Youth custody: Educational influences and labour market consequences

Richard Dorsett

with

Alex Bowyer, Emma Gorman, Greta Morando, Veruska Oppedisano, Min Zhang, David Thomson, Claire Cathro, Giulia Tagliaferri, Alex Sutherland, Matt Dickson, Stephen Machin, Sandra McNally and Jenifer Ruiz-Valenzuela

January 2023

Acknowledgments

We thank members of our advisory group for their time and expert advice on the research as it evolved: Tim Bateman (University of Bedfordshire), Chris Goulden (Youth Futures Foundation), David Dawson (Ministry of Justice), Helen Dyson (NACRO), Millie Harris (Alliance for Youth Justice), Neil Harrison (University of Oxford), Di Hart, Tim Leunig (HM Treasury), Osama Rahman (Department for Education), Sophie Riley (Youth Justice Board) and Will Teager (Youth Endowment Fund).

We are grateful to the Nuffield Foundation for funding this research. Our particular thanks go to Ash Patel for his guidance and support throughout.

The Nuffield Foundation is an independent charitable trust with a mission to advance social well-being. It funds research that informs social policy, primarily in Education, Welfare, and Justice. It also funds student programmes that provide opportunities for young people to develop skills in quantitative and scientific methods. The Nuffield Foundation is the founder and co-funder of the Nuffield Council on Bioethics and the Ada Lovelace Institute. The Foundation has funded this project, but the views expressed are those of the authors and not necessarily the Foundation. Visit <u>www.nuffieldfoundation.org</u>.



This work was produced using statistical data from ONS. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates. We thank the Department for Education for providing access to the NPD and LEO data and particularly Dave Burnett and Philippa Norgrove for their support throughout.

This project brought together teams from several institutions to undertake a programme of research into the educational influences and labour market consequences of youth custody. This comprised six discrete studies, as follows:

- The school to work transition for young people who experience custody Alex Bowyer and Richard Dorsett (University of Westminster) and David Thomson (FFT Education Datalab)
- School exclusions and youth custody Claire Cathro, Giulia Tagliaferri and Alex Sutherland (Behavioural Insights Team)
- School peers' ability and youth custody Richard Dorsett, Emma Gorman, Veruska Oppedisano and Min Zhang (University of Westminster); Greta Morando (UCL)
- The effect of education participation on youth custody: causal evidence from England – Matt Dickson (University of Bath)
- School qualifications and youth custody Stephen Machin (LSE), Sandra McNally University of Surrey) and Jenifer Ruiz-Valenzuela (University of Barcelona)
- The impact of incarceration on the early labour market outcomes of children in care – Richard Dorsett and David Thomson

UNIVERSITY OF WESTMINSTER[#]







Centre for Economic Performance



File education datalab

Contents

Executive summary	2
A. Introduction	6
B. Data	9
C. Summary of research projects10	С
1. The school to work transition for young people who experience custody1	1
2. School exclusions and youth custody18	3
3. School peers' ability and youth custody24	4
4. The effect of education participation on youth custody: causal evidence from England28	3
5. School qualifications and youth custody	5
6. The impact of incarceration on the early labour market outcomes of children in care4	3
D. Implications of the findings	С
References	2

Executive summary

This report provides a summary of research conducted as part of the Nuffield Foundation – funded study *Youth custody: Educational influences and labour market consequences.*¹ The study uses linked administrative data for children in England to describe the key pathways into and out of custody, to estimate how aspects of schooling affect the probability of being imprisoned and, beyond school, estimate the impact of prison on subsequent labour market outcomes. The aim of this report is to provide an overview of the research, drawing out the key findings and distilling the main messages for policy. For full detail, readers should refer to the full papers available on the <u>project website</u>.

The number of children in custody is small and has declined markedly over the past decade. Over the years considered in this study, there were roughly 1,000 children in custody at any one time and there are currently fewer than 500 (Youth Custody Service, 2022). This represents only a fraction of those having some interaction with the criminal justice system. In 2020/21, 15,800 children were cautioned or sentenced but only 670 were sentenced to custody (Youth Justice Board, 2022). Youth custody is heavily concentrated among the most disadvantaged young people and this has been accentuated as the population has narrowed to a hard-core of individuals with the most challenging problems. This raises concerns around equity, particularly if being incarcerated might itself compound disadvantage by worsening labour market prospects and other outcomes. In addition, youth custody imposes significant societal costs. Crime itself of course has potentially long-lasting effects on its victims. The processing of children who have committed offences also imposes substantial public costs, particularly with regard to accommodating those children in custody.

The research summarised in this report uses population-level administrative data to provide a deeper understanding of some of the causes and consequences of youth custody. The National Pupil Database (NPD) provides information on school experiences and on youth custody. Some of the research links this to the Longitudinal Education Outcomes (LEO) data, which provides information on employment, earnings and benefit receipt. The resulting NPD-LEO data allows individuals to be tracked beyond school. We show how custody features in young people's experiences and how it has a lasting effect on labour market prospects. A particular focus is on how aspects of schooling can influence the probability of custody. Understanding this is important as a means of identifying interventions and practices that may help reduce the extent of youth custody at source.

The data used has particular strengths that permit the research to make new and important contributions to the evidence base. First, its large size allows the highly-selected group of children who experience custody to be observed in full and considered in detail at an individual level, alongside all other children. Second, the NPD contains definitive records on aspects of children's experiences at school and rich background information on pupil characteristics. Third, individuals' experiences are observed beyond compulsory schooling, allowing longer-term outcomes to be revealed and longer-term impacts to be estimated.

¹ Grant reference number: JUS/43820

The study involved six component analyses:

1. The school to work transition for young people who experience custody

We use NPD-LEO data to characterise the pathways followed by young people in England who experience custody. We identify a typology of pathways up to age 18 and a separate typology covering ages 19-22. Our results confirm the generally poor prospects among this group, showing 80 per cent to be firmly established as not in employment, education or training (NEET) by age 22. Despite the high level of deprivation in the population considered, prospects are still found to vary with specific markers of disadvantage. Managing to avoid NEET when 16-18 is an important part of the strategy for avoiding NEET when older. This suggests the importance of policy interventions aimed at re-engagement of those who experience custody as a young person.

2. School exclusions and youth custody

There is a strong relationship between being excluded from school and experiences of criminality and custody later in life. We examine the extent to which school exclusion has a causal impact on the likelihood of entering youth custody, considering both permanent removal and temporary removal from a school. The analysis is based on NPD and estimates the effect of being permanently excluded or temporarily suspended in Year 10 (when aged 14-15) on the probability of experiencing custody at age 15-17. It does this by exploiting the fact that academisation – a maintained school converting to an academy – increased the likelihood of exclusion. Assuming academisation does not in itself increase the likelihood of custody, this increase in exclusions can be viewed as essentially random, allowing us to assess its impact on custody. We find that exclusion/suspension increases the probability of custody by 1.3 percentage points. Against the custody rate of 0.1% among pupils as a whole, this constitutes a sizeable increase. However, the estimated impact relates to children excluded as a result of academisation; their expected custody rate in the absence of academisation is not observed but is likely to be higher than that seen among pupils as a whole.

3. School peers' ability and youth custody

Previous research has provided evidence that pupils' attainment is increased when they have high-attaining peers. We explore a related question: whether peer ability affects the probability of youth custody. Schools may be an important setting for the onset of criminal behaviour yet little is known about this relationship. We use an estimation approach that aims to control for self-selection into peer groups and also the bi-directional relationship between pupils and their peers. Our results are consistent across a range of estimation strategies. We estimate that increasing to the median the fraction of peers with at least grade C in maths at KS4 would reduce youth custody by 14% (roughly, from a baseline of 0.1% to 0.08%). The effect is concentrated among pupils in schools where other pupils tend to live in areas of growing deprivation and among pupils in the lower third of the KS2 distribution. The implications are relevant for school sorting: decreasing school segregation may decrease crime through the formation of more virtuous peer networks.

4. The effect of education participation on youth custody

In England, continued participation in education beyond the minimum age of 16 is strongly negatively correlated with the probability of experiencing custody at ages 17 or 18. We estimate the causal effect of continued participation in education or training at age 17 on the likelihood of custody for young men, exploiting the 'raising of the participation age' in England in 2012 as a natural experiment. The effect of the policy was to increase participation in education or training at age 17 by 1.5 percentage points from a base of 81.0% prior to the reform. The increase in participation at 17 had no impact on the likelihood of custody at ages 17 or 18. The result is robust to estimation method and to whether we consider participation as participation in education or training or as participation in school. The implications are that (a) the policy was not strongly enforced, hence compliance was very low; and (b) for those who participated because of the policy, there was no impact on their likelihood of later custody.

5. School qualifications and youth custody

Previous research has shown a causal relationship between educational achievement and the probability of youth crime. We planned to evaluate whether there is a relationship between GCSE qualifications in English and maths and the probability of entering youth custody. However, we are hindered in this because the majority of young people who end up in youth custody are not entered or fail their GCSEs in these subjects. Although regression results are consistent with educational achievement being a factor in why children end up in youth custody, they strongly suggest that both non-entry/low achievement and youth custody are correlated with severe vulnerabilities which are partially picked up by the explanatory variables available in administrative data. For many, problems either emerge or become evident in early or middle adolescence. The high prevalence of special needs and non-mainstream education suggests a useful focus for future research.

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

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 influences on custody. We find that custody reduces males' employment and earnings by more than 10% up to age 21. This is from a low base; by age 21, those in custody would be expected to only spend roughly 3 weeks in work had they not experienced custody. 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 some evidence for discrimination being a factor.

