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The Labour Supply Effect of Education Maintenance Allowance and its Implications for Parental Altruism

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ABSTRACT

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Education Maintenance Allowance (EMA) was a UK government cash transfer paid directly to children aged 16-18, in the first two years of post-compulsory full-time education. This paper uses the labour supply effect of EMA to infer the magnitude of the transfer response made by the parent, and so test for the presence of an 'effectively altruistic' head-of-household, who redistributes resources among household members so as to maximise overall welfare. Using data from the Longitudinal Study of Young People in England, an EMA payment of £30 per week is found to reduce teenagers' labour supply by 3 hours per week and probability of employment by 13 percentage points from a base of 43%. We conclude that parents withdraw cash and in-kind transfers from their children to a value of between 22% and 86% of what the child receives in EMA. This means we reject the hypothesis of an effectively altruistic head-of-household, and argue that making this cash transfer directly to the child produces higher child welfare than if the equivalent transfer were made to parents.

JEL Classification: I38, J22, H53

Keywords: Education Maintenance Allowance, altruism, transfers, rotten kid, labour supply

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NON-TECHNICAL SUMMARY

Education Maintenance Allowance (EMA) of up to £30 per week was paid to 16-18 year-olds in full-time education and from low income backgrounds (household income below £30,810 per year). The programme ran across the UK from 2004 until 2011, when it was withdrawn in England, but has been retained in Scotland, Wales and Northern Ireland. It was designed as an incentive to stay in education or training beyond the minimum school leaving age.

Government transfer programmes targeting children are usually made 'in-kind' (e.g. a Free School Meal, rather than the cash equivalent) or paid to their parents (e.g. Child Benefit). Past evidence shows that cash transfers paid to adults are rarely spent as intended: The 'winter fuel payment' scarcely increases spending on fuel, and 'Child Benefit' may not increase spending on children's items any more than an increase in income from any other source. This means that paying £30 per week to parents, even where explicitly framed as a reward for their child, can be expected to raise the child's welfare by the same amount as a £30 per week increase in the parent's general income. However, even with the money paid straight to the child, the parent may still (i) reduce the pocket money they give their child and (ii) make fewer purchases (in-kind transfers) on the child's behalf, potentially leaving the child no better off than without EMA.

This paper tests whether paying EMA directly to the child makes the child better off than if the equivalent transfer were paid to his parents. There is no data on pocket money or in-kind transfers, so we address this research question by testing for a change in the child's labour supply when he receives EMA. The idea is that if, for example, the government gives £30 EMA to the child and the parent's response is to take £30 away from the child, the child is left in the same financial position as had EMA never been given. He will therefore choose to carry on earning from part-time employment for the same hours as before. If the parent takes away less than £30 however, the child has more unearned income, so will reduce his labour supply to benefit from some extra leisure.

Using data for a recent cohort of English teenagers aged 16-18, an EMA payment of £30 per week is found to reduce teenagers' labour supply by 3 hours per week and probability of employment by 13 percentage points from a base of 43%. We conclude that parents withdraw cash and in-kind transfers from their children to a value of between 22% and 86% of what the child receives in EMA, making the child significantly better off than if the same money had been given to his parents.

1. Introduction

Publicly provided transfers targeted at children are usually made in-kind or as a hypothecated cash transfer paid to parents. There are two mechanisms which may mitigate the benefit from these transfers to the intended recipient. Firstly, if the transfer is paid to the parent, there is an agency problem: The parent is not compelled to spend the benefit on the child. For example, Blow et al (2012) find that unanticipated variation in the level of Child Benefit in the UK affects expenditure predominantly on adult-assignable goods, while Kooreman (2000) finds strong positive effects of the Dutch Child Benefit on child-assignable goods. (The 'labelling effect' of the programme's name clearly differs between these countries – see Beatty et al, 2014). It also matters *which* parent receives the welfare payment, with a switch from father to mother ('wallet to purse') being shown to raise expenditure on child care and children's clothing, and reduce expenditure on alcohol and tobacco, for example (Lundberg et al., 1997; Phipps and Burton, 1998). Secondly, regardless of who receives the transfer, parental altruism may substantially offset the gain to the targeted household member, as an altruistic head-of-household may redistribute resources among household members so as to maximise household welfare (Becker, 1974, 1981). In this case, an in-kind transfer may still benefit the child if the household is induced to consume more of the good than it would voluntarily (Currie and Gahvari, 2008), or if the parent does not perceive the publicly provided good to be a close substitute for a privately provided good. For example, Bingley and Walker, (2013), show that day care milk or milk tokens in the UK crowd out private expenditure on milk (an essentially homogeneous product) to 80% of these transfers' value, but Free School Meals (for which there is no close market substitute) only crowd out expenditure on food to 15% of their value. Nevertheless, von Hinke Kessler Scholder (2011) finds no effect of the withdrawal of Free School Meals from some groups on their bodyweight, suggesting that targeted children receive no better an overall diet than in the absence of the programme.

The extent to which the incidence of the net benefit of a transfer programme is retained by the targeted recipient is referred to as the 'Intrahousehold Flypaper Effect' (Jacoby, 2002). In this paper we evaluate the magnitude of this effect for the UK's Education Maintenance Allowance (EMA) programme. EMA was a means-tested cash transfer of up to £30 per week paid by the UK government to students undertaking the first two years of full time post-compulsory education (aged 16 or 17 on 31st August at the start of the school year). Eligibility was determined by household income, according to the thresholds shown in Table 1.[1] At its peak in the 2009-10 school year the scheme cost £580m and served 643,000 recipients (see Bolton, 2011, p.2).

Table 1: Eligibility Thresholds for EMA

Household Income, per year	EMA Entitlement, per week
<£20,818	£30
£20,818 - £25,521	£20
£25,522 - £30,810	£10
>£30,810	Zero

[1] Income earned by the child through part-time work or their own welfare receipt was disregarded. These thresholds and entitlements were unchanged in nominal terms over the scheme's life in England, 2004-2011.

EMA differs from most high profile conditional cash transfers (CCTs), such as Bolsa Família in Brazil, Opportunidades in Mexico, and Opportunity NYC in the United States, in two ways. Firstly, it is a late intervention, targeting the continued human capital development of 'children' (in fact young adults) beyond the compulsory schooling age, rather than school attendance or health programme participation among primary-age children. Secondly, it is paid straight to the child, rather than to the mother.

Paying CCTs to the mother requires her agency, to pass the (benefits of the) transfer to the intended recipient, on behalf of the state. Because EMA was paid directly to the child, there is no agency problem. This and several other contextual features make EMA ideal to investigate the extent of crowd-out of private transfers by this public transfer, and to attribute this effect to the mechanism of parental altruism. EMA was paid in cash, which is a perfect substitute for cash transfers from parents, and for the parents' own income. Moreover, barriers to participation were low (students needed a bank account in their name and a parental declaration of income once each academic year) and stigma unlikely to be a problem (the eligibility criteria were wide and take-up high - in our data 86% of those apparently eligible for the highest payment, and 45% of all students, receive EMA), so conditional on participation in full-time education the direct non-pecuniary costs associated with receipt of the benefit should be negligible. The intervention was also generous, worth up to £1170 per year. Altogether, this means that the parent's transfer response to EMA should provide a clean test of whether the parent's behaviour is consistent with that of an effectively altruistic head-of-household.

The extent to which the public transfers are crowded out by family transfers is usually evaluated using data on household expenditure patterns for 'child-assignable goods' (Kooreman, 2000; Hoddinott and Skoufias, 2004; Attanasio and Mesnard, 2006; Blow et al, 2012). A challenge to the identification of this degree of crowd-out is lack of (or measurement error in) data on (the value of) shared services or in-kind transfers within private households. For this reason, one approach is to focus on units of the extended family that are not co-resident, and so in which the shared services can be assumed to be zero. For example, Jensen (2003) showed that each unit increase in public pension income in South Africa reduced receipt of private transfers from the pensioner's children living outside the home by 0.20-0.30 units. However, when considering public transfers paid to young adults, co-residence is likely to represent a significant proportion of the support received from their parents. Rosenzweig and Wolpin (1994), for example, use changes in welfare rules over time and between states to show that a $1000 increase in Aid for Families with Dependent Children (AFDC) by young women with children reduces their probability of receiving financial aid from their parents by 3.4%, and of co-residing by 4.7%. While this provides evidence that the net benefit of the AFDC programme is mainly captured by its recipients, the authors' data do not enable them to identify the effective rate at which parents 'tax' their children's benefit receipt.

All students in our sample are co-resident with a parent,[2] but we do not have data on cash transfers made by parents to children receiving EMA, and we expect additional unobserved heterogeneity in in-kind transfers or the items that children are expected to purchase themselves. We therefore propose an alternative strategy to identify the net change in the child's opportunity set, which does not depend on any survey instruments designed to capture intrahousehold transfers. Our identification strategy instead stems from the insight,

[2] We drop teenagers in social care from our sample.

formalised in the theoretical model set out in section 2, that if parents respond to the child's receipt of EMA by withdrawing cash and in-kind transfers of an equal value (consistent with the parent 'fully insuring' the child's consumption), then the child's opportunity set is unchanged, and he should not alter his labour supply. Correspondingly, the larger the child's reduction in labour supply, the smaller the redistributive response made by parents, or equivalently, the greater the proportion of the EMA the child has been permitted to keep.

To pre-empt our results, estimates from linear, Tobit and logistic regression methods using data from the Longitudinal Study of Young People in England (LSYPE), in both cross-sectional and panel data frameworks, firmly reject a model of effectively altruistic parents. An EMA payment of £30 per week reduces teenage labour supply by between 2.4 and 3.2 hours per week at the intensive margin. These results are robust to estimation on the sub-sample of non-credit-constrained households, for whom we argue participation in post-compulsory education is unlikely to be affected by eligibility for EMA. Using estimates of teenagers' labour supply response to unearned income obtained from elsewhere in the literature (Dustmann et al, 2009, Wulff Pabilonia, 2001), we calculate this to be consistent with parents withdrawing cash and in-kind transfers from the child to between 22% and 86% of the value of EMA.

While the (non-) altruistic behaviour of parents has implications for the targeting of transfers – our results indicate that the child's welfare benefit from EMA is higher than had an equivalent transfer been made to parents - the labour supply effect of EMA has implications for the efficacy of conditional cash transfers in raising educational performance. In-school employment is widespread. In our data, 43% of 17 year-olds in the first year of post-compulsory education are in employment. In-school employment may improve teenagers' stock of cognitive and non-cognitive human capital (for example, financial literacy, communication skills and lower discount rates – Oettinger, 1999; Light, 2001) or preference for education as a route to higher-skilled work in future (Dustmann and van Soest, 2007). However, by crowding out time and effort devoted to study (e.g. Kalenkoski and Wulff Pabilonia, 2013) it may reduce the child's educational performance, particularly above a moderate number of hours per week or in close proximity to high-stakes examinations (Lillydahl, 1990, Ruhm, 1997; Payne, 2004). Hence, to the extent that EMA reduces labour supply at least at the higher end of the working hours distribution, this should feed through to an improvement in their academic and future labour market outcomes.[3]

While there are indications from hypothetical questions that EMA reduced recipients' labour supply (RCU Market Research, 2007), to our knowledge we are the first to quantify this labour supply effect using observational data. Although EMA closed to new applicants in England in January 2011, it was replaced by the '16-19 bursary' programme, with a smaller budget of £180m, and automatic entitlement reduced in scope to approximately 12,000 of the "most vulnerable" students. EMA has been retained in the rest of the UK. It will be important for policymakers to account for the labour supply effect of this scheme in considering any future reforms.

