Complex Discontinuity Designs Using Covariates

Juan D. Díaz† Josué R. Zubizarret†

Abstract

Regression discontinuity designs are extensively used for causal inference in observational studies. However, they are usually confined to settings with simple treatment rules, determined by a single running variable, with a single cutoff. In this paper, we propose a framework and methods for complex discontinuity designs that encompasses multiple treatment rules. These rules may be determined by multiple running variables, each with many cutoffs, that possibly lead to the same treatment. Moreover, the running variables may be discrete and the treatments do not need to be binary. In this framework, the observed covariates play a central role for identification, estimation, and generalization of causal effects. Identification is non-parametric and relies on a local strong ignorability assumption; that is, on local unconfoundedness and local positivity assumptions. Estimation proceeds as in any observational study under strong ignorability, yet in a neighborhood of the cutoffs of the running variables. We discuss estimation approaches based on matching and weighting, including additional regression adjustments in the spirit of doubly robust estimators. We present assumptions for generalization; that is, for identification and estimation of average treatment effects for target populations beyond the study sample that reside in a neighborhood of the cutoffs. We also propose two approaches to select the neighborhood for the analyses and assess the plausibility of the assumptions. We motivate and illustrate this framework with an example of the impact of grade retention on educational and juvenile crime outcomes.

Keywords: Causal Inference; Observational Studies; Regression Discontinuity Design
1 Introduction

The regression discontinuity design (Thistlethwaite and Campbell 1960) or, simply, the discontinuity design, is widely recognized as one of the strongest designs for causal inference in observational studies. In a discontinuity design, the treatment assignment is governed by an assignment, forcing, or running variable, such that for values of this variable smaller or greater than a given cutoff, subjects are assigned to treatment or control. In this design, treatment effects are essentially estimated by contrasting weighted average outcome values across treatment groups at the cutoff or in a small neighborhood around it. See, for example, the reviews by Imbens and Lemieux (2008), Lee and Lemieux (2014), and Cattaneo et al. (2019b, 2019a).

Following Cattaneo et al. (2019b, 2019a), there are two frameworks for interpreting and analyzing discontinuity designs: the continuity-based framework, which is asymptotic and identifies the effect of treatment at the cutoff (e.g., Hahn et al. 2001; Calonico et al. 2014; Gelman and Imbens 2018; Imbens and Wager 2019), and the local randomization framework, which is limitless, and formulates the design as a local randomized experiment around the cutoff (e.g., Cattaneo et al. 2015; Li et al. 2015; Mattei and Mealli 2016; Sales and Hansen 2020). While both frameworks have strengths, they are usually confined to settings with simple treatment rules, determined by a single continuous running variable, with a single cutoff, and where covariates other than the running variable are not simultaneously used to adjust and test for covariate balance in order to select the neighborhood for analysis.

In this paper we propose a framework for complex discontinuity designs, where treatment assignment may be determined by multiple treatment rules, each with multiple running variables and several cutoffs, that possibly lead to the same treatment. This framework builds on the local randomization framework and is exemplified by estimating the impact of school grade retention (i.e., repetition) on educational and juvenile crime outcomes. In Chile, grade
retention (the “treatment”) is determined by two rules, each of which is composed of two (discrete) school subject grades, with one grade common to both rules, but with a different cutoff. In this setting, baseline covariates that approximate the basic academic skill of the student are imbalanced, even in the smallest neighborhood of the cutoffs that we can define (see Section 2 for details). Although the continuity-based framework has been extended to separately incorporate a discrete running variable, more than one running variable, and multiple cutoffs (e.g., Cattaneo et al. 2016; Kolesar and Rothe 2018; Imbens and Wager 2019; Branson et al. 2019), to our knowledge, existing frameworks for discontinuity designs cannot accommodate this type of setting, with two or more treatment rules, each with many discrete running variables, and imbalance in the observed covariates. In this paper, we propose a framework and methods for this and other complex discontinuity designs.

In our framework, the observed covariates other than the running variable(s) play a central role for identification. In essence, in our framework, identification relies on a local strong ignorability assumption; that is, on local unconfoundedness and local positivity assumptions. Estimation proceeds as in any observational study under strong ignorability, yet in a neighborhood of the cutoffs of the running variables. This implies that we can use matching, weighting, or other regression-assisted approaches in the spirit of doubly robust estimation, which to our knowledge have not been used in discontinuity designs before. We can also select the neighborhood for analysis using novel methods. Our work builds on the work by Keele et al. (2015), who use the local unconfoundedness assumption in the context of a geographic discontinuity design. Their framework, however, encompasses simple treatment rules and their procedure for selecting the neighborhood for analysis does not test for implications of this assumption (see also Keele et al. 2017). In this paper, we propose two methods to select the neighborhood for analysis, both of which explicitly use covariates other than the running variables and are part of the design phase of the study in the spirit of Rubin (2008). The first method is similar to the methods by Cattaneo et al. (2015) and Li et al. (2015), but is based on a multidimensional non-parametric test, which separately adjusts for some
covariates and tests for balance on other relevant covariates, using the ideas and methods by Imbens and Rubin (2015, Chapter 21) and Heller et al. (2010), respectively, for observational studies under unconfoundedness. The second method uses split samples in the spirit of Rosenbaum (2010, Chapter 18) and Heller et al. (2009) in observational studies. In our framework, it is also straightforward to accommodate discrete running variables, non-binary (multi-valued) treatments, and intermediate outcomes. Furthermore, generalization of effect estimates to target populations of special interest is facilitated by identifying effect estimates in a neighborhood of the cutoffs as opposed to the (point) intersection of the cutoffs.

One of the points we wish to make in this paper is that, under the assumption of local strong ignorability, we can not only handle complex discontinuity designs in a straightforward way, but also facilitate different outcome analyses than conventionally done in discontinuity designs. As we illustrate in this paper, this framework facilitates (i) simple graphical display of the outcomes across treatment groups as is done in the analysis of clinical trials, (ii) sensitivity analyses on near equivalence tests in matched observational studies, and (iii) regression assisted approaches in the spirit of doubly robust estimation. To our knowledge, these analyses have not been done in discontinuity designs. As we discuss below, under certain assumptions we can also generalize the study findings of the discontinuity design to a target population. Of course, the validity of these analyses is predicated on the assumption of local strong ignorability. We note that this assumption will not always be plausible and is different from the one invoked under the continuity-based framework. The plausibility of these assumptions will depend on their specific context.

To develop and illustrate this framework, in this paper we use as a running example a study of the impact of grade retention on educational and juvenile crime outcomes in Chile. In Section 2 we explain the school grade retention rules in Chile. In Section 3 we describe our data: a new, large administrative data set with extensive educational and criminal records of the same students, observed for 15 years. In Section 4 we present our framework for complex
discontinuity designs, including the notation, estimands, and assumptions for identification and generalization. In Section 5 we discuss estimation using matching, weighting, and regression-assisted matching and weighing approaches. In Section 6 we propose different methods for selecting the neighborhood for analysis. In Section 7 we use our running example to illustrate different analyses that can be done in discontinuity designs. In Section 9 we close the paper with some remarks.

2 Running example: grade retention and juvenile crime

In Chile, school grades vary between 1 and 7 by increments of 0.1. 7 is “Outstanding,” 4 denotes “Sufficient,” and 1 stands for “Very Deficient.” A student fails a subject with a grade below 4.0. A student repeats the year under either of the two following rules: (1) having a grade below 4 in one subject and having an average grade across all subjects lower than 4.5; or (2) having a grade below 4 in two subjects and having an average grade across all subjects lower than 5. In either case, the student repeats the year. The question is what is the impact of grade retention on educational and juvenile crime outcomes in Chile.

In Chile, incarceration rates are high in comparison to other OECD countries: while in these countries there are on average 145.5 inmates per 100,000 habitants, in Chile this rate is 266 per 100,000 inhabitants. Although there is an extensive literature connecting grade retention and juvenile crime, the empirical evidence is scarce, especially in countries like Chile (Hirschfield 2009; Cook and Kang 2016; Depew and Eren 2016; Díaz et al. 2018). In part this is due to the difficulty of finding comprehensive data sets and implementing study designs that enable credible causal inference of the impact of grade retention, as we discuss in the next sections.

