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## 1. Introduction

Education has 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 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, 2004) shows that in a league table of 30 developed OECD countries the US has slipped down the tables from 1st to 10th place with 87 per cent of 25-34 year olds having at least upper secondary education in 2002.[4] Elsewhere the UK has fallen to 22nd place, down from 13th place just a generation ago[5], with just 70 per cent of 25-34 year olds with upper secondary education as of 2001 which is 17 percentage points lower than the corresponding figures for the US despite continuing problems with drop-out rates in some US cities[6]. This compares with over 90 per cent in countries such as the Japan, Korea, and Sweden: thus the proportion of youngsters

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

[2] Juhn, Murphy and Pierce (1995).

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

[4] In the US, 84 per cent of 55-64 year olds had at least upper secondary education in 2002 which is the highest of all 30 countries covered.

[5] 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. 56 per cent of UK 55-64 year olds had at least upper secondary education in 2002 which places the UK 13th out of the 30 countries covered – see OECD (2004), Table A2.2.

[6] 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 per cent of secondary-level students aged 16 and older according to the latest US Department of Education figures (see http://www.ed.gov).

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.

There has been worldwide focus on school dropout problems and a number of policies devised to help reduce school dropout 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 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 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[7] and for the remaining for 28 OECD countries ranges from 14 to 18[8].

Making secondary education free and increasing the compulsory school leaving age had an effect on school dropout and completion rates and a number of these reforms have been analysed in previous research.[9] 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

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[7] 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.

[8] See OECD (2004), Table C.1.2.

[9] 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.

leaving age.[10] The available evidence on the importance 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 use of the evaluation data from the PROGRESA program in Mexico (see Schultz, 2003). 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 and reject the hypothesis. These results highlight the importance of direct evidence of the impact of subsidies for policy evaluation. Another interesting recent 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.

This paper examines the impact of a program that subsidizes children to remain in school for up to two years beyond the statutory age. 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

[10] 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 and Santiago, 2005), and the recently introduced *Familias en Accion* program in Colombia modelled on PROGRESA (Attanasio *et al.* 2005).

otherwise of a conditional cash transfer to 16 and 17 year olds on school dropout rates is of general policy interest to policy makers worldwide[11].

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[12]. 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[13]. However, it can provide valuable information on whether such subsidies, which effectively reduce the cost of education, actually reduce school dropout rates, which at present is the central policy concern[14].

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 increase participation, but also retention in full-time education. The initial effects are largest for those who receive the maximum payment although

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11 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).

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.

13 Some papers that have looked at this question include Cameron and Heckman (1998), Carneiro and Heckman(2002), Cameron and Taber (2000), Dale and Krueger (1999) and Keane and Wolpin (1997) amongst others.

14 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 14-19 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.

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-income families who qualify for the full EMA payment have very high drop out rates and the EMA has proved especially effective in plugging the dropout 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 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[15].

**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 whom it was paid (parent or 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[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

[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 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.

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

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 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 less than 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[18]. 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[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), prior school achievement as well as administrative data on the quality of schooling in the child's neighbourhood as well as

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

[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 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.

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 (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. 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 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 geographic mobility in early life. We have also matched on

[20] 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.

publicly available data on the pre-reform quality of the child's nearest Year 11 state school[21] and distance to the nearest state year 12 educational provider (post 16 education)[22]. Summary Statistics for our remaining variables used in the analysis are provided in Appendix 2.

[21] 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.

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

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

| | Pilot Areas | | Control Areas | | All England | |
|---|---|---|---|---|---|---|
| | 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[1] | 38.36 | 17.00 | 37.05 | 18.64 | 21.70 | 15.39 |
| Income[1] | 30.14 | 11.79 | 29.93 | 13.62 | 18.86 | 11.31 |
| Employment[1] | 16.66 | 6.23 | 16.38 | 7.14 | 10.19 | 6.49 |
| Health Deprivation and Disability[1] | 1.04 | 0.58 | 0.97 | 0.68 | 0.00 | 0.92 |
| Education, Skills and Training[1] | 0.78 | 0.98 | 0.70 | 0.82 | 0.00 | 0.87 |
| Housing[1] | 0.47 | 0.83 | 0.34 | 1.00 | 0.00 | 0.92 |
| Geographical Access to Services[1] | –0.53 | 0.46 | –0.60 | 0.54 | 0.00 | 0.87 |
| Child poverty [1] | 43.78 | 17.12 | 42.70 | 19.61 | 26.74 | 17.02 |
| *Education drop-out rates 1998* | | | | | | |
| Per cent dropping out of school at | 38.9 | | 35.8 | | 30.0 | |
| Per cent not staying on at school[5] | 69.15 | 8.99 | 66.63 | 10.50 | | |
| *Nearest school data* | | | | | | |
| Class sizes 1999 | 21.43 | 2.29 | 21.41 | 2.23 | | |
| Authorised absences (per year) | 8.69 | 1.99 | 8.86 | 2.63 | | |
| % getting 5 GCSE[2] A–C in 1999 | 35.35 | 17.72 | 35.48 | 15.82 | | |
| % getting 0 GCSE[2] A–G in 1999 | 6.93 | 5.09 | 6.54 | 6.08 | | |
| School has 6th form[3] ? | 0.45 | 0.50 | 0.34 | 0.47 | | |
| Distance to nearest year 12 provider | 1,630.7 | 1051.1 | 1,951.6 | 1,480.8 | | |