The remainder of this study is structured as follows: Part 2 sets out a model showing how the labour supply response to EMA provides a test for the presence of an effectively altruistic head-of-household. Part 3 discusses

[3] We do not evaluate this effect directly, and discuss the challenges in doing so in our conclusions.

the data and estimation strategy, Part 4 presents the results and Part 5 sets out the conclusions and recommendations.

2. Theoretical and Empirical Model

In this section we develop a theoretical model for the joint determination of parental transfers and the child's labour supply. We follow closely the structure of Dustmann et al (2009) and Kalenkoski and Wulff Pabilonia (2010) but extend their analysis to account for (i) the introduction of EMA – an exogenous cash transfer paid to the child – and (ii) endogenous selection into post-compulsory education as a function of potential receipt of EMA, parental transfers and labour supply.

We assume that if the child is not in full-time education he will earn the utility $\overline{U}_0$, which we treat as exogenously determined. If $\overline{U}_0$ exceeds the maximum utility attainable from being in full-time education, as determined by the model we now outline, the child will leave full-time education. We return to the issue of endogenous selection into post-compulsory education in section 2.2.

Our structural parameter of interest is the amount, λ, by which parental transfers are reduced for every pound the child receives in EMA. We face the challenge that, for the relevant age group, there exist no data on transfers in the LSYPE. More broadly, even where information on cash transfers is elicited, researchers still lack data on in-kind transfers and the items which children are expected to pay for themselves, which are required for complete identification of models of parental altruism.[4] Our model shows how the child's labour supply response to EMA can be used for inference about parents' withdrawal of both cash and in-kind transfers.

We assume two agents; a selfish child and altruistic parent. Each holds full information about the preferences of the other. Both wish to maximise the present value of their expected lifetime utility. The parent's altruism may be impure, in that she values the child's academic performance more highly than does the child. Each agent may discount future utility at different rates, or hold distinct beliefs about how current behaviour will impact upon future opportunities.

The parent announces a contingent rule specifying the baseline transfer she will make if the child is in full-time education and working zero hours, T, and the amount by which the transfer will be reduced for every pound the child earns in the labour market, t. It is costless to set and revise the transfer level. (Any announcement or child's expectation about the support to be provided if the child is not in full-time education is built into the reservation utility $\overline{U}_0$).

The child is assumed to have no bargaining power. Taking this parental strategy as given, the child then chooses his labour supply $\ell \in [0,1]$ at a constant wage w (and effective wage $w.(1-t)$), to maximise his utility function $U(C, L)$ defined over consumption (C) and Leisure (L), which comprises all non-labour market activities. Normalising the total time available to unity imposes $L = 1 - \ell$. The function $U(C, L)$ is assumed to be strictly increasing, twice differentiable and strictly quasiconcave in its arguments. The child's concerns regarding future consumption or academic performance are nested within his utility from leisure.

[4] The age 16 sweep of the UK's National Child Development Study of a cohort born in 1958 and studied by Dustmann et al, 2009, is an exception.

Without EMA, the child's only source of unearned income, ω, is the transfer from parents. Rewriting U in terms of labour supply, the child's problem can be defined as:

$$\max_{C,l} U(C, 1-l)\ subject\ to\ \ C\ \leq T + w.(1-t).l \qquad (1)$$

Assuming that leisure is a normal good over the relevant domain ensures that the child's optimal labour supply $l*$ is non-increasing in unearned income, ω, and strictly decreasing for for $l* > 0$:

$$\left.\begin{aligned} &\frac{\partial l*}{\partial \omega} \leq 0\ \ if\ \ l* \geq 0 \\ &\frac{\partial l*}{\partial \omega} < 0\ \ if\ \ l* > 0 \end{aligned}\right\} \quad (2)$$

We also assume that optimal labour supply is non-decreasing in the effective wage, $w.(1-t)$,and strictly increasing for $l* > 0$:

$$\left.\begin{aligned} &\frac{\partial l*}{\partial (w.(1-t))} \geq 0\ \ if\ \ l* \geq 0 \\ &\frac{\partial l*}{\partial (w.(1-t))} > 0\ \ if\ \ \ l* > 0 \end{aligned}\right\} \quad (3)$$

The child will undertake paid employment if and only if the effective wage exceeds the marginal rate of substitution of leisure for consumption at the initial endowment point. Formally this may be expressed as:

$$\left.\begin{aligned} &l*\ > 0 \quad iff \quad w.(1-t) > \frac{\partial U(T,1)/\partial L}{\partial U(T,1)/\partial C} \\ &l* = 0 \quad iff \quad w.(1-t) \leq \frac{\partial U(T,1)/\partial L}{\partial U(T,1)/\partial C} \end{aligned}\right\} \quad (4)$$

Finally, we define for each child a reservation utility, $\overline{U}_0$, equal to that which could be obtained by leaving full-time education. If this is not attainable at the optimum position, the child will not participate in post-compulsory full-time education.

This model formalizes the stylized facts from the literature (Dustmann et al, 2009; Kalenkoski and Wulff Pabilonia, 2010; Wolff, 2006; Gong, 2009) that, conditioning on the child being in full-time education, (i) parents provide smaller transfers, or are less likely to provide positive transfers, the more the child works, other things equal, and (ii) children undertake less employment, the greater the transfer received from parents, other things equal. Equations (1-4) also accommodate a discrete choice framework wherein the probability of working positive hours is non-increasing in unearned income and non-decreasing in the effective wage. A discrete framework may be more appropriate if employers are unwilling to hire individuals for less than a minimum number of hours each week.

Retaining the notation developed above, the model is summarised in Figures 1 and 2. Reservation utility (that which the child would gain by leaving compulsory education) is represented by indifference curve IC_0. Higher indifference curves represent higher utility. The budget constraint (BC) represents the upper bound of the child's opportunity set for $l > 0$. Interior optima are defined by the tangency of budget constraint and indifference curve. Figure 1 shows the case with a fixed lump-sum transfer (T_1), and a varying 'tax rate' on the child's earnings (t). Here, the lump-sum transfer T_1 is just sufficient to ensure that the child does not need to take employment in order to meet his education participation constraint. The parent can then induce zero hours of work by 'taxing' the child's income at a rate of 100% (setting $t = 1$), while still ensuring the child stays in education. Reducing t raises the effective wage, increasing the slope of the budget constraint and improving the child's welfare as higher indifference curves become attainable. In line with our assumption in equation (3), reducing the tax rate and raising the effective net wage is here shown to induce longer hours of work.

Figure 2 shows the case with a fixed tax rate ($t = t^*$), but varying the size of the initial lump-sum transfer. A child offered T_1 will, at zero hours of work, be indifferent between staying in and leaving full-time education, but by undertaking his optimum labour supply $l^*(\omega=T_1)$ will have strictly higher utility than the reservation level. A child in this situation will therefore continue in full time education. His welfare can be further improved by raising the lump sum transfer to T_2. In line with our assumption in equation (2), conditioning on meeting the participation constraint, increasing the child's unearned income is here shown to induce shorter hours of work. In Figure 2 however, a child offered T_0, for example, cannot attain his reservation utility at *any* level of employment. Without additional financial support from outside the household, he will leave full time education.

Fig. 1: Labour Supply with $\omega=T_0$ and t varying. (Fixed lump-sum transfer, varying 'tax rate')

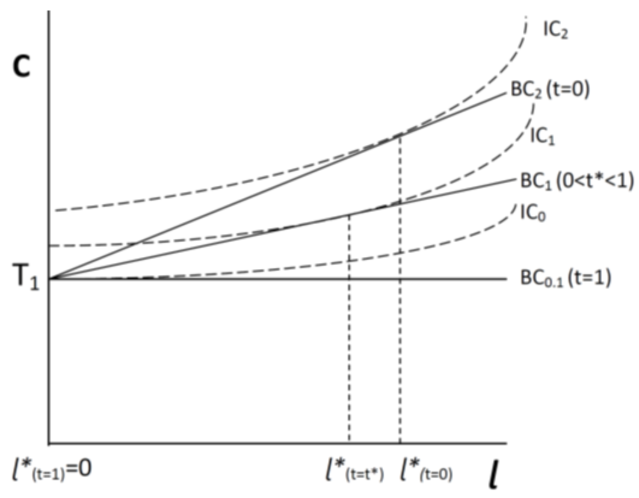
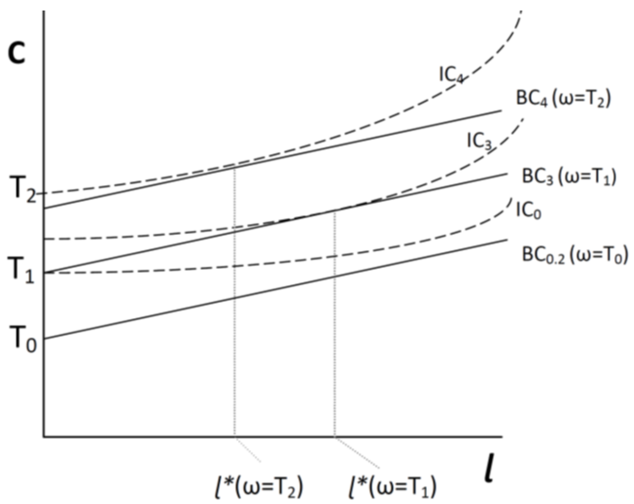


Fig. 2: Labour Supply with t=t* and ω varying. (Varying lump-sum transfer, fixed 'tax rate')



2.1. Introducing EMA

Let us then introduce an additional source of unearned income paid straight to the child; EMA. We first consider the situation of an individual whose education-participation constraint is satisfied without EMA. For this group, EMA can be treated as exogenous, conditional on the parent's income. (The maximum annual difference in EMA payments from moving into a lower income bracket - £390 - is too small for parents profitably to 'fine-tune' their true income). [5]

In Figure 3, EMA initially induces a vertical upward shift in the child's budget constraint. However, in response, the parent may choose to reduce the transfer T by some proportion $\lambda \in [0, 1]$ of the value of EMA received by

[5] £10 per week, 39 weeks per year.

the child. (A lower λ permits the child to 'keep' an increasing proportion of his EMA). The expression for the child's unearned income is now:

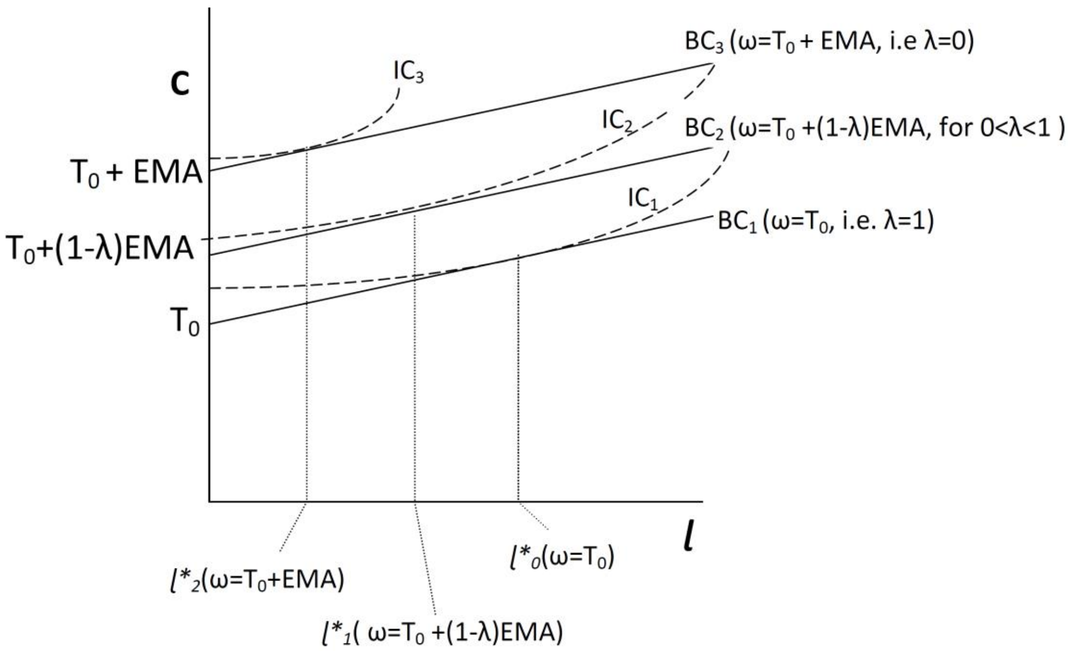
$$\omega = T + (1 - \lambda). \text{EMA} \qquad (5)$$

The parent's response to *earned* income, defined by t, is assumed not to change. The child's problem can now be written:

$$\max_{C,l} U(C, 1 - l) \textit{ subject to } C \leq T + (1 - \lambda). \text{EMA} + w. (1 - t). l \qquad (6)$$

If $\lambda = 1$, the child's EMA is entirely offset by an equivalent reduction in the transfer from the parent. This leaves the child's budget constraint unchanged compared with the initial situation. With the same opportunity set, the child's working hours should also remain unchanged. Hence, if we observe a negative labour supply response to EMA, this implies $\lambda < 1$, and we can reject the null hypothesis of 'full insurance', or parents isolating their children from any income variation.[6] However, as EMA is an exogenous payment to the child, it does not constitute a zero-sum redistribution of household resources. This means that to reject a null hypothesis of an effectively altruistic parent (who redistributes resources so as to maximise household welfare) we must reject $\lambda \geq (1 - \theta)$, where θ is the parent's marginal propensity to transfer to the child out of her own income. We do not have the data to test this directly, but present back-of-the-envelope calculations appealing to results elsewhere in the literature.

Fig. 3: Introducing EMA to the labour supply model.



[6] Failure to reject a labour supply response of zero is not sufficient to conclude that parents are fully insuring their children. This could result from an income-elasticity of labour supply of zero. However, a negative labour supply response is sufficient to reject both an income elasticity of zero and full insurance.

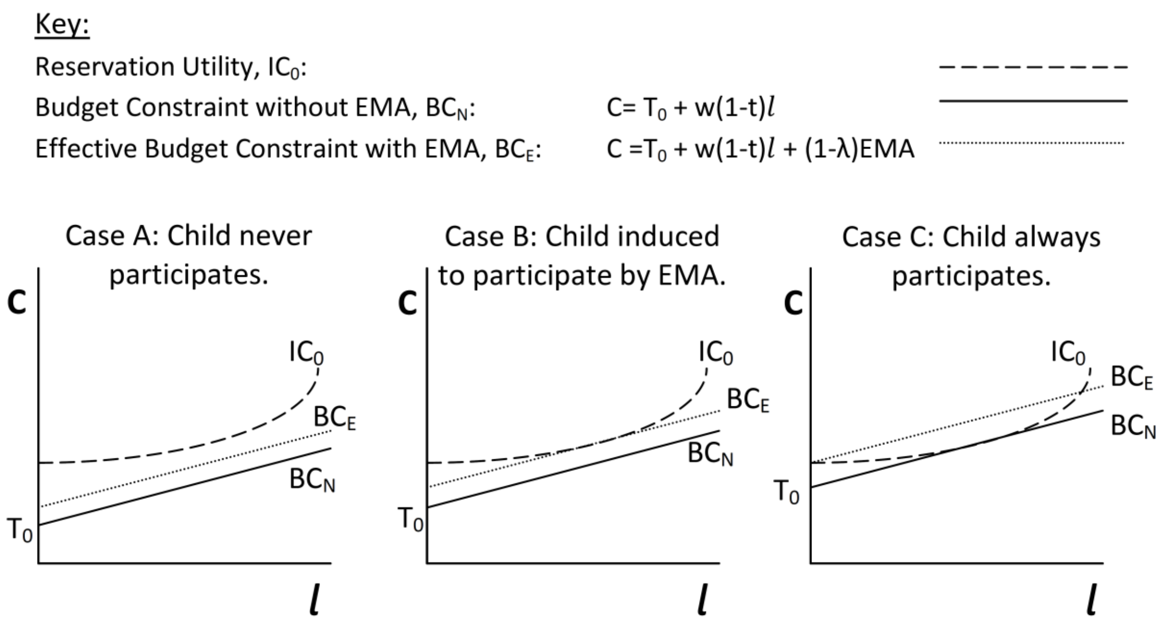
2.2. Endogenous selection into full-time education

The policy objective of EMA was to increase participation in post-compulsory education. Dearden et al (2009) provide an evaluation based on a pilot scheme in matched areas of England, and concluded that EMA raised participation by eligible young people in the first year of post-compulsory schooling by 4.5 percentage points, from a base of 65%. The effect was larger among children living in rented accommodation or social care. The authors suggest this provides evidence that the principal mechanism by which EMA increases participation is by easing credit or liquidity constraints rather than simply reducing the opportunity cost of education.

In our model, the condition for EMA to induce a child to stay in full-time education is illustrated in case B of Figure 4. Net of the parent's response, adding EMA to the child's effective budget constraint must make the reservation utility newly attainable. If the initial parental transfer were any smaller than in case B, the child would still be worse-off in full-time education *and* receiving EMA than if he dropped out (case A). On the other hand, if the initial parental transfer were sufficiently larger the child would continue in full-time education even without EMA (case C). EMA is therefore a binding consideration in the education participation decision of only a narrow group of people.

Nevertheless, it is clear that the treated group (receiving EMA) is fundamentally different to the non-treated group in that it contains some individuals ('inducees') who are only present in full time education *because* they receive EMA. The non-treated group (non-recipients in full-time education) only contains individuals who would have continued in full time education without EMA.

Fig. 4: How EMA may affect the decision to participate in post-compulsory education.



Household income in wave 4 of the LSYPE is recorded only in bands, the threshold of which do not accord with those for EMA eligibility, and the period which income is recorded does not correspond to that over which eligibility is determined. Moreover, no information on parental income is collected at all in wave 5

(Considerable information is instead collected on own income and benefit receipt among those cohort members who had left full-time education). This makes it impractical to assign a counterfactual EMA status for those who did not continue in education and thus model the role of EMA in the selection process empirically. Instead, we present a series of empirical specifications and show that our results are robust to a series of assumptions under which the bias caused by inducees (or indeed, other unobserved differences between EMA recipients and non-recipients) is eliminated.

Dearden et al (2009, p.837) argue that most inducees were drawn from "financially unproductive activities" rather than paid work. This suggests that the type of individual for whom EMA makes a difference to continued education is poorly motivated with respect to labour market activities, or more likely to live in deprived areas where there are fewer *opportunities* to work. We are able to proxy for local labour market opportunities using regional dummies and the Index of Multiple Deprivation for the child's area of residence. Assuming there is no residual difference in unobserved motivation between recipients and non-recipients, our separate cross-sectional estimates for students in the first (wave 4) and second (wave 5) years of post-compulsory schooling will be unbiased. However, if, conditional on our set of individual and household characteristics, the child's motivation is positively correlated with hours of work and negatively correlated with EMA, the estimated labour supply effect of EMA will be downward biased. In addition, therefore, we control for time invariant differences in unobserved individual motivation or labour market opportunities using fixed-effect regression and conditional fixed-effect logit models. We also re-estimate our models using non-credit-constrained households, who we argue will not have been influenced by EMA when making their education participation decision, and show that the labour supply effect of EMA for this group does not significantly differ from that for the general population. We undertake additional robustness and sensitivity analyses by estimating on the sub-sample of male and female cohort members, and those interviewed during school term time.

3. Data

We use data from the Longitudinal Study of Young People in England (LSYPE), which tracked a cohort of individuals born between 1st September 1989 and 31st August 1990, and so in the same academic year at school. We use data from the third wave, conducted mainly in March-June of 2006, when respondents were 15 or 16 years old and in their final year of compulsory education, and the fourth and fifth waves, conducted mainly in June-July of 2007 and 2008 when respondents were 16-17 and 17-18 years old and those continuing in post-compulsory education were in their first and second years respectively. In wave 4, of the 11,801 respondents, 8971 were in full time education. 4359 of these received EMA and 3795 reported positive hours of paid employment. Corresponding figures for wave 5 are 10,430 (reflecting sample attrition), 6953 (additionally reflecting dropout from full-time education), 3384 (take-up of EMA remained the same among those still in education) and 3632 (so a larger proportion of those in education had a part-time job at wave 5 than wave 4). There are no data documenting cash or in-kind transfers from parents, and what the child is required to pay for himself. We do observe the level of EMA received and their usual weekly hours of paid employment.

The profile of EMA take-up against household income in both waves 4 and 5 of the LSYPE, together with raw sample numbers, is shown in Table 2. Our data show substantial numbers of both students who would be eligible for EMA according to their current or previous year's household income but do not receive EMA, and apparently ineligible students who do receive EMA. Those in the second group may have experienced a rise in family income after having applied for EMA, but retained their entitlement until the end of the academic year (only then must they reapply). They may instead have obtained EMA through false reporting of household income, or be reporting income in the survey with error.