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1European Institute for Crime Prevention and Control, affiliated with the United Nations (2010).
3 Educational and criminal administrative records

To study the impact of grade retention on educational and juvenile crime outcomes in Chile, we use a unique longitudinal administrative data set with extensive educational and criminal records of the same students, followed for 15 consecutive years. We assembled this data set from administrative records from the ministries of Education and Justice in Chile. The resulting data set covers the period 2002-2016 and is a census: it includes all students who were enrolled in any grade in the Chilean educational system in the period 2002-2016. The data set contains detailed educational and sociodemographic variables of the students, their families, and their schools, including the student’s gender, age, attendance and grades, the parents’ education and incomes, and the school’s average standardized test scores and socioeconomic status. In 2007, we have the school grades of all students, disaggregated by subject. As explained in Section 2, these variables determine grade retention in Chile, and are central to our discontinuity design. At the end of 2007, we register whether the student was retained because of low school grades (the school year ends in December in Chile). Between 2008 and 2016, we measure educational outcomes of the student and whether he or she was prosecuted for a criminal offense. In total, we have 1,377,089 student observations in our data set; 4.4% of them repeated the grade they had taken in 2007, and 3.3% of them were prosecuted for a criminal offense between 2008 and 2016.

4 A framework for complex discontinuity designs

We consider discontinuity designs where the treatment assignment is determined by multiple rules that may lead to the same treatment. Importantly, each rule may depend on several running variables and some running variables may be common to multiple rules. For each running variable there may be various cutoffs and neighborhoods around those cutoffs where the identification assumptions hold. In our running example, all the treatment assignment
rules lead to the same binary treatment, but the framework is more general and the treatment could also be multi-valued. This extension is direct as it constitutes a composition of multiple binary treatments. To describe this framework formally, we introduce the following notation.

### 4.1 Notation

For each unit $i$, let $\mathcal{R}_i = \{R_{ir} : r = 1, ..., n_R\}$ be the set of running variables $R_r$ with $r = 1, ..., n_R$. Let $\mathcal{Q}_i = \{Q_{iq} : q = 1, ..., n_Q\}$ be the set of subsets of running variables $Q_{iq} \subseteq \mathcal{R}_i$ that determine each treatment assignment rule $q$, with $q = 1, ..., n_Q$. Based on $\mathcal{Q}_i$ and $\mathcal{R}_i$, each unit is assigned to treatment according to the rule

$$Z_{iq} = \begin{cases} 
1, & \text{if } Q_{iq} \in \mathcal{P}_q \\
0, & \text{otherwise}
\end{cases}$$

(1)

where $\mathcal{P}_q$ is the partition of the support of the running variables in $Q_{iq}$ where units are assigned to treatment $q$; specifically, $\mathcal{P}_q = \{\times(-\infty, c_{qr}) : r \text{ such that } R_r \in Q_q\}$ denotes the Cartesian product of open intervals where the units are assigned to treatment in rule $q$. Here, $c_{qr}$ is the cutoff for running variable $r$ under rule $q$. For each unit $i$, let $Z_i$ denote a treatment combination represented by an $n_Q$-dimensional vector, where the $q$th element indicates whether unit $i$ receives the $q$th treatment associated to treatment rule $q$ ($Z_{iq} = 1$) or not ($Z_{iq} = 0$).

In our running example, all the treatment assignment rules lead to the same treatment (grade retention) so

$$Z_i = \begin{cases} 
1, & \text{if } Q_{i1} \in \mathcal{P}_1 \text{ or } Q_{i2} \in \mathcal{P}_2 \text{ or } \ldots \text{ or } Q_{in_Q} \in \mathcal{P}_{n_Q} \\
0, & \text{otherwise}
\end{cases}$$

(2)

but this does not need to be the case in general, since the treatment can also be multi-valued.
Let $\mathcal{N} = \{\mathcal{N}_q : q = 1, ..., n_Q\}$ be the set of hyperrectangles

$$
\mathcal{N}_q = \left\{ \times [c_{qr} - \delta_{qr}, c_{qr} + \delta_{qr}] : r \text{ such that } R_r \in \mathcal{Q}_q \right\},
$$

where the identification assumptions described in Section 4.3 hold and where $\delta_{qr}$ and $\delta_{qr}$ are positive scalars that define the window below and above the cutoff, respectively. Let $\mathcal{N}_q^-$ be the subset of $\mathcal{N}_q$ where units receive treatment $q$. Specifically, $\mathcal{N}_q^- = \{\times [c_{qr} - \delta_{qr}, c_{qr}] : r \text{ such that } R_r \in \mathcal{Q}_q\}$. Conversely, define $\mathcal{N}_q^+$ as the subset of $\mathcal{N}_q$ where units do not receive treatment.

To clarify this notation, a few remarks are in order. In our running example, there are $n_R = 3$ running variables, so $\mathcal{R} = \{R_1, R_2, R_3\}$, and there are $n_Q = 2$ treatment assignment rules, so $\mathcal{Q} = \{\mathcal{Q}_1, \mathcal{Q}_2\} = \{\{R_1, R_3\}, \{R_2, R_3\}\}$, where $R_1$ is the lowest grade across all school subjects, $R_2$ is the second lowest grade, and $R_3$ is the average grade across all subjects. In our running example, we can identify the effect of treatment for $\mathcal{Q} \in \mathcal{N} = \{\mathcal{N}_1, \mathcal{N}_2\} = \{[c_{11} - \delta_{11}, c_{11} + \delta_{11}] \times [c_{13} - \delta_{13}, c_{13} + \delta_{13}], [c_{22} - \delta_{22}, c_{22} + \delta_{22}] \times [c_{23} - \delta_{23}, c_{23} + \delta_{23}]\}$. In this neighborhood, units that receive treatment are those ones with $\mathcal{Q} \in \mathcal{N}^- = \{\mathcal{N}_1^-, \mathcal{N}_2^-\} = \{[c_{11} - \delta_{11}, c_{11}) \times [c_{13} - \delta_{13}, c_{13}), [c_{22} - \delta_{22}, c_{22}) \times [c_{23} - \delta_{23}, c_{23})\}$, while those in the control group have $\mathcal{Q} \in \mathcal{N}^+ = \{\mathcal{N}_1^+, \mathcal{N}_2^+\} = \{\mathcal{N}_1 \setminus \mathcal{N}_1^-, \mathcal{N}_2 \setminus \mathcal{N}_2^-\}$. See Figure 1 for details.

For each unit $i$, let $Y_i(z_i)$ denote the potential outcome of unit $i$ under treatment combination $z_i$. This notation implicitly makes the Stable Unit Treatment Value Assumption (SUTVA, Rubin 1980), which states that the treatment assignment of one unit does not affect the potential outcomes of other units (i.e., that there is no interference among individuals) and that there are no hidden versions of the treatment that would result in different potential outcomes beyond those encoded by $Y_i(z_i)$. In our running example, the treatment is binary so the potential outcomes for unit $i$ are given by $Y_i(1)$ and $Y_i(0)$. Finally, for each unit $i$, let $X_i$ denote a vector of observed, pretreatment covariates other than the running variables.
Figure 1: In our running example, there are two treatment rules that lead to the same treatment, $Z$, which is grade retention. Treatment rule one is defined by running variables $R_1$ and $R_3$, which are the lowest grade and the average grade across all school subjects, respectively. If $R_1 < c_{11} = 4$ and $R_3 < c_{13} = 4.5$, then the student is retained. Treatment region under rule one, $P_1$, is the shaded area with dashed lines in the left figure. Treatment rule two is defined by $R_2$ and $R_3$, which are the second lowest grade and average grade across all school subjects, respectively. If $R_2 < c_{22} = 4$ and $R_3 < c_{23} = 5$, then the student is retained. Treatment region under rule two, $P_2$, is the shaded area with dashed lines in the right figure. The shaded areas with grey denote the neighborhoods $\mathcal{N}_1$ and $\mathcal{N}_2$ where assumptions 1a' and 1b' hold. These regions do not need to be symmetric around the cutoffs.

