Online Appendix: Coarsening bias: How coarse treatment measurement upwardly biases instrumental variable estimates

John Marshall*

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Contents

*PhD candidate, Department of Government, Harvard University. jlmarsh@fas.harvard.edu. I thank John Bullock, Anthony Fowler, Andy Hall, Torben Iversen, Rakeen Mabud, Horacio Larreguy, Brandon Stewart, Dustin Tingley and Tess Wise for illuminating discussions or useful comments. Replication materials are available online as Marshall (2016*b*).

1 Trends in the use of instrumental variables

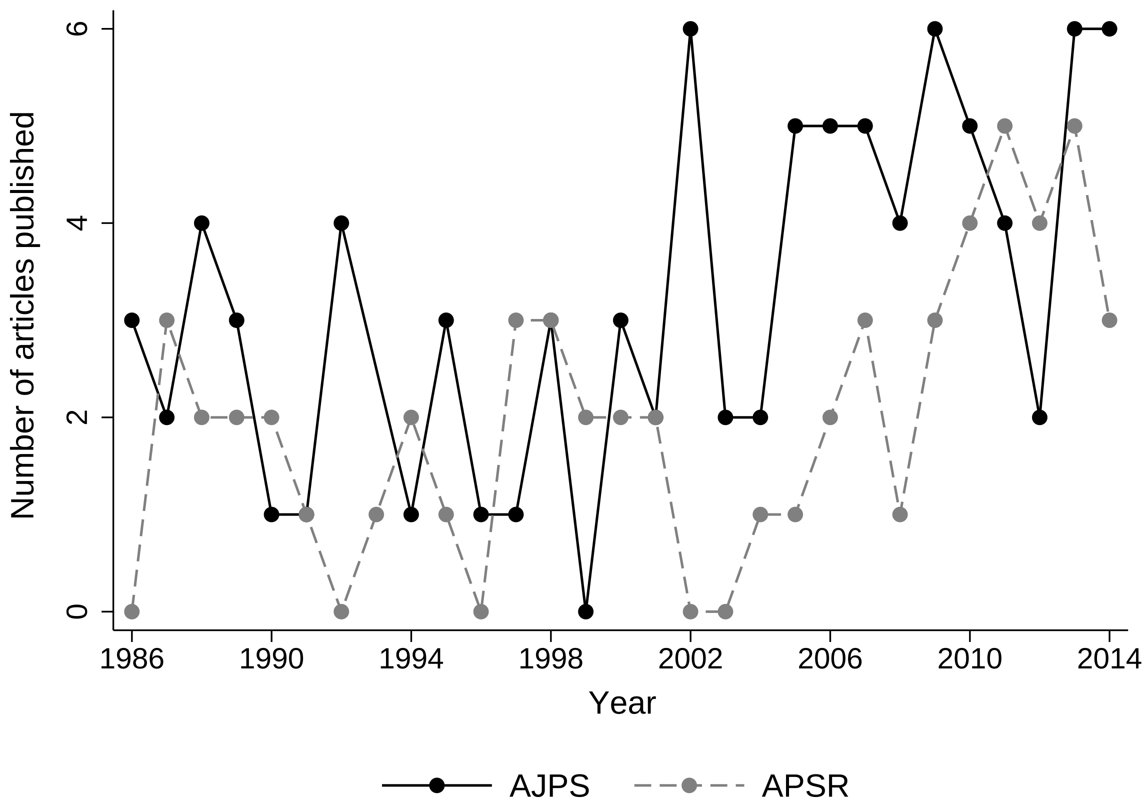


Figure A1: Annual trends in the use of instrument variable techniques in political science

Notes: Counts are based on data provided by Allison Carnegie (from Sovey and Green 2011) and the author's own reading of AJPS and APSR articles. Any reference to implementing an IV technique is included.

2 Proofs from main article

Proof of Proposition 1. First note that by the law of large numbers and Slutsky's theorem:

$$\underset{N\to\infty}{\text{plim}}\ \hat{\beta}_k^{IV} = \underset{N\to\infty}{\text{plim}} \left(\frac{\sum\limits_{\{i:Z_i=1\}} Y_i - \sum\limits_{\{i:Z_i=0\}} Y_i}{\sum\limits_{\{i:Z_i=1\}} D_{ik} - \sum\limits_{\{i:Z_i=0\}} D_{ik}} \right) = \frac{\mathbb{E}[Y_i|Z_i=1] - \mathbb{E}[Y_i|Z_i=0]}{\mathbb{E}[D_{ik}|Z_i=1] - \mathbb{E}[D_{ik}|Z_i=0]}.$$

