WIDER BENEFITS OF LEARNING RESEARCH REPORT NO.14

Education and youth crime: effects of introducing the Education Maintenance Allowance programme

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on the Wider
Benefits of Learning

EDUCATION AND YOUTH CRIME: EFFECTS OF INTRODUCING THE EDUCATION MAINTENANCE ALLOWANCE PROGRAMME

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January 2005

Published by: Centre for Research on the Wider Benefits of Learning
Institute of Education, 20 Bedford Way, London, WC1H 0AL

ISBN: 0-9547871-4-5

Individual copy price: £6.00

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Executive summary

1. The paper utilises the variation introduced by the piloting of the Education Maintenance Allowance (EMA) programme to estimate wider impacts of education in terms of reduced crime.

2. The EMA programme was piloted in some Local Education Authorities (LEAs) of England, creating a quasi-experimental setting. It is possible to evaluate changes in crime before and after the introduction of the programme between those areas where the programme was implemented and other areas in the country.

3. This methodology would be robust to estimating a 'true' effect of the EMA programme on crime reduction assuming random assignment and comparability between those LEAs that introduced the EMA programme and those that didn't. This is, however, not the case.

4. Areas that introduced the EMA have, on average, higher crime rates. This implies a non -random assignment of the EMA programme. We deal with this issue by including controls in our estimations.

5. A further complication arises from the fact that areas that introduced the EMA programme are likely to have been selected as action areas for other crime initiatives. Therefore, what seems to be an externality of an educational programme may be induced by the combination of other direct and indirect initiatives to reduce crime. We analyse the combined effects of the Home Office's Reducing Burglary Initiative (RBI) and the EMA programme of the Department for Education and Skills.

6. Data were gathered from different sources. The Offenders Index dataset were utilised to generate indicators of juvenile crimes. Juvenile crimes are measured by the number of convictions due to violent crimes, burglaries and thefts in each LEA. Educational data by LEA came from the LEA School Information System and economic, demographic and census information by LEA from the Office of National Statistics.

7. Using difference-in-differences estimation techniques, this paper shows that burglary convictions by males aged 16 to 18 in areas piloted by the EMA programme fell by significantly more after the introduction of the programme relative to the change in other Local Education Authorities (LEA). The relative reduction in burglary rates is about 1 less conviction per 1,000 pupils in EMA areas relative to other LEAs.

8. We also find that, without the inclusion of statistical controls, convictions for thefts were reduced by nearly 2 per 1,000 pupils in EMA areas relative to non-EMA areas. The estimated reduction is consistent across several specifications, but not statistically significant when area-level controls are included.

9. We do not find a significant difference in convictions for violent crimes for EMA areas. This result is in line with our prior hypothesis.

10. We re-estimated the analysis for older individuals to establish whether there was a general reduction in crime across all age groups or whether it was specific to the EMA target group, i.e. those aged 16 to 18. Our results show a reduction in burglary for young people aged 19 to 21 in EMA areas relative to other LEAs but this is explained by general trend differences between EMA areas and other LEAs. Once these trends are accounted for there are no declines in burglary convictions for those aged 19 to 21. For the population older than 21 years there are no significant differences in burglary rates in EMA areas relative to other LEAs. These findings suggest that burglary reduction occurred mainly for 16 to 18 year old males in line with the explanation that the effects are due to the EMA programme.

11. When we analyse the effect of the Reducing Burglary Initiative in conjunction with the EMA programme, we find that in LEAs where the RBI and the EMA were introduced jointly, burglary rates fell between 1.1 and 1.5 offences per 1,000 pupils relative to areas that did not introduce any of these policies. The reduction in areas that introduced only the EMA programme or only the RBI was not significantly different from that in other LEAs.

12. This paper highlights the importance of connections between government programmes for young people, such as the EMA and the RBI, and demonstrates the high levels of potential social benefit that may flow from programmes that recognise the connections between key agencies, government departments and stakeholders. The paper also shows the capability of education provision in meeting wide-ranging policy objectives.

Acknowledgements

We would like to thank Steve Machin for his comments in relation to the application of methods and Kirstine Hansen for her useful insights on the topic. We would also like to thank research staff at the Centre for Research on the Wider Benefits of Learning for their useful comments on this paper. Other useful suggestions were received from Professor Robert Haveman, University of Wisconsin-Madison, and from audiences when the paper was presented at the Department of Economics, University of Sussex.

We would like to thank the Home Office for making the Offenders Index Data available and HM Treasury and the Department for Education and Skills for their financial support of this project.

Contents

Tables and figures

1. Introduction

Education is a potentially large influence on individual propensities to offend and possibly an important source of area-level variation in crime rates. Crime statistics for England indicate that crime rates are lower in areas with higher levels of education, which are also areas of higher per capita income and contain a higher proportion of families belonging to the highest socio-economic status (Home Office, 2003). Whether the association between education and crime is causal, or whether it masks a number of possible effects that may not be due to education, is less clear.

The empirical literature on the economic incentives to commit crime has made progress in dealing with the issue of causality of crime outcomes. Machin and Meghir (2000) use panel data to look at cross-area changes in crime across areas in relation to changes in wage distribution for the 25th lowest percentile. They found a negative association between falling wages and increased instances of theft and handling of stolen goods, even after controlling for other variables including demographic change and measures of deterrence. They found a similar association for changes in burglary, vehicle and property crime and changes in low wages. Similarly Witt, Clarke and Fielding (1999), using police force area data in England and Wales from 1988 to 1996, find that wage inequality changes are positively correlated with changes in crime.

Recent findings by Hansen and Machin (2004) show robust evidence that changes in minimum wages affect crime rates. They use the introduction of the national minimum wage in the UK as a quasi-experiment to look at crime rates before and after the introduction of the policy. The introduction of a minimum wage to a previously unregulated labour market provided substantial pay rises to low-waged workers. Comparing levels in crime by police force area before and after the introduction of the minimum wage regulation, they show that economic incentives produced a significant reduction in crime. In a different approach to economic incentives, Machin and Marie (2004) investigate how changes in benefits affect unemployment and how this in turn can predict crime. Using a similar design, they show that in areas more affected by the Jobseeker's Allowance, i.e. less claimants, crime rose by more. Their results seem to suggest that benefit cuts may well have caused individuals previously on the margins to engage in crime.

These latter analyses look at government policies aimed to change the labour market's economic incentives and evaluate their impact on crime reduction. In particular, these investigations look at changes in crime rates before and after the introduction of the government policy and compare changes in crime rates between areas. Although this methodology has been applied to policies that affect wages or unemployment benefits, it has rarely been applied to educational policies. In the UK, a number of policies have been introduced to improve participation in further education such as transport cost subsidies, childcare assistance for teenage parents and weekly payments to help with the cost of learning (DfES, 2004). If indeed education does have any effect on crime, then one would expect that participating in education would reduce the likelihood of being involved in criminal activities. Hence, one may expect a significant reduction in crime in areas where targeted educational policies for vulnerable groups have been introduced as compared to areas where such policies have not been introduced.

In estimating the effects of education on crime using non-experimental data, Lochner and Moretti (2004) use instrumental variable techniques to estimate the effect of high school graduation on participation in criminal activities in the US. They use the exogenous variation induced by changes in state post-compulsory school attendance as an instrument to high school graduation. They find that completing high school reduces the probability of incarceration by about 0.76 percentage points for whites and 3.4 percentage points for blacks. They find that the most significant consequence of increased graduation is a reduction in violent crime, assaults and motor vehicle thefts. Interestingly, their results suggest that endogeneity of schooling decisions does not appear to bias estimates of the effects of education on crime. In other words, the possibility that the association between education and crime may be due to unobservable factors that affect both outcomes appears to be, if any, small.