4.2 Estimand

We wish to estimate the average treatment effect in a neighborhood of cutoffs, $\mathcal{N}$, where the treatment assignment is unconfounded given the observed covariates. We call this estimand the Neighborhood Average Treatment Effect (NATE), and define it as

$$\tau_{\text{NATE}} := \mathbb{E}\left\{ Y_i(z) - Y_i(z') \mid Q_i \in \mathcal{N} \right\} = \mathbb{E}\left[ \mathbb{E}\left\{ Y_i(z) - Y_i(z') \mid X_i, Q_i \in \mathcal{N} \right\} \mid Q_i \in \mathcal{N} \right], \quad (3)$$

where $z$ and $z'$ define two competing multi-valued treatments. In words, $\tau_{\text{NATE}}$ is the average effect of treatment $z$ compared to $z'$ on the units with values of the running variables in the neighborhood of the cutoffs $\mathcal{N}$. Note that the second expectation on the right-hand side
of (3), i.e., $E\left\{ Y_i(z) - Y_i(z') \bigg| X_i, Q_i \in \mathcal{N} \right\}$, is the Conditional Average Treatment Effect (CATE) on the subset of units with running variables satisfying $Q_i \in \mathcal{N}$. For details about the CATE in observational studies, see, e.g., Chapter 12 of [Imbens and Rubin] (2015).

In our running example, the treatment is binary and $\tau_{\text{NATE}}$ simplifies to

$$
\tau_{\text{NATE}} = E\left\{ Y_i(1) - Y_i(0) \bigg| Q_i \in \mathcal{N} \right\} = E\left[ E\left\{ Y_i(1) - Y_i(0) \bigg| X_i, Q_i \in \mathcal{N} \right\} \bigg| Q_i \in \mathcal{N} \right].
$$

(4)

Our framework builds on and encompasses the traditional local randomization framework. The estimand is similar, but it is defined in a multidimensional neighborhood and is identified under different assumptions. In essence, these assumptions require local randomization conditional on covariates as opposed to unconditional local randomization (see the discussion and references below). In the local randomization framework, the covariates do not play a role for identification, whereas in our framework they play a role that is central. As we discuss in Section [7] in our running example, even in the smallest neighborhood of the cutoffs that one can select, there are meaningful imbalances in observed covariates other than the running variables, so one needs to adjust for them. Also, in our framework it is clear how to generalize the estimates, since under certain conditions on the observed covariates, (3) and (4) have the potential to be generalized to target populations of interest beyond the cutoff (see Section 4.3.2). In the following section, we present the identification assumptions needed in our framework.

4.3 Assumptions

4.3.1 Assumptions for identification

In this section we state the assumptions needed to identify the NATE. First, we state the assumptions for multi-valued treatments and, afterwards, we state them for the particular case of binary treatments, as required in our running example.
Assumption 1. (local strong ignorability of treatment assignment via the running variables).

Assumption 1a

\[ Y_i(z) \perp R_i \mid X_i, Q_i \in N \]

Assumption 1b

\[ 0 < \Pr(Z_i = z \mid X_i, Q_i \in N) < 1 \]

Assumption 1 has two components: local unconfoundedness and local positivity of treatment assignment via the running variables. Assumption 1a states that, in a neighborhood of the cutoffs, the running variables are independent of the potential outcomes given the observed covariates. In other words, in a neighborhood of the cutoffs, the running variables — and, therefore, the assignment to treatments — are essentially random given the observed covariates. One implication of this assumption is that, after controlling for the observed covariates, the regression functions are constant (flat) functions of the running variables in a neighborhood of the cutoffs. See Cattaneo et al. (2015) for a discussion of this implication under the local randomization framework with simple treatment rules and without controlling for the observed covariates. Assumption 1b states that, in a neighborhood of the cutoffs, every unit has a positive probability of receiving any version of the treatment given the observed covariates. The assumption of positivity is sometimes called the assumption of common support. See Li et al. (2015) for a discussion of this assumption in the context of the local randomization framework.

The idea of using strong ignorability in a discontinuity design is not new. Our work builds on an important contribution by Keele et al. (2015) who use the assumption of local unconfoundedness in the context of geographic discontinuity designs, albeit for simple treatment rules; that is, a single treatment rule with a single composite running variable and a single cutoff (see also Keele et al. 2017). In addition to local unconfoundedness, local positivity (Assumption 1b) is needed for non-parametric identification of the neighborhood average.
treatment effect (4). Considering these two assumptions, the methods in Section 6 for selecting the neighborhood for analysis simultaneously adjust and test for balance, whereas the method by Keele et al. (2015) trades sample size and proximity to the cutoff subject to covariate balance requirements. Similar assumptions to Assumption 1a have been invoked by Battistin and Rettore (2008), Angrist and Rokkanen (2015), and Forastiere et al. (2017) in contexts of simple treatment rules. Mattei and Mealli (2016) articulates how this assumption and positivity form local strong ignorability with simple treatment rules. In Section 4.3.2, we build on assumptions 1a and 1b to generalize study findings to target populations.

In essence, Assumptions 1a and 1b are the usual conditions required by the strong ignorability assumption in observational studies (Rosenbaum and Rubin 1983), but in a neighborhood of the cutoffs. In other words, in a neighborhood of the cutoffs, conditional on the observed covariates, a discontinuity design can be analyzed as an observational study under strong ignorability. As we discuss below, this “local strong ignorability” assumption opens many opportunities for handling multiple running variables, accommodating multi-valued treatments, and generalizing effect estimates. For a more detailed discussion about the assumption of strong ignorability in observational studies, see, e.g., Chapter 12 of Imbens and Rubin (2015) and Chapter 5 of Rosenbaum (2017).

In this way, under Assumptions 1a and 1b, we can non-parametrically identify $\tau_{NATE}$ in (3) as follows

$$E\{Y_i(z) - Y_i(z') | Q_i \in \mathcal{N}\} = E\{E(Y_i | Z_i = z, X_i, Q_i \in \mathcal{N}) - E(Y_i | Z_i = z', X_i, Q_i \in \mathcal{N}) | Q_i \in \mathcal{N}\}.$$ 

In our running example, the treatment is binary. Thus, in our specific example we require the following two assumptions.

**Assumption 1’** (local strong ignorability of treatment assignment via the running variables
for binary treatments).

**Assumption 1a’**

\[ R_i \perp \{Y_i(1), Y_i(0)\} \mid X_i, Q_i \in \mathcal{N} \]

**Assumption 1b’**

\[ 0 < \Pr(Z_i = 1 \mid X_i, Q_i \in \mathcal{N}) < 1 \]

Under assumptions 1a’ and 1b’, we can identify \( \tau_{NATE} \) in (4) as follows

\[
\mathbb{E}\left\{Y_i(1) - Y_i(0) \mid Q_i \in \mathcal{N}\right\} = \mathbb{E}\left\{\mathbb{E}(Y_i \mid Z_i = 1, X_i, Q_i \in \mathcal{N})\right\} - \mathbb{E}\left\{Y_i \mid Z_i = 0, X_i, Q_i \in \mathcal{N}\right\} \mid Q_i \in \mathcal{N}.\]

Essentially, with assumptions 1a’ and 1b’ we are transforming a sharp discontinuity design into an observational study under unconfoundedness and overlap in a neighborhood of the cutoffs. As a consequence, we can benefit from all the tools developed for estimation and inference in observational studies with strong ignorability. For instance, in our running example we use matching methods to find samples in a neighborhood of the cutoffs that are balanced, facilitating simpler outcome analyses and sensitivity analyses to hidden biases.

In Section 5, we also discuss other methods for estimation, including regression-assisted approaches in the spirit of doubly robust estimators (which to our knowledge have not been used in standard regression discontinuity designs before).