Table 2: Take-up of EMA in estimation sample, by household income band and entitlement bracket

Annual Household Income Band (measured in wave 4)	Conditional on participation in full-time education						
	Wave 4			Wave 5			
	Sub-Sample Size	EMA recipients	EMA take-up	Sub-Sample Size	EMA recipients	EMA take-up	Weekly EMA entitlement
<£2600	74	56	75.68%	48	39	81.25%	£30
£2,600 - £5,199	220	180	81.82%	150	122	81.33%	
£5200 - £10,399	617	573	87.03%	422	355	84.12%	
£10,400 - £15,599	834	711	85.25%	543	457	84.16%	
£15,600- £20,799	698	580	83.09%	474	390	82.28%	
£20,800 - £25,999	675	491	72.74%	438	315	71.92%	£10, £20 or £30
£26,000 - £31,199	664	371	54.37%	448	236	52.68%	Zero or £10
£31,200 - £36,399	525	134	25.52%	365	69	18.90%	Zero
£36,400 - £41,599	461	67	14.53%	325	37	11.38%	
£41,600 - £46,799	417	36	8.63%	284	22	7.75%	
£46,800 - £51,999	410	23	5.61%	281	15	5.34%	
≥£52,000	1405	36	2.56%	1011	19	1.88%	
All	**7000**	**3212**	**45.89%**	**4789**	**2076**	**43.35%**	
Contains all observations for which neither household income nor EMA receipt entries are missing.							

Non-take-up will partly depend on observed characteristics. For example, the informational demands when applying for EMA are greatest for those with self-employed parents, and the opportunity cost of parents' time (to help with the application) is likely to be related to their income and occupation. An omitted variables bias will occur if receipt of EMA is partially correlated with omitted variables that also help determine working hours. For example, more highly motivated teenagers are likely to pursue the application process most ardently, while also being likely to work longer hours, other things equal. This will positively bias the labour supply effect of EMA.

Hourly wages are not directly elicited. Instead of introducing measurement error by dividing weekly earnings by weekly hours, and necessitating a selection model (since the counterfactual wage of those not in employment is not observed), we omit wages from our model and assume they are partially uncorrelated with receipt of EMA. This assumption seems plausible. As argued by Wolff (2006), the teenagers considered here are likely to work predominantly at fixed hourly rates of pay close to the legal minimum wage. Motivation or any other unobserved personality traits, which may also affect receipt of EMA, are unlikely to be rewarded with higher wages.

Table 3: Sample descriptive statistics for wave 4 (first year of post-compulsory education) by covariate sub-group.

	Wave 4				Wave 5			
Covariate Sub-group	Prop'n of Sub-group In Full-time Ed'	Conditional on participating in full-time education			Prop'n of Sub-group In Full-time Ed'	Conditional on participating in full-time education		
		Unconditional Mean Hours of Work (Standard Deviation)	Prop'n reporting Positive Hours of Work	Conditional on Positive Hours: Mean Hours of Work (Standard Deviation)		Unconditional Mean Hours of Work (Standard Deviation)	Prop'n reporting Positive Hours of Work	Conditional on Positive Hours: Mean Hours of Work (Standard Deviation)
All	77.05%	5.28 (7.85)	43.10%	12.26 (7.59)	70.12%	5.77 (8.08)	45.80%	12.90 (7.34)
EMA Weekly Payment								
Zero	100%	6.22 (7.99)	52.17%	11.92 (7.37)	100%	6.64 (8.36)	53.07%	12.70 (7.53)
£10	100%	6.22 (7.71)	52.67%	11.81 (6.85)	100%	7.63 (8.61)	55.30%	14.11 (7.53)
£20	100%	5.83 (7.82)	47.29%	12.33 (6.97)	100%	6.51 (8.30)	50.68%	13.15 (7.19)
£30	100%	3.86 (7.45)	29.53%	13.05 (8.22)	100%	4.14 (7.24)	33.19%	13.07 (7.00)
Household Income Band (measured in wave 4)								
<£2600	76.77%	3.59 (9.39)	21.05%	17.06 (14.00)	72.73%	2.56 (5.16)	22.91%	11.18 (4.37)
£2,600 - £5,199	72.35%	3.13 (6.68)	26.22%	11.95 (8.08)	71.09%	2.61 (6.23)	21.33%	13.24 (7.54)
£5200 - £10,399	68.91%	3.49 (7.00)	26.81%	13.02 (7.66)	69.98%	4.13 (7.36)	32.30%	16.63 (7.05)
£10,400 - £15,599	74.20%	3.73 (7.17)	29.75%	12.55 (7.89)	67.62%	4.24 (7.54)	33.83%	12.96 (7.78)
£15,600- £20,799	74.63%	4.95 (7.98)	38.39%	12.90 (7.96)	68.30%	5.30 (7.64)	43.67%	12.45 (6.93)
£20,800 - £25,999	75.50%	6.11 (8.77)	45.76%	13.36 (8.45)	66.46%	6.34 (8.35)	49.20%	13.32 (7.31)
£26,000 - £31,199	77.36%	6.61 (8.49)	52.30%	12.65 (7.84)	68.18%	7.46 (8.37)	56.92%	13.33 (6.83)
£31,200 - £36,399	73.74%	6.28 (8.10)	51.70%	12.15 (7.45)	67.22%	7.25 (8.20)	58.63%	12.54 (7.06)
£36,400 - £41,599	76.49%	6.33 (7.31)	56.28%	11.26 (6.29)	69.44%	7.80 (9.44)	57.23%	13.80 (8.65)
£41,600 - £46,799	81.08%	6.68 (7.79)	56.90%	11.74 (6.87)	69.10%	6.83 (7.66)	56.54%	12.25 (6.22)
£46,800 - £51,999	85.63%	7.07 (8.31)	58.64%	12.05 (7.59)	70.25%	7.53 (9.30)	58.00%	13.15 (8.78)
≥£52,000	88.74%	6.10 (7.77)	53.62%	11.38 (7.25)	75.17%	6.29 (7.80)	53.17%	11.98 (6.91)
Parent's Highest Qualification								
Degree	92.52%	4.71 (7.03)	43.65%	10.79 (6.90)	79.97%	5.27 (7.60)	44.69%	12.02 (7.12)
A-Levels	79.18%	6.37 (7.99)	52.42%	12.15 (7.17)	67.34%	7.12 (8.26)	56.39%	12.85 (7.05)
GCSEs	70.94%	6.17 (8.54)	47.77%	12.92 (13.94)	63.09%	6.71 (8.35)	51.70%	13.27 (7.13)
No Qualifications	71.22%	3.28 (7.17)	23.51%	13.94 (8.38)	71.65%	3.70 (7.58)	28.11%	13.77 (8.67)
Index of Multiple Deprivation:								
First Quintile: Most deprived	73.13%	2.90 (6.79)	21.73%	13.35 (8.54)	73.05%	3.49 (7.03)	26.89%	13.53 (7.49)
Second Quintile	72.98%	4.37 (7.90)	31.46%	13.88 (8.14)	68.13%	5.19 (8.85)	36.28%	14.94 (8.94)
Third Quintile	75.57%	5.97 (8.44)	45.70%	13.06 (7.97)	67.93%	6.23 (8.41)	47.91%	13.30 (7.54)
Fourth Quintile	79.31%	6.66 (8.31)	54.92%	12.13 (7.71)	69.34%	6.88 (7.96)	56.53%	12.37 (6.77)
Fifth Quintile: Least deprived	84.28%	6.25 (7.12)	38.44%	10.73 (6.23)	71.15%	6.90 (7.70)	59.21%	11.81 (6.54)
Credit Constraint Proxies								
Live in owned home	80.87%	5.66 (7.83)	47.23%	11.97 (7.37)	71.20%	6.25 (8.18)	50.00%	12.74 (7.32)
Rented Accom'	69.05%	3.81 (7.61)	27.50%	13.85 (8.47)	66.55%	4.13 (7.51)	31.48%	13.74 (7.43)
Sex								
Male	73.15%	5.09 (8.34)	38.58%	13.17 (8.59)	66.67%	5.51 (8.62)	40.41%	14.01 (8.35)
Female	81.51%	5.30 (7.35)	45.49%	11.64 (6.70)	73.37%	5.99 (7.58)	50.39%	12.14 (6.46)

We do control for a full range of covariates that might be expected to influence the child's and/or parent's attitudes to the child's employment and study, the parents' attitudes to transfers, and local labour market conditions. These include housing tenure, a measure of local deprivation, the employment status and qualifications of the parents, and household income. We include household income in our models with terms for the midpoint of the reported income band, its square, and a dummy each for missing and topcoded observations.[7] Household income is not recorded in wave 5, so we substitute the wave 4 value. We discuss the bias this induces in each specification in the results section. As the survey is linked to the National Pupil Database, we can also control for prior educational performance up to age 16.

Sample descriptive statistics for selected explanatory variables are set out in Table 3. Here, we show that participation in full-time education tends to be lower among those living in deprived areas or lower income households, or those with parents from lower educational backgrounds. Participation is also substantially lower among males than females, and among those whose parents are 'credit-constrained'; defined in accordance with Dearden et al (2009), as those living in rented accommodation or social care; than those who are not.

Among those participating in full-time education, those in employment are positively selected by socio-economic background. Children from progressively higher income households have higher unconditional mean hours of work and a greater probability of working positive hours, except at the very highest income band in wave 4 (a flattening off at around 58% occurs from a lower band in wave 5). A similar pattern is observed in relation to local deprivation (in wave 4 children from more affluent areas work more, until reaching the least deprived quintile, while in wave 5 the pattern is monotonic) and parental qualifications (the tendency to work is lowest for the children of parents with no qualifications, rising for those of parents with GCSEs (age 16) and A-Level (age 18 or university entrance) qualifications in turn, but falling again for those of parents with degrees).

Those receiving EMA of £30 per week work substantially less than those receiving lower payments or none, but particularly in wave 5, this is substantially accounted for by the lower propensity to work at all, rather than a reduction in hours conditional on working. For this reason we shall present results for both the choice of hours, and the discrete choice to work positive hours.

4. Results

To estimate the effect of EMA on hours of employment we use Tobit regressions in cross-sectional and random-effects panel data specifications. We expect there to be unobserved heterogeneity in individuals' work opportunities or motivation, but individual fixed-effects cannot be conditioned out of the Tobit estimator, and implementing a fixed-effects Tobit with individual dummy variables will produce biased estimates. Therefore, we also present linear fixed- and random-effect specifications and Hausman tests for presence of this unobserved heterogeneity. Cross-sectional OLS regressions are also presented for an indication of the baseline conditional correlation between EMA receipt and the level of labour supply. In our linear specifications we treat observations with zero hours in the same way as those with positive hours. For the discrete choice to work positive hours we estimate logistic regressions with results presented as the odds ratio for working positive hours. The logit is chosen in preference to the probit because it enables the implementation of a conditional fixed-effects estimator. Our regressors of interest are dummy variables for receipt of £10, £20, or £30 payments of EMA each week. Our standard errors account for clustered sampling at the school level.

[7] Results, not shown for reasons of space, do not change if dummies for each income band are used instead, though standard errors are marginally greater with this less parsimonious approach, in which the degree of collinearity with the lower income bands and EMA receipt is higher.

