

# Can Education Subsidies stop School Drop-outs? An evaluation of Education Maintenance Allowances in England

Lorraine Dearden, Carl Emmerson, Christine Frayne and Costas Meghir\*

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## Abstract

This paper evaluates whether a means tested grants paid to secondary students 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 during 1999 and 2000 using a specially designed dataset 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) being around 4.5 percentage points higher. Full-time participation rates one year later are found to have increased by around 6.4 percentage points which is largely due to the EMA having a significant effect on retention in post compulsory education, particularly for boys. These effects vary by eligibility group with those receiving the full payment having the largest initial increase in participation, whilst the effects for those who are partially eligible are only significantly different from the control group in the second year of the program. There is some evidence that participation effect is stronger for boys, especially in the second year, and that the policy goes some way to reducing the gap in drop-out rates between boys and girls. It is also clear that the policy has the largest impact on children from the poorest socio-economic background.

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\* Dearden, Emmerson and Frayne: Institute for Fiscal Studies, 7 Ridgmount St, London, WC1E 7AE, UK (e-mail: [ldearden@ifs.org.uk](mailto:ldearden@ifs.org.uk), [cemerson@ifs.org.uk](mailto:cemerson@ifs.org.uk), [cfrayne@ifs.org.uk](mailto:cfrayne@ifs.org.uk)); Meghir: Institute for Fiscal Studies and Department of Economics, University College London, Gower St, London WC1E 6BT (e-mail: [c.meghir@ucl.ac.uk](mailto:c.meghir@ucl.ac.uk)). The authors would like to Erich Battistin, Richard Blundell, Emla Fitzsimons, Alissa Goodman and Barbara Sianesi for useful comments on earlier versions of this paper. We are also indebted to our evaluation colleagues Sue Middleton, Sue McGuire and Karl Ashworth from the Centre for Research in Social Policy and Stephen Finch from the National Centre for Social Research for their support and advice. We also received invaluable advice and comments from John Elliott and Ganka Mueller from the UK Department for Education and Skills (DfES) who funded the evaluation. Despite all this help, the usual disclaimer applies.

## 1. Introduction

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 to most developed countries. The most recent OECD figures (OECD (2003)) shows that in a league table of 30 developed OECD countries, the UK has fallen to 22<sup>nd</sup> place, down from 13<sup>th</sup> place just a generation ago<sup>1</sup>. The proportion of 25-34 year olds with upper secondary education as of 2001 stood at just 68 per cent. This compares with over 90 per cent in countries like the Czech Republic, Japan, Korea, Norway, Sweden and Switzerland. The US has also slipped down the tables from 1<sup>st</sup> to 9<sup>th</sup> place with 88 per cent of 25-34 year olds having at least upper secondary education in 2001 <sup>2</sup>, however this figure is still 20 percentage points higher than the corresponding figures for the UK despite continuing problems with drop-out rates in some US cities<sup>3</sup>.

Education has also been at the centre of anti-poverty and pro-growth policies both in the developing world as well as 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 developments.<sup>4</sup> The recent increase in the returns to education in the US<sup>5</sup> and the UK<sup>6</sup> has reinforced this view. Education is also seen as a way for individuals to escape poverty and welfare

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<sup>1</sup> By a generation ago we look at where the UK stood in terms of the percentage of 55 to 64 year olds with at least upper secondary education in 2001. 55 per cent of UK 55-64 year olds had at least upper secondary education in 2001 which places the UK 13<sup>th</sup> out of the 30 countries covered – see OECD(2003), Table A1.2.

<sup>2</sup> In the US, 83 per cent of 55-64 year olds had at least upper secondary education in 2001 which is the best of all 30 countries covered.

<sup>3</sup> 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 dropouts are not considered to have completed school and no certificate or award is issued at this stage. The U.S. dropout rate is just over 11 percent of secondary-level students aged 16 and older according to the latest US Department of Education figures (see <http://www.ed.gov>).

<sup>4</sup> See among many others Benhabib and Spiegel(1994), Krueger and Lindahl (2001) and Vandenbussche, Aghion and Meghir (2003)

<sup>5</sup> Juhn, Murphy and Pierce (1995)

<sup>6</sup> Gosling, Machin and Meghir (2000)

dependency and this perception has motivated numerous policies both in the UK and worldwide that promote education as the long solution to these problems.

There has been worldwide focus on school drop-out problems and a number of policies devised to help reduce school drop-out rates. One of the key policy changes in most OECD countries after World War II, was to introduce free secondary school 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 occurred between 1910 and 1940 (see Goldin (1999)). In the UK fees for secondary schools were abolished by the Education Act 1944 (The Butler Act) and the compulsory school leaving ages 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<sup>7</sup> and for the remaining for 28 OECD countries ranges from 14 to 18<sup>8</sup>.

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<sup>9</sup>. In recent years a number of countries, both developed and developing, have instead introduced means tested grants in an attempt to encourage students to stay in school, rather than simply raising the compulsory school leaving age<sup>10</sup>. This paper examines the impact of such a program that was first piloted in

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<sup>7</sup> 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.

<sup>8</sup> See OECD(2003) Table .

<sup>9</sup> 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 (2003) who exploit changes in the Swedish Secondary Education system to estimate the returns to education.

<sup>10</sup> Prominent examples are the AUSTUDY program introduced in Australia in 1988 for children in their last 2 years of secondary school (now called YOUTH ALLOWANCE) (see Dearden and Heath (1996)), the PROGRESA program in Mexico which covers children from primary school to the end of high school (see Schultz, 2000, Attanasio, Meghir

a number of regions in England in 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 on school drop-out rates is of general policy interest to policy makers worldwide<sup>11</sup>.

The presumption of the policy makers has been that these low levels of education are due to financial constraints rather than to the outcome of an informed choice in an unconstrained environment.<sup>12</sup> The evaluation of this programme *cannot* provide information on the importance of liquidity constraints on education, since it changes the relative costs of remaining in school<sup>13</sup>. However, it can provide 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<sup>14</sup>.

