Jump or Kink? Regression Probability Jump and Kink Design for Treatment Effect Evaluation

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Abstract

This paper evaluates the impacts of participation in two social programs: elite school attendance in the United Kingdom and compulsory military service in Russia. A common feature of the two programs is that each entails a binary treatment, and the treatment assignment rule exhibits a dramatic kink with little or no jump. As a result, the standard RD design does not work well empirically. This paper extends the standard RD design with a binary treatment to allow for causal identification under more general conditions. I show that without a jump, one can still identify a causal effect utilizing a slope change (a kink) in the treatment probability. This kink based identification requires only minimal further smoothness relative to the standard RD design. The required smoothness is readily satisfied given a weak and testable behavioral assumption. I then propose a new Regression Probability Jump and Kink (RPJK) design that is valid regardless of whether there is a jump, or a kink, or both in the treatment probability. In sharp contrast to the results of the standard RD design, the proposed identification approaches yield plausible estimates of causal impacts in the two social programs under consideration.

JEL Codes: C21, C25, I90, J100

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1 Introduction

Many public policies or social programs set explicit eligibility thresholds for treatment. It may then be possible to evaluate program effects locally near the policy threshold. The standard regression discontinuity (RD) design relies on a discrete change (i.e., a jump) in the treatment probability to identify a causal effect of treatment. Identification fails when there is no discontinuity, or is weak when the discontinuity is small. See discussions in Imbens and Lemieux (2008), chapter 6 of Angrist and Pischke (2008), Imbens and Wooldridge (2009), and Lee and Lemieux (2010).

This paper evaluates the impacts of participation in two social programs: elite school attendance in the United Kingdom and compulsory military service in Russia. Despite the very different institutions, a common feature of the two programs is that each entails a binary treatment, and the treatment assignment rule exhibits a dramatic kink with little or no jump. As a result, the standard RD design does not work well empirically.

To accommodate empirical scenarios similar to the above, this paper extends the standard RD design with a binary treatment to allow for causal identification under more general conditions. In particular, I discuss identification using a kink instead of, or in addition to, a jump in the treatment probability. I show that identification using a kink in the treatment probability is valid under minimal further smoothness conditions than those required by the standard RD design. While the standard RD design identifies a local average treatment effect (LATE), a kink in the treatment probability generally identifies a marginal treatment effect (MTE), which can be interpreted as a limiting form of LATE (see Heckman and Vytlacil, 2005, 2007, and Carneiro, Heckman, and Vytlacil, 2010).

Note that this paper considers a binary treatment. In contrast, the recent regression kink (RK) design (Card, Lee, Pei, and Weber, 2015) considers a continuous treatment.[1] I therefore refer to the new kink design proposed here as the regression probability kink (RPK) design.

Just as a jump in the standard RD design is generated by the policy rule, kinks in treatment probabilities should likewise be dictated by policy rules. For example, in Clark and Del Bono (2016), the elite school assignment rule is based on an assignment score. Below a certain low threshold score, one is ineligible for the treatment, i.e., of attending an elite school, so the probability of treatment is nearly zero. Once above a certain high threshold, one is eligible (with some exceptions), so the probability of

[1] Their *sharp RK* design differs from the RPK design and leads to a different estimand. Their *fuzzy RK* design leads to an estimand that is comparable to the RPK estimand; However, the design setup, the interpretation of the identified parameter, and the identifying assumptions are entirely different.

treatment is almost one. Students in between the two thresholds are ordered based on merit, and so the further one's score is above the low threshold, the greater is the probability of treatment. As a result of this assignment rule, at each of these two thresholds there is a large kink, and little or no jump, in the probability of attending an elite school.

As another example, demilitarization in Russia leads to a kinked profile in the induction rate across birth cohorts after the cold war (Card and Yakovlev 2014). Such a steady rather than sudden change in the share of men drafted into the military can be justified on the grounds of reducing adjustment costs. Analogous to the standard RD design, identification based on kinks also requires that individuals cannot precisely manipulate the running variable (e.g., the assignment score or birth date), so random chance determines whether one falls just above or just below the kink point.

Let T be a binary treatment indicator, such as attending an elite school, or serving in the army. Let R be the so-called running or forcing variable, such as a test score or birth date. Treatment is determined at least in part by whether the the running variable crosses a known threshold value r_0. Let $Z = I\{R \geq r_0\}$, where $I\{\cdot\}$ is an indicator function equal to 1 if the expression in the bracket is true and 0 otherwise. Assume $T = 1\{P(R) - U \geq 0\}$, where U is normalized to follow a uniform distribution over the unit interval. Without loss of generality, one can rewrite $T = 1\{P_1(R) Z + P_0(R)(1 - Z) - U \geq 0\} = 1\{P_1(R) - U \geq 0\} Z + 1\{P_0(R) - U \geq 0\}(1 - Z)$, where the function $1\{P_z(R) - U \geq 0\}, z = 0, 1$ describes the assignment of treatment below or above the threshold. For example, the elite school assignment rule near the low eligibility threshold r_0 features $P_0(R) = 0$ and locally linear probability $P_1(R) = a + b(R - r_0)$ for some coefficients a and b.

More generally, a kink in a choice probability can arise from a kinked cost or benefit schedule for the choice. Assume that $P(R)$ is a (normalized) cost schedule, while U represents the (normalized) willingness-to-pay for the treatment. A kinked cost schedule implies $dP_0(r)/dr|_{r_0} \neq dP_1(r)/dr|_{r_0}$, which is a kink in the treatment probability. For example, Simonsen, Skipper and Skipper (2016) show that due to a kinked reimbursement schedule for prescription drugs, the probability of purchasing prescription drugs is a kinked function of total medical spending. In addition, Nielsen, Sørensen, and Taber (2010) note that college financial aid is a kinked function of parental income, and consequently, there is a kinked relationship between the probability of college enrollment and parental income.[2]

Frequently, the treatment probability may show both a jump and a kink at some policy threshold.

[2]In fact, RK designs are often applied in situations where the outcome is binary, even though they generally require continuous treatment. An RK design with a binary outcome (as exemplified by the above two studies) implies a kink in the probability of that outcome.

For example, serving in the Russian army during the demilitarization after the Cold War features this. I therefore discuss a general research design, the regression probability jump and kink (RPJK) design. The RPJK design is valid regardless whether there is a jump, a kink, or both in the treatment probability. It incorporates the standard RD design and the RPK design as special cases. This general RPJK design is especially useful when an empirically observed jump is small or statistically insignificant. I propose a simple local two stage least squares (2SLS) estimator for the RPJK design, and show that the corresponding estimand is asymptotically equivalent to the standard RD estimand when there is a real jump in the treatment probability, and otherwise reduces to the RPK estimand.

These new identification results make it possible to evaluate causal effects of the two programs under consideration. In both cases, any jump that might be present is too small for the standard RD estimators to work.[3] In contrast, the proposed RPK and RPJK designs yield plausible estimates. For example, in the evaluation of elite school attendance, I confirm previous estimates resulted from a parametric constant effect model, and in the evaluation of compulsory military service, I estimate significant negative impacts that are consistent with economic theory when education is interrupted and entry into the labor force is delayed. These negative effects, induced by the particular institutional setting in Russia, are in sharp contrast to the prevailing evidence from the US and other OECD countries.

Other related topics, such as identifying complier (or marginal complier) characteristics and checking for monotonicity, in addition to the usual tests for smoothness conditions, are briefly discussed.

The rest of the paper is organized as follows. Section 2 discusses the RPK design. Section 3 discusses the more general RPJK design that allows for either a jump, a kink, or both in the treatment probability. Section 4 relates the identification results to instrumental variables estimation and provides a local 2SLS estimator that is valid regardless of whether there is a jump, a kink, or both in the treatment probability. Section 5 discusses identification of other parameters of interest and provides checks for the underlying model assumptions. Sections 6 and 7 evaluate the UK elite school and Russian demilitarization programs by the proposed approaches. Section 8 concludes.

[3] It is possible to apply the results of Feir, Lemieux and Marmer (2016) to deal with weak identification in standard RD designs; however, this practice is by ignoring another possible sources of identification, slope changes.

2 Regression Probability Kink (RPK) Design

This section first presents the RPK design by the MTE framework, and then discusses equivalent results using the Angrist, Imbens, and Rubin (AIR) framework (Imbens and Angrist 1994, Angrist, Imbens, and Rubin 1996).

2.1 RPK Identification [4]

Let Y denote the outcome of interest. Assume $Y = g(R, T, W)$, where the unobservable W is allowed to be multidimensional. The binary treatment indicator T is endogenous, since it can be correlated with the running variable R and the unobservables W.[5] For notational convenience, define $Y_t \equiv g(R, t, W)$ for $t = 0, 1$. Y_1 and Y_0 are an individual's potential outcomes from being treated or not, respectively (Neyman, 1923, Rubin, 1974, Gronau, 1974, Heckman 1974, Quandt, 1972). The observed outcome of interest can then be written as $Y = Y_1 T + Y_0 (1 - T)$.

Let $F_{\cdot|\cdot}(\cdot|\cdot)$ denote a conditional cumulative density function, and $f_{\cdot}(\cdot)$ denote an unconditional probability density function. The following assumption holds for $r \in (r_0 - \varepsilon, r_0 + \varepsilon)$ for some small $\varepsilon > 0$.

Assumption:

A1 *(Selection Model): Assume* $T = 1\{P(R) - U \geq 0\}$, *where* $U \sim Unif(0, 1)$.

A2 *(Smoothness):* $P(r)$ *is continuously differentiable everywhere in* $r \in (r_0 - \varepsilon, r_0 + \varepsilon)$ *except at* $r = r_0$. $E[Y_t|U = u, R = r]$, $t = 0, 1$, *is continuously differentiable.* $E[|Y_t||U = u, R = r] < \infty$. *The density of the running variable* $f_R(r)$ *is continuous and strictly positive at* $r = r_0$.

A1 assumes that U follows a uniform distribution, which is simply a free normalization (Vytlacil, 2002). More generally if $T = 1\{h(R) - V \geq 0\}$, where the conditional distribution of V given R, $F_{V|R}(V|R)$, is strictly increasing, then one can let $U \equiv F_{V|R}(V|R) \sim Unif(0, 1)$ and $P(R) \equiv F_{V|R}(h(R)|R)$. Vytlacil (2002) shows that Assumption A1 is equivalent to the monotonicity assumption of the LATE model (Imben and Angrist, 1994, Angrist, Imbens, and Rubin, 1996). In Section 5, I briefly discuss how one can check for monotonicity in empirical applications.

A2 is a smoothness parallel of the independence assumption required to identify LATE or MTE. For example, Vytlacil (2002, Assumption S-1, (ii)) assumes a binary instrumental variable $Z \perp (U, Y_0, Y_1)$

[4] This section benefited from constructive suggestions from one of the anonymous referees.
[5] All of the functions defined here could also depend on additional covariates X, which are omitted for clarity.

to identify LATE. Heckman and Vytlacil, (2007, section 6) assume a continuous instrumental variable $Z \perp (U, Y_t)$, $t = 0, 1$, to identify MTE.[6]

Before providing formal theorems, I first define LATE and MTE in the present context, and obtain some required equalities. Since $T = 1\{P(R) - U \geq 0\}$, $P(r) = E[T|R = r]$. Similarly define $G(r) \equiv E[Y|R = r]$. The following discussion utilizes one-sided limits and one-sided derivatives at $r = r_0$. For any function $H(r)$, let $H_+ \equiv \lim_{r \downarrow r_0} H(r)$ and $H_- \equiv \lim_{r \uparrow r_0} H(r)$ be the right and left limits, respectively, when they exist. Further let $H'_+ \equiv \lim_{r \downarrow r_0} \partial H(r)/\partial r$ and $H'_- \equiv \lim_{r \uparrow r_0} \partial H(r)/\partial r$ be the right and left derivatives, respectively, when they exist. Then define

$$\tau_{LATE}(P_+, P_-, r_0) \equiv E\left[Y_1 - Y_0 | P_- < U \leq P_+, r = r_0\right].$$

Further define an MTE parameter as follows,

$$\tau_{MTE}(u, r) \equiv E[Y_1 - Y_0 | U = u, R = r].$$

For simplicity, when the MTE is evaluated at r_0, let $\tau_{MTE}(u) \equiv E[Y_1 - Y_0 | U = u, r = r_0]$.

When there is a jump in the treatment probability at r_0, $P_+ \neq P_-$, and then $\tau_{LATE}(P_+, P_-, r_0) = \frac{1}{P_+ - P_-} \int_{P_-}^{P_+} E[Y_1 - Y_0 | U = u, R = r_0]\, du = \frac{1}{P_+ - P_-} \int_{P_-}^{P_+} \tau_{MTE}(u)\, du$. When there is no jump, $P_+ = P_- = P(r_0)$, and hence $\tau_{LATE}(P_+, P_-, r_0) = \tau_{MTE}(P(r_0))$ by definition. Under Assumptions A1 and A2, when $P_+ \neq P_-$, $\tau_{LATE}(P_+, P_-, r_0) = \frac{G_+ - G_-}{P_+ - P_-}$, as shown in Hahn, Todd, and van der Klaauw (2001).

Rewrite $Y = \alpha T + Y_0$, where $\alpha \equiv Y_1 - Y_0$. Then

$$\begin{aligned} G(r) &\equiv E[Y|R = r] \\ &= E[\alpha 1\{U \leq P(r)\} | R = r] + E[Y_0 | R = r] \\ &= \int_0^{P(r)} E[\alpha | U = u, R = r]\, du + E[Y_0 | R = r]. \end{aligned} \tag{1}$$

By A2, $E[\alpha | U = u, R = r]$ and $E[Y_0 | R = r]$ are continuously differentiable in r, since $E[Y_0 | R = r] = \int_0^1 E[Y_0 | U = u, R = r]\, du$, and under standard regularity conditions (when dominated convergence

[6] More recently, Chiang and Sasaki (2017) extend this paper's RPK setup to further identify quantile treatment effects (QTEs). They impose smoothness on the conditional cumulative distribution function $F_{Y_t|U,R}(y|u, r)$ instead of the conditional mean function $E[Y_t | U = u, R = r]$, $t = 0, 1$.

theorem holds), one can differentiate under the integral sign. Further by A2, $P(r)$ is continuously differentiable in $r \in (r_0 - \varepsilon, r_0 + \varepsilon) \setminus \{r_0\}$, then $\int_0^{P(r)} E[\alpha | R = r, U = u]\, du$ is continuously differentiable in $r \in (r_0 - \varepsilon, r_0 + \varepsilon) \setminus \{r_0\}$ by applying Leibniz' rule.

Taking right and left limits at $r = r_0$ on both sides of equation (1) yields

$$\begin{aligned} G_+ - G_- &= \lim_{r \downarrow r_0} \int_0^{P(r)} E[\alpha | R = r, U = u]\, du - \lim_{r \uparrow r_0} \int_0^{P(r)} E[\alpha | R = r, U = u]\, du \\ &= E\left[\alpha 1\{U \le P_+\} | R = r_0\right] - E\left[\alpha 1\{U \le P_-\} | R = r_0\right] \\ &= E\left[\alpha 1\{P_- < U \le P_+\} | R = r_0, \right] \\ &= E\left[Y_1 - Y_0 | P_- < U \le P_+, R = r_0\right](P_+ - P_-). \end{aligned}$$

Therefore, we obtain the familiar fuzzy RD estimand

$$\tau_{LATE}(P_+, P_-, r_0) \equiv E\left[Y_1 - Y_0 | P_- < U \le P_+, R = r_0\right] = \frac{G_+ - G_-}{P_+ - P_-}. \tag{2}$$

Now consider the case where there is no jump, but a kink in the treatment probability, i.e., $P_+ = P_-$ and $P'_+ \neq P'_-$.

Theorem 1 *Suppose that Assumptions A1 and A2 hold. If $P_+ = P_-$, and $P'_+ \neq P'_-$, then*

$$\tau_{MTE}(P(r_0)) = \frac{G'_+ - G'_-}{P'_+ - P'_-}.$$

First notice that for $r \in (r_0 - \varepsilon, r_0 + \varepsilon) \setminus \{r_0\}$,

$$\frac{\partial}{\partial r} G(r) = P'(r) E[\alpha | U = P(r), R = r] + \int_0^{P(r)} \frac{\partial}{\partial r} E[\alpha | U = u, R = r]\, du + \frac{\partial}{\partial r} E[Y_0 | R = r]. \tag{3}$$

Then because $E[\alpha | U = u, R = r]$ *and* $E[Y_0 | R = r]$ are continuously differentiable in r, taking right and left limits at $r = r_0$ on both sides of equation (3) yields

$$\begin{aligned} G'_+ - G'_- &= P'_+ E\left[\alpha | U = P_+, R = r_0\right] - P'_- E\left[\alpha | U = P_-, R = r_0\right] \\ &\quad + \int_0^{P+} \frac{\partial}{\partial r} E[\alpha | U = u, R = r]|_{r_0} du - \int_0^{P-} \frac{\partial}{\partial r} E[\alpha | U = u, R = r]|_{r_0} du \\ &= P'_+ \tau_{MTE}(P_+) - P'_- \tau_{MTE}(P_-) + \int_{P_-}^{P+} \frac{\partial}{\partial r} E[\alpha | U = u, R = r]|_{r_0} du. \end{aligned} \tag{4}$$

When there is no jump, $P_+ = P_- = P(r_0)$, then equation (4) reduces to $G'_+ - G'_- = \left(P'_+ - P'_-\right)\tau_{MTE}(P(r_0))$. Therefore,

$$\tau_{MTE}(P(r_0)) \equiv E[Y_1 - Y_0 | U = P(r_0), R = r_0] = \frac{G'_+ - G'_-}{P'_+ - P'_-}. \tag{5}$$

That is, when there is no jump, but a kink, the ratio of kinks above identifies an average treatment effect for marginal compliers at $r = r_0$. Assuming $T = 1\{P(R) \geq U\}$, marginal compliers are individuals with $U = P(r_0)$, i.e., they are just indifferent to being treated or not at the kink point.