Educational programmes in the US have proved to have external benefits. For example, the Quantum Opportunity Program (QOP) was designed to increase the likelihood that youths would complete high school and enter a further education and training programme. The programme was also intended to improve the youth's grades and achievement test scores and to reduce risky behaviours such as substance abuse, crime and teenage childbearing (Taggart, 1995). The programme enrolled youths from disadvantaged backgrounds, especially those with a high probability of dropping out from high school. The implementation of the programme provided the students with a tutor and supplementary school activities with educational and community projects. Financial incentives were also designed to encourage high school graduation. Two years after the programme's completion, randomly assigned participants were 34 percent more likely to achieve high school diploma and criminal activity, in the form of number of times arrested, was 28 percent lower than non-participants (Penn, 2000). This last result, however, is not consistent across all areas, e.g. in Philadelphia crime increased whereas in Washington crime remained unchanged (Maxfield et al., 2003)

In this paper we explore the relationship between education and crime using a natural experimental setting. We evaluate the Educational Maintenance Allowances (EMA)–a Department for Education and Skills (DfES) programme designed to increase participation in post-compulsory education – on juvenile crime reduction. To do this, we first address the theoretical reasons why education can have an effect on crime reduction (Section 2) and the methodology to evaluate them (Section 3). We then present the results from our estimates of the change in conviction rates for burglary, thefts and violent crimes, for 16 to 18 year old males, in EMA areas relative to other areas (Section 4).

To validate our results, we perform two sensitivity tests. The first test aims to capture the age-specific conviction rates' reduction (Section 4.2). If indeed the EMA is having an effect on crime, it will be through shifting 16 to 18 year old youths away from criminal activities or potential criminal activities and into education. The second test aims to capture the possibility that other crime reduction initiatives were in action in EMA areas and that the EMA is a confounder for the effect of these other initiatives (Section 4.3). In particular, we explore the Reducing Burglary Initiative: a crime prevention programme introduced by the Home Office in England and Wales around the same time that the EMA were being piloted. We lay out conclusions from this research in Section 5.

2. Theoretical framework

There are a number of theoretical reasons why the provision of education may have an effect on crime. Feinstein (2002) reports five potential channels where education can have an effect on individuals' criminal behaviour: income, parenting, pleasure, patience and risk aversion. For the case of income, education may increase the expected value of legal work earning, which raises the opportunity cost of time spent engaged in criminal activities. Education also increases the cost associated with incarceration, since it increases the value of any time foregone (Lochner, 2004).

In terms of parenting, education could have effects on parenting skills, which have implications for the criminality of their children (Rutter et al. 1998). For example, parenting skills such as erratic or harsh discipline, low supervision or maternal rejection have been shown to be associated with subsequent criminal involvement.

Pleasure from criminal activity is another channel by which education may have effects, particularly since pleasure is the other one of the main determinants of juvenile crime (Farrington, 2001). Education may be important for teenagers in terms of limiting opportunities for participating in criminal activity. This could be a simple matter of time allocation (i.e. the custodial function of education) or, more subtle, an effect on peer group membership, with subsequent implications for behaviours, including criminology. Calvo-Armengol and Zenou (2004) developed a theoretical model of crime decision where delinquents influence each others' decisions, both positively and negatively. They find that peer group effects can give rise to multiple equilibria, where individuals connected to a network can end up with different outcomes: either in employment, as isolated criminals or within a network of criminals. However, evidence on the effects of peer groups is mixed.

The learning benefit from education may influence crime through effects on patience and risk aversion (Lochner and Moretti, 2001). Education increases patience, which reduces the discount rate of future earnings and hence reduces the propensity to commit crimes.

In terms of risk aversion, education may increase risk aversion, which, in turn, increases the weight given by individuals to the possible punishment and hence reduces the likelihood of committing crimes. Education may also affect the decision to engage in crime by impacting on maturity and development of youths (Hirschi and Gottfredson, 1995).

Finally, education may also have a direct effect on crime. For instance, education increases the earnings that one can derive from crime and the tools learnt in school may be inappropriately used for criminal activities. In this sense, education may have an upward effect on crime (Levitt and Lochner, 2000).

From the reasons mentioned above, it is further expected that an educational programme, which encourages participation through income support, will have additional effects on crime reduction. One of the most cited reasons to engage in criminal activities is to gain money (Palmer et al., 2002). The EMA could, at least partially, tackle the income-related need for youths to engage in crime since those supported by the programme receive a weekly allowance, an attendance bonus

payment each term and an end-of-course achievement payment as long as they attend a course leading to a qualification. Therefore, eligible youths can use their allowance to satisfy their needs.

3. Methodology

One of the main difficulties for empirical research is the identification of a causal effect. A simple correlation between education and crime may reflect a number of unobservable characteristics affecting both variables. Methods currently utilised in research to deal with the selectivity bias of education include Instrumental Variables (IV) and/or Fixed Effects (FE). The first method uses a variable, called an instrument, which predicts education but does not predict crime. Finding such an instrument, however, has proven to be troublesome (Rosenzweig and Wolpin, 2000). FE methods make use of panel data available and eliminate the bias in education induced by unobservable factors that do not change over time. FE, however, is not robust to the possibility that time-variant unobservable factors may still influence education and crime (Hsiao, 2003).

Another method used is the Generalised Methods of Moments (GMM), which also makes use of data over time. With this method, past values of the explanatory variable, in this case education, are used as instruments of current education. One of the problems with using this method here is the availability of educational data. The ideal measurement of education would be learning, so that past learning serves as the instrument to predict current learning.

Another method is the examination of policies or pilot programmes that allow evaluation of effects using a quasi-experimental setting. In this quasi-experimental setting, we may infer that changes in educational outcomes resulted from a government policy. It is possible to look at what happened to the outcome before and after this policy change. This is the approach that we follow in this paper, with the EMA pilots providing a natural experiment.

The piloting of Education Maintenance Allowances (EMA) began in September 1999 in 15 Local Education Authority areas[1], with a view to raising participation, retention and achievement in post-compulsory education among 16-18 year olds.

The EMA programme was piloted in areas with low participation in post-16 education. These areas are also known to have higher levels of deprivation, more families renting rather than owning their accommodation and a greater proportion of individuals without qualifications and whose parents experience high labour market inactivity.

The 15 pilots introduced four models of the main EMA including: varying the terms of the weekly amount of EMA available, to whom it is paid and amounts which are paid for retention and achievement bonuses.

[1] LEA areas where the EMA was piloted are: Middlesbrough, Walsall, Southampton, Cornwall, Leeds, Lambeth, Southwark, Lewisham, Greenwich, Oldham, City of Nottingham, Bolton, Doncaster, Stoke-on-Trent, and Gateshead.

Table 1: Design and coverage of main EMA pilot areas

Model	LEA Pilot areas	Awards
Variant 1	Middlesborough, Walsall, Southampton, Cornwall, Leeds, Inner London (Lambeth, Southwark, Lewisham, Greenwich).	£30 per week plus £50 retention and £50 achievement bonus
Variant 2	Oldham, Nottingham	£40 per week plus £50 retention and £50 achievement bonus
Variant 3	Bolton, Doncaster	£30 per week paid to parents plus £50 retention and £50 achievement bonus
Variant 4	Stoke-on-Trent, Gateshead	£30 per week plus £80 retention and £140 achievement bonus.

In September 2004 EMA became a national scheme in which £30, £20 or £10 per week is paid directly to the young person depending on household income. It is paid in addition to any Child Benefit that is claimed for a young person in post-16 education. The national scheme includes retention bonuses worth up to £500 per year, however it does not include an achievement bonus. The national scheme also uses a central assessment and payment body, rather than the Local Education Authorities.

Ashworth et al. (2001 and 2002) and Heaver at al. (2002), in their evaluations of the first two years of the EMA programme indicate that overall, 63% of young people in low income families in pilot areas applied to the EMA programme in the first year and applications for the second year increased slightly by just 0.6 percentage points. The majority of the applications for the EMA were made by young people who continued with full-time education. However, 11% of eligible people who were in work or training, 15% who were categorised as unemployed or looking for a job and 23% of those who identified themselves as undertaking another activity such as looking after the home or the family had applied. The EMA programme is estimated to have had a significant impact on participation in education in urban areas and to have had a larger effect for young men.

