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

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Abstract

The negative relationship between education and crime is well documented for many countries. In England, continued participation in education beyond the compulsory minimum school leaving age of 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. This paper estimates the causal effect of continuing in education post-16 on the probability of experiencing youth custody at ages 17 and 18, addressing the endogeneity of continued participation by exploiting the natural experiment created by the 'raising of the participation age' in England 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 exogenous variation in participation between cohorts we estimate the causal effect of continued participation on custody outcomes at ages 17 and 18 using Two-Stage Least Squares and Regression Discontinuity methods. The effect of the law change was to increase the proportion of young people participating in education at age 17 by 1.7pp (1.2pp) for boys (girls), from a base of 82.1% (85.0%) immediately prior to the reform. Despite this increase in participation, there was no identified effect on the probability of custody when aged 17 or 18. This suggests that the 0.64pp (0.04pp) reduction in probability of custody associated with continued participation for boys (girls) estimated by OLS is driven by selection. Results are robust to estimation method and whether the treatment is participation in education or training, participation in school, years of post-16 education or training or years of post-16 schooling.

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Highlights

- 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 and women, 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 1.7pp (1.2pp) for men and women respectively from a base of 82.1% (85.0%) immediately 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 whether the treatment is participation in education or training or participation in school.

- The implications are that (a) the policy was not strongly enforced, hence compliance was very low; and (b) for the compliers with the policy, the additional educational participation did not impact on their likelihood of later custody.

1 Introduction

The negative relationship between education and crime is well documented for many countries around the world (see *inter alia* Lochner & Moretti, 2004; Machin et al., 2011; Hjalmarsson et al., 2015). 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 or later 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.

We use a large linked-administrative dataset containing the near total population of young people in education in England in the cohorts immediately around the reform. This rich education data is then linked with month-by-month information on each individual's recorded activity in the following two academic years, including participation in education or training and experience of custody. The analysis uses both a parametric two-stage least squares approach to estimation as well regression discontinuity (RD) methods to derive instrumental variables (IV) estimates of the return to education participation in reduced crime.

The contribution of the paper is threefold. Firstly, this is the first study to examine the impact of the RPA in England on educational participation, establishing the extent to which the policy has succeeded in its stated aim i.e. increasing post-16 education and training participation. Secondly, the paper then exploits this natural experiment to derive an estimate of the causal effect of participation on custody outcomes. Thirdly, we derive important policy implications around the implementation of compulsory education reforms and the extent to which they achieve compliance, and concerning the extent to which compulsory education post-16 can be a useful policy lever to prevent later custody given the particular complier group impacted by the policy.

The rest of this paper is organised as follows. Section 2 sets out the policy context, before section 3 introduces the data and provides a descriptive analysis; section 4 details the methods, section 5 presents the results before sections 6 and 7 offer discussion, conclusions and policy recommendations.

2 The Raising of the Participation Age in England

The English schooling system assigns students to a cohort (often referred to as a 'school year') based on their date of birth, with a cohort including all individuals born between 1st September in year t and the 31st August in year *t+1* inclusive. All children must start school in the academic year[1] in which they will turn 5-years old and since the most recent 'Raising of the School Leaving Age' (RoSLA) in 1972, students must remain in school until the end of the academic year in which they turn 16-years old. A large majority of each cohort do remain in education or training in the immediate post-compulsory years however for the last thirty years England has had a persistent problem of a non-trivial proportion of young people disengaging from education and training at the earliest opportunity, contributing to a relatively high and stubborn NEET (not in education, employment or training) rate among 16-18-year-olds. Dept. for Education analysis using the UK Labour Force Survey data shows that 8.4% of England's 16-18 year olds were classified as NEET in 1994 and this remained stable through to 2012 when it was 9.2% (DfE, 2022).

The idea that compulsory schooling reforms can increase educational participation, qualification attainment and lead to increased employment, earnings and other positive outcomes to benefit individuals and society has strong empirical support (see Buscha and Dickson, 2023, for a comprehensive summary of findings in the literature on the returns to education exploiting compulsory schooling reforms). Within the UK specifically numerous papers have shown that the 1972 'Raising of the School Leaving Age' (RoSLA) improved average years of schooling by approx. 0.25-0.33 years, reduced the proportion of each cohort with no qualifications and increased the proportion with CSE and O-level qualifications (Dickson and Smith, 2011; Buscha and Dickson, 2012; Avendano et al, 2020). The causal effect of this additional education has been seen in terms of increased employment (Dickson and Smith, 2011), higher wages (Grenet, 2013, Buscha and Dickson, 2018, Delaney and Devereux, 2019), improved health (Davies et al., 2018; Silles, 2009), and reductions in teenage childbearing (Silles, 2011; Wilson, 2012). Importantly for this study, the 1972 RoSLA has been shown to have had an impact to reduce crime – focusing on the cohorts immediately before/after the reform, Machin et al. (2011) estimate that the policy led to an 8% reduction in the conviction rate, an 11 percentage point drop in the proportion of individuals with no qualification and an increase of an average of 0.38 years of schooling. These reduced form estimates imply that a 10% reduction in the proportion with no qualifications reducing the conviction rate by 7% and an additional 0.1 years of schooling reduces the conviction rate by 3%.

In light of this large body of research showing positive impacts of the 1972 RoSLA on education and other life outcomes, the UK Government viewed RPA as a way to address the NEET issue in England, boost education and training performance in comparison to other OECD countries

[1]The academic year in England technically runs from 1st September in year t until 31st August in year *t+1* though school typically starts at some point in the first week of September and ends mid-way through July.

and improve young people's economic and social outcomes (DfES, 2007). The RPA policy was introduced in September 2012 such that the cohort starting year 11 that month must remain in some form of education or training until the end of academic year that they turn 17; the cohort starting year 11 in Sept 2013 must stay to until they reach their 18th birthday or until they achieve a Level 3 qualification (A-level equivalent). Unlike previous compulsory school leaving age policies, such as the 1972 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.

3 Data

To estimate the effect of continued participation on likelihood of custody, we use linked administrative dataset from the National Pupil Database (NPD), specifically: the Schools Census (SC), Pupil Referral Unit Census, Key Stage 2 and Key Stage 4 results, the 'Looked after Children' dataset, the 'Children in Need' dataset, and the National Client Caseload Information System (NCCIS). The NCCIS data allows us to construct the activity of each individual on a monthly basis for the two academic years following the end of their compulsory schooling period (i.e. year 12 and year 13). From this we can create an indicator for their continued participation in any form of education or training, and from SC data we can create an equivalent indicator for presence on a school register. In each case we consider those appearing in education/training or on a school register for at least 6 months of the academic year to be continuing their participation. Data from the NPD gives a rich array of information on the socio-demographic characteristics of each individual and their prior attainment in school from age 11 up to age 16. The constructed dataset contains information from all individuals in state-funded 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.

