The impact of incarceration on the early labour market outcomes of children in care*

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Abstract

We investigate the labour market effects of imprisoning children in care who offend. Using linked administrative data on four English birth cohorts allows this small population to be observed in full, permitting impacts to be estimated for both males and females. The richness of the data provides a means of controlling for self-selection into custody and the estimates also allow for unobserved heterogeneity to influence custody. We find that custody reduces males' employment and earnings by more than 10% up to age 21, the latest age observed. The earnings impact mostly reflects the negative employment effect although there is some evidence of a negative impact on wages for those entering work. For females, there is no overall impact on employment or earnings but, for those entering work, custody reduces wages by 25%. Probing the mechanism behind the impact for males provides weak evidence for discrimination being a factor.

Every year, more than 30,000 children in England enter care. Such 'looked after' children constitute a vulnerable group – most have suffered abuse or neglect – and they tend to experience worse outcomes than other children with regard to a range of outcomes including mental health (Department for Education, 2020), schooling (Sebba et al., 2015), attainment (Berger et al., 2015), higher education (Harrison, 2020) and employment (Berlin et al., 2011).

They are also more likely than other children to spend time in custody. The degree of over-representation is substantial; those in care account for less than 1% of all children

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but approximately half of all children in custody (Laming, 2016). There are no available statistics comparing the characteristics or outcomes of looked after children who enter custody with those of looked after children as a whole but, as will be shown later, they are characterised by a further concentration of disadvantage.

In this paper, we examine how the experience of custody affects subsequent education and early labour market outcomes for children in care. Robust evidence on the effect of incarceration has, until relatively recently, been lacking. Often, research interest has focused on the question of how prison affects recidivism rather than employment but even for that outcome robust evidence was scarce (Nagin et al., 2009). A key obstacle to estimating the causal impact of incarceration is the infeasibility of random assignment of custodial sentences. The self-selected nature of the prison population is such that unobserved influences on incarceration and labour market outcomes are likely to be highly correlated.

Recent contributions to the literature have exploited the quasi-random assignment of defendants to judges who vary in their sentencing tendencies. US evidence using this approach appears inconsistent. Loeffler (2013) finds no significant effect on employment 5 years post-indictment in Cook County, Chicago. Mueller-Smith (2015), on the other hand, finds incarceration reduces post-release employment and wages in Harris County, Texas. For the case of Norway, Bhuller et al. (2020) highlight impact heterogeneity, providing evidence of positive effects among those not working prior to prison but negative effects among those previously employed.

A similar approach has been applied to the case of child imprisonment. Aizer and Doyle Jr (2015) provide evidence that incarceration reduces high school completion and increases adult recidivism in Chicago. They also find negative impacts on employment and earnings, although these are not precisely estimated. Other US studies of the effect of juvenile incarceration on education and employment rely on observing all important individual characteristics likely to affect selection (Apel and Sweeten, 2010) or use panel data to control for fixed effects (Hjalmarsson, 2008).

In fact, much of the existing empirical evidence relates to the case of the US. This may not be informative of other countries with different criminal justice systems. For example, Scandinavian countries emphasise rehabilitation while the system in the US is more punitive. The main contribution of this paper is to provide evidence specific to the case of England. We consider children reaching the age of 18 between 2014 and 2017. The oldest of these children reached the age of criminal responsibility in 2004. Since that time, the number of young people in custody has fallen steeply. Despite this, it is a period during which punishment played an important role in the English system. The 2016 review of the youth justice system – the *Taylor Review* – remarked on this and recommended '…a shift in the way society, including central and local government, thinks about youth justice so that we see the child first and the offender second.' (Taylor, 2016). Beyond this philosophical stance, there have also been questions around the adequacy of the system. A government-commissioned report concluded that the secure estate for children was not fit for purpose (Wood et al., 2017) and a subsequent inspection found unacceptably low levels of safety (HM Chief Inspector of Prisons, 2017).

Our analysis uses administrative data that links individual education records (from the National Pupil Database, NPD) to post-school outcomes taken from tax, benefit and further/higher education records (so-called Longitudinal Education Outcomes, LEO). The linked NPD-LEO data allow nearly the full population of school children in England to be observed and tracked as they proceed through school and into early adulthood. Since the proportion experiencing custody is small, using population data is necessary to observe sufficient numbers to support detailed analysis.

The NPD-LEO data have particular strengths. First, as administrative data, they avoid the problems encountered with survey data: non-response, imperfect recall or withholding of sensitive information. Second, the data allow monthly histories to be constructed for individuals. We consider transitions from age 13 (incarceration is rare below this age, see Youth Custody Service (2021)) and this high frequency of observation allows the two-way relationship between custody and education to be captured. Among children in care, non-compliance with school attendance requirements is relatively common and our analysis allows this to be captured. Third, NPD-LEO provides a rich set of time-varying characteristics which can help control for selection into conviction. In particular, it allows the population of children who experience care to be identified and, within that, the population of those who have had some contact with the criminal justice system. Among this subgroup, some will have received a caution rather than been imprisoned. Similar to Apel and Sweeten (2010), we use this group as a comparison group for those who are imprisoned. Fourth, the size of the LEO data means that it becomes possible to estimate impacts not only for boys (who account for the majority of children in prison) but also girls. Given the over-representation of boys, girls are often excluded from empirical analyses. Our paper is able to help fill this evidence gap.

Estimation uses a multivariate mixed proportional hazard (MMPH) model of transitions between education, custody, employment and NEET (**N**ot in **E**mployment, **E**ducation or **T**raining). Ward et al. (2021) provide a recent example of using such a model in a similar context. For those entering work, earnings are modelled using the hazard approach introduced by Donald et al. (2000). All hazards are allowed to be influenced by unobserved heterogeneity and hazard-specific unobserved heterogeneity is allowed to correlate freely, thereby controlling for selection on unobservables.

In summary, our approach attempts to estimate the causal impact of custody among children in care by comparing outcomes of those who experience custody against outcomes among a closely comparable group who did not experience custody, after controlling for differences in both observed and unobserved characteristics. As far as we are aware, this amounts to the most rigorous analysis applied to the case of England.

In addition to our base model, we consider extensions that attempt to test theories of how impacts might be generated. Broadly, there are two channels through which impacts may materialise. First, incarceration might alter children's characteristics and behaviour in a way that has downstream impacts on education and labour market outcomes. This may arise in several ways. Most obviously, incarceration interrupts schooling and may result in children being 'left behind'. Equally, children in prison may be negatively influenced by their peers within the correctional facility (Bayer et al., 2009). Incarceration may also harm children's mental health and reinforce their view of themselves as criminals. More positively, incarceration could provide an opportunity for rehabilitation and support to address needs that would otherwise be left unmet.

Second, individuals who have experienced custody might be stigmatised by schools and employers and experience discrimination as a result (Bernburg and Krohn, 2003). They may also be treated differently by police and, if charged, by courts. Again, such treatment risks compounding their criminal identity.

We probe these mechanisms in two ways. First, we examine whether impacts vary with the length of incarceration. The intuition is that the first channel – referred to by Aizer and Doyle Jr (2015) as the 'behavioural channel' – relies on a sufficiently long period of custody. Should impacts be unaffected by duration of incarceration, we take that as suggesting a lesser role for the behavioural channel. Second, we test whether impacts vary with the local unemployment rate. The intuition in this case is that employer in tight labour markets are less able to discriminate in favour of applicants that do not have a criminal record. A finding that incarceration imposes a greater penalty in areas with higher unemployment would therefore indicate the importance of the second channel (the 'labelling' channel). Biddle and Hamermesh (2013) use an equilibrium search model to formalise this intuition. Several empirical studies provide relevant evidence. Dustmann et al. (2010) found a greater effect of economic shocks on unemployment among immigrants than among natives in Germany and the UK. They found no such differential for wage responses, unlike Bratsberg et al. (2006) in the case of the US. Baert et al. (2015) used a correspondence test to demonstrate that school-leavers in Belgium with a foreign-sounding name found it more difficult to be invited for interview in occupations where vacancies were easy to fill but not where they were hard to fill.

We find impacts that differ by gender. For males, custody reduces employment up to age 21 by more than 10%. This causes earnings to fall, but there is also an indication that custody reduces pay among those who find work. For females, there is no evidence of an impact on employment or on earnings as a whole. As with males, custody reduces pay among those in work but with females this impact is statistically significant. It is also sizeable, amounting to a 25% reduction. An interpretation of this is that while female employment may be unaffected by custody, the quality of the jobs they find is reduced. Probing the mechanism behind the observed impact for males, neither the test for the behavioural channel nor the labelling channel provide conclusive results, although there is perhaps more support for the labelling interpretation. We speculate that it might be the experience of custody rather than its duration that influences self-perception and behaviour in a way that has negative consequences for labour market outcomes.

The paper proceeds as follows. In the next section, we provide brief details of the child social services system and the youth justice system in England. In Section 2, we describe the data and provide summary statistics on the study population. Section 3 sets out the empirical approach. Estimation results are presented and discussed in Section 4. Section 5 concludes.

1 Children in care and in custody in England

1.1 Children's social care in England

Children are defined as 'looked after' according to the legal definition in the Children Act, 1989. A child may be regarded as looked after where a local authority provides accommodation for more than 24 hours in order to safeguard the child's welfare. This is subject to the child's consent or, for children under the age of 16, the consent of those with parental responsibility. The local authority must provide accommodation if there is no-one with parental responsibility or able to provide suitable accommodation or care, or if the child is lost or abandoned. Looked after children also include those subject to a care order. These orders are issued by a court in response to an application by a local authority or authorised person to place a child in the care or under the supervision of a designated local authority. Placement orders give a local authority the legal authority to place a child for adoption. These are also court orders that confer looked after status but, in contrast to care orders, can only be applied for by local authorities.

During the year to March 2021, 108,070 children were looked after at some point (Department for Education, 2021). Most common was a foster placement; as of end-March 2021, 71% of looked after children were fostered. Relevant to the focus of this paper, 62% were at least 10 years old, the age of criminal responsibility. More than half of all children in care, 55%, were looked after for at least 12 months. This group is the focus for the empirical analysis on account of the fact their offending outcomes are recorded; specifically, whether they were convicted or subject to a warning or reprimand under the Crime and Disorder Act 1998.

1.2 The youth justice system in England

The modern youth justice system was established by the Crime and Disorder Act 1998. Under the Act, young people charged with a criminal offence have their cases heard, at least initially, in the youth court; a type of magistrates' court for young people aged between 10 and 17. Serious crimes are passed to the Crown Court. For other crimes, the youth court can pass a range of custodial and non-custodial sentences.

Custodial sentences usually take the form of a Detention and Training Order (DTO) and last between 4 months and 2 years; the first half served in custody, the second in the community. The custodial period may be served at a young offender institution (YOI), a secure training centre (STC) or a secure children's home (SCH). Despite only serving boys aged 15–17, YOI's account for most of the youth custody population; 73% in 2018/19, compared to 17% in STC and 10% in SCH. YOIs mainly hold children considered to be more resilient. STCs are smaller and accommodate more vulnerable children. SCHs are smaller still and designed for especially vulnerable children.

Non-custodial sentences include Referral Orders (whereby the offender agrees a contract with restorative commitments lasting 3-12 months which aim to make up for harm caused by their offending and support the child towards living a safe and crime-free life) and Youth Rehabilitation Orders which specify requirements the offender must comply with for up to three years (such as curfew, electronic tagging, voluntary work). The courts can also use Liaison and Diversion services to divert vulnerable offenders to the appropriate places of treatment at sentencing.

Figure 1 shows the trends in the numbers of looked after children (Department for Education, 2020) and children in custody (Youth Custody Service, 2021). The looked after population has grown steadily over the last decade to more than 80,000 in 2020. The number of children in custody, by contrast, shows a marked drop and by 2020 was below 700. The focus in this paper is on individuals whose last year as children ranged from 2013 to 2016, during which time the numbers in custody averaged a little over 1,000.