Ashworth et al. also indicate that, despite the relative success of the LEAs where the EMA programme was piloted to publicise the programme, 37% of income eligible families in the pilot areas, with young people both in and out of full-time education, had not applied for the EMA during year one. This percentage was reduced during the second year due to an increase in public awareness of the availability of the programme. Given that eligibility for financial support depends on parental income

and that not all eligible youths applied for the programme, any positive externality of the programme is likely to be biased downwards.

There may be effects of the EMA programme on youth crime through changes in educational participation with subsequent benefits for youth in terms of income, peer group memberships and social inclusion. The EMA provides a direct income effect on crime reduction due to the income support provided to youths. Moreover, participation in education induces a trade-off in time allocation between participation in criminal activities and education. This may be of particular importance for crime that requires market skills and planning, such as burglary, but perhaps not so much for crimes that require little market skills such as handling stolen goods or drug dealing.

Participation in education may also have long-term implications. According to Lochner (2004), an educational policy aimed at increasing schooling is likely to reduce crime more in the long-term than in the short-term by increasing skill levels. The accumulation of skills over time increases the opportunity cost of engaging in criminal activities, so the expected effects of schooling are likely to be greater as individuals age.

3.1 Data

Crime data come from the Home Office Offenders Index database (OI). The OI contains a history of criminal convictions from 1963 from England and Wales. The sample of individuals is a census of all court cases that occur during four weeks of the year. The sampling weeks are the first week of March, the second week of June, the third week of September, and the third week of November. Once a person is sampled, the complete history of prosecutions and convictions are recorded and remains part of the sample in case of any future court appearance. The period used for this paper is from 1996 to 2002.

The OI contains information on the type of offence for which the individual has been convicted and on individuals' characteristics such as age, gender and ethnicity. As it is a court-based system, there is no information on dates of offending. There is also no information on unsuccessful prosecutions and cautions. Therefore the combination of age, gender and type of offence allows us to generate specific indicators of convictions, for example male burglary committed by 16 year olds.

Educational data for Local Education Authorities (LEA) come from the LEA School Information System (LEASIS). Information in LEASIS is at school level. We used the location of the schools to establish their location within the 150 current LEAs. Then information was aggregated, and weighted averages of educational variables calculated. We gathered information on total number of pupils, full-time equivalent number of qualified teachers, percentage of children eligible for free school meals, pupil-teacher ratio, full-time equivalent number of supplementary staff for ethnic minorities, percentage of 15 year olds achieving 5 or more GCSE A to C, percentage of 15 year olds not achieving GCSE and percentage of secondary students with unauthorised half-days missed.

Two particular data issues needed to be resolved. First, it was necessary to homogenise educational information using the current classification of 150 LEAs in

England. Classification of LEA changed between 1997 and 1998. From having 109 LEAs in England in 1996 there are now 150 today. Large LEAs were divided into smaller authorities, in some cases up to 4 or 5 smaller units. By using the current classification of LEAs we are able to identify all areas in which the Education Maintenance Allowance programmes were piloted.

Secondly, it is important to calculate age-specific crime rates because the outflow from the education system represents an addition to the human capital *stock* but the effect of change in the flow in the overall crime will be severely limited. Rather, we calculate conviction rates for each cohort of adolescents able to leave full-time education at age 16. These age-specific conviction rates can be explained, in part, by the educational success of the cohort.

With the OI data we aggregate offences to the LEA-level. Linking *petty* crime areas, the area where the court has jurisdiction, to LEAs was not simple. Some magistrates courts have changed over time, merging with others. Additionally, we assume that no offences are committed across regions. This is a reasonable assumption for large areas, especially for juvenile crime. However, for the case of London, we combined 13 LEAs and respective courts into 4 main zones (North-East, North-West, South-East and South-West London). Three other LEAs had to be combined to match with the court level data. These were Bournemouth and Dorset; Bracknell Forest, Slough, Windsor and Maidenhead and Wokingham; and Essex, Southend and Thurrock. Without including the LEA for the City of London we were left with 132 areas for the analysis, 12 of which received EMA support.

For each LEA we obtained information on convictions for burglary, theft and violent crimes committed by 16 year old males between 1996 and 2002. We obtained the same data for offences committed by 17 year olds between 1997 and 2002 and for 18 year olds between 1998 and 2002. Property and violent crime are typically high for teenagers, typically reaching a peak during late teenage years. Burglary includes breaking into, or attempts to break into, residential and non-residential properties. Theft includes stealing vehicles or from vehicles and shoplifting or handling stolen goods. The most common violent crimes include murder, cruelty to children, child abduction, kidnapping, rioting, endangering wildlife or sea life, endangering railway passengers and procuring illegal abortion.

We evaluate the externalities of the EMA programme for different types of convictions, i.e. burglary, theft and violent crime. This is because the decision to engage in a delinquent activity such as burglary or theft is different than for violent crime. Even between burglary and theft there may be differences; although these may be more subtle. For example, burglary requires more time and knowledge of the target property whereas stealing a mobile phone may depend more on opportunistic circumstances. Grouping offences into burglary, theft and violent crimes in the OI data takes into account any changes in offence codes and disposal or sentencing codes between 1996 and 2002 to obtain a consistent treatment of the data over time (Home Office, 1998).

Finally, the total number of convictions per LEA is standardised using the number of pupils aged 15 by LEA. In order to account for changes due to population mobility,

the growth rate of population aged 15 to 19 by LEA was applied it to the 15 year old pupils. Therefore, conviction rates are measured per 1,000 pupils.

3.2 Estimation

To evaluate the effects of the EMA programme on our indicator for juvenile crime we applied difference-in-differences techniques. We assessed the changes in male, juvenile conviction rates before and after EMA piloting in treatment and non-treatment LEAs by:

$$\begin{aligned} &C_{at}^{EMA} - C_{at-1}^{EMA} = \Delta C_{at}^{EMA} \quad \text{and} \quad C_{at}^{NEMA} - C_{at-1}^{NEMA} = \Delta C_{at}^{NEMA} \\ &\Delta C_{at}^{EMA} - \Delta C_{at}^{NEMA} = \phi \end{aligned} \tag{1}$$

where *a* stands for LEA area, *t* is time, *EMA* denotes areas where the EMA programme was piloted and *NEMA* non-EMA LEAs. In this paper *t-1* covers 1996 to 1999 and *t* 1999 to 2002.

The difference in these differences (ϕ) is an estimate of the effect of the EMA programme on male, juvenile convictions. This estimate is unbiased when the programme is introduced randomly so that there are no differences between treatment and non-treatment areas with respect to conviction rates (Sherman at al., 1998). However, this is not the case here since the selection of LEA areas to participate in the EMA pilot was not random. Areas were chosen that were known to have relatively high levels of deprivation, low participation rates in post-16 education and low levels of attainment in Year 11 examinations (Ashworth et al, 2001). These areas are also areas with high juvenile crime and high conviction rates.

One way of handling this potential bias is to introduce controls for the characteristics of areas, estimating an equation of the form:

$$C_{at} = \alpha + \beta T_a + \phi T_a * PolicyOn + \varphi X_{at-1} + u_{at} \tag{2}$$

where T_a is a binary dummy variable indicating areas that piloted the EMA programme and *PolicyOn* is a binary dummy variable indicating periods after the introduction of the EMA. The parameter ϕ is the difference in means before and after policy introduction, i.e. the difference-in-differences. This will be biased (in a treatment-control policy evaluation setting) if β is estimated to be non-zero, and in particular if it is positive as when the policy was introduced to high crime areas. The matrix **X** contains area-level characteristics which are incorporated in the estimation to remove the initial difference in treatment versus non-treatment LEAs, evaluated by β.