The implementation of the policy means that in our data, the first two cohorts were not subject to RPA, but the latter two were. The cohort born from September 1996-August 1997 (who therefore start year 11 in September 2012) are required to be in education or training for the following academic year (year 12) and those born from September 1997-August 1998 are in addition required to stay for at least the part of year 13 that takes them up to their 18th birthday. Given these timings, we have two cohorts with a consistent treatment of being required to remain in education or training throughout the academic year in which they turn 17 (year 12) and so this is the treatment that we focus on in the analysis.

The data for reduced form estimation contains 1,282,709 observations for males, 1,231,647 for females, with the estimation samples for models including covariates slightly smaller at 816,089 and 792,159 respectively. Table 1 contains the means and standard deviations of key variables broken down by gender and pre- and post-RPA periods.

Table 1: Education and custody outcomes by policy period and gender

	All		RPA=0		RPA=1	
Male	Mean	Std.Dev	Mean	Std.Dev	Mean	Std.Dev
Ever in custody age 17 or 18	0.00403	0.06336	0.00478	0.06896	0.00329	0.05726
Participating in educ/training age 17	0.82341	0.38132	0.80998	0.39232	0.83674	0.36960
Participating in school 17	0.34364	0.47492	0.33809	0.47306	0.34914	0.47670
N	1,282,709		638,644		644,065	

	All		RPA=0		RPA=1	
Female	Mean	Std.Dev	Mean	Std.Dev	Mean	Std.Dev
Ever in custody age 17 or 18	0.00027	0.01639	0.00033	0.01823	0.00021	0.01433
Participating in educ/training age 17	0.85233	0.35478	0.83938	0.36718	0.86518	0.34153
Participating in school 17	0.38219	0.48592	0.37475	0.48406	0.38958	0.48765
N	1,231,647		613,480		618,167	

As can be seen in the table, custody is a rare experience with only 0.4% of males in the data experiencing custody aged 17 or 18 and for females it is an order of magnitude lower at 0.03%. The large standard deviations similarly reflect that for most individuals in these cohorts the outcome variable is zero. In terms of participation, a large percentage of students remain in some form of education or training in the first year post-compulsory schooling age, 82.4% of males and 85.2% of females across all of the data. Just over one-third of each cohort of males in the data are continuing in school post-16 (34.4%), with the corresponding proportion of females being slightly higher (38.2%).

For both genders the post-RPA period shows a lower probability of custody than is the case before implementation, and also a higher propensity to remain in education or training overall and a higher propensity to remain in school specifically. Though these raw numbers imply a correlation between the policy, participation and custodial outcomes, these variables exhibit strong trends over time which we now turn to explore.

3.1 Descriptive Picture

Impact of RPA on education participation at age 17

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 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 2pp in the cohort immediately prior to RPA. Following RPA this trend continues in the next cohort, before flattening out. Figure 2 shows that the pattern is almost exactly the same for females, though from a starting point of slightly higher participation. Visual inspection of the fitted lines suggests little evidence of a discontinuity in participation at age 17 as a result of RPA.

Figure 1: Proportion continuing in education or training by year-month of birth, males

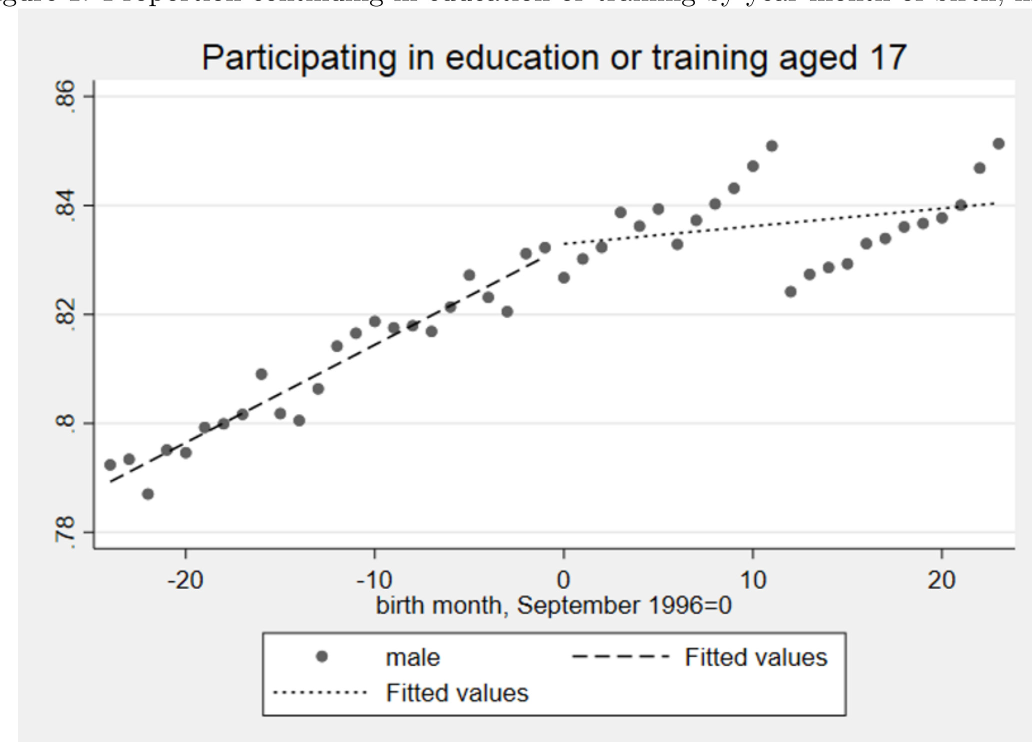
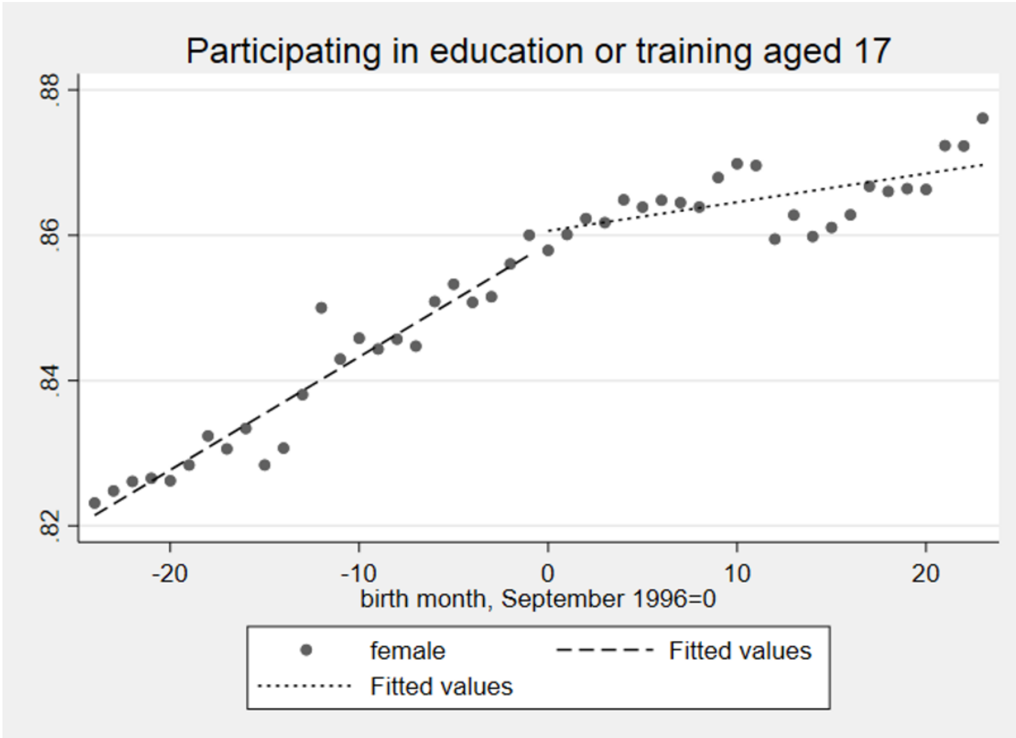