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 4.8 percentage points and eligible females by 4.2 percentage points. In the second year this increases to 7.6 percentage points for eligible males and 5.3 percentage points for females, suggesting that the effect of the policy is not only to

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and Santiago, 2002), the recently introduced *Familias en Accion* program in Colombia modelled on PROGRESA (Attanasio et al. 2003).

<sup>11</sup> 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).

<sup>12</sup> “We recognise that for some young people there are financial barriers to participating in education, particularly for those from lower income households.” *Education Maintenance Allowance: An Introduction* [http://www.dfes.gov.uk/ema/pdfs/003158\\_A4%20singles.pdf](http://www.dfes.gov.uk/ema/pdfs/003158_A4%20singles.pdf)

<sup>13</sup> Papers that have attempted to look at this question include Cameron and Heckman (1998), Cameron and Taber (2000) and Dale and Krueger (1999).

<sup>14</sup> With respect to dropping out at 16, following the GCSE qualification which is obtained at that age, the 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 14-19 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 (pt 3). A recent survey of government policy by Johnson (2004) also highlights this concern. 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

increase participation, but also retention in full-time education. The initial effects are largest for those who receive the maximum payment although the retention effects are concentrated among individuals who are only partially eligible. We estimate that just over half of individuals who stayed in education were drawn from inactivity rather than work. The overall impact of the EMA was not diminished when it was paid to the mother rather than to the child, though there is some weak evidence that paying to the child is more effective for those fully eligible whereas the opposite is true for those who are partially eligible.

We also find that the effect of EMA is largest for children coming from a poorer socio-economic background. Both girls and boys coming from low-socio economic backgrounds<sup>15</sup> have very high drop out rates and the EMA has proved especially effective in plugging the drop-out gap for this vulnerable group.

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

## **2. Background and Data**

The Education Maintenance Allowance (EMA) pilots were launched in September 1999 in 10 Local Education Authorities. The scheme paid a means-tested benefit to 16–18 year-olds who remained in full-time education after year 11, when education ceases to be compulsory (i.e. after 16 years of age approximately). The payments consisted of a

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very substantial differences in staying-on rates by social background, it is also of concern from an equity point of view” (pp 177-178).

weekly allowance (during term time only), a retention bonus every term for those attending consistently and an achievement bonus paid at the end of the course if students fulfilled goals set out in a learning agreement agreed signed by parents and students when they first start receiving EMA. The benefit could be claimed for up to 2 years (or 3 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<sup>16</sup>.

**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	Parent	£50	£50
4	£30	Young person	£80	£140

Four different variants of the EMA were piloted and these are outlined in Table 2.1. In this paper we look at the overall impact of EMA and whether this impact varied according to either the generosity of the scheme and/or to who it was paid (parent of child). The basic EMA variant 1 was piloted in 3 urban areas and 1 rural area. Variants 2, 3 and 4 were all piloted in 2 urban areas.

In each area the maximum EMA weekly payment (£30 or £40) could be received by young people whose parents' incomes were £13,000 or below.<sup>17</sup> The benefit was tapered

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<sup>16</sup> 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 program 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 4 variants were piloted. Full results for all cohorts and individuals who participated in the pilots are available from the authors.

<sup>17</sup> Income is defined as the taxable income of the biological parents in the previous tax year.

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)<sup>18</sup>. The children also received an achievement bonus on successful completion of the 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 just under 40 hours' work a week. Thus the maximum eligibility for the EMA, depending on the variant, replaces around a third of post tax earnings.

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<sup>19</sup>. The data used to evaluate the programme are based on initial face-to-face interviews with both the parents and the children and 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<sup>20</sup>. 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), prior school achievement as well as administrative

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<sup>18</sup> This bonus was paid to the child in ALL variants (including variant 3).

<sup>19</sup> 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.

<sup>20</sup> We used data from the British Cohort Studies to choose our control areas so as to ensure 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.

data on the quality of schooling in the child’s neighbourhood as well as other measures of neighbourhood quality measured prior to the introduction of the EMA <sup>21</sup>.

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 (which are based on government benefit figures and are produced annually by Oxford University). 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.

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

	Pilot Areas		Control Areas		All England	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
<i>Number of observations</i>	4,518		2,320			
<i>Measures of local deprivation (index)</i>						
Multiple deprivation score <sup>1</sup>	38.36	17.00	37.05	18.64	21.70	15.39
Income <sup>1</sup>	30.14	11.79	29.93	13.62	18.86	11.31
Employment <sup>1</sup>	16.66	6.23	16.38	7.14	10.19	6.49
Health Deprivation and Disability <sup>1</sup>	1.04	0.58	0.97	0.68	0.00	0.92
Education, Skills and Training <sup>1</sup>	0.78	0.98	0.70	0.82	0.00	0.87
Housing <sup>1</sup>	0.47	0.83	0.34	1.00	0.00	0.92
Geographical Access to Services <sup>1</sup>	-0.53	0.46	-0.60	0.54	0.00	0.87
Child poverty <sup>1</sup>	43.78	17.12	42.70	19.61	26.74	17.02
<i>Pre-pilot Education participation rates</i>						
per cent staying on at school <sup>4</sup>	69.15	8.99	66.63	10.50		
per cent not going to university	89.34	6.86	88.23	7.79		
<i>Nearest school data</i>						
Class sizes 1999 (number of pupils)	21.43	2.29	21.41	2.23		
Authorised absences (per year)	8.69	1.99	8.86	2.63		
% getting 5 GCSE <sup>2</sup> A–C in 1999	35.35	17.72	35.48	15.82		
% getting 0 GCSE <sup>2</sup> A–G in 1999	6.93	5.09	6.54	6.08		
School has 6th form <sup>3</sup> ?	0.45	0.50	0.34	0.47		

<sup>21</sup> The neighbourhood data we used was based at ward level which can cover as few as 400 people to as many as 30,000 people, but usually between 5,000 to 7,000 individuals. There are just under 9,000 wards in England and Wales.