However, even after having a similar initial baseline in convictions, there could be different pre-policy conviction *trends* across areas where the EMA was implemented and the rest of the LEAs. This may still cause a serious bias to the difference-in-differences estimate using equation (2). One way to control for this issue is to add linear time trends as controls to equation (2).

There are some issues regarding the validity of our estimates. Firstly, the EMA programme was piloted in 15 LEAs in 1999. After the first two years, the programme was extended to approximately one-third of the LEAs in England. If these areas have a large reduction in convictions, the parameter ϕ may be biased. We attempt to check how robust our initial estimate of the difference-in-differences is by re-estimating the model excluding these areas.

Another way to control for the rolling-over of the EMA programme is to include a control for the areas where the programme was rolled over. Therefore, we estimate the equation:

$$C_{at} = \alpha + \beta T_a + \phi_1 T_a * PolicyOn_1 + \phi_2 T_b * PolicyOn_2 + \varphi X_{at-1} + u_{at} \qquad (3)$$

where *1* stands for the original 15 LEA where the EMA programme was piloted in 1999 and *2* stands for the LEAs where the programme was rolled over in 2001.

After estimating the difference-in-differences parameter we perform two sensitivity analyses. First, analysis by age group is done to verify if indeed the EMA programme had an effect on reducing conviction rates for the targeted group. In other words, we expect that any external benefits of the EMA programme will be captured by a decline in convictions for people aged 16 to 18, but not so for older individuals. This sensitivity analysis is performed for males aged 19 to 21 and also for males aged 20 to 69.

Second, our estimates are not robust to the possibility that the introduction of other programmes in these LEAs could account for any observed differences between EMA and non-EMA areas. These may be programmes designed to combat crime, such as the Street Crime Initiative (SCI) or the Reducing Burglary Initiative (RBI), or economic programmes that have been shown to have knock-on effects on crime, such as the Jobseeker's Allowance.

Of particular interest is the introduction of the RBI in 1999. The aim of this initiative was to reduce burglary nationally by targeting areas with the worst domestic burglary problems. Some of these areas also introduced the EMA programme. In this case, we can capture the effect of EMA areas, RBI areas and both EMA-RBI areas on conviction rates by using the following equation:

$$C_{at} = \alpha + \beta_i T_{ia} + \phi_i T_{ia} * PolicyOn + \varphi X_{at-1} + u_{at} \qquad (4)$$

where i = {EMA programme, RBI programme, EMA-RBI programmes}. This means that there are three difference-in-differences estimators, one for areas that implemented the EMA programme only, another for areas that introduced the RBI only and, finally, another for areas that introduced both programmes.

4. Results

Table 2 presents results from the difference-in-differences estimates without controls using equation (1). Convictions considered are for males aged 16 to 18 in terms of whether they were prosecuted and convicted for (i) violent crime, (ii) burglary, and (iii) theft.

Table 2: Difference in juvenile convictions (16 to 18 year old males) per 1,000 pupils

		Before EMA	After EMA	Within Group Difference
EMA Areas	Violent	1.645	1.468	-0.177
				(0.180)
	Burglary	4.219	2.230	-1.989
				(0.379)***
	Theft	14.366	9.563	-4.803
				(1.102)***
Non-EMA Areas	Violent	1.137	0.977	-0.160
				(0.045)***
	Burglary	2.227	1.176	-1.051
				(0.098)***
	Theft	7.643	4.817	-2.826
				(0.260)***
				Difference-in-differences
Between Group Difference	Violent	0.509	0.491	-0.017
		(0.227)**	(0.203)**	(0.179)
	Burglary	1.991	1.054	-0.937
		(0.673)***	(0.525)**	(0.377)**
	Theft	6.723	4.746	-1.977
		(2.923)**	(2.007)**	(1.091)*

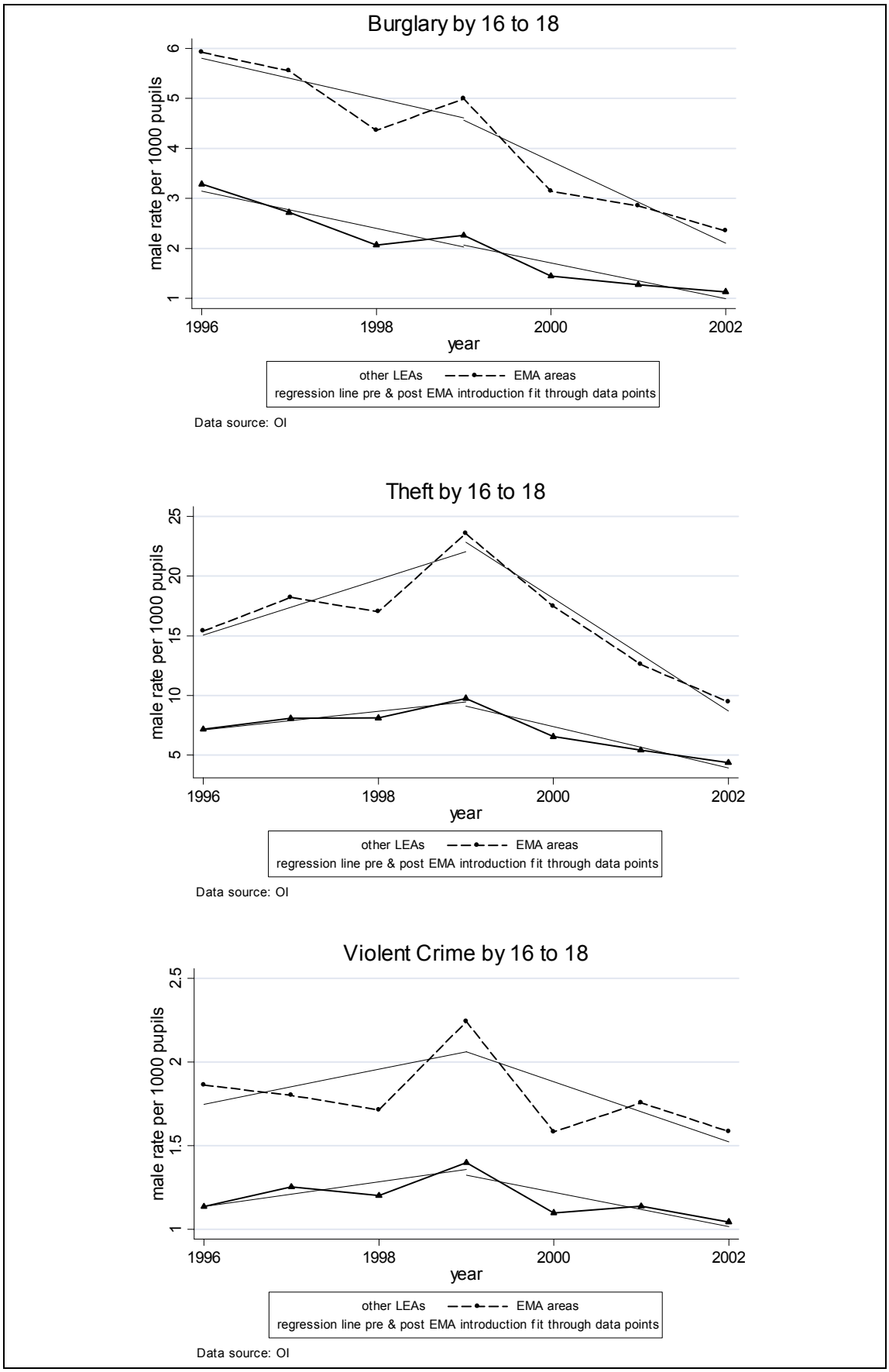
Note: Estimation based on 15 areas where the EMA programme was piloted. "Before EMA" is defined as 1996 to 1999 and "After EMA" as post 1999. Asterisks (*),(**),(***) represent significance at 10, 5 and 1 percent levels, respectively. Estimations are weighted by population. Standard errors, in parenthesis, are clustered by LEA.