Figure 2: Proportion continuing in education or training by year-month of birth, females



Impact of RPA on school participation at age 17

Despite the lack of evidence for a discontinuous increase in participation in all forms of education or training, Figure 3 shows that there does appear to be more of a visible impact on the type of participation at age 17 following RPA. There is an increase in the proportion of the cohort remaining in school from approximately 0.34 to 0.35 between the cohorts pre- and post-RPA. Correspondingly for females (Figure 4) there a slightly larger and more obvious increase from around 0.37 to 0.39.

Figure 3: Proportion continuing in school by year-month of birth, males

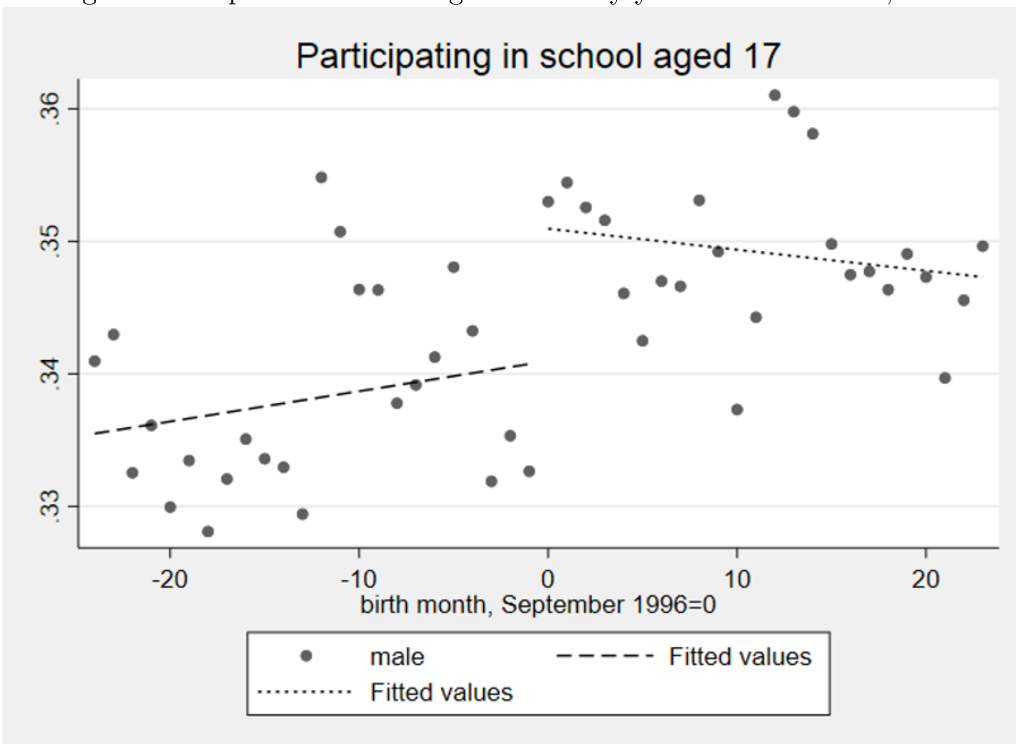
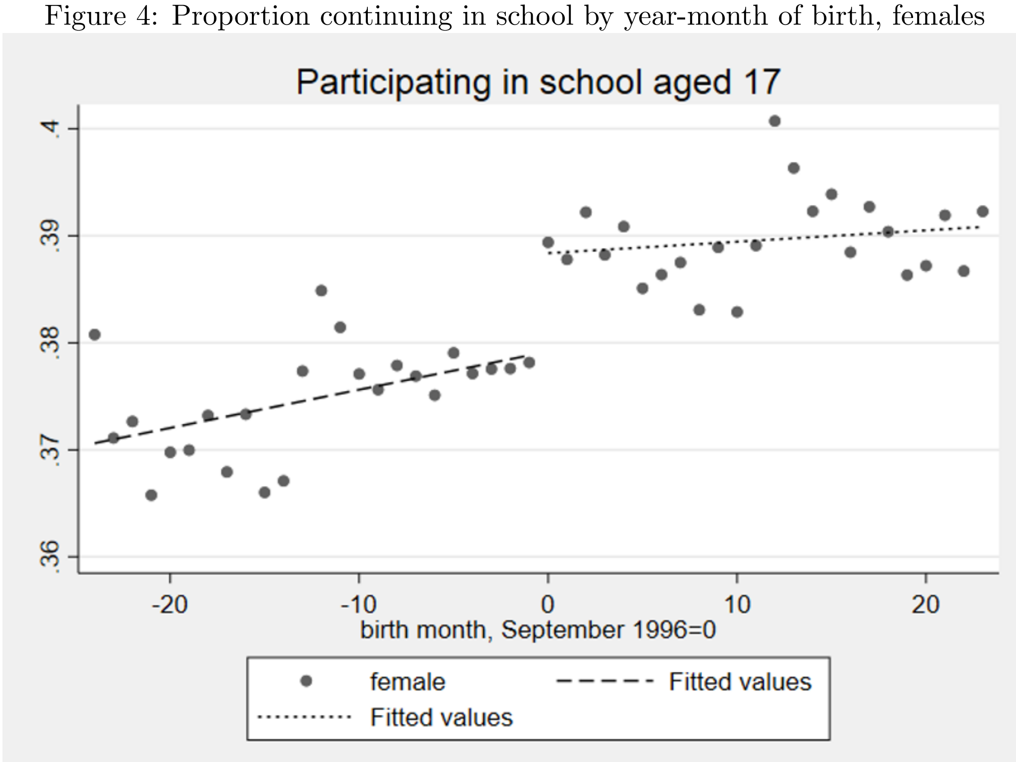


Figure 4: Proportion continuing in school by year-month of birth, females



Impact of RPA on custody at age 17 or 18

As shown in Table 1, experience of custody at ages 17 or 18 is extremely rare: pooled over the four cohorts of data, the proportion who are ever recorded in custody at 17 or 18 is 0.0040 for males, 0.0003 for females; moreover, the proportion has been falling consistently over recent years. Figures 5 and 6 show this pattern against the year and month of birth.