By A1, the potential outcomes of Y_i and T_i can respectively be written as Y_{izt} and T_{iz}. Without loss of generality, let A4 hold such $T_{i1} - T_{i0} \geq 0$, and thus $p_t \geq 0$.[1] Following Angrist and Imbens (1995), the probability limit of the reduced form can be written as:

$$\begin{aligned}
\mathbb{E}[Y_i|Z_i = 1] - \mathbb{E}[Y_i|Z_i = 0] &= \mathbb{E}[Y_{i1T_{i1}} - Y_{i0T_{i0}}] \\
&= \mathbb{E}\left[\sum_{t=2}^{J} I(T_{i1} \geq t)[Y_{i1t} - Y_{i1t-1}] - \sum_{t=2}^{J} I(T_{i0} \geq t)[Y_{i0t} - Y_{i0t-1}]\right] \\
&= \mathbb{E}\left[\sum_{t=2}^{J} [I(T_{i1} \geq t) - I(T_{i0} \geq t)][Y_{it} - Y_{it-1}]\right] \\
&= \sum_{t=2}^{J} \Pr[T_{i1} \geq t > T_{i0}]\mathbb{E}[Y_{it} - Y_{it-1}|T_{i1} \geq t > T_{i0}] \\
&= \sum_{t=2}^{J} p_t \beta_t,
\end{aligned}$$

where the first line uses A2, the second decomposes the first over all treatment intensity levels, the third uses A5, and the fourth uses A4 (which implies that $T_{i1} \geq t > T_{i0}$ is either 0 or 1).

By definition, D_{ik} is purely a function of t. Using A2, the probability limit of the first stage for the coarsened treatment indicator D_{ik} is given by:

$$\begin{aligned}
\mathbb{E}[D_{ik}|Z_i = 1] - \mathbb{E}[D_{ik}|Z_i = 0] &= \mathbb{E}\left[\sum_{t=2}^{J} [I(T_{i1} \geq t) - I(T_{i0} \geq t)][D_{ik}(t) - D_{ik}(t-1)]\right] \\
&= \mathbb{E}\Big[I(T_{i1} \geq k) - I(T_{i0} \geq k)\Big] \\
&= \Pr(T_{i1} \geq k > T_{i0}) \\
&= p_k,
\end{aligned}$$

where the second line follows from the fact that $D_{ik}(k) - D_{ik}(k-1) = 1$ and $D_{ik}(t) - D_{ik}(t-1) = 0, \forall t \neq k$, while the third line follows from A4.

[1] All results hold for $T_{i0} - T_{i1} \geq 0$, where β_T and p_t are respectively redefined as $\beta_k \equiv \mathbb{E}[Y_{ik} - Y_{ik-1}|T_{i0} \geq k > T_{i1}]$ and $p_t \equiv \Pr(T_{i0} \geq t > T_{i1})$.

Combining the first stage and reduced form, the probability limit of the Wald IV estimator is:

$$\operatorname*{plim}_{N\to\infty} \hat{\beta}_k^{IV} = \frac{\sum_{t=2}^{J} p_t\beta_t}{p_k} = \beta_k + \frac{\sum_{t=2,t\neq k}^{J} p_t\beta_t}{p_k},$$

where the final term is the bias of the estimator (beyond finite sample bias). The condition $p_k \neq 0$ ensures that β_k^{IV} is well-defined.

It is immediate that the bias is positive whenever $\sum_{t=2,t\neq k}^{J} p_t\beta_t > 0$, given that A4 ensures $p_t \geq 0, \forall t$. $\beta_t > (<)0$, for all t such that $p_t \neq 0$ is a sufficient condition for positive (negative) bias. Consequently, $sign(\beta_k) = sign(\beta_t)$, for all t such that $p_t \neq 0$ implies $|\beta_k| \leq |\beta_k^{IV}|$. ■

Proof of Proposition 2. Consistency requires that $\sum_{t=2,t\neq k}^{J} p_t\beta_t = 0$. I now show that A5* is a sufficient condition. Both A5 and A5* entail that $[Y_{izt} - Y_{izt-1}] = [Y_{it} - Y_{it-1}]$. Furthermore, A5* entails that $[Y_{it} - Y_{it-1}] = 0$ for all $t \neq k$ where $p_t \neq 0$. Consequently,

$$\begin{aligned}
\mathbb{E}[Y_i|Z_i=1] - \mathbb{E}[Y_i|Z_i=0] &= \mathbb{E}\left[\sum_{t=2}^{J}[I(T_{i1}\geq t) - I(T_{i0}\geq t)][Y_{it} - Y_{it-1}]\right] \\
&= \mathbb{E}\left[\sum_{t=2,t\neq k}^{J}[I(T_{i1}\geq t) - I(T_{i0}\geq t)]\right][Y_{it} - Y_{it-1}] + \\
&\quad\ \mathbb{E}\Big[[I(T_{i1}\geq k) - I(T_{i0}\geq k)][Y_{ik} - Y_{ik-1}]\Big] \\
&= p_k\beta_k,
\end{aligned}$$

where the first line follows from A5 and the third line requires A5*. Under A5*, it is thus clear that the Wald estimator then yields:

$$\operatorname*{plim}_{N\to\infty} \hat{\beta}_k^{IV} = \frac{\mathbb{E}[Y_i|Z_i=1] - \mathbb{E}[Y_i|Z_i=0]}{\mathbb{E}[D_{ik}|Z_i=1] - \mathbb{E}[D_{ik}|Z_i=0]} = \frac{p_k\beta_t}{p_k} = \beta_k.$$