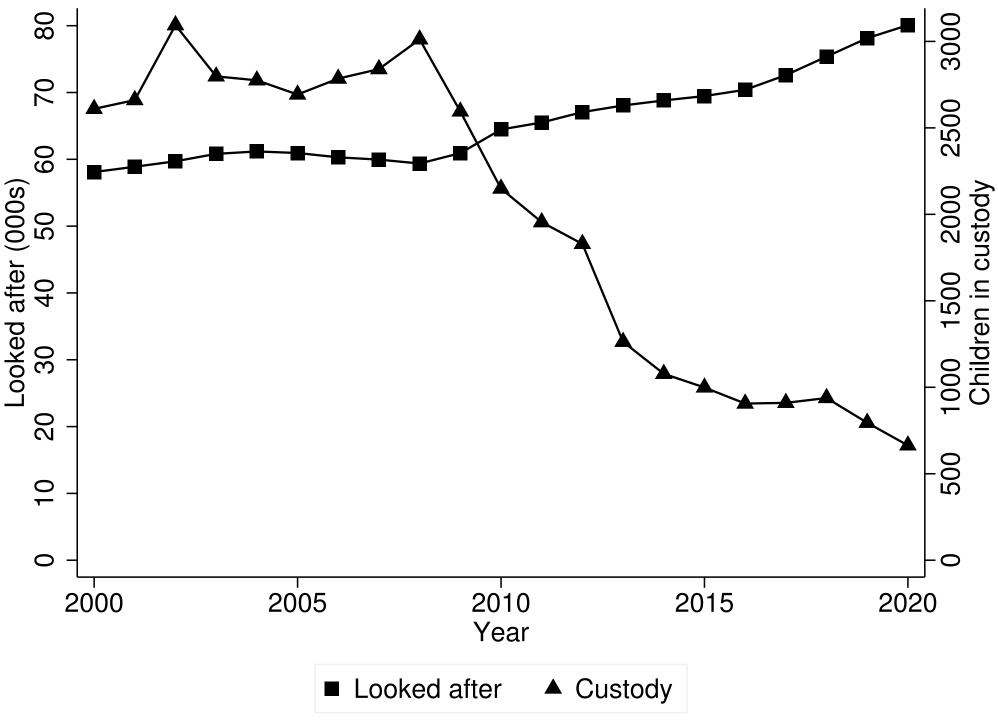

Figure 1: *Children looked after and children in custody, 2000-2020.*

2 Data

The analysis in this paper is based on the linked National Pupil Database (NPD) and Longitudinal Education Outcomes (LEO) data. This is a person-level dataset constructed by matching individuals' education data with their employment, benefits and earnings data. It is drawn entirely from administrative records and covers nearly all school children in England, tracking them on an ongoing basis after they leave school.

NPD-LEO has several strengths that make it well-suited to the analysis in this paper. First, it identifies looked-after children. Local authorities provide an annual return (the so-called SSDA903 return) to the Department for Education (DfE), giving details of all children who have been looked after at some point in the previous year. Dates of spells in care are supplied as well as details such as category of need, legal status, type of placement and the reason for spells ending. For those looked after for at least a year, information is also supplied on whether the child was convicted during the course of the year. Although this variable is not generally available as part of NPD it can be provided on request from DfE. It is this convicted subgroup of looked-after children that forms the focus for the analysis in this paper.

A second strength of NPD-LEO is that it identifies children who enter custody. The SSDA903 return identifies those children whose legal status falls under the auspices of the youth justice system. From school-leaving age onwards, there is an additional source: the NCCIS dataset, which forms part of the NPD.[1] Local authorities are required to supply data to DfE so that they can monitor the extent to which young people are meeting their duty to participate in education or training until their 18th birthday, as required under the Education and Skills Act 2008. Among the information supplied is a monthly status variable. This information allows custody to be observed on a month-by-month basis up to the age of 18.

A third strength of NPD-LEO is its large size which allows the examination of small

[1] National Client Caseload Information System - `https://www.gov.uk/government/publications/nccis-management-information-requirement`.

population subgroups. In particular, the experience of custody is observed in sufficient numbers to support detailed analysis. Furthermore, as administrative data, it is not reliant on respondent cooperation so does not encounter the same issues as survey data with regard to attrition or non-response more generally. Instead, individuals are tracked using administrative data (at little cost), over an extended period of time.

Finally, NPD-LEO offers a rich set of conditioning variables useful for econometric estimation. In the analysis, we focus on children from age 13 onwards since custody prior to this is extremely rare. We incorporate information on characteristics that do not change over time, such as gender, date of birth, ethnic background as well as summary measures of education and social care histories prior to age 13. We also include time-varying variables including information on free school meal eligibility, types of school attended, special educational needs, school exclusions, school attendance, periods of being in need, periods of being looked after, educational attainment, activity status, earnings and local authority of residence.

The NPD-LEO data also has some limitations, however. In particular, for those observed to be convicted, it does not record the nature of the offence committed. Furthermore, conviction itself is only recorded for those who have been in care for 12 months or more. Lastly, for those entering employment the number of hours worked is not observed. This means it is only possible examine the effect on earnings rather than hourly wages (which may be more informative of the quality of a job).

We use data on four cohorts of young people, those born between 1st September 1994 and 31st August 1998. Outcomes are tracked until March 2017. At this point, the oldest (earliest) cohort was academic age (hereafter, 'age') 21 and the youngest was age 18.[2] Table 1 summarises some of the key characteristics of the population (see Appendix A for a glossary of key terms). Four columns are shown. The first two make up the population of children who have been looked after for 12 months or longer and who have been convicted of a crime. Some among these received a custodial sentence;

[2] Academic age is age in years on 31st August prior to the start of the academic year.

their characteristics are given in column (1). The remainder received a non-custodial sentence and their characteristics are shown in column (2). For context, columns (3) and (4) present characteristics for all looked after children and all children respectively.

As an initial comment, we note that looked after children form a small subset of all children; roughly two per cent. They are much more likely to be in receipt of free school meals, to have attended a non-mainstream school and to have special educational needs (SEN). Their attainment is much worse than the population as a whole for both English and maths. Overall, the impression is of looked after children forming a highly selected and disadvantaged group. Looked after children convicted of a crime appear in some ways to be more marginalised still. Compared to looked after children as a whole, columns (1) and (2) suggest that those convicted are more likely to have been outside mainstream education and more likely to have special educational needs on the grounds of behavioural, emotional and social difficulties. Their attainment in English and maths is also worse.

Those convicted who do not receive a custodial sentence are of interest since they offer a partial means of controlling for selection into an offending subgroup. Columns (1) and (2) constitute the estimation sample, with the idea being that the non-custodial subgroup provide a means of identifying counterfactual outcomes of the custodial subgroup. However, a comparison of the two columns reveals several differences, particularly noticeable perhaps with attainment. Such differences can be controlled for in estimation but the concern is that not all sources of differences can be observed. Our econometric model addresses this by controlling for unobserved heterogeneity, as described below.

Table 2 provides details on the nature of children's need and, for those in care, the type of placement. These relate to the most recently observed need and placement. Among all children (column 4), 12% were categorised as being in need at some point. This contrasts with looked after children, where nearly all were categorised as in need. The category of need is fairly consistent across columns, with abuse or neglect being the most common reason but family circumstances (acute stress, dysfunction) also being

Table 1: Population characteristics

	(1)	(2)	(3)	(4)
	Looked after, convicted:		Looked after	All children
	Custody	No custody		
Female (%)	16	39	47	49
English as additional language (%)	6	4	11	14
Free school meals (%)	33	34	46	21
Attended PRU (%)	39	33	17	2
Attended special school (%)	26	19	20	2
Attended local authority AP (%)	36	20	11	1
SEN – school action (%)	16	23	26	21
SEN – school action plus (%)	39	47	37	11
SEN – statemented (%)	33	27	27	4
SEN – BESD (%)	52	50	31	5
SEN – MLD (%)	7	9	11	3
Ethnic group (col. %)				
White	77	85	78	82
Mixed	10	7	6	4
Asian	3	1	5	8
Black/Caribbean/Other	9	5	8	5
Other	1	1	1	1
Key Stage 4 English (col. %)				
- U	65	48	39	5
- G	4	4	3	1
- F	6	9	7	3
- E	7	13	12	7
- D	6	11	15	16
- A*-C	3	10	21	67
Key Stage 4 maths (col. %)				
- U	57	40	35	5
- G	7	11	9	3
- F	8	13	10	6
- E	7	12	10	7
- D	6	9	11	11
- A*-C	5	11	21	67
N (rounded to nearest 10)	1,380	2,990	46,270	2,213,580

important. Socially unacceptable behaviour is particularly high among the custody group (column 1). We caveat this characterisation of need by highlighting that, for each child, the data record only the main driver rather than the possibly multiple categories of need that may apply.

Table 2 again confirms the low incidence of care in the population as a whole; only 2% of children had been looked after at some point. There are marked differences between looked after children as a whole and looked after children who had been convicted. Among looked after children as a whole, their last placement was most commonly with foster parents. This was much less common among the convicted subgroup, especially those who experienced custody. Among the convicted group, most common was community placement (37%); more than double the rate among looked after children as a whole (16%). This is essentially independent living, possibly where there is an employment or training component (it includes armed forces). Secure units, children's homes and semi-independent living (residential accommodation where some supervisory or advice staff are employed) account for 28% of the convicted subgroup compared to 21% among looked after children as a whole. Other residential settings are much more common among those who experienced custody (16%). This is unsurprising since this category includes YOIs and STCs.

We construct a monthly activity indicator for each individual from academic age 13 until the end of March 2017. We define four activities: education, custody, employment and NEET. We then collapse continuous periods of the same activity into spells. Table 3 describes the spell structure of the data. Columns (1) and (2) relate to the convicted subgroup who received a custodial sentence (column (1) of Table 1), shown separately for males and females. The median male in this group is observed to have 7 spells, compared to 6 for the median female. Males tend to be observed for slightly longer too; a median of 90 months relative to 78 months. Since the empirical analysis considers children from age 13 onwards, this indicates that the average individual is followed up until the end of their 19th year. The length of observation varies by cohort, with those in

Table 2: Category of need and nature of placement

	(1)	(2)	(3)	(4)
	Looked after, convicted:		Looked after	All children
	Custody	No custody		
CIN (%)	100	100	96	12
Category of need among those in need (col. %)				
- Not stated	4	4	3	5
- Abuse or neglect	43	48	40	39
- Child disability or illness	1	2	11	8
- Parent disability or illness	2	2	3	2
- Family in acute stress	16	15	12	11
- Family dysfunction	22	21	18	18
- Socially unacceptable behaviour	8	4	5	4
- Low income	-	-	1	-
- Absent parenting	3	3	4	2
- Missing	-	-	3	-
CLA (%)	100	100	100	2
Placement type among looked after children (col. %)				
- Foster placement	12	25	38	
- Placed for adoption	-	-	-	
- Placed with parent/guardian	5	8	4	
- Other community placements	37	37	15	
- Secure units/children's home	28	28	21	
- Other residential settings	16	2	5	
- Residential schools	1	1	1	
- Other placement	1	1	-	
- Missing	-	-	14	
N (rounded to nearest 10)	1,380	2,990	46,270	2,213,580

Note: To guard against possible disclosiveness, percentages are rounded to the nearest whole number. Where this is 0, the entry is replaced with "-" and where this suppression occurs within a categorical variable (category of need, placement type), a second entry is likewise suppressed.

the academic year 1994/95 observed into their 21st year (an average actual age of nearly 22). Hence, the data allow us to follow individuals for some years after school-leaving age.

Table 3: Spell characteristics

	(1) Custody, male	(2) Custody, female	(3) No custody, male	(4) No custody, female
Number of spells	8527	1741	11367	6833
Spells per person				
- mean	7.4	6.2	7.4	6.2
- median	7	6	7	6
- max	22	21	22	21
Months observed				
- mean	86.2	83.7	86.2	83.7
- median	90	78	90	78
- max	102	102	102	102

Spell length by type of spell is shown in Table 4. Across the four states we consider, education and NEET spells are longer than custody and employment spells. This is true for both males and females. Perhaps the most marked difference between males and females is with regard to censoring of custody spells, 13% and 1% respectively.