Table 2 shows a clear decline in juvenile convictions in all local education authority areas. The exception is for convictions due to violent crimes for which there is no significant change in EMA areas. The difference-in-differences estimator calculates difference in conviction rates both within and between EMA and non-EMA areas. For burglary rates committed by male youths aged 16 to 18, there is a reduction of 0.937 crimes per 1,000 pupils in EMA areas relative to non-EMA areas. The extra relative reduction for theft among 16 to 18 year old males is nearly 2. These reductions are significantly different from zero at the 5% and 10% levels, respectively. However, there are no differences in convictions for violent crime in EMA relative to non-EMA areas.

Results from the difference-in-differences estimate in convictions by EMA and non-EMA areas obtained in Table 2 are also illustrated in Figure 1. This figure shows trends in conviction rates for burglary, theft and violent crimes in EMA and non-EMA areas. Trends are divided into pre- and post- introduction of the EMA programme (in 1999). Between-group differences in conviction rates are illustrated by comparing trends between EMA and non-EMA areas. Within-group differences are illustrated by comparing trends in EMA before and after 1999 and similarly for non-EMA areas. The difference-in-difference is the change in conviction rates in EMA areas before and after the introduction of the programme relative to the same change for other LEAs.

Using the above explanation, the burglary rate falls more in EMA areas after the introduction of the pilots relative to other LEAs (-0.937). Similarly, the theft rate also falls more rapidly in EMA areas relative to non-EMA areas (-1.977), however the fall in conviction rates for violent crimes in EMA areas after the introduction of the programme is not significantly different from other LEAs (-0.017). The figures also make clear how EMA areas differ from the rest of the LEAs in terms of conviction rates, with higher burglary, theft, and violent crimes conviction rates in EMA relative to non-EMA areas.

Figure 1: Trends in convictions in EMA and non-EMA areas



Using equation (2) and regression analysis we then test the difference-in-differences estimate controlling for other area level information (Table 3). The first column is the difference-in-differences estimate with no controls, and shows there to be a significant reduction in burglary and thefts in EMA areas relative to non-EMA areas (-0.94 and -1.98, respectively). Notice that these estimates correspond to the ones previously estimated in Table 2 under the heading *difference-in-difference*. It also shows that burglary and theft rates were higher in EMA areas before the introduction of the programme (1.99 and 6.72, respectively). These estimates correspond to the *between group difference* prior to the introduction of the EMA programme in Table 2.

We include controls in the estimation to reduce the coefficient β to zero. This coefficient indicates the non-random allocation of the EMA programme to areas with low participation in post-compulsory education. In order to condition out some of this initial difference, estimates in Table 3 use the following measures of area characteristics:

- *Column (2) economic and educational variables by LEA from 1996 to 2002*: unemployment rate for individuals under 25; the current, annual proportion of pupils eligible for free school meals (as a measure of area-level deprivation); full-time equivalent number of qualified teachers; percentage of pupils attaining no GCSEs at age 16; percentage of unauthorised half-days missed in secondary education; pupil-teacher ratio; and full-time equivalent number of supplementary staff for ethnic minorities.

- *Column (3) Census 2001 variables by LEA*: the percentage of male working population in manufacture; the percentage of working population in low socio-economic occupations; the percentage of the population under 25; the percentage of the population with no qualifications; the percent of ethnic minorities and the percentage of the population living in social housing.

- *Column (4) persistence in crime, or state dependence*: this is included as the average of conviction rates pre-policy and the average total adult convictions in the area pre-policy. State dependence in crime, or past crime, has been shown to be an important determinant of current crime outcomes (Imai and Krishna, 2004).

- *Column (5) area trends*: to account for linear changes in crime by LEA over time.

Table 3: Regression analysis on 16 to 18 year olds' conviction rates

	(1)	(2)	(3)	(4)	(5)
Dependent variable: Male **Violent** Crimes					
T_a	0.509	0.122	0.065	-0.028	-0.209
	(0.227)**	(0.239)	(0.222)	(0.072)	(0.164)
T_a *policy-on	-0.017	0.025	0.023	-0.077	-0.299
	(0.179)	(0.198)	(0.202)	(0.172)	(0.191)
Dependent variable: Male **Burglary**					
T_a	1.991	1.270	1.301	0.571	-0.256
	(0.673)***	(0.556)**	(0.487)***	(0.149)***	(0.378)
T_a *policy-on	-0.937	-0.742	-0.761	-0.841	-0.982
	(0.377)**	(0.388)*	(0.387)*	(0.368)**	(0.504)*
Dependent variable: Male **Theft**					
T_a	6.723	3.942	3.459	1.256	-0.815
	(2.923)**	(2.569)	(2.401)	(0.633)**	(0.813)
T_a *policy-on	-1.977	-1.038	-1.196	-1.953	-2.184
	(1.091)*	(1.218)	(1.196)	(1.143)*	(1.639)
Controls					
Economic variables	No	Yes	Yes	Yes	Yes
Edu. Variables	No	Yes	Yes	Yes	Yes
Census Variables	No	No	Yes	Yes	Yes
Avg. crime pre policy and adult crime	No	No	No	Yes	Yes
Yearly controls	Yes	Yes	Yes	Yes	Yes
Cohort controls	No	Yes	Yes	Yes	Yes
LEA trends	No	No	No	No	Yes
Number of Obs.	2,373	2,373	2,373	2,373	2,373

Notes: Notation follows from equation (2), where *Ta* stands for areas that introduced the EMA programme and *Policy-On* indicates periods after the introduction of the EMA programme.

Asterisks (*),(**),(***) represent significance at 10, 5 and 1 percent levels, respectively. Estimations are weighted by population. Standard errors, in parenthesis, are clustered by LEA.

As we add controls in the regressions, the initial difference between areas becomes statistically insignificant. In other words, the estimation is indeed controlling for the initial between group difference between EMA and other LEAs prior to 1999. For theft, for example, just controlling for economic and educational variables reduces the initial difference between areas by nearly half (from 6.72 to 3.94) and this value (3.94) is not statistically significant at 10% level. Difference in the initial levels of burglary rates decreases from 1.99 to 1.27 when adding the economic and educational

controls. Notice that the incorporation of Census 2001 variables does not reduce this difference any further. This is because both educational and economic variables may have already captured the main differences between EMA and non-EMA areas.

It may also be that there are differential pre-policy trends in observables and/or convictions. Thus, in column 4 we add pre-policy conviction rates to try and standardise for previously high conviction areas. This causes another significant reduction in the initial difference in burglary rates between EMA and non-EMA. Here, the difference is reduced to 0.57, but it is still significantly different from zero. Finally, by incorporating area trends, the initial difference in burglary rates becomes statistically insignificant whereas the difference-in-differences parameter remains statistically significant with a p-value of 0.054.[2] There is a reduction of 0.98 in burglary rates in EMA areas relative to non-EMA areas in column 5.

It is worth noting that the difference-in-differences estimate for theft shows a consistent reduction in EMA relative to non-EMA areas. For some specifications this parameter is significant (columns 1 and 4). The relatively low number of pilot LEAs makes statistical significance harder to achieve. Therefore, we do not imply that the lack of statistical significance implies the absence of effects, in particular in the case of theft.

4.1 Extension of the EMA programme to other areas

As mentioned in section 3.2, the EMA programme was extended to about one-third of all LEAs in England during 2001. We test the robustness of our results by estimating equation (2) excluding areas that introduced the EMA programme after 2001. These results are shown in Table 4 under *Test 1*. Column (A) shows estimated parameters for the model that includes the full set of controls, i.e. economic, educational, Census characteristics and initial conditions for conviction rates, whereas column (B) shows estimated parameters when *trends* are included as controls.