Therefore, $\hat{\beta}_k^{IV}$ is a consistent estimator under A1, A2, A3, A4 and A5* (which implies A5). ■

Proof of Proposition 3. Using the proof of Proposition 1, $\underset{N\to\infty}{\text{plim}}\ \hat{\beta}^{W,J}_{LAPTE} = \frac{\sum_{t=2}^{J} p_t \beta_t^J}{\sum_{t=2}^{J} p_t} = \tau$ and $\underset{N\to\infty}{\text{plim}}\ \hat{\beta}^{W,\alpha J}_{LAPTE} = \frac{\sum_{t=2}^{\alpha J} p_t \beta_t^{\alpha J}}{\sum_{t=2}^{\alpha J} p_t} = \tau/\alpha$, where the linearity of the causal effect at each intensity interval implies $\alpha\beta_t^{\alpha J} = \beta_t^J$. The result follows. ■

3 Illustrating the analytical results: simulated data

In addition to examining coarsening bias in a political application with observational data, as I do in the main paper, it is useful to also illustrate and validate the analytical results using purely simulated data that can control the true empirical relationships. The simulation results reinforce the analytical results showing that the size of the effect at each treatment intensity on the outcome (i.e. the shape of the CRF) and the strength of the first stage at intensity k relative to the first stage at all other intensities ($\sum_{t\neq k} p_t / p_k$) are critical in determining the extent of coarsening bias.

3.1 Monte Carlo simulations

The analytical insights are captured in a simple simulation framework. For each simulated dataset, I draw a random sample of 1,000 independent observations. Using complete randomization, half the observations are randomly assigned to treatment and control. The observation's endogenous treatment intensity T_i is given by the nearest round number to $Z_i + \xi_i$, where $\xi_i \sim Normal(10, \sigma^2)$. The treatment thus takes multiple integer intensities, where the mean for observations that did not receive the instrument is 10 and the mean for observations that did is 11. Absracting from noise, in terms of potential outcomes, $T_i = (1 - Z_i)T_{i0} + Z_i T_{i1} = (1 - Z_i) * 10 + Z_i * (10 + 1)$. Importantly, by increasing the variance parameter σ^2, the instrument can be allowed to exert a relatively larger effect on reaching treatment intensities other than 11. Finally, the treatment is coarsened as an indicator for receiving an intensity of at least 11: $D_i \equiv 1(T_i \geq 11)$.

I consider two types of causal relationship. I first examine a *linear CRF* where each additional intensity level increases the outcome by 0.05 units:

$$Y_i = 0.05 * \sum_{t=1}^{T} 1(T_i \geq t) + \eta_i \tag{1}$$

where $\eta_i \sim Normal(0.4, 0.2)$ determines the location of the outcome and its variation. This is held constant across CRFs. Abstracting from the noise, potential outcomes are thus $Y_{it} = Y_{i0} + 0.05t = 0.4 + 0.05t$. I also consider a *single jump CRF* where only the eleventh intensity level increases the outcome by 0.05 units:

$$Y_i = 0.05 * 1(T_i \geq 11) + \eta_i. \tag{2}$$

Abstracting from the noise, potential outcomes are thus $Y_{it} = Y_{i0} + 0.05t = 0.4 + 0.05I(t \geq 11)$. In both cases, the standard IV assumptions (A1-A5) hold. However, the strong exclusion restriction (A5*) only holds for the single jump CRF.

To illustrate the bias associated with coarsening, I vary σ^2 to examine how two stage least squares (2SLS) estimates for the two different CRFs depend upon the relative first stage at intensities other than k, or $\sum_{t \neq k} p_t / p_k$, which increases in σ^2. For each σ^2, I examine 1,000 simulated datasets and present the coarsened IV estimates (using the indicator D_i for reaching the eleventh intensity) and the LAPTE (treating T_i as linear).[2]