The above identification assumptions are different from the usual assumptions in the continuity-based and the local randomization frameworks. One could argue that the above assumptions are stronger than the usual ones in the continuity-based framework (because in the continuity-based framework the mean potential outcome functions need to be continuous functions of the running variable whereas our framework requires them to be constant given
the observed covariates); however, the frameworks target different estimands (see, for instance, De la Cuesta and Imai 2016 and Mattei and Mealli 2016). In our framework the estimand is the Neighborhood Average Treatment Effect, whereas in the continuity-based framework the estimand is the average effect at the cutoff. One could argue that there is a trade-off between invoking different assumptions and identifying different estimands. While our framework builds on the local randomization framework, its assumptions are more plausible in practice than the ones in the local randomization setting, because in many applications local independence conditional on covariates is a more realistic assumption than unconditional local independence. For example, in our running example, local randomization implies that the mean potential outcome functions do not depend on the running variables $R_1$ (the lowest grade across all school subjects), $R_2$ (the second lowest grade), and $R_3$ (the average grade across all subjects) in a neighborhood of the cutoffs. However, more skillful students, who are likely to obtain higher grades in $R_1$, $R_2$, and $R_3$, are also likely to be systematically different from those students whose scores are lower (less skillful students). Therefore, it is implausible that the mean potential outcome functions are constant functions of the grades, even in a neighborhood of the cutoffs. A more plausible assumption is that the mean potential outcome functions are constant functions of the grades after adjusting for covariates. This is stated by assumptions 1a’ and 1b’. In short, if we are able to adjust for covariates that capture the students’ skills and the environment where they are educated (such as their scores in standardized tests before exposure to the treatment, their previous educational attainments, school attended, and teacher’s characteristics), then it is more plausible that the mean potential outcomes do not depend on the running variables in a neighborhood of the cutoffs. As we describe below, a feature of this framework is that assumptions 1a’ and 1b’ in principle have testable implications, which are useful to select the neighborhood for analysis. Finally, this framework naturally handles cases where the running variables are discrete and intermediate outcomes are present.
4.3.2 Assumptions for generalization

For generalization — that is, for identification and estimation of average treatment effects in target populations beyond the sample considered in the analysis — we consider two relevant cases. In both cases, selection into the sample is determined by observed covariates, in addition to the running variables. The key distinction is whether the target population has values of the running variables inside or outside the neighborhood. The case where the target population has values of the running variables outside the neighborhood naturally involves stronger assumptions that, as we discuss below, contradict Assumptions 1a′ and 1b′, as they require treatment assignment to be strongly ignorable throughout the ranges of the running variables.

For simplicity, consider a binary treatment, although what follows can easily be extended to the multi-valued treatment case. Let \( \mathcal{P} \) be a target population of \( N \) units indexed by \( i = 1, ..., N \), and \( S \) be the sample of units from \( \mathcal{P} \) selected into the neighborhood of the cutoffs where we believe assumptions 1a′ and 1b′ hold. Write \( S_i = 1 \) if unit \( i \) is selected into the sample and \( S_i = 0 \) otherwise. Here, we wish to identify and estimate the target average treatment effect (TATE)

\[
\tau_{\text{TATE}} := \mathbb{E}_{\mathcal{P}} \{Y_i(1) - Y_i(0)\}.
\]

This is the average treatment effect in the target population \( \mathcal{P} \); if \( \mathcal{P} \) is finite, \( \tau_{\text{TATE}} = \frac{1}{|\mathcal{P}|} \sum_{i \in \mathcal{P}} Y_i(1) - Y_i(0) \).

- **Case 1:** \( \mathcal{P} \) such that \( Q_i \in \mathcal{N} \) for all \( i \in \mathcal{P} \)

In this case, we assume the following two conditions hold.

**Assumption 2.** (strong ignorability of study selection in a neighborhood).

**Assumption 2a**

\[
\{Y_i(1), Y_i(0)\} \perp \!\!\!\!\perp S_i \mid X_i, Q_i \in \mathcal{N}
\]
Assumption 2b

\[ 0 < \Pr(S_i = 1 | X_i, Q_i \in \mathcal{N}) < 1 \]

Assumption 2a states that selection into the sample is independent of the potential outcomes given the observed covariates and running variables satisfying \( Q_i \in \mathcal{N} \). This assumption can be relaxed in order to require conditional mean independence only; that is, \( \mathbb{E}\{Y_i(z) | X_i, S_i = 1, Q_i \in \mathcal{N}\} = \mathbb{E}\{Y_i(z) | X_i, Q_i \in \mathcal{N}\} \). Assumption 2b states that every unit in the target population \( \mathcal{P} \) has a positive probability of selection into the sample given the observed covariates and that the running variables have values in \( \mathcal{N} \).

Under assumptions 1a\(^\prime\), 1b\(^\prime\), 2a, and 2b, we can identify \( \tau_{\text{TATE}} \) from the observed data as follows

\[
\mathbb{E}_{\mathcal{P}}\{Y_i(z) | Q_i \in \mathcal{N}\} = \mathbb{E}_{\mathcal{P}}\{\mathbb{E}(Y_i | S_i = 1, Z_i = z, X_i, Q_i \in \mathcal{N}_i) | Q_i \in \mathcal{N}\},
\]

for \( z = 0, 1 \).

- **Case 2**: \( \mathcal{P} \) such that \( Q_i \notin \mathcal{N} \) for some \( i \in \mathcal{P} \)

In this case, we assume the following two conditions hold.

**Assumption 2\(^\prime\)** (strong ignorability of study selection).

**Assumption 2a\(^\prime\)**

\[ \{Y_i(1), Y_i(0)\} \perp S_i | X_i \]

**Assumption 2b\(^\prime\)**

\[ 0 < \Pr(S_i = 1 | X_i) < 1 \]

Assumptions 2a\(^\prime\) and 2b\(^\prime\) are stronger than Assumptions 2a and 2b. In a sense, assumptions 2a\(^\prime\) and 2b\(^\prime\) contradict assumptions 1a\(^\prime\) and 1b\(^\prime\). In fact, in order to identify \( \tau_{\text{TATE}} \) from the observed data under assumptions 2a\(^\prime\) and 2b\(^\prime\), then we must have
Assumption 1" (strong ignorability of treatment assignment via the running variable).

Assumption 1a"

\[ R_i \perp \{Y_i(1), Y_i(0)\} | X_i \]

Assumption 1b"

\[ 0 < \Pr(Z_i = 1 | X_i) < 1 \]

In other words, in order to identify \( \tau_{TATE} \) from the observed data under assumptions 2a' and 2b', then assumptions 1a'' and 1b'' must hold and the assignment of the running variables must be strongly ignorable given the observed covariates throughout the entire range of the running variables.

5 Estimation and inference

We consider three estimation approaches based on matching, weighting, and a combination of the two with additional regression adjustments in the spirit of doubly robust estimators.

5.1 Matching approaches

Let \( I_T \) and \( I_C \) be the set of indices of the treated and control units in \( S \). Specifically, let \( I_T = \{i : Q_{iq} \in N_i^{-} \text{ for any } q = 1, \ldots, n_Q\} \) and \( I_C = \{i : Q_{iq} \in N_i^{+} \text{ for any } q = 1, \ldots, n_Q\} \). Following Zubizarreta et al. (2014), we may find the largest pair-matched sample \( m^* \) of
treatment and control units that is balanced in a neighborhood of the cutoffs as

$$\arg\max_m \left\{ \sum_{t \in T} \sum_{c \in C} m_{tc} : 
\left| \sum_{t \in T} \sum_{c \in C} m_{tc} (B_k(X_t) - \sum_{i \in P} B_k(X_i)) \right| \leq \delta_k \sum_{t \in T} \sum_{c \in C} m_{tc}, \ k = 1, \ldots, K; \right. $$

$$\left. \sum_{t \in T} \sum_{c \in C} m_{tc} (B_k(X_c) - \sum_{i \in P} B_k(X_i)) \right| \leq \delta_k \sum_{t \in T} \sum_{c \in C} m_{tc}, \ k = 1, \ldots, K; $$

$$m_{tc} \in \{0,1\}, t \in T, c \in C \right\}$$

where $m_{tc}$ are binary decision variables that determine whether treated unit $t$ is matched to control unit $c$; $B_k(X_t)$ are suitable transformations of the observed covariates that span a certain function space; and $\delta_k$ is a tolerance that restrains the imbalances in the functions $B_k(\cdot)$ of the covariates (see Wang and Zubizarreta 2020 for a tuning algorithm). Thus, the matching given by (5)-(8) is the maximal size pair-matching that approximately balances the transformations $B_k(\cdot)$ of the covariates relative to the target population $\mathcal{P}$. Following Zubizarreta et al. (2014), we can re-match the matching $m^*$ in order to minimize the total sum of covariate distances between matched units, while preserving aggregate covariate balance. After matching, we can do inference using randomization techniques as in Rosenbaum (2002b) or the large sample approximations in Abadie and Imbens (2006).