Table 4: Spell length by type of spell

	Education	Custody	Employment	NEET
	Males			
Mean length (months)	15.6	6.8	4.9	11.8
Median length (months)	9	4	3	6
Maximum length (months)	70	48	40	90
Right censored (%)	1	13.1	7.7	30.9
Number of spells	2957	1298	859	3413
	Females			
Mean length (months)	12.5	5.9	5.2	12.9
Median length (months)	8	5	4	7
Maximum length (months)	62	27	33	68
Right censored (%)	1.7	1.1	10.5	30.7
Number of spells	693	273	114	661

Figures 2 and 3 provide a visualisation of the duration of different spells and the probability of exiting to a specific state. We distinguish between under-16s (for whom employment does not feature as a state) and those aged 16 and over. Figure 2 relates to children while they remain under the age of 16. The charts on the leading diagonal are the empirical (Kaplan-Meier) survival curves for education, custody and NEET, respectively. These show negative duration dependence in all cases. No custody spells and virtually no NEET spells endure 36 months whereas this is not the case for education, reflecting ongoing spells beyond age 15. The impression is of similar behaviour among boys and girls, although girls appear less likely to spend more than a year in custody.

The off-diagonal charts plot the cumulative incidence curves; that is, the probability of having exited the initial state (in the row) to a specific destination (in the column) as the spell lengthens. These charts provide additional insight into the nature of transitions. The probability of entering custody from education (first row of Figure 2) grows at a steady pace (yet remains low) whereas the probability of exiting to NEET increases notably towards the end of the period, reflecting the end of compulsory schooling. Custody spells (row 2) are shorter and, at this age, are more likely to end in a move to education rather than NEET. Boys are perhaps more likely to return to education while girls are more likely to become NEET. Lastly, row 3 shows that it is most common for NEET spells to end in a return to education, particularly for girls. Custody is a less common destination, but noticeably more so for boys.

Figure 3 presents similar charts for individuals aged 16 or over. Employment is included as an additional state. Considering first the survival curves (on the diagonal), very few education spells last more than 24 months. This is also the case for custody spells, although this is by construction; since we are focused on children in custody, such spells are censored at age 18. As before, custody is a more common destination for males than females. Employment spells are similarly unlikely to last more than two years. In fact, it is NEET spells that are more likely to last beyond this point, although still that is the minority of cases.

Turning to the cumulative incidence curves, education spells (row 1) are most likely to end with the individual becoming NEET. Employment is the second most common

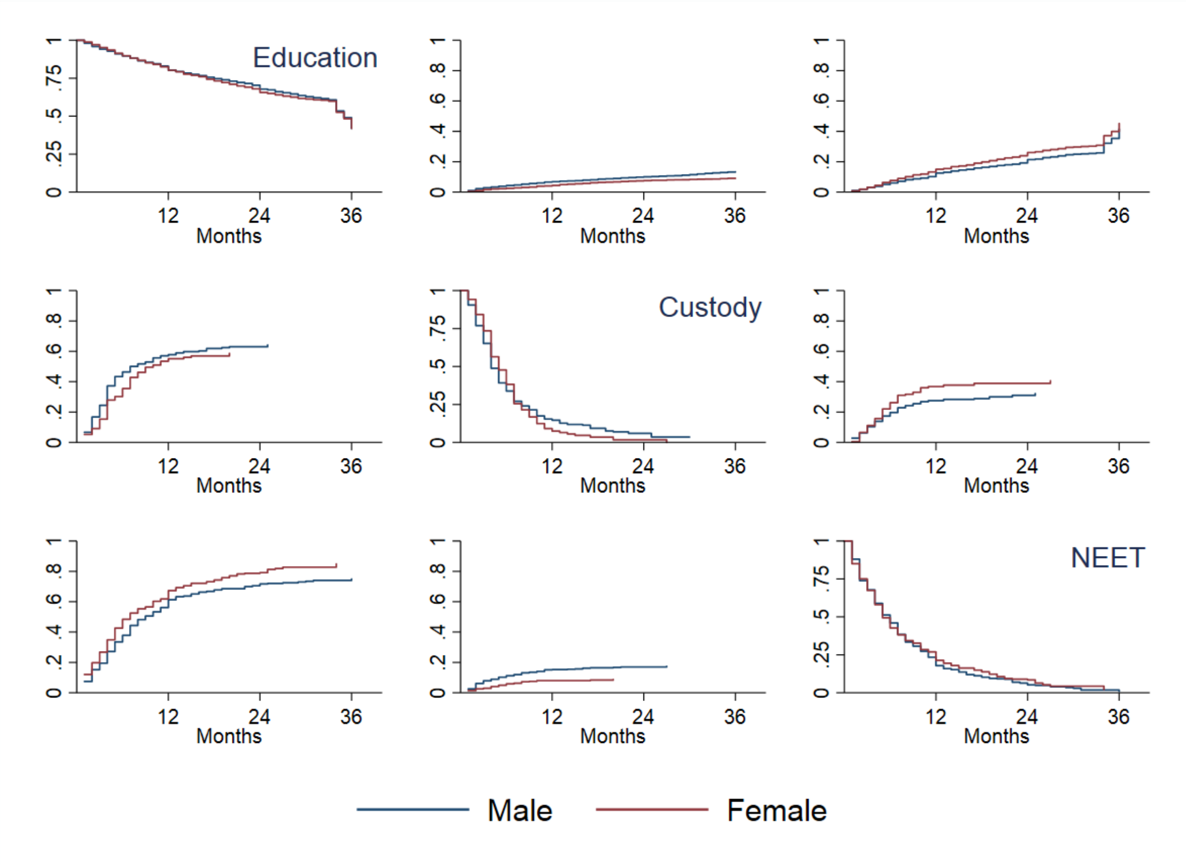

Figure 2: *Survival and cumulative incidence curves, under-16s.*

destination, while custody accounts for relatively few. Individuals in custody (row 2) are most likely to become NEET at the end of their spell (especially males) or to move into education (especially females). Employment, by contrast, is very rare as a destination on leaving custody (this rarity has consequences for modelling such transitions, as discussed below). Similarly, very few employment spells (row 3) end in custody. Instead, NEET and education are the most common exit statuses. Lastly, row 4 shows that most exits from NEET are to education or employment. Males are more likely than females to exit to custody.

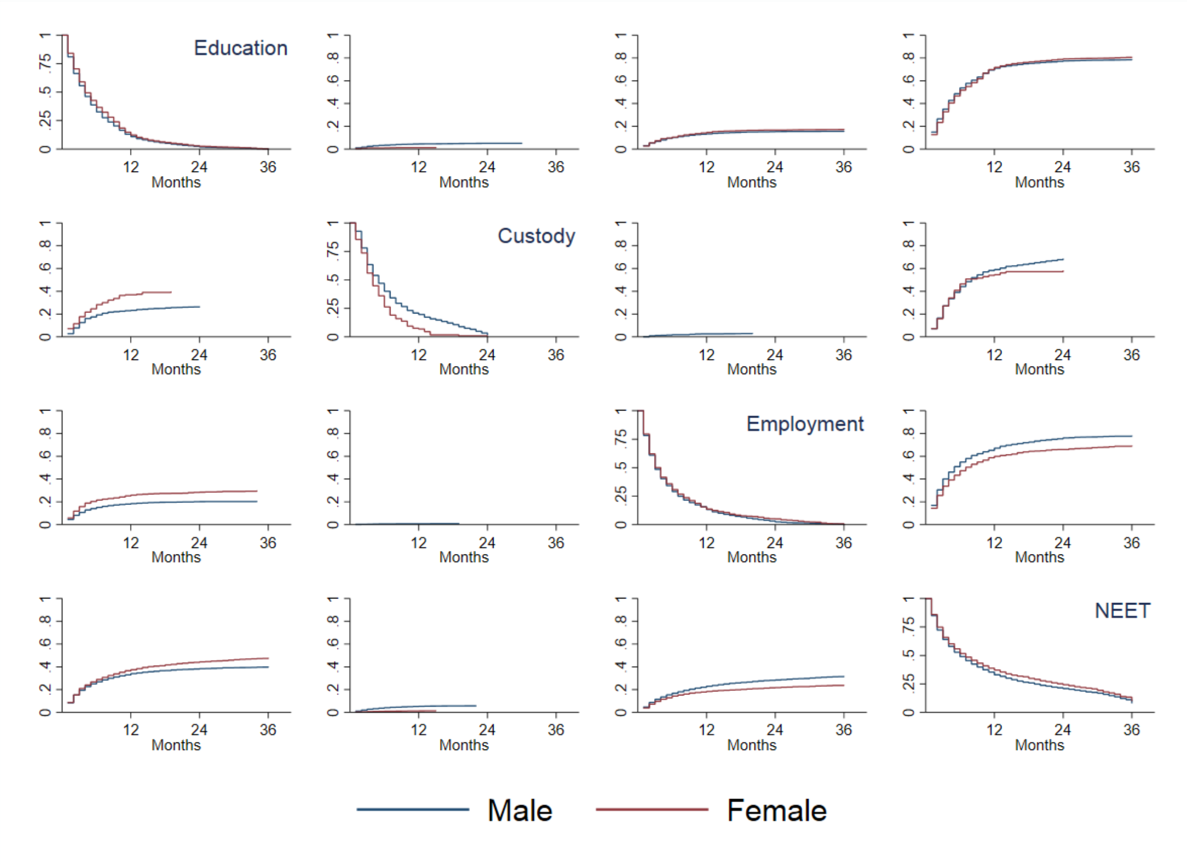

Figure 3: *Survival and cumulative incidence curves, over-16s.*

3 Empirical strategy

3.1 Econometric model

We use a multivariate mixed proportional hazard (MMPH) model of transitions between education, custody, employment and NEET. Our approach resembles that of Cockx and Picchio (2012). It assumes transitions occur in continuous time but are interval-censored, as is the case here. Transitions between states are modelled as separate hazard functions. A hazard function is also used to model earnings, following Donald et al. (2000). Doing so has the advantage of flexibility since it avoids the restrictiveness of assuming *a priori* that earnings follows a particular distribution.

3.1.1 The likelihood function

Following Gaure et al. (2012), we write the integrated period-specific hazard rate associated with moving from origin state j to destination state k in month t for individual i, θ_{ijkt}, as a function of observed variables, x_{ijkt}, and (time-invariant) unobserved characteristics υ_{ijk}:

$$\theta_{ijkt} = \int_{t-1}^{t} \phi_{ijks} \mathrm{ds} = \exp(x_{ijkt}\beta_{jk} + \upsilon_{ijk}) \tag{1}$$

where ϕ_{jks} is the underlying continuous-time hazard rate, assumed to be constant within each month.

Earnings at the start of a new job are also conceptualised as a hazard rate. The probability of individual i earning exactly w given earnings of at least w is ϕ_{iw}. The integrated hazard rate within band b, which runs from w_{b-1} to w_b, is a function of observed variables, x_{iwt}, the baseline hazard (specified piecewise and captured by segment dummies with coefficient ζ_b) and (time-invariant) unobserved characteristics υ_{iw}:

$$\theta_{iwb} = \int_{w_{b-1}}^{w_b} \phi_{is} \mathrm{ds} = \exp(x_{iwt}\gamma_w + \zeta_b \mathbb{1}\left(w_{b-1} < w \leq w_b\right) + \upsilon_{iw}). \tag{2}$$

Leaving the i subscript implicit, the contribution to the likelihood function of a spell with origin state j that is not observed to have ended (i.e. a censored spell) after duration d is

$$L_{c,jj} = \prod_{r=1}^{d} \exp\left(-\sum_{k \neq j} \theta_{jkr}\right). \tag{3}$$

The contribution to the likelihood function of a spell with origin state j that ends with a transition to destination state $k, j \neq k$, at duration d (an uncensored spell) is

$$L_{u,jk} = \left[1 - \exp\left(-\sum_{k \neq j} \theta_{jkd}\right)\right] \frac{\theta_{jkd}}{\sum\limits_{k \neq j} \theta_{jkd}} \prod_{r=1}^{d-1} \exp\left(-\sum_{k \neq j} \theta_{jkr}\right) \tag{4}$$

as derived in Cockx (1997), equations 22-29.

Where the transition is to employment, there is a further contribution to the likelihood from the earnings equation. In this case, failure is in segment s of the earnings distribution

$$L_e = [1 - \exp(-\theta_{ws})] \prod_{r=1}^{s-1} \exp(-\theta_{wr}). \tag{5}$$

The contribution of a spell starting in state j can be written generally as

$$L_j = L_{c,jj}{}^{1-\sum\limits_{k\neq j} y_{jk}} \times \prod_{k\neq j} L_{u,jk}{}^{y_{jk}} \times L_e{}^{y_{je}} \tag{6}$$

where y_{jk} is a dummy variable taking the value 1 where a spell starting in state j ends with a transition to state k (zero otherwise) and y_{je} is a dummy variable taking the value 1 where a spell starting in state j ends with a transition to employment, state e.