In the case of burglary we find that the difference-in-differences estimator remains statistically significant – with a p-value of 0.052. The reduction in convictions for burglary in EMA areas relative to areas that did not pilot the programme is 0.88 convictions per 1,000 pupils (column B in Table 4). For thefts the significant reduction of 2 convictions per 1,000 pupils (column A in Table 4) may be explained by area trends (column B in Table 4).

Again, we find that there are no differences between convictions in EMA relative to other LEAs due to violent crimes.

[2] The estimated parameter for male burglary (0.98) and associated standard error (0.50) is statistically significant at 10% level, as indicated in Table 3. However, the p-value (0.054) is closer to a 5% significance level than to a 10%. Given the few number of EMA areas (15) statistical significance is harder to achieve. Therefore, reporting the p-values where necessary seems reasonable and important to support the empirical argument.

Table 4: Verification: piloted areas versus non-EMA areas only

	TEST 1			TEST 2	
	(A)	(B)		(C)	(D)
Dependent variable: Male **Violent** Crime					
T_a	0.096	-0.195	T_a	-0.037	-0.235
	(0.102)	(0.148)		(0.072)	(0.164)
T_a *policy-on	-0.050	-0.290	T_a *policy-on1	-0.034	-0.309
	(0.184)	(0.193)		(0.169)	(0.192)
			T_b *policy-on2	-0.200	-0.169
				(0.106)*	(0.123)
Dependent variable: Male **Burglary**					
T_a	0.575	-0.021	T_a	0.552	-0.250
	(0.149)***	(0.399)		(0.147)***	(0.381)
T_a *policy-on	-0.953	-0.880	T_a *policy-on1	-0.759	-0.980
	(0.303)**	(0.490)*		(0.369)**	(0.507)*
			T_b *policy-on2	-0.387	-0.046
				(0.178)**	(0.225)
Dependent variable: Male **Theft**					
T_a	1.289	-0.543	T_a	1.198	-0.945
	(0.538)**	(0.876)		(0.621)*	(0.819)
T_a *policy-on	-2.196	-2.211	T_a *policy-on1	-1.688	-2.230
	(1.201)*	(1.658)		(1.141)	(1.646)
			T_b *policy-on2	-1.249	-0.851
				(0.694)*	(0.607)
Number of Obs.	1,707	1,707		2,373	2,373

Notes: Notation follows from equation (3), where *Ta* stands for the original LEAs where the EMA programme was piloted in 1999 and *Tb* for areas where the programme was rolled over in 2001. *Policy-On* indicates periods after the introduction of the EMA programme with *subscript 1* for the original 15 LEAs and *subscript 2* for the LEAs where the programme was rolled over.

Asterisks (*),(**),(***) represent significance at 10, 5 and 1 percent levels, respectively. Estimations are weighted by population. Standard errors, in parenthesis, are clustered by LEA. Columns (A) and (C) incorporate full set of controls whereas columns (B) and (D) also add area trends.

The effect of the expansion of the EMA programme is also accounted by estimating equation (3). Under this estimation, the relative reduction in conviction rates is estimated for 15 EMA pilots versus other LEAs, and for the 15 EMA pilots plus extended LEAs that introduced the EMA programme in 2001 versus other LEAs. In other words, we divide the difference-in-differences estimator into original EMA pilot areas relative to the rest of the LEAs post 1999 and original EMA pilot areas plus expanded areas relative to other LEAs post 2001. Results from this estimation are shown in Table 4 under *Test 2*. Similar to the previous estimations, column (C)

presents estimated parameters from the model that incorporates the full set of controls whereas column (D) adds area trends.

Our findings confirm the significant differences in burglary rates in EMA areas versus the rest of the LEAs post the introduction of the EMA programme (-0.76 burglaries per 1,000 pupils in column C). Interestingly, there is a significant difference in burglary and theft rates between EMA areas plus expanded EMA areas and the rest of the LEAs (-0.39 burglaries per 1,000 pupils and 1.25 thefts per 1,000 pupils in column C). However, only burglary rates fell significantly more in the original EMA areas relative to other LEAs, 0.98 less burglaries per 1,000 pupils in column D.

4.2 Sensitivity analysis 1: burglary for older individuals

So far, results show a significant reduction in conviction rates for burglary offences committed by 16 to 18 year olds in areas where the EMA programme was piloted relative to the rest of the LEAs. Still, one may attribute this relative reduction to a general reduction in burglary offences across the population. If this were the case, then difference-in-differences estimates for burglary rates for other age groups will also be statistically significant. We verify this issue by estimating the model first for individuals aged 19 to 21 (Table 5) and then for different age groups of the population (Table 6).

In terms of convictions for burglary offences committed by 19 to 21 year olds, EMA areas have higher rates before the introduction of the programme than other LEAs. Similar to the analysis performed in Table 3, the introduction of controls in Table 5 aims to standardise the baseline for these areas. By incorporating area trends in the estimation, the pre-policy difference in conviction rates is eliminated (column 5 in Table 5).

Table 5: Regression analysis: conviction rates for 19 to 21 year old males, burglary

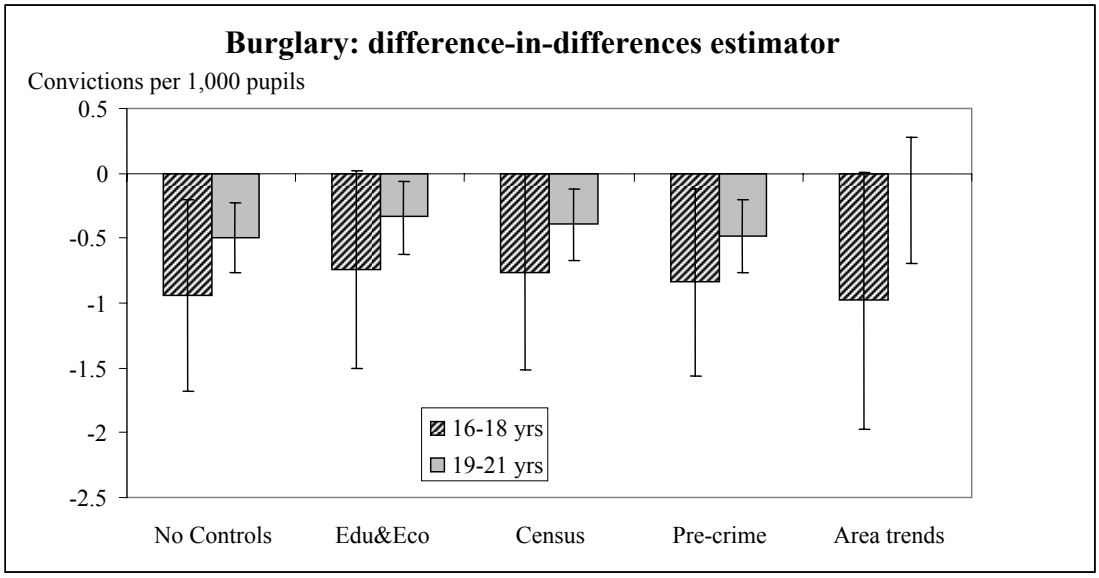
	(1)	(2)	(3)	(4)	(5)
T_a	0.965 (0.284)***	0.554 (0.217)**	0.529 (0.251)**	0.159 (0.066)**	0.251 (0.224)
T_a *policy-on	-0.498 (0.138)***	-0.336 (0.147)**	-0.386 (0.145)***	-0.487 (0.144)***	-0.009 (0.352)
Controls					
Economic variables	No	Yes	Yes	Yes	Yes
Edu. Variables	No	Yes	Yes	Yes	Yes
Census Variables	No	No	Yes	Yes	Yes
Avg.crime pre policy and adult crime	No	No	No	Yes	Yes
Yearly controls	Yes	Yes	Yes	Yes	Yes
Cohort controls	No	Yes	Yes	Yes	Yes
LEA trends	No	No	No	No	Yes
Number of Obs.	2,772	2,772	2,772	2,772	2,772

Notes: Notation follows from equation (2), where *Ta* stands for areas that introduced the EMA programme and *Policy-On* indicates periods after the introduction of the EMA programme. Asterisks (*),(**),(***) represent significance at 10, 5 and 1 percent levels, respectively. Estimations are weighted by population. Standard errors, in parenthesis, are clustered by LEA. Controls defined as in Table 3.