As shown in Figure 5, the proportion of males experiencing custody at ages 17 or 18 is highest among the older cohorts, declining from around 0.0054 for those born in the earliest cohort to around 0.0029 for those in the youngest. The fitted lines suggest no discontinuity in experience of custody at the point at which the RPA is implemented. For females (Figure 6) there is a similar fall, from a lower starting position: the proportion amongst the oldest cohort being around 0.00039, falling to around 0.00019 for the youngest cohort in the data. As with participation in any form of education or training, the linear fitted lines suggest no discontinuity at the point of RPA.

Figure 5: Proportion experiencing custody by year-month of birth, males

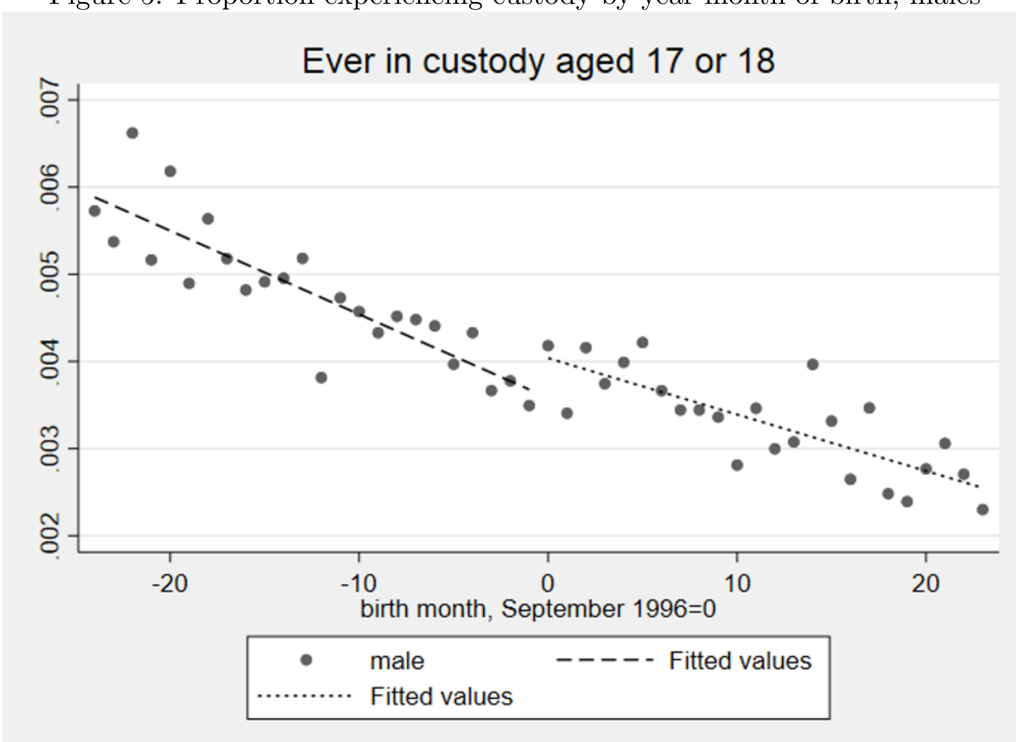
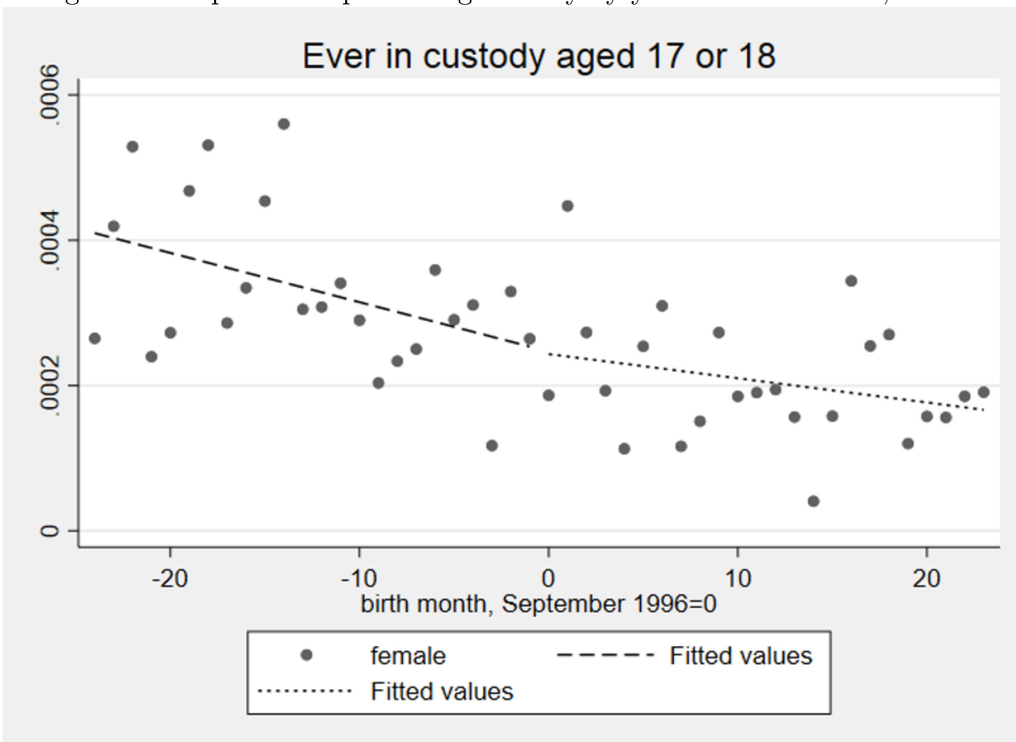


Figure 6: Proportion experiencing custody by year-month of birth, females



4 Methods

Formal estimation will initially estimate the impact of the RPA policy on education participation and on the outcome variable i.e. experience of custody when aged 17 or 18-years old:

$$E_i = \alpha_1 + \beta_1 RPA_i + g_1(c) + \mathbf{X}_i'\gamma_1 + \varepsilon_{1i} \tag{1}$$

$$Y_i = \alpha_2 + \beta_2 RPA_i + g_2(c) + \mathbf{X}_i'\gamma_2 + \varepsilon_{2i} \tag{2}$$

in which (1) and (2) are the reduced form equations in which E_i represents the education outcome measures (sustained participation in education or training in the first school year post-16, sustained participation in school in the first school year post-16) and Y_i is a dummy variable equal to 1 if the young person experiences custody when aged 17 or 18.