We follow Heckman and Singer (1984) and discretely approximate the unobserved heterogeneity joint distribution by M mass points, $\upsilon^m, m = 1, 2, \ldots, M$, where $\upsilon^m = \{\upsilon^m_{ec}, \upsilon^m_{ew}, \upsilon^m_{en}, \upsilon^m_{ce}, \upsilon^m_{cw}, \upsilon^m_{cn}, \upsilon^m_{we}, \upsilon^m_{wc}, \upsilon^m_{wn}, \upsilon^m_{ne}, \upsilon^m_{nc}, \upsilon^m_{nw}, \upsilon^m_{w}, \}$. Here, the paired subscripts denote the type of transition by specifying the origin and destination states – e (education), c (custody), w (employment) or n (NEET) – while υ^m_w is the unobserved heterogeneity term in the earnings equation. The probability attached to υ^m is specified as $p^m = \exp(\lambda^m)/(\sum_{g=1}^{M} \exp(\lambda^g)), m = 1, ..., M$, where $\lambda^1 = 0$. The number of mass points, M, is unknown a priori but chosen on the basis of specification tests. Intuitively, we consider that individuals may be in one of M subgroups. For transitions, we then have $\theta^m_{ijkt} = \exp(x_{ijkt}\beta_{jk} + \upsilon^m_{jk})$ and $\theta^m_{iwb} = \exp(x_{iwt}\gamma_w + \zeta_b \mathbb{1}(w_{b-1} < w \leq w_b) + \upsilon^m_w)$ for subgroup m.

Writing the contribution to the likelihood of a full spell for an individual in subgroup m as L^m_{is}, the contribution of individual i (conditional on being type m) is the product of all i's spells, S_i:

$$L^m_i = \prod_{s=1}^{S_i} L^m_{is}. \tag{7}$$

Integrating out the unobserved heterogeneity, the overall contribution of individual

i is

$$L_i = \sum_{m=1}^{M} p^m L_i^m \tag{8}$$

Across all individuals, I, the likelihood is

$$L = \prod_{i=1}^{I} L_i = \prod_{i} \sum_{m=1}^{M} p^m L_i^m. \tag{9}$$

Since we want to work with the log-likelihood, we write this out in full as

$$\ln L = \sum_{i=1}^{I} \ln \sum_{m=1}^{M} p^m L_i^m. \tag{10}$$

3.2 Identification

Horny and Picchio (2010) show that, under the MPH assumption, both the unobserved heterogeneity distribution and the structural parameters of the model – including the lagged dependencies – are non-parametrically identified. To further assist identification, we restrict the specification of hazards to be similar across multiple spells of the same type (Frijters, 2002). Brinch (2007) proves that exogenous variation in covariates over time and across individuals is sufficient for identification, without the need for proportionality. We include in our model several time-varying covariates: receipt of free school meals, SEN status and type, child need and looked after status, type of schooling, exclusion, quarterly dummies and monthly local unemployment rate. These series vary exogenously over time and, in the case of local unemployment, by local authority area. Furthermore, due to differences between individuals in when they start each spell and the fact that we observe multiple spells of differing durations, there is variation in these covariates across individuals at the same point in their spell. This provides another source of identification and thereby reduces reliance on the assumption of proportionality.

Lastly, we note that most identification results relate to continuous time processes. Gaure et al. (2007) provide extensive Monte Carlo evidence that the parameters of the underlying continuous time model can be recovered using discrete data, so long as the likelihood function reflects the discrete nature of the available data.

3.3 Parameterising the model

Modelling transitions between four states gives rise to 12 types of transition. Duration dependence for each of these transitions is captured through the use of a piecewise constant baseline hazard. The nature of the baseline hazard varies with transition type. Transitions from education, for example, exclude duration dependence terms since they are heavily concentrated at the end of compulsory schooling. With other transition types, the fineness of the segmentation depends on the frequency of observed transitions at particular durations.

The key variable in each case is the lagged custody indicator. This takes the value 1 when the preceding spell was custody. Lagged employment and lagged NEET indicators, defined analogously, are also included. In addition, specifications of the model designed to test hypotheses around how impacts arose include lagged duration terms (for custody, employment and NEET) and an interaction of the lagged custody indicator with the local unemployment rate. Estimation controls also for a range of other characteristics. These include age, whether white and whether English is spoken as an additional language. Gender is not included since results are estimated separately for males and females. A number of time-varying variables are included:

- Special educational needs – when recorded by the school as SEN, the changing severity of these needs and their nature
- Children's social services – when identified as being a child in need, when entered care
- Non-mainstream education – when attended PRU, AP or special school
- Exclusion from school – when excluded from school
- Attainment – as captured by qualifications achieved at age 16 or later

- Local unemployment rate – measured at the local authority level.

Alongside the transitions, initial earnings for those entering work are also modelled in hazard form. Rather than a piecewise constant representation of spell duration, the earnings hazard is a piecewise constant representation of the distribution of earnings. In effect, the distribution of daily earnings is divided into deciles which constitute the segments of the baseline hazard.

As recommended by Gaure et al. (2007), the number of points of support used to characterise unobserved heterogeneity was chosen to minimise the Akaike Information Criterion. For both males and females, this was with $M = 3$ points of support.

There are two complications that the specification addresses. First, for females, transitions between custody and employment (in both directions) are too infrequent to model. Such transitions are therefore omitted. Second, custody is only included as a distinct state up to age 17. This is primarily driven by the fact that it complies with the definition of a child within the youth justice system. However, even without this motivation it would be necessary to handle the fact that custody is unobserved beyond age 18. We address this by treating as censored those custody spells that are ongoing when turning 18. Such censored spells are reclassified as NEET from age 18 onwards and regarded as having lasted since the start of the custody spell.

4 Results

Our main estimates allow the nature of exits from spells to depend on whether the preceding spell was custody (occurrence dependence). The estimation results are provided in full in Appendix B. In this section, the key findings are highlighted and discussed. Results allowing for lagged duration dependence and sensitivity to local labour market conditions are also presented in order to probe the question of whether the main impacts are likely to arise through the behavioural channel or the labelling channel.

4.1 Goodness of fit

Figure 4 provides a visualisation of model fit, as captured by how well the estimation results can replicate observed trends in the data. In each of the charts, the lines depict the observed proportions in each state from age 13 up to age 21. The estimated model was used to simulate transitions between states over this same age range. This was done 1,000 times for every child, with each simulation based on a different draw from the distribution of estimated parameters. The shaded areas in Figure 4 correspond to percentiles 2.5 and 97.5 of the simulated distributions. The proportions in each state fall within the simulated confidence interval in most cases indicating that the estimated model successfully allowed the main features of the observed trends to be reproduced. This is true even where there are rapid changes, such as reaching the end of compulsory schooling (towards the end of academic age 15) when there is a substantial movement from education to NEET.

4.2 Main results – the impact of custody

The key coefficient relates to the indicator of whether the individual was in custody immediately prior to the current spell. This coefficient itself represents the effect of such prior custody on a specific hazard rate. As such, it is perhaps difficult to interpret and this is even more the case because the model relates to a dynamic system, whereby equations interact and prior custody can, through its influence on the current spell, exert an indirect effect on subsequent spells. To make the results more meaningful, the approach in this paper is mainly to summarise impacts through simulations using the estimated model coefficients.

Table 5 presents the results of testing for the statistical significance of the prior custody variables. This is labelled "a) Preceding spell custody". For males, prior custody increases transitions from any state back into custody. It reduces transitions into education, increases transitions from education into NEET and reduces transitions from

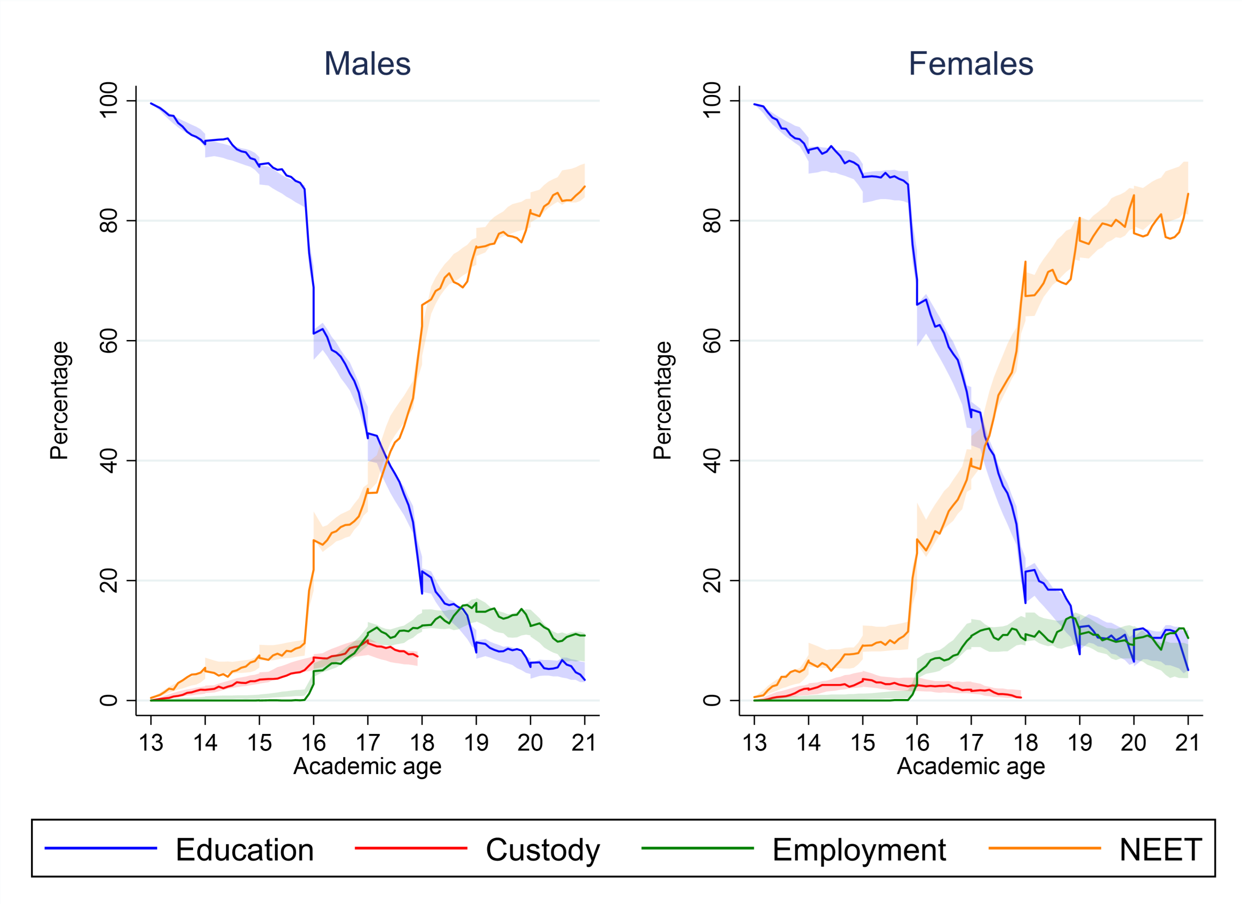

Figure 4: *Observed and modelled levels of education, custody employment and NEET.*

Notes: The lines in these charts depict the rates of Education, Custody, Employment and NEET from age 13 up to age 21 observed in the data. The shaded areas represent percentiles simulated rates using the results of the estimated model. Simulation involved 1,000 replications and the shaded areas correspond to percentiles 2.5 and 97.5 of the simulated distributions.

NEET into work. Broadly, prior custody appears to increase flows towards either further custody or NEET. There is some suggestion of a negative impact on earnings (a positive coefficient implies 'failing' in the lower reaches of the earnings distribution), although this falls short of the conventional level of statistical significance.