Results from Table 5 show greater reduction in conviction rates due to burglary in EMA areas relative to non-EMA areas for 19 to 21 year old males. The difference-in-differences estimator is statistically significant even with the inclusion of initial conditions for convictions (column 4). However, once we control for linear area trends, the effect completely disappears. For burglary offences by 19 to 21 year olds, initial pre-policy *trends* may explain the statistically significant difference-in-differences estimator.

We compare results for 19 to 21 year olds with those that we obtained for the age group that was directly affected by the EMA programme (16 to 18 years old). Figure 2 shows estimated difference-in-differences parameters for these two age groups, with and without the inclusion of controls. Two issues are worth mentioning. First, the relative reduction in burglary rates for those aged 16 to 18 is nearly twice as large as for 19 to 21 year olds. Secondly, the difference-in-differences estimator for the 16 to 18 year olds remains statistically significant even after the inclusion of area time trends, whereas it is reduced to nearly zero for the 19 to 21 year olds. This provides an indication that the EMA programme may be having an effect on reducing convictions for the 16 to 18 year olds. This reduction is larger and remains significant for this age group.

Figure 2: Difference-in-differences estimator using regression analysis, convictions for burglary offences only, 16-18 vs. 19-21 year olds



Further, we investigate the relative reduction in conviction rates in EMA areas compared to other LEAs for males of different age groups. The difference-in-differences estimate for burglary using equation (2) is re-estimated for males aged 20 to 69. Table 6 shows that the relative difference between EMA and other LEAs in terms of burglary rates is not statistically significant for any of the groups. The values reported in Table 6 do not contain any controls for initial area differences.

Table 6: Convictions for burglary offences by age groups

	(20-29)	(30-39)	(40-49)	(50-69)
T_a	0.684 (0.409)*	0.183 (0.080)**	0.111 (0.028)***	0.006 (0.005)
T_a *policy-on	0.019 (0.165)	0.043 (0.041)	-0.032 (0.035)	0.005 (0.006)
Number of Obs.	1,848	1,848	1,848	4,620

Notes: Notation follows from equation (2), where *Ta* stands for areas that introduced the EMA programme and *Policy-On* indicates periods after the introduction of the EMA programme. Asterisks (*),(**),(***) represent significance at 10, 5 and 1 percent levels, respectively. Estimations are weighted by population and NO CONTROLS are included. Standard errors, in parenthesis, are clustered by LEA.

We do find that burglary rates pre-1999 for individuals aged 20 to 49 were significantly higher in EMA areas than in the rest of the LEAs (Table 6). This result confirms our initial statement that EMA areas were also likely to be areas with high conviction rates.

4.3 Sensitivity analysis 2: the RBI

One clear obstacle in the methodology is the fact that EMA areas are themselves likely to be, or already will have been, selected to implement other crime prevention initiatives. This is especially the case if, as we have seen before, EMA areas are also high crime areas. The case of burglary offers a unique opportunity to study this issue. Between 1999 and 2002, the Home Office introduced the RBI. The aim of this initiative was to reduce burglary nationally by targeting areas with the worst domestic burglary problems (Home Office, 2004). The Home Office invested around £25 million in projects in areas that had a significant burglary problem. These areas or communities were defined as areas that had, at the time, a burglary rate of at least one-and-a-half times the national average at that time, i.e. 36 or more burglaries, including attempts, per year per thousand households.

The RBI funded a total of 63 separate burglary reduction schemes in England and Wales. For England, these schemes were located in 60 LEAs. Projects were initially granted around £60,000 each, irrespective of the scale of the problem. In later rounds, applicants could bid for £100 for each burglary that occurred in the area or community over the previous 3 years. Early findings from an evaluation of 55 projects indicate that, on average, they reduced burglary by 20% (Kodz and Peace, 2003). Findings from 21 programmes in the Northern region of England show that the reduction in burglary was 10 percentage points higher than in the rest of country (Hirschfield, 2004). We verify here the impact of the RBI in terms of convictions.

Of the 15 LEAs that piloted the EMA programme, 7 were also selected by the RBI (Gateshead, Lambeth, Leeds, Middlesbrough, Nottingham, Oldham, and Southwark) and 53 introduced the RBI only.

Table 7 shows the difference-in-differences estimate for the relative reduction in conviction rates due to burglary offences between areas that implemented the EMA only, those that implemented the RBI only and those with both programmes, against the rest of the LEAs (equation 4).

Several results are worth highlighting from Table 7. From column 1, EMA-RBI areas had the highest average pre-programme conviction rates: 2.87 more convictions per 1,000 pupils than the rest of the LEAs. Areas where only the EMA was piloted had on average higher rates (1.49 convictions per 1,000 pupils) than the rest of the LEAs and higher than the compared average of RBI areas versus other LEAs (0.93 convictions per 1,000 pupils).

We find that areas that only introduced the EMA programme do not differ in terms of initial levels of conviction rates (columns 2 to 5) or relative changes in rates to other LEAs (columns 1 to 5). Areas that introduced only the RBI have a relative change in convictions of about 0.5 convictions per 1,000 pupils (average between columns 1 to 4). The initial difference between these areas and the rest of the LEAs disappears with the introduction of area-level economic and educational controls (column 2) and remains insignificant when more controls are included (columns 3 to 5).

Table 7: Difference-in-differences in burglary for EMA, RBI and EMA-RBI areas

	(1)	(2)	(3)	(4)	(5)
T_{EMA}	1.447 (0.792)*	0.853 (0.695)	0.742 (0.536)	0.381 (0.212)	0.283 (0.343)
T_{RBI}	0.934 (0.331)***	0.505 (0.307)	0.348 (0.297)	0.155 (0.119)	0.183 (0.197)
$T_{EMA-RBI}$	2.874 (1.011)***	2.013 (0.820)**	2.135 (0.698)**	0.774 (0.225)***	-0.541 (0.559)
T_{EMA}*policy-on	-0.780 (0.648)	-0.641 (0.620)	-0.691 (0.606)	-0.770 (0.658)	-0.137 (0.603)
T_{RBI}*policy-on	-0.595 (0.247)**	-0.477 (0.255)*	-0.505 (0.249)*	-0.534 (0.234)**	-0.048 (0.295)
$T_{EMA-RBI}$*policy-on	-1.367 (0.403)***	-1.087 (0.479)**	-1.115 (0.471)**	-1.215 (0.357)***	-1.551 (0.470)***
Controls					
Economic variables	No	Yes	Yes	Yes	Yes
Edu. Variables	No	Yes	Yes	Yes	Yes
Census Variables	No	No	Yes	Yes	Yes
Avg.crime pre policy and adult crime	No	No	No	Yes	Yes
Yearly controls	Yes	Yes	Yes	Yes	Yes
Cohort controls	No	Yes	Yes	Yes	Yes
LEA trends	No	No	No	No	Yes
Number of Obs.	2,373	2,373	2,373	2,373	2,373

Notes: Notation follows from equation (4), where T_{EMA} stands for LEAs where the EMA programme was piloted only, T_{RBI} for areas where the RBI programme was introduced only and $T_{EMA-RBI}$ for areas where both initiatives were introduced. *Policy-On* indicates periods after the introduction of the initiatives.

Asterisks (*),(**),(***) represent significance at 10, 5 and 1 percent levels, respectively. Estimations are weighted by population. Standard errors, in parenthesis, are clustered by LEA. Controls defined as in Table 3.