In the multivariate regression approach to estimating these reduced form equations, $g_k(c)$ are linear or quadratic functions of school cohort, the vector $\mathbf{X}$ contains a range of characteristics of the individual students including age-within-cohort, ethnicity, histories of absence and exclusions, special educational needs, free school meal eligibility history, family income deprivation decile (at age 11 and age 16), looked after child status, school type, and prior attainment at KS4 and KS2. β_1

captures the impact of being in a cohort affected by RPA on the educational participation measures, while β_2 captures the impact of being in an RPA affected cohort on experience of custody. ε_1 and ε_2 are two normally distributed, mean-zero error terms.

Given the administrative nature of the data, we are able to pursue a regression discontinuity approach that does not impose the parametric restrictions of the multivariate regressions, instead relying on the fact that the large sample nature of the data should mean that cohorts before and after RPA are well balanced in their observed and unobserved characteristics such that we just need to take account of the ongoing smooth changes over cohorts in educational and other outcomes, and estimate the discrete change induced by the RPA policy. As such, no additional control variables are included in the RD specifications. Following the best-practice recommendations in the literature (Imbens and Lemieux, 2008; Gelman and Imbens, 2014) we will use the local linear regression approach to RD, with a rectangular kernel and number of different bandwidths around the discontinuity. To estimate the value of β_1 (and similarly for β_2) we fit a linear regression function to observations within a distance h on either side of the discontinuity point (month-of-birth = September 1996). Formally, the local linear regression approach solves the equations:

$$\min_{\alpha_{preRPA};\beta_{preRPA}} \Sigma_{i:c-h<mi<c} \left(E_i - \alpha_{preRPA} - \beta_{preRPA}.(m_i - c)\right)^2 \tag{3}$$

and

$$\min_{\alpha_{postRPA};\beta_{postRPA}} \Sigma_{i:c-h<mi<c} \left(E_i - \alpha_{postRPA} - \beta_{postRPA}.(m_i - c)\right)^2 \tag{4}$$

in which E_i is the education measure, m_i is the number of months an individual is born before/after the September 1996 discontinuity for which $c = 0$, the αs and βs are the regression intercepts and slope values computed for data in the region surrounding the discontinuity, c, within bandwidth, h. These parameters are calculated separately for the pre-RPA cohorts to the left-hand side of c, born pre-September 1996 (equation 3) and the post-RPA cohorts to the right-hand side of c, born post-September 1996 (equation 4). The intercept values at the discontinuity, $\mu_{preRPA}(c)$ and $\mu_{postRPA}(c)$ can then be computed:

$$\hat{\mu}_{preRPA}(c) = \hat{\alpha}_{preRPA} + \hat{\beta}_{preRPA}.(c - c) = \hat{\alpha}_{preRPA} \tag{5}$$

and

$$\hat{\mu}_{postRPA}(c) = \hat{\alpha}_{postRPA} + \hat{\beta}_{postRPA}.(c - c) = \hat{\alpha}_{postRPA} \tag{6}$$

We can then compute the treatment effect of RPA on our education measure as:

$$\hat{\beta}_1 = \hat{\alpha}_{postRPA} - \hat{\alpha}_{preRPA} \tag{7}$$

To avoid possible contamination of treatment effects with age-within-school cohort effects, we need to use bandwidths which are multiples of 12-months and as we only have two cohorts either side of the discontinuity, we are limited to bandwidths of 12- and 24-months.

Since the RPA policy change was an exogenous event that increased levels of participation we can identify the causal effect of the additional education on custody using the estimates from the reduced form models.

A structural form equation capturing the impact of education participation E_i on custody Y_i can be given by:

$$Y_i = \alpha_3 + \beta_3 E_i + \mathbf{X}_i' \gamma_3 + \varepsilon_3 \tag{8}$$

We can firstly estimate this equation by ordinary least squares regression (i.e. using a linear probability model) but because we anticipate the coefficient estimates in this case to contain bias – owing to the correlation between continued educational participation and unobservable characteristics that also affect the probability of custody and are captured in ε_3 – we construct an instrumental variables (IV) estimate of β_3 in equation (8) from the ratio of the reduced form coefficients from equations (1) and (2): $\beta_3 = \beta_2/\beta_1$. This IV estimate of β_3 uses only the exogenous variation in E_i induced by the RPA policy and as such should not be correlated with ε and allow a consistent estimation of the parameter β_3.[2]

In exactly the same way as the parametric approach outlined above, the fuzzy RD approach uses the estimates of β_1 and β_2 from equation (7) and the corresponding equation for β_2, to construct an estimate of β_3.[3]

5 Results

5.1 Regression Estimates

We now examine these visual patterns more formally through regression discontinuity methods and a more parametric two-stage least squares approach as set out above. Results from a local linear regression discontinuity approach, using the maximum bandwidth possible in the dataset (24-months), are displayed in Table 2. Confirming the evidence from the figures, the RPA is estimated to have a very small positive effect on the probability of continued participation in education or training for both males and females, but in neither case is this statistically significantly different

[2]In practice the two-stage least squares estimate is computed in a single routine via the `ivreg2` command in stata in order to compute the correct standard errors.

[3]The local linear fuzzy RD approach will be implemented using the rdrobust command in stata.

to zero. The RPA is associated with a small *increase* in the probability of custody at ages 17 or 18 for males, by 0.0004. The pre-RPA average probability of custody is 0.00478, therefore this represents a fairly small increase albeit significant at the 5% level. For females the point estimate of the impact on probability of custody is a very tiny negative (-0.00000577) with a standard error two orders of magnitude larger. The RD estimate therefore provide little evidence of an effect of RPA on education and training participation overall, with the only significant estimate suggesting a slight increase in probability of custody for males aged 17 or 18 as a result of the policy.

Table 2: Regression discontinuity estimates of the RPA effect on probability of custody, education and training participation and the implied impact of education participation on custody, bandwidth 24 months

Males	coeff.	std. err.	z	p
Impact of RPA on custody	0.0004	0.0002	2.03	0.042
Impact of RPA on participation	0.0005	0.0013	0.37	0.713
Wald estimate of impact of participation on custody	0.9192	2.5772	0.36	0.721
N=1,282,709				

Females	coeff.	std. err.	z	p
Impact of RPA on custody	-0.0000	0.0001	-0.10	0.921
Impact of RPA on participation	0.0018	0.0013	1.39	0.164
Wald estimate impact of participation on custody	-0.0033	0.0330	-0.10	0.921
N=1,231,647				

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, with only two cohorts either side of the discontinuity and a need to use bandwidths in multiples of 12-months to avoid contamination with age-within-year effects, there is little space for the local linear regression functions to fit to the data. Moreover, 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. Tables 3 and 4 contains the estimates from this approach.