2.2 AIR Framework Representation

This section re-casts the identification results in the previous section in the AIR framework. To that end, assume $T = h(R, V)$ for some unobservable V. Given $Z = 1\{R \geq r_0\}$, one can rewrite $T = h_1(R, V) Z + h_0(R, V)(1 - Z)$.

For an individual with $R = r$, define $T_z(r) \equiv h_z(r, V)$, $z = 0, 1$, as her potential treatment status if she is below or above the cutoff. The following extends the definitions of individual types in Imbens and Angrist (1994) and Angrist, Imbens, and Rubin (1996) to the RD setup. In a common probability space (Ω, F, P), always taker $A(r)$ is the event that $T_1(r) = T_0(r) = 1$; never taker $N(r)$ is the event that $T_1(r) = T_0(r) = 0$; complier $C(r)$ is the event that $T_1(r) - T_0(r) = 1$; defier $D(r)$ is the event that $T_1(r) - T_0(r) = -1$. For simplicity, whenever there is no confusion, I will use A, N, C, and D to denote individual types, and similarly use T_z to denote $T_z(r)$, $z = 0, 1$. Let $S \equiv (Y_1, Y_0, T_1, T_0)$. The following assumption holds for $r \in (r_0 - \varepsilon, r_0 + \varepsilon)$.

Assumption:

B1 *(Monotonicity):* $\Pr(D(r)) = 0$ *for all* $r \in (r_0 - \varepsilon, r \ + \varepsilon)$.

B2 *(Smoothness):* $F_{S|R}(s|r)$ *is continuously differentiable for all* $s \in supp(S)$. $f_R(r)$ *is continuous and strictly positive at* $r = r_0$.

B1 is the standard assumption ruling out defiers, so changes in treatment status can only be in one direction. This assumption is equivalent to A1 in the previous section (Vytlacil, 2002). By A1, $T = 1\{P(R) - U \geq 0\}$. Without loss of generality, rewrite it as $T = 1\{P_0(R)(1 - Z) + P_1(R) Z \geq U\}$. Then $C(r)$ are individuals with $P_0(r) < U \leq P_1(r)$ and $C(r_0)$ are individuals with $P_- < U \leq P_+$. When $P_+ - P_- \neq 0$, $\tau_{LATE}(P_+, P_-, r_0)$ defined previously is then $E[Y_1 - Y_0 | C, R = r_0]$ in this section's notation.

B2 assumes that the joint distribution of (Y_0, Y_1, T_0, T_1) given the running variable R is smooth in the neighborhood of r_0. B2 is a smoothness parallel of the LATE type independence assumption in the causal model framework (see, e.g., Imbens and Angrist, 1994), which requires (Y_0, Y_1, T_0, T_1) to be independent of a binary instrument. Here the binary instrumental variable (IV) is $Z = I\{R \geq r_0\}$.

Further B2 implies that $E[Y_t|R = r, \Theta]$ for $t = 0, 1$ and $\Pr(\Theta|R = r)$ for $\Theta \in \{A, N, C, D\}$ are continuously differentiable in the neighborhood of $r = r_0$. These weaker conditions are sufficient to identify mean treatment effects. In contrast, B2 is sufficient, but stronger than necessary. B2 can be used to identify distributional effects as discussed in Frandsen, Frölich, and Melly (2012).

B2 allows for essential heterogeneity of treatment effects and self-selection into different types, since no restrictions are imposed on the joint distribution of (Y_0, Y_1, T_0, T_1). For example, individuals may self-select to be compliers for higher idiosyncratic gains. Following the arguments in Lee (2008) and McCrary (2008), B2 in practice implies that individuals can not precisely manipulate the running variable to sort around the RD threshold.

Continuous differentiability in B2 or A2 rules out not only jumps but also kinks. One can therefore perform falsification tests to test smoothness (no jumps and kinks) of the conditional means of covariates, conditional on the running variable. Alternatively, one may perform the 'manipulation' tests as in McCrary (2008).

Theorem 2 *Assume that B1 and B2 hold.* $P_+ - P_- = \Pr(C|R = r_0)$ *and* $P'_+ - P'_- = \frac{\partial}{\partial r}\Pr(C|R = r)|_{r_0}$. *Further if* $P_+ \neq P_-$, *then* $(G_+ - G_-)/(P_+ - P_-) = E[Y_1 - Y_0|C, R = r_0]$; *otherwise, as* $P_+ - P_- \to 0$, $\tau_{LATE}(P_+, P_-, r_0) \to \tau_{MTE}(P(r_0))$, *and when* $P'_+ \neq P'_-$,

$$\tau_{MTE}(P(r_0)) = \frac{G'_+ - G'_-}{P'_+ - P'_-}. \tag{6}$$

Theorem 2 states that given a kink, when the jump goes arbitrarily close to zero, $\left(G'_+ - G'_-\right)/\left(P'_+ - P'_-\right)$ identifies a limiting form of RD LATE, $\tau_{MTE}(P(r_0))$. The probability of compliers or the compliance rate is a function of r by definition. Under B1 and B2, a jump in the treatment probability exists if the compliance rate at $r = r_0$ is not zero, while a kink exists if the derivative of the compliance rate at $r = r_0$ is not zero.[7]

[7] Given monotonicity, one can assume that the treatment is generated by $T \equiv 1\{P(R) \geq U\}$. Then when $P_+ = P_-$, what is in the conditioning set of $\tau_{LATE}(P_+, P_-, r_0)$ is $U = P(r_0)$, namely marginal compliers at $r = r_0$.

3 Regression Probability Jump & Kink (RPJK) Design

The discussion so far has given a causal interpretation of the kink estimand when there is no jump in the treatment probability at the RD threshold. This section now considers what happens when a jump might be present. I first show that when both a jump and a kink are present, the kink estimand generally loses its causal interpretation. I then discuss causal identification regardless of whether there is a jump, or a kink, or both by a RPJK design.

By assumption A1, $T = 1\{P(R) - U \geq 0\}$. Without loss of generality, rewrite it as $T = 1\{P_0(R)(1-Z) + P_1(R)Z \geq U\}$. To facilitate the discussion in this section, assume smoothness of $P_z(r)$.

Assumption:

A3 (Smoothness): $P_z(r)$, $z = 0, 1$, is continuously differentiable in r for $r \in (r_0 - \varepsilon, r_0 + \varepsilon)$.[8]

By Assumption A3, $P_+ = P_1(r_0)$, $P_- = P_0(r_0)$, $P'_+ = \frac{\partial}{\partial r} P_1(r)|_{r_0}$, and $P'_- = \frac{\partial}{\partial r} P_0(r)|_{r_0}$. Then when $P_1(r) \neq P_0(r)$, define

$$\begin{aligned} \tau(r) &\equiv \tau_{LATE}(P_1(r), P_0(r), r) \equiv E[\alpha | P_0(r) \leq U \leq P_1(r), R = r] \\ &= \frac{1}{P_1(r) - P_0(r)} \int_{P_0(r)}^{P_1(r)} \tau_{MTE}(u, r)\, du, \end{aligned} \tag{7}$$

so $\tau(r_0) = \tau_{LATE}(P_+, P_-, r_0)$, and further define $\tau'(r_0) \equiv \frac{\partial}{\partial r}\tau(r)|_{r_0}$.[9] By equation (7),

$$\tau(r)(P_1(r) - P_0(r)) = \int_{P_0(r)}^{P_1(r)} \tau_{MTE}(u, r)\, du. \tag{8}$$

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[8] Section 2 only assumes smoothness of the actual propensity score $P(r)$. This section further assumes smoothness of the potential $P_z(r)$ to define $\tau'(r_0)$ after equation (7). As I will show, $\tau'(r_0)$ partly depends on how the size of jump $P_1(r) - P_0(r)$ changes with the running variable r (given the fixed threshold r_0) near r_0.

[9] $\tau'(r_0)$ measures how the treatment effect varies with the running variable near the fixed threshold r_0. Dong and Lewbel (2015) refer to $\tau'(r_0)$ as the treatment effect derivative (TED) and show that TED is nonparametrically identified given a jump in the treatment probability and the smoothness conditions in this paper. Cerulli, Dong, Lewbel, and Poulsen (2017) show how TED can be used to test stability and hence external validity of RD estimates.

Taking derivatives on both sides of equation (8) at $r = r_0$ yields

$$\begin{aligned} & \tau'(r_0)(P_+ - P_-) + \tau(r_0)\left(P'_+ - P'_-\right) \\ = \ & \frac{\partial}{\partial r}\left(\int_{P_0(r)}^{P_1(r)} \tau_{MTE}(u,r)\,du\right)|_{r_0} \\ = \ & P'_+\tau_{MTE}(P_+) - P'_-\tau_{MTE}(P_-) + \left(\int_{P_-}^{P_+} \frac{\partial}{\partial r}\tau_{MTE}(u,r)|_{r_0} du\right) \\ = \ & G'_+ - G'_-, \end{aligned}$$

where the last equality follows from equation (4). If both a jump and a kink are present, i.e., $P'_+ \neq P'_-$ and $P_+ \neq P_-$, then

$$\frac{G'_+ - G'_-}{P'_+ - P'_-} = \tau(r_0) + \frac{P_+ - P_-}{P'_+ - P'_-}\tau'(r_0). \tag{9}$$

That is, the RPK estimand no longer equals the causal parameter unless $\tau'(r_0) = 0$. The potential bias is given by $(P_+ - P_-)\tau'(r_0)/\left(P'_+ - P'_-\right)$. Using the AIR framework, the proof of Theorem 2 similarly shows $G'_+ - G'_- = \tau'(r_0)(P_+ - P_-) + \tau(r_0)\left(P'_+ - P'_-\right)$, and hence equation (9) holds.[10]

The following Theorem provides a way to identify a causal effect regardless of whether there is a jump, a kink, or both.

Theorem 3 *Assume that A1 and A2 hold. Further assume that* $P_+ - P_- = 0$ *and* $P'_+ - P'_- = 0$ *do not both hold (i.e., there is either a jump, or a kink, or both). Given any sequence of nonzero weights* ω_n *such that* $\lim_{n\to\infty}\omega_n = 0$,

$$\tau_{LATE}(P_+, P_-, r_0) = \lim_{\omega_n \to 0} \frac{G_+ - G_- + \omega_n\left(G'_+ - G'_-\right)}{P_+ - P_- + \omega_n\left(P'_+ - P'_-\right)}. \tag{10}$$

Let $\eta_n = \frac{G_+ - G_- + \omega_n\left(G'_+ - G'_-\right)}{P_+ - P_- + \omega_n\left(P'_+ - P'_-\right)}$ be a 'drifting parameter' that changes with sample size n. Theorem 3 then shows that $\tau_{LATE}(P_+, P_-, r_0)$ equals the limit of this drifting parameter. When there is no jump, $P_+ - P_- = 0$ and $G_+ - G_- = 0$, making $\tau_{LATE}(P_+, P_-, r_0) = \tau_{MTE}(P(r_0))$, and equation (10) becomes the RPK estimand. When there is a jump, then regardless of whether there is a kink or not, equation (10) becomes the standard RD estimand.

Here the sequence ω_n is required to go to zero as the sample size goes to infinity. The next section

[10] See equation (18) and the discussion in the proof of Theorem 2. In the AIR model notation, RD LATE is equivalently defined as $\tau(r_0) \equiv E[Y_1 - Y_0|C(r_0), R = r_0]$. Further, TED is equivalently defined as $\tau'(r_0) \equiv \frac{\partial}{\partial r}E[Y_1 - Y_0|C(r), R = r]|_{r_0}$, because assuming $T = 1\{P_0(R)(1-Z) + P_1(R)Z \geq U\}$, those with $P_0(r) < U \leq P_1(r)$ are $C(r)$.

shows that the weights in a local 2SLS estimator, a special case of the estimator corresponding to Theorem 3, have this property. Equation (10) (and its special case, the local 2SLS discussed later) has the advantage that it provides a way to identify a valid causal effect if one is unsure whether a jump is present or not.

The following Corollary 1a shows the conditions under which the RPK estimand and the standard RD estimand identify the same parameter, and so in this special case both could be used.

Corollary 1a *Assume that A1, A2, and A3 hold, and* $\tau'(r_0) = 0$*. If* $P_+ \neq P_-$ and $P'_+ \neq P'_-$*, i.e., both a jump and a kink exist, then*

$$\tau_{LATE}(P_+, P_-, r_0) = \frac{G_+ - G_-}{P_+ - P_-} = \frac{G'_+ - G'_-}{P'_+ - P'_-}.$$

$\tau'(r_0) = 0$ when the treatment effect is locally constant.[11] Having $\tau'(r_0) = 0$ is a strong restriction that is not required anywhere else in this paper. By equation (9), $\tau'(r_0) = \frac{G'_+ - G'_-}{P_+ - P_-} - \frac{P'_+ - P'_-}{P_+ - P_-}\tau(r_0)$, which is estimable when $P_+ \neq P_-$ and hence $\tau'(r_0) = 0$ is a testable restriction.[12]

When $\tau'(r_0) = 0$, the jump estimand and the kink estimand are both equal, and hence both can be used. Similarly, Heckman and Vytlacil (2005) note that when the treatment effect is (locally) constant, LATE equals its limiting MTE value.

Corollary 1b *Assume that A1, A2, and A3 hold, and* $\tau'(r_0) = 0$*. If either a jump, or a kink, or both exist, then*

$$\tau_{LATE}(P_+, P_-, r_0) = \frac{G_+ - G_- + \omega\left(G'_+ - G'_-\right)}{P_+ - P_- + \omega\left(P'_+ - P'_-\right)} \tag{11}$$

for any $\omega \neq -(P_+ - P_-)/\left(P'_+ - P'_-\right)$.

Unlike Theorem 3, Corollary 1b employs fixed weights ω, and so exploits both the jump and the kink when both are present. Lastly, it is worth mentioning that the above results can be readily extended to identification using higher-order derivative changes at the RD threshold. Further discussion of this is provided in Appendix III.

[11] $\tau'(r_0) = 0$ is a strictly weaker condition than assuming a locally constant treatment effect, because the latter would imply that all derivatives of $\tau(r)$ at $r = r_0$ were zero, not just the first derivative $\tau'(r_0)$.

[12] For example, one can estimate $\tau'(r_0)$ as the coefficient of $T(R - r_0)$ in a local linear regression of Y on T, $(R - r_0)$ and $T(R - r_0)$. Alternatively, one can directly estimate $\tau'(r_0) = \left(G'_+ - G'_-\right)/(P_+ - P_-) - (G_+ - G_-)\left(P'_+ - P'_-\right)/(P_+ - P_-)^2$. Details can be found in Dong and Lewbel (2015).

4 Instrumental Variable Interpretation and Local 2SLS estimation

This section relates the identification results to instrumental variable (IV) models. Like all RD type estimators, the 2SLS estimator discussed in this section has a bandwidth that shrinks to zero as the sample size goes to infinity. It is therefore referred to as a local 2SLS estimator. When there is either a jump, or a kink, or both, the local 2SLS estimator proposed here automatically provides weights that satisfy the properties required by Theorem 3. For ease of exposition, I assume a uniform kernel for the discussion in this section. Using more complicated kernel functions would just complicate the presentation, but not the conclusions in this section.

Consider the following local linear regression representation of Y for $R \in (r_0 - \varepsilon, r_0 + \varepsilon)$,

$$Y = a + b(R - r_0) + \tau T + e, \tag{12}$$

where a, b, and τ are coefficients, and the error e may be correlated with the treatment indicator T. In general, e might also be correlated with R and hence Z for any strictly positive ε. Consider further the local linear representation for the first-stage reduced-form treatment equation

$$T = \beta_1 Z + \beta_2 (R - r_0) Z + \beta_3 (R - r_0) + \beta_4 + \upsilon, \tag{13}$$

where β_j, $j = 1, 2, 3, 4$ are the coefficients in this equation.[13]

The standard fuzzy design RD estimator is numerically equivalent to the IV estimator of τ in equation (12), using Z as an 'exclusion restriction.' Under this paper's identifying assumptions, neither a level change (a jump) nor a slope change (a kink) should be present in the structural outcome equation. Therefore, both Z and $Z(R - r_0)$ are excluded from equation (12). Z and $Z(R - r_0)$ can be correlated with e for any strictly positive ε. Nevertheless, the standard RD estimator and similarly the proposed new estimators are consistent when the bandwidth $\varepsilon \to 0$ as the sample size $n \to \infty$.