The greater reduction in convictions occurs between areas that introduced the EMA and the RBI programme against the rest of the LEAs. In these areas, conviction rates fell between 1.1 and 1.5 convictions per 1,000 pupils more than the rest of the LEAs (columns 1 to 5). This result is significant even after the inclusion of area-level controls, initial conditions on convictions and time trends. It is the incorporation of time trends that reduce the initial difference in burglary rates measured by the parameter β to zero (column 5).

The relative change in convictions in areas that introduced the EMA programme as well as the RBI is more than twice the one estimated for areas that introduced only the RBI. This result supports the hypothesis that the EMA programme may have had a knock-on effect on burglary reduction and may have contributed to the overall reduction in convictions for burglary in these areas.

5. Conclusions

This paper provides an empirical evaluation of the additional benefits that an educational programme, such as the EMA, may have in terms of crime reduction. The EMA programme was designed to increase participation in post-compulsory education and piloted in 15 LEAs in 1999. By providing financial support to youths from disadvantaged economic backgrounds, the EMA aimed to remove financial barriers to participation in education.

Theoretically, it is expected that participation in education may contribute to crime reduction by increasing income and hence the opportunity cost to engage in criminal activities. Education may also improve parenting skills, which are important in the inter-generational transmission of advantage that may prevent young people from criminal involvement. Other mechanisms fostered by education are patience and risk aversion, which may also deter the likelihood of committing crimes. The EMA programme may have had additional economic incentives that could have further contributed to crime reduction since it provided income support for youths to stay-on in education after compulsory schooling. By raising income, youths may not have the short-term need to obtain income through illegal activities.

We use area-level data on convictions for male juvenile burglary, theft and violent crimes (16 to 18 years old) from 1996 to 2002 to evaluate whether the EMA programme has had knock-on effects in terms of crime reduction. Using difference-in-differences estimation techniques, our results show that male conviction rates for burglary and theft offences fell more in the 15 LEAs that piloted the EMA programme than in the rest of the LEAs in England. This did not occur with violent crime. The significant difference in burglary rates in EMA areas relative to non-EMA areas remains significant after controlling for area differences using economic, educational, Census indicators, pre-policy average crime rates and area trends. This is not the case for thefts. Therefore, there is an indication that areas where the EMA programme was piloted had a significant reduction in conviction rates for burglary that may have been due to the programme.

We verify the potential bias in these estimates from the fact that the EMA programme was rolled-over to about one-third of the LEAs in 2001. We find that the extension of the programme does not seem to have had a significant effect on our estimates. Estimated difference-in-differences parameters for burglary and theft rates with and without controlling for the rolling-over of the programme were very similar.

Our results raise two issues. First, if the EMA targeted 16 to 18 year olds, do we find significant difference-in-differences in convictions exclusively for this group or also across the whole population? Second, if EMA areas were selected due to low participation in post-compulsory education and have, on average, higher crime and conviction rates, is it possible that other crime prevention initiatives were in action in EMA areas? If so, is it the effect of other initiatives rather than the EMA programme what is driving our results? And finally, do we find increasing social benefits from the establishment of these two government programmes rather than each of them separately?

For the first of these issues, we assess whether the EMA had an impact on convictions for burglary for other age groups. Burglary convictions by 19 to 21 years old in EMA areas fall by relatively more than in other LEAs. However, this result may be explained by general area trends in burglary. For other age groups between 20 and 69 years, we do not find difference-in-differences in conviction rates between EMA and non-EMA areas. What we do find is that, between 1996 and 1999, EMA areas have higher average conviction rates across most age groups than the rest of the LEAs. This result confirms the fact that EMA areas are also high conviction areas and therefore likely to have been selected for other government social programmes.

The main finding of this paper is established when burglary reduction is evaluated for areas that introduced both direct targeting on burglary, the RBI, and the EMA programme. Between 1996 and 1999, these areas had 2.9 more convictions for burglary per 1,000 pupils than other LEAs. In these areas, burglary fell between 1.1 and 1.5 convictions per 1,000 pupils relative to areas where these programmes were not introduced. At the rate of 1.1 less convictions, we estimate that it will take nearly 6 years for conviction rates for burglary in these areas to reach the average of the other LEAs.

For RBI action areas, burglary was reduced by 0.5 convictions per 1,000 pupils relative to other LEAs. Between 1996 and 1999, these areas had only 0.9 less convictions than other LEAs. Given the smaller difference in convictions between these areas and other LEAs, we estimate that at the rate of 0.5 less convictions it will take 3 years for these areas to reach the levels of other LEAs. Finally, we find a uniform pattern of conviction reductions in EMA areas relative to the rest of the LEAs. However, these estimates are not statistically significant, perhaps due to the low number of observations. Still, we estimate that at the rate of 0.7 less convictions in EMA areas relative to other LEAs, it may take up to 3 years to reach the levels of other LEAs.

The importance of our result is highlighted by four factors. First, our indicator of crime is convictions. Not all crimes are prosecuted and convicted, only a proportion of total crimes. Therefore, the relative reductions found in this paper may be larger than estimated. Secondly, the relatively low number of LEAs that piloted the EMA programme implies that statistical significance is harder to achieve. In this paper we achieve statistical significance of the difference-in-differences estimator that is close to 5 percent. Thirdly, not all the individuals in the LEAs that piloted the EMA programme were entitled to receive financial support. Only those with a parental income less than £20,000 were entitled. Moreover, between 63% and 65% of eligible youths applied for EMA support. Therefore, our estimates of the potential effects of the EMA programme on crime reduction are biased downwards. Finally, areas where the EMA and the RBI programmes were introduced are those where our estimates are robust to the inclusion of all controls plus area trends. This provides a strong indication that it was not the RBI or the EMA that worked in isolation to prevent crime. Areas where these programmes were introduced were affected the most.

In conclusion, we find clear grounds that the introduction of the EMA together with the RBI programme had significant and substantive effects on conviction rates for burglary offences by 16 to 18 year olds. Therefore, educational policies could have substantive external effects and could complement direct interventions for crime

prevention. Our findings raise the importance of the interconnectivity of departmental programmes. Crime prevention could be achieved not only by tough policing or increasing the number of officials but also by providing the police force with the information of alternative opportunities viable for youths. Therefore, a combination of programmes in deprived areas could have important social benefits for the population.

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WIDER BENEFITS OF LEARNING RESEARCH REPORT NO.14

Education and youth crime: effects of introducing the Education Maintenance Allowance programme

Education is a potentially large influence on individual propensities to offend and possibly an important source of area-level variation in crime rates. Crime statistics for England indicate that crime rates are lower in areas with higher levels of education, which are also areas of higher per capita income and contain a higher proportion of families belonging to the highest socio-economic status (Home Office, 2003). Whether the association between education and crime is causal, or whether it masks a number of possible effects that may not be due to education, is less clear.

This report explores the relationship between education and crime using a natural experimental setting. To do this, we evaluate the Educational Maintenance Allowances (EMA) – a Department for Education and Skills (DfES) programme designed to increase participation in post-compulsory education – in terms of juvenile crime reduction. We first address the theoretical reasons why education can have an effect on crime reduction and the methodology to evaluate the EMA programme. We then present the results from our estimates of the change in conviction rates for burglary, thefts and violent crimes, for 16 to 18 year old males, in EMA areas relative to other areas. In particular, we explore the effects of the EMA programme on burglary reduction in combination with the Reducing Burglary Initiative, a crime prevention programme introduced by the Home Office in England and Wales around the same time that the EMA were being piloted.

Our research highlights the importance of connections between government programmes for young people, such as the EMA and the RBI, and demonstrates the high levels of potential social benefit that may flow from programmes that recognise the connections between key agencies, government departments and stakeholders. This report also shows the capability of education provision in meeting wide-ranging policy objectives.

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ISBN 0-9547871-4-5