
Fully eligible for EMA	–0.039	0.015
Pilot area	0.005	0.012
Male	0.019	0.011
Weekly family income	0.000	0.000
Family receives means-tested benefit	–0.014	0.017
Mother and father figure present	–0.015	0.032
Father figure present	–0.028	0.021
Owner-occupier	–0.085	0.025
Council or Housing Association	–0.031	0.023
Has statemented special needs	–0.001	0.018
Mother's age	–0.002	0.001
Father's age	–0.001	0.001
Mother has A levels or higher	0.001	0.017
Mother has O levels or equivalent	0.001	0.014
Father has A levels or higher	–0.065	0.018
Father has O levels or equivalent	–0.022	0.017
Father manager or professional	–0.014	0.021
Father clerical or similar	0.017	0.016
Mother manager or professional	–0.029	0.020
Mother clerical or similar	–0.014	0.013
Father variables missing	–0.015	0.036
1 or 2 parents in work when born	–0.011	0.016
Attended 2 primary schools	–0.021	0.012
Attended more than 2 primary schools	0.030	0.021
Received childcare as a child	0.002	0.019
1 set of grandparents around when child	–0.008	0.015
2 sets of grandparents around when child	0.004	0.016
Grandparents provided care when child	0.007	0.012
Ill between 0 and 1	0.010	0.013
Number of older siblings	0.017	0.006
Number of younger siblings	–0.010	0.005
Older sibling educated to 18	–0.036	0.013
White	–0.020	0.022
Father in full-time work	0.033	0.020
Father in part-time work	–0.004	0.039
Mother in full-time work	–0.002	0.017
Mother in part-time work	–0.030	0.015
Measures of local deprivation		
Income	–0.001	0.002
Employment	–0.007	0.003
Health deprivation and disability	0.033	0.020
Education, skills and training	0.023	0.011
Housing	0.010	0.012
Geographical access to services	0.004	0.014
Child poverty	0.002	0.001

% not staying on post-16	–0.002	0.001
% not going to university	–0.002	0.002
Class sizes in 1999	–0.003	0.002
Authorised absences	0.000	0.004
% getting 5 GCSE A–C in 1999	0.001	0.001
% getting 0 GCSE A–G in 1999	0.001	0.001
School has 6th form	–0.002	0.013
Distance to nearest Year 12 provider	0.000	0.000
Maths GCSE score	–0.014	0.006
English GCSE score	–0.015	0.005
GCSE score missing	–0.003	0.025
Number of observations	6,838	
Observed probability	0.253	

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