Table 5: Key coefficients

	$e \to c$	$e \to w$	$e \to n$	$w \to e$	$w \to c$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	$earnings$
Males										
a) Preceding spell custody:										
- coefficient	1.73	-0.04	0.39	-2.21	2.69	0.16	-0.43	0.65	-0.37	0.29
- P-value	0.00	0.85	0.00	0.03	0.00	0.52	0.00	0.00	0.01	0.08
b) Lagged duration dependence:										
- 1-3 months	0.23	0.28	0.30		2.03	0.25	0.18	-0.26	0.33	-0.69
- P-value	0.30	0.57	0.06		0.08	0.65	0.31	0.17	0.19	0.05
- 4-6 months	0.35	-0.23	0.41			0.93	0.35	-0.57	0.05	1.28
- P-value	0.13	0.72	0.02			0.08	0.07	0.01	0.88	0.00
- P-value for test of no LDD	0.31	0.62	0.05			0.22	0.19	0.04	0.38	0.00
c) Unemployment interaction:										
- coefficient	-0.06	0.03	-0.07	-0.66	1.33	0.04	-0.15	-0.03	0.07	0.11
- P-value	0.20	0.82	0.03	0.48	0.01	0.80	0.00	0.56	0.38	0.27
Females										
a) Preceding spell custody:										
- coefficient	1.71	-0.16	0.19	0.75		-0.02	0.03	1.49	0.13	0.62
- P-value	0.00	0.69	0.16	0.32		0.98	0.80	0.00	0.68	0.05
b) Lagged duration dependence:										
- 1-3 months	-0.20	0.20	-0.30				-0.08	0.33	1.26	0.24
- P-value	0.66	0.85	0.30				0.80	0.49	0.13	0.74
- 4-6 months	-0.31	1.08	-0.21				0.09		0.83	-0.34
- P-value	0.51	0.23	0.44				0.78		0.35	0.68
- P-value for test of no LDD	0.80	0.41	0.55				0.85		0.32	0.71
c) Unemployment interaction:										
- coefficient	-0.02	-0.06	-0.13	0.47		0.21	-0.03	0.00	-0.07	-0.21
- P-value	0.90	0.75	0.03	0.19		0.70	0.66	1.00	0.70	0.13

For females, the results suggests instead that custody begets further custody but there is no suggestion that, for those who avoid further custody, there is a channelling towards NEET, nor a reduction in moves from NEET to employment. As with males, there is a negative impact on wages (a positive impact on the earnings hazard); this time though it is significant at the conventional level.

Figure 5 summarises the main results for males (first column) and females (second column). Each row relates to a different outcome: education (row 1), employment (row 2), NEET (row 3) and earnings for those in employment (row 4). Hence, there are eight graphs. Each of these follows a similar format, showing the estimated impact on the respective outcome between the ages of 18 and 21. The impacts are shown as lines in the graphs, with shaded areas depicting confidence intervals. The impacts are generated by simulating outcomes. Each replication is based on a single draw from the joint distribution of estimated coefficients which is then used to simulate transitions between states and earnings from ages 18 to 21 for the population of individuals observed to be in custody at some point. The observed states and characteristics at age 18 are used as initial conditions, except for the lagged terms included to capture occurrence dependence. Each draw is used to generate a pair of simulated histories; one under the restriction that individuals were in education immediately prior to their initial (age 18) state, the other under the restriction that they were in custody immediately before. These restrictions are imposed by appropriately setting the initial conditions of the lagged terms. To achieve a simulated impact of prior custody relative to education on, for example, employment, we can then subtract the proportion simulated at that age to be in employment under the first restriction from the corresponding proportion under the second restriction. The graphs show the mean simulated impacts across 1,000 replications times, along with confidence intervals corresponding to percentiles 2.5 and 97.5 of the simulated distribution of impacts.

An initial comment on Figure 5 is that the effect of prior custody is very different for males and females. For males, prior custody reduces education and employment, and so correspondingly increases NEET. These impacts are often significant, as indicated by the confidence intervals excluding zero. This significance is most evident up to age 20 in the case of education and employment but is sustained in the case of NEET. The bottom graph shows the impact on earnings for those simulated to be in work. Here the impact is negative in the short-term but quite quickly gravitates towards zero. Prior custody reduces earnings for those in work by 14%, although, as evident from Table 5, this falls short of the conventional significance level.

For females, the simulations suggest no significant impact of prior custody on education, employment or NEET. Not only do the confidence intervals span zero, but the impact estimates are also close to zero throughout the 18-21 age range. For those in work, the impact on earnings resembles that seen for males. While the confidence intervals include zero, they do not span zero. In fact, the distribution of earnings impacts is heaped at zero, and less than 1% of the distribution of simulated impacts exceeds zero. Hence, while the results cannot reject the null hypothesis of zero earnings impact of prior custody among those in work, they would reject the null hypothesis of a positive effect. In other words, the estimated impacts are non-positive. For females entering work, prior custody reduces earnings by 25%, significant at the 95% significance level.

Table 6 summarises the estimates in Figure 5, showing impacts on number of months spent in each state or, for earnings, the total amount earned, at ages 18, 19, 20 and 21. It also shows the baseline number of months worked (or total earned) each year. This allows a qualitative assessment of how meaningful the estimated impacts are.

For males, confidence intervals indicate that annual impacts on education, employment and NEET are statistically significant at all ages. Focusing on employment, prior custody reduces months worked while 18 by 0.157 months. This is from a baseline of 1.239 months worked in that year on average, a reduction of approximately 13%. The reduction peaked at age 19 (14%) before falling slightly to 10% at age 21. Overall, this points to a sustained employment penalty arising from custody. The impact on NEET is smaller in percentage terms due to the larger proportion of the population that is neither studying nor in work. Prior custody increases the number of months spent NEET by 5% at age 18, falling to 2% at age 21. Lastly, earnings are significantly reduced by prior custody. Unlike Figure 5, earnings here are not conditional on employment. Instead, earnings for those with prior custody are compared against earnings simulated for those with no prior custody. Consequently, the earnings impact in Table 6 reflects both the employment impact and the impact on amount paid for those in work. The reduction at age 18 is 16%. This falls to 10% at age 21, by which point the impact is no longer statistically significant. For females, by contrast, none of the results is statistically significant. This is consistent with the impression from Figure 5.

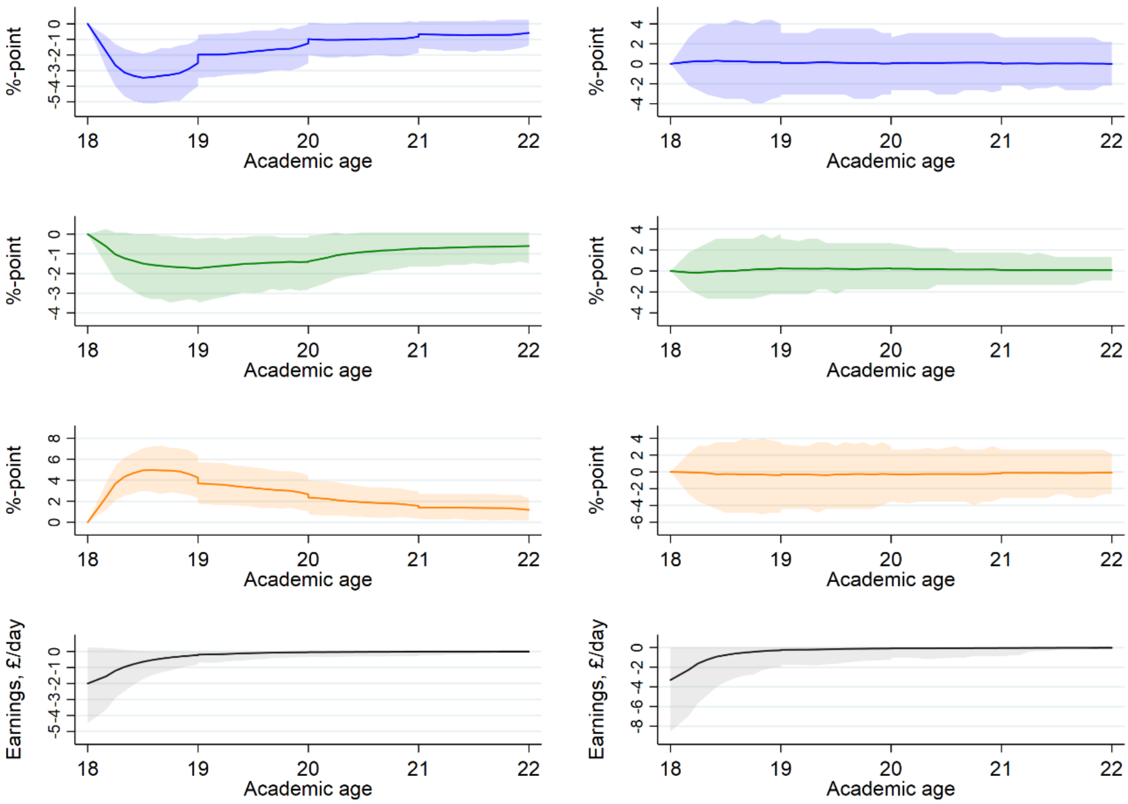

Figure 5: *The impacts of having been in custody immediately before current spell.*

Notes: These charts show the estimated impacts of being in custody rather than education immediately prior to the spell underway on turning 18. Impacts on Education/training, Employment, NEET and earnings (respective row order) are presented for males (left column) and females (right column). Each panel in the chart summarises the result of simulating outcomes and impacts 1,000 times. The lines in the charts show mean impacts estimated across all replications. The shaded areas show percentiles 2.5 and 97.5 of the simulated distribution.

Table 6: Estimates of annual impacts

		Education (months)	Employment (months)	NEET (months)	Earnings (£)
Males					
Age 18:	- Baseline	1.772	1.239	8.989	667
	- Impact	-0.329	-0.157	0.486	-108
	- CI	(-0.458, -0.190)	(-0.302, -0.025)	(0.319, 0.666)	(-194, -32)
Age 19:	- Baseline	1.134	1.314	9.552	784
	- Impact	-0.207	-0.181	0.388	-111
	- CI	(-0.340, -0.090)	(-0.329, -0.049)	(0.217, 0.583)	(-206, -21)
Age 20:	- Baseline	0.81	0.963	10.227	584
	- Impact	-0.117	-0.113	0.23	-68
	- CI	(-0.218, -0.010)	(-0.225, -0.011)	(0.094, 0.384)	(-144, 0)
Age 21:	- Baseline	0.698	0.782	10.521	473
	- Impact	-0.083	-0.078	0.162	-46
	- CI	(-0.178, -0.001)	(-0.164, -0.005)	(0.053, 0.293)	(-111, 5)
Females					
Age 18:	- Baseline	2.29	0.897	8.814	524
	- Impact	0.024	0.004	-0.028	-22
	- CI	(-0.291, 0.344)	(-0.207, 0.262)	(-0.421, 0.304)	(-159, 154)
Age 19:	- Baseline	1.872	0.968	9.159	598
	- Impact	0.01	0.026	-0.036	12
	- CI	(-0.300, 0.324)	(-0.181, 0.291)	(-0.421, 0.324)	(-136, 185)
Age 20:	- Baseline	1.675	0.708	9.617	409
	- Impact	0.011	0.019	-0.03	8
	- CI	(-0.251, 0.302)	(-0.145, 0.225)	(-0.348, 0.260)	(-97, 135)
Age 21:	- Baseline	1.639	0.531	9.829	309
	- Impact	0.003	0.011	-0.014	5
	- CI	(-0.236, 0.251)	(-0.112, 0.150)	(-0.282, 0.256)	(-76, 95)

Notes: For education, employment and NEET, the mean simulated baseline (no prior custody) number of months in that state during the specified age is shown along with the impact of prior custody and the confidence interval around this estimate (percentiles 2.5 and 97.5 of the simulated impact distribution). Unlike Figure 4, earnings are not conditional on someone with prior custody being employed, so the earnings impact in reflects both the employment impact and the impact on amount paid for those in work. 1,000 replications.

4.2.1 Exploring the behavioural channel

As described earlier, the likelihood of the observed impacts arising through the behavioural channel may be greater the longer previously spent in custody. Testing lagged prior custody duration dependence can therefore help understand the role of the behavioural channel.

Table 5 presents the key results from the model that allows for lagged duration dependence (LDD). For males, there is evidence of this in the education to NEET and NEET to custody transitions. The estimated coefficients suggest shorter prior spells in custody increase transitions to NEET by more than longer prior spells. For all other transitions, the null hypothesis of no lagged duration dependence cannot be rejected. Intuitively, under the behavioural channel hypothesis one might expect to find lagged duration dependence in the NEET to employment transition. In fact, no such effect is found. It could also manifest itself in the earnings transition. Here, there is evidence of lagged duration dependence but this is non-monotonic, with spells of 7 months or longer reducing wages more than spells of 1-3 months but less than spells of 4-6 months. This pattern of results does not readily support the behavioural channel interpretation. For females, the smaller number of observations meant that it was not possible to include lagged duration dependence in the transitions from employment. For the remaining transitions, and for earnings too, there was no evidence of lagged duration dependence.