[1] A higher score indicates a higher incidence of deprivation. Scores across different measures are not comparable. [2] GCSE exams are taken in the last year of compulsory education (Year 11) and are graded A to G. [3] The 6th form is the two years of post-compulsory schooling, Years 12 and 13. The all England data is calculated on the basis of ward level data (small subdivisions of municipalities). There are 8,414 wards in England. [4] This data is 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[23]. [5] 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 (ward). 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.

## 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

[23] This is necessary as in 2 of our control LEAs we sampled half as many individuals as in our other control LEAs.

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 Appendix 5[24]. It turns out that a simple fully interacted OLS model imposing common support gives almost identical results to our preferred matching estimator, confirming the findings of Blundell, Dearden and Sianesi (2005).

As a final step we also carry out some sensitivity analysis using difference in differences based on aggregate data and on the behaviour of older siblings. In the former case we consider aggregate school participation data for 16 year olds including eligible and ineligible pupils[25]. In the latter 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 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 in

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[24] Rosenbaum and Rubin (1983), Heckman, Ichimura and Todd (1994).

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

education or drop out. Nevertheless, this sensitivity analysis confirms the results we find with matching.

In all cases the standard errors are computed using the block bootstrap with a cluster being defined as a school.

## 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 gender) 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).

For the purposes of demonstrating robustness across econometric methods we present results based on comparing simple means (unmatched), linear OLS, OLS with interactions for heterogeneous effects (fully interacted linear matching) and non-parametric kernel based matching[26]. All give very similar results and in our discussion from now on we focus on fully interacted linear matching allowing always for common support.

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[27]. This

[26] 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 cased our preferred matching estimator gave the best results in terms of various covariance balancing measures (see Appendix 3).

[27] 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

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 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. This does raise the issue of the quality of individuals attracted to education from the subsidy, since it seems to consist largely of individuals 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 the have to attend one can hypothesize they are receiving valuable training. Ultimately however this can only be evaluated using eventual labour market outcomes, not available to us.

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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.

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

| | Participation in Pilot Areas | Unmatched | OLS | Fully Interacted OLS | Matching Estimate |
|---|---|---|---|---|---|
| *Total:* | | | | | |
| FT Education | 69.2 | 3.9 | 3.8 | 4.5 | 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 | 5,299 | 5,299 |
| *Males:* | | | | | |
| 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 |
| *Females:* | | | | | |
| 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: All standard errors allow for clustering at school level. The standard errors reported for our matching estimator are based on 1,000 replications and use stratification at the Local Education Authority level. Our fully interacted OLS model imposes common support for males and females.

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

*4.2 The longer term impact of the EMA.*

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 2nd year, altering significantly the entire path post 16. 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 2nd 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 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 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 overall impact of EMA on full-time education participation with our male estimates being slightly but not significantly larger[28] and our female estimates being slightly but not significantly smaller[29]. Whilst this is reassuring, it is also clear that the distribution of observable characteristics has changed, as a result of attrition in the 2nd wave. In particular the ones who did not drop out of the sample originate from a better

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[28] 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).