Looking across the different strands of this project, we draw several conclusions:

- It remains important to focus on policies to encourage remaining in school or training post-16. To retain dis-engaged young people in education or training requires a coherent and consistent post-16 offer, supported by adequate funding.
- If qualifications can help prevent entry to custody as is intuitive and is at least consistent with the results, even if causality is unclear – Key stage 3 onwards may represent a critical phase. In addition, the high prevalence among the youth custody population of special needs designation and non-mainstream education suggests a potential focus for policies to improve attainment among this group.
- Headteachers, teachers and policymakers should recognise the possibility that exclusion may increase the probability of a child experiencing custody. More generally, additional research to understand the mechanism behind this impact is important for the identification of ways to ameliorate it.
- The existence of peer attainment effects suggests school interventions may have multiplier effects beyond their intended aim. The significance of social networks is relevant to considerations around selection into schools and policies around streaming.
- It seems likely that males and females with experience of custody face different obstacles in the labour market. For males, discrimination may be important, raising issues for policy around disclosure of criminal records. For females, it may be that discrimination prevents access to higher-earnings jobs rather than any jobs. More generally, low attainment is likely to present a further obstacle for males and females, highlighting the importance of re-engaging with education both during custody and post-release.

Looking ahead, there are many possible avenues to build on the results uncovered by this research or to deepen understanding of them. Since the start of this study, new data has become available in the form of the linked Department for Education and Ministry of Justice data that allows for a richer exploration of the relationship between education and youth offending. This has the potential to move us closer to understanding the behaviour that effective policy must influence if it is to reduce the use of youth custody still further or to reduce offending more generally.

A. Introduction

This report provides a summary of research conducted as part of the Nuffield Foundation – funded study *Youth custody: Educational influences and labour market consequences.*² The study uses linked administrative data to describe the key pathways into and out of custody, to estimate how aspects of schooling affect the probability of being imprisoned and, beyond school, estimate the impact of prison on subsequent labour market outcomes. The aim of this report is to provide an overview of the research, drawing out the key findings and distilling the main messages for policy. For full detail, readers should refer to the full papers available on the <u>project website</u>.

Children who are incarcerated are disproportionately drawn from among the most disadvantaged young people in the country. This has become more acute over time as the number incarcerated has fallen. The steepness of this drop is illustrated in Figure A.1; the 2021 level is just one-sixth that of 2008). Those committing less serious crimes are more easily diverted onto non-custodial pathways by the courts. In 2020/21, 15,800 children were cautioned or sentenced but only 670 were sentenced to custody (Youth Justice Board, 2022). A consequence of this is that young people entering custody increasingly constitute a 'hard core' with more complex needs that are even more challenging to meet.



Figure A.1 Numbers of under-18s in custody, April by year

Source: Youth Custody Service (2022)

² Grant reference number: JUS/43820

The equity motivation for understanding this population is highlighted by the fact that the fall in youth custody has not been equally distributed among young people: for example, minority ethnic representation among young offenders increased from 25% to 41%. As shown in Figure A.2 for the year to April 2021, they are much more likely to be male. More generally, many young people in custody show extreme disadvantage in respect of literacy, numeracy, school attendance, exclusions, care and mental health. Furthermore, once young people come into contact with the justice system they are likely to do so again and, in the longer term, to struggle in the labour market.



Figure A.2 Mean numbers of under-18s in custody over 12 months to April 2021, by age

Source: Youth Custody Service (2022); data supplement³

Beyond the effect on the child, crime and imprisonment impose significant costs. First and most difficult to quantify are the potentially long-lasting effects felt by many victims of crime. Beyond those, public costs run high. The <u>National Audit Office</u> in 2011 estimated the costs of crime arising from a single cohort of 83,000 young offenders who entered the justice system in 2000 — first time offenders — to average £8,000 per offender. Among those in custody, the costs were almost £5,000 per month. More <u>recent figures</u> estimate the annual cost in 2018 of places in secure children's homes, secure training centres and young offenders institutions to be £210,000, £160,000 and £76,000 respectively. Hence, there is also a financial motivation to reduce both offending and custody.

³

https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/1102946/y outh-custody-data-july-2022.ods

The research summarised in this report aims to make use of population-level administrative data to provide a deeper understanding of youth custody. The large size of the available data allows this highly-selected group to be considered in detail at an individual level. We show how custody features in young people's experiences and how it has a lasting effect on labour market prospects. A particular focus is on how aspects of schooling can influence the probability of custody. Understanding this is important as a means of identifying interventions and practices that may help prevent youth custody at source.

The report begins with a description of the data used. It then presents summaries of the analyses conducted. These can be viewed as forming three distinct strands:

Strand 1 – Descriptive

• Describing routes into and out of custody for young people who experience it

Strand 2 – the impact of aspects of school on the probability of youth custody:

- Exclusion: estimating the impact of exclusion, and how this varies according to the nature of exclusion
- Peers: estimating the impact of higher-attaining peers
- Participation: using the September 2013 raising of the participation age to estimate the impact of participation in education or training
- Qualifications: examining the influence of academic qualifications.

Strand 3 – the impact of youth custody:

• Estimating how youth custody affects subsequent labour market outcomes.

The last section looks across the research strands to highlight the main messages.

B. Data

All analyses in this project are based on administrative data. This has a number of significant advantages. In particular, it provides a large number of observations for estimation and it avoids the standard problems that dog survey data such as non-response bias and recall error. A drawback is that it is less rich, a consequence of not having been expressly designed for research purposes. While the data used in this study is rich by administrative data standards, it remains the case that some potentially informative variables are not observed. It does not, for example, provide information on attitudes or family circumstances which might be important for understanding who experiences custody.

The main database, used in all analyses, is the National Pupil Database (NPD). This provides a history of enrolments, attainment, attendance and exclusions while in the state-funded school system. Personal information such as special educational needs, eligibility for free school meals (a measure of disadvantage) and area of residence is also included. Several other government datasets are included within the NPD. Of particular relevance is the information on whether children are in care (children looked after) or receiving social services (children in need). Monthly activities in the two to three years following compulsory schooling originate from the National Client Caseload Information System (NCCIS) and are also included. This allows identification of time spent in youth custody.

Two of the analyses use NPD linked to the Longitudinal Education Outcomes (LEO) data. LEO data is drawn from several government sources and provides information on labour market and education outcomes beyond school-leaving age. These include enrolments and achievements in further and higher education (colleges and universities), spells in employment, spells in receipt of state benefits, earnings in employment and earnings from self-employment. 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.

Two of the projects merge in additional data: the analysis of exclusions uses data on schools characteristics from the <u>Consistent Schools Database</u>; the analysis of qualifications uses data from assessment boards on the scores underlying the achieved grades at GCSE.

C. Summary of research projects

This section summarises the research conducted as part of this project. Full detail is provided in papers available from the project <u>website</u>.

1. The school to work transition for young people who experience custody

Alex Bowyer, Richard Dorsett and David Thomson

Introduction

On reaching school-leaving age, individuals make choices that will shape their future. Reflecting the importance of this life stage, numerous studies have sought to carefully document the school to work transition or understand how it varies with economic circumstances. The continued interest arises from the recognition that adverse circumstances or experiences when young can have lasting consequences. As an illustration of this, considerable empirical evidence points to scarring effects of youth unemployment on employment and earnings (e.g. Gregg 2001; Gregg and Tominey 2005). Consequently, the school to work transition offers a lens through which to understand the origins of adult inequality.

While many young people manage a successful entry into the labour market, previous research suggests that about 10 per cent in Britain do not (Dorsett and Lucchino 2014). Unsurprisingly, this is not a random subgroup. It is notable that members of this group – and their parents – are more likely to have no or low qualifications than is true of other children.

This paper considers the school-to-work transition of young people who experience custody. It uses NPD-LEO to provide a detailed characterisation of early pathways for young people who experience custody. Evidence along these lines has previously been lacking, perhaps in large part due to the survey datasets typically used for this type of analysis encountering small-sample limitations. NPD-LEO provides the basis for a step-change in our understanding of the pathways into and out of custody since we are able to examine small population subgroups in a way that is not possible using survey datasets.

This paper builds on existing work (e.g. Anders and Dorsett 2017) describing and analysing young people's transitions between education, employment and other states. It describes how custody features in their experiences, further enhancing the evidence base for policy aimed at helping at-risk young people achieve positive transitions into adulthood. These early years are important. Previous research has found that, in the population as a whole, an understanding of early pathways provides a strong basis for predicting pathways when older.

Approach

The analysis uses a data-driven technique to identify groups of young people following similar pathways. The attraction of this approach is that it incorporates full information on the dynamics around young people who experience custody including the order in which events occur, the number of spells in custody and the multiple potential end states for young people who experience custody. Rather than being reduced to a summary statistic, pathways are depicted in full detail through visualisations. These give an immediate insight into the

patterns of transition within each group, allowing, for example, brief repeated periods of custody to be distinguished from longer periods.

Our analysis is based on all those born between September 1994 and August 1995 who are observed to experience custody. Individuals are observed from age 13 to age 22. We divide the data into two periods, comparing trajectories during adolescence (between the ages of 13 and 18) with those of early-adulthood (ages 19-22). This division is prompted by the fact that, in the available data, custody is fully observed up to age 18 but only partially beyond that point. Specifically, between ages 19-22, custody is inferred from education records showing that an individual participates in a course of study that is delivered in a prison. Hence, the analysis relates to individuals who either experienced youth custody or who studied while in prison.

Table 1.1 shows the highly selected nature of the custody subgroup. About 1 per cent of the cohort experiences custody at some point. This subgroup has quite different characteristics from the population as a whole: much more likely to be male and non-white, to have been identified as having special educational needs, to have been on the Children in Need register, have been the subject of a Child Protection Plan, or have been looked after by the state. They are also likely to have resided in more deprived areas -- as captured by the IDACI (Income Deprivation Affecting Children Index) measure -- and to have scored less well at Key Stage 4 exams.

	Full cohort	Custody subgroup
Female	48.7%	7.4%
Non-white	28.9%	40.9%
Special educational needs (ever)	46.8%	87.1%
Child in need (ever)	9.2%	51.1%
Child Protection Plan (ever)	6.8%	37.6%
Child Looked After (ever)	1.9%	25.0%
mean IDACI rank	14999	7866
mean KS4 score	38.0	18.0
Total (rounded to nearest 10)	663 <i>,</i> 330	7,440

Table 1.1. Summary statistics comparing the sample with the population

In the early period (ages 13-18), the possible states we consider are: education while looked after, education while not looked after, employment, NEET, custody and missing (unobserved). The need for this latter state reflects issues with data collection and quality in the underlying systems. We distinguish education by looked-after status during this period in view of the over-representation of care-experienced children among those in custody. In the later period (ages 19-22), the possible states are reduced to education, employment and NEET, where NEET now implicitly includes those in custody.

