NBER WORKING PAPER SERIES

EXTERNAL VALIDITY IN FUZZY REGRESSION DISCONTINUITY DESIGNS

Marinho Bertanha
Guido W. Imbens

Working Paper 20773
http://www.nber.org/papers/w20773

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2014

We are grateful for comments by Joshua Angrist, Brigham Frandsen and Arthur Lewbel on an earlier version of this paper. We are are also grateful to Brian Jacob, Lars Lefgren, and Jordan Matsudaira for making available the data used in this paper. Bertanha gratefully acknowledges the financial support received as a B.F. Haley and E.S. Shaw Fellow from the Stanford Institute for Economic Policy Research. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed a financial relationship of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w20773.ack

External Validity in Fuzzy Regression Discontinuity Designs
Marinho Bertanha and Guido W. Imbens
NBER Working Paper No. 20773
December 2014
JEL No. C14

ABSTRACT

Many empirical studies use Fuzzy Regression Discontinuity (FRD) designs to identify treatment effects when the receipt of treatment is potentially correlated to outcomes. Existing FRD methods identify the local average treatment effect (LATE) on the subpopulation of compliers with values of the forcing variable that are equal to the threshold. We develop methods that assess the plausibility of generalizing LATE to subpopulations other than compliers, and to subpopulations other than those with forcing variable equal to the threshold. Specifically, we focus on testing the equality of the distributions of potential outcomes for treated compliers and always-takers, and for non-treated compliers and never-takers. We show that equality of these pairs of distributions implies that the expected outcome conditional on the forcing variable and the treatment status is continuous in the forcing variable at the threshold, for each of the two treatment regimes. As a matter of routine, we recommend that researchers present graphs with estimates of these two conditional expectations in addition to graphs with estimates of the expected outcome conditional on the forcing variable alone. We illustrate our methods using data on the academic performance of students attending the summer school program in two large school districts in the US.

Marinho Bertanha
Department of Economics
Stanford University
579 Serra Mall
Stanford, CA 94305-6072
bertanha@stanford.edu

Guido W. Imbens
Graduate School of Business
Stanford University
655 Knight Way
Stanford, CA 94305
and NBER
Imbens@stanford.edu

1 Introduction

In empirical studies in economics and other social sciences researchers are often concerned about the possible endogeneity of key variables. Recently, many studies have used regression discontinuity designs, originating in the psychology literature (Thistlewaite and Campbell, 1960) to address these concerns. See Van Der Klaauw (2008), Imbens and Lemieux (2008), and Lee and Lemieux (2010) for recent surveys, and Cook (2008) for a historical perspective. In the current paper we study comparisons of regression discontinuity estimators and estimators based on unconfoundedness or exogeneity-type assumptions. In the closely related setting of linear instrumental variables models with constant coefficients, such comparisons are often based on Hausman tests (Hausman, 1978). In the local average treatment (LATE) setting with heterogenous treatment effects (Imbens and Angrist (1994), Angrist, Imbens and Rubin (1996)), Angrist (2004) discusses the interpretation of the Hausman test and suggests a more attractive test for homogeneity of treatment effects in that context. In the current paper, we discuss the extension of the Hausman and Angrist tests to the fuzzy regression discontinuity (FRD) settings. Using the FRD set up developed by Hahn, Todd and Van Der Klaauw (2001) and allowing for heterogeneous treatment effects, we propose a more attractive and novel test for homogeneity of treatment effects.

In a Hausman test, the parameter estimated by least squares is compared to the parameter estimated by instrumental variables. In the regression discontinuity context, the first parameter identifies the average treatment effect at the threshold under exogeneity or unconfoundedness assumptions, whereas the second parameter identifies the average treatment effect at the threshold for the subpopulation of compliers under the FRD identification assumptions.

Our main point is that, instead of testing the single equality of the Hausman approach, researchers should test jointly a pair of restrictions on the potential outcomes of compliers, always-takers, and never-takers using the Imbens-Angrist terminology in the FRD design. The pair of restrictions are: (i) the equality between the average outcome of treated always-takers and compliers at the threshold; (ii) the equality between the average outcome for non-treated never-takers and compliers at the threshold. If both equalities hold, we argue that is more plausible that one can extrapolate the average effect for compliers to other subpopulations; that is, it is more likely that the estimates have external validity. Moreover, external validity also allows for identification of treatment effects on subpopulations with values for the forcing variable that are different than the threshold.

We show that this pair of restrictions is equivalent to continuity of the conditional expectation of the outcome as a function of the forcing variable at the threshold, separately for each of the two treatment regimes. Currently researchers applying fuzzy regression discontinuity designs typically present graphs containing estimates of the conditional expectation of the outcome given the forcing variable without conditioning on the treatment to illustrate the identification strategy. We recommend that, in addition to those graphs, researchers present graphs containing estimates of the conditional expectations of the outcome given the forcing variable separately by treatment status. A discontinuity at the threshold in these conditional expectations provides evidence against exogeneity or unconfoundedness assumptions, and, thereby, evidence against external validity of the estimates.

In the recent causal literature there have been alternative proposals for assessing and improving the external validity of regression discontinuity and instrumental variables estimates. In an interesting approach, Dong and Lewbel (2014) point out that at the threshold one cannot only estimate the magnitude of the discontinuity, but also the change in the first (or even higher order) derivatives of the regression function. Under smoothness of the two conditional mean functions, knowledge of the higher order derivatives would allow the researcher to extrapolate at least locally away from the threshold. The Dong and Lewbel methods apply both in the sharp and in the fuzzy regression discontinuity design. Angrist and Rokkanen (2012) exploit the presence of additional exogenous covariates, and assess whether conditional on these covariates the influence of the forcing variable vanishes. This allows for extrapolation away from the threshold. Their methods also apply both in the case of sharp and fuzzy regression discontinuity designs. Angrist and Fernandez-Val (2010), in a conventional instrumental variables setting that can be generalized to the fuzzy regression discontinuity setting, consider extrapolating local average treatment effects by exploiting the presence of other exogenous covariates. Their key assumption, which they label conditional effect ignorability, is that conditional on these additional covariates the average effect for compliers is identical to the average effect for all compliance types. These three approaches are all complementary to ours. In contrast to these extrapolation methods, our approach requires the regression discontinuity design to be fuzzy but it does not require additional covariates.

The remainder of this paper is organized as follows. In Section 2, we introduce the notation for the FRD setting, define parameters of interest, and state regularity and identification conditions. Section 3 discusses methods to assess the plausibility of exogeneity and external validity. Section 4 illustrates our proposed methods using two data sets previously used to estimate the effect of summer school programs on academic performance; the first data set was originally analyzed by Jacob and Lefgren (2004), and the second data set was previously studied by Matsudaira (2008). An appendix presents proofs for the results in this paper.

2 Set Up

Here we set up the framework for analyzing fuzzy regression discontinuity designs. We largely follow the widely used Rubin Causal Model set up, extended to the fuzzy regression discontinuity design by Hahn, Todd, and Van Der Klaauw (2001), HTV from hereon. Recent theoretical contributions include Porter (2003), McCrary (2008), Imbens and Kalyanaraman (2012), Calonico, Cattaneo and Titiunik (2014abc), Dong (2014), Dong and Lewbel (2014), Bertanha (2014), and Gelman and Imbens (2014). Influential applications include Black (1999), Berk and Rauma (1983), Angrist and Lavy (1999), Van Der Klaauw (2002), Jacob and Lefgren (2004), Battistin and Rettore (2008), Lee (2008), Lalive (2008), and Matsudaira (2008).

2.1 Notation

We consider a setting where we have a random sample from a large population, with the units in the sample indexed by $i = 1, \ldots, N$. Let W_i^{obs} be the binary treatment of interest, which may be endogenous. We are interested in the causal effect of the treatment on an outcome Y_i. Let $Y_i(0)$ and $Y_i(1)$ denote the potential outcomes. The realized and observed outcome is

$$Y_i^{\text{obs}} = \begin{cases} Y_i(0) & \text{if } W_i^{\text{obs}} = 0, \\ Y_i(1) & \text{if } W_i^{\text{obs}} = 1. \end{cases}$$

In addition we observe for each unit a covariate, which we refer to as the forcing variable, denoted by X_i. The support of the distribution of the forcing variable X_i is denoted by $\mathbb{X}$. The forcing variable is a fixed characteristic of the individual that is not affected by the treatment. At a particular value for this forcing variable, the threshold, denoted by $T^\star$, the incentives to participate in the treatment change. We view the value of this threshold as a quantity that can potentially be manipulated. We assume that changing the value of the threshold does not change the values of the potential outcomes.

Our set up for the determination of the treatment received is slightly different from that in HTV, and more line with Dong (2014) and Dong and Lewbel (2014) in that we explictly allow for different values of the threshold. Let $W_i(t)$ be the potential outcome denoting whether individual i would participate in the treatment if the threshold was set equal to t. We assume there are three types of individuals, with the type for individual i denoted by G_i. First, never-takers, with $G_i = \mathrm{n}$, for whom $W_i(t) = 0$ for all t. Second, always-takers, with $G_i = \mathrm{a}$, for whom $W_i(t) = 1$ for all t, and compliers, with $G_i = \mathrm{c}$, who participate if the threshold that requires participation is set to the value of their forcing variable or higher, so that $W_i(t) = \mathbf{1}_{X_i \leq t}$. We observe $W_i^{\mathrm{obs}} = W_i(T^\star)$, at the actual threshold $T^\star$.

To make this concept more accessible, consider the application we are using in this paper. In the summer school example the forcing variable X_i is a test-score prior to the summer program. The school district sets a threshold t, with students scoring below or at the threshold, that is, students with $X_i \leq t$, required to participate in the summer program. The function $W_i(t)$ describes whether student i would participate in the program if the district had set the threshold for participation at t. Given the score X_i for student i, in many cases students would participate if they score less than or equal to the threshold set by the district, that is, if $X_i \leq t$, but not otherwise, so that for those students the participation decision is $W_i(t) = \mathbf{1}_{X_i \leq t}$. There are some students who would participate in the summer program even if their score is above the threshold, the always-takers with $W_i(t) = 1$, and some students who would not participate even if their score is below the threshold, the never-takers, with $W_i(t) = 0$.

2.2 Causal Estimands

Here we define the main causal estimands.