Distance to nearest year 12 provider	1,630.7	1051.1	1,951.6	1480.8
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<sup>1</sup> A higher score indicates a higher incidence of deprivation. Scores across different measures are not comparable. <sup>2</sup> GCSE exams are taken in the last year of compulsory education and are graded A to G. <sup>3</sup> The 6<sup>th</sup> form is the two years of post-compulsory schooling. The all England data is calculated on the basis of ward level data (small subdivisions of municipalities). There are 8414 ward in England. <sup>4</sup> This data is 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. Child benefit is payable for all children under 16 and all those over 16 in secondary education. It has nearly 100% take-up. As very few 19 year-olds are in secondary – rather than tertiary - education, this figure is an underestimate (by about 1/3) of the proportion of young people staying in post-compulsory education and should be understood as a proxy for this figure.

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 under 3 percentage points higher in our pilot areas than our control areas. This highlights the importance of appropriately weighting our control group as if we do not take this pre-reform difference into account we are likely to underestimate the EMA impact.

To control for differences between pilot and control areas we also use individual level data from our survey as well as this administrative and local area data. The variables we use include individual based characteristics on prior achievement, household income, parental occupation and education, household composition, ethnicity; childhood variables on early health problems, early childcare and grandparental inputs, special needs, and mobility in early. We have also matched on publicly available data on the pre-reform quality of the child’s nearest Year 11 state school<sup>22</sup> and distance to the nearest state year 12 educational provider (post 16 education)<sup>23</sup>. Summary Statistics for our remaining variables used in the analysis are provided in Appendix 2.

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<sup>22</sup> We have address grid references for every child in our survey as well as for every Year 11 school in the country. This 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 from that school.

<sup>23</sup> A number of studies have shown that distance to school is an important determinant of educational decisions (see Card ()).

### **3. The evaluation Methodology – Matching**

The outcome of interest in this paper will be participation in post-compulsory school, i.e. in year 12 and year 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 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, as they may have been following a successful and large scale randomisation. To allow for the fact that this was not going to be a randomised experiment, we have collected a large array of individual and local area characteristics, which should control for any relevant differences in the treatment and control areas before the program was introduced.

The method we use to balance the distribution of observable characteristics is propensity score matching. We provide a brief description in the Appendix.<sup>24</sup> It turns out that a simple fully interacted OLS model (fully interacted linear matching) imposing common support gives almost identical results to our preferred matching estimator. (Outline method here).

#### **Difference in Differences based on sibling data**

As a final step we also carry out some sensitivity analysis using difference in differences based on the behaviour of older siblings. In this case 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 reason this is not our

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<sup>24</sup> Rosenbaum and Rubin (1983) , Heckman, Ichimura and Todd (1994).

main evaluation method is that not all children have older siblings of the same gender and secondly 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 with High school or drop out. Nevertheless, this sensitivity analysis confirms the results we find with matching.

#### **4. The results**

As a first indication of the impact of the EMA we can carry out a simple difference in differences calculation based on aggregate data for the pilot and control areas<sup>25</sup>. The growth in participation before and after the reform in the urban pilot areas is 5.1 percentage points. This can be compared to a decline of 0.2 percentage point in the urban controls, which implies an EMA effect of 5.3 percentage points increase in schooling at the age of 17.<sup>26</sup>

These results are indicative that the policy did have an effect. However, we now investigate this in greater detail using matched comparisons between the treatment and control areas. This will ensure that the observable characteristics between the treated and control areas are balanced.

##### *Impact of the EMA on Year 12 Destinations*

In our work we compared the covariate balancing indicators of a number of matching methods including nearest neighbour matching, Mahalanobis matching and various kernel based methods. As seen above, the differences in characteristics between our control and pilot areas was already very good. This meant that our results were not very

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<sup>25</sup> See Department for Education and Skills (2004), “Statistics of Education: Participation in Education and Training by 16 and 17 Year Olds in Each Local Area in England, End 2001”, February 2004.

<sup>26</sup> Since these results are based on four aggregate observations no standard errors can be derived.

sensitive to the different methods used, but the best balancing of covariates was achieved using an Epanechnikov kernel with a bandwidth of 0.06<sup>27</sup>. Our preferred matching estimates were in all cases almost identical to fully interacted OLS estimates of the average treatment on the treated.

Table 4.1 shows estimates of the overall impact of the EMA on young people's initial decisions to remain in full-time education, to move into employment or to be inactive (NEET - Not in Education, Employment or Training). The EMA effect has been estimated separately for men and women using both matched and unmatched samples.<sup>28</sup> We also report OLS and fully interacted OLS estimates of the ATT for purposes of comparison<sup>29,30</sup>.

The EMA has had a positive and significant effect on post-compulsory education participation among eligible young people. The overall estimate for both rural and urban areas is 3.7 percentage points from a baseline of 67.7% in our sample of controls.<sup>31</sup> This increase has drawn young people from both employment and the inactivity group (NEET) in equal parts in the urban areas. This is significant because it shows that to a

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<sup>27</sup> Full details are available from the authors.

<sup>28</sup> Unmatched samples refer to just the mean of the relevant variable across the entire subgroup of interest. For example in unmatched samples the average participation in education among cohort 1 urban women in the pilot areas is compared with the mean in the control areas. It differs from the descriptive analysis later in this report since that compares only individuals from areas which have been deemed to have broadly similar characteristics.

<sup>29</sup> Of course, in the case of simple OLS the ATT is, by definition, the same as the ATE and ATNT.

<sup>30</sup> Our preferred matching estimate uses an Epanechnikov kernel with a bandwidth of 0.06. We tested a number of different methods of matching including Epanechnikov kernels with a variety of bandwidths, nearest neighbour matching, and Mahalanobis-metric matching method and based our decision on which method gave us the best covariance balancing indicators. In all cases our preferred matching estimator gave the best results in terms of various covariance balancing measures (see Appendix 3).

<sup>31</sup> The 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 figures work with age and not with school years as we do. Thus the aggregate figures relate 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 non-response is different between pilots and controls. In fact the results on attrition imply that any non-response will be balanced between pilots and controls.

large extent the policy is not displacing individuals from work, but from unproductive activities, thus implying an overall lower cost of providing this incentive to education.