The approach used to group individuals on the basis of shared histories combines sequence analysis – a technique that compares histories for each pair of individuals and provides a measure of similarity – and cluster analysis – a technique for grouping together similar

individuals into a number of qualitatively distinct clusters. Despite being data-driven, judgment is required as to the most informative number of groups (or "clusters") permitted. This was guided by statistical measures but also by qualitative consideration of the intuitive coherence of the resulting groups. This resulted in the identification of five clusters for both the earlier and late periods (13-18 and 19-22).

Results

The histories of individuals within these clusters are depicted through index plots that show, in full detail, the individual histories of each member of the cluster. Each individual's history is represented as a horizontal series, colour-coded according to status in a particular month. Stacking the series for all individuals in each group creates the index plot, which gives an immediate visualisation of the general dynamics characterising that cluster.

The index plots for the early and late clusters are presented in Figure 1.1. An immediate impression is of the clusters being well-differentiated and of there being some discernible commonality across histories within each cluster. Labelling the 'early' clusters E1-E5 and the `late' clusters L1-L5, we suggest the following characterisation:

E1: Children in care who become NEET after	L1: Young people mostly staying in
age 16	education
E2: Mostly missing / not observed	L2: Young people mostly staying in
	employment
E3: Non-CLA children who become NEET	L3: Young people initially in employment,
after age 16	then transitioning to NEET
E4: Non-CLA children who enter	L4: Young people mostly NEET
employment sometime by age 18	
E5: Non-CLA children who remaining in	L5: Young people moving from NEET into
education post 16.	employment around age 20-21



Figure 1.1 Visualising the early and late clusters (index plots)

Figure 1.2 depicts the relative sizes of the clusters and shows the flows between early and late clusters. Among the early clusters, the largest is E3. When combined with E1, it is the case that roughly half those with experience of custody are mostly NEET shortly after school-leaving age. It is notable that E1 is the only cluster in which looked-after children feature. The fact that NEET is the dominant E1 outcome is indicative of the challenges faced by members of this group. More positively, roughly the same number of young people are in E4 and E5. These are young people who appear to have successfully entered the labour market or at least remain engaged in study beyond school-leaving age.

Among the late clusters, L1, L2 and L5 are the most positive groups in the sense of being made up of young people who mostly seem to avoid being NEET over this age range (although there is some suggestion with L1 of the NEET rate increasing towards the end of this period). Nevertheless, even when combined, these groups are small in number relative to the L4 'NEET' group. Taking L3 and L4 together, nearly 80 per cent of young people with experience of custody are long-established NEET by age 22.

While Figure 1.2 shows that most flows are to these NEET clusters, there are important flows into the more postive (L1, L2, L5) clusters. These mainly originate from individuals in early clusters E4 and E5.

To explore this further, we estimated the probability of late cluster membership using a multinomial regression to control for the influence of other characteristics. Doing this showed that, for instance, the probability of being NEET throughout the later (19-22) period is higher among females, non-whites, those with special educational needs, those with lower attainment and those in more deprived areas.

Our main interest is in the predictive power of the early clusters. The regression results confirm that, even after allowing for the role of the characteristics mentioned above, being in E4 or E5 does most to increase the chances of being in one of the more positive later clusters. E4 members are 12 percentage points (ppts) more likely than the base category (E3) to be in L2 (Employment). E5 members are 9 ppts more likely to be in L1 or L2.

Figure 1.2 Flows between early and late clusters



Conclusion

The analysis in this paper contributes new evidence on the school to work transition of those who experience custody as a young person. The results confirm the generally poor prospects facing many such individuals. Roughly 80 per cent appear established as NEET by age 22. For comparison, the level in the population as a whole has been found in other studies to be 10 per cent.

It is perhaps not surprising to find that outcomes are so much poorer among this group. It reflects the fact that many young people who experience custody are from disadvantaged backgrounds and might be expected to have poor outcomes regardless. Given the focus on such a deprived group it is notable that, as with previous studies for the population as a whole, the results still find that labour market prospects vary with personal characteristics and circumstances in a familiar way. This underscores the need to avoid regarding those who experience custody when young as undifferentiated; even among such a disadvantaged group, some will fare better than others

The results suggest that managing to avoid NEET when 16-18 is an important part of the strategy for avoiding NEET when older. In view of this, policy interventions aimed at engagement and re-engagement have a clear role. Promoting education and training and providing support for employment can help steer young people towards the pathways shown in this paper to be associated with more promising outcomes. While education and

employment programmes exist, it is important that they are sufficiently tailored to the specific needs of young people with experience of custody who, in addition to disadvantage, face the challenge on release of re-adjusting from a highly-structured environment. Previous explorations (Youth Justice Board, 2008) have identified a shortage of research into effective engagement with young people. There is therefore an argument for strengthening the evidence base in this regard.

2. School exclusions and youth custody

Claire Cathro, Giulia Tagliaferri and Alex Sutherland

Introduction

School exclusion is one of the most persistent and well-established risk factors for youth offending (<u>Valdebenito et al., 2018</u>), but, with some exceptions (<u>HM Government, 2019</u>) rarely features in policy relating to crime reduction.

Exclusions – whether temporary ("suspensions", or fixed-term exclusions) or permanent – are the most severe punishments available in the English school system. For each of the last 25 years in England, around three to four hundred thousand suspensions were issued, affecting around 3% of the school-age population, and 5-12,000 pupils were permanently excluded from school each year. In spite of this, we know very little about the impacts of exclusion as a sanction on those excluded.

Each year, between one-quarter and one-third of suspensions are issued because of what is termed 'persistent disruptive behaviour', nearly one-fifth for assaulting pupils or teachers, and around 18% for verbal abuse of peers or teachers (<u>HM Government, 2021</u>). Overall, this means two-thirds to three-quarters of suspensions are for externalising aggressive behaviour. Males are three times more likely than females to be permanently excluded or suspended, and for Black-Caribbean pupils the risk is at least twice as high as for white pupils (<u>HM Government, 2021</u>).

While the relationship between exclusion and later involvement in the justice system is well-recognised, we do not know whether the observed correlation is *causal*; that is, whether being excluded increases the risk of later prison over and above selection processes or pre-existing behavioural problems. This paper uses quasi-experimental methods to help identify causal effects of being permanently excluded or suspended in year 10, when pupils are 14-15 years old, on the probability of custody age 15-17 (inclusive). We also aim to look at how the effect of exclusion varies according to the type of exclusion a pupil experiences: permanent exclusion or suspension (also known as a fixed-term exclusion).

Approach

The analysis is based on four NPD cohorts, who are in year 10, when pupils are 14-15 years old, between 2009/10 and 2013/14. This is augmented with data from the <u>Consistent</u> <u>Schools Database</u> (CSD) to identify characteristics of schools and how they change over time. This allows us to observe the academic year in which a school becomes an academy, though not the exact date of academisation. We restrict our analysis to only pupils that attend schools that become academies by academic year 2013/2014 and focus on pupils observed in the same school in both year 9 and year 10 (and who did not repeat year 10). The resulting sample includes 1,064,555 pupils. For a robustness check, a smaller sample (752,675 pupils) of children in year 10 in the academic year in which academy conversion takes place only is also used.

Nearly 50% of the young people we observe in custody aged 15-17 received at least one exclusion when they were in year 10 (<u>Table 2.1</u>). Pupils receiving an exclusion of any type in year 10 are 1% more likely to also experience custody before age 18, compared with pupils that received no exclusions.⁴ This does not directly imply that exclusions lead to custody, as other factors may play a role. For example, it may be that children with family problems are more likely to be excluded from school (<u>DfE, 2019</u>) *and also* to engage in behaviours that lead to custody (Smith, 2017; Local Government Association, 2018).

Table 2.1: rate of exclusion	ons for pupil	s that do / d	o not have cust	tody experiences
starting age 15-17				

2,129,846

1,737

starting age 15-17						
Pupil receives any exclusion in year 10	No		Yes			
Pupil experiences custody starting age 15-17	Frequency	Percentage	Frequency	Percentage		

93.98%

53.61%

136,403

1,503

6.02%

46.39%

Statistics based on all pupils

No

Yes

To account for unobserved differences in the characteristics of pupils that receive exclusions, we use an instrumental variable approach. In a nutshell, we looked for a factor that could influence the chance that a pupil is excluded but does not have an effect on the pupil's likelihood of experiencing custody later in life. Previous research has suggested that after converting to academy status, schools adopt strict disciplinary policies, resulting in a significant increase in the number of pupils excluded post-academisation (Machin & Sandi, 2020). We exploit changes in a pupil's probability of being excluded in year 10 as a result of a change in their school's academy status. This allows us - conditional on some assumptions - to make causal inferences about the impact of exclusion on custody.

As of 2021, 79% of all secondary schools and 38% of all primary schools are academies (ONS, 2021), over 85% of which are run as part of a chain of schools, called multi-academy trusts. Like the local authority-run schools that preceded them, academies are state-funded, non-fee charging and nonselective. Initially the purpose of academisation was to improve the quality of education, particularly in underperforming schools that served primarily disadvantaged pupils, by enabling a board of governors for the school to have far more autonomy over staffing, pay, curriculum and school policies. Critically, autonomy over school policies meant that academy schools could set-up and enforce their own disciplinary procedures. Machin and Sandi (2020) find that the higher exclusion rate in

 ⁴ This is the result from an analysis that accounts for pupils' gender, free school meal status, academic year, and which school they attended in year 10. Results are available in Appendix B, <u>Table</u>
 <u>4</u>

academies reflects more rigorous discipline being enforced. For this reason, we use academisation of local authority schools as a factor which creates a "shock" in exclusion rates for pupils that attend these schools.