First, the average causal effect of the treatment for individuals for whom the value of the forcing variable is equal x:

$$\tau^{\text{ate}}(x) = \mathbb{E}\left[Y_i(1) - Y_i(0) | X_i = x\right].$$

Of particular interest is the average effect for individuals with $X_i = T^\star$:

$$\tau^{\text{ate}}(T^\star) = \mathbb{E}\left[Y_i(1) - Y_i(0) | X_i = T^\star\right].$$

In addition, define the local average treatment effect,

$$\tau^{\text{late}} = \mathbb{E}\left[Y_i(1) - Y_i(0) | G_i = \text{c}, X_i = T^\star\right].$$

Define also the overall average effect

$$\tau^{\text{ate}} = \mathbb{E}\left[Y_i(1) - Y_i(0)\right] = \mathbb{E}\left[\tau^{\text{ate}}(X_i)\right].$$

2.3 Fuzzy Regression Discontinuity and Exogenous Estimands

Next, we consider what is being estimated by methods that rely on FRD assumptions or on exogeneity/unconfoundedness type assumptions. First, define

$$\tau^{\text{frd}} = \frac{\lim_{h \downarrow 0} \left\{ \mathbb{E}[Y_i^{\text{obs}} | T^\star < X_i < T^\star + h] - \mathbb{E}[Y_i^{\text{obs}} | T^\star - h < X_i < T^\star] \right\}}{\lim_{h \downarrow 0} \left\{ \mathbb{E}[W_i^{\text{obs}} | T^\star < X_i < T^\star + h] - \mathbb{E}[W_i^{\text{obs}} | T^\star - h < X_i < T^\star] \right\}}.$$

Second, we consider a comparison between treated and control units for the subpopulation with covariate value close to the threshold:

$$\tau^{\text{exo}} = \lim_{h \downarrow 0} \left\{ \mathbb{E}[Y_i^{\text{obs}} | W^{\text{obs}} = 1, T^\star - h < X_i < T^\star + h] \right.$$
$$\left. - \mathbb{E}[Y_i^{\text{obs}} | W^{\text{obs}} = 0, T^\star - h < X_i < T^\star + h] \right\}.$$

2.4 Assumptions

We make the following assumptions, which are standard assumptions in the regression discontinuity literature. See HTV, Lewbel and Dong (2014), Porter (2003), Calonico, Cattaneo and Titiunik (2014).

Assumption 1. *The sample is a random sample from a large population.*

Assumption 2. *The probability* $\mathrm{pr}(Y_i(0) \leq y_0, Y_i(1) \leq y_1, G_i = g | X_i = x)$ *is continuous in* x *for all* $y_0, y_1 \in \{-\infty, \infty\}$ *and* $g \in \{\mathrm{n}, \mathrm{a}, \mathrm{c}\}$, *and the conditional mean of* $Y_i(w)$ *given* $X_i = x$ *is finite for all* x *and* w.

Assumption 3. *The distribution of* X_i *is continuous with density* $f_X(x)$. *The* $\mathrm{pr}(W_i^{\mathrm{obs}} = 1 | X_i = x)$ *is strictly between zero and one for all values of* x *in the support of* X_i.

Assumption 4. *The actual threshold is* $T^\star$, *with the density* $f_X(T^\star)$ *positive.*

The following result is due to HTV (2001), and it is stated without proof.

Lemma 1. *Suppose Assumptions 1-4 hold. Then:*

$$\tau^{\mathrm{frd}} = \tau^{\mathrm{late}}.$$

3 Testing for Exogeneity and External Validity in Fuzzy Regression Discontinuity Settings

In this section we discuss the main restrictions we wish to assess.

3.1 The Hausman Test in Fuzzy Regression Discontinuity Designs

To set the stage, let us first consider the standard (that is, non-regression discontinuity) instrumental variables setting, with a single binary endogenous regressor and a single binary instrument. In a constant coefficient model we have

$$Y_i^{\mathrm{obs}} = \alpha + \tau \cdot W_i^{\mathrm{obs}} + \varepsilon_i,$$

with a binary instrument Z_i. The Hausman test compares the ordinary least squares estimator for τ with the instrumental variables estimator using Z_i as an instrument for W_i^{obs}. In large samples the test compares the two quantities,

$$\mathrm{plim}(\hat{\tau}^{\mathrm{ols}}) = \mathbb{E}[Y_i^{\mathrm{obs}} | W_i^{\mathrm{obs}} = 1] - \mathbb{E}[Y_i^{\mathrm{obs}} | W_i^{\mathrm{obs}} = 0],$$

with

$$\text{plim}(\hat{\tau}^{\text{iv}}) = \frac{\mathbb{E}[Y_i^{\text{obs}}|Z_i = 1] - \mathbb{E}[Y_i^{\text{obs}}|Z_i = 0]}{\mathbb{E}[W_i^{\text{obs}}|Z_i = 1] - \mathbb{E}[W_i^{\text{obs}}|Z_i = 0]}.$$

The first result explores the interpretation of the restriction $\text{plim}(\hat{\tau}^{\text{ols}}) = \text{plim}(\hat{\tau}^{\text{iv}})$ settings with heterogenous treatments, under monotonicity, exogeneity of the instrument, and the exclusion restriction. These are the instrumental variables assumptions used in Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996). Let $G_i \in \{\text{n}, \text{c}, \text{a}\}$ denote the compliance type for unit i, let $\pi_{\text{n}} = \text{pr}(G_i = \text{n})$, $\pi_{\text{c}} = \text{pr}(G_i = \text{n})$, and $\pi_{\text{a}} = \text{pr}(G_i = \text{n})$ denote their population shares, and let $p_z = \Pr(Z_i = 1)$ denote the population probability that the instrument takes on the value one.

Lemma 2. *Suppose that Z_i is independent of $(Y_i(0), Y_i(1), G_i)$, and that there are no defiers. Then the Hausman null hypothesis*

$$H_0^{H,\text{iv}}: \ \text{plim}(\hat{\tau}^{\text{ols}}) = \text{plim}(\hat{\tau}^{\text{iv}}),$$

is equivalent to the null hypothesis

$$\begin{aligned} H_0^{H,\text{iv}\prime}: \quad & \frac{\pi_{\text{a}}}{\pi_{\text{a}} + \pi_{\text{c}} \cdot p_z} \cdot \Big(\mathbb{E}[Y_i(1)|G_i = \text{a}] - \mathbb{E}[Y_i(1)|G_i = \text{c}] \Big) \\ & = \frac{\pi_{\text{n}}}{\pi_{\text{n}} + \pi_{\text{c}} \cdot (1 - p_z)} \cdot \Big(\mathbb{E}[Y_i(0)|G_i = \text{n}] - \mathbb{E}[Y_i(0)|G_i = \text{c}] \Big). \end{aligned} \tag{3.1}$$

All proofs are in the Appendix.

In the fuzzy regression design setting, the natural analogue of the Hausman test is the null hypothesis that the fuzzy regression discontinuity estimand is identical to the local comparison of treated and control units:

$$H_0^{H,\text{frd}}: \quad \tau^{\text{exo}} = \tau^{\text{frd}}. \tag{3.2}$$

The following lemma shows the equivalence between the equality $\tau^{\text{exo}} = \tau^{\text{frd}}$ and a equality of weighted averages of potential outcomes of compliers, always-takers and never-takers. Now let $\pi_{\text{n}}(x) = \text{pr}(G_i = \text{n}|X_i = x)$, $\pi_{\text{a}}(x) = \text{pr}(G_i = \text{a}|X_i = x)$, and $\pi_{\text{c}}(x) = \text{pr}(G_i = \text{c}|X_i = x)$ be the type probabilities conditional on the value of the forcing variable, and let $\pi_{\text{n}} = \pi_{\text{n}}(T^*)$, $\pi_{\text{a}} = \pi_{\text{a}}(T^*)$, and $\pi_{\text{c}} = \pi_{\text{c}}(T^*)$ be shorthand for the type probabilities at the threshold.

Lemma 3. *Suppose that Assumptions 1-4 hold. Then the null hypothesis*

$$H_0^{H,\text{frd}}: \ \tau^{\text{exo}} = \tau^{\text{frd}},$$

is equivalent to the null hypothesis

$$H_0^{H,\text{frd}\prime}: \quad \frac{\pi_\text{a}}{\pi_\text{a} + \pi_\text{c}/2} \cdot \Big(\mathbb{E}[Y_i(1)|G_i = \text{a}, X_i = T^\star] - \mathbb{E}[Y_i(1)|G_i = \text{c}, X_i = T^\star] \Big)$$
$$= \frac{\pi_\text{n}}{\pi_\text{n} + \pi_\text{c}/2} \cdot \Big(\mathbb{E}[Y_i(0)|G_i = \text{n}, X_i = T^\star] - \mathbb{E}[Y_i(0)|G_i = \text{c}, X_i = T^\star] \Big).$$

The slight change in the weights comes from the fact that by assumption the probability of being to the left or to the right of the threshold, conditional on being very close to the threshold, is equal to 1/2, because of the continuity of the distribution of the forcing variable.

Comment 1: The first point of the paper is that in general the Hausman hypotheses $H_0^{H,\text{iv}\prime}$ and $H_0^{H,\text{frd}\prime}$ are difficult to interpret outside the setting with constant treatment effects that they were originally developed for. These null hypotheses allow for differences in average outcomes between always-takers and compliers with the treatment, and between never-takers and compliers without the treatment, as long as the particular weighted average of those differences cancel out. The weights depend on the shares of the compliance types. □

3.2 The Angrist (2004) Restrictions

Angrist (2004) suggests testing for selection bias in the conventional instrumental variables setting by testing the null hypothesis

$$H_0^{A,\text{iv}}: \ \frac{\mathbb{E}[Y_i^{\text{obs}}|Z_i = 1] - \mathbb{E}[Y_i^{\text{obs}}|Z_i = 0]}{\mathbb{E}[W_i^{\text{obs}}|Z_i = 1] - \mathbb{E}[W_i^{\text{obs}}|Z_i = 0]}$$
$$= \mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 1, Z_i = 0] - \mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 0, Z_i = 1].$$

This is equivalent to the null hypothesis

$$H_0^{A,\text{iv}\prime}: \ \mathbb{E}[Y_i(1) - Y_i(0)|G_i = \text{c}] = \mathbb{E}[Y_i(1)|G_i = \text{a}] - \mathbb{E}[Y_i(0)|G_i = \text{n}],$$

or, to stress the difference with the Hausman test,

$$H_0^{A,\text{iv}\prime\prime}: \ \mathbb{E}[Y_i(1)|G_i = \text{a}] - \mathbb{E}[Y_i(1)|G_i = \text{c}] = \mathbb{E}[Y_i(0)|G_i = \text{n}] - \mathbb{E}[Y_i(0)|G_i = \text{c}].$$

Compared to the Hausman null hypothesis $H^{H,\text{iv}\prime}$, in the Angrist null hypothesis $H^{A,\text{iv}\prime\prime}$ the weights have been dropped, and we simply compare the average effect for compliers to the difference in average outcomes for always-takers and never-takers, which appears to be a more natural comparison.