We present our results in two parts, discussing firstly the results from estimates for the cross-section of children observed in their first year, and then in their second year, of post-compulsory schooling (aged 16-17, and 17-18 respectively). Secondly we present estimates exploiting changes in receipt of EMA over time within individuals due to (i) meeting the income eligibility criteria on entering the first-year of post-compulsory education, and (ii) transitioning into or out of eligibility between the first and second years of post-compulsory schooling.

4.1. Cross-sectional specifications

Estimates obtained using the cross-section of individuals in full-time education in waves 4 and 5 are shown in Table 4. The results show a significant negative correlation between receipt of the highest category of EMA (£30 per week) and both hours of employment and the probability of working positive hours, conditional on observed characteristics, and also a negative monotonic relationship between the size of EMA payments and their coefficient for labour supply (though the effect of the smaller payments is significant at, at best, only the 10% level). We note that to the extent to which unobserved motivation or labour market opportunities are negatively correlated with receipt of EMA, these downward represent biased estimates of the causal effect of EMA on child's labour supply. This exercise nevertheless provides an indication of the difference in the inference from the linear specification, which does not account for censoring at zero, and the Tobit, which does.

Table 4: Cross-sectional specifications: Marginal effects on hours worked and probability of working positive hours.

	Marginal effects on Hours Worked				Odds ratios for probability of working positive hours	
	OLS		Tobit		Logit	
EMA	Wave 4	Wave 5	Wave 4	Wave 5	Wave 4	Wave 5
£10	-0.323 (-0.421)	0.044 (0.649)	-0.285 (0.791)	-0.464 (1.174)	1.003 (0.115)	0.895 (0.142)
£20	-0.686* (0.283)	-0.865 (0.585)	-1.430* (0.838)	-1.919* (1.136)	0.830 (0.097)	0.785 (0.121)
£30	-1.267*** (0.283)	-1.508*** (0.362)	-3.096*** (0.629)	-3.201*** (0.778)	0.662*** (0.053)	0.704*** (0.066)
N	7517	4907	7517	4907	7561	5013
R^2 / Pseudo R^2	0.09	0.09	0.04	0.03	0.15	0.14

Clustered standard errors in parentheses. * *p<0.1,* ** *p<0.05,* *** *p<0.01.* **Additional controls:** Household income (measured at wave 4 in both specifications), socio-economic class, parent's highest qualification, local deprivation index, region, type of school attended, prior academic performance at age 11 and 14, quarter of birth, ethnicity, parental employment, sibling composition, lone parent, sex, free school meal eligibility.

For example, the OLS estimates show the best linear prediction is that EMA of £30 per week reduces employment by 1.3 hours per week in wave 4, and 1.5 hours in wave 5. The interpretation of the Tobit estimates, on the other hand, is that EMA of £30 per week reduces desired labour supply by just over 3 hours per week on average (this figure accounts for individuals who reach zero hours but might still prefer more leisure and less consumption). The OLS coefficients are smaller in absolute value because observations of zero hours are treated in the same way as positive hours, meaning the observed effect of EMA is capped by the fact that hours worked cannot fall below zero. That EMA has a significant negative association with employment at the extensive margin is clear from the logit model. Receipt of £30 per week EMA reduces the odds of participation by slightly more than 30%. The wave 4 and wave 5 odds ratios of 0.662 and 0.704 are equivalent to marginal effects on the probability of participation of -9.7 and -8.4 percentage points from a base of 43%.

We note that, since we are controlling for wave 4 income in the wave 5 models (household income not being collected in wave 5), receipt of EMA in wave 5 will be negatively correlated with the measurement error in income. Households receiving EMA will on average have a lower income than accounted for by our observed covariates, meaning lower parental transfers and higher child employment. We expect this to positively bias our wave 5 results, towards a coefficient of zero or odds ratio of one. Nevertheless, the difference in coefficients between waves is never statistically significant.

4.2. Panel data specifications

We now discuss estimates for the labour supply effect of EMA in fixed- and random-effect panel data specifications. The purpose of controlling for individual fixed effects is to eliminate the effect of time invariant unobserved individual differences in motivation, ability, household resources or labour market opportunities, and so, provided there are no time-varying unobserved differences of this nature correlated with receipt of EMA within-person, to produce consistent estimates of the effect of EMA on labour supply. The random effects specifications are inconsistent in the presence of time-invariant unobserved effects correlated with EMA, but in their absence, constitute the more efficient estimator. We conduct Hausman tests for the presence of time-invariant unobserved effects, in order to select our preferred model.

These estimates are presented for the balanced panel of individuals observed in full-time education over two alternative time periods. Estimates of the labour supply effect of EMA are first shown for waves 3-4, the final year of compulsory and first year of post-compulsory schooling. No-one was eligible for EMA during compulsory schooling, so here the fixed-effect estimates are identified by transitions into receipt of EMA for those meeting the income criteria compared with those not meeting the income criteria on entering post-compulsory education. We next show results for waves 4-5, the first two years of post-compulsory schooling, and during both of which EMA was available to all those meeting the income criteria. Here the fixed-effect estimates are identified by within-person transitions in receipt of EMA between the two waves. Here, however, because no data on household income is available in wave 5, there is likely to be an unobserved, time varying, reduction in household resources associated with a transition into receipt of EMA. This violates the identifying assumption of fixed-effects regression. Since a reduction in household resources will, other things equal, reduce parental transfers and increase labour supply, we expect this specification to produce positively biased coefficients for the labour supply effect of EMA.

We might also expect differences in the source of identification of these specifications to produce different results. In the wave 3-4 specification all of those who receive EMA in wave 4 undergo a transition, while in the wave 4-5 specification, only those who subsequently lose their EMA, or gain EMA for the first time, in wave 5 undergo a transition. This latter group is likely to come from more affluent households, with incomes closer to the eligibility threshold on average. On a related note, we also show that the standard errors in the fixed-effect specifications for waves 4-5 are approximately twice the size of those estimated for waves 3-4, due to the smaller number of transitions in receipt of EMA contributing to identification. [8]

The results for the key coefficients on receipt of EMA from the models on both time periods are shown in Table 5. (The coefficients on the complete set of explanatory variables for all the specifications shown here are presented in Appendix Table A1). We may generally state from Table 5 that the monotonic relationship between the size of EMA payments and their coefficient in determining labour supply seen for the cross-sectional results is borne out in all the specifications shown in table 5, but that in all but one case (the random-effects linear regression for waves 4-5) the estimated effect of the highest payment of EMA is larger than seen in the cross-sectional estimates (reductions in hours between 1.7 and 2 per week, and odds of participation between 40% and 50%, equivalent to between 12.1 and 15.5 percentage points).

Focusing on the linear fixed-effect specifications, we estimate from our wave 3-4 specification that a transition into receipt of £30 EMA on entering post-compulsory schooling reduces labour supply by 1.9 hours per week: and from our wave 4-5 specification that a transition into EMA receipt during post-compulsory schooling reduces labour supply by 1.7 hours per week. The wave 4-5 coefficient is positively biased (towards zero) due to the omission of time varying household income from this specification, but in this case the Hausman test does not reject equality of the random and fixed-effects coefficient vectors, so we interpret the random effects coefficient here of -1.3 hours as a more efficient estimate of this upper bound.

Turning now to the participation decision at the extensive margin, the conditional fixed effects logit model produces consistent estimates for the wave 3-4 specification but (as with the wave 4-5 specification), the Hausman test rejects consistency of the more efficient random-effects logit. Our interpretation is therefore that receipt of £30 EMA reduces the odds of participation in employment by 43%, or the probability by approximately 13 percentage points from the wave 4 level. Although we expect the omission of household income in the wave 4-5 model to produce positively biased estimates the odds ratio estimated for this period is only marginally closer to one, and not statistically or economically different.

Although for waves 4-5, the Hausman test cannot reject consistency of the linear random effects specification, the Hausman test does reject consistency of the random effects logit on participation. As a result, we are reluctant to make any claim about the consistency of the random-effects Tobit estimator in this context. Nevertheless, we note that the interpretation in the wave 4-5 specification, that receipt of EMA of £30 per week reduces desired labour supply by just over three hours per week, is identical to that obtained in both waves' cross-sectional estimates, and larger in absolute value than its linear counterpart in the same proportion as for the cross-sectional estimates. The wave 3-4 estimate is somewhat smaller in absolute value (albeit both estimates have large standard errors and are not significantly different), with £30 per week EMA reducing desired labour supply by just 2.4 hours per week.

[8] We also estimated on all three waves, and obtained results not statistically different from either specification presented here. We focus on our two wave models for clarity about the source of identification: in the three-wave model, the fixed-effect estimates are identified by changes in EMA receipt both due to meeting the income criteria on reaching the eligible age-group, and movements into and out of eligibility among those already old enough. As with the wave 4-5 specification, the 3-5 estimates are also biased by the absence of wave 5 income requiring us to treat income as time invariant.

Table 5: Panel data effects on hours worked and probability of working positive hours.

	Marginal effects on Hours Worked			Odds ratios for probability of working positive hours	
	Linear regression		Tobit	Logit	
	Random-effects	Fixed-effects	Random-effects	Random-effects	Conditional fixed-effects
Waves 3-4					
EMA £10	-0.088 (0.408)	0.307 (0.415)	0.262 (0.742)	1.054 (0.145)	0.950 (0.216)
EMA £20	-0.666* (0.395)	-0.774 (0.431)	-1.125 (0.729)	0.767** (0.102)	0.608** (0.124)
EMA £30	-1.781*** (0.208)	-1.869*** (0.203)	-2.358*** (0.408)	0.592*** (0.046)	0.574*** (0.062)
Hausman test χ^2 (*p*-value)	37.64 (0.0003)		.	26.75 (0.0134)	
R^2 | Log (pseudo)likelihood	0.1656 overall	0.2187 within	-25152.63	-8196.90	-1230.12
N	7150	7150	7150	8046	2387
Waves 4-5					
EMA £10	-0.355 (0.395)	-0.894 (0.621)	-1.035 (0.770)	0.849 (0.131)	0.902 (0.289)
EMA £20	-0.791** (0.369)	-0.957* (0.565)	-1.780** (0.775)	0.731** (0.112)	0.871 (0.313)
EMA £30	-1.311*** (0.245)	-1.713*** (0.476)	-3.219*** (0.531)	0.504*** (0.051)	0.560** (0.154)
Hausman test χ^2 (*p*-value)	9.91 (0.3579)		.	53.85 (0.0000)	
R^2 | Log (pseudo)likelihood	0.0979 overall	0.0388 within	-21983.71	-5969.02	-679.11
N	4608	4608	4608	4745	1074

Clustered standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Hausman test conducted assuming no clusters. Conditional fixed-effect logit standard errors not cluster robust. Sample size for conditional fixed effects logit is number of individuals transitioning into or out of employment. R^2 is 'overall R^2' for random-effects regression, and 'within R^2' for fixed-effects regression. Log likelihood reported for conditional fixed-effects logit, and log-pseudolikelihood for the random effects specifications. **Additional controls**: *Time-varying:* Household income (wave 3-4 specification only), type of school attended, parental employment, lone parent. *Time invariant:* Household income (not observed in wave 5, so held constant at wave 4 value in wave 4-5 specification), socio-economic class, parent's highest qualification, local deprivation index, region, prior academic performance at age 11 and 14, quarter of birth, ethnicity, , sibling composition, sex, free school meal eligibility.