[29] 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 using our preferred kernel matching technique. The important conclusion that comes from Table 4.2 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 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 | Impact (Kernel Matching) |
|---|---|---|
| *Male:* | | |
| Education Y12 → Education Y13 | 58.7 | 8.1 |
| (S.E) | | (2.8) |
| Education Y12 → Other activity Y13 | 13.1 | –3.1 |
| (S.E) | | (2.1) |
| Other activity Y12 → Education Y13 | 1.7 | –0.5 |
| (S.E) | | (0.9) |
| Other activity Y12 → Other activity Y13 | 26.4 | –4.5 |
| (S.E) | | (2.6) |
| Retention Rate (for those in Edn in Y12) | 81.7 | 6.1 |
| (S.E) | | (3.0) |
| Sample size | 1211 | |
| *Female:* | | |
| Education Y12 → Education Y13 | 63.4 | 4.4 |
| (S.E) | | (2.8) |
| Education Y12 → Other activity Y13 | 13.8 | –0.9 |
| (S.E) | | (2.2) |
| Other activity Y12 → Education Y13 | 2.9 | 0.8 |
| (S.E) | | (0.8) |
| Other activity Y12 → Other activity Y13 | 19.9 | –4.4 |
| (S.E) | | (2.3) |
| Retention Rate (for those in Edn in Y12) | 82.1 | 2.0 |
| (S.E) | | (2.8) |
| Sample size | 1295 | |
| *TOTAL:* | | |
| Education Y12 → Education Y13 | 61.1 | 6.2 |
| (S.E) | | (3.3) |
| Education Y12 → Other activity Y13 | 13.5 | –2.0 |
| (S.E) | | (2.3) |
| Other activity Y12 → Education Y13 | 2.4 | 0.2 |
| (S.E) | | (1.1) |
| Other activity Y12 → Other activity Y13 | 23.0 | –4.4 |
| (S.E) | | (2.5) |
| Retention Rate (for those in Edn in Y12) | 81.9 | 4.0 |
| (S.E) | | (3.3) |
| Sample size | 2506 | |

Note: All standard errors allow for clustering at school level. The standard errors reported for our matching estimator are based on 1,000 replications and use stratification at the Local Education Authority level.

*4.3 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 an income higher than £13,000. 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 that were not means-tested.

For the results presented in the following Tables and all subsequent analysis we use fully interacted linear matching[30].

[30] The point estimates from these model were always very close to our preferred kernel matching estimates but have greater precision. All kernel based matching estimates are available from the authors on request.

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

| | Males Particip-ation in Pilot Area | Impact | Females Particip-ation in Pilot Area | Impact | All Particip-ation in Pilot Area | Impact |
|---|---|---|---|---|---|---|
| *Fully eligible:* | | | | | | |
| 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 |
| *Taper:* | | | | | | |
| 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 |
| *Ineligible:* | | | | | | |
| 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 allow for clustering at school level.

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). Thus the response of those fully eligible is larger than in the population who are facing the taper. 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. 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 both 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 Who gets the payment – Does it matter?*

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) and the level of retention bonuses does not result in any significant impact on measured outcomes in Years 12 or Year 13 compared to those in variant 1.

In one of the EMA variants piloted (variant 3) 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.

[31] He says, "The UK has a relatively low staying-on rate in full time education after age 16. Given high returns this is,

In order to investigate this we compare outcomes in variants 1 and variants 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 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. We estimate the effect by sex and eligibility group but we only report the results by eligibility group, as this is where important differences are detected.[32] 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 |
|---|---|---|
| *Fully eligible:* | | |
| FT Education | 10.3 | 4.8 |
| (S.E) | (2.6) | (2.1) |
| Work/Training | -1.8 | -1.6 |
| (S.E) | (2.2) | (1.9) |
| NEET | -8.6 | -3.2 |
| (S.E) | (2.1) | (1.8) |
| Sample size | 1346 | 1378 |
| *Partially Eligible:* | | |
| FT Education | -4.0 | 6.9 |
| (S.E) | (2.9) | (3.6) |
| Work/Training | 2.5 | -6.6 |
| (S.E) | (2.6) | (3.0) |
| NEET | 1.6 | -0.3 |
| (S.E) | (2.5) | (1.9) |
| Sample size | 824 | 830 |

Note: All standard errors allow for clustering at school level

If we do not distinguish by eligibility, the impact of both variants is almost identical (4.9 percentage points for variant 1 and 5.6 percentage points for variant 3). However, this appears to hide interesting differences by eligibility, some of which are significant at the 5% level. From Table 4.5 we see that for variant 1, where the money

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).

is paid directly to the child, the EMA impact is concentrated solely among those who are fully eligible. Participation in full-time education is increased by 10.3 percentage points. Almost 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 ranging between 4.8 percentage points (fully eligible) and 6.9 percentage points (partially eligible) and these effects are not significantly different from each other.

These finding 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 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[33].

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 for those who are fully eligible is much more similar across variants. Again, for those who receive only a partial payment, there appears to be a bigger retention effect, but this is only for variant 3. By Year 13, the only big difference between the variants is that variant 3 is

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[32] The results by sex are available from the authors.