In the fuzzy regression discontinuity setting the natural analogue of Angrist's null hypothesis is

$$H_0^{H,\text{frd}}: \quad \tau^{\text{frd}} = \lim_{h \downarrow 0} \mathbb{E}[Y_i^{\text{obs}} | W_i^{\text{obs}} = 1, X_i < T^\star + h] - \mathbb{E}[Y_i^{\text{obs}} | W_i^{\text{obs}} = 0, T^\star - h < X_i].$$

Lemma 4. *Suppose that Assumptions 1-4 hold. Then the null hypothesis*

$$H_0^{A,\text{frd}}: \quad \tau^{\text{frd}} = \lim_{h \downarrow 0} \mathbb{E}[Y_i^{\text{obs}} | W_i^{\text{obs}} = 1, X_i < T^\star + h] - \mathbb{E}[Y_i^{\text{obs}} | W_i^{\text{obs}} = 0, T^\star - h < X_i],$$

is equivalent to the null hypotheses

$$H_0^{A,\text{frd}\prime}: \quad \mathbb{E}[Y_i(1) | G_i = \text{c}, X_i = T^\star] - \mathbb{E}[Y_i(0) | G_i = \text{c}, X_i = T^\star]$$

$$= \mathbb{E}[Y_i(1) | G_i = \text{a}, X_i = T^\star] - \mathbb{E}[Y_i(0) | G_i = \text{n}, X_i = T^\star],$$

and

$$H_0^{A,\text{frd}\prime\prime}: \quad \mathbb{E}[Y_i(1) | G_i = \text{a}, X_i = T^\star] - \mathbb{E}[Y_i(1) | G_i = \text{c}, X_i = T^\star]$$

$$= \mathbb{E}[Y_i(0) | G_i = \text{n}, X_i = T^\star] - \mathbb{E}[Y_i(0) | G_i = \text{c}, X_i = T^\star].$$

Angrist (2004) motivates his proposed test by arguments concerning the statistical power. We view his test also as more attractive than the Hausman test because it has a more intuitive interpretation.

3.3 External Validity

Under the assumptions we laid out, regression discontinuity estimates are valid locally, where the qualifier "locally" limits their generalizability in two aspects. These estimates are valid only for units with the value of the forcing variable X_i close to the threshold $T^\star$, and they are valid only for compliers. Often we are interested in causal effects also for non-compliers, and for units with values for the forcing variable away from the threshold. In this section, we explore the assumptions that validate extrapolation (external validity) of treatment effects to individuals with different values of the forcing variable and individuals of different compliance types.

We consider the following assumption.

Assumption 5. (External Validity)
The study is externally valid if,

$$G_i \perp\!\!\!\perp \Big(Y_i(0), Y_i(1)\Big) \Big| X_i. \tag{3.3}$$

This assumption is related to what Angrist (2004) calls the "no-selection bias" condition in the conventional instrumental variables setting, and what Dong and Lewbel call the "local invariance assumption". Angrist shows that, in the case of binary instrument and binary treatment, the no-selection assumption leads to four restrictions with two free parameters, or to a pair of testable restrictions. Here we explore and extend these findings to the regression discontinuity setting. First, this assumption implies that the average treatment effect is identified:

Lemma 5. *Suppose Assumptions 1-5 hold. Then τ^{ate} is identified.*

In the FRD case, we observe treated and non-treated individuals with values for the forcing variable that are different than the threshold. In general, comparing treated and non-treated outcomes away from the threshold does not identify treatment effects because treated and non-treated individuals belong to different compliance subpopulations (i.e. compliers, always-takers, never-takers). Under external validity, the average potential outcome does not vary across compliance subpopulations, and the comparison of treated and non-treated outcomes away from the threshold does identify treatment effects.

Let us now explore the testable restrictions implied by the external validity assumption. External validity implies the following conditional independence restriction:

$$G_i \perp\!\!\!\perp Y_i(1) \Big| X_i = T^\star. \tag{3.4}$$

This in turn implies

$$G_i \perp\!\!\!\perp Y_i(1) \Big| G_i \in \{a, c\}, X_i = T^\star, \tag{3.5}$$

so that

$$\mathbb{E}[Y_i(1)|G_i = c, X_i = T^\star] = \mathbb{E}[Y_i(1)|G_i = a, X_i = T^\star], \tag{3.6}$$

and, by a similar argument,

$$\mathbb{E}[Y_i(0)|G_i = c, X_i = T^\star] = \mathbb{E}[Y_i(0)|G_i = n, X_i = T^\star], \tag{3.7}$$

These two restrictions are still in terms of unobserved variables. However, they imply a pair of restrictions on observed variables, as summarized in the following lemma.

Lemma 6. *Suppose Assumptions 1-5 hold. Then:*

$$\lim_{h\downarrow 0} \mathbb{E}\left[Y_i^{\text{obs}} \middle| W_i^{\text{obs}} = w, T^\star < X_i < T^\star + h \right]$$
$$= \lim_{h\downarrow 0} \mathbb{E}\left[Y_i^{\text{obs}} \middle| W_i^{\text{obs}} = w, T^\star - h < X_i < T^\star \right], \tag{3.8}$$

for $w = 0, 1$.

Comment 2: The two restrictions (3.6) and (3.7) together imply the restriction tested by the Hausman test, $H_0^{H,\text{frd}\prime}$, as well as the restriction tested by Angrist's test, $H_0^{A,\text{frd}\prime\prime}$. However, the two restrictions are stronger than the restrictions tested by either the Hausman or the Angrist test on their own. Therefore, testing the pair of restrictions (3.6) and (3.7) jointly is more attractive. For example, suppose the Angrist null hypothesis $H_0^{A,\text{frd}\prime\prime}$ holds, but not the pair of restrictions (3.6) and (3.7). In that case the Angrist null hypothesis would no longer hold after a transformation of the outcome, say from Y_i^{obs} to $\ln(Y_i^{\text{obs}})$. □

Comment 3: Equation (3.8) is the key equation in the paper. There is a simple graphical representation of this equality: the expectation of Y_i^{obs} conditional on $W_i^{\text{obs}} = w$ and X_i is continuous in the forcing variable at the threshold $T^\star$. This can be assessed by estimating this conditional expectation $\mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = w, X_i = x]$ and checking for a discontinuity at $X_i = x$, for both $w = 0, 1$. The implementation is very similar to estimating a treatment effect in a regression discontinuity design except for the fact that the density of X_i conditional on $W_i = w$ may be discontinuous at the threshold. □

4 Two Applications

In this section, we illustrate our methods using data from Jacob and Lefgren (2004) and Matsudaira (2008). These papers estimate the effect of remedial summer school programs on academic performance of students using fuzzy regression discontinuity methods.

4.1 The Jacob and Lefgren (2004) Data

In this section, we illustrate the equality of means among different compliance groups in a FRD setting using data from Jacob and Lefgren (2004). Jacob and Lefgren use administrative data from the Chicago Public Schools which instituted in 1996 an accountability policy that tied summer school attendance and promotional decisions to performance on standardized tests. This policy was followed by other school districts in the country. In section 4.2, we apply our methods to Matsudaira (2008) data from another urban school district in the Northeast who adopted a similar policy. The standard rule is to send a student to summer school if the minimum between his reading and math test-score is below a certain threshold. The final decision is up to teacher's discretion which considers some other indicators which leads to a FRD design.

There are reasons to expect that in this setting those who do not attend summer school despite scoring below the threshold (never-takers) are different from compliers: they may have been judged to have scored below expectations and viewed as not in need of summer school, and thus be better than compliers with similar pre-summer school scores, or they may be reluctant to participate in summer school and actually be worse in terms of academic ability than compliers. Similarly, it could be that always-takers are students viewed as particularly in need of the extra instruction, and thus be worse in terms of academic ability than compliers with the same scores prior to summer school, or these could be students that are particularly eager for additional educational experiences and who would have done better than compliers regardless of their summer school participation.

Jacob and Lefgren use observations around a test-score cutoff to estimate the impact of attending summer school on academic performance. They find a positive impact on achievement for third graders. We use the data for third graders in years 1997-99. The forcing variable X_i is the minimum between reading and math score before the summer school minus the threshold (2.75 in this application). The outcome variable Y_i^{obs} is measured by the math score after the summer school.

Following the work by Hahn, Todd and Van Der Klaauw (2001) and Porter (2003), we fit a local linear regression on each side of the cutoff using the edge kernel $k(u) = \mathbf{1}_{|u|\leq 1} \cdot (1-|u|)$. The optimal bandwidth was computed based on the Imbens and Kalyanaraman

(2012) optimal bandwidth rule. In Figure 1a we plot the probability of attending summer school given the value of the forcing variable. Most of the students that are eligible for summer school based on the minimum test-score do attend summer school, and the change in the probability of attending summer school is estimated at 0.894 (s.e. 0.006) (Table 1). Next, in Figure 1b, we plot the conditional mean of the math test-score after summer school given the minimum test-score prior to summer school. Conditional on having the minimum test-score close to the threshold, there is a significant increase in the outcome for those who are required to attend summer school compared to those who are not required to attend summer school. The causal effect of summer school on subsequent academic performance for the subpopulation of compliers is around 0.20 (s.e. 0.03).