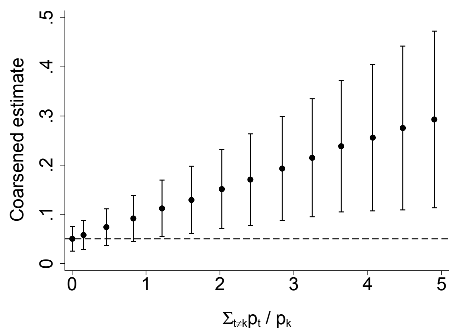
I first analyze the linear CRF. As demonstrated in Proposition 1, Figure 2(a) shows that the bias associated with coarsening the treatment can be substantial and is increasing linearly in $\sum_{t \neq k} p_t / p_k$. When only the coarsened intensity is affected by the instrument (i.e. $p_k \neq 0$ and $p_t = 0, \forall t \neq k$), or as $\sum_{t \neq k} p_t / p_k \downarrow 0$, the IV estimate is essentially unbiased. However, bias increases in the relative magnitude of the effect of the instrument on other intensities. Once only 50% (25%) of the instrument's effect occurs at intensity k, the IV estimate is fully two (four) times larger than the true causal effect. This bias reflects that fact that the instrument is inducing many changes

[2]Specifically, I consider 14 values of σ^2, increasing from 0.17 to 2.33 in intervals of 0.17.

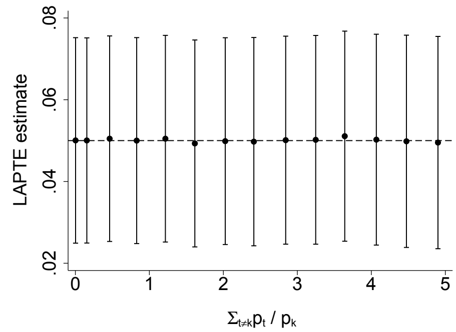
in the outcome through intensities that are not captured by the coarsened first stage. Conversely, Figure 2(b) shows that the LAPTE estimate correctly identifies the true causal effect regardless of which intensities are affected by the instrument, and does not lose precision because each intensity is equally relevant. This is not surprising given that the LAPTE is designed precisely for this case.

Second, consider the single jump CRF, where the researcher correctly identifies the sole effect at the eleventh intensity. Reinforcing Proposition 2, Figure 2(c) confirms that coarsening the treatment unsurprisingly produces an unbiased estimate of the effect at the eleventh intensity in this rare instance where the strong exclusion restriction holds. The precision of this estimate is decreasing in the relative first stage at other (uninformative) intensities. Turning to the LAPTE estimate, Figure 2(d) demonstrates that—as shown analytically in the main paper—the LAPTE only returns the causal effect at the eleventh intensity when the instrument only affects this intensity. The size of the LAPTE declines with $\sum_{t\neq k} p_t / p_k$, as the relative weight attached to compliers with zero effects increases. However, it is important to reiterate that the LAPTE remains a consistent causal estimate—it just weights the effects at all the intensities affected by the instrument. Furthermore, the comparison of Figures 2(c) and 2(d) shows the general results that the LAPTE will always be smaller than the coarsened IV estimate. When the CRF is neither linear nor discontinuous at a single threshold, the LAPTE thus provides both consistent and conservative estimates.

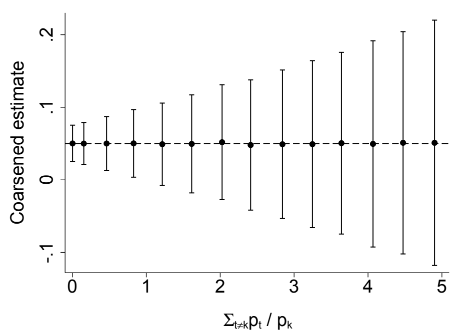
Finally, consider the same single jump CRF, but where the researcher incorrectly coarsens the treatment at intensity 12. Figure 2(e) shows that coarsening often incorrectly ascribes a significant positive effect to the twelfth intensity where no such effect exists. This occurs where, in addition to inducing subjects to receive intensity 12 (where there is no effect), the instrument also induces subjects to receive the eleventh treatment intensity where there exists a positive effect. As the figure shows, this is not simply an issue of interpretation because the points systematically fail to even recover the true effect of the eleventh intensity.



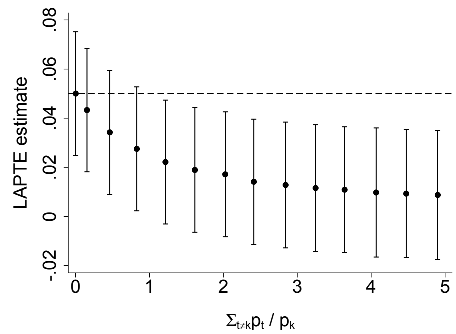
(a) Linear CRF, coarsened IV estimate



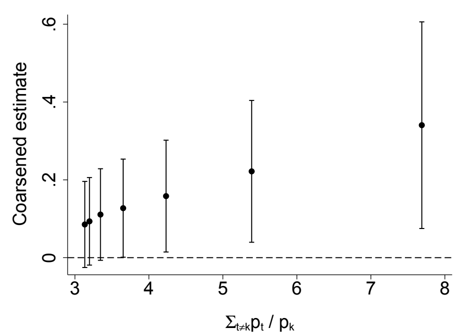
(b) Linear CRF, LAPTE estimate



(c) Single jump CRF, correctly coarsened IV estimate



(d) Single jump CRF, LAPTE estimate



(e) Single jump CRF, incorrectly coarsened IV estimate

Figure A2: Monte Carlo simulations illustrating how IV estimates of a 0.05 causal effect depend upon the CRF, coarsening, and the relative strength of the instrument on different treatment intensities (95% confidence intervals)