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

	Participation in Pilot Areas	Unmatched	OLS	Fully Interacted OLS	Matched
<i>Total:</i>					
FT Education	69.2	3.9	3.8	4.4	4.5
(S.E)		(1.4)	(1.3)	(1.4)	(2.3)
Work/Training	16.4	-0.4	-1.0	-1.7	-1.7
(S.E)		(1.1)	(1.1)	(1.1)	(2.4)
NEET	14.5	-3.5	-2.8	-2.7	-2.7
(S.E)		(1.1)	(0.9)	(1.0)	(2.0)
Sample size:		5,315	5,315	5,310	
<i>Males:</i>					
FT Education	66.4	5.3	4.8	5.0	4.8
(S.E)		(2.0)	(2.0)	(2.1)	(2.3)
Work	19.7	-1.5	-2.1	-2.5	-2.9
(S.E)		(1.7)	(1.8)	(1.8)	(2.0)
NEET	13.9	-3.8	-2.7	-2.4	-1.8
(S.E)		(1.5)	(1.5)	(1.5)	(1.7)
Sample size:		2,653	2,653	2,643	2,643
<i>Females:</i>					
FT Education	71.9	2.5	2.9	4.0	4.2
(S.E)		(1.9)	(1.7)	(1.8)	(2.3)
Work	13.0	0.7	0.4	-0.4	-0.5
(S.E)		(1.4)	(1.4)	(1.5)	(2.0)
NEET	15.1	-3.2	-3.3	-3.6	-3.6
(S.E)		(1.6)	(1.2)	(1.4)	(1.7)
Sample size:		2,662	2,662	2,656	2,656

Notes: (1) All standard errors allow for clustering at school level. The standard errors reported for our matching estimator are based on 1000 replications and use stratification at the Local Education Authority level. Our fully interacted OLS model imposes common support for males and females.

Another important dimension of difference is between men and women. Women have higher post-16 school participation rates but it appears that the EMA goes some way to reducing this gap as the impact for men is slightly higher than that for women. This difference, however, is not significant at conventional levels.

#### *Impact of the EMA on Year 13 Destinations*

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 designed to last for two

years. Thus an important question is whether the impact of the EMA persists in the 2<sup>nd</sup> year, altering significantly the entire path post 16. To answer this question, in this section, we focus on individuals which 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 2<sup>nd</sup> 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. In Appendix 4 we show that the likelihood of remaining in the sample is higher for those who are eligible for the EMA relative to those who are not. However, the pattern of attrition is the same for the treatment and control areas, possibly implying that any biases due to attrition balance out. In Appendix 4 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 more 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 who do not drop out of the panel we obtain slightly lower estimates of the impact of EMA on full-time education participation with our male estimates being slightly but not significantly larger<sup>32</sup> and our female estimates being slightly but not significantly smaller<sup>33</sup>. Whilst this is reassuring, it is also clear that the distribution of observable characteristics has changed, as a result of attrition in the 2<sup>nd</sup> wave. In particular the ones who did not drop out of the sample originate from a better

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<sup>32</sup> 5.0 percentage points with a standard error of 2.7, compared to our estimate of 4.8 percentage points for the full sample (see Table 4.1).

<sup>33</sup> 3.5 percentage points with a standard error of (2.4), compared to our estimate of 4.2 percentage points for the full sample (see Table 4.1).

family background and were more likely to be in school in wave 1 of the data (see Table 4.2 below). In this sense the population for which we will be looking at the longer term outcomes is different than the one for which we can look at the shorter term ones. 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 program.

We define the potential outcomes that could occur two years after the introduction of the program 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. The important conclusion that comes from Table 4.2 is that where the EMA has been effective it has led to 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 it. They do not just sample it only to find that it is not for them and to drop out a few months later. It 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. EMA increased average retention rates by 4.0 percentage points (from 77.9 per cent to 81.9 per cent), with a particularly large effect for men (6.1 percentage points).

**Table 4.2: Impact of EMA on Year 12 and Year 13 destinations for Eligibles**

	Participation in Pilot Area	Participation in Control Area	Increase
<i>Male:</i>			
Education Y12 → Education Y13 (S.E)	58.7	50.6	8.1 (2.8)
Education Y12 → Other activity Y13 (S.E)	13.1	16.3	-3.1 (2.1)
Other activity Y12 → Education Y13 (S.E)	1.7	2.2	-0.5 (0.9)
Other activity Y12 → Other activity Y13 (S.E)	26.4	30.9	-4.5 (2.6)
Retention Rate (for those in Edn in Y12) (S.E)	81.7	75.7	6.1 (3.0)
Sample size			
<i>Female:</i>			
Education Y12 → Education Y13 (S.E)	63.4	59.0	4.4 (2.8)
Education Y12 → Other activity Y13 (S.E)	13.8	14.7	-0.9 (2.2)
Other activity Y12 → Education Y13 (S.E)	2.9	2.1	0.8 (0.8)
Other activity Y12 → Other activity Y13 (S.E)	19.9	24.2	-4.4 (2.3)
Retention Rate (for those in Edn in Y12) (S.E)	82.1	80.0	2.0 (2.8)
Sample size			
<i>TOTAL:</i>			
Education Y12 → Education Y13 (S.E)	61.1	54.9	6.2 (3.3)
Education Y12 → Other activity Y13 (S.E)	13.5	15.5	-2.0 (2.3)
Other activity Y12 → Education Y13 (S.E)	2.4	2.2	0.2 (1.1)
Other activity Y12 → Other activity Y13 (S.E)	23.0	27.5	-4.4 (2.5)
Retention Rate (for those in Edn in Y12) (S.E)	81.9	77.9	4.0 (3.3)
Sample size			

Note: The confidence intervals reported for our matching estimator are the bias corrected confidence intervals based on 1000 replications and allow for clustering at school level and use stratification at the Local Education Authority level.

### *Impact of EMA in Year 12 and Year 13 by Eligibility Groups*

We now turn to comparing the impact of the policy separately for those who are eligible for the full amount of the EMA, those who are only eligible for a fraction, because their parents have a higher income. The impact between the two groups may be different for a number of conflicting reasons. First, because the subsidy is lower it may have a lower 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 the results that follow we distinguish between full eligibility, partial eligibility and ineligibility to see if the impact of 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.