A crucial assumption of our analysis strategy is that academisation affects the likelihood of custody *only through its impact on exclusion*. That is: being in a school that academises does not increase or decrease the probability of experiencing custody later in life, *except through* the higher probability of being excluded. We cannot know or test whether this is occurring; however, to address this concern, in our main robustness check we look only at exclusions in the academic year that a school converts to an academy, as we hypothesise that new discipline policies may be implemented and enforced quickly in newly formed academies, compared with other changes that may take longer to be implemented and would also impact probability of custody. This is consistent with anecdotal evidence from some converted academies, in which the newly appointed principal introduced strict discipline policies.⁵

Results

To be relevant, our instrument must influence the probability of exclusion. We provide results of the impact of a school's academisation on the probability of being excluded while in year 10 in <u>Table 2.2</u>. We find that being a year 10 pupil in a school after it converted to an academy increases the probability of receiving a suspension or permanent exclusion by 2.6 percentage points on average (column 1). This compares to a raw exclusion rate of 5.6% of all pupils in our sample, or 5.3% among only pupils in our sample that attend schools that have not yet converted to academies. 2.6 percentage points is therefore a sizable increase - it means moving from approximately 3 to 4 pupils excluded in every two classes of pupils (52 pupils total).

Academisation increases both the probability of permanent exclusion and suspension, but the effect seems to be mainly concentrated on suspensions (2.6 percentage points increase versus a 0.1 percentage points increase for permanent exclusions, cols (3) and (2)). This may be because permanent exclusions are much rarer than suspensions; among all pupils in our main sample, only 0.2% receive permanent exclusions compared to 5.6% receiving fixed term exclusions. There are approximately 28 times more pupils with any suspensions than pupils with any permanent exclusions in each academic year in our main sample.

When looking at the effect of academisation in the year of academisation exclusively (that is, restricting the analysis to the year of academy conversion), we find a similar picture (columns 4-6).

These results, all of which are strongly significant, reassure us that academisation is a strong predictor (strong instrument) for the probability of receiving exclusions, regardless of the type of exclusion and whether this is in the longer or shorter term.

⁵ One example comes from schools under the Inspiration Trust academy sponsors' umbrella. In 2017, the BBC covered the sudden change in discipline policy in newly converted Inspiration Trust academies in a series of articles (examples available <u>here</u> and <u>here</u>).

	Attendance at academy in year of or after conversion after conversion			<i>Robustness check</i> : Attendance at academy in year of conversion		
	(1) Any	(2) Permanent exclusions	(3) Suspension	(4) Any	(5) Permanent exclusions	(6) Suspension
Exclusion or suspension	0.026** (12.71)	0.001** (4.30)	0.026** (12.60)	0.029** (12.61)	0.001** (3.54)	0.028** (12.52)
F	161.4	18.5	158.7	159.0	12.5	156.7
Mean (exclusion)	0.057	0.002	0.056	0.054	0.002	0.053
N	1,064,555	1,064,555	1,064,555	752,675	752,675	752,675

Table 2.2: effect of being in an academy on probability of receiving exclusion (2SLS, first stage) - main analysis

+ p<0.05, * p<0.01, ** p<0.001

Regressions control for: gender, FSM eligibility in year 10, SEN in year 10, year fixed effects, school fixed effects

T-statistic in parentheses

Statistics based on all pupils that attend schools that ever become academies, do not switch schools or repeat year 10.

1 singleton observation dropped from regressions in columns (4)-(6)

Table 2.3 presents estimates of the impact of exclusions on custody. We find that the higher probability of receiving an exclusion in the academic year of academisation (because of the academisation) affects the probability of custody at ages 15-17. Receiving any exclusion in year 10 while attending a school that has academised causes a statistically significant 1.3 percentage point increase in the probability of custody (col 1). Receiving a suspension has a similar effect size (column 3). The overall effect masks an even larger impact of permanent exclusion on custody: a permanent exclusion in year 10 while attending a school that has academised causes a 33 percentage point increase in probability of custody (column 2). Against the custody rate of 0.1% among pupils as a whole, this constitutes a sizeable increase. However, the estimated impact relates to compliers – those who are excluded as a result of academisation – whose expected custody rate in the absence of academisation is not observed but is likely to be higher than that seen among pupils as a whole.

When looking at the impact of exclusions on custody including cohorts attending academy schools in the year of conversion only, we find results that are consistent with the main analysis in that they are of a similar size but they are non-significant (columns 4-6). The main analysis using multiple years *is* significant thanks to its larger sample size, but it is possible that the effect is somewhat overstated if academies introduced other measures in subsequent years that impact on custody through means other than exclusion.

	Attendance at academy in year of or after conversion a			<i>Robustness check</i> : Attendance at academy in year of conversion		
	(1) Any	(2) (3) (4) Permanent Suspension exclusions		(4) Any	(5) Permanent exclusions	(6) Suspension
Exclusion or suspension	0.013 ⁺ (2.20)	0.328 ⁺ (2.04)	0.013 ⁺ (2.20)	0.011 (1.85)	0.313 (1.73)	0.011 (1.84)
Mean (custody)	0.001	0.001	0.001	0.001	0.001	0.001
N	1,064,555	1,064,555	1,064,555	752,675	752,675	752,675

 Table 2.3: effect of receiving exclusion on probability of custody before age 18 (second stage, 2SLS) - main analysis

+ p<0.05, * p<0.01, ** p<0.001.

Regressions control for: gender, FSM eligibility in year 10, SEN in year 10, year fixed effects, school fixed effects

T-statistic in parentheses

Sample: Pupils that attend schools that ever become academies, do not switch schools or repeat year 10

1 singleton observation dropped from first year academisation regressions (columns (4)-(6))

Conclusion

We find that attending a school that converts to an academy in Year 10, the year when pupils are most likely to be excluded, increases the probability of receiving a suspension or permanent exclusion by 3 percentage points (statistically significant at 0.1%). This is against a raw exclusion rate of 5-6%. The finding, consistent with other research, that academisation increased the likelihood of exclusion is important, given that academisation has been a priority for government. This alone does not mean that academisation should not be used, but that policymakers can be clearer about the consequences of the policy and possible trade-offs that are being made.

We find evidence that a Year 10 pupil attending a school that academised resulted in a statistically significant increase in the probability of custody age 15-17, with impacts varying depending on the type of exclusion. Specifically, receiving a *permanent* exclusion increases the probability of custody by 33 percentage points (statistically significant at 5%); for *suspension*, the increase is 1.3 percentage points (statistically significant at 5%). The raw custody rate for pupils in the sample was less than 0.1%, so these are substantial effect sizes. We caveat that additional analysis (robustness checks) were not statistically significant at conventional levels. However, the coefficients on exclusion were positive in all the models we ran; this provides further reassurance on the likely direction of the effect.

Our interpretation of these results is that *exclusion presents a small but non-ignorable risk of increases in custody, warranting, at the very least, further exploration*. Given the consequences (and cost) of both exclusion and custody, the possibility of a negative effect should be taken seriously by researchers and policy practitioners alike. Greater sensitivity to this possibility may also be needed given the long-standing and widespread use of exclusion in England. Put differently, if these results were reversed – that exclusion *reduced* the risk of custody by up to thirty percentage points – there would be significant interest.

Our results highlight an onward negative criminal justice outcome of education policy that, to the best of our knowledge, has not previously been quantified. This evidence does not mean that exclusion as a policy should cease – although some may advocate that. What it does mean is that headteachers, teachers and policymakers should take seriously the idea of exclusion *as an intervention* that can and should be better understood in terms of its short and long-run impacts. This can happen through better use of data and evidence that is already available to government to look at the impacts of exclusion on those who are excluded but also on their peers (for whom there may be unquantified benefits). This can also happen through the willingness of government - including approaches that are 'known to work' or are promoted but do not have evidence that they work in terms of improving behaviour or other outcomes.

3. School peers' ability and youth custody

Richard Dorsett, Emma Gorman, Greta Morando, Veruska Oppedisano and Min Zhang

Introduction

Schools are social places, where children's behaviour and outcomes are influenced by their peers. Previous research has provided evidence that pupils' attainment is increased when they have high-attaining peers. Such interactions are important, implying that education and education interventions can have multiplier effects. Peer effects are also seen in other contexts. Evidence documents the negative influence of criminally active peers in a variety of settings, indicating the presence of social interactions within neighbourhoods, juvenile correctional facilities and schools (Deming, 2011; Bayer et al., 2009; Billings et al., 2019).

This paper looks at the overlap between these two research areas. It seeks to establish whether high-attaining peers reduce the probability of youth custody. Schools may be a particularly important setting for the onset of criminal behaviour and can have a role to play in preventing youth crime. Despite this, little is known about the relationship between peers' ability in school and youth criminal behaviour. If such a relationship exists, it further strengthens the evidence for a multiplier effect of educational interventions.

There are several reasons why the research question is important. First, crime generates enormous costs to society so measures to reduce it can be cost-effective. Second, incarceration during adolescence interrupts education at a critical time, often leading to greater criminal activity and reduced future wages (Aizer and Doyle, 2015). Third, those in custody are the most disadvantaged young people, so there is a strong equity motivation.

Approach

Estimating causal effects arising from social interactions poses formidable challenges. Key among these is the simultaneity arising from the fact that while peers may exert an influence on a pupil, that same pupil in turn exerts an effect on the peers; the so-called 'reflection' problem. In addition, peers and focal pupil alike may be influenced by group characteristics and unobserved characteristics that are potentially correlated. Furthermore, self-selection into peer groups is non-random. Individuals have some degrees of choice over where to live, which school to attend and with whom to associate.

We can control for the reflection problem using instrumental variable regression. The requirement here is to identify a variable – the instrument – that influences peer ability without directly influencing the probability of custody. In keeping with previous studies we rely on the idea that ability of a pupil's peers is partly influenced by their former classmates at primary school. Since secondary school pupils come from a number of different primary schools, ability of a pupils' peers will be partly affected by an independent influence (that of the peers who attended a primary school different from that of the pupil). We use as our instrument the key stage 2 attainment of these 'peers of peers': the primary school peers of an individual's secondary school peers who attended a different primary and secondary school. In addition, to address the issue of self-selection, we include school-level summaries of pupil characteristics among the control variables.