Notes: Estimates at each level of $\sum_{t\neq 11} p_t\,/\,p_k$ are based on 1,000 simulated datasets containing 1,000 observations. For the linear CRF, the effect of each additional intensity is 0.05. For the Single jump CRF, the effect is 0.05 only at intensity $k = 11$. When correctly coarsened, the coarsened treatment is defined as $D_i = 1(T_i \geq k)$. When incorrectly coarsened, the coarsened treatment is defined as $D_i' = 1(T_i \geq k+1)$. See the main text for the underlying equations and distributions. Point estimates are the average estimate across simulations; the horizontal dashed line denotes the true causal effect the researcher seeks to estimate. 95% confidence intervals are based on variation within and between simulated estimates.

3.2 Monte Carlo simulation code

The R code generating the simulation results above is provided below:

```
# Load SEM package for IV estimation
library(sem)

# Set number of simulated datasets
n <- 1000

# Set total number of variance parameters to examine
largest <- 14

######## Linear CRF

# Generating matrices to hold the results
FS <- FSK <- RF <- IV1 <- IV2 <- matrix(nrow=largest,ncol=4)
poverp <- matrix(nrow=largest,ncol=2)

# Set seed
set.seed(12345)

# Loop over the variance parameter sigma^2 (=j/6)
for (j in 1:largest){
# Set variance
par <- j/6

# Create temporary holding matrices
fs <- fsk <- ols <- iv1 <- iv2 <- matrix(nrow=n,ncol=2)

# First stage (linear), first stage (coarsened), reduced form, and IV regressions  for each of the
# 1,000 datasets
for (i in 1:n){
z <- sample(rep(c(0,1),n/2))
x <- round(z + rnorm(n,10,par))
y <- 0.05*as.numeric(x>=1) + 0.05*as.numeric(x>=2) + 0.05*as.numeric(x>=3) +
  0.05*as.numeric(x>=4) + 0.05*as.numeric(x>=5) + 0.05*as.numeric(x>=6) + 0.05*as.numeric(x>=7) +
  0.05*as.numeric(x>=8) + 0.05*as.numeric(x>=9) + 0.05*as.numeric(x>=10) + 0.05*as.numeric(x>=11) +
  0.05*as.numeric(x>=12) + 0.05*as.numeric(x>=13) + 0.05*as.numeric(x>=14) + 0.05*as.numeric(x>=15) +
  0.05*as.numeric(x>=16) + 0.05*as.numeric(x>=17) + 0.05*as.numeric(x>=18) + rnorm(n,0.4,.2)
```

```
d <- as.numeric(x>=11)

fs[i,] <- summary(lm(x ~ z))$coefficients[2,1:2]
fsk[i,] <- summary(lm(d ~ z))$coefficients[2,1:2]
ols[i,] <- summary(lm(y ~ z))$coefficients[2,1:2]
iv1[i,] <- summary(tsls(y ~ d, ~ z))$coefficients[2,1:2]
iv2[i,] <- summary(tsls(y ~ x, ~ z))$coefficients[2,1:2]
}

# First stage ratio shown on the x axis
poverp <- ( mean(fs[,1]) - mean(fsk[,1]) ) / mean(fsk[,1])

FS[j,] <- cbind( par, poverp, mean(fs[,1]), mean(fs[,2])+(1+1/n)*var(fs[,1]) )