[13]When implementing the standard RD estimator, empirical practitioners tend to include the interaction term $Z(R - r_0)$ in both the first-stage and the structural outcome equations. Inclusion of $Z(R - r_0)$ in the first stage allows for flexible functional form and hence consistent estimation of the size of the jump. By further including $Z(R - r_0)$ in the structural outcome equation, local identification of the treatment effect relies solely on a jump in the treatment probability at $r = r0$, so any kink that might be present is not used for identification. However, under this paper's smoothness conditions (requiring continuous differentiability), both Z and $Z(R - r_0)$ should be excluded from the structural outcome equation.

Substituting equation (13) into equation (12) yields the reduced form Y equation

$$Y = \gamma_1 Z + \gamma_2 (R - r_0) Z + \gamma_3 (R - r_0) + \gamma_4 + u, \tag{14}$$

where $\gamma_1 = \beta_1 \tau$, $\gamma_2 = \beta_2 \tau$, $\gamma_3 = b + \tau \beta_3$, $\gamma_4 = a + \tau \beta_4$ and $u = \tau \upsilon + e$.

Then in the limit the coefficients in these local linear regressions (13) and (14) equal the conditional means of T and Y and derivatives of the conditional means, regardless of their true functional forms (as long as they are sufficiently smooth). Therefore, regardless of the true functional forms of Y and T asymptotically we have

$$P_+ - P_- = \beta_1, \quad P'_+ - P'_- = \beta_2, \tag{15}$$

$$G_+ - G_- = \gamma_1, \quad G'_+ - G'_- = \gamma_2. \tag{16}$$

Let Y^*, T^*, Z_1^*, and Z_2^* be Y, T, Z, and $(R - r_0)\, Z$ after partialling out $(R - r_0)$, respectively, i.e., they are the residuals from local linear regressions of Y, T, Z, and $(R - r_0)\, Z$ on a constant and $(R - r_0)$. Then equations (13) and (12) can be rewritten as

$$\begin{aligned} T^* &= \beta_1 Z_1^* + \beta_2 Z_2^* + \upsilon, \\ Y^* &= \tau T^* + e, \end{aligned}$$

and the reduced-form Y equation (14) can be rewritten as

$$Y^* = \gamma_1 Z_1^* + \gamma_2 Z_2^* + u.$$

The 2SLS estimator in this case therefore provides an estimate of

$$\begin{aligned} \tau &= \frac{cov\left(Y^*, \beta_1 Z_1^* + \beta_2 Z_2^*\right)}{cov\left(T^*, \beta_1 Z_1^* + \beta_2 Z_2^*\right)} = \frac{cov\left(\gamma_1 Z_1^* + \gamma_2 Z_2^*, T^*\right)}{cov\left(\beta_1 Z_1^* + \beta_2 Z_2^*, T^*\right)} \\ &= \frac{cov\left(T^*, Z_1^*\right) \gamma_1 + cov\left(T^*, Z_2^*\right) \gamma_2}{cov\left(T^*, Z_1^*\right) \beta_1 + cov\left(T^*, Z_2^*\right) \beta_2}. \end{aligned}$$

Let $\omega_1 = cov\left(T^*, Z_1^*\right)$ and $\omega_2 = cov\left(T^*, Z_2^*\right)$, so these weights reflect the relative strength of the two IVs, Z_1^* and Z_2^*. Then

$$\tau = \frac{\omega_1 \gamma_1 + \omega_2 \gamma_2}{\omega_1 \beta_1 + \omega_2 \beta_2}.$$

Plugging in γ_1, γ_2, β_1 and β_2 gives

$$\tau = \frac{\omega_1 (G_+ - G_-) + \omega_2 \left(G'_+ - G'_-\right)}{\omega_1 (P_+ - P_-) + \omega_2 \left(P'_+ - P'_-\right)}. \tag{17}$$

When there is no jump, but a kink, $\beta_1 = \gamma_1 = 0$ and $\omega_1 = 0$, and so equation (17) reduces to γ_2/β_2, which asymptotically equals the RPK estimand $\left(G'_+ - G'_-\right) / \left(P'_+ - P'_-\right)$. So in this case the kink estimand identifies the causal effect of the treatment.

The 2SLS estimator in equation (17) is a special case of the estimator corresponding to Theorem 3. The weights here satisfy the property specified in Theorem 3. To see this, consider first the case where a jump is present. In this case, as $\varepsilon \to 0$, $Z(R - r_0)$ goes to zero. Then $\omega_2 = cov\left(T^*, Z_2^*\right)$ and hence ω_2/ω_1 both go to zero. It follows that if there is a jump, i.e., if $\beta_1 \neq 0$, asymptotically

$$\frac{\omega_1\gamma_1 + \omega_2\gamma_2}{\omega_1\beta_1 + \omega_2\beta_2} = \frac{\gamma_1}{\beta_1} = \frac{G_+ - G_-}{P_+ - P_-}.$$

Alternatively, if there is no jump, i.e., $\beta_1 = 0$ and hence $\gamma_1 = 0$, then

$$\frac{\omega_1\gamma_1 + \omega_2\gamma_2}{\omega_1\beta_1 + \omega_2\beta_2} = \frac{\gamma_2}{\beta_2} = \frac{G'_+ - G'_-}{P'_+ - P'_-}.$$

One can then implement the RPJK design as a 2SLS estimator for a chosen bandwidth. That is, estimate a regression of Y on a constant, T, and a low-order polynomial of $(R - r_0)$, using Z, and $(R - r_0)Z$ to instrument on T. If desired, kernel weights can be applied the resulted estimator is a weighted local 2SLS. A generic feature of 2SLS is that it chooses efficient weights for combining instrumental variables (see, e.g., Davidson and MacKinnon, 1993, and Chapter 4 of Angrist and Pischke, 2008).

In practice, based on both theory and empirical evidence, when there is a jump, one can apply the standard RD design. When there is no jump, but a kink, one can apply the RPK design. However, when there is a kink but one is unsure if a significant jump is also present, the RPJK design is more appropriate. The RPJK design is preferred particularly when there is a relatively large kink with a small jump, and when the kink can also be justified on institutional grounds (as in this paper's two empirical applications).

5 Identifying Other Parameters and Checking for Monotonicity

Results derived in the previous sections can be readily extended to identify other parameters of interest, e.g., distributional effects or QTEs for compliers or marginal compliers at the cutoff. Identification of these parameters has been discussed in the standard RD design (Frandsen, Frölich, and Melly 2012). However, one can similarly identify these parameters under this paper's more general conditions, i.e., in the presence of either a jump, or a kink, or both. These additional results are summarized in Corollary 2 in Appendix II.

In addition, one can identify and easily estimate $E\left[X|P_- < U \leq P_+, R = r_0\right]$ or more generally $F_{X|P_- < U \leq P_+, R=r_0}(x)$ for some pre-determined covariate X, i.e., the mean or distribution of characteristics for complier (or marginal complier) at $R = r_0$. This identification assumes that the smoothness and monotonicity assumptions hold (if only smoothness holds, then what is identified is the weighted difference in means or distributions between compliers and defiers). Corollary 3 in Appendix II provides formal identification results. Below I summarize these results and briefly discuss how to check for monotonicity using the identified complier (or marginal complier) characteristics assuming that smoothness conditions hold.

Define $\mathbb{X}(r) \equiv E\left[XT|R = r\right]$. Following the notational conventional to let $\mathbb{X}_+$ and $\mathbb{X}_-$ be the right and left limits, and $\mathbb{X}'_+$and $\mathbb{X}'_-$ be the right and left derivatives of $\mathbb{X}(r)$ at $r = r_0$, respectively, whenever they exist. Assume that A1 holds and that A2 holds after replacing Y_t with X. Then $E\left[X|P_- < U \leq P_+, R = r_0\right] = \frac{\mathbb{X}_+ - \mathbb{X}_-}{P_+ - P_-}$ when $P_+ \neq P_-$, and $E\left[X|P_- < U \leq P_+, R = r_0\right] = \frac{\mathbb{X}'_+ - \mathbb{X}'_-}{P'_+ - P'_-}$ when $P_+ = P_-$, but $P'_+ \neq P'_-$. More generally, as long as $P_+ = P_-$ and $P'_+ = P'_-$ do not both hold, then $E\left[X|P_- < U \leq P_+, R = r_0\right] = \lim_{n\to\infty} \frac{(\mathbb{X}_+ - \mathbb{X}_-) + \kappa_n\left(\mathbb{X}'_+ - \mathbb{X}'_-\right)}{(P_+ - P_-) + \kappa_n\left(P'_+ - P'_-\right)}$, where $\kappa_n \to 0$ as $n \to \infty$. One can similarly identify $F_{X|P_- < U \leq P_+, R=r_0}(x)$ by replacing X with $1(X \leq x)$ in the definition of $\mathbb{X}(r)$.

Identifying complier (or marginal complier) characteristics provides an opportunity to check for the monotonicity assumption A1 or B1, in addition to the usual tests for smoothness. Assuming smoothness holds, with no defiers, the identified distribution of covariates for compliers must lie between zero and one. If the estimate lies significantly outside the range of zero to one, one can reject the assumption that smoothness and monotonicity both hold. This is analogous to Kitagawa's (2015) test of the LATE assumptions. Kitigawa's (2015) test utilizes the fact that given monotonicity along with the other LATE assumptions, the identified probability density distributions of potential outcomes for compliers should be nonnegative. Here I test covariates. Typically, binary covariates such as gender, race, or ethnicity indicators are available for these tests. Let $\overline{X}_S \equiv \lim_{n\to\infty} \frac{(\mathbb{X}_+ - \mathbb{X}_-)+\kappa_n(\mathbb{X}'_+ - \mathbb{X}'_-)}{(P_+ - P_-)+\kappa_n(P'_+ - P'_-)}$. With a binary covariate, one can simply test that the mean X for compliers (or marginal compliers) $\overline{X}_S$ lie between zero and one. More generally if X has a known sign, say $X \geq 0$, then monotonicity implies $\overline{X}_S \geq 0$; otherwise, $\overline{X}_S$ may be negative. Alternatively if $X \in [x_{min}, x_{max}]$, monotonicity implies $x_{min} \leq \overline{X}_S \leq x_{max}$, and then to check for monotonicity in empirical applications, one can simply check whether $\overline{X}_S$ is bounded between x_{min} and x_{max}.

6 Elite School Attendance and Educational Attainment

This section re-evaluates the impacts of attending an elite school on educational attainment, using the data from Clark and Del Bono (2016). The analysis here serves primarily to illustrate the technique, using the new identification results to confirm the results of a previous global parametric analysis.[14]

Here T is a binary indicator for attending an elite school.[15] The outcome Y is post-secondary education in years. Secondary education is compulsory in the UK for the cohorts considered (those born in the 1950's). The assignment score R is the sum of four test scores and a teacher assessment component. Two of the test scores are from Verbal Reasoning Quotient (VRQ) tests, one is from an English attainment test and the other is from an arithmetic attainment test. Each component is standardized to have mean 100 and standard deviation 15.

Students with assignment scores below 540 were almost all allocated to a non-elite school. Students with assignment scores above 560 were allocated to an elite school unless 1) they were assessed by their head teacher as "unsuitable," or 2) one of their VRQ test scores was below 112 and their overall assignment score was below 580. Students with scores between 540 and 560 were assigned to an elite school partly based on their order of merit, and so the higher their scores, the more likely they were assigned to an elite school. Details of the construction of the assignment score and the school assignment procedure can be found in Clark and Del Bono (2016).

Whether attending a better school by random chance leads to better school achievements and long-run outcomes has been highly debated. In the following I explore the jumps and kinks in the school

[14]Clark and Del Bono (2016) propose an IV approach using the entire nonlinear school assignment formula as an IV for global estimation of a single effect. They show that their approach is valid in a constant effect parametric regression model.

[15]These schools are not officially called "elite schools". In Scotland they are called Senior Secondary Schools, in England Grammar Schools. The term "elite school" is generally used in the education literature to refer to selective high schools. See, e.g., Abdulkadiroğlu, Angrist, and Pathak (2012) for similar schools in the US.

assignment formula (as shown in Figure 1 below) for local identification of causal effects at each of the two thresholds. Performance of the proposed RPK and RPJK estimators is compared to that of the standard RD estimator. I focus on males. Results for females lead to similar general conclusions, though they are less precise, partly due to females' greater heterogeneity in schooling choices. To save space, results for females are provided in Appendix IV.

Figure 6.1 shows the probability of attending an elite school as a function of the assignment score for males. Consistent with the assignment rule, it shows noticeable kinks, with small or possibly nonexistent jumps, at the threshold scores 540 and 560. Figure 4 shows post-secondary education. The pattern in Figure 6.2 largely mimics that in Figure 6.1 in terms of kinks and possible but small jumps.

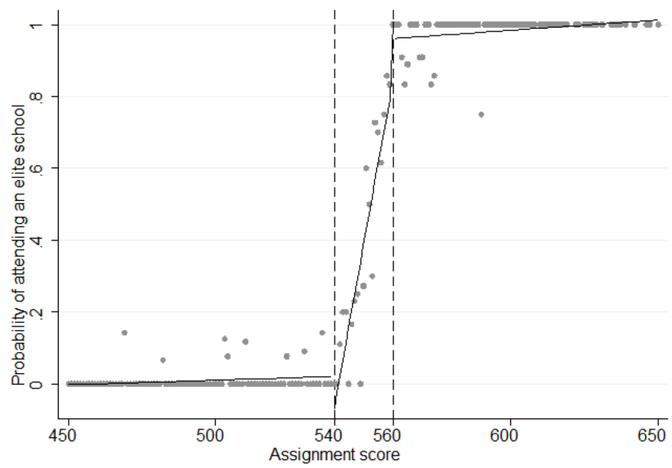


Figure 6.1 Assignment score and elite school attendance for males

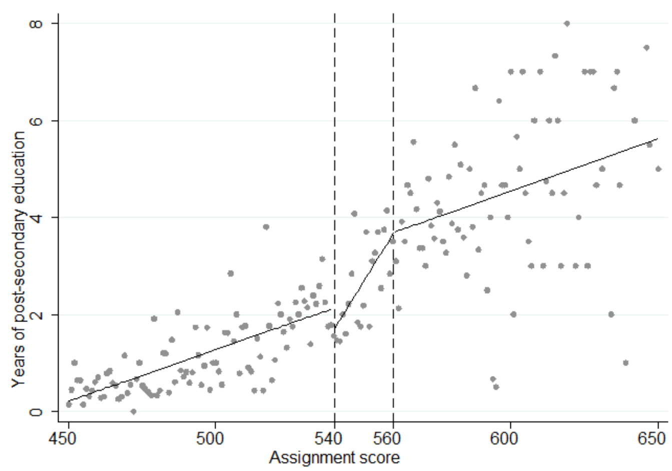


Figure 6.2 Assignment score and post-secondary education for males

I use the "upper" and "lower" assignment score samples to estimate the two local effects at $R = 560$ and $R = 540$ separately. For the main analysis, the largest possible symmetric window (± 20) around

Table 6.1 Smoothness of the density of assignment score

$R \in [540, 580]$		$R \in [520, 560]$	
1(R>560)	0.003	1(R<540)	-0.007
	(0.004)		(0.004)
(R-560)1(R>560)	0.000	(R-540)1(R<540)	-0.000
	(0.000)		(0.000)

Note: Robust standard errors (in parentheses) are clustered at the assignment score level.

each cutoff, 540 or 560, is chosen. That is, the “upper" sample consists of scores between 540 and 580 and the “lower" sample between 520 and 560. The sample sizes are 361 and 409, respectively. The small sample size and the narrow distance between the cutoffs greatly limit the range of feasible bandwidths. I estimate local linear regressions with a uniform kernel. Additional results using different bandwidths are reported in Table A1 in Appendix IV.

To evaluate validity of the smoothness conditions A2 or B2, I test smoothness of the empirical density of the running variable (the fraction of observations at each value of the assignment score) by local linear regressions. The results are presented in Table 6.1. No significant jumps or kinks are found at either threshold. Figure 6.3 further shows smoothness of the empirical density of the assignment score.