We conduct pupil-level instrumental variable regression of custody on peer group. The outcome variable – whether or not the pupil experienced custody at age 17 or 18 – is binary, so we adopt a probit specification in order to constrain estimated probabilities to lie in the 0 to 1 range. We define peer ability in two ways. Our preferred measure is the proportion who achieve at least grade C in maths at GCSE. This measure captures the extent to which a contingent of peers performs very poorly at GCSE, failing to secure what is commonly regarded as a 'good' qualification. It partly reflects the extent to which groups of peers effectively opt out of education. It may be that such peers exert a more negative influence than the group of peers as a whole – as measured by mean attainment – tend to do. Nonetheless, mean attainment is a natural measure to consider so we also present results showing how peers' mean GCSE attainment in maths affects custody.

Results

This paper makes use of data for entire cohorts of secondary school leavers in the summers of 2011-2014. Estimated impacts are presented in Table 3.1. Column 1 shows the results when measuring peer ability by the proportion achieving at least C grade in maths at GCSE. Column 2 shows the results when measuring peer ability by mean maths attainment at GCSE. In both cases, we find that having higher-attaining peers reduces the probability of experiencing youth custody. This is significant at the 95% level. Also shown in Table 3.1 is the 'first stage' coefficient from the regression of peer ability on the instrument (peers of peers' ability). The significance level of this association suggests it is a strong instrument.

	(1)	(2)
	proportion of peers with	mean maths GCSE grade of
	grade C maths GCSE	peers
Peer ability	-3.717*	-0.456*
	(1.786)	(0.228)
First stage:		
Peers of peers' ability	0.00621***	0.0503***
	(0.00120)	(0.00677)
Observations:	2120391	2120391

Table 3.1: Impact of peers' ability on the probability of custody

Estimates control for pupil characteristics at age 11 (gender; ethnicity; English as an additional language (EAL); free school meals (FSM); Education Health and Care (EHC) plan; special educational needs (SEN); ever a child looked after (CLA); ever a child in need (CiN); unauthorised absences; attainment at KS1 and KS2), cohort and aggregated age-11 characteristics characteristics (proportion female; proportion EAL; proportion FSM; proportion EHC; proportion SEN; proportion ever CLA; % ever CiN, mean unauthorised absences; mean attainment at KS1 and KS2). Standard errors (in parentheses) are clustered by secondary school. Asterisks denote statistical significance: +=90%, *=95%, **=99% ***=99.9%

Table 3.2 shows how impacts of peer ability – measured as the proportion of peers with grade C maths at KS4 – vary across subgroups of individual pupils. We consider three dimensions: free school meal status (to consider whether disadvantaged pupils are more likely to be affected); whether the areas in which a school's pupils live have become more deprived between 2011 and 2014; and attainment level at Key Stage 2. For FSM status, we

see no evidence that the impact on those receiving FSM differs from that on those not receiving FSM. However, for area deprivation we see that impacts are considerably more negative among those living in areas of increasing deprivation. Hence, while individual impacts do not seem to vary by own-disadvantage, they do seem to vary by disadvantage of one's peers. There is also a relationship with KS2 attainment. Impacts are concentrated among those in the bottom third.

Change in mean area							
FSM		deprivatio	deprivation, 2011-14		KS2 tertile		
			No more	More			
	Not FSM	FSM	deprived	deprived	KS2 low	KS2 mid	KS2 high
Peer ability	-3.705	-4.297	-2.336	-7.683**	-5.588**	-1.023	-2.931
	(2.513)	(2.648)	(2.239)	(2.932)	(2.004)	(2.912)	(5.680)
Observations	1,554,886	565,505	1,743,680	376,711	737,472	683,263	699,330
	~ ~						

See notes to Table 3.1.

We consider two channels through which these impacts might arise. First, if being among higher ability peers increases an individual's own educational attainment, this may act to increase the opportunity cost of criminal behaviour. Second, higher-attaining peers are themselves likely to be better behaved and therefore less likely to lead individuals to be disruptive and or engage in risky behaviour. To understand which mechanism is likely to be at play, we estimate our preferred regression, replacing the dependent variable with educational attainment (Maths and English) and then with two measures of disruptive behaviours: unauthorized absences and permanent exclusion. Results presented in Table 3.3 indicate that peers' ability has a significant and positive impact on attainment and a significant negative impact on absences but not on exclusions. This suggests that peers' ability may reduce youth crime through their positive impact on educational attainment more than through the reduction of disruptive behaviour.

			Unauthorised	Permanent
	KS4 Maths	KS4 English	absences	exclusion
Peer effects	3.195***	2.673***	-0.115***	2.795*
	(0.370)	(0.575)	(0.0300)	(1.369)

	Table 3.3: IV	model on	educational	and beł	navioural	outcomes
--	---------------	----------	-------------	---------	-----------	----------

See notes to Table 3.1

Lastly, we turn to the question of how big the estimated impacts are. For our preferred measure of peer ability, we show how raising the proportion with a C grade to the median proportion in the population would affect the probability of youth custody. Table 3.4 uses the estimation results to derive the probability of custody. It then changes the level of peer ability, increasing to the median all those for whom the proportion of peers with a C grade or higher at GCSE is below that level and recalculates the probability. Doing so, the estimated probability falls by 0.013 percentage points; a reduction of 14%.

We follow a similar approach to show the impact of a one standard deviation increase in mean peer ability. Using this alternative approach, we find a reduction of 31%.

	<u> </u>		
	% of peers with grade C at KS4	mean KS4 of peers	
Coefficient (Table 3.1)	-3.717*	-0.456*	
Predicted custody:			
- baseline	0.096 %	0.096 %	
- raising % with C to median	0.082 %		
- increasing mean by 1 SD		0.066 %	
- difference from baseline	-0.013 %-pts	-0.030 %-pts	
- differences as %	-14 %	-31 %	

Table 3.4 – The impact of peer ability on the probability of youth custody

See notes to Table 3.1

Conclusion

An emerging literature documents the negative influence of criminal activity and risky behaviour among peers in a variety of settings. The effects of such peers on criminal outcomes or risky behaviour for youth seem to be larger than peer effects on education. Despite the strong arguments linking education and crime, very few studies have investigated the relationship between peer quality at school and crime. Our results therefore contribute novel findings and suggest non-negligible effects of peer ability on youth custody. Increasing attainment among lower-attaining schools such that the fraction of pupils with at least grade C in KS4 is brought to the level of the median is estimated to reduce the probability of youth custody by 14%. These impacts suggest that efforts to improve attainment in lower-attaining schools in particular will have the knock-on benefit of reduced youth custody. The results are also relevant to considerations around streaming in schools and underscore the importance of paying attention to the encouragement of virtuous peer networks when grouping pupils.

4. The effect of education participation on youth custody: causal evidence from England

Matt Dickson

Introduction

The negative relationship between education and crime is well documented for many countries around the world. In England, continued participation in education beyond age 16 is strongly associated with a lower probability of experiencing custody in later teenage years, however the non-random selection of young people into continued participation means cross-sectional estimates of the relationship are likely to contain considerable bias and therefore the participation effect on custody cannot be considered to be causal.

To overcome this issue and identify the causal effect of continued participation on the probability that young men will experience custody at ages 17 and 18, this paper exploits the natural experiment created by the raising of the participation age in England (RPA) in 2012/13. Unlike previous cohorts who could leave education aged 16, young people starting the final year of compulsory schooling in September 2012 were required to remain in education or training until the end of the school year in which they turned 17, and those starting the final year in September 2013 were required to remain in education or training until age 18. Using this policy-induced variation in participation between cohorts we estimate the causal effect of continued participation on custody outcomes. The full paper provides results also for females and for population subgroups. These support the findings presented here but are excluded for brevity.

There are important policy implications relating to this study: firstly, around the implementation of compulsory education reforms and the extent to which they achieve compliance, and secondly concerning the extent to which compulsory education post-16 can be a useful policy lever to prevent later custody given the particular group impacted by the policy.

Approach

The analysis uses a dataset constructed from the NPD and includes all individuals in statefunded schools in England who were born in the four cohorts: September 1994-August 1995, September 1995-August 1996, September 1996-August 1997, and September 1997 to August 1998.

Estimation uses two approaches. The first – regression discontinuity – relies on the preexisting propensity to experience custody being disrupted by the increased engagement in education/training induced by the RPA. Since it reasonable to believe that the RPA should only affect custody through its effect on education/training, a comparison of custody levels before and after the RPA can provide a credible estimate of the impact of participation.

Unlike previous compulsory school leaving age policies, such as the 1972 'Raising of the School Leaving Age' (RoSLA) which mandated additional time in secondary school, the additional education or training under RPA could comprise full-time study in a school or college, or with a training provider, but could also be full-time work or volunteering (20

hours per week or more) combined with part-time education or training (approximately one day per week), or an apprenticeship or traineeship. The implementation of the policy means that in our data, the first two cohorts were not subject to RPA, but the cohort born in the 1996/97 academic year (who started year 11 in September 2012) were required to participate in year 12 and those born in the 1997/98 academic year were required to participate until their 18th birthday. Since both these later cohorts are required to remain in education or training throughout year 12 this is the treatment that we focus on in the analysis.

A regression discontinuity design can be depicted graphically. Formal estimation uses regression analysis. Also presented are instrumental variable estimates, where the discontinuity is the instrument. These allow for the influence of other characteristics to be controlled for, something that is likely to be important when custody is an outcome experienced by only a very small proportion of each cohort.

Results

In order for the RPA to reduce the probability of custody, there should be an increase in the proportion who remain in education or training as a result of the policy's implementation. Figure 4.1 shows how the proportion of each cohort remaining in some form of education or training changes across the four cohorts in the data. Each figure is centred around the birth month September 1996 which is the point at which individuals become affected by the RPA. Approximately 80% of the cohort two years prior to RPA are participating in some form of education or training at age 17, and this increases by 2 percentage points in the cohort immediately prior to RPA. Following RPA this trend continues in the next cohort, before flattening out. Visual inspection of the fitted lines suggests little evidence of a discontinuity in participation at age 17 as a result of RPA.