In the last two figures based on the Jacob-Lefgren data, Figures 1c and 1d, we present estimates of the expected value of the outcome conditional on both the forcing variable and treatment status $\mathbb{E}[Y_i|X_i, W_i = w]$ for $w \in \{0, 1\}$. We find that there is not a substantial difference between never-takers and control compliers, with the difference in average outcomes estimated at 0.07 (s.e. 0.06). On the other hand, there is a substantial difference in average outcomes between always-takers and treated compliers, at 0.36 (s.e. 0.13). Always-takers perform substantially worse than treated compliers, consistent with the notion that the always-takers are guided towards the summer program even if they score slightly above the threshold. In this application, the Hausman and Angrist null hypotheses are rejected at conventional levels, suggesting heterogeneity of treatment effects across compliance groups. Our approach finds evidence supporting homogeneity of treatment effects between compliers and never-takers.

4.2 The Matsudaira (2008) Data

Matsudaira(2008) uses administrative data from a large urban school district in Northeastern United States to evaluate the impact of attending summer school on students' academic performance. One of the promotion criteria in this school district requires students in third grade or above to score above a given cutoff score on year-end examinations in both math and reading in order to pass to the next grade. Students who fail to score above the cutoff are more likely to be required to attend the summer school program. Besides passing the test-score in reading and math, there are other criteria for promotion like attendance which is not recorded in these data. This makes the assignment to summer school fuzzy around the test cutoff, similar to the Jacob-Lefgren study.

We use individual level data for fifth grade students with reading and math test-scores taken in 2001 and 2002. The forcing variable X_i is the minimum of the 2001 reading score and the 2001 mathematics score, minus the threshold for passing. The outcome variable Y_i is the standardized mathematics score in 2002.

We present the same plots as in the Jacob-Lefgren application. First, in Figure 2a we present the probability of attending summer school given the value of the forcing variable. Next, in Figure 2b, we present the conditional mean of the math test-score after summer school given the minimum test-score before summer school. Conditional on having the minimum test-score close to the threshold, there is a significant increase in the outcome for those who are required to attend summer school compared to those who are not required to attend summer school, implying a positive causal effect of summer school on subsequent academic performance. Finally, we report the graph for the expected value of the outcome conditional on both the forcing variable and treatment status $\mathbb{E}[Y_i|X_i, W_i = w]$ for $w \in \{0, 1\}$ in Figures 2c and 2d. The average potential outcome if treated is statistically different between always-takers and compliers, and the average potential outcome if not treated is also statistically different between never-takers and compliers (Table 2). In contrast to the findings for the Jacob-Lefgren data, we find in the Matsudaira data substantial evidence of heterogeneity in achievement between students that never need summer school (never-takers) and those that might need depending on their test-scores (compliers).

5 Conclusion

By their very nature, the external validity of fuzzy regression discontinuity analyses is often a concern. The identification is credible only around the threshold of the forcing variable. In many cases, however, researchers are also interested in generalizing the findings to subpopulations with values of the forcing variable away from the threshold and to subpopulations other than that of compliers. In this paper we explore assumptions that allow for such generalizations, and we derive testable implications of such assumptions. We show that these implications can be assessed by inspecting the conditional expectation of the outcome given the forcing variable separately by treatment status. We recommend that researchers present graphs containing estimates of these conditional expectations and test for continuity at the threshold.

Appendix

Proof of Lemma 2: Under independence of Z_i and $(Y_i(0), Y_i(1), G_i)$, and the absence of defiers, the standard LATE result is that

$$\text{plim}(\hat{\tau}_{\text{iv}}) = \mathbb{E}[Y_i(1)|G_i = \text{c}] - \mathbb{E}[Y_i(0)|G_i = \text{c}].$$

Next, consider the first term in $\text{plim}(\hat{\tau}_{\text{ols}})$, $\mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 1]$:

$$\begin{aligned}
&\mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 1] = \mathbb{E}[Y_i(1)|W_i^{\text{obs}} = 1] \\
&\quad = \mathbb{E}[Y_i(1)|W_i^{\text{obs}} = 1, G_i = \text{a}] \cdot \text{pr}(G_i = \text{a}|W^{\text{obs}} = 1) \\
&\qquad + \mathbb{E}[Y_i(1)|W_i^{\text{obs}} = 1, G_i = \text{c}] \cdot \text{pr}(G_i = \text{c}|W^{\text{obs}} = 1) \\
&\quad = \mathbb{E}[Y_i(1)|G_i = \text{a}] \cdot \text{pr}(G_i = \text{a}|W^{\text{obs}} = 1) + \mathbb{E}[Y_i(1)|G_i = \text{c}] \cdot \text{pr}(G_i = \text{c}|W^{\text{obs}} = 1) \\
&\quad = \mathbb{E}[Y_i(1)|G_i = \text{a}] \cdot \text{pr}(G_i = \text{a}|W^{\text{obs}} = 1, Z_i = 1) \cdot \text{pr}(Z_i = 1|W_i^{\text{obs}} = 1) \\
&\qquad + \mathbb{E}[Y_i(1)|G_i = \text{a}] \cdot \text{pr}(G_i = \text{a}|W^{\text{obs}} = 1, Z_i = 0) \cdot \text{pr}(Z_i = 0|W_i^{\text{obs}} = 1) \\
&\qquad + \mathbb{E}[Y_i(1)|G_i = \text{c}] \cdot \text{pr}(G_i = \text{c}|W^{\text{obs}} = 1, Z_i = 1) \cdot \text{pr}(Z_i = 1|W_i^{\text{obs}} = 1) \\
&\qquad + \mathbb{E}[Y_i(1)|G_i = \text{c}] \cdot \text{pr}(G_i = \text{c}|W^{\text{obs}} = 1, Z_i = 0) \cdot \text{pr}(Z_i = 0|W_i^{\text{obs}} = 1) \\
&\quad = \mathbb{E}[Y_i(1)|G_i = \text{a}] \cdot \frac{\pi_{\text{a}}}{\pi_{\text{a}} + \pi_{\text{c}}} \cdot \frac{\text{pr}(W_i^{\text{obs}} = 1|Z_i = 1) \cdot \text{pr}(Z_i = 1)}{\text{pr}(W_i^{\text{obs}} = 1)} \\
&\qquad + \mathbb{E}[Y_i(1)|G_i = \text{a}] \cdot \frac{\text{pr}(W_i^{\text{obs}} = 1|Z_i = 0) \cdot \text{pr}(Z_i = 0)}{\text{pr}(W_i^{\text{obs}} = 1)} \\
&\qquad + \mathbb{E}[Y_i(1)|G_i = \text{c}] \cdot \frac{\pi_{\text{c}}}{\pi_{\text{a}} + \pi_{\text{c}}} \cdot \frac{\text{pr}(W_i^{\text{obs}} = 1|Z_i = 1) \cdot \text{pr}(Z_i = 1)}{\text{pr}(W_i^{\text{obs}} = 1)} \\
&\qquad + 0 \\
&\quad = \mathbb{E}[Y_i(1)|G_i = \text{a}] \cdot \frac{\pi_{\text{a}}}{\pi_{\text{a}} + \pi_{\text{c}}} \cdot \frac{(\pi_{\text{a}} + \pi_{\text{c}}) \cdot p_z}{\pi_{\text{a}} + \pi_{\text{c}} \cdot p_z} \\
&\qquad + \mathbb{E}[Y_i(1)|G_i = \text{a}] \cdot \frac{\pi_{\text{a}} \cdot (1 - p_z)}{\pi_{\text{a}} + \pi_{\text{c}} \cdot p_z} \\
&\qquad + \mathbb{E}[Y_i(1)|G_i = \text{c}] \cdot \frac{\pi_{\text{c}}}{\pi_{\text{a}} + \pi_{\text{c}}} \cdot \frac{(\pi_{\text{a}} + \pi_{\text{c}}) \cdot p_z}{\pi_{\text{a}} + \pi_{\text{c}} \cdot p_z} \\
&\quad = \mathbb{E}[Y_i(1)|G_i = \text{a}] \cdot \frac{\pi_{\text{a}}}{\pi_{\text{a}} + \pi_{\text{c}} \cdot p_z} + \mathbb{E}[Y_i(1)|G_i = \text{c}] \cdot \frac{\pi_{\text{c}} \cdot p_z}{\pi_{\text{a}} + \pi_{\text{c}} \cdot p_z}.
\end{aligned}$$

By a similar argument,

$$\mathbb{E}[Y_i^{\text{obs}}|W_i = 0] = \mathbb{E}[Y_i(0)|G_i = \text{n}] \cdot \frac{\pi_{\text{n}}}{\pi_{\text{n}} + \pi_{\text{c}} \cdot (1 - p_z)} + \mathbb{E}[Y_i(0)|G_i = \text{c}] \cdot \frac{\pi_{\text{c}} \cdot (1 - p_z)}{\pi_{\text{n}} + \pi_{\text{c}} \cdot (1 - p_z)}.$$

Then,

$$\text{plim}(\hat{\tau}_{\text{ols}}) - \text{plim}(\hat{\tau}_{\text{iv}})$$

$$
\begin{aligned}
&= \mathbb{E}[Y_i(1)|G_i = \mathrm{a}] \cdot \frac{\pi_\mathrm{a}}{\pi_\mathrm{a} + \pi_\mathrm{c} \cdot p_z} + \mathbb{E}[Y_i(1)|G_i = \mathrm{c}] \cdot \frac{\pi_\mathrm{c} \cdot p_z}{\pi_\mathrm{a} + \pi_\mathrm{c} \cdot p_z} - \mathbb{E}[Y_i(1)|G_i = \mathrm{c}] \\
&- \mathbb{E}[Y_i(0)|G_i = \mathrm{n}] \cdot \frac{\pi_\mathrm{n}}{\pi_\mathrm{n} + \pi_\mathrm{c} \cdot (1 - p_z)} + \mathbb{E}[Y_i(0)|G_i = \mathrm{c}] \cdot \frac{\pi_\mathrm{c} \cdot (1 - p_z)}{\pi_\mathrm{n} + \pi_\mathrm{c} \cdot (1 - p_z)} - \mathbb{E}[Y_i(0)|G_i = \mathrm{c}] \\
&= \mathbb{E}[Y_i(1)|G_i = \mathrm{a}] \cdot \frac{\pi_\mathrm{a}}{\pi_\mathrm{a} + \pi_\mathrm{c} \cdot p_z} - \mathbb{E}[Y_i(1)|G_i = \mathrm{c}] \cdot \frac{\pi_\mathrm{a}}{\pi_\mathrm{a} + \pi_\mathrm{c} \cdot p_z} \\
&\qquad - \left(\mathbb{E}[Y_i(0)|G_i = \mathrm{n}] \cdot \frac{\pi_\mathrm{n}}{\pi_\mathrm{n} + \pi_\mathrm{c} \cdot (1 - p_z)} - \mathbb{E}[Y_i(0)|G_i = \mathrm{c}] \cdot \frac{\pi_\mathrm{n}}{\pi_\mathrm{n} + \pi_\mathrm{c} \cdot (1 - p_z)} \right) \\
&= \Big(\mathbb{E}[Y_i(1)|G_i = \mathrm{a}] - \mathbb{E}[Y_i(1)|G_i = \mathrm{c}] \Big) \cdot \frac{\pi_\mathrm{a}}{\pi_\mathrm{a} + \pi_\mathrm{c} \cdot p_z} \\
&\qquad - \Big(\mathbb{E}[Y_i(0)|G_i = \mathrm{n}] - \mathbb{E}[Y_i(0)|G_i = \mathrm{c}] \Big) \cdot \frac{\pi_\mathrm{n}}{\pi_\mathrm{n} + \pi_\mathrm{c} \cdot (1 - p_z)}.
\end{aligned}
$$