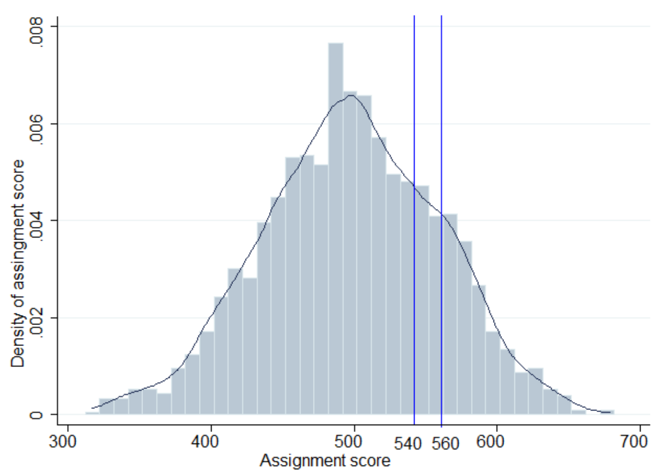


Figure 6.3 (a) Empirical density of assignment score and the fitted kernel density function

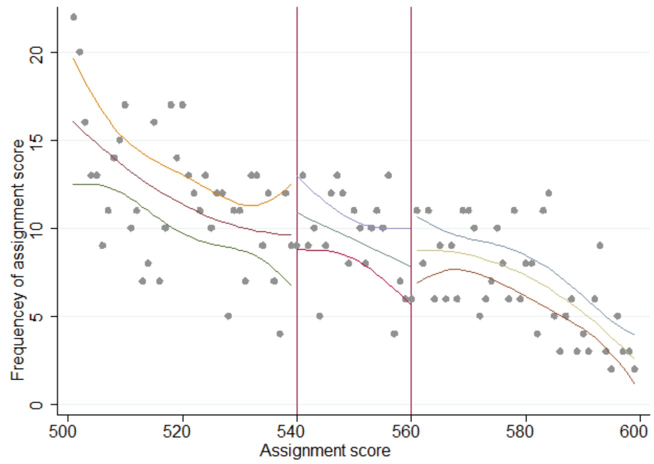


Figure 6.3 (b) Frequency of assignment score, local polynomial fit, and 95% CI

I next evaluate whether the conditional means of pre-determined covariates are smooth at the thresholds. This test can be seen as a falsification test. Following the standard practice, I test that treatment has no false significant effects on these covariates. That is, I apply the RPJK estimator using the covariates as dependent variables. Among the 46 covariates, attending an elite school shows a weakly significant impact only on one indicator (out of eight) for mother's SES category. That is, the false rejection rate is roughly 2 percent. These results suggest that the required smoothness is plausible in this case. Monotonicity pertains to the interpretation of the identified effects and will be assessed after presenting the estimated complier or marginal complier characteristics.

Table 6.2 Complier or marginal complier characteristics

Age (months)	Birth order	Test7	Test9	High SES	Middle SES	Low SES
Upper threshold: R=560						
123.5	2.490	115.7	119.4	0.098	0.567	0.335
(3.968)***	(0.420)***	(4.190)***	(3.161)***	(0.091)	(0.132)***	(0.140)**
Lower threshold: R=540						
124.1	1.778	121.1	123.8	0.230	0.467	0.304
(2.747)***	(0.183)***	(1.510)***	(1.887)***	(0.071)***	(0.062)***	(0.046)***

Note: Robust standard errors are clustered at each integer test score value; Age refers to that in Dec. 1962; Test7 is test score at age 7 and test9 is test score at age 9; high SES refers to father in professional or technical etc. non-manual occupation; Middle SES refers to father in skilled manual profession; Low SES refers to father in unskilled, semiskilled profession or unemployed, disabled, etc. **Significant at the 5% level; ***Significant at the 1% level.

Who are compliers or marginal compliers at the lower or upper kink points?[16] Table 6.2 presents

[16] If the head teacher is the decision maker, then marginal compliers are those who the head teacher is indifferent to the choices of assigning to an elite school or not. Put it differently, marginal compliers are the last (marginal) students the head teacher would assign to an elite school if they scored just above the threshold, and would not otherwise.

the estimated average characteristics for compliers or marginal compliers. These characteristics include age in December 1962, birth order, test scores at age 7 and age 9, and SES in three categories.[17] Compliers or marginal compliers at the lower kink point on average show more advantageous characteristics than those at the upper kink point. For example, those at the upper threshold have lower test scores at age 7 and age 9. They are less likely to be from a high SES family and more likely to be from a middle SES family. They are also slightly younger and have a higher birth order. These results might appear to be counter-intuitive. However, recall that at the lower threshold compliers or marginal compliers only need to be just above the lower threshold to attend an elite school, while at the upper threshold, they need to above the higher threshold to attend an elite school and would not otherwise. Therefore, it is plausible that there is a positive selection into compliers or marginal compliers at the lower threshold, and a negative selection at the upper threshold.

Table 6.3 First-stage and reduced-form outcome estimates

Dependent var.	Elite school	Elite school	Education	Education
		$R \in [540, 580]$		
1(R>560)	0.074	0.089	-0.050	-0.045
	(0.057)	(0.065)	(0.478)	(0.555)
(R-560)1(R>560)	-0.044***	-0.043***	-0.077**	-0.066*
	(0.004)	(0.005)	(0.035)	(0.038)
Covariates	N	Y	N	Y
R-squared	0.549	0.622	0.086	0.283
		$R \in [520, 560]$		
1(R<540)	0.098**	0.111**	0.689*	0.968**
	(0.044)	(0.047)	(0.361)	(0.412)
(R-540)1(R<540)	-0.046***	-0.044***	-0.062**	-0.059*
	(0.004)	(0.004)	(0.029)	(0.032)
Covariates	N	Y	N	Y
R-squared	0.439	0.524	0.053	0.259

Note: All specifications control for a linear term of the assignment score, with or without additionally an extensive set of covariates, including fixed effects for the school and grade attended in 1962, father's social class in eight categories, mother's occupation in nine categories, age within grade, and linear and quadratic terms of test scores at ages 7 and 9. Robust standard errors are in parentheses and are clustered at the assignment score level; * Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

The estimated complier or marginal complier characteristics all have the plausible positive sign and all estimated probabilities are between 0 and 1. Formal one-sided tests cannot reject that the estimated

[17] These high, middle, and low SES indicators are constructed from the eight father's social classes with some similar adjacent categories combined.

mean characteristics are positive and that the probabilities are between 0 and 1 at any conventional significance level. Overall, no evidence against monotonicity is found.

Table 6.3 presents estimation results from the first-stage and reduced-form outcome regressions. Consistent with Figure 6.1, these estimates suggest that the probability of attending an elite school has significant kinks at both thresholds, no significant jump at the upper threshold, and a somewhat imprecisely estimated jump of 10 to 11 percent at the lower threshold. The estimated jumps and kinks remain very similar regardless of whether one controls for the extensive set of covariates or not. This is what one would expect if the design is valid.

Estimation of the reduced-form outcome equation reveals a similar pattern to the first-stage. For example, at the upper threshold, both the first-stage and the reduced-from outcome estimates show no significant jumps. Joint tests (provided in Table A2 in Appendix IV) further confirm that these estimated jumps are jointly insignificant at the upper threshold, with or without including covariates. If there were no jump in the first-stage, but a significant jump in the reduced-form outcome regression, one would then be concerned about the validity of the monotonicity or smoothness assumptions.

Table 6.4 Impacts of elite school attendance on years of post-secondary education for males

	RD		RPK		RPJK	
			$R \in [540, 580]$			
	-0.678	-0.509	1.735**	1.546*	1.703**	1.503*
	(6.330)	(5.709)	(0.805)	(0.824)	(0.853)	(0.888)
Covariates	N	Y	N	Y	N	Y
			$R \in [520, 560]$			
	6.995	8.696*	1.335**	1.332**	1.536**	1.705**
	(4.563)	(4.516)	(0.604)	(0.646)	(0.647)	(0.731)
Covariates	N	Y	N	Y	N	Y

Note: The covariates included are fixed effects for the school and grade attended in 1962, father's social class in eight categories, mother's occupation in nine categories, age within grade, and linear and quadratic terms of test scores at ages 7 and 9. Robust standard errors are clustered at the assignment score level and are in parentheses; *Significant at the 10% level; **Significant at the 5% level.

Table 6.4 presents the estimated impacts of attending an elite school on educational attainment. In the following I focus on estimates when controlling for the extensive set of covariates, though results are largely robust to controlling for covariates or not. At the upper threshold, the jump is insignificant. Consequently, the standard RD estimator leads to estimates with much larger standard errors and an economically implausible negative sign. In contrast, the RPK estimator generates an estimated effect of about 1.5 years, which is significant at the 5% level, so attending an elite school increases completed post-secondary education by 1.5 years for males. The RPJK estimator generates estimates close to those of the RPK estimator. This is consistent with Theorem 3 and Corollary 1b in that the 2SLS estimator using both a jump and a kink as IVs reduces to the kink estimator when there is no jump.

At the lower threshold, the jump is significant at the 5% level. Recall that when a jump exists, the RPK estimator is biased even with large samples, unless $\tau'(r_0)$ equals 0. Based on equation (9), the bias in the kink estimator equals $\tau'(r_0)(P_+ - P_-)/(P'_+ - P'_-)$. Here the jump appears to be much less precisely estimated than the kink, having a standard error that is an order of magnitude larger than the standard error of the kink estimate (see Table 6.4). So unless the bias in the kink estimator is very large, estimation based on the kink will be much more accurate than the jump estimator, at least in terms of root mean squared error (RMSE). The bias in the kink estimator is likely to be very small, both because $P'_+ - P'_-$ is relatively quite large (this is visible in Figure 6.2) and because $\tau'(r_0)(P_+ - P_-)$ is likely to be quite small.

To evaluate the possible bias in the RPK estimator, I formally test $\tau'(r_0)(P_+ - P_-) = 0$. Under the null hypothesis that either $\tau'(r_0) = 0$ or $(P_+ - P_-) = 0$, the kink estimator is valid. I estimate $\tau'(r_0)(P_+ - P_-)$ using $\tau'(r_0)(P_+ - P_-) = G'_+ - G'_- - \tau\left(P'_+ - P'_-\right)$ from equation (9), where $G'_+ - G'_-$ and $P'_+ - P'_-$ are obtained from the slope changes in the reduced-form outcome and first-stage treatment equations, respectively, and τ is estimated by the RPJK estimator. Estimating τ by the RPJK estimator is valid under the null. It is also preferred because the standard RD estimator yields largely misleading estimates of τ, as shown in the first two columns of Table 6.5.

Table 6.5 Biases in the kink estimator

	$R \in [540, 580]$		$R \in [520, 560]$	
	0.001	0.001	-0.008	-0.008
	(0.010)	(0.009)	(0.010)	(0.009)
Covariates	N	Y	N	Y

Note: Bootstrapped standard errors are based on 1,000 simulations.

Table 6.5 presents estimates of $\tau'(r_0)(P_+ - P_-)$. For both the upper and lower samples, the estimates are both numerically very small and are statistically insignificant, regardless of whether one controls for covariates or not. These results strongly suggest that, by greatly decreasing variance while adding little bias, the RPJK estimator provides a much more accurate estimate of the treatment effect than the jump estimator at the lower threshold $R = 540$, despite the possible presence of a nonzero jump there. The improved efficiency is particularly evident from the much smaller standard errors or larger t statistics for the RPK and RPJK estimators. At the lower threshold, the RPK and RPJK estimators produce estimates close to the estimates at the upper threshold. This is in sharp contrast to the implausibly large positive estimates of 7 to over 8 years produced by the standard RD estimator.

Table A1 in Appendix IV provides additional results extending the lower assignment score sample to be between 450 and 560 and the upper assignment score sample to be between 540 and 650. Again it is clear that the RPK estimator and the RPJK estimator produce estimates reasonably close to those reported in Table 6.2, while the standard RD estimator yields estimates either with an improbable negative sign or implausibly large.

These nonparametric local analyses generally confirm the global parametric results in Clark and Del Bono (2016). Their results were based on a constant treatment effect assumption. The results here support that assumption, since similar treatment effects are found at each of the two thresholds, and both local estimates are comparable to those obtained by Clark and Del Bono (2016). Taken together, the empirical estimates above strongly support the theoretical results derived earlier. Specifically, these estimates show that, in practice, one only needs to implement the RPJK estimator when the treatment probability shows a relatively large kink and a small jump.

7 Military Service, Educational Attainment, and Earnings

This section presents a new empirical application and investigates program impacts in a different setting. More than 60 countries around the world have mandatory military conscription.[18] Conscription typically either ends or interrupts an otherwise continuous educational path. Young men are usually drafted at an important age for education. For example, the official conscription age in Russia is 18. In addition, military service delays entry into the labor market, and shortens the lifetime over which one can accumulate returns to (nonmilitary) human capital investments. Both effects may depress the demand for higher education among conscripts.

Despite this expected reduction in the demand for higher education, the prevailing evidence from the US and other OECD countries is that conscription increases college education. This increase has been attributed to draft avoidance behavior (going to college to defer or avoid military service) or, in the US, veterans obtaining additional education using subsidies provided by the G.I. bill. Leading work documenting the former explanation includes Card and Lemieux (2011) and Bauer, Bender, Paloyo,

[18] A number of European countries have recently abolished it (France in 1996, Italy in 2005, Sweden in 2010, and Germany in 2011).

and Schmidt (2011). The latter is discussed in Angrist and Chen (2011) as well as Bound and Turner (2002).

Below I show that, contrary to the prevailing evidence in the US and other OECD countries, conscription in Russia causes a significance decrease in college education. Such a decrease is consistent with the special circumstances of Russia. In particular, Russia did not have institutions comparable to the G.I. bill, and alternatives to going to college existed there for avoiding conscription, so draft eligible young men do not have to go to college in order to avoid being drafted. The institutional setting in Russia resulted in what one would expect from economic theory regarding educational outcomes and earnings when education is interrupted and entry into the labor force is delayed.

In order to obtain the causal effects of serving in the Russian army, I take advantage of the fact that, due to the demilitarization process in Russia that began at the end of 1980s, there is a kinked profile in the military induction rate across birth cohorts. This has been used in Card and Yakovlev (2014). They apply a kink design to estimate the causal effects of serving in the Russian army on alcohol consumption, cigarette smoking, and related health problems. Here I look at how the compulsory military service affects educational outcomes and earnings.

Based on the institutional setting, it is not clear whether there is a jump or not in the induction rate starting from a particular cohort onward. However, a kink in the induction rate is very likely, due to demilitarization occurring gradually over time rather than all at once. Empirically it is shown that both a kink and a small jump appear to be present, so the RPJK design is what I will focus on.

Let T be a binary indicator for serving in the Russian army. The running variable R is the month-and-year when one turned 18, the official conscription age in Russia.[19] Following Card and Yakovlev (2014), the cutoff point is taken to be January 1989. So $Z = \{R \leq r_0\}$ indicates whether one turned 18 before January 1989 or not.

The analysis draws on the phase II data of the Russia Longitudinal Monitoring Survey (RLMS).[20] RLMS is a nationally-representative survey. At the time of writing, the phase II longitudinal data have been collected annually (with two exceptions, 1997 and 1999) from 1994 to 2014. The data cover about 4,000 households (about 10,000 individual respondents). Questions on military service were asked in 5 waves, in years 2005 and 2011-2014. Given the longitudinal nature of the survey, one can

[19]All month and year of birth observations are centered at the mid-month, i.e., it is assumed that individuals were born in the mid-month. This alleviates the potential bias caused by using a discrete running variable (Dong, 2015).

[20]This is the Russia Longitudinal Monitoring survey, RLMS-HSE, conducted by Higher School of Economics and ZAO "Demoscope" together with Carolina Population Center, University of North Carolina at Chapel Hill and the Institute of Sociology RAS. (RLMS-HSE sites: http://www.cpc.unc.edu/projects/rlms-hse, http://www.hse.ru/org/hse/rlms).

track down military service status for other waves. Conscription in Russia then was supposed to a 24 months of mandatory military service for all male citizens aged 18 - 27.[21] The majority of males served in the military before age 23. Here I focus on male adults aged 30-60, allowing for time to complete education. The analysis looks at completed education by 2014 (by which time the youngest cohort in the analysis are in their 30's).

Table 7.1 Summary Statistics

	Served in army			Not served in army		
	N	Mean	SD	N	Mean	SD
Turned 18 after Jan, 1989	5,153	0.326	0.469	2,116	0.630	0.483
College or above	5,141	0.179	0.384	2,107	0.427	0.495
High School	5,141	0.749	0.433	2,107	0.468	0.499
Less than HS	5,141	0.072	0.258	2,107	0.105	0.306
Earnings last month	31,73	15.05	34.17	9,377	19.78	31.92

Note: The sample consists of male adults aged 30 - 60 from RLMS 1994-2014; education is the highest education observed by the last survey (2014), while earnings are those observed in all 23 survey (1994 - 2014).

Table 7.1 presents sample summary statistics. Those who turned 18 after January 1989 have a much lower probability of serving in the army, less than 33% compared to 63% among those who turned 18 before January 1989. In addition, those who served in the army are much less likely to have a college education and much more likely to have only a high school education. Serving in the army is also associated with lower monthly earnings. However, one cannot interpret these simple correlations as causal effects. The conscription procedure in Russia ordinarily consists of a medical exam by physicians to determine a candidate's fitness for military service, and a determination by the draft board as to whether the candidate should be exempted from military service, given a deferral, or drafted. Conscripts are officially positively selected on health status; however, men from disadvantageous backgrounds are more likely to be enlisted, due to the prevalence of draft avoidance behavior of the rich.[22]

[21] The conscription periods each year are from October 1 through December 31 and from April 1 through June 30.