Figure 4.1: Proportion continuing in education or training aged 17 by year-month of birth, males

Despite the lack of evidence of any jump in participation in education or training as a whole, Figure 4.2 shows that there does appear to be more of a visible impact on the proportion of the cohort remaining in school, which rises from approximately 0.34 to 0.35 between the cohorts pre- and post-RPA. The figure shows a strong age-within-cohort effect on participation – those born earliest in the school year (i.e. September) are the most likely to participate with the proportion participating generally falling in each subsequent birth month. This means that it is important to compare whole 12-month cohorts rather than those in just the birth months either side of the policy threshold, otherwise we would falsely attribute the within-year difference between the August and September born students to the effect of the policy. The fitted lines take account of the within-cohort patterns to estimate the overall policy impact.



Figure 4.2: Proportion continuing in school by year-month of birth, males

Experience of custody at ages 17 or 18 is extremely rare. Figure 4.3 shows that the proportion experiencing custody at ages 17 or 18 is highest among the older cohorts, declining from around 0.006 for those born in the earliest cohort to around 0.003 for those in the youngest. The fitted lines suggest no marked discontinuity in experience of custody at the point at which the RPA is implemented.



Figure 4.3: Proportion experiencing custody aged 17 or 18 by year-month of birth, males

Table 4.1 presents regression estimates that confirm the impression from the figures. The first row corresponds to Figures 1 and 2 and shows that the impact of the RPA on education/training participation is not significant while the impact on school participation is a significant increase of 1.0 percentage point. The second row corresponds to Figure 4.3 and shows that RPA is associated with a small *increase* in the probability of custody at ages 17 or 18, of 0.04 percentage points. Given the pre-RPA average probability of custody is 0.48%, this represents a very small increase albeit significant at the 5% level. The third row represents the implied impact of participation on custody. The impact of education/training as a whole is not significant. The impact of school participation is positive.

Table 4 1. Regression	discontinuity e	estimates of	the RPA ef	fect on n	orobability o	of custody
Table 4.1. Regression	uiscontinuity e	estimates of	INE NEA EI	iect on p	nobability (JI CUSLOUY

Participation in education/ training	n	Participatior in school	1
coeff.	std. err.	coeff.	std.
			err.
0.0005	0.0013	0.0103***	0.0017
0.0004**	0.0002	0.0004**	0.0002
0.9192	2.5772	0.0437*	0.0229
	Participation in education/ training coeff. 0.0005 0.0004** 0.9192	Participation in education/ training coeff. std.err. 0.0005 0.0004** 0.9192 2.5772	Participation in education/ trainingParticipation in schoolcoeff.std. err.coeff.0.00050.00130.0103***0.0004**0.00020.0004**0.91922.57720.0437*

N=1,282,709. Significance levels: *** 99%; ** 95%; * 90%.

With a sample size in excess of one million observations, the characteristics of individuals should in theory be well balanced between the pre- and post-RPA cohorts. However, given that experience of custody is extremely rare it is possible that these regression discontinuity estimates could be affected by imbalances between the pre- and post-RPA cohorts in socio-demographic and prior attainment characteristics. The linked administrative data contains a rich array of background information on each individual, hence we can further investigate the RPA effect on participation and custody using a two-stage least squares estimation approach that takes account of the impact of characteristics on the probability of custody. Table 4.2 contains the estimates from this approach.

	coeff.	std. err.	coeff.	std. err.
First stage	Participation in		Participation in	
dependent	education/		school	
variable:	training			
RPA (born Sept	0.0169***	0.0014	0.0113***	0.0020
1996 or later)				
Second stage	Ever in custody		Ever in custody	
dependent	aged 17 or 18		aged 17 or 18	
variable:				
	coeff.	std. err.	coeff.	std. err.
Participation	0.0039	0.0128	0.0059	0.0193
impact				

Table 4.2: Instrumental variable estimates of the RPA effect on probability of custody

N=816,089. Significance levels: *** 99%; ** 95%; * 90%.

For education/training as a whole, the RPA is estimated to increase participation by 1.7 percentage points. The instrumental variable estimate of the impact of participating in education or training is however non-significant, consistent with the regression discontinuity estimates. For participation in school, we again see a positive and significant increase (of 1.1 percentage points) associated with the RPA, which accords with the visual evidence in Figure 4.2, but no significant impact of that increase in participation on the probability of custody.

Conclusion

The results suggest the RPA had little impact on continued participation in education and training at age 17 for young men, and for those who were impacted, the additional education did not affect their likelihood of experiencing custody at ages 17 or 18. The size of the impact on participation is small, at around 1.5 percentage points, though given that 81% of the cohort are already participating at age 17, this increase is just under 10% of those who might be affected. Moreover, since only around 10% of the cohort prior to RPA were NEET (not in education, employment or training) we might infer that the potential complier group is closer to 10% of the cohort, in which case a 1.5 percentage points impact is closer to 15% of the target group having their participation changed as a result of the policy.

As continued participation was not enforced in the same way that compliance with the compulsory minimum school leaving age is enforced, this additional participation is perhaps larger than we might have expected. That the participation was voluntary suggests that the compliers may be those within the target group who are more connected to the education system and who therefore may be less likely to be at risk of custody. It is perhaps then less surprising that we do not see an effect on likelihood of custody as a result of the RPA.

5. School qualifications and youth custody

Stephen Machin, Sandra McNally and Jenifer Ruiz-Valenzuela

Introduction

There is a known causal relationship between education and crime, which is often explored using changes in compulsory school laws to establish the direction of causality from education to crime. Much of this work in the economics of crime field focusses on crime measures that do not result in incarceration (like arrests or convictions), although some work does study the likelihood of ending up in prison as an adult.

In this paper, we investigate the link between education, specifically GCSE grades, and the probability of entering youth custody in England. Achieving higher qualifications at school leaving exams (GCSEs) enables people to move on to the next stage with a broader range of options and therefore become more employable. Machin et al. (2020) show that even those who marginally fail to get a good grade at GCSE increase the risk of ending up 'not in education, employment or training' (NEET) by age 18. It is plausible that faced with such prospects, young people may turn to crime. In fact, there is evidence to show that if young people are NEET, this puts them at a high risk of wage scarring effects and crime participation resulting from youth unemployment in the longer term (Gregg and Tominey, 2005; Bell, Bindler and Machin, 2018).

Only a very small proportion of young people enter youth custody (and we restrict our attention here to those aged between 16 and 18), but the consequences are severe – with those in our data spending an average of 7 months in secure centres. Such a penalty is imposed by courts because the crime is so serious there is no other suitable option, or the young person has committed crimes before, or the judge or magistrate thinks the young person is a risk to the public.⁶ Apart from being a very negative outcome for young people themselves, this is expensive for the public purse.

The question is whether education can act as a mediating mechanism to prevent young people ending up in youth custody in the first place. While existing evidence suggests that education is a relevant factor, in this paper we show that young people entering youth custody are often not even entered for GCSE exams. Unsurprisingly, but empirically validated here with administrative data, the correlation between GCSE grades and the probability of ending up in youth custody is driven by variation at the lower end of the distribution (when other factors are accounted for). Enabling a student to get a pass grade might plausibly have some causal effect on their probability of ending up in youth custody. Our results point to the importance of investigating the efficacy of existing interventions that are already in place for these young people. This is especially the case for interventions that take place in those institutions concentrating the majority of young people that will subsequently enter youth custody, such as Pupil Referral Units.

⁶ https://www.gov.uk/young-people-in-custody

Approach

We use NPD data on the cohorts who sat their GCSE exams in the academic years 2011-12, 2012-13 and 2013-14.⁷ We link this with exam board data on marks for GCSE English, GCSE English Language (both counting towards the requirement of getting a C in GCSE English) and GCSE mathematics that are available for all years for the AQA exam board for English specifications and available in 2014 for both subjects for all four exam boards in England. The AQA exam board accounts for well over half of all exam entries in GCSE English.

The analysis begins with descriptive statistics showing how those who enter youth custody differ from the general student population in their observable characteristics. As the vast majority of those in youth custody are male (94%), our analysis is for boys only. We then use regression analysis to show whether the probability of custody age 16-18 is correlated with qualifications after controlling for a range of background characteristics. Then, for those students who can be matched to marks, we implement a regression discontinuity approach by estimating regressions that include the grade achieved alongside the underlying mark. These regressions are estimated for groups of students whose grade falls within narrow categories (i.e. C to D; D-E; E-F; F-G; G-U) and capture the impact of achieving the higher grade as regression discontinuity. There are hardly any students in youth custody that obtain any higher grade than a C and hence grades above this are not relevant. Whereas 'C' is seen as a 'good' grade within the GCSE system, 'G' is another important threshold, especially for low attaining students, because it represents a pass.⁸

Results

We are interested in whether qualifications at age 16 affect the probability of entering youth custody (restricting the sample to males only). Many of those entering youth custody do not even have an entry for GCSE English or maths. This is shown in Figures 1 and 2.

As many as 45 per cent of those who subsequently enter youth custody have a missing entry for English (including those who take a non-GCSE English subject in Year 11) and this number is 30 per cent for maths. Where there is an entry, those who enter youth custody at age 16/17 are very likely to have received very low grades relative to other males attending state schools. If we sum those who either have a missing entry (including non-GCSE subjects), no award or a fail in GCSEs, this comes to 66 per cent for English and 78 per cent for maths. Where there is a definite fail of U (as opposed to 'no award' or missing entry), this is 16 per cent for English and 17 per cent for maths, and compares to only 1 and 3.7 per cent for English and maths, respectively, among the rest of the male population. At the other end of the distribution, it is rare for a person who subsequently ends up in youth custody to have done well at GCSE: 9 percent and 6 per cent receive a grade C in English and maths, respectively, and it is very rare to have got a grade B or more.

⁷ We denote each academic year by the year in which the exams were taken (i.e. 2011-12 is referred to as 2012).

⁸ In more recent years, the GCSE grading system has changed to a numeric grading system, but this does not affect the cohorts of interest here.