FSK[j,] <- cbind( par, poverp, mean(fsk[,1]), mean(fsk[,2])+(1+1/n)*var(fsk[,1]) )
RF[j,] <- cbind( par, poverp, mean(ols[,1]), mean(ols[,2])+(1+1/n)*var(ols[,1]) )
IV1[j,] <- cbind( par, poverp, mean(iv1[,1]), mean(iv1[,2])+(1+1/n)*var(iv1[,1]) )
IV2[j,] <- cbind( par, poverp, mean(iv2[,1]), mean(iv2[,2])+(1+1/n)*var(iv2[,1]) )
}

FS; FSK; RF; IV1; IV2

######## DISCONTINUOUS CRF

FS <- FSK <- RF <- IV1 <- IV2 <- matrix(nrow=largest,ncol=4)

set.seed(12345)

for (j in 1:largest){
par <- j/6

fs <- fsk <- ols <- iv1 <- iv2 <- matrix(nrow=n,ncol=2)

for (i in 1:n){
z <- sample(rep(c(0,1),n/2))
x <- round(z + rnorm(n,10,par))
y <- 0.05*as.numeric(x>=11) + rnorm(n,0.4,.2)
d <- as.numeric(x>=11)

fs[i,] <- summary(lm(x ~ z))$coefficients[2,1:2]
```

```
fsk[i,] <- summary(lm(d ~ z))$coefficients[2,1:2]
ols[i,] <- summary(lm(y ~ z))$coefficients[2,1:2]
iv1[i,] <- summary(tsls(y ~ d, ~ z))$coefficients[2,1:2]
iv2[i,] <- summary(tsls(y ~ x, ~ z))$coefficients[2,1:2]
}

poverp <- ( mean(fs[,1]) - mean(fsk[,1]) )/mean(fsk[,1])

FS[j,] <- cbind( par, poverp, mean(fs[,1]), mean(fs[,2])+(1+1/n)*var(fs[,1]) )
FSK[j,] <- cbind( par, poverp, mean(fsk[,1]), mean(fsk[,2])+(1+1/n)*var(fsk[,1]) )
RF[j,] <- cbind( par, poverp, mean(ols[,1]), mean(ols[,2])+(1+1/n)*var(ols[,1]) )
IV1[j,] <- cbind( par, poverp, mean(iv1[,1]), mean(iv1[,2])+(1+1/n)*var(iv1[,1]) )
IV2[j,] <- cbind( par, poverp, mean(iv2[,1]), mean(iv2[,2])+(1+1/n)*var(iv2[,1]) )
}

FS; FSK; RF; IV1; IV2

######## INCORRECTLY IDENTIFIED DISCONTINUOUS CRF

FS <- FSK <- RF <- IV1 <- IV2 <- matrix(nrow=largest,ncol=4)

set.seed(12345)

for (j in 3:20){
par <- j/12

fs <- fsk <- ols <- iv1 <- iv2 <- matrix(nrow=n,ncol=2)

for (i in 1:n){
z <- sample(rep(c(0,1),n/2))
x <- round(z + rnorm(n,10,par))
y <- 0.05*as.numeric(x>=11) + rnorm(n,0.4,.2)
d <- as.numeric(x>=12)

fs[i,] <- summary(lm(x ~ z))$coefficients[2,1:2]
fsk[i,] <- summary(lm(d ~ z))$coefficients[2,1:2]
ols[i,] <- summary(lm(y ~ z))$coefficients[2,1:2]
iv1[i,] <- summary(tsls(y ~ d, ~ z))$coefficients[2,1:2]
iv2[i,] <- summary(tsls(y ~ x, ~ z))$coefficients[2,1:2]
```

```
}

poverp <- ( mean(fs[,1]) - mean(fsk[,1]) )/mean(fsk[,1])

FS[j,] <- cbind( par, poverp, mean(fs[,1]), mean(fs[,2])+(1+1/n)*var(fs[,1]) )
FSK[j,] <- cbind( par, poverp, mean(fsk[,1]), mean(fsk[,2])+(1+1/n)*var(fsk[,1]) )
RF[j,] <- cbind( par, poverp, mean(ols[,1]), mean(ols[,2])+(1+1/n)*var(ols[,1]) )
IV1[j,] <- cbind( par, poverp, mean(iv1[,1]), mean(iv1[,2])+(1+1/n)*var(iv1[,1]) )
IV2[j,] <- cbind( par, poverp, mean(iv2[,1]), mean(iv2[,2])+(1+1/n)*var(iv2[,1]) )
}

FS; FSK; RF; IV1; IV2
```