Simulation results show how these effects on specific transitions combine to produce overall outcomes. Figure 6 presents estimates of the impact of the prior custody spell being 4-6 months in duration rather than 1-3 months. The results show no significant effect on any outcome at any age. This is true for both males and females. Results not presented (but available on request) demonstrate a similar finding when comparing spells of 7 months or longer against spells of 4-6 months. Hence, these simulation results confirm the impression from the estimated coefficients that lagged duration dependence does not play an important role. This finding is consistent with Kling (2006) who similarly finds no substantial evidence of a negative effect of incarceration length on employment or earnings. With regard to how they help understand the mechanisms driving the estimated negative impact of prior custody, the results do not suggest an important role for the behavioural channel.

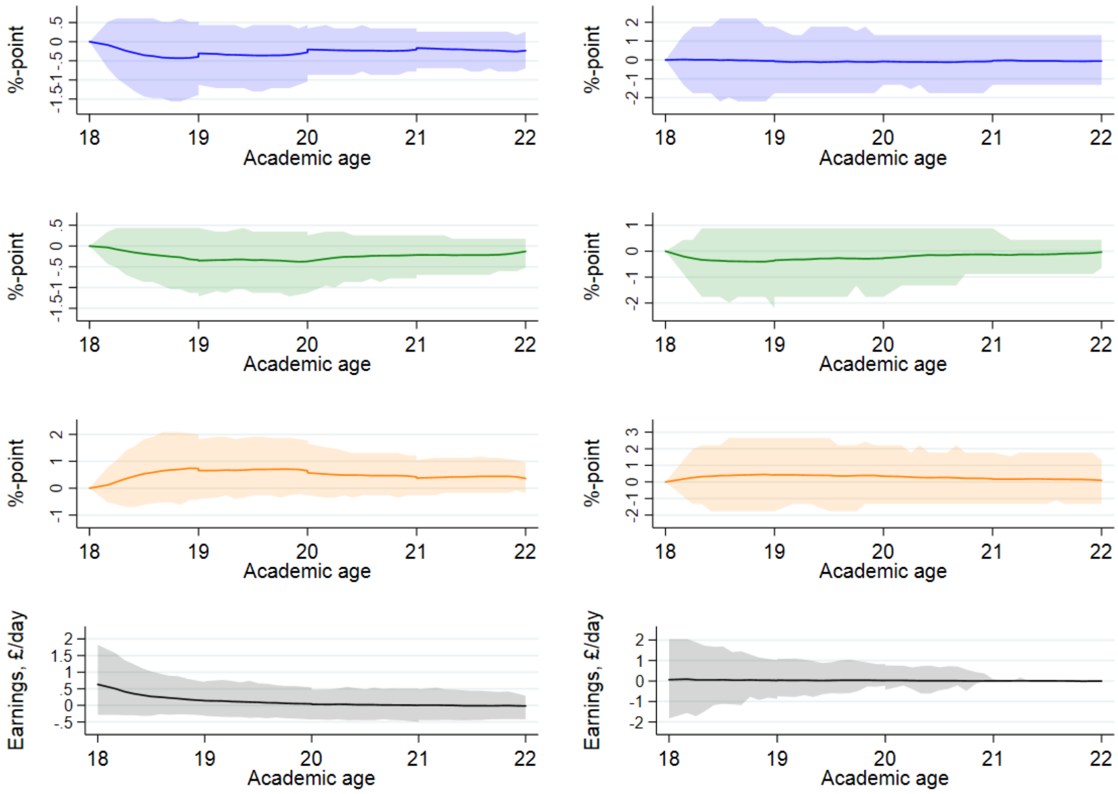

Figure 6: *Sensitivity of the impacts of prior custody to the length of custodial spell.*

Notes: These charts show how the estimated impact of being in custody rather than education immediately prior to the spell underway on turning 18 differs according to whether the custody spell lasted 4-6 months rather than 1-3 months. Impact variation on Education/training, Employment, NEET and earnings (respective row order) are presented for males (left column) and females (right column). Each panel in the chart summarises the result of simulating outcomes and impacts 1,000 times. The lines in the charts show mean impacts estimated across all replications. The shaded areas show percentiles 2.5 and 97.5 of the simulated distribution.

4.2.2 Exploring the labelling channel

The alternative hypothesis we explore is the labelling channel, whereby employers (and others) may discriminate against those who have been in custody. Our test of this is to examine whether the impact of prior custody is greater in loose labour markets (where employers have more freedom to discriminate) than in tight labour markets (where they do not).

Table 5 presents the key results from the model that allows for the interaction of prior custody with local unemployment. We interpret higher unemployment as indicating a looser labour market, with employers able to select from a greater number of applicants. For males, the impacts on transitions between education and NEET in both directions are reduced when unemployment is higher. By contrast, the impact on transitions from employment into custody is greater. Most relevant though is the impact on transitions into employment and on earnings. In neither case does the impact of prior custody vary significantly with local unemployment. For females, the impression is of even less impact variation with unemployment. The only significant coefficient is in the education to NEET transition; like males, this is reduced in higher unemployment areas. Overall, these results provide little immediate support for the labelling hypothesis.

Figure 7 presents simulation results showing how impacts differ when unemployment is fixed at a rate corresponding to the 5th percentile across local authorities (low unemployment) rather than the 95th percentile (high unemployment). For males, the differences are mostly not statistically significant. While this is true across all outcomes, it is marginal in the case of short-term differences in the impact of being NEET. For the first six months, the variation is statistically significant at 10% level. This may be viewed as consistent with a labelling effect since it indicates that prior custody increases the probability of NEET more in high unemployment areas than in low unemployment areas. We take this as weak evidence in support of the signalling hypothesis for males. For females, there is less evidence of a signalling effect. The differences in impacts between low and high unemployment areas are never statistically significant.

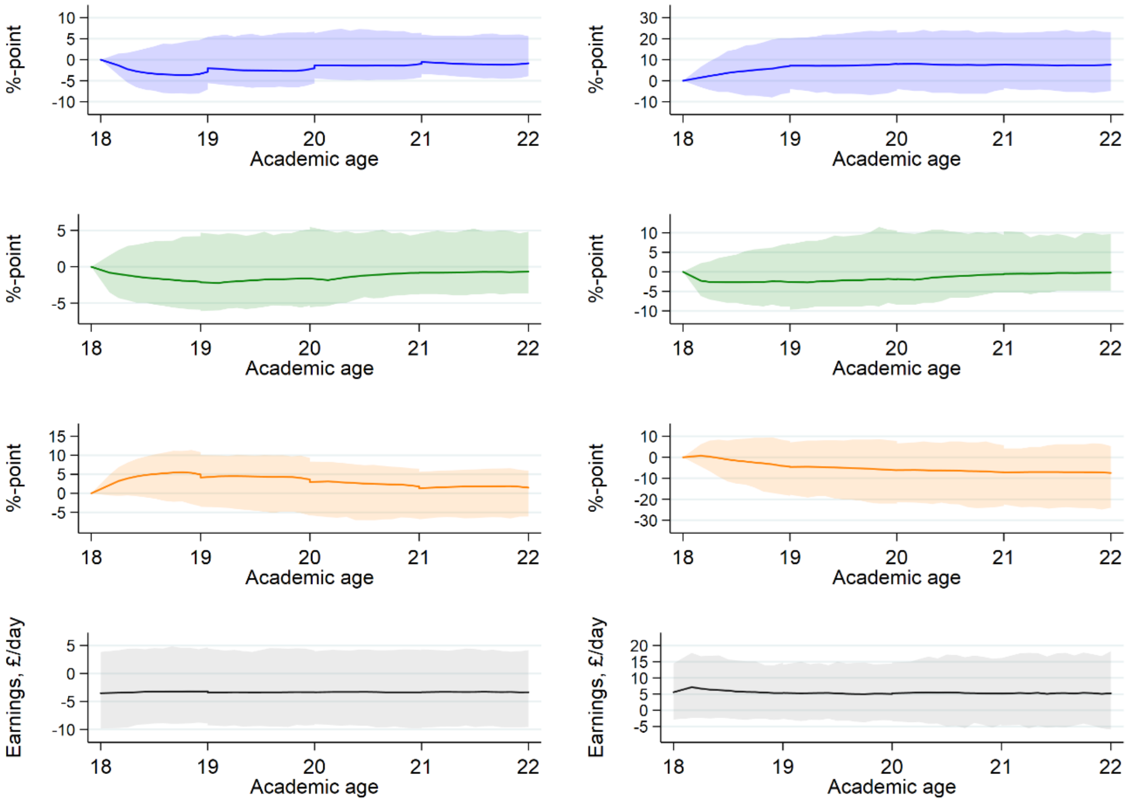

Figure 7: *Sensitivity of the impacts of prior custody to the local unemployment rate.*

Notes: These charts show how the estimated impact of being in custody rather than education immediately prior to the spell underway on turning 18 differs according to whether the individual lives in a low unemployment area or a high unemployment area. Each chart shows the impact in the high unemployment area minus the impact in the low unemployment area. Impact variation on Education/training, Employment, NEET and earnings (respective row order) are presented for males (left column) and females (right column). Each panel in the chart summarises the result of simulating outcomes and impacts 1,000 times. The lines in the charts show mean impacts estimated across all replications. The shaded areas show percentiles 2.5 and 97.5 of the simulated distribution.

5 Conclusion

Looked after children constitute one of the most vulnerable and marginalised groups in society. Relative to other children, they have a particularly bad start in life and are more likely to have poor prospects as adults. Some children find themselves in trouble with the law and will be incarcerated as a result. This paper has concerned itself with estimating the impact of this incarceration on outcomes as a young adult. It provides novel evidence on the extent to which custody imposes a longer-term penalty on a group of already-disadvantaged young people.

The impacts differ by gender. For males, custody reduces employment up to age 21 by more than 10%. This causes earnings to fall, but there is also an indication that custody reduces pay among those who find work. For females, there is no evidence of an impact on employment or on earnings as a whole. As with males, custody reduces pay among those in work but with females this impact is statistically significant. It is also sizeable, amounting to a 25% reduction.

This is the first study we are aware of that is able to produce separate results for males and females and it is perhaps to be expected that it raises questions worthy of further investigation. It is unclear why there should be such a difference. Motherhood might offer one explanation if it reduces employment among young women such that their NEET status is more fixed than that of young men, who may be looking for and available for work. However, this is not discernible from the available data. The strong negative impact on earnings for those entering employment suggests that prior custody reduces the quality of jobs that females are able to access.

To try to understand the mechanism driving the estimated impact for males, we tested the importance of the length of time spent in custody and of local economic circumstances. The first of these tests is intended to be informative of whether impacts are likely to have arisen through changed behaviour while the second is informative of whether impacts appear to be the result of employer discrimination. The results provide perhaps more support for the second interpretation than for the first but neither test provides a strong conclusion. There are perhaps two points to bear in mind when assessing this finding. First, it may be that looked after children's experiences and characteristics already shape their behaviour or draw discrimination to the extent that the marginal impact of custody in these regards is negligible. Second, it may be that the tests themselves are too blunt. For instance, some aspects of individuals' behaviour or characteristics may be influenced simply by the experience of custody rather than its duration, reducing the extent to which length of prior custody spell is informative of the behavioural channel. With respect to the labelling channel, the test considered in this paper is perhaps handicapped by the the use of local unemployment rate as a proxy for labour market tightness. Ideally, the ratio of vacancies to unemployment would instead be used; however, vacancy data at the local level do not exist. Hence, while the tests do not provide strong support for either the behavioural or labelling interpretations, the limitations of the tests means that the lack of such support does not confirm that these channels are not important.

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Appendix A Glossary

Special educational needs : pupils are classified as having special educational needs (SEN) if they have learning difficulties or disabilities that hinder their learning compared to most children of their age. Additional support is provided by schools and, in some cases, external services. At the time the four cohorts were at school, the 2001 SEN code of practice was in force which distinguished between three categories of SEN depending on the level of support required:

- School Action: additional support provided by the school (e.g. use of additional teachers, specialist equipment or different materials)
- School Action Plus: As above, plus additional support from external services (e.g. a speech and language therapist)
- Statement: A legal document which sets out a pupil's educational needs and how they will be addressed

Schools record the primary and secondary SEN types of pupils. Note that primary here means most significant (rather than referring to primary schools). The most common types when the cohorts studied in this report were at school were behavioural, emotional and social difficulties (BESD), moderate learning difficulties (MLD) and speech, language and communication needs (SLCN).