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

more effective in increasing participation amongst those who are partially eligible.

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

| | Variant 1 | Variant 3 |
|---|---|---|
| *Fully eligible:* | | |
| Education Y12 → Education Y13 | 8.7 | 6.7 |
| (S.E) | (3.0) | (2.7) |
| Education Y12 → Other activity Y13 | 2.5 | –0.8 |
| (S.E) | (2.4) | (2.1) |
| Other activity Y12 → Education Y13 | –1.1 | 0.3 |
| (S.E) | (1.2) | (1.0) |
| Other activity Y12 → Other activity Y13 | –10.4 | –6.1 |
| (S.E) | (2.9) | (2.7) |
| Sample size | 875 | 619 |
| *Partially Eligible:* | | |
| Education Y12 → Education Y13 | 2.9 | 11.3 |
| (S.E) | (3.4) | (3.5) |
| Education Y12 → Other activity Y13 | –3.2 | –6.2 |
| (S.E) | (2.8) | (2.8) |
| Other activity Y12 → Education Y13 | 0.9 | 1.2 |
| (S.E) | (0.9) | (1.1) |
| Other activity Y12 → Other activity Y13 | –0.6 | –6.2 |
| (S.E) | (3.1) | (3.3) |
| Sample size | 619 | 642 |

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

### *4.6 Does the impact vary by prior academic achievement?*

We have already seen that the EMA has its largest impact on kids from relatively poor families. Another key question is whether children with low prior academic achievement can be made to stay in school longer, possibly improving their skills before labour market entry. Thus, in Table 4.7 we present results where the sample is split by 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

[34] This 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 added together to obtain a score out of 16. Our high ability kids had a score of 10 or above and this was roughly just under half our sample.

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. This casts some doubt on the longer-term returns of the policy.

**Table 4.7 Impact of EMA on Year 12 and Year 13 destinations of eligibles: by prior academic achievement**

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

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

### *4.7 Sensitivity Analysis*

#### ***Aggregate data***

We now present simple difference in difference estimates based on aggregate school participation data for 16 year olds. We use three post policy periods compared to 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 the policy had no effect on the ineligible individuals we need to multiply the effect by a factor of between 1.3 and 2.

The three difference-in-difference estimates for the 1999, 2000 and 2001 are respectively 2.7, 2.3 and 4.7 percentage points always with 1998 as the baseline. If we multiply these by 1.3 we obtain respectively 3.5, 3.0 and 6.1 percentage point effects, which are remarkably close to the effect we obtained with matching (4.5) 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 that of the older sibling 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 2.8), which is larger than the effect we reported above. The difference is not significant at conventional levels.[35] 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 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.

*4.8 A back of the envelope costs-benefit calculation*

Based on our results that the EMA increased the percentage of individuals from income-eligible families completing 2 years of post compulsory education by 6.2% from 61.1% to 67.3%, and that half of this increase represents individuals who would have otherwise been in paid employment, we estimate that those brought into education would need to experience a real increase in future earnings of 5.6% as a result of the additional 2 years of education for the programme to break even, allowing for the opportunity cost of education.[36] If we also allow £3,000 for the annual extra cost of educating those who stay on in secondary education[37], the

---

[35] The standard error allows for serial correlation and cluster effects.

[36] To do this calculation we find the rate of return to education (r) which solves: $\sum_{t=0}^{1} \frac{p.EMA_t + (\lambda/2)w_t + \lambda C_t}{(1+R)^t} = \sum_{2}^{47} \frac{\lambda r w_t}{(1+R)^t}$ where $EMA_t$ is the annual average EMA transfer payment allowing for the fact that not all are fully eligible. This is estimated to be £750 a year – £25 a week for 30 weeks plus £150 in bonuses, and p is the proportion eligible for the EMA (estimated to be 67.3%). $\lambda$ is the increase in participation in education (assumed to be 6.2ppt), with half of this coming from those who would have otherwise been in paid employment. $C_t$ is the marginal cost of those brought into education as a result of the EMA and $w_t$ represents the estimated lifecycle wage profile based on the 2002–03 *Family Resources Survey*. We assume 2% a year real growth in future wages. R is the discount rate which is assumed to be 3½% which is the recommended discount rate in the UK HM Treasury Green Book (http://greenbook.treasury.gov.uk/).