$\square$

Proof of Lemma 3:
By the arguments in Hahn, Todd and Van Der Klaauw (2001),

$$
\tau^{\mathrm{frd}} = \mathbb{E}[Y_i(1)|G_i = \mathrm{c}, X_i = T^\star] - \mathbb{E}[Y_i(0)|G_i = \mathrm{c}, X_i = T^\star].
$$

Define the following events and probabilities:

$$
\begin{aligned}
&A_{i,h} = \{T^\star - h < X_i < T^\star + h\}, \\
&A_{i,h}^{+} = \{T^\star < X_i < T^\star + h\}, \\
&A_{i,h}^{-} = \{T^\star - h < X_i \leq T^\star\}, \\
&\pi_\mathrm{a} = \pi_\mathrm{a}(T^\star) = \mathrm{pr}(G_i = \mathrm{a}|X_i = T^\star), \\
&\pi_\mathrm{c} = \pi_\mathrm{c}(T^\star) = \mathrm{pr}(G_i = \mathrm{c}|X_i = T^\star),
\end{aligned}
$$

and

$$
\pi_\mathrm{n} = \pi_\mathrm{n}(T^\star) = \mathrm{pr}(G_i = \mathrm{n}|X_i = T^\star).
$$

Next, consider the first term in τ_{exo}, $\lim_{h\downarrow 0} \mathbb{E}[Y_i^{\mathrm{obs}}|W_i^{\mathrm{obs}} = 1, A_{i,h}]$ without the limit operator. Following the same argument as in the proof of Lemma 2, this is equal to:

$$
\begin{aligned}
&\mathbb{E}[Y_i^{\mathrm{obs}}|W_i^{\mathrm{obs}} = 1, A_{i,h}] = \mathbb{E}[Y_i(1)|W_i^{\mathrm{obs}} = 1, A_{i,h}] \\
&= \mathbb{E}[Y_i(1)|W_i^{\mathrm{obs}} = 1, G_i = \mathrm{a}, A_{i,h}] \cdot \mathrm{pr}(G_i = \mathrm{a}|W_i^{\mathrm{obs}} = 1, A_{i,h}) \\
&\qquad + \mathbb{E}[Y_i(1)|W_i^{\mathrm{obs}} = 1, G_i = \mathrm{c}, A_{i,h}] \cdot \mathrm{pr}(G_i = \mathrm{c}|W_i^{\mathrm{obs}} = 1, A_{i,h}) \\
&= \mathbb{E}[Y_i(1)|G_i = \mathrm{a}, A_{i,h}] \cdot \mathrm{pr}(G_i = \mathrm{a}|W_i^{\mathrm{obs}} = 1, A_{i,h}) \\
&\qquad + \mathbb{E}[Y_i(1)|G_i = \mathrm{c}, A_{i,h}^{-}] \cdot \mathrm{pr}(G_i = \mathrm{c}|W_i^{\mathrm{obs}} = 1, A_{i,h})
\end{aligned}
$$

$$
= \mathbb{E}[Y_i(1)|G_i = \mathrm{a}, A_{i,h}] \cdot \mathrm{pr}(G_i = \mathrm{a}|W_i^{\mathrm{obs}} = 1, A_{i,h}^{-}) \cdot \mathrm{pr}(A_{i,h}^{-}|W_i^{\mathrm{obs}} = 1, A_{i,h})
$$

$$+\mathbb{E}[Y_i(1)|G_i=\mathrm{a},A_{i,h}]\cdot \mathrm{pr}(G_i=\mathrm{a}|W_i^{\mathrm{obs}}=1,A_{i,h}^{+})\cdot \mathrm{pr}(A_{i,h}^{+}|W_i^{\mathrm{obs}}=1,A_{i,h})$$
$$+\mathbb{E}[Y_i(1)|G_i=\mathrm{c},A_{i,h}^{-}]\cdot \mathrm{pr}(G_i=\mathrm{c}|W_i^{\mathrm{obs}}=1,A_{i,h}^{-})\cdot \mathrm{pr}(A_{i,h}^{-}|W_i^{\mathrm{obs}}=1,A_{i,h})$$
$$+\mathbb{E}[Y_i(1)|G_i=\mathrm{c},A_{i,h}^{-}]\cdot \mathrm{pr}(G_i=\mathrm{c}|W_i^{\mathrm{obs}}=1,A_{i,h}^{+})\cdot \mathrm{pr}(A_{i,h}^{+}|W_i^{\mathrm{obs}}=1,A_{i,h})$$

$$\begin{aligned}
=\ &\mathbb{E}[Y_i(1)|G_i=\mathrm{a},A_{i,h}]\cdot \mathrm{pr}(G_i=\mathrm{a}|G_i\in\{\mathrm{a},\mathrm{c}\},A_{i,h}^{-})\\
&\qquad\cdot\frac{\mathrm{pr}(W_i^{\mathrm{obs}}=1|A_{i,h}^{-})\mathrm{pr}(A_{i,h}^{-})}{\sum_{s\in\{-,+\}}\mathrm{pr}(W_i^{\mathrm{obs}}=1|A_{i,h}^{s})\mathrm{pr}(A_{i,h}^{s})}\\
&+\mathbb{E}[Y_i(1)|G_i=\mathrm{a},A_{i,h}]\cdot \mathrm{pr}(G_i=\mathrm{a}|G_i=\mathrm{a},A_{i,h}^{+})\\
&\qquad\cdot\frac{\mathrm{pr}(W_i^{\mathrm{obs}}=1|A_{i,h}^{+})\mathrm{pr}(A_{i,h}^{+})}{\sum_{s\in\{-,+\}}\mathrm{pr}(W_i^{\mathrm{obs}}=1|A_{i,h}^{s})\mathrm{pr}(A_{i,h}^{s})}\\
&+\mathbb{E}[Y_i(1)|G_i=\mathrm{c},A_{i,h}^{-}]\cdot \mathrm{pr}(G_i=\mathrm{c}|G_i\in\{\mathrm{a},\mathrm{c}\},A_{i,h}^{-})\\
&\qquad\cdot\frac{\mathrm{pr}(W_i^{\mathrm{obs}}=1|A_{i,h}^{-})\mathrm{pr}(A_{i,h}^{-})}{\sum_{s\in\{-,+\}}\mathrm{pr}(W_i^{\mathrm{obs}}=1|A_{i,h}^{s})\mathrm{pr}(A_{i,h}^{s})}\\
&+\mathbb{E}[Y_i(1)|G_i=\mathrm{c},A_{i,h}^{-}]\cdot \mathrm{pr}(G_i=\mathrm{c}|G_i=\mathrm{a},A_{i,h}^{+})\\
&\qquad\cdot\frac{\mathrm{pr}(W_i^{\mathrm{obs}}=1|A_{i,h}^{+})\mathrm{pr}(A_{i,h}^{+})}{\sum_{s\in\{-,+\}}\mathrm{pr}(W_i^{\mathrm{obs}}=1|A_{i,h}^{s})\mathrm{pr}(A_{i,h}^{s})}
\end{aligned}$$

$$\begin{aligned}
=\ &\mathbb{E}[Y_i(1)|G_i=\mathrm{a},A_{i,h}]\cdot \mathrm{pr}(G_i=\mathrm{a}|G_i\in\{\mathrm{a},\mathrm{c}\},A_{i,h}^{-})\\
&\qquad\cdot\frac{\mathrm{pr}(G_i\in\{\mathrm{a},\mathrm{c}\}|A_{i,h}^{-})}{\mathrm{pr}(G_i\in\{\mathrm{a},\mathrm{c}\}|A_{i,h}^{-})+\mathrm{pr}(G_i=\mathrm{a}|A_{i,h}^{+})\mathrm{pr}(A_{i,h}^{+})/\mathrm{pr}(A_{i,h}^{-})}\\
&+\mathbb{E}[Y_i(1)|G_i=\mathrm{a},A_{i,h}]\cdot 1\\
&\qquad\cdot\frac{\mathrm{pr}(G_i=\mathrm{a}|A_{i,h}^{+})}{\mathrm{pr}(G_i\in\{\mathrm{a},\mathrm{c}\}|A_{i,h}^{-})\mathrm{pr}(A_{i,h}^{-})/\mathrm{pr}(A_{i,h}^{+})+\mathrm{pr}(G_i=\mathrm{a}|A_{i,h}^{+})}\\
&+\mathbb{E}[Y_i(1)|G_i=\mathrm{c},A_{i,h}^{-}]\cdot \mathrm{pr}(G_i=\mathrm{c}|G_i\in\{\mathrm{a},\mathrm{c}\},A_{i,h}^{-})\\
&\qquad\cdot\frac{\mathrm{pr}(G_i\in\{\mathrm{a},\mathrm{c}\}|A_{i,h}^{-})}{\mathrm{pr}(G_i\in\{\mathrm{a},\mathrm{c}\}|A_{i,h}^{-})+\mathrm{pr}(G_i=\mathrm{a}|A_{i,h}^{+})\mathrm{pr}(A_{i,h}^{+})/\mathrm{pr}(A_{i,h}^{-})}\\
&+0
\end{aligned}$$