[22] For example, men from wealthy families can pay bribes to members of draft boards or doctors to avoid being drafted. See Lokshin and Yemtsov (2008) for discussion of draft avoidance behavior in Russia.

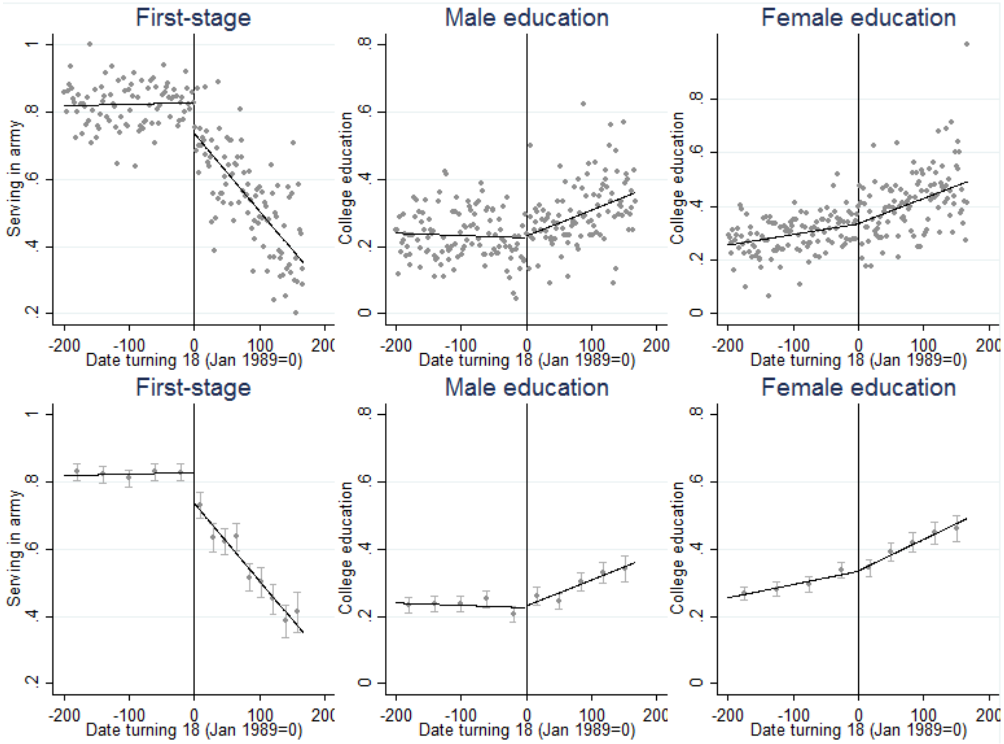


Figure 7.1 (a) Cohort profiles of college education (200 months bandwidth)

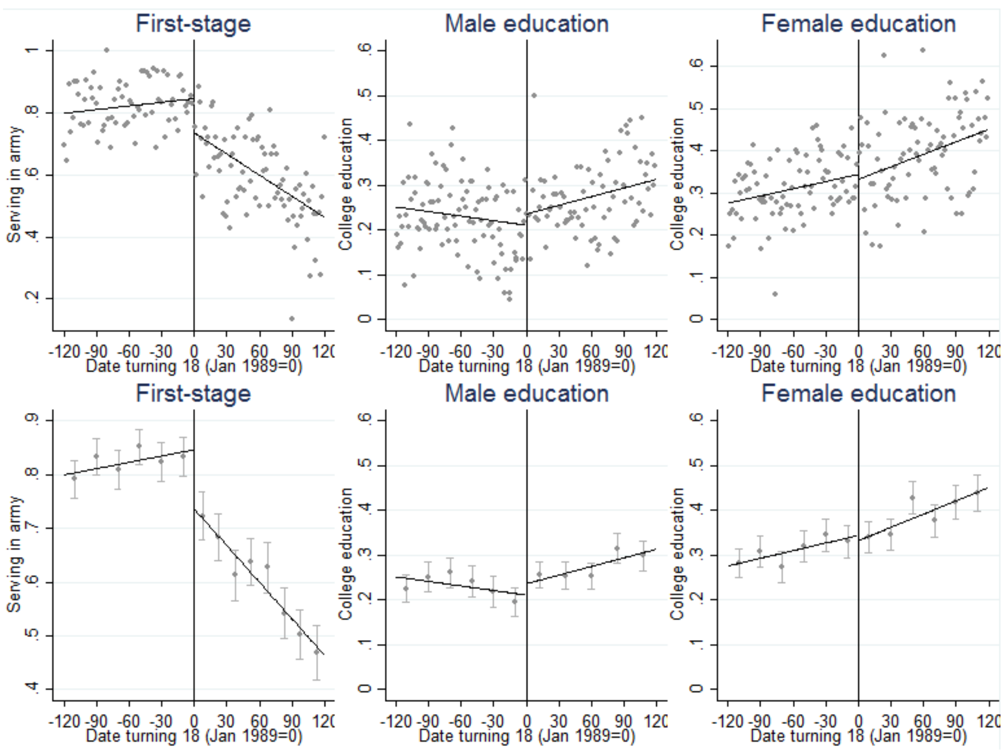


Figure 7.1 (b) Cohort profiles of college education (120 months bandwidth)

The top row of Figure 7.1(a) presents the cohort (month-year turning 18) profiles of serving in the army and college education for males and females, separately. The bottom row shows the same profiles but wider bins are used for cell means (so there are fewer dots) to facilitate showing the curvature of the raw data along with 90% confidence intervals. The figures clearly show that the share of males serving in the army has a dramatic decline (a slope change) just to the right of the cutoff (normalized to be 0). One can also see a possible small discontinuity at the cutoff. Correspondingly, college education among males shows a visible slope change in the opposite direction to that in the first-stage figure. There also appears to be a less noticeable discontinuity at the cutoff (still in the opposite direction). In contrast, college education among females shows only a slight slope change. Moreover, when looking closer to the cutoff (i.e., using a smaller bandwidth), the slope change among females starts to disappear.

Figure 7.1(b) presents the same plots as those in Figure 7.1(a), but uses a smaller bandwidth (120 months instead of 200 months on either side of the cutoff). There is little if any slope change for female college education around the cutoff. In theory, a small kink for both men and women is possible due to macro changes occurring at the time.[23] I therefore use females as a control group in the main analysis.This follows Card and Lemieux (2011), who use trends in the schooling of men relative to those of women to measure draft effects. This strategy was also adopted by Card and Yakovlev (2014) to measure the draft effects on alcohol consumption and cigarette smoking.

Table 7.2 Smoothness Tests

	R=[-200, 199]		R=[-120,119]	
	Density of running var.			
Jump	-5.269	(3.628)	-4.650	(4.230)
Kink	-0.139	(0.094)	0.084	(0.177)
	Covariates			
Father college education	0.087	(0.073)	-0.004	(0.109)
Mother college education	0.128	(0.086)	0.043	(0.140)

Note: Estimates are based on RLMS 1994-2014; the density tests use a regression of the frequency of observations at each value of the running variable on Z, $Z(R-r_0)$, $(R-r_0)$, and $(R-r_0)^2$, where standard errors are clustered at each value of the running variable; Covariate tests use the same RPJK specification as the main specification for college education, controlling for region fixed effects and region center fixed effects; Robust standard errors are clustered at gender by month-year of birth level.

I first verify the smoothness conditions required for the RPJK estimator. Figure 7.2 provides visual evidence showing smoothness of the empirical density of birth cohort (date turning 18). Table 7.2 presents formal test results. No significant jumps and kinks are found in the empirical density of birth

[23]For example, after the collapse of the Soviet Union, the number of higher education institutions in Russia increased due to the emergence of private institutions, as well as greater enrollment of tuition paying students in public universities.

cohort. Falsification tests on the two directly relevant covariates for the outcomes of interest, mother's and father's college education show insignificant treatment effects.[24] Monotonicity is discussed after presenting the estimated characteristics of compliers.

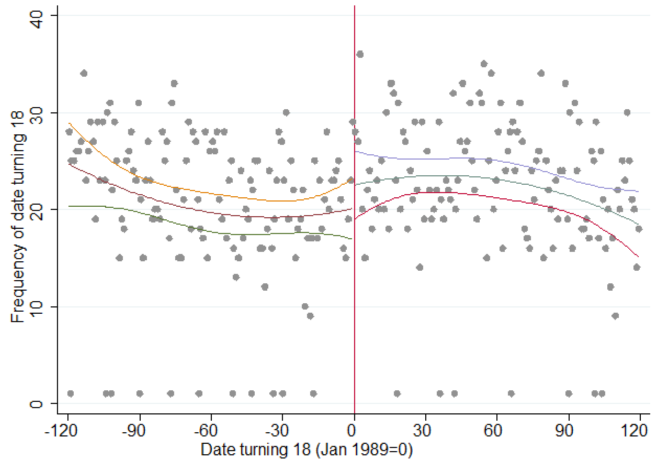


Figure 7.2 Frequency of birth cohort, local polynomial fit, and 95% CI

Who no longer joined the Russian Army due to the advent of demilitarization? Table 7.3 presents the estimated average complier or marginal complier characteristics, including family size, mother having a college education, father having a college education, living in a regional center, and living in Moscow or St. Petersburg. Note that these two cities had much lower draft quotas than other places (Lokshin and Yemtsov, 2008). Compliers in this case are those who would serve in the military if and only if they were born before demilitarization but would not otherwise. These mean characteristics are estimated by the approach discussed in Section 5. For comparison purposes, also reported are the average characteristics of never takers and always takers.[25] Never takers broadly consist of two distinct types, 1) those who were ineligible (due to, e.g., real disability) even before the demilitarization, and 2) those who would avoid conscription regardless. For example, it is well known that those who live in larger cities have better access to information and greater career opportunities and hence a higher chance of avoiding being drafted. There is also evidence that those who are from smaller families are more likely to avoid the draft (Lokshin and Yemtsov, 2008).

Indeed, estimates in Table 7.3 show that never takers are more likely to live in a regional center, to

[24] College education is coded as 1 if a father or mother has any education from a technical community college, medical institute, university, academy, or taking a post-graduate course, and 0 otherwise.

[25] The average characteristics of never takers are identified as $E\left[X|N, R=r_0\right] = \frac{\lim_{r\downarrow r_0} E[XT|R=r]}{\lim_{r\downarrow r_0} E[TR=r]}$ and those of always takers are identified as $E\left[X|A, R=r_0\right] = \frac{\lim_{r\uparrow r_0} E[X(1-T)T|R=r]}{\lim_{r\uparrow r_0} E[1-T|R=r]}$.

Table 7.3 Complier, never taker, and always taker characteristics

	Region center	Moscow/St Peters-burg	Family size	Father college edu.	Mother college edu.
Complier	0.378	0.106	4.305	0.165	0.248
	(0.009)***	(0.005)***	(0.028)***	(0.006)***	(0.008)***
Never taker	0.499	0.171	4.002	0.245	0.318
	(0.041)***	(0.030)***	(0.129)***	(0.035)***	(0.036)***
Always taker	0.400	0.112	4.446	0.228	0.333
	(0.015)***	(0.009)***	(0.053)***	(0.012)***	(0.014)***

Note: Estimates are based on RLMS 1994-2014; Moscow means whether the respondent lives in Moscow city or St. Petersburg; Robust standard errors are clustered at year-month of birth level; ***Significant at the 1% level.

be from Moscow or St. Petersburg, and to come from smaller families. Note that selection on parental education for never takers can go in either direction, depending on the specific reasons they did not serve in the military. In contrast, compliers are slightly less likely to live in a regional center or live in Moscow or St. Petersburg. They on average are from slightly larger families and are less likely to have college educated parents. Overall it looks like those who no longer joined the Russian Army thanks to the start of demilitarization are those from average family backgrounds, having mean characteristics comparable to the sample means of those who served in the military.

These estimated complier characteristics all carry the plausible positive signs; the estimated probabilities are well between 0 and 1. Formal one-sided tests fail to reject that the mean characteristics are positive or the probabilities are between 0 and 1. Therefore, monotonicity is not rejected.

Table 7.4 reports the 2SLS estimates of the impact of military service on college education. Also reported are the estimated jump and kink in the first-stage. Considering that macro changes other than demilitarization or simply specification errors may lead to kinks in the probability of college education, I use females as a control group. The underlying assumption is that any macro changes or specification errors would lead to the same trend changes for males and females.[26]

The top panel of Table 7.4 reports estimates using a bandwidth of 200 months on either side of the cutoff (corresponding to Figure 7.1(a)) and the bottom panel reports estimates using a smaller bandwidth of 120 months (corresponding to Figure 7.1(b)). As expected, estimates in the top panel are

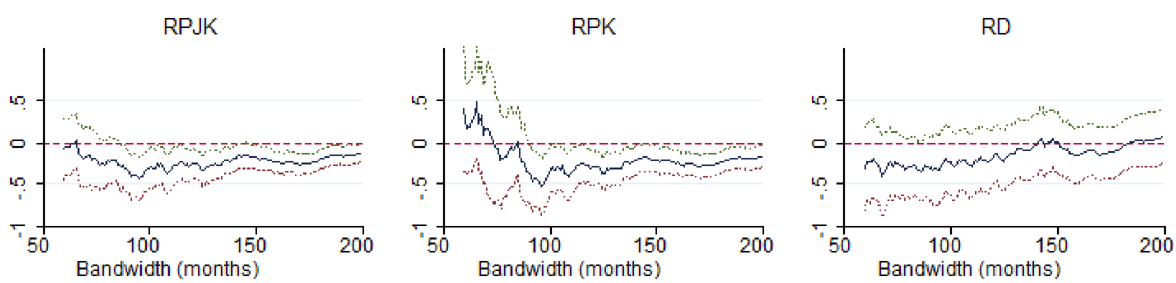
[26] In particular, I estimate a regression of college education Y on T, $(R-r_0)$, Z, $Z(R-r_0)$, $male$, and $male \times (R-r_0)$, using either the jump $male \times Z$ or the kink $male \times Z \times (R-r_0)$, or both in the male probability of serving in the army, as the excluded IVs for T. Essentially, the jump and kink in the numerator of the RPJK estimator are the male-female differences.

Table 7.4 Impacts of serving in the army on college education

First-stage		2SLS estimates		
Jump	Kink	RD	RPK	RPJK
		R=[-200,199]		
0.094	0.002	0.045	-0.184	-0.145
(0.022)***	(0.000)***	(0.202)	(0.081)**	(0.070)**
		R=[-120,119]		
0.114	0.003	-0.180	-0.281	-0.256
(0.026)***	(0.000)***	(0.201)	(0.126)**	(0.111)**

Note: Estimates are based on RLMS 1994-2014; All estimates use females as a control group, and additionally control for region fixed effects for 39 regions, regional center fixed effect, and father's education and mother's education in 12 categories each; Robust standard errors are clustered at gender by month-year of birth level; **Significant at the 5% level; ***Significant at the 1% level.

in general more precise due to a larger sample size, and hence are what I will focus on in the following discussion. Both the estimated jump and kink in the first-stage are statistically significant. However, the jump appears to be too small to yield precise estimates of the effects of military service. In contrast, the RPJK design yields significant negative impacts. In the top row, serving in the army is shown to reduce a male adult's probability of obtaining a college education by about 14.5%. The RPK design yields similar negative effects. Estimates using only males are provided in Table A3 in Appendix V. Using only the male sample generally overestimates the impacts of serving in the army.



Note: RLMS 1994 - 2014; Local linear estimates for college education along with 90% CI (male-female).

Figure 7.3 Estimated impacts on college education with different bandwidths

Figure 7.3 further plots estimates over a large range of bandwidths along with the 90% confidence intervals. The RPJK and RPK designs yield statistically significant negative estimates that are stable over a large range of bandwidths (though, not surprisingly, estimates become insignificant when very small bandwidths are used). In contrast, the standard RD design generates estimates that are never significant.

To corroborate the estimated significant negative impacts on college education, I conduct a falsification analysis. Serving in the army interferes with the transition from high school to college, but should rarely if ever interfere with the opportunity of obtaining a less than high-school education. Table 7.5 presents results for less than high school education, using the same specifications as those used for college education. The estimated jump and kink in the first-stage are the same, while the estimated impacts on less than high education are small and positive yet insignificant in all cases.

Table 7.5 Impacts of serving in the army on less than high school education

First-stage		2SLS estimates		
Jump	Kink	RD	RPK	RPJK
		R=[-200,199]		
0.094	0.002	0.037	0.044	0.043
(0.022)***	(0.000)***	(0.140)	(0.059)	(0.050)
		R=[-120,119]		
0.114	0.003	0.053	0.097	0.037
(0.026)***	(0.000)***	(0.140)	(0.085)	(0.140)

Note: Estimates are based on RLMS 1994-2014; All estimates use females as a control group, and additionally control for region fixed effects, regional center fixed effect, and father's education and mother's education dummies; Robust standard errors are clustered at gender by month-year of birth level; ***Significant at the 1% level.