[37] See Department for Education and Skills (2003), Table7.

required return to education for the two years is 7%.[38] Research in to the returns from staying on in post-compulsory education suggests that the returns are in fact 11% for men and 18% for women (Dearden, McGranahan and Sianesi, 2004). However, the returns to education for men of either low-ability or low social class (i.e. the group on which the EMA seems to have the largest impact) is estimated at between 6%-8% for these last two years, which from a financial point of view makes the programme just about break-even. However, there may well be other benefits to the policy; infra-marginal individuals may reduce hours of work and increase effort put into education; there may be crime reductions. These are hard to evaluate benefits but they should not be discounted without further research.

## 5. Conclusions

Despite a steady increase the participation in education following completion of compulsory schooling in England remained relatively low. The government decided to pilot a financial incentive scheme to encourage more pupils from lower income families to stay on in school – the Education Maintenance Allowance (EMA). Since September 2004 the EMA program has been rolled out nationally.

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 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. Taking into account that

[38] 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.

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 average net earnings.

The results also suggest that the impact of the 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 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 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.

Finally, it appears that the EMA had its largest impact on children coming from families from the poorest socio-economic background (based on parental income). 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 dropout 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 mentioned above is that we do not know what returns those induced into staying on by the subsidy will enjoy. Finally, we really have very little idea of how these returns may change now that the programme has been rolled out nationally and with it the future supply of educated workers. 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<br>Children in Income Support households (DSS) for 1998<br>Adults in Income Based Job Seekers Allowance households (DSS) for 1998<br>Children in Income Based Job Seekers Allowance households (DSS) for 1998<br>Adults in Family Credit households (DSS) for 1999<br>Children in Family Credit households (DSS) for 1999<br>Adults in Disability Working Allowance households (DSS) for 1999<br>Children in Disability Working Allowance households (DSS) for 1999<br>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<br>People out of work but in TEC delivered government supported training (DfEE)<br>People aged 18–24 on New Deal options (ES)<br>Incapacity Benefit recipients aged 16–59 (DSS) for 1998<br>Severe Disablement Allowance claimants aged 16–59 (DSS) for 1999 |
| Health Deprivation and Disability | Comparative Mortality Ratios for men and women at ages under 65. District level figures for 1997 and 1998 applied to constituent wards (ONS)<br>People receiving Attendance Allowance or Disability Living Allowance (DSS) in 1998 as a proportion of all people<br>Proportion of people of working age (16–59) receiving Incapacity Benefit or Severe Disablement Allowance (DSS) for 1998 and 1999 respectively<br>Age and sex standardized ratio of limiting long-term illness (1991 Census)<br>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 1995–1998<br>Children aged 16 and over who are not in full-time education (Child Benefit data – DSS) for 1999<br>Proportions of 17–19 year old population who have not successfully applied for HE (UCAS data) for 1997 and 1998<br>KS2 primary school performance data (DfEE, converted to ward level estimates) for 1998<br>Primary school children with English as an additional language (DfEE) for 1998<br>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<br>Household overcrowding (1991 Census)<br>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<br>Access to food shops (Data Consultancy) 1998<br>Access to a GP (NHS, BMA, Scottish Health Service) for October 1997<br>Access to a primary school (DfEE) for 1999 |
| Child poverty | 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). |

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 |
| 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'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 $R^2$ | Probit pseudo $R^2$ | $P>\chi^2$ | Median bias | Median bias | % lost to common support |
|---|---|---|---|---|---|---|---|---|
| | Before | Before | Before | After | After | Before | After | After |
| | | | (1) | (2) | (3) | (4) | (5) | (6) |
| *Males:* | | | | | | | | |
| 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 |
| *Females:* | | | | | | | | |
| 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 $R^2$ 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 $R^2$ 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) and (5) Median absolute standardised bias before and after matching, median taken over all the matching. 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.

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

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- 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 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 |
| 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 | 6,838 | |
| Observed probability | 0.253 | |

**Appendix 5: Identifying assumptions 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 ith 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. $E(Y_i^1 - Y_i^0 \mid P_i = 1)$, where $P$ is one for individuals in the pilot areas and zero in the control areas. What we do observe is $E(Y_i^1 \mid P_i = 1)$, which is the average participation rate for those exposed to the EMA. To construct the counterfactual $E(Y_i^0 \mid P_i = 1)$ we assume that $E(Y_i^0 \mid P_i = 1, X_i) = E(Y_i^0 \mid P_i = 0, X_i)$ 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 $\Pr(P_i = 1 \mid X_i)$, 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 $E(Y_i^0 \mid P_i = 1, \Pr(P_i = 1 \mid X_i)) = E(Y_i^0 \mid P_i = 0, \Pr(P_i = 1 \mid X_i))$. It follows that we can estimate the counterfactual by the sample analogue of

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

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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