### 5.2 Weighting approaches

Following Wang and Zubizarreta (2019), the matching indicators from (5)-(8) can be seen as a special case of a weighting scheme where the weights are binary (or a constant fraction) that encode an assignment between matched units. An alternative is to relax the above
matching problem into a weighting problem as follows,

\[
\arg\min_w \left\{ \sum_{i \in I_C} (w_i - \bar{w})^2 : \right. \\
\left. \sum_{i \in I_C} w_i Z_i B_k(\mathbf{X}_i) - \frac{1}{|I_T|} \sum_{i \in I_T} B_k(\mathbf{X}_i) \leq \delta_k, \ k = 1, \ldots, K; \right. \\
\left. \sum_{i \in I_C} w_i = 1, w_i \geq 0, i \in I_C \right\}. 
\] (9)

These are the weights \( \mathbf{w}^* \) of minimum variance that approximately balance the covariates. The asymptotic properties and inferential methods with balancing weights are discussed in Zhao (2019) and Wang and Zubizarreta (2020).

5.3 Regression-assisted approaches

The above matching and weighting approaches can be supplemented with additional regression adjustments in the spirit of doubly robust estimators (Robins et al. 1994). Under assumptions 1a’ and 1b’, a discontinuity design is a locally unconfounded observational study. Therefore, in a neighborhood of the running variables, we may estimate (3) using traditional methods for observational studies where the unconfoundedness and positivity assumptions hold. Here we present two regression-assisted approaches that follow the above matching and weighting approaches.

For matching, following Rubin (1979), we may use a regression-assisted matching (RAM) estimator of the form

\[
\hat{\tau}_{\text{RAM}} = (\bar{y}_T. - \bar{y}_C.) - (\bar{x}_T. - \bar{x}_C.) \hat{\beta}
\] (12)

where \( \bar{y}_T. \) and \( \bar{y}_C. \) are the means of \( Y \) in the matched treated and control samples, and similarly, \( \bar{x}_T. \) and \( \bar{x}_C. \) are the means of \( \mathbf{x} \). \( \hat{\beta} \) are the estimated linear regression coefficients of the matched-pair differences in outcomes on the matched-pair differences in covariates.

For weighting, following Athey et al. (2018), we may use a regression assisted weighting
estimator of the form

$$\hat{\tau}_{\text{RAW}} = \bar{x}^T \hat{\beta}_T - \left( \bar{x}^T \hat{\beta}_C + \sum_{i \in I_C} w_i (y_i - x_i \hat{\beta}_C) \right)$$

(13)

where $\hat{\beta}_T$ and $\hat{\beta}_C$ are the estimated regression coefficients for the treated and control samples, respectively. For inference, given the selected neighborhood, we may use the methods in Athey et al. (2018) and Hirshberg and Wager (2018). See Robins et al. (1994) and Abadie and Imbens (2011) for other estimators in this spirit.

### 6 Selection of the neighborhood

A central question in practice is how to select a neighborhood for analysis; that is, the set of hyperrectangles $N$ where the identification assumptions are likely to hold. Assumptions 1a and 1b require that at least one $N$ exists, and $N$ does not need to be unique. In order to maximize the precision of the study, we will find the largest neighborhood $N$ where the assumptions are plausible.

Assumption 1a is not directly testable from the observed data. The reason is that, for each unit, we can observe only one of the potential outcomes. However, there are implications of Assumption 1a that can be tested. In this section, we present two sets of methods for testing such implications and selecting $N$. The first set of methods are design-based in the sense that they do not use outcome information; instead, they use auxiliary sets of covariates other than the ones needed for conditioning on assumptions 1a and 1b (either secondary covariates, lagged outcomes, or lagged running variables). The second set of methods are semi-design-based in that they use outcome information, but in a planning sample, separate from the sample used for the actual outcome analyses. See Cattaneo et al. (2015), Li et al. (2015), and Cattaneo and Vazquez-Barco (2016) for methods for selecting a neighborhood in regression discontinuity designs in the local randomization framework.
6.1 Design-based approaches

Assumption 1a says that treatment assignment is independent of the potential outcomes conditional on the observed covariates in a neighborhood of the cutoffs. One implication of this assumption is balance on the potential outcomes and any other variable measured before treatment assignment, once we adjust for the observed covariates $X_i$. Let $X^\text{test}_i$ denote such other variables. These variables can be secondary covariates, lagged outcomes, or lagged running variables, and are different from the observed covariates $X_i$ required in Assumption 1a. Conditional on $X_i$, Assumption 1a implies that $X^\text{test}_i$ is balanced across treatment groups in terms of its joint distribution, and therefore, in terms of its moments.

To assess the plausibility of Assumption 1a, we may test for joint, multivariate covariate balance on $X^\text{test}_i$ conditional on $X_i$. With binary treatments, we may use, for instance, the cross-match test, which is a non-parametric test used to compare multivariate distributions. \cite{Rosenbaum2005, Heller2010}. We will consider the assumption of local unconfoundedness to be plausible if we fail to reject the null hypothesis that $X^\text{test}_i$ has the same multivariate distribution across treatment groups after adjusting for $X_i$. In addition, we may test the validity of Assumption 1a by checking the following moment balance conditions

$$
\mathbb{E}\left[\mathbb{E}\{h(X^\text{test}_i) \mid Z_i = z, X_i, Q_i \in \mathcal{N}\} - \mathbb{E}\{h(X^\text{test}_i) \mid Z_i = z', X_i, Q_i \in \mathcal{N}\} \mid Q_i \in \mathcal{N}\right] = 0 \tag{14}
$$

for any $z$ and $z'$ and any function $h(\cdot)$. For instance, \cite{14} can be tested after matching using randomization techniques or asymptotic approximations. The idea is to find the largest $\mathcal{N}$ such that $X^\text{test}_i$ is balanced after adjusting for $X_i$ by matching. In practice, a conservative way to select $\mathcal{N}$ is to implement different tests with different power for univariate and multivariate covariate balance and select the largest $\mathcal{N}$ where we fail to reject the null hypothesis of covariate balance for any of the tests (see Section \ref{sec:7.1} for an example).
In what follows, \( X_{i}^{\text{test}} = Y_{i}^{\text{lagged}} \), the lagged outcome (i.e., the outcome variable measured before treatment exposure), but \( X_{i}^{\text{test}} \) might as well be a lagged running variable or a secondary covariate. We see \( Y_{i}^{\text{lagged}} \) as a proxy of the potential outcome under control (see Chapter 21 of Imbens and Rubin 2015 for a discussion). If Assumption 1a holds, then \( Y_{i}^{\text{lagged}} \) should be balanced across treatment groups for all units in \( \mathcal{N} \), after adjusting for \( X_{i} \). We thus assess the plausibility of Assumption 1a by checking whether the following mean independence condition holds

\[
\mathbb{E}\left\{ g(Y_{i}^{\text{lagged}}) \mid R_{i}, X_{i}, Q_{i} \in \mathcal{N} \right\} = \mathbb{E}\left\{ g(Y_{i}^{\text{lagged}}) \mid X_{i}, Q_{i} \in \mathcal{N} \right\},
\tag{15}
\]

for any function \( g(\cdot) \) with finite moments. Here, \( X_{i} \) does not include \( Y_{i}^{\text{lagged}} \).

There are several ways of testing (15), both in parametric and nonparametric setups. In a parametric setup, we can regress \( g(Y_{i}^{\text{lagged}}) \) on \( R_{i} \) and \( X_{i} \) for the units in \( \mathcal{N} \) and test the null hypothesis that all the coefficients of the running variables are jointly zero. We select the largest \( \mathcal{N} \) where we fail to reject the null. However, although within this parametric setup we can posit a flexible model for the conditional mean function, mean independence is stronger than regression independence (for a given family of regression functions). Thus, in order to directly test the mean independence condition in (15), we can instead use nonparametric regression. For a comprehensive review and methods for related nonparametric regression tests, see Fan and Li (1996), Delgado and Gonzalez (2001), and Lavergne et al. (2015).