Now, take the limit as $h\downarrow 0$. By assumption, the conditional mean of potential outcomes given each compliance type and the forcing variable is a continuous function of the forcing variable; also, the conditional probability of each compliance type given the forcing variable is a continuous function of the forcing variable; lastly, continuity of the density of the forcing variable at the threshold implies $\lim_{h\downarrow 0}\mathrm{pr}(A_{i,h}^{-})/\mathrm{pr}(A_{i,h}^{+})=1$. Therefore,

$$\lim_{h\downarrow 0} \mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 1, A_{i,h}]$$

$$= \mathbb{E}[Y_i(1)|G_i = \text{a}, X_i = T^\star] \cdot \frac{\pi_a}{\pi_a + \pi_c} \cdot \frac{\pi_a + \pi_c}{2\pi_a + \pi_c}$$
$$+\mathbb{E}[Y_i(1)|G_i = \text{a}, X_i = T^\star] \cdot \frac{\pi_a}{2\pi_a + \pi_c}$$
$$+\mathbb{E}[Y_i(1)|G_i = \text{c}, X_i = T^\star] \cdot \frac{\pi_c}{\pi_a + \pi_c} \cdot \frac{\pi_a + \pi_c}{2\pi_a + \pi_c}$$

$$= \mathbb{E}[Y_i(1)|G_i = \text{a}, X_i = T^\star] \cdot \frac{\pi_a}{\pi_a + \pi_c/2} + \mathbb{E}[Y_i(1)|G_i = \text{c}, X_i = T^\star] \cdot \frac{\pi_c/2}{\pi_a + \pi_c/2}$$

By a similar argument

$$\lim_{h\downarrow 0} \mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 0, A_{i,h}]$$

$$= \mathbb{E}[Y_i(0)|G_i = \text{n}, X_i = T^\star] \cdot \frac{\pi_{\text{n}}}{\pi_{\text{n}} + \pi_{\text{c}}/2} + \mathbb{E}[Y_i(0)|G_i = \text{c}, X_i = T^\star] \cdot \frac{\pi_{\text{c}}/2}{\pi_{\text{n}} + \pi_{\text{c}}/2}$$

Finally,

$$\tau_{\text{exo}} - \tau^{\text{frd}}$$

$$= \mathbb{E}[Y_i(1)|G_i = \text{a}, X_i = T^\star] \cdot \frac{\pi_a}{\pi_a + \pi_c/2} + \mathbb{E}[Y_i(1)|G_i = \text{c}, X_i = T^\star] \cdot \frac{\pi_c/2}{\pi_a + \pi_c/2}$$
$$-\mathbb{E}[Y_i(0)|G_i = \text{n}, X_i = T^\star] \cdot \frac{\pi_{\text{n}}}{\pi_{\text{n}} + \pi_{\text{c}}/2} - \mathbb{E}[Y_i(0)|G_i = \text{c}, X_i = T^\star] \cdot \frac{\pi_{\text{c}}/2}{\pi_{\text{n}} + \pi_{\text{c}}/2}$$
$$-\mathbb{E}[Y_i(1)|G_i = \text{c}, X_i = T^\star] + \mathbb{E}[Y_i(0)|G_i = \text{c}, X_i = T^\star]$$

$$\tau_{\text{exo}} - \tau^{\text{frd}}$$

$$= \frac{\pi_a}{\pi_a + \pi_c/2} \left(\mathbb{E}[Y_i(1)|G_i = \text{a}, X_i = T^\star]\right) - \mathbb{E}[Y_i(1)|G_i = \text{c}, X_i = T^\star]$$
$$-\frac{\pi_n}{\pi_n + \pi_c/2} \left(\mathbb{E}[Y_i(0)|G_i = \text{n}, X_i = T^\star] - \mathbb{E}[Y_i(0)|G_i = \text{c}, X_i = T^\star]\right)$$

□

Proof of Lemma 4: The equality of $H_0^{A,\text{frd}\prime}$ and $H_0^{A,\text{frd}\prime\prime}$ is immediate. The proof therefore focuses on the equality of $H_0^{A,\text{frd}\prime}$ and $H_0^{A,\text{frd}}$. As shown before,

$$\tau^{\text{frd}} = \mathbb{E}[Y_i(1)|G_i = \text{c}, X_i = T^\star] - \mathbb{E}[Y_i(0)|G_i = \text{c}, X_i = T^\star],$$

so all that remains to be shown is

$$\lim_{h\downarrow 0} \mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 1, T^\star < X_i < T^\star + h] - \mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 0, T^\star - h < X_i < T^\star]$$

$$= \mathbb{E}[Y_i(1)|G_i = \text{a}, X_i = T^\star] - \mathbb{E}[Y_i(0)|G_i = \text{n}, X_i = T^\star],$$

By definition,

$$\lim_{h\downarrow 0} \mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 1, T^\star < X_i < T^\star + h] - \mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 0, T^\star - h < X_i < T^\star]$$

$$= \lim_{h\downarrow 0} \mathbb{E}[Y_i(1)|W_i^{\text{obs}} = 1, T^\star < X_i < T^\star + h] - \mathbb{E}[Y_i(0)|W_i^{\text{obs}} = 0, T^\star - h < X_i < T^\star].$$

This is equal to

$$\lim_{h\downarrow 0} \mathbb{E}[Y_i(1)|W_i^{\text{obs}} = 1, G_i = \text{a}, T^\star < X_i < T^\star + h] - \mathbb{E}[Y_i(0)|W_i^{\text{obs}} = 0, G_i = \text{n}, T^\star - h < X_i < T^\star].$$

$$= \lim_{h\downarrow 0} \mathbb{E}[Y_i(1)|G_i = \text{a}, T^\star < X_i < T^\star + h] - \mathbb{E}[Y_i(0)|G_i = \text{n}, T^\star - h < X_i < T^\star].$$

$$= \mathbb{E}[Y_i(1)|G_i = \text{a}, X_i = T^\star] - \mathbb{E}[Y_i(0)|G_i = \text{n}, X_i = T^\star],$$

which finishes the proof. $\square$

Proof of Lemma 5: First, notice that

$$\tau^{\text{ate}} = \mathbb{E}\left[\mathbb{E}[Y_i(1) - Y_i(0)|X_i]\right]$$

so that it suffices to show that $\mathbb{E}[Y_i(1) - Y_i(0)|X_i = x]$ is identified for every $x \in \mathbb{X}$ (support of X_i) because the distribution of X_i is identified.

Fix an arbitrary $x \in \mathbb{X}$. By external validity (assumption 5), it follows that :

$$\mathbb{E}[Y_i(0)|G_i = g, X_i = x] = \mathbb{E}[Y_i(0)|X_i = x] \quad \forall g \in \{\text{n}, \text{a}, \text{c}\} \tag{A.1}$$

$$\mathbb{E}[Y_i(1)|G_i = g, X_i = x] = \mathbb{E}[Y_i(1)|X_i = x] \quad \forall g \in \{\text{n}, \text{a}, \text{c}\} \tag{A.2}$$

Case I: $x \leq T^\star$

Using the definitions of G_i and W_i^{obs}:

$$G_i \in \{\text{c}, \text{a}\} \Longleftrightarrow W_i^{\text{obs}} = 1 \;\Big|\; X_i = x \tag{A.3}$$

$$G_i = \text{n} \Longleftrightarrow W_i^{\text{obs}} = 0 \;\Big|\; X_i = x \tag{A.4}$$

Consider

$$\mathbb{E}[Y_i^{\text{obs}}|W_i^{\text{obs}} = 0, X_i = x]$$

$$= \mathbb{E}[Y_i(0)|W_i^{\text{obs}} = 0, X_i = x]$$

$$
\begin{aligned}
&= \mathbb{E}[Y_i(0)|G_i = \mathrm{n}, X_i = x] \\
&= \mathbb{E}[Y_i(0)|X_i = x]
\end{aligned}
$$

where we used (A.4) in the second equality, and (A.1) in the third equality.

Next, consider

$$
\begin{aligned}
&\mathbb{E}[Y_i^{\mathrm{obs}}|W_i^{\mathrm{obs}} = 1, X_i = x] \\
&\quad = \mathbb{E}[Y_i(1)|W_i^{\mathrm{obs}} = 1, X_i = x] \\
&\quad = \mathbb{E}[Y_i(1)|G_i \in \{\mathrm{c}, \mathrm{a}\}, X_i = x] \\
&\quad = \mathbb{E}[Y_i(1)|G_i = \mathrm{c}, X_i = x] \cdot \mathrm{pr}(G_i = \mathrm{c}|G_i \in \{\mathrm{c}, \mathrm{a}\}, X_i = x) \\
&\qquad\quad + \mathbb{E}[Y_i(1)|G_i = \mathrm{a}, X_i = x] \cdot \mathrm{pr}(G_i = \mathrm{a}|G_i \in \{\mathrm{c}, \mathrm{a}\}, X_i = x) \\
&\quad = \mathbb{E}[Y_i(1)|X_i = x]
\end{aligned}
$$

where we used (A.3) in the second equality, and (A.2) in the fourth equality.

Therefore, $\mathbb{E}[Y_i(1) - Y_i(0)|X_i = x]$ is identified in case I.

Case II: $x > T^\star$
Again, by the definitions of G_i and W_i^{obs}:

$$
G_i = \mathrm{a} \Longleftrightarrow W_i^{\mathrm{obs}} = 1 \;\Big|\; X_i = x \tag{A.5}
$$

$$
G_i \in \{\mathrm{n}, \mathrm{c}\} \Longleftrightarrow W_i^{\mathrm{obs}} = 0 \;\Big|\; X_i = x \tag{A.6}
$$

Consider

$$
\begin{aligned}
&\mathbb{E}[Y_i^{\mathrm{obs}}|W_i^{\mathrm{obs}} = 0, X_i = x] \\
&\quad = \mathbb{E}[Y_i(0)|W_i^{\mathrm{obs}} = 0, X_i = x] \\
&\quad = \mathbb{E}[Y_i(0)|G_i \in \{\mathrm{n}, \mathrm{c}\}, X_i = x] \\
&\quad = \mathbb{E}[Y_i(0)|G_i = \mathrm{n}, X_i = x] \cdot \mathrm{pr}(G_i = \mathrm{n}|G_i \in \{\mathrm{n}, \mathrm{c}\}, X_i = x) \\
&\qquad\quad + \mathbb{E}[Y_i(0)|G_i = \mathrm{c}, X_i = x] \cdot \mathrm{pr}(G_i = \mathrm{c}|G_i \in \{\mathrm{n}, \mathrm{c}\}, X_i = x) \\
&\quad = \mathbb{E}[Y_i(0)|X_i = x]
\end{aligned}
$$

where we used (A.6) in the second equality, and (A.1) in the fourth equality.