4 BES survey data

All variables are from the British Election Survey (BES). The BES uses a multi-stage design, randomly selecting postal addresses from several wards from randomly sampled constituencies (stratifying by region). The BES has been conducted following every general election since 1964, although I only use the surveys since 1979 where appropriate variables are available. As noted in the main paper, the sample is restricted to working age respondents (i.e. aged 70 or below), and those aged 18 at the time of the survey. The sample is restricted to those aged below 70 given the likelihood that education affects political behavior through earned income (see Marshall 2016*a*). Summary statistics for the RD and full samples are provided in Table A1.

- *Vote Conservative*. Indicator coded one for respondents who reported voting for the Conservative party at the last general election. Only respondents which refused to respond, did not answer or did not vote were excluded.

- *Years of schooling*. Years of schooling is calculated as the age that the respondent left full time education minus five (the age at which students start formal schooling). Years of schooling is top-coded at 13 years to ensure comparability and focus on state-provided education.

Table A1: Summary statistics: RD and full BES samples

	RD sample (1947 reform)					**Full BES sample**				
	Obs.	Mean	Std. dev.	Min.	Max.	Obs.	Mean	Std. dev.	Min.	Max.
Dependent variable										
Vote Conservative	4,820	0.409	0.492	0	1	13,853	0.343	0.475	0	1
Endogenous treatment variables										
Years of schooling	4,820	10.317	1.417	0	13	13,853	10.982	1.436	0	13
Completed high school	4,820	0.400	0.490	0	1	13,853	0.642	0.480	0	1
Excluded instruments										
Post 1947 reform	4,820	0.589	0.492	0	1	13,853	0.493	0.500	0	1
Post 1972 reform	4,820	0.000	0.000	0	0	13,853	0.314	0.464	0	1
Pre-treatment covariates										
Male	4,820	0.474	0.499	0	1	13,853	0.466	0.499	0	1
White	4,820	0.984	0.126	0	1	13,853	0.965	0.183	0	1
Black	4,820	0.006	0.079	0	1	13,853	0.009	0.094	0	1
Asian	4,820	0.007	0.084	0	1	13,853	0.016	0.127	0	1
Age	4,820	54.732	8.323	35	69	13,853	43.365	14.073	18	69
Father manual/unskilled job	3,820	0.724	0.447	0	1	9,922	0.672	0.470	0	1
Survey	4,820	1988.912	7.342	1979	2010	13,853	1991.697	8.926	1979	2010
Birth year	4,820	1934.180	6.465	1922	1944	13,853	1948.332	16.153	1910	1992

Indicators for 10 and 11 years of schooling are defined according to this measure.

- *Completed high school*. Indicator coded one for respondents that answered that either: (1) possess a grade 1 Certificate of Secondary Education (CSE), 5 O-levels at A-C, 5 General Certificates of Secondary Education (GCSEs) at A*-C or a lower grade on the Scottish Certification of Education (SCE); or (2) left school at age 16 or later.

- *Birth year*. Birth-year is estimated by subtracting age at the date of the survey from the year in which the survey was conducted. I then add 14 for year aged 14. Non-responses were deleted.

- *Post 1947 reform*. Indicator coded one for students aged 14 or below in 1947, and aged 15 or above in 1972.

- *Post 1972 reform*. Indicator coded one for students aged 14 or below in 1972.

- *Male*. Indicator coded one for respondents identifying as male. Non-responses were deleted.

- *Age*. Standardized age at the date of the survey.

- *Race*. Indicators coded one for respondents who respectively identify their ethnicity as white, black, or Asian (including South Asian ethnicities and Chinese).

- *Father manual/unskilled job*. Indicator coded one for respondent's who answered that their father had a manual or unskilled job.

- *Survey year*. Year in which the survey was conducted.

5 Using the 1972 school leaving reform to identify coarsening bias

As noted in the main text, the availability of two instruments offers the opportunity to precisely estimate the extent of coarsening bias. In particular, the availability of two instruments means that it is possible to instrument for two treatment variables—in this case completing the penultimate year of high school (i.e. $Penultimate_{ic} = 1(Schooling_{ic} = 10)$) and completing at least the final year of high school (as defined in the main paper). Since Figure 5 in the main paper and Figure A3 respectively show that the 1947 and 1972 reforms did not affect students leaving at older or younger ages, the reforms only affected whether students remained in school for the penultimate or final year of high school. Consequently, the first stage at all other levels of the treatment is zero (i.e. $p_t = 0, \forall t \neq 10, 11$). Therefore, instrumenting for two indicator variables—completing the penultimate and final years of high school—does not suffer from coarsening bias because the special case that $p_t = 0$ at all other t is satisfied. This enables me to estimate β_t for both additional years of high school, and thus to exactly identify the effect of completing high school beyond completing the penultimate year of high school. This latter quantity can be then be compared to the estimate for completing high school in the main paper to adduce the extent of coarsening bias.

To simultaneously estimate the effects of the penultimate and final year of high school, I use 2SLS to estimate the following structural equation:

$$Vote\ Conservative_{ic} = \beta_1 Penultimate_{ic} + \beta_2 Completed\ high\ school_{ic} + f(Birth\ year_c) + \varepsilon_{ic}, \quad (3)$$

where the first stage regressions generating exogenous variation in educational attainment are given