Essentially, for units with \( Q_{i} \in \mathcal{N} \), in this nonparametric setup we test

\[
H_{0} : \quad \mathbb{E}\left\{ g(Y_{i}^{\text{lagged}}) \mid R_{i}, X_{i} \right\} = \mathbb{E}\left\{ g(Y_{i}^{\text{lagged}}) \mid X_{i} \right\} \quad \text{a.s.}
\]

versus the corresponding alternative hypothesis

\[
H_{1} : \quad \text{Pr} \left[ \mathbb{E}\left\{ g(Y_{i}^{\text{lagged}}) \mid R_{i}, X_{i} \right\} = \mathbb{E}\left\{ g(Y_{i}^{\text{lagged}}) \mid X_{i} \right\} = 0 \right] \quad \text{a.s.}
\]
Despite the obvious appeal of this nonparametric testing procedure, its main disadvantage is its limited power in practice. To overcome this limitation, alternatively we can use the matching approach to assess (15) by checking

$$
E\left\{ g(Y_i^{\text{lagged}}) \mid Z_i = z, X_i, Q_i \in \mathcal{N} \right\} - E\left\{ g(Y_i^{\text{lagged}}) \mid Z_i = z', X_i, Q_i \in \mathcal{N} \right\} = 0. \tag{16}
$$

Here, the idea is to use matching to adjust for $X_i$ and to conduct a sequence of balance tests for different $\mathcal{N}$, selecting the largest $\mathcal{N}$ that is compatible with (16).

Assessing balance of pretreatment variables between treatment groups to evaluate the plausibility of the unconfoundedness assumption is a common strategy in observational studies (Imbens and Rubin 2015, Chapter 21). To our knowledge, the idea of selecting the neighborhood based on covariates in a discontinuity design was first proposed by Cattaneo et al. (2015). In the local randomization framework, they propose using a sequence of tests for balance on covariates other than the running variable to assess the assumption of local randomization and select the neighborhood (see also Cattaneo et al. 2017). In a Bayesian framework, Li et al. (2015) introduce a sequence of tests for covariate balance to choose a neighborhood where the local randomization assumption is plausible, while Mattei and Mealli (2016) employ randomization-based tests that adjust for multiple comparisons. We adapt these approaches to a setting where the researcher counts with two set of covariates: a first set involved in the conditional statement of Assumption 1a, and a second set that allows us to select the largest $\mathcal{N}$ by sequentially assessing the balance condition presented in (14).

In this paper we propose a procedure that employs two sets of covariates other than the running variables to simultaneously adjust and test for covariate balance in order to select the neighborhood for analysis in a discontinuity design under local strong ignorability. These approaches are implemented in the design stage of the study, without using the outcomes and therefore blinding the investigator from the study results until after the selection of the neighborhood.
6.2 Semi-design-based approaches

The previous approach does not use the outcomes and in that sense is design-based (Imbens and Rubin 2015, Chapter 21). The approach that follows, by contrast, uses the outcomes, but in a split, smaller planning sample (different from the analysis sample) and in that sense is semi-design-based (Rosenbaum 2010, Chapter 18). This approach is in the spirit of [Heller et al. (2009)] who use a planning sample to guide the design of an observational study. For outcome analyses and estimation of treatment effects, they discard this planning sample to preclude the use of the same outcome data twice (in the design stage and also in the analysis stage of the study). We adapt this idea to our framework to assess the plausibility of the assumptions and select \( \mathcal{N} \). This approach selects \( \mathcal{N} \) from the planning sample using the outcomes and then uses this \( \mathcal{N} \) in the analysis sample for estimation of the average treatment effects.

Consider splitting the available study sample at random into two parts, one planning sample of size \( n_p \) and an analysis sample of size \( n_a \) with \( n_p + n_a = n \). For simplicity, here we discuss the case of a binary treatment, but this procedure carries over to multi-valued treatments. Specifically, given \( \mathcal{N} \), Assumption 1a implies the next two conditions that can be evaluated using the planning sample

\[
\mathbb{E}[g(Y_i(1)) | R_i, X_i, Q_i \in \mathcal{N}^+] = \mathbb{E}[g(Y_i(1)) | X_i, Q_i \in \mathcal{N}^+], \tag{17}
\]

\[
\mathbb{E}[g(Y_i(0)) | R_i, X_i, Q_i \in \mathcal{N}^-] = \mathbb{E}[g(Y_i(0)) | X_i, Q_i \in \mathcal{N}^-], \tag{18}
\]

where \( g(\cdot) \) is an arbitrary function of the covariates with finite moments. In order to test whether the conditions (17) and (18) hold, one can employ any of the three empirical strategies we have mentioned in the previous subsection, namely, a parametric model, a nonparametric significance testing procedure, or a matching-style approach.

Although splitting the sample can reduce the power in the analysis stage, we think that it
is an objective way of assessing the critical assumptions required to conduct credible causal inference. Moreover, if the sample size is large enough, the consideration of a smaller planning sample should not be an issue. Finally, as [Heller et al. (2009)] point out, it is recommended to consider repeated splitting samples in order to check the stability of the results.

7 Results from the running example

As discussed in Section 2, two rules define whether a student repeats the academic year in Chile: Rule 1, having a grade below 4 in one subject and having an average grade across all subjects below 4.5; and Rule 2, having a grade below 4 in two subjects and having an average grade across all subjects below 5. Following our notation, there are three running variables, \( R = \{R_1, R_2, R_3\} \), and two treatment assignment rules, so \( Q = \{Q_1, Q_2\} = \{\{R_1, R_3\}, \{R_2, R_3\}\} \), where \( R_1 \) is the lowest grade across all school subjects in 2007, \( R_2 \) is the second lowest grade in 2007, and \( R_3 \) is the average score across all subjects in 2007. We estimate the effect of grade retention for students in a neighborhood of the cutoffs, such that \( Q \in N = \{N_1, N_2\} = \{\{[c_{11} - \delta_{11}, c_{11} + \delta_{11}] \times [c_{13} - \delta_{13}, c_{13} + \delta_{13}]\}, \{[c_{22} - \delta_{22}, c_{22} + \delta_{22}] \times [c_{23} - \delta_{23}, c_{23} + \delta_{23}]\}\} \), where \( c_{11} = 3.9 \) and \( c_{13} = 4.4 \) are the cutoffs of \( R_1 \) and \( R_3 \) under Rule 1, and \( c_{22} = 3.9 \) and \( c_{23} = 4.9 \) are the cutoffs of \( R_2 \) and \( R_3 \) under Rule 2, and the terms \( \delta \) are positive constants to be determined as follows.

7.1 Finding the neighborhood

To find the neighborhood, we follow the design-based approach described in Section 6.1, where secondary covariates are available. Here, \( X^{test} \) is a three-dimensional vector that includes the mother and father’s education, and the household per capita income. \( X \) includes the student’s school attended and grade level in 2007, gender, birth year and month, number of times of previous grade retention, and standardized test scores in language and math.

Under Assumption 1a’, \( X^{test} \) should be balanced across treatment groups for students in
a neighborhood of the cutoffs \((Q \in \mathcal{N})\), after conditioning on \(X\). The idea is that, after conditioning on \(X\), if the running variables (and therefore, the treatment) do not affect the potential outcomes for students with \(Q \in \mathcal{N}\), then nor do they affect the secondary pretreatment variables for the same group of students. Since \(X^{\text{test}}\) is observed irrespective of the realized treatment status, the validity of Assumption 1a′ can be evaluated using the cross-match test. Specifically, we can check whether \(X^{\text{test}}\) has the same multivariate distribution across treatment groups, after adjusting for \(X\). In addition, we can check the following univariate balance condition

\[
E(X_{i}^{\text{test}} | Z_i = 1, X_i, Q_i \in \mathcal{N}) = E(X_{i}^{\text{test}} | Z_i = 0, X_i, Q_i \in \mathcal{N}).
\] (19)

We use matching to adjust for the observed covariates and select the largest neighborhood that satisfies both of these conditions. More specifically, we implement a sequence of balance tests on \(X^{\text{test}}\) conditioning on \(X\) for an increasing sequence of \(\mathcal{N}\) and select the largest \(\mathcal{N}\) where we fail to reject the above balance tests. We take the following steps in order to select the neighborhood \(\mathcal{N}\) for analysis.