Table 3: Two-stage least squares and OLS estimates of the RPA effect on education and training participation and probability of custody, males

	Reduced form				2SLS: first stage			
Dependent variable:	Ever in custody aged 17 or 18				Participating in educ/training aged 17			
	coeff.	std. err.	t	p	coeff.	std. err.	t	p
RPA (born Sept 1996 or later)	0.0001	0.0002	0.31	0.759	0.0169	0.0014	12.12	0.000
	R^2=0.0534				R^2=0.0794			
					F-stat on excluded instrument=146.9			

	OLS				2SLS: second stage			
Dependent variable:	Ever in custody aged 17 or 18				Ever in custody aged 17 or 18			
	coeff.	std. err.	t	p	coeff.	std. err.	t	p
Participating in educ/training aged 17	-0.0064	0.0003	-20.92	0.000	0.0039	0.0128	0.31	0.760
	R^2=0.0551				R^2=0.0508			

N=816,089

Note: controls included: year, age-within-cohort, ethnicity, IDACI decile age 11, IDACI decile age 16, % of primary school time FSM, % secondary school time FSM, ever looked after up to age 16, ever child in need up to age 16, ever subject to a child protection plan, SEN at primary school, SEN at secondary school, KS4 av. points at GCSE, KS4 English points score, KS4 Maths points score, attainment at 16 (dummies for sub-level1, level 1, level 3), KS2 English score, KS2 Maths score, KS2 Science score, KS2 av. points score, KS4 school type (dummies for independent, alternative provision, other), attended pupil referral unit (secondary), permanently excluded (primary), permanently excluded (secondary), any temp. exclusions (secondary), prop. sessions unauthorised absence at age 11, 12, 13, 14, 15, 16.

Table 4: Two-stage least squares and OLS estimates of the RPA effect on education and training participation and probability of custody, females

	Reduced form				2SLS: first stage			
Dependent variable:	Ever in custody aged 17 or 18				Participating in educ/training aged 17			
	coeff.	std. err.	t	*p*	coeff.	std. err.	t	*p*
RPA (born Sept 1996 or later)	0.00001	0.0001	0.22	0.830	0.0123	0.0012	9.86	0.000
	R^2=0.0049				R^2=0.0807			
					F-stat on excluded instrument=97.2			

	OLS				2SLS: second stage			
Dependent variable:	Ever in custody aged 17 or 18				Ever in custody aged 17 or 18			
	coeff.	std. err.	t	*p*	coeff.	std. err.	t	*p*
Participating in educ/training aged 17	-0.0004	0.0001	-4.57	0.000	0.0009	0.0044	0.22	0.830
	R^2=0.0050				R^2=0.0040			

N=792,159

Note: for controls included see Table 3.

For Tables 3 and 4 each quadrant is from a different regression model. The top left and top right quadrants show the reduced form impact of the RPA on ever experiencing custody aged 17 or 18 (left) and the first stage impact of RPA on participation in education or training age 17 (right). The lower left quadrant shows the ordinary least squares (OLS) estimate of the impact of continuing to participate in education or training at age 17 on likelihood of experiencing custody at age 17 or 18, controlling for a rich array of socio-demographic and prior attainment characteristics of each individual and the lower right shows the IV estimate of the impact of participation on custody.

Focusing first on Table 3, the OLS estimate suggests that all else equal continued participation reduces likelihood of custody by 0.6pp. The overall custody risk for males in the data is 0.4% which indicates the strength of the correlation between participation and probability of custody. Even controlling for a large number of characteristics, selection into participation is likely to be correlated with unobservable characteristics that also affect probability of custody. As such, this estimate is likely to be negatively biased (i.e. the true effect of education participation in reducing likelihood of custody is likely to be smaller in absolute value than 0.6pp).

The upper right quadrant of Table 3 shows the first stage estimate of the effect of RPA on continued participation in education or training at age 17. Controlling for the full range of characteristics, the estimate is a statistically significant 1.7pp increase in participation, which accords with the visual evidence in Figure 1. The F-stat on the excluded instrument is 146.9, providing confidence that there is no concern around weak instruments bias. The top left quadrant of Table 3 shows that when we add controls to the regression model, the reduced form effect of RPA on likelihood of custody is smaller than in the estimate in Table 2 and very imprecisely estimated. As such, given the positive first stage effect, the instrumental variables estimate of the impact of participating in education or training is small, positive and very imprecisely estimated. This suggests that the impact of continued participation in education or training on the probability of custody is zero for the group of young men whose participation was positively impacted by the reform.

Turning to Table 4, as with the case for males, the OLS suggests continued participation in education or training reduces the chances of custody. For females the coefficient suggests a reduction by 0.04pp, this is relative to a base percentage experiencing custody of 0.03%, again reflecting the strength of the conditional correlation between participation and (not) experiencing custody.

The first stage impact of RPA on participation is a 1.2pp increase, statistically significant at all conventional levels and again with the F-statistic of 97.2 there is no concern about the strength of the instrument. However, the top-left quadrant of Table 4 shows that the reduced form impact of RPA on probability of custody is essentially zero, with a coefficient of 0.00001. Combined with the first stage impact, this translates to a small, positive IV estimate though again essentially zero.

Taking the two regression approaches together, along with the visual inspection of the data, it appears that the RPA had a small impact on continued participation in education or training for both males and females, increasing participation by approx. 1.5pp and 1.2pp respectively. However, this increased participation did not have a significant impact on likelihood of custody.

We next carry out the same exercise to examine the effect of RPA on participation in school specifically. Table 5 contains the RD estimates, with Tables 6 and 7 containing the corresponding 2SLS estimates. The estimates of the impact of RPA on custody directly are by construction identical to those in Tables 2, 3 and 4, since it is only the treatment (participation in school rather than in education or training more generally) that is changing. The difference comparing Table 5 with Tables 6 and 7 is that in contrast to when comparing Tables 2, 3 and 4, the first stage estimates of the effect of RPA on school participation are more precisely estimated and for each gender the RDD and two-stage least squares estimates accord with each other. For males, the RD estimate is a 1.0pp increase in the probability of participating in school at age 17, whereas the two-stage least squares estimate is a 1.1pp increase. The RD estimate for females is 0.9pp increase, with a two-stage least squares estimate of 1.4pp. These are relative to participation rates in school in the cohort immediately prior to RPA of 34.2% for males and 37.8% for females.

Given the reduced form impact of RPA on custody estimated by each approach is essentially zero for females, the implied estimates of the impact of continued participation in school on custody are both essentially zero, one small positive, one small negative with standard errors an order of magnitude larger than the coefficients.