Differences between our wave 3-4 and wave 4-5 results may also be due to heterogeneous effects driven by the composition of the treated group (those affected by changes in EMA receipt *during* post-compulsory education will be more affluent on average than those gaining EMA on *entry* into post-compulsory education), by the age of the cohort (parents' willingness to make transfers to their children, and children's relative valuation of consumption and leisure may change with increasing expectations of independence), and by the composition of the sample (those still observed in education in wave 5 will be more positively selected with respect to educational aspirations and expectations). Nevertheless, we point to the stability of the coefficients obtained with each estimator across the cross-sectional and panel data settings. From the linear and logit specifications, we argue that a representative range for the net labour supply effect of £30 per week EMA is an 8-13% percentage point reduction in the probability of working positive hours and a 1.25 to 1.9 reduction in actual hours worked per week, and that the reduction in *desired* hours is larger, and though not bounded, in the order of magnitude of 2.4 to 3.2 hours per week.

4.3. Inference on parental altruism

The negative labour supply response we observe here, which is significantly different from zero in all specifications, is sufficient to reject that $\lambda = 1$, representing 'full insurance' by parents. However, these estimates do not recover structural parameters regarding the magnitude of the parental response. For inference towards these, we appeal to results elsewhere in the literature. For the UK in 1974, Dustmann et al (2009) indicate that 16-year-olds work 0.307 hours less each week for each additional £1 transferred from parents. For the US in 1997, Wulff Pabilonia (2001), indicates that the earnings of 16-year-olds fell by \$0.654 per \$1 of parental transfer. If children in the UK reduce their earnings by the same proportion per pound of parental transfer, then at the median wage of those working positive hours in our estimation sample (£4.77) this equates to children working 0.137 hours per week less for every pound received in additional transfers.

The ratio of the labour supply response to EMA (taking into account the reduction in transfers made by the parents) to either estimate of the child's labour supply response to *all* unearned income gives an estimate for the net increase in the child's unearned income, or equivalently, the amount of his EMA which the child is permitted to 'keep'. In their estimates, both Dustmann et al (2009) and Wulff Pabilonia (2001) account for the censoring of hours worked at zero, so using this strategy it is the *desired* reduction in labour supply, rather than linear prediction, which enables identification of the parameter of interest, λ.

Our Tobit results suggest that, on receiving an EMA payment of £30 per week, a teenager would, on average, like to reduce his labour supply by between 2.4 and 3.2 hours. Assuming that the child treats cash from parents as a perfect substitute for cash from the state, and his labour supply response to unearned income is equal to -0.307 hours per pound per week (as in Dustmann et al, 2009), this implies a net increase in unearned income of between £7.82 and £10.42. Repeating this exercise using the income-responsiveness of 0.137 hours per pound per week obtained from Wulff Pabilonia (2001) give a range of £17.52 to £23.36. Alternatively stated, in response to a weekly EMA payment of £30, the parent withdraws cash transfers, extracts cash contributions or compels the child directly to purchase goods previously provided in kind, to a combined value at least £6.64 (in the theoretical framework set out here implying $\lambda = 0.22$) but less than £22.18 ($\lambda = 0.74$) respectively. We acknowledge that, without being able to condition on individual fixed-effects, the direction of the bias in any individual Tobit estimate is indeterminate. However, the overall bias must be smaller than that from the linear random-effects specification, which is affected by the same omitted variables bias but additionally that caused by failure to account for censoring at zero, which operates in the same direction. Therefore, repeating this exercise using the coefficient obtained in the wave 4 OLS specification (-1.267 hours, the smallest in absolute value), we place a lower bound on the net gain in unearned income of £4.13, giving $\lambda = 0.86$.

The condition for parental behaviour to be consistent with an effective altruist redistributing income to maximise household welfare is $\lambda = (1 - \theta)$, where θ is the parent's marginal propensity to transfer cash to the child out of their own income. Dustmann et al (2009) estimate $\theta = 0.005$, and at the mean parental income for each sample subgroup, Kalenkoski and Wulff Pabilonia (2010) estimate $\theta = 0.015$ for two-year and 0.032 for four-year college students. These figures correspond to $(1-\theta)$ = 0.995, 0.985, and 0.968 respectively. The estimates of λ calculated above are all considerably smaller than this. Though ours is a rough calculation using parameters obtained from different institutional backgrounds, the net effect of EMA has clearly been to raise the child's unearned income by substantially more than had the equivalent transfer been made by parents. Thus, we reject both the 'full insurance' and 'effective altruist' hypotheses.

4.4. Robustness checks

We now show that this conclusion is robust to a series of sensitivity checks. Results for a series of sub-groups of the population; by household credit constraint, time of interview, and gender; are shown in Table 6, estimated using eight of the specifications discussed above. We do not show the wave 5 cross-sectional estimates, which are likely to be most biased by measurement error in household income, or the random effects linear and logit specifications, showing instead their (conditional-) fixed-effect counterparts. Results for the whole estimation sample, but with subsets of control variables omitted to reduce multicollinearity among the covariate vector, are shown in Table 7, again for eight specifications. There we omit the fixed-effect estimators (since the regressors we omit, all time invariant, have no bearing on the coefficients obtained) and their random-effect counterparts, but show the random-effects Tobit results.

4.4.1. *Non-credit-constrained sub-group*

Following the reasoning of Dearden et al (2009), EMA is less likely to be a binding consideration in the child's education participation decision for the children of non-credit-constrained parents, here defined as those living in owner-occupied accommodation. Any bias due to endogenous selection into post-compulsory education should not be present for this reduced sub-sample. The marginal effects of interest for this group are presented in the top section of Table 6.

For the non-credit-constrained group, like the whole sample estimates, across the first seven specifications the magnitude of the labour supply effect is greater for EMA payments of £30 than of £20 and in turn £10, with only the estimates for £30 being persistently significant at the 5% or 1% levels. (In common with all the sub-samples shown in Table 6, there are very few transitions into and out of employment between waves 4 and 5, producing very imprecise and in some case incorrectly directioned, though never statistically significant, estimates of the odds ratio in the wave 4-5 conditional fixed effect logit estimates. We do not discuss these further).

The Tobit estimates show the £30 payment to reduce the desired labour supply of children in non-credit-constrained households by between 22 and 48 minutes more per week than estimated for the overall population, though the difference in coefficients is not statistically significant. The wave 4-5 linear specification produces a small and insignificant difference in the opposite direction, while differences in the remaining coefficients are trivial.

This finding adds robustness to our rejection of the effective altruist model. Parents in credit constrained households are more likely than those from non-credit-constrained households to lack the ability to redistribute resources to maximise household welfare but the balance of the estimates shown here suggests that altruistic redistributions are the same size or smaller in magnitude for the group most able to make them.

Table 6: Sensitivity of key coefficients and odds ratios to estimation by sub-group.

	Wave 4 cross-sect		Waves 3-4 panel			Waves 4-5 panel		
	Tobit	Logit	Linear FE	Tobit RE	CFE Logit	Linear FE	Tobit RE	CFE Logit
N (main specs)	7516	7561	7150	7150	2387	4608	4608	1074
Non-credit constrained								
EMA £10	-0.670 (0.818)	0.933 (0.112)	0.260 (0.454)	-0.104 (0.737)	0.847 (0.200)	-0.305 (0.642)	-1.089 (0.771)	1.066 (0.376)
EMA £20	-1.477* (0.826)	0.811* (0.103)	-0.649 (0.440)	-0.967 (0.736)	0.620** (0.137)	-0.359 (0.651)	-1.890** (0.794)	1.147 (0.895)
EMA £30	-3.899*** (0.689)	0.589*** (0.056)	-1.959*** (0.251)	-2.727*** (0.460)	0.617*** (0.083)	-1.336** (0.629)	-3.943*** (0.590)	0.895 (0.275)
N	5777	5807	5516	5516	1946	3661	3661	862
Term-time only								
EMA £10	-0.127 (0.867)	1.045 (0.136)	0.507 (0.477)	0.427 (0.788)	1.154 (0.317)	-0.377 (0.573)	-0.933 (0.781)	0.855 (0.480)
EMA £20	-1.175 (0.945)	0.875 (0.121)	-0.440 (0.462)	-0.631 (0.798)	0.900 (0.240)	-0.355 (0.700)	-1.677** (0.794)	1.286 (0.751)
EMA £30	-2.980 (0.707)	0.683*** (0.064)	-1.804*** (0.217)	-2.185*** (0.427)	0.583*** (0.072)	-1.251* (0.657)	-3.230*** (0.558)	0.890 (0.433)
N	5836	5866	5533	5533	1800	2255	2255	488
Male								
EMA £10	-1.247 (1.426)	0.893 (0.154)	-0.353 (0.635)	-0.957 (1.303)	0.860 (0.303)	-0.868 (0.859)	-2.049 (1.419)	0.490 (0.261)
EMA £20	-1.179 (1.517)	0.835 (0.144)	-0.511 (0.292)	-0.840 (1.249)	0.531** (0.159)	-1.517 (0.969)	-2.429* (1.459)	0.507 (0.262)
EMA £30	-2.953*** (1.059)	0.676*** (0.078)	-1.511*** (0.292)	-2.379*** (0.670)	0.556*** (0.086)	-2.113*** (0.699)	-3.381*** (0.954)	0.586 (0.210)
N	3570	3594			1060			504
Female								
EMA £10	0.226 (0.944)	1.100 (0.193)	0.773 (0.546)	0.998 (0.867)	0.983 (0.299)	-0.931 (0.868)	-0.474 (0.858)	1.449 (0.620)
EMA £20	-1.705* (0.948)	0.808 (0.134)	-1.123** (0.523)	-1.395 (0.861)	0.647 (0.186)	-0.536 (0.659)	-1.477* (0.853)	1.556 (0.794)
EMA £30	-3.329*** (0.748)	0.633*** (0.072)	-2.200*** (0.270)	-2.360*** (0.502)	0.585*** (0.091)	-1.377** (0.623)	-3.241*** (0.606)	0.625 (0.235)
N	3947	3967	3393	3393	1327	2106	2106	570

Clustered standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Hausman test conducted assuming no clusters. Conditional fixed-effect logit standard errors not cluster robust. **Additional controls:** Time-varying: Household income (wave 3-4 specification only), type of school attended, parental employment, lone parent. Time invariant: Household income (not observed in wave 5, so held constant at wave 4 value in wave 4-5 specification), socio-economic class, parent's highest qualification, local deprivation index, region, prior academic performance at age 11 and 14, quarter of birth, ethnicity, , sibling composition, sex, free school meal eligibility.