1. Start with a small neighborhood.
2. Find a matched sample that balances the covariates \(X\).
3. Test that \(X^{\text{test}}\) has the same distribution across treatment groups using the cross-match test for multivariate balance and the permuntational t-test for mean balance.
   (a) If the minimum \(p\)-value is less than 0.1, then repeat steps 1 to 3 starting with a smaller neighborhood.
   (b) If the minimum \(p\)-value is greater than or equal to 0.1, then expand the neighborhood and repeat steps 2 and 3 until the new minimum \(p\)-value is smaller than 0.1.
4. Retain the largest neighborhood with a minimum \(p\)-value greater than or equal to 0.1.

In our running example, in step 2 we match students exactly in terms of age (in months), gender, school, grade, and past grade retention (number of times). In addition, we match
with mean balance for the standardized test scores in language and mathematics. More specifically, we use cardinality matching (Zubizarreta et al. 2014) to find the largest pair-matched sample that is balanced according to these two exact matching and mean balance criteria. For mean balance, we consider the value 0.05 as the maximum acceptable standardized difference in means. Despite the potential cost of dropping a large number of students from the analysis, we view exact matching on age, gender, school, grade, and past grade retention of primary importance, as it makes Assumption 1a more plausible. In fact, by matching exactly on covariates, we are also balancing unobserved covariates that are constant within interactions of categories of the exact matching covariates. For example, by matching exactly on school and grade, we are also exactly balancing unobserved school and classroom characteristics (including characteristics of the principal and teachers, the school’s support network, and the social environment where the school is located) which may vary by age and gender. See Appendix A in the Online Supplementary Materials for a summary of covariate balance before and after matching.

Table 1: Selection of the neighborhood \( \mathcal{N} \)

| Neighborhood \( N_1 \) × \( N_2 \) | Matched sample size | \( p \)-value for the cross-match test | \( p \)-value for no effect on secondary covariates | Plausibility of Assumption 1a′ |
|-------------------------------------|---------------------|--------------------------------------|-------------------------------------------------|-----------------------------|
| \([3.8, 4.1] \times [4.3, 4.6]\) × \([3.8, 4.1] \times [4.8, 5.1]\) | 243 243 | 1.00 | 0.33 | ✓ |
| \([3.7, 4.2] \times [4.3, 4.6]\) × \([3.7, 4.2] \times [4.8, 5.1]\) | 524 524 | 1.00 | 0.87 | ✓ |
| \([3.6, 4.3] \times [4.3, 4.6]\) × \([3.6, 4.3] \times [4.8, 5.1]\) | 841 841 | 1.00 | 0.81 | ✓ |
| \([3.5, 4.4] \times [4.3, 4.6]\) × \([3.5, 4.4] \times [4.8, 5.1]\) | 1,141 1,141 | 1.00 | 0.16 | ✓ |
| \([3.4, 4.5] \times [4.3, 4.6]\) × \([3.4, 4.5] \times [4.8, 5.1]\) | 1,359 1,359 | 1.00 | 0.03 | ✗ |
| \([3.3, 4.6] \times [4.3, 4.6]\) × \([3.3, 4.6] \times [4.8, 5.1]\) | 1,578 1,578 | 1.00 | 0.01 | ✗ |

Notes: the third to last column reports the \( p \)-value for the Cross-match Test for comparing two multivariate distributions, while the second to last column reports the minimum \( p \)-value for the permutational \( t \)-test for matched pairs of differences on secondary covariates.

Table 1 summarizes the process of selecting a neighborhood in our running example. In the first row of the table, we start with a neighborhood defined by \( \delta_{qr} = \tilde{\delta}_{qr} = 0.1 \) for all the cutoffs \( qr \) (0.1 is the minimum increment of school grades in Chile). This results in a matched sample of 243 pairs comprising 486 students. For this matched sample, we fail to reject the null hypothesis of no differences in secondary covariates (the minimum \( p \)-value is 0.33); therefore we expand the neighborhood as noted by the second line of the table.
A natural question to ask is how to expand the neighborhood. In typical discontinuity designs with only one running variable, the neighborhood is expanded symmetrically in both directions of the cutoff, but this is a simplification and the neighborhood may be expanded asymmetrically in one direction first. In more complex designs with many running variables and multiple treatment rules, like in our running example, the question is more complex as there are more directions in which the neighborhood can be expanded first (in our running example, there are eight such directions). In our running example, it is more plausible to believe that, after controlling for the observed covariates, students will be comparable in a larger neighborhood of the cutoff of a single school subject ($R_1$ or $R_2$) than in a neighborhood of equal size of the average across all subjects ($R_3$). For this reason, we first expand the neighborhoods of $R_1$ and $R_2$. For $R_1$ and $R_2$ we expand the neighborhood symmetrically for simplicity, but this does not need to be the case in general. After expanding the neighborhood in this way (each time by 0.1 for $R_1$ and $R_2$, symmetrically), we obtain a matched sample of 1,141 pairs comprising 2,282 students, and a neighborhood $\mathcal{N} = \{\mathcal{N}_1, \mathcal{N}_2\} = \{[3.5, 4.4] \times [4.3, 4.6], [3.5, 4.4] \times [4.8, 5.1]\}$, where we fail to reject the null hypothesis of no differences on secondary covariates (minimum $p$-value of 0.16), but for the next largest neighborhood, we reject the null of secondary covariate balance across treatment groups (minimum $p$-value of 0.03). Thus, we retain as our final neighborhood the matched sample with 1,141 pairs of students. In our notation, this neighborhood is $\mathcal{N} = \{\mathcal{N}_1, \mathcal{N}_2\} = \{\{c_{11} - \delta_{11}, c_{11} + \delta_{11}\} \times [c_{13} - \delta_{13}, c_{13} + \delta_{13}], [c_{22} - \delta_{22}, c_{22} + \delta_{22}] \times [c_{23} - \delta_{23}, c_{23} + \delta_{23}]\}$, where $c_{11} = 3.9$ and $c_{13} = 4.4$ are the cutoff values of $R_1$ and $R_3$ under the Rule 1, $c_{22} = 3.9$ and $c_{23} = 4.9$ are the cutoff values of $R_2$ and $R_3$ under the Rule 2, with $\delta_{11} = 0.4, \delta_{13} = 0.5, \delta_{13} = 0.1, \delta_{13} = 0.2, \delta_{22} = 0.4, \delta_{22} = 0.5, \delta_{23} = 0.1$, and $\delta_{23} = 0.2$.

### 7.2 Three analyses

Having found the neighborhood $\mathcal{N}$, we proceed to estimate the effect of grade retention on subsequent school grades, repeating another grade, dropping out of school, or committing a
juvenile crime. Within $\mathcal{N}$, we matched 1,141 students who repeated the academic year in 2007 (treated students) to 1,141 students who advanced to the next grade (control students). We illustrate our framework with three different analyses: one exploratory, where we visualize the impact of grade retention on subsequent grades across time; one based on randomization inference, where we perform a sensitivity analysis on an equivalence test; and one that combines approximately balancing weights and regularized linear regression in the spirit of doubly robust estimation. To the best of our knowledge, these types of analyses have not been conducted in discontinuity designs with complex treatment rules and are enabled by the proposed framework.

7.2.1 Visualizing patterns of effects

In Figure 2, we plot the average school grades of the matched students within the selected neighborhood in years 2008, 2009, 2010, and 2011; that is, one, two, three, and four years after repeating or passing in 2007. In blue, we display the boxplots and densities of the average grades of the students who repeated, and in gray, of the students who passed. In 2008 (this is, in the year immediately after repeating or passing), the students who repeated had higher average grades than the matched students that passed. The difference is approximately two decimal points (see Section 7.2.3 for point estimates and confidence intervals). However, this difference is progressively reduced in the three following years, declining to an average difference of 0.05 decimal points in 2011. Interestingly, in 2011 the difference of the modes of the distributions of grades appears to be reverted, suggesting that after four years following repeating or passing, there are more students with lower grades after repeating than after passing, nonetheless the opposite happens in 2008. Between 2008 and 2011 the dispersion of the matched pair differences in outcomes increases from 0.56 to 0.75, suggesting that heterogeneity in treatment effects is increasing with time.
7.2.2 Estimating effects using randomization tests

Here we estimate the effect of grade retention on repeating another grade, dropping out of school, or committing a juvenile crime. We use the methods in Rosenbaum (2002a) and Zubizarreta et al. (2013). See Appendix B in the Online Supplementary Materials for details.