Unlike some evidence from the US and other OECD countries, the above analysis shows that serving in the army significantly decreases one's probability of obtaining a college degree in Russia. This is what one would expect from economic theory when education is interrupted and entry into the labor force is delayed. In particular, Russia does not provide financial support for higher education to returning conscripts. Also, attending college may not be the primary means of avoiding being drafted in Russia, since illegitimate (arguably less expensive) alternatives are prevalent.[27,28] Assume that at least some eligible draftees avoid being drafted through attending college. Then the estimates here would be underestimates for the true interruption impact of compulsory military service on higher education.

[27] According to the General Staff of the Armed Forces, every year approximately 30,000 young men dodge the draft. As a result, Less than 10 percent of eligible males were actually enlisted in the army in the early 2000s.

[28] Spivak and Pridemore (2014) note that of the three primary grounds upon which conscription can be exempted, educational deferment, certain social hardships, and medical disqualification, the latter is the most commonly exploited. In addition, simply evading the draft is prevalent.

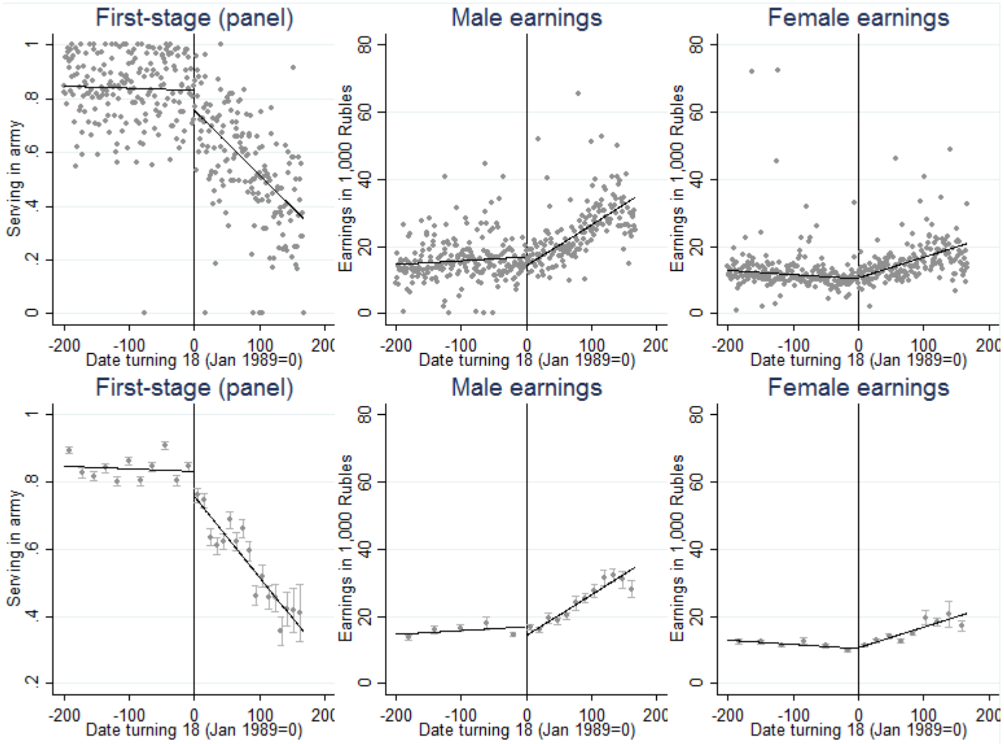


Figure 7.4 (a) Cohort profiles of earnings (200 months bandwidth)

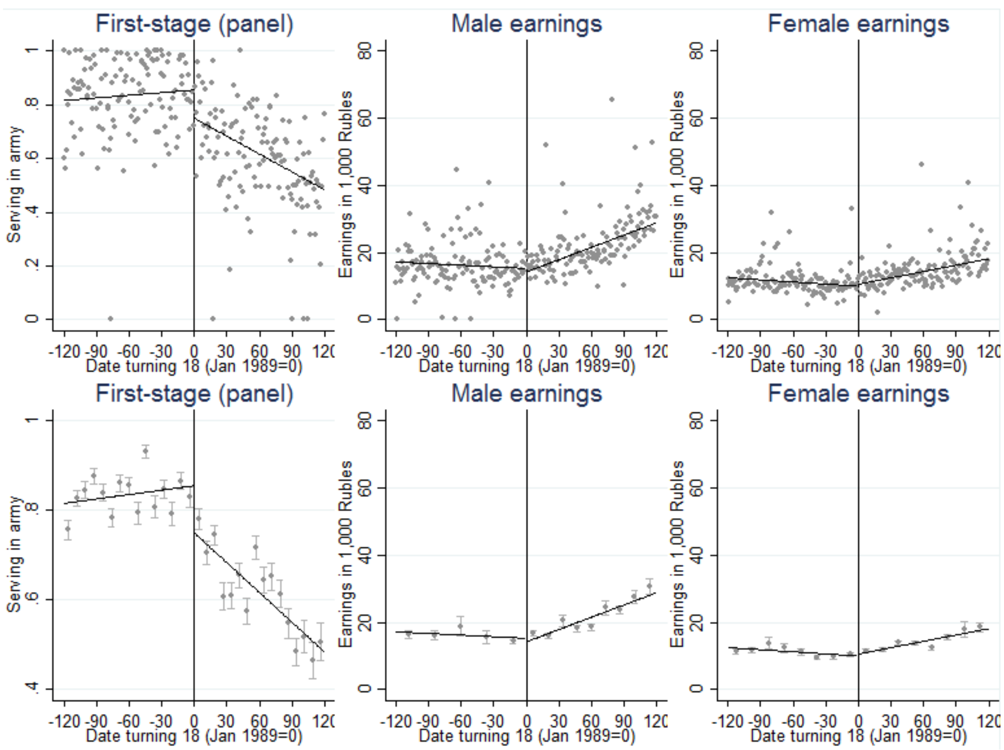


Figure 7.4 (b) Cohort profiles of earnings (120 months bandwidth)

I next investigate whether conscription leads to lower earnings, given its negative impacts on higher education. Figure 7.4 (a) presents the birth cohort profiles of monthly earnings (total income in the last

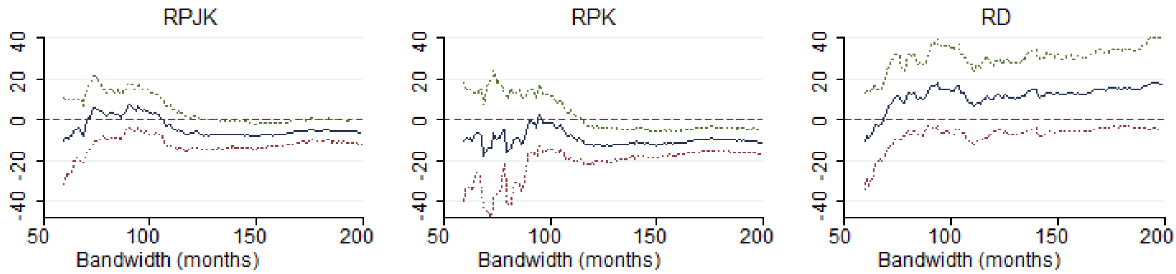
30 days) for males and females, respectively. All earnings are in 2014 constant thousands of Rubles. Unlike completed education, earnings are not time invariant. Longitudinal data are then used for these figures as well as for the estimation. The first-stage figure now using the longitudinal data similarly shows a dramatic slope change and a small jump in the probability of serving in the Russian army in January 1989.

Table 7.6 Impacts of serving in the army on earnings

First-stage		2SLS estimates		
Jump	Kink	RD	RPK	RPJK
		R=[-200,199]		
0.072	0.002	17.37	-10.91	-6.117
(0.026)***	(0.000)***	(13.54)	(3.718)***	(3.389)*
		R=[-120,119]		
0.107	0.003	10.08	-12.06	-6.126
(0.031)***	(0.000)***	(98.46)	(5.444)**	(4.596)

Note: Estimates are based on RLMS 2000-2014; All estimates use females as a control group, and additionally control for region fixed effects, regional center fixed effect, and father's education and mother's education dummies; Robust standard errors are clustered at gender by month-year of birth level; * Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 7.6 presents results for earnings by the RD, RPK, and RPJK designs. These results are produced by the same specifications as those for college education, except that due to the longitudinal nature, I additionally control for year fixed effects as well as age and age squared. The standard RD design yields positive and insignificant estimates. In contrast, the RPJK design yields a significant estimate of -6.117 thousands of Rubles (about 95 USD, or roughly a 30 percent decrease in reference to the mean earnings of those who do not serve in the army). The RPK design with the 200 months bandwidth yields a negative effect of -10.91 thousands of Rubles (about 170 USD). Using the smaller bandwidth leads to similar but less precise estimates. Only data from 2000 or later are used for estimates in Table 7.5, because the labor market in the 1990's in Russia went though many changes, including the collapse of the Soviet Union. For comparison purposes, estimates using all years' data from 1994 to 2000 are provided in Table A4 in Appendix V. Interestingly, the estimates are largely similar regardless of whether earlier years' data are included or not, though precision is lower when the noisier earlier years' data are included.



Note: RLMS 2000 - 2014; Local linear estimates for earnings along with 90% CI (male-female); Earnings in 1,000 2014 Rubles.

Figure 7.5 Estimated impacts on college education with different bandwidths

Figure 7.5 further presents estimates with 90% confidence intervals over a wide range of bandwidths. The standard RD design yields estimates that are never significant and mostly positive. In contrast, the RPJK and RPK designs yield negative and significant estimates for a large range of bandwidths. Overall these estimates point to negative effects of serving in the Russian army on earnings. Estimates leaving out females as controls are provided in Table A5 in Appendix V. The conclusions are similar except that the estimates are more sensitive to bandwidth choice.

Existing studies using the US or other OECD countries data show mixed evidence of conscription on earnings, from positive, zero to negative impacts. These differences could be because conscription experience varies greatly across countries. This variation is emphasized by Hjalmarsson and Lindquist (2016). In countries like the US and OECD, where most studies find positive effects of conscription on education, the resulting impacts on earnings could be offset by negative effects such as delayed entry into the labor force. In contrast, I find large negative effects of conscription on earnings in Russia, likely because military service in Russia reduces education, and so compounds rather than offsets negative effects such as delayed entry into the labor force. Additional sources of negative effects on earnings could be due to findings by Card and Yakovlev (2014), who show that compulsory military service in Russia leads to significant increases in alcohol consumption and cigarette smoking as well as related chronic health problems among conscripts. One caveat regarding these results is that, unlike completed education, gender differences in earnings might be more sensitive to labor market changes and hence it is not clear that women provide as good a control group as the case of college education.

In situations where a significant jump in the conscription probability across birth cohorts exists, e.g., when compulsory military service is either introduced or abolished, one may apply the standard RD design (see, e.g., Bauer, Bender, Paloyo, and Schmidt 2011 and references therein). However, when one is unsure if jumps are present or large enough to strongly identify treatment effects, as in the case of Russia, the above results illustrate the advantage of the RPJK design over the standard RD design.

8 Conclusion

This paper evaluates the impacts of participation in two social programs, elite school attendance in the UK and supposedly compulsory military service in Russia. In both cases, the probability of participation has large kinks but either a small jump or no jump. As a result, evaluating the program impacts is largely impossible using the standard RD estimator. This paper extends the standard RD design with a binary treatment to allow for causal identification in these or similar scenarios. In particular, I discuss identifying causal parameters using a kink, instead of or in addition to a jump, in the treatment probability at a policy threshold.

Kinks in the treatment probability can frequently arise, as when the assignment of treatment depends on how far one is from the cutoff. Kinks can also arise due to kinked benefit or cost schedules that affect individuals treatment decisions. Similar to the standard RD design, identification based on kinks is valid under smoothness conditions. These smoothness conditions are plausible when individuals cannot sort around the kink point by precisely manipulating the running variable. To empirically check validity of these smoothness conditions, one can test smoothness (i.e., no jumps and kinks) of the empirical density of the running variable, or the conditional means of pre-determined covariates.

This paper focuses on identification. Robust nonparametric inference for the resulting RPK estimator is discussed in Calonico, Cattaneo, and Titiunik (2014). In addition, Chiang, Hsu, and Sasaki (2017) discuss bootstrap validity a class of discontinuity based estimators that includes the RPK estimator.

In practice, treatment probabilities may show both a jump and a kink at the RD threshold. This paper then discusses a general RPJK design that is valid regardless of whether there is either a jump, a kink or both. The corresponding local 2SLS estimator asymptotically reduces to the standard RD estimator when a jump is present in the treatment probability, and otherwise reduces to the newly proposed PRK estimator if there is no jump. Higher order nonsmoothness, like a second-order derivative change, could also be exploited using methods similar to the RPK and RPJK designs.

Additional identification results, such as identifying complier or marginal complier characteristics in the RD, RPK, and RPJK designs, are also discussed. Identifying complier or marginal complier characteristics provides a convenient way of checking for monotonicity in empirical applications.

In sharp contrast to the results of the standard RD design, the proposed RPK or RPJK designs yield plausible estimates of causal impacts of the two social programs under consideration. Attending an elite school in the UK is estimated to increase education by 1.3-1.5 years for men. Serving in the Russian army decreases the probability of obtaining a college education by 14.5 percent among men.

It is worth mentioning that this paper discusses identification in a static model. This is similar to the standard RD design. When the running variable is time, kinks in outcomes may result from dynamic effects, such as variable delays in response to treatment. Extending the results in this paper to a dynamic context would be an interesting direction for future research.

9 Appendix I: Proofs

PROOF of THEOREM 1: The conclusion follows by the derivation in the text.

PROOF of THEOREM 2: For $r \geq r_0$,

$$\begin{aligned} G(r) &\equiv E\left[Y_0 + (Y_1 - Y_0)T|R = r\right] = E\left[Y_0 + (Y_1 - Y_0)T_1|R = r\right] \\ &= E\left[Y_0|R = r, T_1 = 0\right]\Pr\left(T_1 = 0|R = r\right) + E\left[Y_1|R = r, T_1 = 1\right]\Pr\left(T_1 = 1|R = r\right) \\ &= E\left[Y_0|R = r, N\right]\Pr\left(N|R = r\right) + E\left[Y_1|R = r, A\right]\Pr\left(A|R = r\right) + E\left[Y_1|R = r, C\right]\Pr\left(C|R = r\right), \end{aligned}$$

where the first equality follows from the fact that for $r \geq r_0$, $T = T_1$, and the last equality follows from the monotonicity assumption B1.

Similarly for $r < r_0$,

$$\begin{aligned} G(r) &\equiv E\left[Y_0 + (Y_1 - Y_0)T|R = r\right] = E\left[Y_0 + (Y_1 - Y_0)T_0|R = r\right] \\ &= E\left[Y_0|R = r, T_0 = 0\right]\Pr\left(T_0 = 0|R = r\right) + E\left[Y_1|R = r, T_0 = 1\right]\Pr\left(T_0 = 1|R = r\right) \\ &= E\left[Y_0|R = r, N\right]\Pr\left(N|R = r\right) + E\left[Y_1|R = r, A\right]\Pr\left(A|R = r\right) + E\left[Y_0|R = r, C\right]\Pr\left(C|R = r\right). \end{aligned}$$

Under B2, the probabilities of types and type-specific conditional means of potential outcomes are continuously differentiable. Therefore, any terms for always takers and never takers will be differenced out in the following derivation. Then

$$\begin{aligned} G_+ - G_- &= E\left[Y_1|R=r_0, C\right]\Pr\left(C|R=r\right) - E\left[Y_0|R=r_0, C\right]\Pr\left(C|R=r_0\right) \\ &= E\left[Y_1 - Y_0|R=r_0, C\right]\Pr\left(C|R=r_0\right) \\ &= \tau\left(r_0\right)\Pr\left(C|R=r_0\right), \end{aligned}$$

where $\tau\left(r_0\right) \equiv E\left[Y_1 - Y_0|R=r_0, C\right]$. The equivalent definition in Section 2.1 is $\tau\left(r_0\right) = \tau_{LATE}\left(P_+, P_-, r_0\right) \equiv E\left[Y_1 - Y_0|R=r, P_- < U \le P_+\right]$.