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

For the results presented in the following Tables we use fully interacted linear matching rather than kernel based matching to improve the precision of our estimates. As we saw above, both methods provide almost identical estimates of the impact of EMA, but for subgroup analysis precision and the use of analytical standard errors has clear advantages. Once again this does not affect our results in any significant way. Because we have imposed common support within each eligibility group, our samples are slightly smaller than those reported in Table 4.1.

**Table 4.3 Impact of EMA on Year 12 destination: all young people by eligibility**

	Males		Females		All	
	Participation in Pilot Area	Increase	Participation in Pilot Area	Increase	Participation in Pilot Area	Increase
<i>Fully eligible:</i>						
FT Education	66.0	6.7	68.2	6.8	67.1	6.7
(S.E)		(2.4)		(2.4)		(1.7)
Work/Training	18.6	-0.7	12.5	-2.3	15.5	-1.5
(S.E)		(2.2)		(1.9)		(1.5)
NEET	15.4	-6.0	19.3	-4.4	17.4	-5.2
(S.E)		(2.1)		(1.8)		(1.5)
Sample size		1,582		1,595		3,177
<i>Taper:</i>						
FT Education	66.6	4.0	77.5	-1.6	72.1	1.2
(S.E)		(3.6)		(2.9)		(2.3)
Work/Training	21.7	-6.5	14.1	3.6	17.9	-1.4
(S.E)		(3.3)		(2.3)		(2.0)
NEET	11.7	2.5	8.4	-2.1	10.0	0.2
(S.E)		(2.1)		(2.0)		(1.5)
Sample size		1,036		1,035		2,071
<i>Ineligible:</i>						
FT Education	75.7	3.2	88.5	-2.2	81.7	0.7
(S.E)		(3.3)		(2.6)		(2.1)
Work/Training	18.0	0.1	7.4	2.5	13.1	1.2
(S.E)		(2.9)		(1.9)		(1.8)
NEET	6.3	-3.4	4.2	-0.3	5.3	-1.9
(S.E)		(2.1)		(2.0)		(1.5)
Sample Size		762		685		1,447

Note: The standard errors reported for our matching estimator are based on 1000 replications and allow for clustering at school level and use stratification at the Local Education Authority level.

Among those who were estimated to be eligible for a full EMA award, EMA increased full-time education participation in Year 12 by 4.8 percentage points for young men and 5.1 percentage points for young women (Table 4.2). For those estimated to be eligible for only a partial award, the corresponding figures are 0.2 percentage points and 4.5 percentage points. Thus for men the response of those fully eligible is much larger than in the population who are facing the taper. The same does not seem to be true for women, where the difference between the two groups is small.

The results of comparing ineligible young people in the pilot and control areas are given in table 4.3. Although the education participation rates amongst ineligible individuals are estimated that have increased for both males and females, by 2.8 and  $-2.5$  percentage points respectively, neither of these results is statistically significant. Even taken for the entire ineligible samples, there is no evidence of any large or statistically significant spillover effects. This is to be expected since the effect of the policy on the eligibles is not large enough to make an impact on others. It is also comforting because it indicates that there is no serious reason to believe that unobserved area effects are responsible for the EMA effects we estimated for the eligible population.

.In Table 4.4 we again undertake this analysis for the second to see if the findings of the previous section are altered when we split by eligibility groups. The results of doing this are contained in Table 4.4.

**Table 4.4 Impact of EMA on Year 13 destinations: by eligibility**

	Males		Females		All	
	Particip- ation in Pilot Area	Increase	Particip- ation in Pilot Area	Increase	Particip- ation in Pilot Area	Increase
<i>Fully eligible:</i>						
Education Y12 → Education Y13 (S.E)	57.2	6.3 (3.0)	58.7	8.1 (2.7)	58.0	7.2 (2.3)
Education Y12 → Other activity Y13 (S.E)	14.9	1.0 (2.5)	16.0	1.3 (2.4)	15.5	1.2 (1.8)
Other activity Y12 → Education Y13 (S.E)	1.2	-2.0 (1.3)	3.5	1.3 (1.1)	2.4	-0.3 (0.8)
Other activity Y12 → Other activity Y13 (S.E)	26.7	-5.3 (3.0)	21.9	-10.7 (2.9)	24.2	-8.1 (2.1)
Retention Rate (for those in Edn in Y12) (S.E)	79.4	0.8	78.6	1.1	79.0	0.9
Sample size		1,025		1,088		2113
<i>Partially eligible:</i>						
Education Y12 → Education Y13 (S.E)	59.9	10.9 (4.1)	69.4	0.6 (2.9)	64.9	5.6 (2.5)
Education Y12 → Other activity Y13 (S.E)	11.7	-5.6 (3.1)	11.3	-4.9 (2.9)	11.5	-5.2 (2.1)
Other activity Y12 → Education Y13 (S.E)	2.5	0.9 (0.9)	2.3	1.2 (1.0)	2.4	1.1 (0.7)
Other activity Y12 → Other activity Y13 (S.E)	25.9	-6.3 (3.9)	17.1	3.0 (2.5)	21.3	-1.5 (2.3)
Retention Rate (for those in Edn in Y12) (S.E)	83.6	9.8	86.0	5.1	84.5	6.9
Sample size		802		849		1651

Note: The confidence intervals reported for our matching estimator are the bias corrected confidence intervals based on 1000 replications and allow for clustering at school level and use stratification at the Local Education Authority level.

This adds an extra dimension to the analysis and shows that why the initial participation effects are concentrated amongst those who are fully eligible, the retention effects are biggest for those who only receive a partial award. Thus we see that for who get less than the maximum payment, the effect only really kicks in the second year of the program by improving the probability of retention.

*Who gets the payment – Does it matter?*

In one of the EMA variants piloted the payment was made to 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 parents it should not make much difference.