Next, consider

$$
\begin{aligned}
&\mathbb{E}[Y_i^{\mathrm{obs}}|W_i^{\mathrm{obs}} = 1, X_i = x] \\
&\quad = \mathbb{E}[Y_i(1)|W_i^{\mathrm{obs}} = 1, X_i = x] \\
&\quad = \mathbb{E}[Y_i(1)|G_i = \mathrm{a}, X_i = x] \\
&\quad = \mathbb{E}[Y_i(1)|X_i = x]
\end{aligned}
$$

where we used (A.5) in the second equality, and (A.2) in the third equality.

Therefore, $\mathbb{E}[Y_i(1) - Y_i(0)|X_i = x]$ is identified in case II.
□

Proof of Lemma 6: The claims follow directly from the proof of Lemma 5. □

References

Angrist, J., (2004), "Treatment Effect Heterogeneity in Theory and Practice," , *The Economic Journal* 114, C52-C83.

Angrist, J., and I. Fernandez-Val, (2010), "ExtrapoLATE-ing: External Validity and Overidentification in the LATE Framework", NBER Working Paper 16566.

Angrist, J.D., G.W. Imbens, and D.B. Rubin, (1996), Identification of Causal Effects Using Instrumental Variables, *Journal of the American Statistical Association* 91, 444–472.

Angrist, J.D. and A.B. Krueger, (1991), Does Compulsory School Attendance Affect Schooling and Earnings?, *Quarterly Journal of Economics* 106, 979-1014.

Angrist, J.D., and V. Lavy, (1999), Using Maimonides' Rule to Estimate the Effect of Class Size on Scholastic Achievement", *Quarterly Journal of Economics* 114, 533-575.

Angrist, J., and M. Rokkanen, (2012), "Wanna Get Away? RD Identification Away from the Cutoff", NBER Working Paper 18662.

Battistin, E. and E. Rettore, (2008), "Ineligibles and Eligible Non-Participants as a Double Comparison Group in Regression-Discontinuity Designs," *Journal of Econometrics* 142(2), 715-730

Berk, R., and D. Rauma, (1983), "Capitalizing on Nonrandom Assignment to Treatments: A Regression-Discontinuity Evaluation of a Crime-Control Program," *Journal of the American Statistical Association*, Vol. 78(381): 21-27.

Bertanha, M., (2014), "Regression Discontinuity Design with Many Thresholds," Unpublished Manuscript, Department of Economics, Stanford.

Black, S., (1999), Do Better Schools Matter? Parental Valuation of Elementary Education, *Quarterly Journal of Economics* 114, 577-599.

Calonico, S., M. Cattaneo, and R. Titiunik, (2014a), "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs" forthcoming, *Econometrica.*

Calonico, S., M. Cattaneo, and R. Titiunik, (2014b), "Optimal Data-Driven Regression Discontinuity Plots." Unpublished Working Paper.

Calonico, S., M. Cattaneo, and R. Titiunik, (2014c), "Robust Data-Driven Inference in the Regression-Discontinuity Design" forthcoming, *Stata Journal.*

Cattaneo, M., B. Frandsen, and R. Titiunik, (2014), "Randomization Inference in the Regression Discontinuity Design: An Application to Party Advantages in the U.S. Senate" forthcoming, *Journal of Causal Inference.*

Cook, T., (2008), "Waiting for Life to Arrive: A History of the Regression-Discontinuity Design in Psychology, Statistics and Economics." *Journal of Econometrics*, 142(2): 636-54.

Dong, Y., (2014), "An Alternative Assumption to Identify LATE in Regression Discontinuity Designs," Unpublished Manuscript.

Dong, Y., and A. Lewbel (2014): "Identifying the Effect of Changing the Policy Threshold in Regression Discontinuity Models" forthcoming, *Review of Economics and Statistics*.

Gelman, A., and G. Imbens, , (2014), "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs," NBER Working Paper 20405.

Hahn, J., P. Todd and W. Van der Klaauw, (2001), Identification and Estimation of Treatment Effects with a Regression Discontinuity Design, *Econometrica* 69, 201-209.

Imbens, G., and J. Angrist, (1994), "Identification and Estimation of Local Average Treatment Effects", *Econometrica* 61, 467-476.

Imbens, G., and K. Kalyanaraman, (2012), "Optimal Bandwidth Choice for the Regression Discontinuity Estimator," *Review of Economic Studies* 79(3), 933-959

Imbens, G., and T. Lemieux, (2008), "Regression Discontinuity Designs: A Guide to Practice," *Journal of Econometrics*, Vol. 142(2): 615-35.

Jacob, B.A., and L. Lefgren, (2004), Remedial Education and Student Achievement: A Regression-Discontinuity Analysis, *Review of Economics and Statistics* 68, 226-244.

Lalive, R., (2008), How do Extended Benefits affect Unemployment Duration? A Regression Discontinuity Approach, *Journal of Econometrics*, 142(2), 785-806

Lee, D., (2008), "Randomized Experiments from Non-random Selection in U.S. House Elections," *Journal of Econometrics*, 142(2) 675697

Lee, D., and T. Lemieux, (2010), "Regression Discontinuity Designs in Economics," *Journal of Economic Literature*, Vol. 48, 281-355.

Ludwig, J., and D. Miller, (2005), Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design, NBER working paper 11702.

Ludwig, J., and D. Miller, (2007), Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design, *Quarterly Journal of Economics* 122(1), 159-208.

Matsudaira, J., (2008), "Mandatory Summer School and Student Achievement", *Journal of Econometrics*, 142(2) 829850.

McCrary, J., (2008), "Testing for Manipulation of the Running Variable in the Regression Discontinuity Design," *Journal of Econometrics*, 142(2) 698714.

Porter, J., (2003), Estimation in the Regression Discontinuity Model," mimeo, Department of Economics, University of Wisconsin, http://www.ssc.wisc.edu/ jporter/reg_discont_2003.pdf.

Rubin, D., (1974), Estimating Causal Effects of Treatments in Randomized and Non-randomized Studies, *Journal of Educational Psychology* 66, 688-701.

Thistlewaite, D., and D. Campbell, (1960), Regression-Discontinuity Analysis: An Alternative to the Ex-Post Facto Experiment, *Journal of Educational Psychology* 51, 309-317.

Trochim, W., (1984), *Research Design for Program Evaluation; The Regression-Discontinuity Design* (Sage Publications, Beverly Hills, CA).

Trochim, W., (2001), Regression-Discontinuity Design, in N.J. Smelser and P.B Baltes, eds., *International Encyclopedia of the Social and Behavioral Sciences* 19 (Elsevier North-Holland, Amsterdam) 12940-12945.

Van Der Klaauw, W., (2002), Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach, *International Economic Review* 43, 1249-1287.

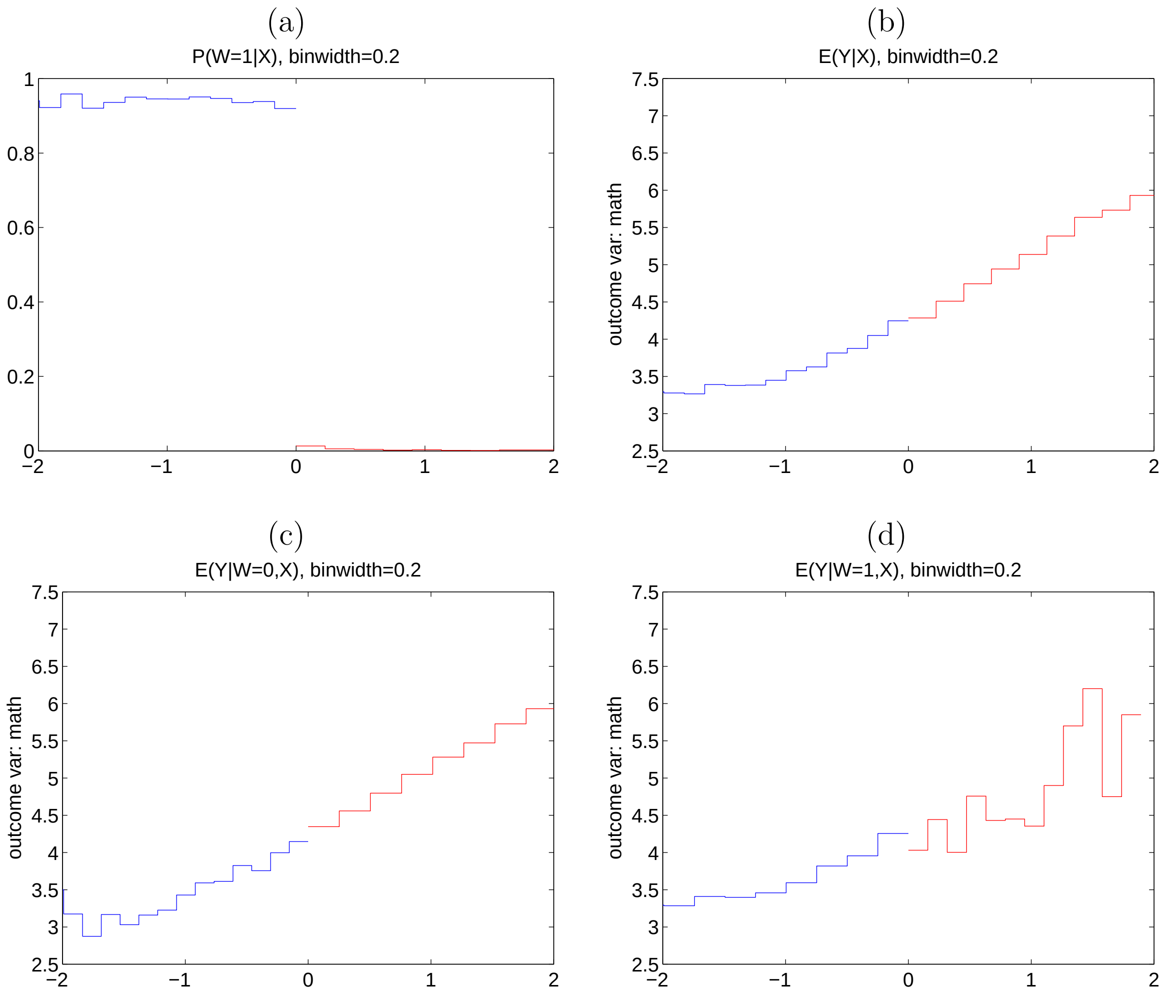
Van Der Klaauw, W., (2008) "Regression-Discontinuity Analysis: A Survey of Recent Developments in Economics." *Labour,* Vol. 22(2): 21945.