4.4.2. *Term-time interviews*

Some interviews took place in the school holidays, when EMA is not paid. The survey question about receipt of EMA asks "Do you get Education Maintenance Allowance?" In the main results shown here we assumed that interviewees respond according to what they receive during term time. We also assumed that the survey question regarding employment, emphasising hours "usually" worked, is interpreted to refer to term time, except in wave 3, where this qualification is explicitly stated in the question. Except for the appreciable reduction in the sample size and resulting loss of precision, the results for those interviewed outside August in wave 4 and July-September in wave 5, demonstrate no statistically significant changes compared with the whole population sample. In most cases the size of the change is also economically trivial, with the exception that in the wave 4-5 linear FE specification the £30 payment reduces labour supply by around 30 minutes per week less.

4.4.3. *Gender differences*

Distinct coefficients by gender could result from a greater responsiveness of labour supply to unearned income among female teenagers than males, but could also be due one of the following explanations. Firstly, a larger proportion of males than females may be induced by EMA to participate in post-compulsory education (Dearden et al, 2009, p.830), so other things equal, this selection bias will be stronger among males than females. Secondly, the partial correlation of EMA take-up with unobservable characteristics determining labour supply may be stronger for one gender than the other. Thirdly may it be the case that parents let daughters 'keep' a different proportion of unearned EMA income than their sons. Nevertheless, the third and fourth sections of Table 6 show no appreciable differences in the labour supply effect of the highest EMA payment, with the exception of the linear fixed effect estimates. The coefficient is considerably more negative for girls in waves 3-4 (equivalent to 41 minutes greater reduction, and marginally significant), with the positions reversed at waves 4-5, though such is the imprecision of these later estimates that the gap is no longer significant.

4.4.4. *Sensitivity to covariate vector*

We acknowledge the potential for the correlation between EMA receipt and several of the dummy variable covariate sets used in estimation to induce a problem of multicollinearity, inflating the variance of the estimates for the effect of EMA on labour supply. Table 7 shows results obtained when, in turn, parent's socio-economic classification, parent's educational qualifications, and local area deprivation, are omitted from the vector of covariates.

There are no statistically or economically significant changes in the conclusions drawn from these specifications, compared with the complete covariate set. However, very tentatively, it can be seen that across the specifications, the direction of the bias is negative when the dummies for 'higher' deprivation than the omitted category are excluded (these being positively correlated with EMA and negatively determining labour market opportunities), and is positive when dummies for 'higher' socio-economic status and parental qualifications are omitted (these being negatively correlated with EMA and, we would expect, positively determining the child's employability). Therefore, it is likely that these new results reflect changes due to the expected bias, rather than providing evidence for collinearity having an impact on coefficients in the main specification. As a final robustness check, we additionally show results obtained when all three of these variables are omitted. From the final section of Table 7 it is clear that the results again remain stable, and that the improvement in precision is minimal.

Table 7: Sensitivity of key coefficients and odds ratios to coefficient vector

	Marginal effects on Hours Worked						Odds ratios for probability of working positive hours	
	OLS		Tobit				Logit	
	Wave 4	Wave 5	Wave 4	Wave 5	RE w' 3-4	RE w' 4-5	Wave 4	Wave 5
N (main specs)	7517	4907	7517	4907	7150	4608	7561	5013
Drop parents' socio-economic classification								
EMA £10	-0.279 (0.423)	0.106 (0.652)	-0.169 (0.797)	-0.342 (1.108)	0.320 (0.742)	-0.931 (0.769)	1.019 (0.117)	0.908 (0.145)
EMA £20	-0.620 (0.419)	-0.756 (0.584)	-1.249 (0.844)	-1.702 (1.135)	-1.042 (0.729)	-1.606** (0.775)	0.850 (0.100)	0.811 (0.125)
EMA £30	-1.185*** (0.278)	-1.396 (0.363)	-2.911*** (0.620)	-2.975*** (0.785)	-2.263*** (0.407)	-3.025*** (0.529)	0.678*** (0.054)	0.725*** (0.068)
Drop parents' educational qualifications								
EMA £10	-0.271 (0.420)	0.025 (0.653)	-0.179 (0.788)	-0.473 (1.181)	0.321 (0.743)	-1.002 (0.770)	1.022 (0.116)	0.896 (0.141)
EMA £20	-0.646 (0.414)	-0.829 (0.585)	-1.364 (0.735)	-1.830 (1.139)	-1.089 (0.730)	-1.695** (0.777)	0.841 (0.097)	0.795 (0.122)
EMA £30	-1.202*** (0.282)	-1.479*** (0.358)	-2.990*** (0.630)	-3.145*** (0.768)	-2.316*** (0.408)	-3.146*** (0.532)	0.674*** (0.054)	0.716*** (0.067)
Drop local area deprivation								
EMA £10	-0.337 (0.422)	-0.004 (0.647)	-0.440 (0.794)	-0.679 (1.170)	0.139 (0.742)	-1.128 (0.768)	0.962 (0.109)	0.858 (0.136)
EMA £20	-0.699* (0.412)	-0.928 (0.586)	-1.573* (0.830)	-2.102* (1.140)	-1.283* (0.727)	-1.863** (0.773)	0.803* (0.093)	0.758* (0.118)
EMA £30	-1.341*** (0.281)	-1.628*** (0.360)	-3.368*** (0.628)	-3.546 (0.779)	-2.540*** (0.407)	-3.432 (0.529)	0.630*** (0.050)	0.663*** (0.062)
Drop all three of above regressors								
EMA £10	-0.180 (0.421)	0.089 (0.654)	-1.128 (0.794)	-0.476 (1.185)	0.282 (0.742)	-0.917 (0.771)	1.006 (0.113)	0.885 (0.138)
EMA £20	-0.539 (0.412)	-0.713 (0.583)	-1.244 (0.833)	-1.672 (1.142)	-1.143 (0.729)	-1.503* (0.775)	0.839 (0.096)	0.808 (0.124)
EMA £30	-1.123*** (0.274)	-1.396*** (0.355)	-2.976*** (0.618)	-3.095*** (0.772)	-2.393*** (0.406)	-3.048*** (0.526)	0.663*** (0.051)	0.710*** (0.065)

Clustered standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. **Additional controls:** Time-varying: Household income (wave 3-4 specification only), type of school attended, parental employment, lone parent. Time invariant: Household income (not observed in wave 5, so held constant at wave 4 value in wave 4-5 specification), socio-economic class, parent's highest qualification, local deprivation index, region, prior academic performance at age 11 and 14, quarter of birth, ethnicity, , sibling composition, sex, free school meal eligibility.

5. Discussion and Conclusions

We have shown that an EMA cash transfer of £30 per week causes a statistically and economically significant reduction in the labour supply of teenagers in full-time education at both the intensive and extensive margin. The effects of £10 and £20 payments are smaller in magnitude and less precisely estimated. This labour supply response is one mechanism by which EMA is likely to have improved educational and labour market outcomes for recipients, especially among those working the longest hours. Although later waves of the LSYPE collect retrospective data on educational performance at age 17 and 18, and allow us to observe progression into Higher Education and the labour market, we do not evaluate the magnitude of this effect here. There are other mechanisms by which EMA may have a direct effect on performance, such as through raising individuals' educational expectations and aspirations, or self-esteem of individuals with a greater independent resource, which mean EMA is unlikely to be a valid instrumental variable. The endogenous selection into post-compulsory schooling induced by EMA would also represent a significant challenge to causal inference.

Instead, the focus of this paper has been to use the labour supply effect of EMA for inference regarding the altruistic behaviour of parents. We developed a theoretical model in which parents specify a transfer rule contingent on the child's labour supply, which children take as given when choosing their utility maximising hours of work. In this framework, EMA acts as an exogenous income shock received by the child as a cash transfer from the state. This contrasts with most existing empirical applications of Becker's (1974, 1981) 'effectively altruistic head of household' model, which consider the effects of in-kind transfers to children or hypothecated cash payments to parents. Though data deficiencies prevent structural identification of this model, our theoretical model shows the overall labour supply effect of EMA to depend on the degree to which parents redistribute household resources in response to EMA.

The results obtained here reject the hypotheses that parents are 'effective altruists' or provide 'full insurance' for their child's consumption, but do suggest that for every pound the child receives from the state, the parent withdraws between £0.22 and £0.86 from the child. Although the child's welfare gain is greater than were the equivalent transfer to be made to parents, particularly towards the upper bound, the results do imply that a substantial proportion of the government's outlay is appropriated by the parents, rather than their children. With this in mind, if endogenous selection can adequately be addressed, we may follow the work of Ebens et al (2011) for the Netherlands in identifying the extent to which grants supporting students from low-income backgrounds participating in Higher Education crowd out parental support. This will enable a fruitful contribution to be made in evaluating the efficiency of 'widening participation initiatives' being implemented alongside the recent rise in university tuition fees in the UK. (The extent to which parents react differently to grants substituting for loans than to grants with no substitute in the same time period, may also provide lessons for improving the targeting of public transfers).

Our inference here relies on reasonable assumptions about the responsiveness of in-school labour supply to unearned income or resource endowments. Data pertaining to the cash and in-kind transfers made by parents to children receiving EMA would be required to identify the structural parameters and make inference regarding the magnitude of the parental response to EMA with greater robustness.

Teenagers in post-compulsory full-time education represent a unique component of the family for whom existing theories of parental altruism or provision are clearly insufficient. Exploration of the bargaining process undertaken by parents and teenagers in this situation would certainly be merited. It would also be interesting to learn whether this dynamic is affected by the current extension of compulsory education or training to the age of 18 in the UK. Data on a second cohort of young people in England ('LSYPE2' or 'Our Futures') is currently being collected (they will reach post-compulsory education in 2015-16). This will provide an excellent resource to pursue both these questions.