In order to investigate this we again use fully interacted linear matching with common support and compare outcomes in variants 1 and variants 3 where the only difference in the scheme is who received the weekly payment. We also estimate within eligibility group and focus on first year impacts. The results of doing this are shown in Table 4.5

**Table 4.5 Impact of EMA on Year 12 destinations: by eligibility and receipt**

	Variant 1		Variant 3	
	Males	Females	Males	Females
<i>Fully eligible:</i>				
FT Education	14.2	6.7	5.6	4.0
(S.E)	(3.8)	(3.5)	(3.1)	(2.9)
Work/Training	-4.3	0.5	-2.8	-0.4
(S.E)	(3.4)	(2.7)	(2.9)	(2.4)
NEET	-9.9	-7.3	-2.8	-3.6
(S.E)	(2.8)	(3.1)	(2.6)	(2.5)
Sample size	654	692	700	678
<i>Partially Eligible:</i>				
FT Education	0.6	-8.2	7.4	6.3
(S.E)	(4.0)	(4.3)	(5.5)	(4.0)
Work/Training	-2.7	7.9	-10.9	-1.5
(S.E)	(3.8)	(3.6)	(5.0)	(3.0)
NEET	3.3	0.3	3.5	-4.8
(S.E)	(3.5)	(3.6)	(2.7)	(2.6)
Sample size	394	430	449	381

Note: All standard errors allow for clustering at school level

From Table 4.5 we see that for Variant 1, where the money is paid directly to the child, the EMA impact is concentrated solely among those who are fully eligible. Participation in full-time education for males is increased by 14.2 percentage points and full time education participation for females by 6.7 percentage points. All of this increase in participation is drawn from the NEET group. There is no significant full-time education impact for individuals who are partially eligible.

The story is very different for the variant where the payment is made to the child's mother. The impact is now spread much more evenly among all groups who are eligible and is much very similar for boys and girls.

Whilst the overall impact of the EMA on full-time education participation is very similar for both variants, there is a big difference in impact by eligibility groups. The variant in which it is paid to the child has a much larger impact on those who are fully eligible whereas the variant where it is paid to the mother has a much more even effect across all eligibility groups. This finding has obvious policy interest and suggests that if the key interest is in increasing participation among those from the poorest backgrounds (those from families earning less than £13,000 per annum) then payment to the child may be preferred, whereas if the government is keen to impact across the whole eligibility distribution then payment to the mother may be more effective – at least in terms of initial staying on decisions.

In Table 4.6 we look at the results for Year 13 for those who do not drop out of the panel from our sample. Unfortunately sample sizes are quite small which affects precision but we see that by the second year, the results across all groups are much more similar. Again, for those who receive only a partial payment, there appears to be a bigger retention effect. By Year 13, the only big difference between the variants is that variant 1 is more effective drawing people from the group who would not have otherwise

participated in education and that variant 1 is more effective in increasing the participation of men who are fully eligible.

**Table 4.6 Impact of EMA on Year 13 destinations: by eligibility and receipt**

	Variant 1		Variant 3	
	Males	Females	Males	Females
<i>Fully eligible:</i>				
Education Y12 → Education Y13	13.8	4.4	7.3	6.2
(S.E)	(4.6)	(3.9)	(4.2)	(3.5)
Education Y12 → Other activity Y13	2.6	2.5	-1.2	-0.5
(S.E)	(3.4)	(3.3)	(3.2)	(2.8)
Other activity Y12 → Education Y13	-2.0	-0.3	-2.2	2.4
(S.E)	(1.6)	(1.7)	(1.3)	(1.5)
Other activity Y12 → Other activity Y13	-14.5	-6.7	-3.8	-8.1
(S.E)	(4.0)	(4.2)	(4.0)	(3.7)
Sample size	416	459	456	409
<i>Partially Eligible:</i>				
Education Y12 → Education Y13	10.0	-3.0	13.3	9.0
(S.E)	(5.3)	(4.3)	(5.6)	(4.2)
Education Y12 → Other activity Y13	-4.6	-2.1	-7.1	-5.3
(S.E)	(4.6)	(3.5)	(4.2)	(3.8)
Other activity Y12 → Education Y13	0.4	1.3	0.6	1.8
(S.E)	(1.1)	(1.3)	(1.3)	(1.7)
Other activity Y12 → Other activity Y13	-5.9	3.8	-6.9	-5.5
(S.E)	(4.6)	(4.2)	(5.4)	(3.6)
Sample size	281	338	336	306

Note: All standard errors allow for clustering at school level

*Does the impact vary by prior socio-economic background and academic achievement?*

A recent survey of education policy in England by Johnson (2004) has highlighted that one of the key aims of policies like EMA is to improve post compulsory staying-on rates for children from deprived social backgrounds. 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” (pp 177-178).

It is therefore of interest to see whether the EMA has been successful in improving the education participation of children from poorer socio-economic backgrounds. To look at this we split our sample of children by their socio-economic group.

We split our sample into two groups: a high socio-economic group and a low socio-economic group. Our groups are based on the Socio-economic Group (SEG) classification which is based on individual's occupational unit group (1990 SOC group), employment status and the size of establishment in which they work. A full description of the SEG classification is explained in Office for National Statistics (1991). We classify the household according to the highest SEG group of their parents (if more than one parent). Our low group consists of households where no parent is in work or if the highest occupation of the parents involves only unskilled or semi-skilled manual work.

The results of doing this are presented in Table 4.7. We see that almost all of the impact of EMA is concentrated on children coming from families from poor socio-economic backgrounds. For this group, male participation is increased by 6.1 percentage points in Year 12 and 8.7 percentage points in Year 13. The corresponding figure for females from this group are 8.2 percentage points and 12.2 percentage points.

What is interesting to see from this table, is that whilst for the higher socio-economic group, girls are much more likely to stay on than boys, this is not true for girls coming from poor socio-economic backgrounds. The EMA plays an important role in significantly improving the participation of both girls and boys from this vulnerable group.