Table 1: Jacob-Lefgren Data

	Estimand	Estimate	s.e.	p-value
Estimates				
Participation	π_c	0.894	(0.006)	
	π_a	0.016	(0.002)	
	π_n	0.090	(0.005)	
Cond. Means of Potential Outcomes	$\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = c]$	4.394	(0.016)	
	$\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = a]$	4.037	(0.124)	
	$\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = c]$	4.197	(0.017)	
	$\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = n]$	4.267	(0.048)	
LATE	$\mathbb{E}[Y_i(1) - Y_i(0)\|X_i = T^*, G_i = c]$	0.197	(0.025)	
OLS	$\lim_{h\downarrow 0} \mathbb{E}[Y_i^{obs}\|W_i^{obs} = 1, T^* - h < X_i < T^* + h]$ $- \lim_{h\downarrow 0} \mathbb{E}[Y_i^{obs}\|W_i^{obs} = 0, T^* - h < X_i < T^* + h]$	0.173	(0.021)	
Tests				
Hausman	$\frac{\pi_a}{\pi_a+\pi_c/2} \cdot \Big(\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(1)\|X_i = T^*, G_i = a]\Big)$ $- \frac{\pi_n}{\pi_n+\pi_c/2} \cdot \Big(\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(0)\|X_i = T^*, G_i = n]\Big)$	0.024	(0.011)	0.026
Angrist	$\Big(\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(1)\|X_i = T^*, G_i = a]\Big)$ $-\Big(\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(0)\|X_i = T^*, G_i = n]\Big)$	0.427	(0.139)	0.002
Conditional Jumps	$\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(1)\|X_i = T^*, G_i = a]$	0.357	(0.126)	0.005
	$\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(0)\|X_i = T^*, G_i = n]$	-0.070	(0.056)	0.213
Joint F-test		9.431		0.009

Notes: Outcome variable Y is the math score (after the summer school), and forcing variable X is the minimum between the reading and math score (before the summer school) minus the cutoff; W is the participation indicator. Non-parametric estimates are obtained by local linear regression and optimal bandwidth $h = 0.54$ by Imbens and Kalyanaraman (2012). The standard errors of all estimates are computed using 1000 bootstrap iterations. The F-test is a χ^2 statistic computed as a quadratic form of the 2×1 vector of the differences between potential outcomes for treated compliers and always-takers and for non-treated compliers and never-takers inversely weighted by the covariance matrix of such vector.

Figure 1: Jacob and Lefgren



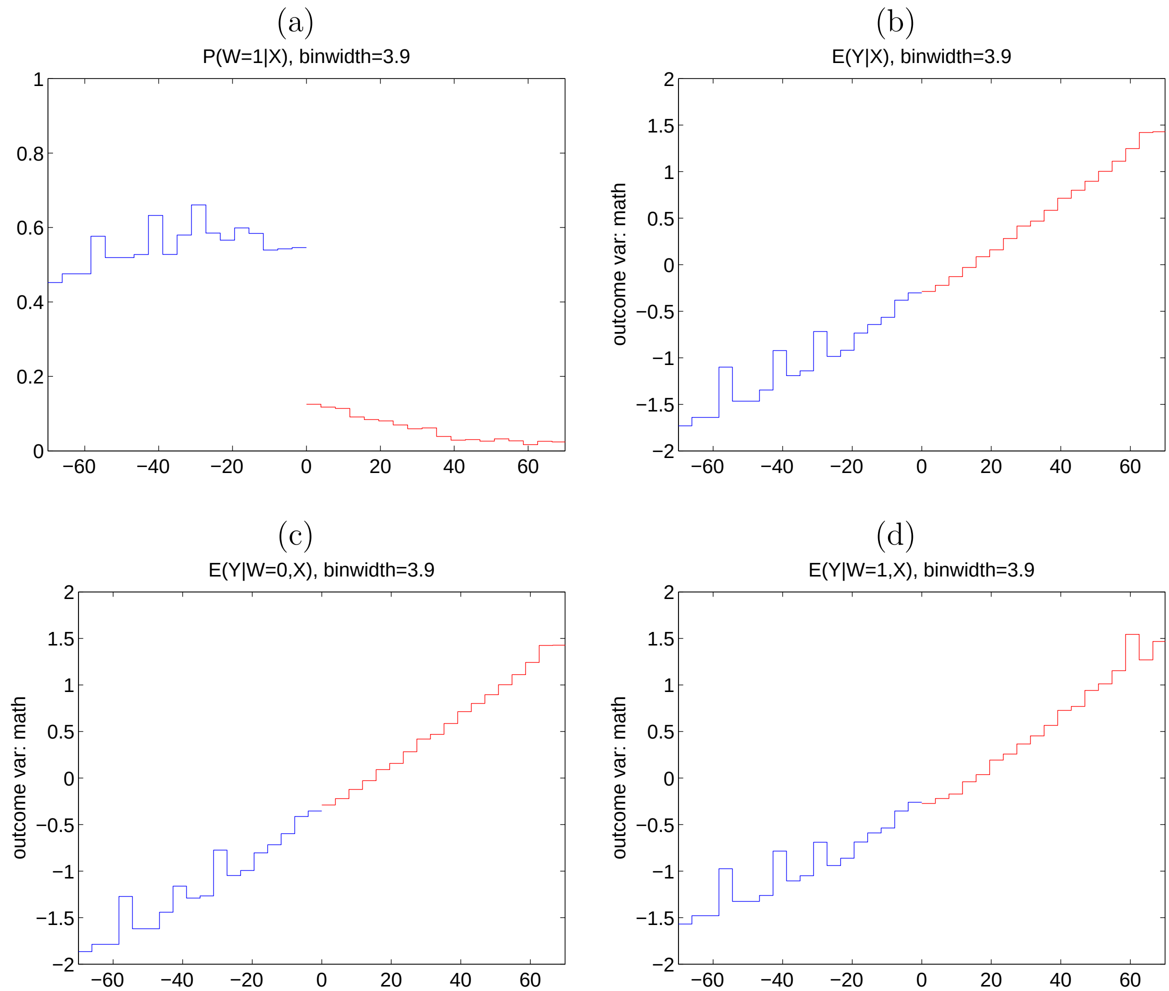
Notes: Outcome variable Y is the math score (after the summer school), and forcing variable X is the minimum between the reading and math score (before the summer school) minus the cutoff; W is the participation indicator. Step functions are made of local averages within each bin, and plots have the same number of equal sized bins on each side of the cutoff.

Table 2: Matsudaira Data

	Estimand	Estimate	s.e.	p-value
Estimates				
Participation	π_c	0.400	(0.010)	
	π_a	0.131	(0.005)	
	π_n	0.469	(0.008)	
Cond. Means of Potential Outcomes	$\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = c]$	-0.213	(0.017)	
	$\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = a]$	-0.359	(0.023)	
	$\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = c]$	-0.413	(0.022)	
	$\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = n]$	-0.325	(0.011)	
LATE	$\mathbb{E}[Y_i(1) - Y_i(0)\|X_i = T^*, G_i = c]$	0.200	(0.027)	
OLS	$\lim_{h\downarrow 0} \mathbb{E}[Y_i^{obs}\|W_i^{obs} = 1, T^* - h < X_i < T^* + h]$ $- \lim_{h\downarrow 0} \mathbb{E}[Y_i^{obs}\|W_i^{obs} = 0, T^* - h < X_i < T^* + h]$	0.080	(0.012)	
Tests				
Hausman	$\frac{\pi_a}{\pi_a + \pi_c/2} \cdot \Big(\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(1)\|X_i = T^*, G_i = a]\Big)$ $- \frac{\pi_n}{\pi_n + \pi_c/2} \cdot \Big(\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(0)\|X_i = T^*, G_i = n]\Big)$	0.120	(0.025)	0.000
Angrist	$\Big(\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(1)\|X_i = T^*, G_i = a]\Big)$ $-\Big(\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(0)\|X_i = T^*, G_i = n]\Big)$	0.234	(0.044)	0.000
Conditional Jumps	$\mathbb{E}[Y_i(1)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(1)\|X_i = T^*, G_i = a]$	0.146	(0.033)	0.000
	$\mathbb{E}[Y_i(0)\|X_i = T^*, G_i = c] - \mathbb{E}[Y_i(0)\|X_i = T^*, G_i = n]$	-0.088	(0.030)	0.003
Joint F-test		28.658		0.000

The forcing variable X is the 2001 minimum score between reading and math minus the cutoff for passing. The outcome variable Y is the standardized math score in 2002, and W is the participation indicator. Non-parametric estimates are obtained by local linear regression and optimal bandwidth $h = 28$ by Imbens and Kalyanaraman (2012). The standard errors of all estimates are computed using 1000 bootstrap iterations. The F-test is a χ^2 statistic computed as a quadratic form of the 2×1 vector of the differences between potential outcomes for treated compliers and always-takers and for non-treated compliers and never-takers inversely weighted by the covariance matrix of such vector.

Figure 2: Matsudaira



Notes: The forcing variable X is the 2001 minimum score between reading and math minus the cutoff for passing. The outcome variable Y is the standardized math score in 2002, and W is the participation indicator. Step functions are made of local averages within each bin, and plots have the same number of equal sized bins on each side of the cutoff.