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Appendix: Complete Estimation Output

Table A1: Tobit coefficients and clustered standard errors for full list of covariates in main specifications

	Marginal effects on Hours Worked						Odds ratios for probability of working positive hours			
	Waves 3-4			Waves 4-5			Waves 3-4		Waves 4-5	
	Linear		Tobit	Linear		Tobit	Logit		Logit	
	RE+	FE	RE	RE	FE	RE	RE	CFE	RE	CFE
EMA (omitted: Zero)										
£10 p.w.	-0.088 (0.409)	0.307 (0.415)	0.262 (0.742)	-0.355 (0.394)	-0.894 (0.957)	-1.034 (0.768)	1.054 (0.145)	0.950 (0.217)	0.849 (0.131)	0.903 (0.290)
£20 p.w.	-0.666* (0.395)	-0.774* (0.431)	-1.125 (0.729)	-0.791** (0.369)	-0.957* (0.565)	-1.780** (0.775)	0.768** (0.102)	0.607** (0.124)	0.731** (0.112)	0.871 (0.313)
£30 p.w.	-1.781*** (0.208)	-1.869*** (0.203)	-2.358*** (0.408)	-1.311*** (0.244)	-1.713*** (0.476)	-3.219*** (0.531)	0.592*** (0.046)	0.574*** (0.062)	0.504*** (0.052)	0.560** (0.154)
Income (£000s)#	0.022 (0.021)	-0.042 (0.036)	0.081 (0.056)	0.099*** (0.033)	.	0.254*** (0.081)	1.010 (0.011)	0.989 (0.019)	1.038** (0.016)	.
[Income (£000s)]^2 /1000	-0.079 (0.376)	0.875 (0.662)	-0.448 (0.959)	-1.032* (0.628)	.	-3.047** (0.001)	0.998 (0.181)	1.285 (0.339)	0.667 (0.269)	.
Income ≥ £52,000	-0.139 (0.304)	0.327 (0.532)	-0.621 (0.712)	-0.075 (0.515)	.	0.337 (1.074)	0.817 (0.101)	0.999 (0.251)	0.864 (0.174)	.
Income missing	0.505 (0.270)	0.365 (0.465)	1.914** (0.781)	1.633*** (0.420)	.	4.064*** (1.115)	1.269 (0.195)	1.084 (0.289)	1.974*** (0.437)	.
School-type (omitted: Further education college/other)										
State	-1.121*** (0.299)	-1.645*** (0.326)	-2.185*** (0.690)	-0.568*** (0.189)	-0.142 (0.230)	-1.170*** (0.395)	0.797 (0.112)	0.807 (0.164)	0.872 (0.077)	0.945 (0.118)
Private	0.160 (1.190)	1.198* (1.498)	1.363 (1.852)	-1.740* (0.885)	-0.876 (1.493)	-5.258*** (1.860)	0.934 (0.623)	2.916** (1.593)	0.205*** (0.083)	0.586 (0.325)
Sixth-form college	-0.108*** (0.249)	-0.113 (0.268)	0.726 (0.467)	0.449* (0.234)	0.277 (0.396)	1.237** (0.505)	1.391*** (0.129)	1.679*** (0.262)	1.607*** (0.203)	1.531 (0.399)
No parent in employment	-0.336* (0.198)	-0.115 (0.464)	-1.740*** (0.571)	-0.814*** (0.268)	-0.488 (0.509)	-2.627*** (0.649)	0.696*** (0.078)	0.597** (0.131)	0.564*** (0.075)	1.013 (0.257)
Lone parent	0.340** (0.150)	0.310 (0.454)	0.670* (0.378)	0.478** (0.230)	-0.583 (0.562)	1.115** (0.503)	1.046 (0.075)	1.047 (0.225)	1.087 (0.107)	0.659 (0.229)
Wave 4/5@	3.559*** (0.297)	3.106*** (0.322)	7.937*** (0.711)	1.468** (0.718)	1.553*** (0.160)	3.117*** (0.277)	3.349*** (0.492)	3.379 (0.682)	1.936*** (0.115)	2.029*** (0.162)
Parent's Socio-economic Classification (omitted: Never worked/Long-term unemployed)										
Higher professional	-0.277 (0.280)	.	1.110 (0.976)	-1.392*** (0.457)	.	-2.515** (1.212)	1.148 (0.241)	.	0.609* (0.160)	.
Low prof' / High supervisory	0.387 (0.240)	.	2.733*** (0.915)	-0.053 (0.399)	.	0.530 (1.124)	1.513** (0.297)	.	1.312 (0.322)	.
Intermediate	0.387 (0.273)	.	2.645*** (0.981)	0.174 (0.471)	.	1.130 (1.219)	1.493* (0.323)	.	1.444 (0.393)	.
Low supervisory / technical	0.475* (0.263)	.	2.765*** (0.950)	0.049 (0.452)	.	0.476 (1.182)	1.510** (0.306)	.	1.101 (0.286)	.
Routine / semi-routine	0.358* (0.263)	.	2.516*** (0.866)	0.070 (0.368)	.	0.781 (1.054)	1.427* (0.279)	.	1.312 (0.321)	.
Small employer / own account	0.883*** (0.250)	.	3.919*** (0.939)	0.318 (0.423)	.	1.194 (1.164)	1.937*** (0.387)	.	1.344 (0.341)	.
Parent's Highest qualification (omitted: No qualifications)										
Degree	-0.693*** (0.189)	.	-1.492*** (0.559)	-1.153*** (0.350)	.	-2.347*** (0.763)	0.776*** (0.077)	.	0.497*** (0.070)	.
A-Levels	0.318** (0.158)	.	0.973** (0.467)	0.480 (0.295)	.	1.569** (0.649)	1.192** (0.096)	.	1.423*** (0.165)	.
GCSEs	0.418** (0.173)	.	1.243*** (0.465)	0.700** (0.313)	.	2.018*** (0.648)	1.271*** (0.105)	.	1.567*** (0.188)	.
N	7150	7150	7150	4608	4608	4608	8046	2387	4745	1074

+ RE: Random-effects. FE: Fixed-effects. CFE: Conditional fixed-effects # Midpoints of bands up to £52,000. @ Dummy for later wave in specification. * p<0.1, ** p<0.05, *** p<0.01. Clustered standard errors in parentheses (except conditional fixed-effect logit).

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	Marginal effects on Hours Worked						Odds ratios for probability of working positive hours			
	Waves 3-4			Waves 4-5			Waves 3-4		Waves 4-5	
	Linear		Tobit	Linear		Tobit	Logit		Logit	
	RE[+]	FE	RE	RE	FE	RE	RE	CFE	RE	CFE
Index of Multiple Deprivation: Quintiles (Omitted: Fifth/Least deprived)										
First (most deprived)	-0.678*** (0.213)	.	-3.894*** (0.576)	-0.593 (0.372)	.	-3.136*** (0.779)	0.385*** (0.046)	.	0.324*** (0.061)	.
Second	-0.327* (0.195)	.	-2.212*** (0.504)	0.231 (0.353)	.	-0.858 (0.694)	0.521*** (0.051)	.	0.499*** (0.077)	.
Third	0.123 (0.179)	.	-0.524 (0.432)	0.385 (0.289)	.	-0.061 (0.598)	0.739*** (0.062)	.	0.673*** (0.084)	.
Fourth	0.291* (0.175)	.	0.094 (0.406)	0.572** (0.276)	.	06701 (0.562)	0.883 (0.068)	.	0.932 (0.103)	.
Region (Omitted: London)										
North East	0.402 (0.275)	.	1.612** (0.792)	0.517 (0.579)	.	1.909* (1.080)	1.221 (0.241)		1.444 (0.572)	.
North West	0.845*** (0.236)	.	3.262*** (0.582)	1.239*** (0.359)	.	3.954*** (0.777)	1.797*** (0.269)	.	2.591*** (0.734)	.
Yorkshire/Humber	1.442*** (0.226)	.	4.628*** (0.609)	1.855*** (0.390)	.	5.015*** (0.811)	2.267*** (0.344)	.	2.966*** (0.916)	.
East Midlands	1.188*** (0.271)	.	4.388*** (0.629)	1.490*** (0.387)	.	4.675*** (0.851)	2.283*** (0.397)	.	3.241*** (1.120)	.
West Midlands	0.584*** (0.190)	.	2.351*** (0.583)	0.879*** (0.333)	.	2.745*** (0.851)	1.448** (0.220)	.	1.899** (0.558)	.
East	1.387*** (0.240)	.	4.938*** (0.601)	2.156*** (0.395)	.	5.923*** (0.812)	2.557*** (0.510)	.	4.365*** (1.645)	.
South East	1.096*** (0.202)	.	4.279*** (0.571)	1.955*** (0.513)	.	5.741*** (0.762)	2.315*** (0.387)	.	4.459*** (1.120)	.
South West	1.647***	.	5.582*** (0.641)	2.920*** (0.514)	.	7.623*** (0.875)	3.030*** (0.509)	.	7.750*** (3.682)	.
Age 11 std'ized ed' performance	0.400*** (0.119)	.	1.295*** (0.346)	0.191 (0.148)	.	1.014*** (0.304)	1.257*** (0.076)	.	1.379*** (0.090)	.
Age 14/16 std'ized ed' perf'[@]	-0.353*** (0.126)	.	-0.679* (0.360)	0.047 (0.161)	.	0.426 (0.326)	0.986 (0.063)	.	1.247*** (0.088)	.
Quarter of Birth (Omitted: Youngest, June-August)										
Oldest: Sep - Nov	0.761*** (0.126)	.	1.924*** (0.329)	0.720*** (0.198)	.	1384*** (0.452)	1.367*** (0.078)	.	1.320*** (0.114)	.
Dec - Feb	0.234* (0.125)	.	0.496 (0.346)	0.314*** (0.216)	.	0.525 (0.477)	1.074 (0.063)	.	1.117 (0.095)	.
Mar- May	0.246* (0.127)	.	0.618* (0.371)	0.414* (0.215)	.	0.742 (0.500)	1.102 (0.068)	.	1.256** (0.120)	.
Ethnicity (Omitted: White)										
Indian / Pakistani / Bangladeshi	-1.684*** (0.154)	.	-7.578*** (0.503)	-2.208*** (0.272)	.	-6.174*** (0.638)	0.216*** (0.025)	.	0.216*** (0.036)	.
Black African / Caribbean	-0.798*** (0.245)	.	-3.162*** (0.738)	-0.338 (0.464)	.	-0.592 (0.950)	0.498*** (0.068)	.	0.778 (0.152)	.
Other	-0.784*** (0.194)	.	-2.550*** (0.553)	-1.089*** (0.319)	.	-2.672*** (0.765)	0.578*** (0.061)	.	0.465*** (0.068)	.
Male	-0.418*** (0.108)	.	-1.527*** (0.619)	-0.495*** (0.185)	.	-2.102*** (0.383)	0.661*** (0.034)	.	0.406*** (0.037)	.
FSM eligible in year 11	-0.121 (0.196)	.	-0.446 (0.619)	-0.082 (0.299)	.	-0.657 (0.799)	0.992 (0.126)	.	0.887*** (0.149)	.
Has older siblings	-0.009 (0.203)	.	0.206 (0.307)	-0.207 (0.196)	.	-0.260 (0.419)	1.071 (0.058)	.	0.951 (0.076)	.
Has younger sib's	0.203* (0.150)	.	0.901*** (0.309)	0.277 (0.189)	.	0.830* (0.422)	1.253*** (0.070)	.	1.272*** (0.105)	.
Live in owned home	-0.081 (0.150)	.	0.079 (0.426)	0.232 (0.277)	.	0.381 (0.598)	1.046 (0.074)	.	0.564*** (0.068)	.
N	7150	7150	7150	4608	4608	4608	8046	2387	4745	1074

[@]Standardized age 14 educational performance in wave 3-4 specification, and age 16 in wave 4-5 specification, such that this variable is wholly predetermined to the process being modelled. * p<0.1, ** p<0.05, *** p<0.01. Clustered standard errors in parentheses (except conditional fixed-effect logit).