**Table 4.7 Impact of EMA on Year 12 and Year 13 destinations of eligibles: by prior socio-economic group**

	Males		Females	
	Participation in Pilot Area	Increase	Participation in Pilot Area	Increase
<i>Low Socio-economic Group</i>				
Education Y12 → Education Y13 (S.E)	54.9	8.7 (3.7)	55.0	12.2 (4.1)
Education Y12 → Other activity Y13 (S.E)	13.2	-2.6 (3.7)	14.4	-4.0 (3.1)
Other activity Y12 → Education Y13 (S.E)	1.9	-1.4 (1.6)	3.9	1.9 (1.3)
Other activity Y12 → Other activity Y13 (S.E)	29.9	-4.7 (3.9)	26.7	-10.1 (3.7)
Sample size	717		786	
<i>High Socio-economic Group</i>				
Education Y12 → Education Y13 (S.E)	64.8	4.1 (2.4)	74.5	2.7 (2.1)
Education Y12 → Other activity Y13 (S.E)	11.5	-0.7 (1.6)	10.7	-0.8 (1.7)
Other activity Y12 → Education Y13 (S.E)	1.6	-0.4 (0.8)	1.8	-0.4 (0.8)
Other activity Y12 → Other activity Y13 (S.E)	22.0	-3.0 (2.3)	13.0	-1.5 (1.7)
Sample size	1,752		1,727	

Note: All standard errors allow for clustering at school level.

**[I have also done a table splitting by GCSE results 9 & less points in Maths and English vs 10+ points] – see Table 4.8 below. I am not sure if we really want to put this in the paper – but the effect is largest for those with poorest prior achievement, but the staying on rates for high achievers is already very high so the chance of EMA impacting is much reduced]. I will leave it up to you Costas. Carl can fill you in on the full details of this]**

**Table 4.8 Impact of EMA on Year 12 and Year 13 destinations of eligibles: by prior academic achievement**

	Males		Females	
	Participation in Pilot Area	Increase	Participation in Pilot Area	Increase
<i>Low Prior Academic Achievement</i>				
Education Y12 → Education Y13 (S.E)	47.4	7.7 (3.1)	51.9	6.7 (3.5)
Education Y12 → Other activity Y13 (S.E)	16.9	-4.9 (2.8)	18.9	-2.6 (3.3)
Other activity Y12 → Education Y13 (S.E)	1.8	-2.1 (1.1)	4.1	1.0 (1.2)
Other activity Y12 → Other activity Y13 (S.E)	33.8	-0.7 (3.0)	25.0	-5.1 (3.3)
Sample size	1,134		1,100	
<i>High Prior Academic Achievement</i>				
Education Y12 → Education Y13 (S.E)	84.4	1.6 (2.4)	89.4	2.7 (2.6)
Education Y12 → Other activity Y13 (S.E)	5.7	0.8 (1.5)	5.6	-1.4 (1.7)
Other activity Y12 → Education Y13 (S.E)	1.6	1.5 (0.6)	0.7	-0.2 (0.8)
Other activity Y12 → Other activity Y13 (S.E)	8.4	-3.9 (2.0)	4.3	-1.1 (1.7)
Sample size	1,061		1,244	

Note: All standard errors allow for clustering at school level.

### Sensitivity Analysis

To validate our results we have carried out a more limited analysis using as a comparison group the older siblings of the children in our pilot and control areas. In this case the approach we use is difference in differences conditioning on pre-program characteristics. We include a full set of cohort and area dummies. We find an EMA effect of 0.0835 (with a standard error of 0.0281), which is similar albeit larger than the effect we reported above. The difference is not significant at conventional levels.<sup>34</sup>

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 as well as in the final year. We find that in all previous periods the “effect” is not significant and the estimate is close to zero. In the final period we obtain a positive and significant effect, again corroborating and strengthening our results.

## 5. Conclusions

Despite a steady increase the participation in education following completion of compulsory schooling in England remained relatively low at about 57% (is this number correct? When is it from). The government decided to pilot an incentive scheme to encourage more pupils from low income families to stay on in school. This schooling subsidy program lasted for a maximum of two years following the end of compulsory schooling. It is entitled the Education Maintenance Allowance (EMA) has now become national policy. In this paper we use a dataset collected by us for the purposes of evaluating the impact of this schooling subsidy program on school participation in England. Our results imply that the scope for affecting education decisions using subsidies to education can be substantial.

More specifically, the results imply that the Educational Maintenance Allowance (EMA) has raised significantly the stay on rates past the age of 16. The initial impact is around 4.5 percentage points while having no effect on ineligibles. The effect for men is slightly higher than for women helping to plug the gap in stay-on rates between girls and boys. 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% of earnings.

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<sup>34</sup> The standard error allows for serial correlation and cluster effects.

The results also suggest that the impact of EMA on participation actually increases in the following year. For those who get the full payment, the increased participation is maintained whereas for those who get partial payment, retention is actually significantly improved. 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 program into education. Importantly, about half of the increase in school participation is due to a decline in inactivity, rather than work. This reduces the implicit costs of the program since the foregone earnings for them are zero.

It appears that the overall impact of EMAs is similar regardless of whether it is paid to the mother or child. But this varies by eligibility and the variant where it is paid to the child is more effective in improving participation rates amongst those who come from the poorest families. Finally, it appears that the EMA had its largest impact on children coming from families from the poorest socio-economic background (based on parents' occupation). This is a particular policy concern and it appears that the EMA has made important inroads in improving the prospects of these children.

The results in this paper demonstrate that a conditional payment to 16 and 17 year olds can significantly reduce school drop-out rates. Of course a number of important questions remain. First, we do not know whether liquidity constraints are an important factor in driving the estimated effects. A second and related issue is that we do not know what returns those induced into staying on by the subsidy will enjoy but most recent estimates in the UK suggest that the return is around 10-11 per cent for men and around

18 per cent for females<sup>35</sup>. Finally, we really have very little idea of how these returns may change when the programme is rolled out in September 2004 and the supply of educated workers changes. 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.

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<sup>35</sup> See Dearden, McGranahan and Sianesi (2004). These estimates are their best estimates of the average treatment effect on the non-treated for staying on in school past the compulsory school leaving age.