Special schools : Schools that provide education for pupils with SEN or a disability. Almost all pupils in the four cohorts observed who attended a special school had a statement of SEN.

Pupil referral unit : Pupil referral units (PRU), including alternative provision free schools and academies, are educational settings attended by pupils who cannot go to a mainstream school. Reasons typically include permanent exclusion, short-term interventions to improve behaviour, refusal, illness or unavailability of a mainstream school place.

Local authority alternative provision : Education commissioned by local authorities outside the state-funded school sector. This includes independent (private) schools (including independent alternative provision and special schools), further education colleges and tuition.

Free school meals : Pupils of compulsory school age are eligible for free school meals if their parents/ carers a) apply for them and b) receive certain means-tested benefits. Eligibility criteria have changed over time.

Appendix B Full estimation results

Table B1: Full estimation results, males

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to w$	$c \to n$	$w \to e$	$w \to c$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	*Earnings*
Preceding state:													
- custody	1.732	-0.043	0.392				-2.205	2.686	0.157	-0.434	0.649	-0.368	0.29
	[0.137]	[0.223]	[0.078]				[1.020]	[0.609]	[0.246]	[0.088]	[0.134]	[0.135]	[0.165]
- employment	-0.029	1.179	-0.074	-1.561	1.192	0.243				-0.541	-0.182	0.919	
	[0.436]	[0.100]	[0.073]	[0.721]	[0.748]	[0.316]				[0.069]	[0.263]	[0.051]	
- NEET	0.709	0.257	0.433	-0.852	-0.306	0.508	-0.934	0.118	0.281				0.091
	[0.118]	[0.088]	[0.043]	[0.124]	[0.446]	[0.096]	[0.100]	[0.478]	[0.059]				[0.088]
Age:													
- 13	-0.278		-2.902	1.044		-0.616				2.803	0.5		
	[0.255]		[0.139]	[0.242]		[0.312]				[0.162]	[0.372]		
- 14	-0.174		-2.861	0.898		-0.571				2.82	1.037		
	[0.238]		[0.135]	[0.204]		[0.208]				[0.154]	[0.234]		
- 15	0.031		-2.873	0.702		-0.425			-1.263	2.179	1.206		
	[0.229]		[0.142]	[0.195]		[0.167]			[0.421]	[0.153]	[0.197]		
- 16	0.194	3.585	-0.84	0.154		-0.335	0.77		-1.115	2.166	0.553	0.407	0.661
	[0.146]	[0.220]	[0.113]	[0.139]		[0.099]	[0.185]		[0.108]	[0.132]	[0.108]	[0.105]	[0.105]
- 17		3.68	-0.316				0.506		-0.275	1.79		0.649	0.346
		[0.228]	[0.110]				[0.172]		[0.078]	[0.127]		[0.082]	[0.091]
- 18		3.811	0.079				-0.031		-0.24	0.972		0.57	0.186
		[0.239]	[0.107]				[0.178]		[0.071]	[0.126]		[0.073]	[0.083]
- 19		3.603	0.096				-0.413		-0.04	0.61		0.393	0.012
		[0.265]	[0.117]				[0.205]		[0.072]	[0.133]		[0.075]	[0.088]
White	-0.375	-0.038	0.124	0.161	0.438	0.128	-0.155	1.4	-0.026	-0.049	-0.178	-0.019	-0.112
	[0.117]	[0.090]	[0.046]	[0.116]	[0.545]	[0.111]	[0.117]	[1.033]	[0.065]	[0.052]	[0.138]	[0.067]	[0.085]
English as additional language	0.206	-0.398	-0.315	-0.087	0.273	0.082	-0.146		-0.352	-0.081	0.567	-0.172	0.225
	[0.216]	[0.169]	[0.087]	[0.202]	[0.896]	[0.180]	[0.222]		[0.141]	[0.099]	[0.260]	[0.140]	[0.175]
Free school meals	-0.012	0.095	0.072	0.028	-0.243	0.017	-0.06	0.011	0.05	-0.071	-0.007	0.043	0.028

Table B1: (continued)

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to w$	$c \to n$	$w \to e$	$w \to c$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	$Earnings$
	[0.102]	[0.071]	[0.035]	[0.105]	[0.494]	[0.095]	[0.091]	[0.477]	[0.048]	[0.041]	[0.116]	[0.050]	[0.066]
Special educational needs status:													
- School action	-0.033	-0.01	-0.034	0.187	-0.617	-0.088	0.271	0.03	-0.052	-0.026	-0.367	0.095	-0.157
	[0.148]	[0.087]	[0.045]	[0.148]	[0.775]	[0.141]	[0.106]	[0.665]	[0.061]	[0.053]	[0.166]	[0.062]	[0.082]
- School action plus	-0.033	-0.078	-0.059	0.039	-0.121	-0.041	0.081	0.649	0.043	0.096	0.002	0.052	-0.173
	[0.132]	[0.099]	[0.047]	[0.139]	[0.690]	[0.124]	[0.126]	[0.628]	[0.066]	[0.054]	[0.148]	[0.068]	[0.089]
- Statemented	-0.168	-0.126	-0.093	0.263	-1.233	0.194	0.121	-0.025	0.205	0.113	-0.345	-0.08	-0.227
	[0.173]	[0.121]	[0.059]	[0.188]	[0.921]	[0.172]	[0.158]	[0.828]	[0.083]	[0.068]	[0.198]	[0.086]	[0.110]
Special educational needs type:													
- behavioural, emotional, social difficulties	0.181	0.182	0.042	-0.004	0.847	-0.088	0.034	-0.035	-0.08	-0.011	0.164	0.093	0.121
	[0.136]	[0.096]	[0.046]	[0.148]	[0.718]	[0.132]	[0.124]	[0.590]	[0.067]	[0.054]	[0.160]	[0.068]	[0.092]
- moderate learning difficulties	-0.567	-0.217	-0.076	0.366	0.42	0.07	0.151		0.129	0.039	-0.283	-0.075	0.463
	[0.225]	[0.141]	[0.064]	[0.239]	[1.103]	[0.231]	[0.172]		[0.096]	[0.075]	[0.247]	[0.097]	[0.132]
Child in need	0.678	-0.52	0.535										
	[0.460]	[0.855]	[0.183]										
Looked after	0.598	1.473	-0.212										
	[0.227]	[0.703]	[0.099]										
Non-mainstream schooling:													
- pupil referral unit	0.451	-0.084	0.185	-0.059	-0.997	0.191	-0.043	-0.021	0.106	-0.186	0.515	-0.028	-0.002
	[0.100]	[0.076]	[0.036]	[0.106]	[0.516]	[0.094]	[0.096]	[0.506]	[0.050]	[0.043]	[0.116]	[0.051]	[0.067]
- special school	0.465	-0.358	0.113	-0.199	0.001	-0.158	-0.082	0.634	-0.052	-0.162	0.361	-0.112	0.3
	[0.151]	[0.114]	[0.053]	[0.149]	[0.690]	[0.141]	[0.147]	[0.770]	[0.079]	[0.062]	[0.170]	[0.081]	[0.104]

Table B1: (continued)

	$e \rightarrow c$	$e \rightarrow w$	$e \rightarrow n$	$c \rightarrow e$	$c \rightarrow w$	$c \rightarrow n$	$w \rightarrow e$	$w \rightarrow c$	$w \rightarrow n$	$n \rightarrow e$	$n \rightarrow c$	$n \rightarrow w$	$Earnings$
- alternative provision	0.383	-0.294	-0.091	-0.164	0.614	-0.002	0.038	1.189	-0.009	-0.09	0.242	-0.18	0.041
	[0.103]	[0.086]	[0.039]	[0.112]	[0.476]	[0.100]	[0.113]	[0.495]	[0.058]	[0.047]	[0.124]	[0.059]	[0.077]
Permanent exclusion	0.53	0.141	0.098	-0.087	0.636	-0.164	-0.122	-0.423	0.04	-0.126	0.186	-0.171	-0.021
	[0.133]	[0.112]	[0.053]	[0.126]	[0.478]	[0.119]	[0.152]	[0.699]	[0.074]	[0.061]	[0.155]	[0.078]	[0.096]
Post 16 qualifications:													
- below level 1	-0.206	-0.019	0.008		0.243		-0.039		0.006	0.008	-0.017	0.051	-0.096
	[0.156]	[0.079]	[0.040]		[0.510]		[0.094]		[0.055]	[0.045]	[0.120]	[0.055]	[0.066]
- above level 2	-0.124	0.097	0.015		1.451		-0.365		-0.002	-0.049	-0.292	0.122	-0.201
	[0.216]	[0.099]	[0.051]		[0.468]		[0.121]		[0.059]	[0.058]	[0.182]	[0.059]	[0.075]
Local authority unemploymenr rate	0.026	-0.076	0.013	0.01	0.14	-0.009	-0.005	-0.041	0.057	0.105	0.145	-0.101	0.01
	[0.045]	[0.025]	[0.012]	[0.042]	[0.113]	[0.031]	[0.031]	[0.142]	[0.019]	[0.013]	[0.035]	[0.019]	[0.023]
Baseline hazard (transitions):													
- month 1							0.74		0.67	1.193		0.938	
							[0.125]		[0.085]	[0.081]		[0.084]	
- month 2							0.717		0.722	1.238		1.063	
							[0.133]		[0.087]	[0.081]		[0.084]	
- month 3							0.563		0.639	0.918		0.736	
							[0.149]		[0.092]	[0.086]		[0.094]	
- months 1-3				0.283		0.074					0.296		
				[0.123]		[0.104]					[0.093]		
- months 4-6				0.613		0.283	0.4		0.351	0.825		0.52	
				[0.125]		[0.109]	[0.128]		[0.085]	[0.075]		[0.078]	
- months 7-12									0.083	0.63		0.318	
									[0.088]	[0.072]		[0.074]	

Table B1: (continued)

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to w$	$c \to n$	$w \to e$	$w \to c$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	*Earnings*
Baseline hazard (earnings):													
- decile 2													0.221
													[0.094]
- decile 3													0.402
													[0.095]
- decile 4													0.602
													[0.097]
- decile 5													0.808
													[0.100]
- decile 6													1.166
													[0.102]
- decile 7													1.491
													[0.111]
- decile 8													2.019
													[0.120]
- decile 9													2.634
													[0.132]
Seasonal and monthly dummies:													
- June, age 15		3.299	2.469										
		[0.296]	[0.120]										
- July, age 15		3.403	1.556										
		[0.290]	[0.132]										
- August, age 15		3.789	2.044										
		[0.282]	[0.110]										
- June, age 17		0.237	0.719										
		[0.245]	[0.104]										

Table B1: (continued)

	$e\rightarrow c$	$e\rightarrow w$	$e\rightarrow n$	$c\rightarrow e$	$c\rightarrow w$	$c\rightarrow n$	$w\rightarrow e$	$w\rightarrow c$	$w\rightarrow n$	$n\rightarrow e$	$n\rightarrow c$	$n\rightarrow w$	*Earnings*
- July, age 17		0.117	0.61										
		[0.240]	[0.101]										
- August, age 17		0.271	0.104										
		[0.257]	[0.106]										
- June		0.236	0.289										
		[0.165]	[0.069]										
- July		0.268	0.648										
		[0.162]	[0.063]										
- August		0.258	1.173										
		[0.170]	[0.055]										
- September				0.523			0.439			0.015			
				[0.143]			[0.155]			[0.063]			
- quarter 1		0.224			0.234		-0.232		-0.078	0.065		-0.042	-0.141
		[0.104]			[0.535]		[0.117]		[0.061]	[0.058]		[0.064]	[0.074]
- quarter 2		0.287			-0.151		-0.443		-0.083	-0.262		0.102	-0.265
		[0.117]			[0.584]		[0.125]		[0.064]	[0.065]		[0.067]	[0.075]
- quarter 3		0.573			0.025		-0.153		0.194	0.53		0.237	-0.193
		[0.124]			[0.554]		[0.128]		[0.057]	[0.057]		[0.061]	[0.068]
Constant	-5.392	-9.577	-2.142	-3.549	-7.059	-2.891	-3.551	-7.799	-2.026	-5.934	-4.129	-5.354	-3.12
	[0.518]	[0.870]	[0.223]	[0.245]	[0.845]	[0.201]	[0.576]	[1.251]	[0.194]	[0.162]	[0.266]	[0.180]	[0.259]
Log of mass points													
- $\ln(\upsilon_2)$	-2.014	0.925	-1.078	0.444	0.354	0.063	0.548		-0.723	0.704	-1.964	1.037	0.007
	[0.205]	[0.474]	[0.091]	[0.194]	[1.001]	[0.274]	[0.524]		[0.141]	[0.126]	[0.374]	[0.177]	[0.251]
- $\ln(\upsilon_3)$	-2.293	1.045	-0.409	0.565	0.916	0.259	0.17		-0.326	0.3	-2.128	0.64	1.231
	[0.209]	[0.482]	[0.084]	[0.192]	[0.828]	[0.224]	[0.526]		[0.134]	[0.100]	[0.218]	[0.170]	[0.236]
Probability of mass points (logistic transform)													

Table B1: (continued)

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to w$	$c \to n$	$w \to e$	$w \to c$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	*Earnings*
- λ_2	0.452												
	[0.191]												
- λ_3	1.045												
	[0.163]												
Resulting probabilities													
- p^1	0.185												
- p^2	0.29												
- p^3	0.525												
Log-likelihood	-68,465.94												
N (rounded to nearest 10)	2,980												

Standard errors in brackets.