For males, the RD specification estimates a small *positive* effect of RPA on probability of custody, significant at the 5% level. Combined with the positive impact on school attendance, this gives a Wald estimate of the impact of school participation on custody of a 4.4pp increase, with a p-value of 0.057. However, this result is not robust to the alternative two-stage least squares estimation approach in which the reduced form is a much smaller (0.01pp as opposed to 0.04pp) effect and not statistically significant. The reduced form estimates are particularly sensitive to the specification, as explored in section 5.3 below.

Both RD and conventional two-stage least squares approaches and the visual evidence suggest that for each gender RPA increased participation in school at age 17 but that this did not translate into any effect on likelihood of custody at ages 17 or 18. Comparing the OLS estimates for participation in education or training and participating in school suggests less of an effect of school than perhaps we would expect. However, this may be because the group who do not participate in school are a mix of those who do not continue to participate in education or training at all and those who continue in a non-school setting. Therefore, the school attenders are more similar to their comparator group than is the case for those participating in any form of education or training and those not, hence a smaller impact for school participation. For females, the OLS estimate suggests a very small, *positive* relationship between school attendance and custody, with school attendance increasing the probability by 0.00004. The overall probability of custody for females in the data is

0.0003 so this represents a very small relationship albeit positive. For females there seems to be a difference in the relationship with custody experience between those participating in some form of education or training at all and those not, and the relationship between school participation or not. It appears that participating in some form of education or training is associated with lower probability of custody, whereas school attendance compared to either participating in other settings or non-participation is positively associated with custody. The positive effect of school is notably an order of magnitude smaller than the negative effect found for participating in education at all in the OLS results in Table 4. However, we need to be careful not to over-interpret the OLS result in Table 7, in a context where the rich array of variables available can still only explain less than half a percent of the variation in custody experience (in contrast to the case for males in which the OLS R-squared is an order of magnitude larger, with 5.4% of variation explained). Even in these OLS regressions, the female standard errors are substantially larger than for males with t-statistics much larger for the male coefficients on participation in education or training or in school specifically. The reality is that for females custody at age 17 or 18 is an extremely rare event and does not appear to be well explained by the vast array of covariates available in the linked administrative dataset.

Given the greater prevalence of custody experience for males, we now focus on the male results only and explore whether there is a heterogeneous effect by individual ethnicity.

Table 5: Regression discontinuity estimates of the RPA effect on probability of custody, school participation, and the implied impact of school participation on custody, bandwidth 24 months

Males	coeff.	std. err.	t	p
Impact of RPA on custody	0.0004	0.0002	2.03	0.042
Impact of RPA on school participation	0.0103	0.0017	6.15	0.000
Wald estimate of impact of participation on custody	0.0437	0.0229	1.90	0.057
N=1,282,709				

Females				
Impact of RPA on custody	-0.0000	0.0001	-0.10	0.921
Impact of RPA on school participation	0.0093	0.0017	5.30	0.000
Wald estimate of impact of participation on custody	-0.0006	0.0063	-0.10	0.921
N=1,231,647				

Table 6: Two-stage least squares and OLS estimates of the RPA effect on school participation and probability of custody, males

	Reduced form				2SLS: first stage			
Dependent variable:	Ever in custody aged 17 or 18				Participating in school aged 17			
	coeff.	std. err.	t	*p*	coeff.	std. err.	t	*p*
RPA (born Sept 1996 or later)	0.0001	0.0002	0.31	0.759	0.0113	0.0020	5.55	0.000
	R^2=0.0534				R^2=0.1984			
					F-stat on excluded instrument=30.7			

	OLS				2SLS: second stage			
Dependent variable:	Ever in custody aged 17 or 18				Ever in custody aged 17 or 18			
	coeff.	std. err.	t	*p*	coeff.	std. err.	t	*p*
Participating in school aged 17	-0.0008	0.0001	-9.87	0.000	0.0059	0.0193	0.31	0.760
	R^2=0.0535				R^2=0.0498			

N=816,089

Note: controls included: year, age-within-cohort, ethnicity, IDACI decile age 11, IDACI decile age 16, % of primary school time FSM, % secondary school time FSM, ever looked after up to age 16, ever child in need up to age 16, ever subject to a child protection plan, SEN at primary school, SEN at secondary school, KS4 av. points at GCSE, KS4 English points score, KS4 Maths points score, attainment at 16 (dummies for sub-level1, level 1, level 3), KS2 English score, KS2 Maths score, KS2 Science score, KS2 av. points score, KS4 school type (dummies for independent, alternative provision, other), attended pupil referral unit (secondary), permanently excluded (primary), permanently excluded (secondary), any temp. exclusions (secondary), prop. sessions unauthorised absence at age 11, 12, 13, 14, 15, 16.

Table 7: Two-stage least squares and OLS estimates of the RPA effect on school participation and probability of custody, females

	Reduced form				2SLS: first stage			
Dependent variable:	Ever in custody aged 17 or 18				Participating in school aged 17			
	coeff.	std. err.	t	*p*	coeff.	std. err.	t	*p*
RPA (born Sept 1996 or later)	0.00001	0.0001	0.22	0.830	0.0141	0.0021	6.65	0.000
	R^2=0.0049				R^2=0.1742			
					F-stat on excluded instrument=44.2			

	OLS				2SLS: second stage			
Dependent variable:	Ever in custody aged 17 or 18				Ever in custody aged 17 or 18			
	coeff.	std. err.	t	*p*	coeff.	std. err.	t	*p*
Participating in school aged 17	0.00004	0.00002	2.42	0.016	0.0008	0.0038	0.22	0.830
	R^2=0.0049				R^2=0.0040			

N=792,159

Note: controls included see Table 6.

5.2 Heterogeneity of impacts

Custody experience is not uniform across ethnicities and it might be that there is more of an impact of RPA for groups with a higher risk of custody. Table 8 replicates Table 3 broken down for males of white and black ethnicity. The lower left quadrant shows that there is a significant negative association between continued participation in education or training and experience of custody for both white and black males, with a much stronger association for black males: -0.0340 (black) compared to -0.0049 (white). As such, there is potential for a greater impact of RPA amongst black males. However, the top-right quadrant of the table shows that while there is a positive impact of RPA on participation for black males it is smaller (1.1pp increase) than is the case for white males (1.6pp), and less precisely estimated. The F-stat on the instrument for black males is only 4.0, indicating it is weak and will lead to an inconsistent second stage estimate. The reduced form estimates (top-left quadrant) indicate that RPA is associated with no change in the probability of custody for either black or white males once characteristics and the underlying time trend is taken into account. Consequently, for both ethnic groups the second stage IV estimates are insignificantly different to zero.