Similarly,

$$P_+ - P_- = \lim_{r\downarrow r_0} E\left[T|R=r\right] - \lim_{r\uparrow r_0} E\left[T|R=r\right] = \Pr\left[C|R=r_0\right].$$

Further,

$$\begin{aligned} &G'_+ - G'_- = \lim_{r\downarrow r_0}\left\{\tfrac{\partial}{\partial r}E\left[Y_1|R=r, C\left(r\right)\right]\Pr\left(C|R=r\right) + E\left[Y_1|R=r, C\left(r\right)\right]\tfrac{\partial}{\partial r}\Pr\left(C\left(r\right)|R=r\right)\right\} \\ &-\lim_{r\uparrow r_0}\left\{\tfrac{\partial}{\partial r}E\left[Y_0|R=r, C\left(r\right)\right]\Pr\left(C\left(r\right)|R=r\right) - E\left[Y_0|R=r_0, C\left(r\right)\right]\tfrac{\partial}{\partial r}\Pr\left(C\left(r\right)|R=r\right)\right\} \\ &= \tfrac{\partial}{\partial r}E\left[Y_1 - Y_0|R=r, C\left(r\right)\right]|_{r_0}\Pr\left(C\left(r\right)|R=r_0\right) + E\left[Y_1 - Y_0|R=r_0, C\left(r_0\right)\right]\tfrac{\partial}{\partial r}\Pr\left(C\left(r\right)|R=r\right)|_{r_0}. \end{aligned}$$

It follows that

$$G'_+ - G'_- = \tau'\left(r_0\right)\Pr\left(C\left(r\right)|R=r_0\right) + \tau\left(r_0\right)\frac{\partial}{\partial r}\Pr\left(C\left(r\right)|R=r\right)|_{r_0},$$

where $\tau'\left(r_0\right) \equiv \frac{\partial}{\partial r}E\left[Y_1 - Y_0|R=r, C\left(r\right)\right]|_{r_0}$. The equivalent definition in Section 2.1 is $\tau'\left(r_0\right) \equiv \frac{\partial}{\partial r}\tau\left(r\right)|_{r_0} \equiv \frac{\partial}{\partial r}E\left[Y_1 - Y_0|R=r, P_0\left(r\right) < U \le P_1\left(r\right)\right]|_{r_0}$. Similarly,

$$P'_+ - P'_- = \frac{\partial}{\partial r}\Pr\left(C\left(r\right)|R=r\right)|_{r_0}.$$

Since $\Pr\left(C\left(r\right)|R=r_0\right) = P_+ - P_-$, then

$$G'_+ - G'_- = \tau'\left(r_0\right)\left(P_+ - P_-\right) + \tau\left(r_0\right)\left(P'_+ - P'_-\right). \tag{18}$$

When the jump $P_+ - P_-$ goes arbitrarily to 0, $\tau\left(r_0\right) = \tau_{LATE}\left(P_+, P_-, r_0\right)$ goes arbitrarily to $\tau_{MTE}\left(P\left(r_0\right)\right)$. Further by the above equation (18), when there is a kink, $P'_+ - P'_- \neq 0$, $\tau_{MTE}\left(P\left(r_0\right)\right)$

is identified by $\tau_{MTE}(P(r_0)) = \left(G'_+ - G'_-\right)/\left(P'_+ - P'_-\right)$.

PROOF of THEOREM 3:

When there is a jump, ω_n goes to zero as $n \to \infty$, and so equation (10) reduces to $\tau_{LATE}(P_+, P_-, r_0) = (G_+ - G_-)/(P_+ - P_-)$, i.e., the standard RD estimand. When there is no jump, by assumption there is a kink, then equation (10) reduces to $\tau_{LATE}(P_+, P_-, r_0) = \tau_{MTE}(P(r_0)) = \left(G'_+ - G'_-\right)/\left(P'_+ - P'_-\right)$, the PRK estimand, which is valid by Theorem 1 or 2.

PROOF of COROLLARIES 1a and 1b:

By equation (2), $\tau_{LATE}(P_+, P_-, r_0) = (G_+ - G_-)/(P_+ - P_-)$. By equation (9), $G'_+ - G'_- = \tau'(r_0)(P_+ - P_-) + \tau(r_0)\left(P'_+ - P'_-\right)$, where recall $\tau(r_0) \equiv \tau_{LATE}(P_+, P_-, r_0)$. Then when $P_+ - P_- \neq 0$, if and only if $\tau'(r_0) = 0$, $G'_+ - G'_- = \tau(r_0)\left(P'_+ - P'_-\right)$ and hence $\tau(r_0) = \left(G'_+ - G'_-\right)/\left(P'_+ - P'_-\right)$. That is, when $\tau'(r_0) = 0$, a jump and a kink identify the same parameter. This proves Corollary 1a

$$\tau_{LATE}(P_+, P_-, r_0) = \frac{G_+ - G_-}{P_+ - P_-} = \frac{G'_+ - G'_-}{P'_+ - P'_-}.$$

It follows that $(G_+ - G_-) = (P_+ - P_-)\,\tau_{LATE}(P_+, P_-, r_0)$ and $\left(G'_+ - G'_-\right) = \left(P'_+ - P'_-\right)\tau_{LATE}(P_+, P_-, r_0)$. One can then take a linear combination of the jump and kink on both sides: $G_+ - G_- + \omega\left(G'_+ - G'_-\right) = \left((P_+ - P_-) + \omega\left(P'_+ - P'_-\right)\right)\tau_{LATE}(P_+, P_-, r_0)$ for any weights $\omega \neq -(P_+ - P_-)/\left(P'_+ - P'_-\right)$. Then

$$\tau_{LATE}(P_+, P_-, r_0) = \frac{G_+ - G_- + \omega\left(G'_+ - G'_-\right)}{P_+ - P_- + \omega\left(P'_+ - P'_-\right)}.$$

for $\omega \neq -(P_+ - P_-)/\left(P'_+ - P'_-\right)$. The above holds when either $P_+ - P_- \neq 0$, or $P'_+ - P'_- \neq 0$, or both, which proves Corollary 1b.

10 Appendix II: Identifying Other Parameters of Interest

Define $\mathcal{G}_t(r) \equiv E[1(T = t)Y|R = r]$ for $t = 0, 1$. Follow the notational convention to let $\mathcal{G}_{t+}$ and $\mathcal{G}_{t-}$ be the right and left limits and $\mathcal{G}'_{t+}$and $\mathcal{G}'_{t-}$ be the right and left derivatives at $r = r_0$, respectively, whenever they exist.

Corollary 2 *Assume that A1 holds and that A2 hold holds after replacing* $E[Y_t|U = u, R = r]$ *with* $F_{Y_t|U,R}(y|u, r)$, $t = 0, 1$. *1) If* $P_+ \neq P_-$, *then* $E\left[Y_t|P_- < U \leq P_+, R = r_0\right] = \frac{\mathcal{G}_{t+} - \mathcal{G}_{t-}}{P_+ - P_-}$; *2) if* $P_+ =$

P_- and $P'_+ \neq -P'_-$, then $E\left[Y_t|U=P\left(r_0\right), R=r_0\right] = \frac{\mathcal{G}'_{t+}-\mathcal{G}'_{t-}}{P'_+-P'_-}$; 3) Assume A3 holds. If $P_+ = P_-$ and $P'_+ = P'_-$ do not both hold, then $E\left[Y_t|P_- < U \leq P_+, R=r_0\right] = \lim_{n\to\infty} \frac{(\mathcal{G}_{t+}-\mathcal{G}_{t-})+\varpi_n(\mathcal{G}'_{t+}-\mathcal{G}'_{t-})}{P_+-P_-+\varpi_n(P_+-P_-)}$, where $\varpi_n \to 0$ as $n\to\infty$ and ϖ_n can be the local 2SLS weights discussed in Section 4. Further replacing Y with $1\left(Y\leq y\right)$ in the definiton of $\mathcal{G}_t\left(r\right)$, one can identify $F_{Y_t|P_-<U\leq P_+,R=r_0}(y)$.

PROOF OF COROLLARY 2: Part 1) is the standard RD design estimand. This result can be proved by following, e.g., the identification results of Frandsen, Frölich, and Melly (2012). Part 3) can be readily proved by following the arguments in Theorem 3 and the discussion in Section 3. Below I provide a quick proof of part 2).

For $t=1$, by definition

$$\begin{aligned}
\mathcal{G}'_{1+}-\mathcal{G}'_{1-} &= \lim_{r\downarrow r_0}\frac{\partial}{\partial r}E\left[TY|R=r\right]-\lim_{r\uparrow r_0}\frac{\partial}{\partial r}E\left[TY|R=r\right] \\
&= \lim_{r\downarrow r_0}\frac{\partial}{\partial r}E\left[TY_1|R=r\right]-\lim_{r\uparrow r_0}\frac{\partial}{\partial r}E\left[TY_1|R=r\right] \\
&= \lim_{r\downarrow r_0}\frac{\partial}{\partial r}E\left[1\left(U\leq P_1\left(r\right)\right)Y_1|R=r\right]-\lim_{r\uparrow r_0}\frac{\partial}{\partial r}E\left[1\left(U\leq P_0\left(r\right)\right)Y_1|R=r\right] \\
&= \lim_{r\downarrow r_0}\frac{\partial}{\partial r}\int_0^{P_1(r)}E\left[Y_1|R=r,U=u\right]du-\lim_{r\uparrow r_0}\frac{\partial}{\partial r}\int_0^{P_0(r)}E\left[Y_1|R=r,U=u\right]du \\
&= P'_+E\left[Y_1|R=r_0,U=P_+\right]-P'_-E\left[Y_1|R=r_0,U=P_-\right]+\int_{P-}^{P+}E\left[Y_1|R=r,U=u\right]du \\
&= \left(P'_+-P'_-\right)E\left[Y_1|R=r_0,U=P\left(r_0\right)\right]
\end{aligned}$$

where the first to third equalities follow from the definition of $\mathcal{G}_t\left(r\right)$ for $t=0$ and the equalities $Y=Y_1T+Y_0\left(1-T\right)$ and $T=T_1Z+T_0\left(1-Z\right)$ for $Z=1\left(r\geq r_0\right)$, and the last equality follows from $P_+-P_-=0$. By Theorem 1 and its proof, $\lim_{r\downarrow r_0}\frac{\partial}{\partial r}E\left[T|R=r\right]-\lim_{r\uparrow r_0}\frac{\partial}{\partial r}E\left[T|R=r\right]=P'_+-P'_-$. It follows that

$$\begin{aligned}
\frac{\mathcal{G}'_{1+}-\mathcal{G}'_{1-}}{P'_+-P'_-} &= \frac{\lim_{r\downarrow r_0}\frac{\partial}{\partial r}E\left[TY|R=r\right]-\lim_{r\uparrow r_0}\frac{\partial}{\partial r}E\left[TY|R=r\right]}{\lim_{r\downarrow r_0}\frac{\partial}{\partial r}E\left[T|R=r\right]-\lim_{r\uparrow r_0}\frac{\partial}{\partial r}E\left[T|R=r\right]} \\
&= E\left[Y_1|R=r_0,U=P\left(r_0\right)\right].
\end{aligned}$$

The case where $t=0$ can be analogously proved.

Corollary 3 *Assume that A1 holds and that A2 holds after replacing Y_t, $t=0,1$ with* some predetermined covariate *X. 1) If $P_+-P_-\neq 0$, then $E\left[X|P_-<U\leq P_+, R=r_0\right] = \frac{\mathbb{X}_+-\mathbb{X}_-}{P_+-P_-}$; 2)*

If $P_+ - P_- = 0$ *and* $P'_+ - P'_- \neq 0$*, then* $E[X|U = P(r_0), R = r_0] = \frac{\mathbb{X}'_+ - \mathbb{X}'_-}{P'_+ - P'_-}$*; 3) Assume A3 holds. If* $P_+ - P_- = 0$ *and* $P'_+ - P'_- = 0$ *do not both hold, then* $E\left[X|P_- < U \leq P_+, R = r_0\right] = \lim_{n\to\infty} \frac{(\mathbb{X}_+ - \mathbb{X}_-) + \kappa_n (\mathbb{X}'_+ - \mathbb{X}'_-)}{(P_+ - P_-) + \kappa_n (P'_+ - P'_-)}$*, where* $\kappa_n \to 0$ *as* $n \to \infty$ and κ_n *can be the local 2SLS weights discussed in Section 4. Further replacing* X *with* $1(X \leq x)$ *in the definiton of* $\mathbb{X}(r)$*, one can identify* $F_{X|P_- < U \leq P_+, R = r_0}(x)$*.*

PROOF OF COROLLARY 3: Recall $\mathbb{X}(r) \equiv E[XT|R = r]$. This corollary can be proved analogously to the proof of Corollary 2 by replacing $1(T = 1)Y$ with XT or $1(T = 1)1(Y \leq y)$ with $1(X \leq x)T$ and noticing that X does not change with $T = t$, $t = 0, 1$.

11 Appendix III: Identification Using Higher-order Derivative Changes

The estimand in Corollary 1b requires $\tau'(r_0) = 0$ to use both a jump and a kink for identification. As mentioned, having $\tau'(r_0) = 0$ means that the treatment effect does not vary linearly with the running variable R.

The following Corollary 4 provides extensions of Corollary 1b to allow $\tau'(r_0) \neq 0$ while still exploiting information in a kink in addition to a jump. For example, if the treatment is attending an elite school, the running variable is test score, and the outcome is educational attainment, then $\tau'(r_0) \neq 0$ would mean that the effect of attending an elite school on educational attainment depends on the pre-treatment test score, and in this case one still could use both jump and kink information to estimate the treatment effect.

For any function $H(r)$, let H''_+ and H''_- be the right and left limits at $r = r_0$ of its second derivative, whenever they exist. For convenience of notation, formally define $\gamma_1 \equiv G_+ - G_-$, $\gamma_2 \equiv G'_+ - G'_-$, $\gamma_3 \equiv G''_+ - G''_-$, $\beta_1 \equiv P_+ - P_-$, $\beta_2 \equiv P'_+ - P'_-$ and $\beta_3 \equiv P''_+ - P''_-$. So γ_1, γ_2, and γ_3 (β_1, β_2, and β_3) as the intercept, slope, and second-order derivative changes, respectively, of the conditional mean function of the outcome (treatment) conditional on r at $r = r_0$. Let $\tau''(r_0) \equiv \frac{\partial^2}{\partial r^2}\tau(r)|_{r_0}$, where recall $\tau(r) \equiv \tau_{LATE}(P_1(r), P_0(r), r) \equiv E[Y_1 - Y_0|P_0(r) \leq U \leq P_1(r), R = r]$.

Corollary 4 *Assume that A1 holds. Further assume that all the funtions in A2 and A3 are continuously twice differentiable. Assume that both* $P_+ \neq P_-$ *and* $P'_+ \neq P'_-$*, and* $\tau''(r_0) = 0$*. Then*

$$\tau_{LATE}(P_+, P_-, r_0) = \frac{\gamma_1 + \widetilde{w}\left(2\beta_2\gamma_2 - \gamma_3\beta_1\right)}{\beta_1 + \widetilde{w}\left(2\beta_2^2 - \beta_3\beta_1\right)} \tag{19}$$

for any weights $\widetilde{\omega} \neq -\beta_1 / \left(2\beta_2^2 - \beta_3\beta_1\right)$.

Continuously twice differentiability and monotonicity imply that all the limits and derivatives involved in the definitions of γ_1, γ_2, γ_3, β_1, β_2 and β_3 exist.

Analogous to Corollary 1b, the assumption that $\tau''(r_0) = 0$ in the above will hold if the treatment effect is locally linear or locally constant. However, while a locally linear or constant treatment effect is sufficient for $\tau''(r_0) = 0$, it is stronger than necessary, because it implies that all derivatives higher than the first are zero, instead of just the second derivative being zero. When $\tau''(r_0) = 0$, Y cannot be a nontrivial function of $T(R - r_0)^2$, but can be a function of T or $T(R - r_0)$ or both.

PROOF of COROLLARY 4:

By equation (18) $G'_+ - G'_- = \tau'(r_0)(P_+ - P_-) + \tau(r_0)\left(P'_+ - P'_-\right)$. When $P_z(r)$, $z = 0, 1$, and $E(Y_t|U = u, R = r)$, $t = 0, 1$, are continuously twice differentiable in the neighborhood of $r = r_0$. Analogous to the derivation of equation (9), we have

$$G''_+ - G''_- = \tau''(r_0)(P_+ - P_-) + 2\tau'(r_0)(P'_+ - P'_-) + \tau(r_0)\left(P''_+ - P''_-\right).$$

The above along with $G_+ - G_- = \tau(r_0)(P_+ - P_-)$ and equation (18) gives the following system of equations:

$$\begin{aligned}
G_+ - G_- &= \tau(r_0)(P_+ - P_-), \\
G'_+ - G'_- &= \tau'(r_0)(P_+ - P_-) + \tau(r_0)\left(P'_+ - P'_-\right), \\
G''_+ - G''_- &= \tau''(r_0)(P_+ - P_-) + 2\tau'(r_0)(P'_+ - P'_-) + \tau(r_0)\left(P''_+ - P''_-\right).
\end{aligned}$$

Rewrite the above system of equations using notations defined in the text, we have

$$\gamma_1 = \tau(r_0)\beta_1,\ \gamma_2 = \tau'(r_0)\beta_1 + \tau(r_0)\beta_2, \text{ and } \gamma_3 = \tau''(r_0)\beta_1 + 2\tau'(r_0)\beta_2 + \tau(r_0)\beta_3.$$

Given $\tau''(r_0) = 0$, one can solve for $\tau(r_0)$ from the second and third equation in the above to have $\tau(r_0) = \left(2\beta_2\gamma_2 - \gamma_3\beta_1\right) / \left(2\beta_2^2 - \beta_3\beta_1\right)$. Given a jump we still have $\tau(r_0) = \gamma_1/\beta_1$. By definition, $\tau(r_0) \equiv \tau_{LATE}(P_+, P_-, r_0)$. It then follows that

$$\tau_{LATE}(P_+, P_-, r_0) = \frac{\gamma_1}{\beta_1} = \frac{2\beta_2\gamma_2 - \gamma_3\beta_1}{2\beta_2^2 - \beta_3\beta_1} = \frac{\gamma_1 + \widetilde{\omega}\left(2\beta_2\gamma_2 - \gamma_3\beta_1\right)}{\beta_1 + \widetilde{\omega}\left(2\beta_2^2 - \beta_3\beta_1\right)},$$

for any $\widetilde{\omega} \neq -\beta_1 / \left(2\beta_2^2 - \beta_3\beta_1\right)$.