Figure 5.2: GCSE Grades in Maths (males only)



Figures 3 and 4 show that attainment in primary school at age 11 in English and maths tests (at Key Stage 2) were already very much worse for those entering youth custody later. But

the representation of the sample who go to youth custody is much better here than it was when looking at KS4 results. For example, Key Stage 2 data for English is only missing for about 7 per cent of people who subsequently enter youth custody.





Figure 5.4: Maths Key Stage 2 test results



The fact that so many of those destined for youth custody are concentrated at the bottom of the qualifications distribution or are not entered at all limits what we can say about the effect of qualifications on the probability of entering youth custody. Nonetheless, it is of

interest to analyse how the correlation between qualifications and youth custody changes as we try to account for contextual factors. It likely that with the available data we are not fully capturing all factors that are correlated with non-entry/poor exam grades and the probability of entering youth custody and so cannot capture the impact of qualifications in this way. The more rigorous regression discontinuity approach described above offers a means of achieving this but can be applied only to a very small number of students who end up in youth custody. Unfortunately, the results are for the most part under-powered to find statistically significant results, though they provide interesting suggestive evidence on the potential influence of GCSE grades.

Table 5.1 shows the association between whether a male student achieves 5 good grades at GCSE (including English and maths) and the probability of ending up in youth custody at age 16 or 17. The different columns show how estimates change as we successively include various controls. If the final estimate in column (4) or (5) were to be taken at face value, this would suggest that enabling students to achieve 5 good grades at GCSE (including English and Maths) would halve the probability of entering youth custody from 0.004 to 0.002 or from 4 in 1000 to 2 in 1000. This would be a very strong 'effect' but is very unlikely to be causal due to the influence of other variables unobserved in the data.

	(1)	(2)	(3)	(4)	(5)
5+GCSEs	-0.007***	-0.006***	-0.005***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Controls:					
FSM		Х	Х	Х	х
Ethnicity		Х	Х	Х	х
KS2 points score			Х	Х	Х
Special schools				Х	Х
SEN				Х	Х
School fixed effects					х

Table 5.1: Relationship between youth custody and whether student achieves 5 or more good grades at GCSE including English and maths.

Notes: robust standard errors. Controls for year dummies and missing variable dummies also included. N=888,774

Figures 5 and 6 show results from regressions where the explanatory variable is in the form of dummy variables for the grade achieved in English and maths. The graphs show how the coefficients change as various controls are added (in the same way as in Table 5.1).



Figure 5.5. The association between English GCSE grade and youth custody

Note. Coefficient on grade (relative to C and above) for GCSE English as controls are added.



Figure 5.6. The association between maths GCSE grade and youth custody

Note. Coefficient on grade (relative to C and above) for GCSE English as controls are added.

Most of the correlation between grades and the probability of entering youth custody is evident at the bottom of the distribution, from Grade F or lower (or non-entry). When full controls are added, there is no association between getting either grade D or E (relative to a grade C) and the probability of later entering youth custody. But there is an association at levels below that, even with controls. For grades F and G (relative to grade C) this is clearer for English than for maths. Failing either subject (grade U) or not being entered is clearly strongly correlated with entering youth custody. In English, not being entered is a stronger correlate of youth custody compared to failing but the opposite is true for maths.

Table 5.2 presents the regression discontinuity approach results for students who can be linked to a GCSE mark within the following grade boundaries: C-D; D-E; E-F; F-G and G-U.

	English		50115	Maths		
Sample C-	·D					
-	(1)	(2)	(3)	(4)	(5)	(6)
Grade C	-0.00019	-0.00027	-0.00033	0.00040	0.00042	0.00032
	(0.00025)	(0.00025)	(0.00025)	(0.00037)	(0.00037)	(0.00036)
ymean	0.00074	0.00074	0.00074	0.00068	0.00068	0.00068
Ν	194251	194251	194251	87185	87185	87185
Sample D-	-E					
Grade D	0.00064	0.00067	0.00068	0.00048	0.00052	0.00007
	(0.00047)	(0.00047)	(0.00047)	(0.00095)	(0.00095)	(0.00095)
ymean	0.00140	0.00140	0.00140	0.00140	0.00140	0.00140
Ν	117810	117810	117810	39191	39191	39191
Sample E-	F					
Grade E	0.00015	0.00009	0.00053	-0.00217	-0.00218	-0.00208
	(0.00108)	(0.00107)	(0.00102)	(0.00161)	(0.00163)	(0.00169)
ymean	0.00259	0.00259	0.00259	0.00212	0.00212	0.00212
Ν	54815	54815	54815	20710	20710	20710
Sample F-	G					
Grade F	0.00228	0.00255	0.00410*	-0.00100	-0.00089	-0.00119
	(0.00213)	(0.00213)	(0.00212)	(0.00155)	(0.00155)	(0.00152)
ymean	0.00469	0.00469	0.00469	0.00236	0.00236	0.00236
Ν	20690	20690	20690	16107	16107	16107
Sample G	-U					
Grade G	-0.00856	-0.00778	-0.00913	-0.00206	-0.00226	-0.00364*
	(0.00588)	(0.00585)	(0.00587)	(0.00182)	(0.00184)	(0.00197)
ymean	0.00927	0.00927	0.00927	0.00429	0.00429	0.00429
Ν	6690	6690	6690	15609	15609	15609

Table 5.2: Regression Discontinuity results

Note: the dependent variable is whether the individual is in youth custody at age 16 or 17. Each panel represents a separate set of regressions. Column 1 controls for the running variable (i.e. final marks) only; column 2 additionally controls for individual level characteristics (see text); and Column 3 also controls for school fixed effects. ymean is the mean of the dependent variable in each specification.

The number of individuals who have a mark and end up in youth custody is very low. This has two consequences for our analysis: first, any results here do not extrapolate to most individuals who enter youth custody, for whom we have no mark; second, the analysis is underpowered to find results that are statistically different from zero. With these caveats in mind, columns (1) to (3) show results for English and (4) to (6) for maths. In columns (1)

and (4), only the underlying score is included as a covariate, in colums (2) and (5) individual-level controls are additionally included and in columns (3) and (6), school fixed effects are also included.

For the most part, estimated coefficients are close to zero and not statistically significant. As the number of students both in the sample and in youth custody is very small in each case, it would be difficult to distinguish between a meaninful 'zero' and a very small causal effect that cannot be detected. To the extent there is some hope of the latter, it is most plausibly around the G-U boundary as this represents the pass-fail distinction and hence may have some positive correlation with future opportunities for the young person (whether in education or the labour market). In the G-U boundary, in every specification the estimated effect of passing has a negative relationship with the probability of entering youth custody. However, it is only significant in one of six specifications (the most detailed specification for maths – column 6). This may reflect causality but it could also have occurred by chance. The size of the coefficient is sizeable in relation to the mean of the dependent variable and thus is consistent with results from OLS regressions. It is certainly plausible that enabling people to achieve a better education would help keep them out of youth custody – though this analysis suggests it might be very challenging to do so given the significant vulnerabilities faced by this group of young people and reflected in their characteristics.

Conclusion

Individuals entering youth custody at a young age are typically male and are much more likely to come from disadvantaged backgrounds than the population as a whole

Against this background, it is not surprising to see that there is a high negative correlation between educational achievement at GCSE and the probability of ending up in youth custody. In fact, the majority of those in youth custody have a missing entry, no award or a fail for English (66 per cent) and/or maths (78 per cent), even if we include non-GCSE subjects in English and maths for this age group. This means, for the majority of individuals in youth custody, we cannot tell from this data whether incremental changes in skill/human capital would make a difference to their probability of entering youth custody. Our results are consistent with some relationship between getting a qualification (even if low-level) and this probability, though causality is not clear.

What we can say is that many of these individuals have problems that become evident in early adolescence; given that primary school test scores were typically better. Also, many students vulnerable to ending up in youth custody have previously been designated as special needs and/or gone to a special type of institution. This points to the need to discover what types of intervention are being undertaken within these SEN or institutional categories and to discover the extent to which they are effective.

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

Richard Dorsett and David Thomson

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, across numerous dimensions, they tend to experience worse outcomes than other children. 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 but approximately half of all children in custody (Laming, 2016).

In this paper, we examine how the experience of custody affects subsequent education and early labour market outcomes of children in care.⁹ Robust evidence on the effect of incarceration has, until relatively recently, been lacking. 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.

The most robust empirical evidence relates to the case of the US. This may not be informative of other countries with different criminal justice systems. 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. This is a period during which punishment played an important role in the English system. The 2016 Taylor review of the youth justice system 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.'

Our analysis uses an econometric model of transitions between states to provide estimates of the impact of youth custody on labour market status and on earnings. We find evidence of significant effects which differ by gender. Probing the mechanisms generating these impacts, while not conclusive, we find weak support for discrimination being a factor.

Approach

Estimation uses a model of transitions between education, custody, employment and NEET (Not in Employment, Education or Training). For those entering work, earnings are also modelled.

The analysis is based 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. This linked NPD-LEO data has several strengths that make it well-suited to the analysis in this paper. First, it identifies looked-after children. For those looked after for at least a year, information is supplied on whether the child was convicted during the course of the

⁹ Additional results for children as a whole are available on the project website.

year. It is this convicted subgroup of looked-after children that forms the focus for the analysis in this paper. Second, it is sufficiently large to allow the examination of small population subgroups. This is essential when considering custody but it also means that we can provide estimates not just for males but also for females. Third, NPD-LEO offers a rich set of conditioning variables. Time-varying variables in particular help strengthen the analysis.

A feature of the data is that 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.

Overall, 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. The effect of prior custody is captured by including a lagged custody indicator among the regressors.

In addition to our base model, we consider extensions that attempt to test theories of how impacts might be generated. To explore whether impacts are due to the effect of incarceration on children's characteristics and behaviour, we test sensitivity to length of incarceration. The intuition is that this 'behavioural channel' is likely to be more pronounced the longer spent in custody. Econometrically, this is handled by including lagged duration terms. Alternatively, to test the role of discrimination we examine whether impacts vary with the local unemployment rate. The intuition in this case is that employers in tight labour markets are less able to discriminate in favour of applicants that do not have a criminal record. This is captured by including a regressor that interacts the lagged custody indicator with the local unemployment rate.