Table 8: Two-stage least squares and OLS estimates of the RPA effect on education/training participation and probability of custody, by ethnicity

	Reduced form			IV: first stage		
Dep. variable:	Ever in custody aged 17 or 18			Participating educ/training aged 17		
	coefficient	std. error	p	coefficient	std. error	p
RPA white males	-0.0001	0.0002	0.528	0.0185	0.0016	0.000
				F-stat on excluded IV:	133.9	
RPA black males	0.0013	0.002	0.511	0.0111	0.0056	0.046
				F-stat on excluded IV:	4.0	

	OLS: participating age 17			IV: second stage		
Dep. variable:	Ever in custody aged 17 or 18			Ever in custody aged 17 or 18		
	coefficient	std. error	p	coefficient	std. error	p
participating in educ/training aged 17						
white males	-0.0049	0.0003	0.000	-0.0073	0.0128	0.528
black males	-0.0340	0.0040	0.000	0.1160	0.0128	0.544

N = 816,089 white males; N = 34,630 black males

Note: controls included see Table 6.

5.3 Sensitivity to controls

The two-stage least squares estimates rather than RD are likely to be the more robust given the rarity of custody at age 17 or 18, which increases the potential that differences between cohorts in individual characteristics may otherwise bias the estimated effect of RPA. This conclusion is supported by examining how estimates of the reduced form effect of RPA on custody probability and the first stage effect on continued participation vary as control variables are successively added to the model. Table 9 starts by reporting in the first row, the raw effect of RPA on custody and participation when no other covariates are added to the regressions. Each successive row add further controls, starting with a linear control for year, then demographic variables, then socio-economic background, the variables capturing experience of the care system, then special educational needs, then prior attainment at ages 16 and 11, then school characteristics, and finally behaviour history (temporary and permanent exclusions) and absent record throughout secondary school. As can be seen in the column relating to the first stage, RPA is associated with a positive impact of 2.7pp when no other variables are included and the pre- and post-RPA cohorts are compared. Once the time trend is included the estimate falls to 0.6pp and remains consistently of this magnitude as further controls are added, in each case strongly statistically significant. It is only when behaviour and unauthorised absence history is added that the coefficient increases to 1.7pp. Comparison of the final two rows suggests that differences in behaviour and absence history between the cohorts initially indicate a lower impact of RPA but once these differences are accounted for, the RPA effect is actually more substantial.

Turning to the row corresponding to the reduced form effect of RPA on experience of custody at age 17 or 18, there is a significant negative effect of RPA on custody only in the regression with no controls – clearly this is picking up the downward time trend in custody over the cohorts in the data. Once a linear control for time is included, the RPA coefficient becomes positive and insignificant. The RPA effect remains insignificantly different to zero for all remaining regressions, with a small point estimate each time, some of which are positive, some negative. It is clear that after taking account of the time trend, the reduced form effect of RPA is very close to zero and imprecisely estimated regardless of the sets of controls included.

Table 9: Reduced form and IV first stage coefficients on RPA, sensitivity to controls, males

Reduced form: ever in custody 17/18			IV first stage: participating in e/t age 17				
RPA coeff	std. err.	p	RPA coeff	std. err.	p		Control variables included
-0.0015	0.0001	0.0000	0.0268	0.0007	0.0000		Zero
0.0005	0.0002	0.0662	0.0062	0.0015	0.0000	plus	Year
0.0004	0.0003	0.0870	0.0057	0.0014	0.0000	plus	demographic
0.0001	0.0003	0.7890	0.0068	0.0014	0.0000	plus	SES
0.0000	0.0003	0.9963	0.0069	0.0014	0.0000	plus	care
0.0000	0.0003	0.9791	0.0069	0.0014	0.0000	plus	SEN
-0.0002	0.0002	0.3825	0.0095	0.0013	0.0000	plus	Prior attainment KS4 and KS2
-0.0001	0.0002	0.6218	0.0084	0.0013	0.0000	plus	School characteristics
0.0001	0.0002	0.7593	0.0169	0.0014	0.0000	plus	Behaviour and unauthorised absences record

6 Discussion

The formal regression estimates of the RPA reported in section 5 are generally robust to alternative ways of implementing the regression discontinuity and two-stage least squares approaches. In all cases, estimated effects of RPA on education participation and likelihood of custody remain very small and for custody in particular, imprecisely estimated despite the large dataset. Therefore we can conclude that the RPA policy had little impact on continued participation in education and training at age 17 for young men and women, and for those who were impacted, the additional education did not affect their likelihood of experiencing custody at ages 17 or 18.

For both males and females, the size of the impact on participation is small, at around 1.5pp and 1.2pp respectively, though given that 81% and 84% respectively of the cohort are already participating at age 17, these increases represent just under 10% of the possible complier group. 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 these RPA impacts are closer to 12% to 15% of the target group having their participation changed as a result of the policy.

Despite these observed impacts on participation, both in any form of education or training, and participation in school specifically, the effect on probability of custody is consistently estimated to be not significantly different to zero, regardless of the estimation method and controls included in the model. This is the case for both males – where custody is a relatively more likely event – and females.

As continued participation was not enforced in the same way that compliance with the compulsory minimum school leaving age is enforced, the fact that it achieved this extent of additional, essentially voluntary, participation is perhaps more of an effect than we might have expected. However, the fact that the participation was effectively voluntary suggests that the compliers may be those within the target group who were already 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.

7 Conclusions and policy implications

The RPA in England had a small impact on continued participation in education or training overall and it appears to have moved some young men and women into school participation rather than other forms of education or training. This small impact is in part due to the small potential complier group for the policy. Other reasons for the lack of impact relate to the implementation of the policy, specifically the lack of monitoring of 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. Unlike the 1972 RoSLA which was prepared for over a 28-year period and fully supported with additional funding, teachers and buildings, the RPA was part of the 2008 Education and Training Act and implemented just four years later. Moreover, RPA was not supported by increased investment, Local Authorities were instead instructed to meet the costs of providing the additional education and monitoring its uptake from existing budgets. With this implementation it was perhaps always likely that any increases in participation would be small. Given that, and the strong existing downward trend in custody experience over successive cohorts, it is therefore not surprising that there was no detectable impact on the probability of custody.

We can draw a number of policy recommendations in light of these findings. First and foremost, policy should focus on ways in which to reduce post-16 attrition rates and early school leaving. This can be achieved only if local authorities are able to provide a coherent and consistent post-16 offer, supported by increased funding, in order to retain dis-engaged young people in education or training beyond the compulsory minimum. There also needs to be a mechanism to track people through the post-16 system – at present there is an ownership vacuum whereby there is no single body with responsibility for ensuring continued participation post-16. Without knowing what disengaged young people are doing it will not be possible to reach them with efforts to re-engage them. The tentative evidence here suggests that those most at risk of later teenage custody are less likely to engage in current education options post-16 voluntarily and will need to be subject to re-engagement policy initiatives.

In addition, sustained funding needs to be made available for such prevention and reintegration initiatives targeted at young people not in education, employment or training.

These recommendations focus on ways in which to enhance voluntary participation in education and training beyond the age of 16, in order to reduce the group who exit at this point and who are at increased risk of poor labour market outcomes and are the most likely to be at risk of custody in later teenage years.

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