Table B2: Full estimation results, females

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to n$	$w \to e$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	*Earnings*
Preceding state:											
- custody	1.707	-0.156	0.19			0.752	-0.022	0.033	1.488	0.127	0.618
	[0.273]	[0.391]	[0.135]			[0.760]	[0.734]	[0.131]	[0.294]	[0.309]	[0.311]
- employment	0.183	1.165	-0.055					-0.443	-0.559	0.795	
	[1.035]	[0.137]	[0.103]					[0.098]	[1.032]	[0.116]	
- NEET	0.574	0.338	0.351	-0.988	0.659	-0.696	0.383				-0.011
	[0.207]	[0.125]	[0.065]	[0.267]	[0.209]	[0.139]	[0.092]				[0.119]
Age:											
-13	1.212		-2.335	0.284	-1.207			2.601	1.99		
	[0.637]		[0.172]	[0.472]	[0.461]			[0.199]	[0.643]		
-14	1.481		-2.324	0.31	-0.964			2.657	2.062		
	[0.615]		[0.165]	[0.373]	[0.312]			[0.182]	[0.539]		
-15	0.77		-2.445	0.455	-0.677		-1.16	2.168	1.667		
	[0.618]		[0.177]	[0.357]	[0.280]		[0.729]	[0.179]	[0.527]		
-16	0.76	2.881	-0.676	0.426	-0.446	0.682	-0.873	1.671	0.901	0.986	1.033
	[0.432]	[0.277]	[0.131]	[0.356]	[0.268]	[0.234]	[0.177]	[0.147]	[0.340]	[0.180]	[0.163]
-17		3.113	-0.06			0.458	0.076	1.212		0.916	0.398
		[0.283]	[0.127]			[0.221]	[0.133]	[0.140]		[0.151]	[0.148]
-18		3.003	0.172			0.02	0.049	0.545		0.751	-0.112
		[0.294]	[0.127]			[0.232]	[0.128]	[0.141]		[0.140]	[0.146]
-19		2.671	0.099			0.044	-0.048	0.217		0.369	-0.202
		[0.330]	[0.144]			[0.254]	[0.138]	[0.153]		[0.150]	[0.158]
White	-0.272	-0.079	0.117	0.176	0.196	-0.128	0.248	-0.313	0.165	-0.385	-0.085
	[0.211]	[0.125]	[0.068]	[0.248]	[0.267]	[0.147]	[0.113]	[0.070]	[0.316]	[0.121]	[0.118]
English as additional language	0.33	0.151	-0.088	-0.175	-0.698	0.11	-0.145	-0.114	0.361	-0.073	-0.485
	[0.345]	[0.197]	[0.116]	[0.387]	[0.452]	[0.228]	[0.204]	[0.127]	[0.476]	[0.215]	[0.204]
Free school meals	0.324	-0.163	0.125	-0.075	-0.33	0.206	0.002	0.008	0.203	-0.105	-0.049

Table B2: (continued)

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to n$	$w \to e$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	*Earnings*
	[0.165]	[0.101]	[0.049]	[0.198]	[0.212]	[0.120]	[0.084]	[0.054]	[0.246]	[0.093]	[0.100]
Special educational needs status:											
- School action	-0.105	-0.069	0.056	0.07	0.037	0.307	0.139	0.143	-0.028	-0.037	0.028
	[0.202]	[0.105]	[0.054]	[0.237]	[0.247]	[0.126]	[0.090]	[0.060]	[0.288]	[0.100]	[0.105]
- School action plus	-0.332	-0.363	-0.011	0.39	0.489	0.188	-0.069	0.096	-0.095	-0.035	0.101
	[0.225]	[0.141]	[0.066]	[0.272]	[0.274]	[0.160]	[0.119]	[0.074]	[0.313]	[0.130]	[0.149]
- Statemented	-0.019	-0.179	0.034	0.346	0.255	0.471	-0.171	0.013	-0.181	-0.195	0.105
	[0.326]	[0.203]	[0.100]	[0.395]	[0.377]	[0.249]	[0.187]	[0.110]	[0.473]	[0.202]	[0.230]
Special educational needs type:											
- behavioural, emotional, social difficulties	-0.172	0.222	0.15	-0.405	-0.471	-0.04	0.058	-0.115	-0.25	-0.034	0.107
	[0.250]	[0.143]	[0.068]	[0.298]	[0.289]	[0.164]	[0.122]	[0.076]	[0.342]	[0.132]	[0.148]
- moderate learning difficulties	-0.364	-0.288	0.336	-0.628	-0.143	-0.22	0.189	-0.088	0.42	-0.344	0.526
	[0.409]	[0.248]	[0.096]	[0.529]	[0.373]	[0.292]	[0.189]	[0.103]	[0.421]	[0.197]	[0.287]
Child in need			-0.153								
			[0.227]								
Looked after	1.705		0.266								
	[0.428]		[0.148]								
Non-mainstream schooling:											
- pupil referral unit	0.352	0.168	0.254	0.059	-0.067	-0.288	0.042	-0.127	-0.24	-0.173	-0.077
	[0.173]	[0.101]	[0.050]	[0.208]	[0.219]	[0.128]	[0.085]	[0.056]	[0.261]	[0.092]	[0.098]
- special school	0.598	-0.568	-0.188	-0.408	0.628	-0.137	0.485	-0.013	0.05	-0.335	-0.071
	[0.329]	[0.251]	[0.109]	[0.392]	[0.370]	[0.302]	[0.200]	[0.121]	[0.531]	[0.228]	[0.209]

Table B2: (continued)

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to n$	$w \to e$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	*Earnings*
- alternative provision	0.378	-0.311	-0.126	0.12	-0.568	-0.241	0.017	-0.287	0.69	-0.32	0.05
	[0.193]	[0.140]	[0.065]	[0.233]	[0.260]	[0.184]	[0.111]	[0.079]	[0.299]	[0.125]	[0.127]
Permanent exclusion	0.656	0.029	0.136	-0.337	0.268	0.036	0.312	0.142	0.463	-0.006	-0.209
	[0.209]	[0.159]	[0.077]	[0.254]	[0.254]	[0.215]	[0.125]	[0.082]	[0.342]	[0.143]	[0.160]
Post 16 qualifications:											
- below level 1	-0.094	0.075	0.059			-0.195	-0.001	0.087	-0.375	-0.045	-0.18
	[0.372]	[0.113]	[0.058]			[0.132]	[0.092]	[0.065]	[0.363]	[0.099]	[0.101]
- above level 2	-1.63	0.305	-0.021			-0.048	-0.085	0.105	-0.053	0.299	-0.189
	[1.038]	[0.139]	[0.076]			[0.147]	[0.100]	[0.084]	[0.484]	[0.109]	[0.119]
Local authority unemploymenr rate	-0.017	-0.009	0.022			-0.032	0.016	0.108	0.067	-0.092	-0.07
	[0.118]	[0.037]	[0.019]			[0.044]	[0.033]	[0.020]	[0.099]	[0.035]	[0.038]
Baseline hazard (transitions):											
- month 1						0.815	0.789	0.97		1.167	
						[0.171]	[0.151]	[0.100]		[0.155]	
- month 2						0.96	0.826	0.915		1.224	
						[0.173]	[0.155]	[0.103]		[0.157]	
- month 3						0.754	0.742	0.894		1.073	
						[0.194]	[0.164]	[0.107]		[0.165]	
- months 1-3				-0.871							
				[0.219]							
- months 4-6				-0.266		0.446	0.539	0.663		0.831	
				[0.212]		[0.174]	[0.148]	[0.094]		[0.142]	
- months 7-12							0.387	0.35		0.369	
							[0.149]	[0.090]		[0.140]	

Table B2: (continued)

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to n$	$w \to e$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	*Earnings*
Baseline hazard (earnings):											
- decile 2											0.252
											[0.155]
- decile 3											0.788
											[0.153]
- decile 4											0.921
											[0.167]
- decile 5											1.224
											[0.176]
- decile 6											1.631
											[0.181]
- decile 7											2.079
											[0.184]
- decile 8											2.426
											[0.191]
- decile 9											3.077
											[0.196]
Seasonal and monthly dummies:											
- June, age 15		2.774	1.996								
		[0.406]	[0.164]								
- July, age 15		3.836	1.128								
		[0.377]	[0.186]								
- August, age 15		3.758	1.691								
		[0.367]	[0.159]								
- June, age 17		-0.152	0.614								
		[0.339]	[0.140]								

Table B2: (continued)

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to n$	$w \to e$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	*Earnings*
- July, age 17		-0.119	0.689								
		[0.411]	[0.138]								
- August, age 17		0.612	0.309								
		[0.330]	[0.155]								
- June		0.265	0.581								
		[0.207]	[0.088]								
- July		-0.053	0.855								
		[0.249]	[0.083]								
- August		0.178	1.187								
		[0.240]	[0.079]								
- September				0.055		-0.083		0.038			
				[0.307]		[0.211]		[0.080]			
- quarter 1		0.279				-0.251	0.103	0.017		0.047	-0.304
		[0.146]				[0.164]	[0.105]	[0.091]		[0.120]	[0.125]
- quarter 2		0.576				-0.14	0.104	-0.218		0.327	-0.533
		[0.154]				[0.159]	[0.107]	[0.099]		[0.119]	[0.120]
- quarter 3		0.658				0.235	0.289	0.911		0.416	-0.459
		[0.171]				[0.159]	[0.100]	[0.080]		[0.110]	[0.113]
Constant	-9.85	-6.968	-3.489	-2.549	-1.847	-3.298	-3.447	-4.714	-7.596	-4.308	-2.862
	[0.905]	[0.267]	[0.267]	[0.695]	[0.511]	[0.304]	[0.229]	[0.200]	[0.740]	[0.264]	[0.236]
Log of mass points											
- $\ln(\upsilon_2)$	1.831	-1.372	0.981	0.457	-0.362	0.215	0.582	-0.545	1.206	-1.334	0.013
	[0.504]	[0.301]	[0.104]	[0.602]	[0.394]	[0.317]	[0.167]	[0.127]	[0.540]	[0.211]	[0.224]
- $\ln(\upsilon_3)$	0.698	-0.117	0.535	0.149	0.071	-0.254	0.426	-0.089		-0.75	1.756
	[0.690]	[0.255]	[0.124]	[0.967]	[0.565]	[0.213]	[0.130]	[0.212]		[0.216]	[0.195]
Probability of mass points (logistic transform)											

Table B2: (continued)

	$e \to c$	$e \to w$	$e \to n$	$c \to e$	$c \to n$	$w \to e$	$w \to n$	$n \to e$	$n \to c$	$n \to w$	*Earnings*
- λ_2	0.359										
	[0.304]										
- λ_3	-0.268										
	[0.408]										
Resulting probabilities											
- p^1	0.313										
- p^2	0.448										
- p^3	0.239										
log-likelihood	-29,249.15										
N (rounded to nearest 10)	1,390										

Standard errors in brackets.