Results

Figure 6.1 shows the how statuses change on average from age 13 up to age 21. In each of the charts, the lines depict the observed proportions in each state. The estimated model was used to simulate transitions between states over this same age range. The shaded areas in Figure 6.1 correspond to percentiles 2.5 and 97.5 of the simulated distributions and provide an indication of model fit. The proportions observed 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.



Figure 6.1: 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.

Table 6.1 presents estimates of the impact of prior custody on the probability of transitioning between two states. 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 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.

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.

	$e \rightarrow c$	$e \rightarrow w$	$e \rightarrow n$	$w \rightarrow e$	$w \rightarrow c$	$w \rightarrow n$	$n \rightarrow e$	$n \rightarrow c$	$n \rightarrow w$	earnings
Males										
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
Females										
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
e – educatio	n, c – cı	ustody, v	w – emp	oloymen	nt <i>,</i> n – N	EET.				

Table 6.1: Estimates of the impact of prior custody on the transition hazard

While these results demonstrate the statistical significance of prior custody on a range of transition types, the coefficients themselves are difficult to interpret. Figure 6.2 uses simulation to summarise the implied impacts of custody 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). Each graph follows a similar format, showing the estimated impact of being in custody rather than education immediately prior to the spell underway on turning 18. The impacts are shown as lines in the graphs, with shaded areas depicting confidence intervals.

An initial comment 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 6.1, 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.



Figure 6.2: The impacts of having been in custody immediately before current spell.

Notes: Impacts on Education/training, Employment, NEET and earnings (respective row order) are presented for males (left column) and females (right column).

Table 6.2 summarises the estimates in Figure 6.2, showing impacts on number of months spent in each state or, for earnings, the total amount earned, at ages 18, 19, 20 and 21. 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 6.2, 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 6.2 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.

Table 6.2: Estimates of annual impacts

		Education	Employment	NEET (months)	Earnings (£)
		(months)	(months)		
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 <i>,</i> -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
	- Cl	(-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 <i>,</i> 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 <i>,</i> 0.302)	(-0.145 <i>,</i> 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: Unlike Figure 6.2, 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.

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. For females, there was no evidence of lagged duration dependence. For males,

31

the lagged durations were significant in some types of transitions but simulation showed that these do not translate into significant effect on any outcome at any age.

We test the alternative hypothesis – the labelling channel, whereby employers (and others) may discriminate against those who have been in custody – by allowing impacts to vary with local unemployment. We interpret higher unemployment as indicating a looser labour market, with employers able to select from a greater number of applicants. The results are considered by comparing simulated outcomes 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, although there is a short-term increase in the probability of NEET that is statistically significant at 10% level. We take this as weak evidence in support of the labelling hypothesis for males; prior custody increases the probability of NEET more in high unemployment areas than in low unemployment areas. For females, no such effect is found.

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 analysis 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.

Estimated 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. 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.

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.

D. Implications of the findings

The research reported here was motivated by the recognition that youth custody imposes a lasting penalty on those who experience it and that school can play a role in tackling and preventing it.

The results have allowed a fresh perspective on the degree to which youth custody is harmful to labour market prospects. Beyond simply confirming the high likelihood of labour market disengagement among this group, one insight is that this likelihood varies with personal characteristics and circumstances in a way resembling that seen in the population as a whole. In view of this, policies targeting those in custody are more likely to be effective if tailored to the individual in a way that reflects their specific needs and obstacles.

Our results suggest that managing to avoid NEET when 16-18 is an important part of the strategy for avoiding NEET when older. Given this, one might expect the requirement to remain in some form of education or training would reduce the likelihood of youth custody. It is therefore disappointing that raising the participation age (RPA) in England was not found to have any such effect, even though it did cause a small increase in the proportion engaged in some sort of study. Weak implementation may be part of the reason behind this lack of impact, specifically the lack of sanction for non-compliance and importantly the lack of an improved post-16 offer for young people whose first choice is to discontinue their education at age 16. RPA was not supported by increased investment; instead, local authorities were instructed to meet costs from existing budgets.

• It remains important to focus on policies to reduce post-16 attrition rates and early school leaving. To retain dis-engaged young people in education or training requires a coherent and consistent post-16 offer, supported by adequate funding.

A key aim of school is to equip children with the cognitive and non-cognitive skills to navigate later life. Formal qualifications provide the credentials to demonstrate cognitive ability so, by increasing labour market opportunities, may be effective in reducing the attractiveness of criminal activity that could result in custody. Our analysis showed that the majority of those who experienced youth custody had either no record of having taken English or maths at GCSE or had been entered but failed. The fact that many students entering youth custody did much better in primary school (at age 11) than they did at secondary school (at age 16) suggests that, for many, problems either emerge or become evident in early or middle adolescence.

 If qualifications can help prevent custody – as is intuitive and is at least consistent with the results, even if causality is unclear – Key stage 3 onwards may represent a critical phase. In addition, the high prevalence among the youth custody population of special needs designation and non-mainstream education suggests a potential focus for policies to improve attainment among this group.

For those who seriously misbehave at school, exclusion represents the most severe punishment in the education system. While it is recognised that exclusion is a risk factor for juvenile delinquency, there is less certainty about its causal impact. Our results suggest 50

exclusion does indeed increase the probability of custody, substantially so in the case of permanent exclusion. In view of the widespread use of exclusion in England, this finding raises an important concern for educational practice.

• Headteachers, teachers and policymakers should recognise the possibility that exclusion may increase the probability of a child experiencing custody. More generally, additional research to understand the mechanism behind this impact is important for the identification of ways to ameliorate it.

Reflecting the social nature of schools, our analysis has provided new evidence on how the ability of a pupil's peers influences the probability of custody. We find a significant relationship; higher-attaining peers reduce the probability of custody. As clues to the mechanism behind this, we find also that having higher-attaining peers increases performance at key stage 4 (suggesting a possible qualifications channel) and reduces unauthorised absence (suggesting a possible behavioural channel).

• The existence of peer effects suggests school interventions may have multiplier effects beyond their intended aim. The significance of social networks is relevant to considerations around selection into schools and policies around streaming.

For care-experienced young people who experience youth custody, we provide new evidence that subsequent labour market outcomes are harmed. This appears to affect males and females differently. For males, youth custody reduces the probability of being in employment while for females the only impact is on earnings among those employed.

• It seems likely that males and females with experience of custody face different obstacles in the labour market. For males, discrimination may be important, raising issues for policy around disclosure of criminal records. For females, it may be that discrimination prevents access to higher-earnings jobs rather than any jobs. More generally, low attainment is likely to present a further obstacle for males and females, highlighting the importance of re-engaging with education both during custody and post-release.

Thoughts on further research

The research conducted as part of this project answers some questions and raises others. There are many possible avenues to build on the results uncovered or to deepen understanding of them. A point we would highlight is that, while we have focused on the experience of custody, this is a status that arises as a result of behaviour and interaction with the justice system, both of which are only partially observed in the data used here. Since the start of this project, new data have become available in the form of the linked Department for Education and Ministry of Justice data that allows for a richer exploration of offending. Again, this is partial, this time to the extent that it only records those offenders who are caught. Nevertheless, it moves us closer to understanding the behaviour that effective policy must influence if it is to reduce youth custody or offending more generally and so is likely to represent a useful focus for future research.

References

Aizer, A., & Doyle Jr, J. J. (2015). Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges. The Quarterly Journal of Economics, 130(2), 759-803.

Anders, J. and Dorsett, R. (2017) What young English people do once they reach schoolleaving age: A cross-cohort comparison for the last 30 years Longitudinal and Life Course Studies 8 (1): 75-103.

Bayer, P., Hjalmarsson, R., & Pozen, D. (2009). Building criminal capital behind bars: Peer effects in juvenile corrections. The Quarterly Journal of Economics, 124(1), 105-147.

Bell, B., A. Bindler and S. Machin (2018). Crime Scars: Recessions and the Making of Career Criminals. Review of Economics and Statistics, 100, 392-404.

Billings, S. B., Deming, D. J., & Ross, S. L. (2019). Partners in crime. American Economic Journal: Applied Economics, 11(1), 126-50.

Deming, D. J. (2011). Better schools, less crime?. The Quarterly Journal of Economics, 126(4), 2063-2115.

Department for Education (2019). School exclusion: a literature review on the continued disproportionate exclusion of certain children.

Dorsett, R. and Lucchino, P. (2014) Explaining patterns in the school-to-work transition: an analysis using optimal matching. Advances in Life Course Research 22: 1-14.

Gregg, P. (2001) The impact of youth unemployment on adult unemployment in the NCDS. Economic Journal 111(475): F626-53.

Gregg, P. and Tominey, E. (2005) The wage scar from male youth unemployment. Labour Economics 12(4): 487-509.

HM Government. (2019). A whole-system multi-agency approach to serious violence prevention.

HM Government. (2021). Permanent exclusions and suspensions in England, Academic Year 2019/20.

Laming, H. (2016). In care, out of trouble: How the life chances of children in care can be transformed by protecting them from unnecessary involvement in the criminal justice system. London: Prison Reform Trust.

Local Government Association. (2018). The relationship between family violence and youth offending.

Machin, S., & Sandi, M. (2020). Autonomous schools and strategic pupil exclusion. The Economic Journal, 130(625), 125-159.

Office for National Statistics. (2021). Open academies, free schools, studio schools and UTCs.

Smith, I. D. (2007). Being tough on the causes of crime: Tackling family breakdown to prevent youth crime. Social Justice Challenge, 1-15.

Valdebenito, S., Eisner, M., Farrington, D. P., Ttofi, M. M., & Sutherland, A. (2018). Schoolbased interventions for reducing disciplinary school exclusion: a systematic review. Campbell Systematic Reviews, 14(1), i-216.

Youth Custody Service (2022) *Monthly Youth Custody Report – July 2022: England and Wales*.

https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment __data/file/1015040/youth-custody-report-july-2021.xlsx