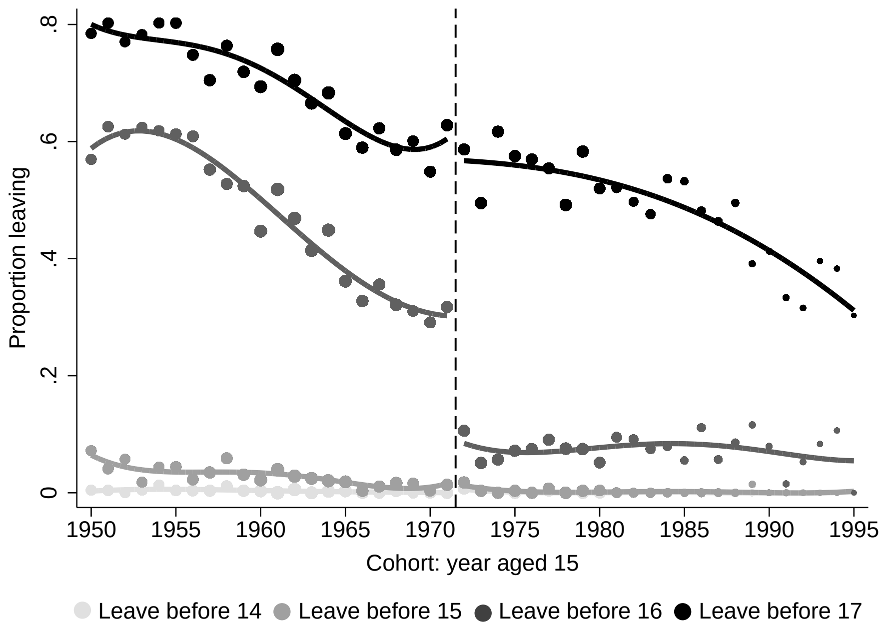


Figure A3: 1972 compulsory schooling reform and student leaving age by cohort

Notes: Data from the BES. Curves represent fourth-order polynomial fits. Grey dots are birth-year cohort averages, and their size reflects their weight in the sample.

by:

$$\begin{aligned} Penultimate_{ic} &= \alpha_1 Post\ 1947\ reform_c + \alpha_2 Post\ 1972\ reform_c + f(Birth\ year_c) + \varepsilon_{ic} \quad (4) \\ Completed\ high\ school_{ic} &= \gamma_1 Post\ 1947\ reform_c + \gamma_2 Post\ 1972\ reform_c + f(Birth\ year_c) + \varepsilon_{ic}. \quad (5) \end{aligned}$$

Because I now use two discontinuities as instruments, this system cannot be estimated using local linear regression, as in the main paper. However, I adopt a similar approach by using the full BES sample and letting f include cubic global polyomials in the running variable (birth year). These flexible polynomial terms are designed to capture general trends in Conservative support across cohorts around both reforms, and thus mimic the local linear approach in the main paper.[3]

[3]Encouragingly, column (1) of Table A2 shows that the cubic polynomials report a similar estimate for the 1947 reform as the local linear regression approach in the main paper. I also found similar results when using fourth, fifth, sixth and seventh-order polynomials.

Table A2: Using the 1972 reform to identify the extent of coarsening bias

	Vote Con. OLS (1)	Vote Con. 2SLS (2)	Vote Con. 2SLS (3)
Post 1947 reform	0.059***		
	(0.021)		
Post 1972 reform	0.079***		
	(0.030)		
Years of schooling		0.148***	
		(0.058)	
Penultimate year of high school			0.098**
			(0.039)
Completed high school			0.269**
			(0.124)
Observations	13,853	13,853	13,853
First stage F statistic		28.5	658.4/52.1

Notes: All specifications include standardized cubic polynomials in birth year. Robust standard errors in parentheses (which are always larger than errors clustered by cohort). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A2 reports the reduced form and IV estimates when exploiting both reforms simultaneously. Column (1) presents the reduced form estimates, and demonstrates that both reforms significantly increase the probability that an individual votes Conservative. Consistent with the 1947 reform having a larger effect on education, the 1947 reform also has a larger effect on Conservative voting: the 1947 increased Conservative voting per cohort by 6 percentage points, while the 1972 reform added a further two percentage points. Column (2) estimates the LAPTE for an additional year of schooling by using the 1947 and 1972 reforms as instruments for years of schooling. Similarly to the main paper, which reports a 11.3 percentage point increase, the estimates suggest that an additional year increases the probability of voting Conservative later in life by 14.8 percentage points. Furthermore, a Sargan overidentification χ_1^2 test fails to reject the null hypothesis that the instruments produce different IV estimates ($p = 0.70$). This provides evidence that both reforms had similar effects on voters, and again suggests that an additional year of compulsory education substantially increases the probability of voting Conservative. The similarity of the effects means that despite using two different instruments suggests that I am able to use both instruments to separate out the effects of the penultimate and final years of schooling.

Finally, I turn to the key element of this exercise: determining the extent of coarsening bias. Column (3) estimates equation (3), and reports the estimates for completing the penultimate and final year of high school. The results indicate that the penultimate year of high school increases Conservative voting by 10 percentage points, while completing high school adds a further 17 percentage points. This implies that the effect of completing high school, over completing a lower level of schooling, is to increase the probability of voting Conservative by 17 percentage points. Therefore, this demonstrates that coarsening bias almost triples the size of this estimate (46 percentage points in the main paper). Although the coefficients come from different samples, plugging the estimates for β_t and p_t into equation (4) in the main paper yields an IV estimate of around 0.5. This is similar to the 0.46 estimate in the main paper.

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