## Appendix 1: Indicators used in each deprivation score

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

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](http://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
<i>Characteristics used in matching</i>			
Weekly family income	389.012	387.505	391.946
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
Owner occupier	0.693	0.686	0.709
Council or Housing Association	0.253	0.266	0.226
Has stated special needs	0.092	0.093	0.090
Mother's age	39.859	39.867	39.843
Father's age	30.096	30.301	29.696
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 - Covariate balancing indicators (best specification): before and after matching

Matching Estimator	$N_1$	$N_0$	Probit pseudo R <sup>2</sup>	Probit pseudo R <sup>2</sup>	$P > \chi^2$	Median bias	Median bias	% lost to common support
	Before	Before	Before	After	After	Before	After	After
			(1)	(2)	(3)	(4)	(5)	(6)
<i>Males:</i>								
Nearest Neighbour	1,753	900	0.085	0.029	0.000	3.9	5.3	0.4
Mahalanobis-metric	1,753	900	0.085	0.086	0.000	3.9	7.4	0.4
Epanechnikov (bw=0.01)	1,753	900	0.085	0.012	0.740	3.9	2.5	0.7
Epanechnikov (bw=0.06)	1,753	900	0.085	0.010	0.921	3.9	2.3	0.4
<i>Females:</i>								
Nearest Neighbour	1,771	891	0.104	0.030	0.000	3.3	3.6	0.2
Mahalanobis-metric	1,771	891	0.104	0.103	0.000	3.3	7.8	0.2
Epanechnikov (bw=0.01)	1,771	891	0.104	0.015	0.306	3.3	2.2	1.0
Epanechnikov (bw=0.06)	1,771	891	0.104	0.014	0.510	3.3	1.5	0.2

#### Notes:

- (1) Pseudo R<sup>2</sup> from probit estimation of the conditional treatment probability, giving an indication of how well our matching regressors  $X$  explain the relevant educational choice.
- (2) Pseudo R<sup>2</sup> from a probit of  $D$  on  $X$  on the *matched* samples, to be compared with (1).
- (3)  $P$ -value of the likelihood-ratio test after matching, testing the hypothesis that the regressors are jointly insignificant, i.e. well balanced in the two matched groups.
- (4) Median absolute standardised bias before and after matching, median taken over all the matching.
- (5) Following Rosenbaum and Rubin (1985), for a given covariate  $X$ , the standardised difference *before* matching is the difference of the sample means in the full treated and non-treated subsamples as a percentage of the square root of the average of the sample variances in the full treated and non-treated groups. The standardised difference *after* matching is the difference of the sample means in the matched treated (i.e. falling within the common support) and matched non-treated subsamples as a percentage of the square root of the average of the sample variances in the full treated and non-treated groups.

$$B_{before}(X) \equiv 100 \frac{\bar{X}_1 - \bar{X}_0}{\sqrt{(V_1(X) + V_0(X))/2}} \quad B_{after}(X) \equiv 100 \frac{\bar{X}_{1M} - \bar{X}_{0M}}{\sqrt{(V_1(X) + V_0(X))/2}}$$

Note that the standardisation allows comparisons between variables  $X$  and for a given variable  $X$ , comparisons before and after matching.

- (6) Share of the treated group falling outside of the common support, imposed at the boundaries.

## Appendix 4: Attrition between wave 1 and wave 2

**Table A4.1 Probability of Attrition between wave 1 and wave 2.**

	Marginal Effect	Standard error
Partially Eligible	-0.002	0.024
Fully Eligible	-0.039	0.015
Pilot Area	0.005	0.012
Male	0.019	0.011
Weekly family	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 stated 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
1 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
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
per cent not staying on post 16	-0.002	0.001
per cent 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	6838	
Observed probability	0.253	

#### Appendix 4: Identification and Estimation method

Suppose the outcome of an individual with characteristics  $X_i$  who is exposed to the EMA is  $Y_i^1$ . The same individual would have outcome  $Y_i^0$  were she/he not to be exposed to the treatment. Obviously, either one or the other outcome is observed. The impact of the policy for the  $i$ th individual ( $Y_i^1 - Y_i^0$ ) is thus not observed. The main evaluation parameter that we will estimate is the impact of treatment on the treated, i.e.  $E(Y_i^1 - Y_i^0 | P_i = 1)$ , where  $P$  is one for individuals in the pilot areas and zero in the control areas. What we do observe is  $E(Y_i^1 | P_i = 1)$ , which is the average participation rate for those exposed to the EMA. To construct the counterfactual  $E(Y_i^0 | P_i = 1)$  we assume that  $E(Y_i^0 | P_i = 1, X_i) = E(Y_i^0 | P_i = 0, X_i)$  which means that given the observable characteristics the allocation to treatment and control is random. Under this assumption it is now well known (see Rosenbaum and Rubin, 1983) that we can reduce the dimension of the conditioning set from  $X$  to just  $\Pr(P_i = 1 | X_i)$ , i.e. the propensity score which is simply the probability of being allocated to the pilot given observed characteristics. This makes the computational exercise feasible and simple. Thus, given

the original matching assumption we can also write that  $E(Y_i^0 | P_i = 1, \Pr(P_i = 1, | X_i)) = E(Y_i^0 | P_i = 0, \Pr(P_i = 1, | X_i))$  It follows that we can estimate the counterfactual by the sample analogue of

$$E(Y_i^0 | P_i = 1) = E_{F_1}[E(Y_i^0 | P_i = 0, \Pr(P_i = 1, | X_i))],$$

where  $E_{F_1}$  denotes an expectation with respect to the distribution of the propensity score in the treatment sample.

Implementing this involves the following steps. In the first step the propensity score is estimated. In the second step we estimate the conditional expectation of the outcome in the control areas given the propensity score using a number of methods. It turns out that for our particular policy experiment, using an Epanechnikov kernel with bandwidth of 0.06 gives us the best covariate balancing indicators amongst a range of matching estimators that we considered. We are careful to ensure that all observations whose value of the propensity score is outside the range of the propensity score in the treatment sample are deleted. This imposes common support avoiding a major source of bias (see Heckman, Ichimura and Todd, 1997). Finally the overall average is constructed using as weights the distribution of the propensity score in the pilot areas.

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