Similar estimands can be constructed if the d-th derivative $\tau^{(d)}(r_0) = 0$ for any finite positive integer d, as is the case if the treatment effect is a polynomial of degree $d-1$ or less in $(R-r_0)$. In this case, the treatment effect can be an arbitrarily high-order (e.g., up to the $(d-1)$-th order) polynomial of $(R-r_0)$, as long as the order is finite. Keep taking derivatives on both sides of $\gamma_1 = \tau(r_0)\beta_1$, until the d-th derivative. With the system of d equations and $\tau^{(d)}(r_0) = 0$, one can back out $\tau(r_0)$, as the system of equations are recursive in nature.

12 Appendix IV: Additional Results (Elite School)

Table A1 Impacts of elite school attendance on post-compulsory education for males (alternative bandwidths)

	RD		RPK		RPJK	
			$R \in [540, 650]$			
	-0.730	-0.416	1.571***	1.582***	1.335***	1.343***
	(3.547)	(2.881)	(0.442)	(0.522)	(0.413)	(0.420)
Covariates	N	Y	N	Y	N	Y
			$R \in [450, 560]$			
	4.100	4.495	1.663***	1.719***	1.364***	1.378***
	(3.492)	(3.520)	(0.422)	(0.428)	(0.349)	(0.339)
Covariates	N	Y	N	Y	N	Y

Note: All specifications control for a linear term of the assignment score, with or without additionally an extensive set of covariates, including fixed effects for the school and grade attended in 1962, father's social class in eight categories, mother's occupation in nine categories, age within grade, and linear and quadratic terms of test scores at ages 7 and 9. Robust standard errors are in parentheses and are clustered at the assignment score level; ***Significant at the 1% level.

Table A2 Joint test of the jumps in the first-stage and reduced-form outcome regressions for males

	Lower threshold: $R = 540$		Upper threshold: $R = 560$	
$\chi^2_{(2)}$	8.22	11.6	1.76	2.23
P value	(0.016)	(0.003)	(0.416)	(0.327)
Covariates	N	Y	N	Y
N	409	409	361	361

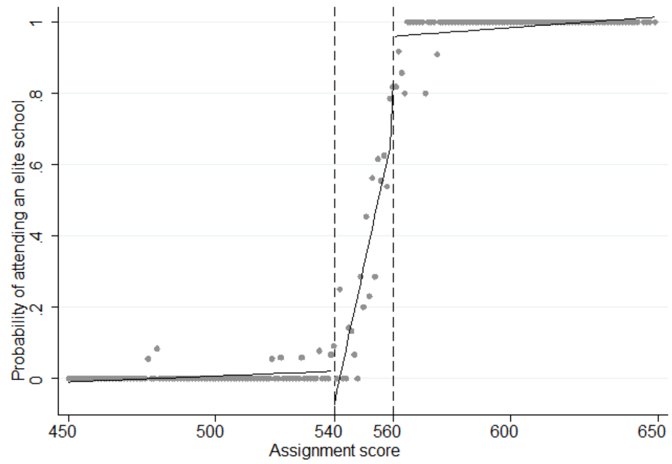


Figure A1 Assignment score and elite school attendance for females

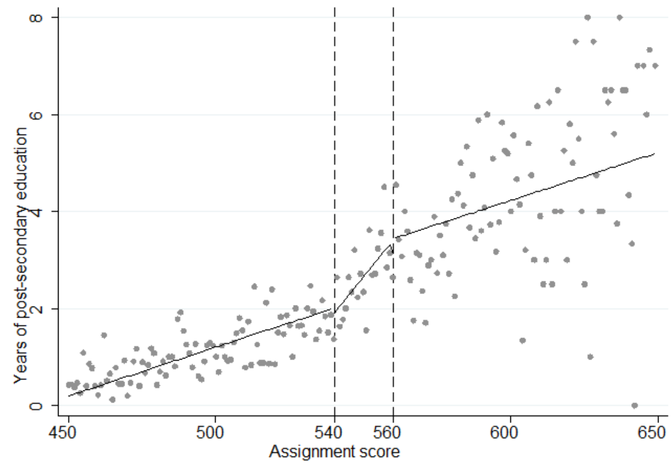


Figure A2 Assignment score and post-secondary education for females

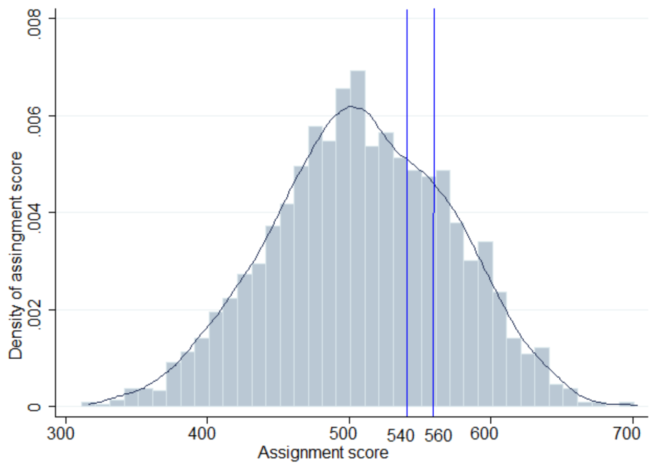


Figure A3 (a) Density of assignment score (females)

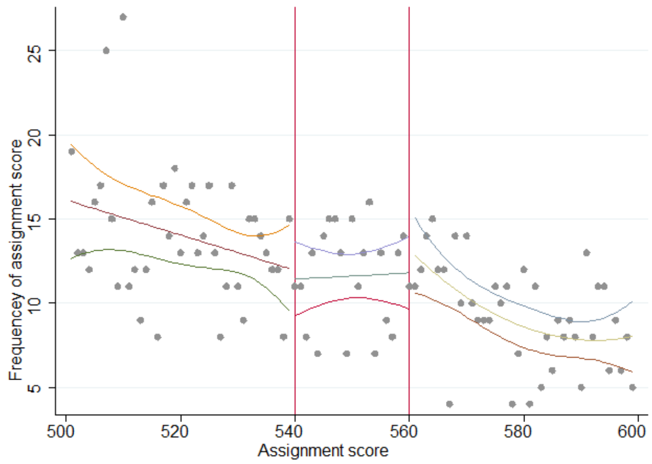


Figure A3 (b) Frequency of assignment score, local polynomial fit, and 95% CI (females)

Table A3 Smoothness of the density of assignment score (females)

$R \in$[540,580]		$R \in$[520,560]	
1(R>560)	0.005	1(R<540)	0.004
	(0.004)		(0.005)
(R-560)1(R>560)	-0.000	(R-540)1(R<540)	-0.000
	(0.001)		(0.001)

Note: Robust standard errors are in parentheses.

Table A4 Complier or marginal complier characteristics (females)

Age (months)	Birth order	Test7	Test9	High SES	Middle SES	Low SES
Upper threshold: *R*=560						
111.7	1.932	119.9	118	0.246	0.615	0.139
(4.506)***	(0.321)***	(5.767)***	(3.385)***	(0.113)**	(0.131)***	(0.134)
Lower threshold: *R*=540						
119.6	1.929	120	123.5	0.118	0.49	0.392
(1.841)***	(0.152)***	(2.015)***	(1.017)***	(0.072)	(0.103)***	(0.072)***

Note: Robust standard errors are clustered at each integer test score value; Age refers to that in Dec. 1962; Test7 is test score at age 7 and test9 is test score at age 9; high SES refers to father in professional or technical etc. non-manual occupation; Middle SES refers to father in skilled manual profession; Low SES refers to father in unskilled, semiskilled profession or unemployed, disabled, etc. **Significant at the 5% level; ***Significant at the 1% level.

Table A5 First-stage and reduced-form outcome estimates for females

Dependent var.	Elite school	Elite school	Education	Education
		$R \in [540,580]$		
1(R>560)	0.167***	0.175***	-0.010	0.093
	(0.059)	(0.056)	(0.416)	(0.482)
(R-560)1(R>560)	-0.033***	-0.031***	-0.084**	-0.050
	(0.005)	(0.005)	(0.036)	(0.038)
Covariates	N	Y	N	Y
R-squared	0.546	0.604	0.029	0.202
		$R \in [520,560]$		
1(R<540)	0.110*	0.091	0.026	-0.225
	(0.056)	(0.064)	(0.263)	(0.264)
(R-540)1(R<540)	-0.039***	-0.039***	-0.046*	-0.018
	(0.004)	(0.004)	(0.023)	(0.028)
Covariates	N	Y	N	Y
R-squared	0.368	0.440	0.069	0.252

Note: All specifications control for a linear term of the assignment score, with or without additionally an extensive set of covariates, including fixed effects for the school and grade attended in 1962, father's social class in eight categories, mother's occupation in nine categories, age within grade, and linear and quadratic terms of test scores at ages 7 and 9. Robust standard errors are in parentheses and are clustered at the assignment score level; * Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table A6 Impacts of elite school attendance on years of post-secondary education for females

	Jump Estimator		Kink Estimator		Jump&Kink Estimator	
			$R \in [540,580]$			
	-0.062	0.532	2.560**	1.620	2.187*	1.427
	(2.438)	(2.592)	(1.268)	(1.229)	(1.204)	(1.204)
Covariates	N	Y	N	Y	N	
			$R \in [520,560]$			
	0.237	-2.468	1.176*	0.469	1.123*	0.365
	(2.382)	(2.850)	(0.629)	(0.674)	(0.662)	(0.699)
Covariates	N	Y	N	Y	N	

Note: The covariates included are fixed effects for the school and grade attended in 1962, father's social class in eight categories, mother's occupation in nine categories, age within grade, and linear and quadratic terms of test scores at ages 7 and 9. Robust standard errors are clustered at the assignment score level and are in parentheses; *Significant at the 10% level; **Significant at the 5% level.

Table A7 Biases in the kink estimator (females)

	$R \in [540,580]$		$R \in 520,560]$	
	-0.010	-0.010	0.001	0.001
	(0.017)	(0.017)	(0.006)	(0.006)
Covariates	N	Y	N	Y

Note: Bootstrapped standard errors are based on 1,000 simulations.

13 Appendix V: Additional Results (Compulsory Military Service)

Table A8 Impacts of serving in the army on college education (males only)

First-stage		2SLS estimates		
Jump	Kink	RD	RPK	RPJK
		$R \in$[-200,199]		
0.091	0.002	0.171	-0.449	-0.353
(0.022)***	(0.000)***	(0.213)	(0.088)***	(0.074)***
		$R \in$[-120,119]		
0.114	0.003	-0.071	-0.421	-0.343
(0.026)***	(0.000)***	(0.197)	(0.127)***	(0.111)***

Note: Estimates are based on RLMS 1994-2014; All estimates control for region fixed effects for 39 regions, regional center fixed effect, and father's education and mother's education in 12 categories each; Robust standard errors are clustered at gender by month-year of birth level; ***Significant at the 1% level.

Table A9 Impacts of serving in the army on earnings (male-female)

First-stage		2SLS estimates		
Jump	Kink	RD	RPK	RPJK
		$R \in$[-200,199]		
0.069	0.002	21.59	-10.85	-5.414
(0.026)***	(0.000)***	(15.13)	(3.807)***	(3.521)
		$R \in$[-120,119]		
0.108	0.003	9.876	-13.16	-6.982
(0.031)***	(0.000)***	(9.836)	(5.869)**	(4.987)

Note: Estimates are based on RLMS 1994-2014; All estimates use females as a control group, and additionally control for region fixed effects for 39 regions, regional center fixed effect, and father's education and mother's education in 12 categories each; Robust standard errors are clustered at gender by month-year of birth level; **Significant at the 5% level; ***Significant at the 1% level.

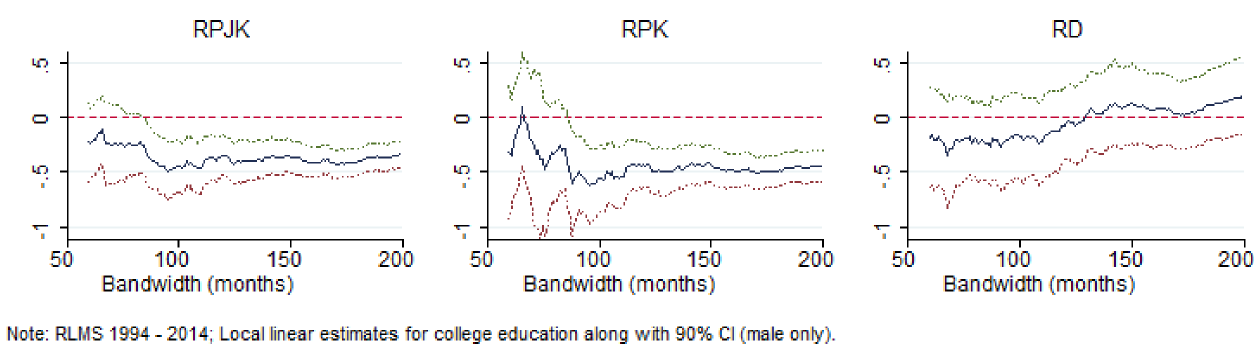
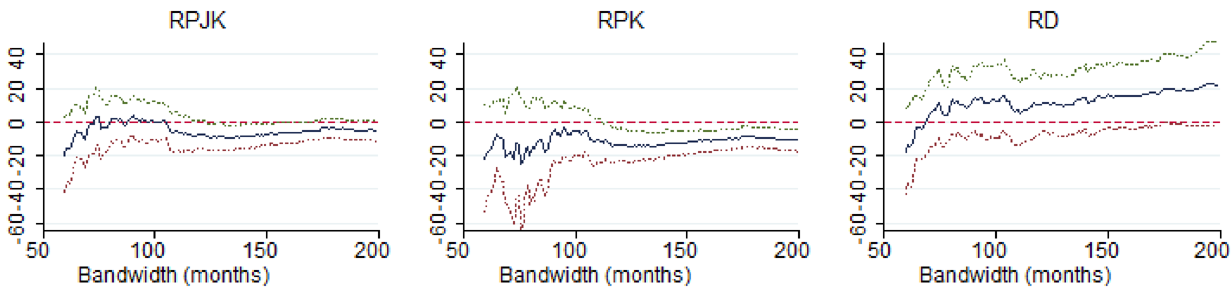


Figure A4 Estimated impacts on college education with different bandwidths (male only)

Table A10 Impacts of serving in the army on earnings (males only)

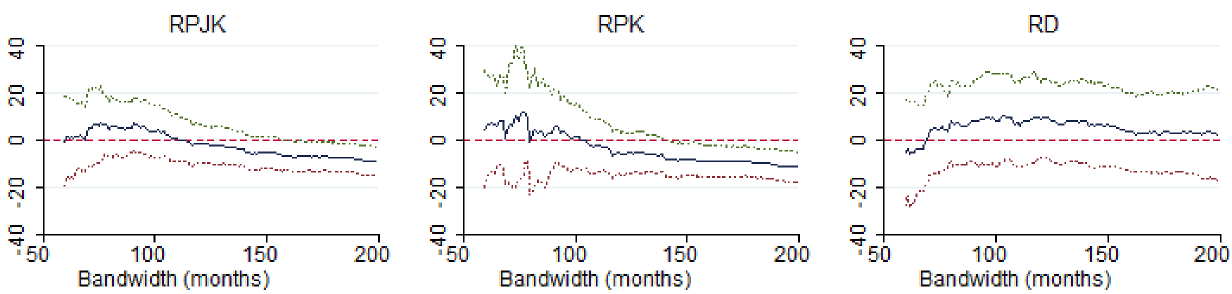
First-stage		2SLS estimates		
Jump	Kink	RD	RPK	RPJK
		$R \in$[-200,199]		
0.073	0.002	1.983	-11.97	-9.605
(0.026)***	(0.000)***	(11.83)	(4.038)***	(3.809)**
		$R \in$[-120,119]		
0.107	0.003	8.369	-5.589	-1.457
(0.031)***	(0.000)***	(9.612)	(5.510)**	(4.793)

Note: Estimates are based on RLMS 2000-2014; All estimates control for region fixed effects for 39 regions, regional center fixed effect, and father's education and mother's education in 12 categories each; Robust standard errors are clustered at gender by month-year of birth level; **Significant at the 5% level; ***Significant at the 1% level.



Note: RLMS 1994 - 2014; Local linear estimates for earnings along with 90% CI (male-female); Earnings in 1,000 2014 Rubles.

Figure A5 Estimated impacts on earnings with different bandwidths (RLMS 1994-2014)



Note: RLMS 2000 - 2014; Local linear estimates for earnings along with 90% CI (male only); Earnings in 1,000 2014 Rubles.

Figure A6 Estimated impacts on earnings with different bandwidths (RLMS 2000-2014, male only)

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