We test the Fisher’s null hypothesis of no treatment effects using McNemar’s test statistic. In our example, this test corresponds to the number of discordant pairs where the student that was retained subsequently repeated another grade, dropped out of school, or committed a juvenile crime. Table 2 presents the point estimates and $p$-values of the NATE on these three outcome variables. The point estimate of the effect of grade retention on juvenile crime is $\tau_{\text{crime}} = 0.006$. In fact, among the 1,141 matched pairs, there are 117 discordant pairs in which only one student committed a crime. Of these, there are 62 pairs in which the student that repeated committed a crime (see Appendix B for details). Thus, in the absence
of hidden bias, there is no evidence that grade retention causes juvenile crime. In a similar
manner, the estimated effect of grade retention on dropping out of school is also very small
($\hat{\tau}_{\text{drop}} = 0.002$) and not statistically significant ($p$-value = 0.379). However, the estimated
effect of grade retention on repeating another grade is $\hat{\tau}_{\text{ret}} = -0.104$. Here, there are 563
discordant pairs, and out of these, there are 222 pairs in which the student who was retained
in 2007 did not repeat another grade in the future, yielding a one-sided $p$-value smaller than
0.001 (see Appendix B). Thus, in the absence of hidden bias, there is strong evidence that
current grade retention causes a reduction in future grade retention. See Appendix E for
generalizing these results.

Table 2: Estimates for the Neighborhood Average Treatment Effect

| Outcome variable          | Matched sample mean | $\hat{\tau}_{\text{NATE}}$ | $H_0 : \tau_{\text{NATE}} = 0$ |
|---------------------------|---------------------|-----------------------------|----------------------------------|
|                            | Treated             | Control                     | One-sided $p$-value               |
| Committing a crime        | 0.059               | 0.053                       | 0.006                            | 0.229                           |
| Repeating another grade   | 0.446               | 0.551                       | -0.104                           | <0.001                          |
| Dropping out of school    | 0.098               | 0.096                       | 0.002                            | 0.379                           |

In the absence of hidden bias, we have found evidence that grade retention does not cause
dropping out of school or committing a juvenile crime. However, bias from a hidden covariate
can give the impression that a treatment effect does not exist when in fact there is one. How
much bias from a hidden covariate would need to be present to mask an actual treatment
effect? We answer this question by conducting a sensitivity analysis on a near equivalence
test (Rosenbaum and Silber (2009), Zubizarreta et al. (2013); for details, see appendices C
and D in the Online Supplementary Materials). In summary, our results reveal that two
students matched on their observed covariates could differ in their odds of repeating the
grade in 2007 by almost 12% and 46% before masking small and moderate effects on juvenile
crime previously documented in the literature. Analogously, two students matched for their
covariates could differ in their odds of repeating by 16% and 33% before masking small and
moderate effects on dropping out of school. See Appendix D for details.
7.2.3 Estimating effects using balancing weights and regularized regression

We use the regression assisted weighting estimator (13) to estimate the effect of grade retention on subsequent average school grades. More specifically, we use the method by Athey et al. (2018) which combines approximately balancing weights and regularized linear regression in the spirit of doubly robust estimation. We use the implementation of this method in the package balanceHD for R, and find that one year after retention, the average school grades of students who repeated is 0.19 points higher than the one of students that passed (95% confidence interval, [0.16, 0.21]; the standard deviation of average school grades this year was 0.46 points). Four years after retention, this effect is considerably reduced, and students who repeated have on average 0.05 more points than students that passed (95% confidence interval, [0.01, 0.08]; the standard deviation of average school grades in that year was 0.63 points).

In summary, our results suggest that grade retention has a positive effect in the short run on future school grades, but that this effect disappears over time — all this being for comparable students in terms of observed covariates that barely pass or repeat the academic year. One possible explanation is that retained students know the material better and are more mature. This is consistent with our other result that reveals that students who barely pass tend to repeat more in the future. Finally, we estimate an almost null effect of grade retention on juvenile crime, with these results being insensitive to small and moderate hidden biases.

8 Other areas of application

We have illustrated our framework for complex discontinuity designs with a running example of the impact of grade retention on education and crime outcomes. However, there are other areas of application of our framework. For example, in education policy, an important question relates to the effects of free-college tuition programs on later-life outcomes. In Chile, free-college programs are in place through specific rules that combine several running
variables, namely, four college admission test scores (in language, mathematics, science, and history) in addition to high school GPA and household income. Students with household income below the country’s median and test scores and GPA above certain cutoffs are eligible for the free-college tuition program. However, the cutoffs vary from college to college, defining different treatment rules with several running variables across colleges.

In labor economics, a complex discontinuity design arises in the study of the effects of the Unemployment Insurance (UI) program in Brazil. In this program, a worker who is laid off receives the UI benefits if two conditions are satisfied: the worker has at least six months of job tenure at layoff and there are at least 16 months between a worker’s layoff date and the layoff date of his/her last successful job application. In this setting, covariates other than the running variables play a central role since unconditional balance checks render standard assumptions for identification implausible; however, it is plausible that, conditional on covariates, treatment assignment is independent of the potential outcomes in a neighborhood of the cutoffs.

In health care policy, the effect of the US Medicare program is a question of interest. Entry into Medicare is based on several criteria, the most common of which is turning age 65 and having paid taxes for ten years (40 quarters) or more. However, one can also enter Medicare through other criteria, including disability and end-stage renal disease. Any of these eligibility criteria could enable a person to enter the Medicare program. End-stage renal disease is in part determined by laboratory tests of kidney function. Disability is determined by physical or mental diagnoses. While many studies have used the age 65 discontinuity, to our knowledge, no studies have combined that with the additional eligibility criteria.

Finally, in clinical medicine, there are many potential examples where the same treatment

\url{https://www.medicare.gov/manage-your-health/i-have-end-stage-renal-disease-esrd/signing-up-for-medicare-if-you-have-esrd}

\url{https://www.kidney.org/sites/default/files/docs/ckd_evaluation_classification_stratification.pdf}
or clinical service is indicated for multiple reasons. For example, if one is interested in the
effect of a procedure, such as surgery, on an economic outcome, that intervention could be
triggered due to various reasons such as bleeding, infection, or trauma, each with its own
threshold for justifying the procedure. In this and other settings in medicine, where many
clinical indications could qualify a patient for a given type of treatment, our framework could
potentially be used.

9 Concluding remarks

Since their introduction in 1960 by Thistlethwaite and Campbell, regression discontinuity
designs have been a powerful method for drawing causal inferences in observational studies.
However, they have often been confined to settings where treatment assignment is determined
by simple treatment assignment rules. Although regression discontinuity designs under the
continuity-based framework have been extended to separately incorporate multiple running
variables or multiple cutoffs, to our knowledge they do not comprehend more multiple treat-
ment rules, such as those found in our running example. To us, a natural and flexible way
of formally understanding a complex discontinuity design is the local randomization frame-
work. However, the identification assumptions in the local randomization framework can be
unrealistic in many settings, because they imply that the mean potential outcome functions
are constant in a neighborhood of the cutoff. In this paper, we conceptualize a discontinuity
design as a local randomized experiment conditional on covariates. This idea has been pro-
posed before by Keele et al. (2015) in the context of a geographic discontinuity design, but
not formalized nor exploited in more generality for more complex treatment assignment rules
(see also Battistin and Rettore 2008, Angrist and Rokkanen 2015, Forastiere et al. 2017, and
Branson and Mealli 2018). This framework allows us to analyze a discontinuity design as
an observational study under strong ignorability, yet in a neighborhood of the cutoffs. As
discussed, this view also facilitates handling discrete running variables, generalizing study
findings, and doing principal stratification analyses under different assumptions.
In the last two sentences of the previous paragraph (and throughout most of this paper) we dropped the word “regression” from “regression discontinuity design,” because, in our view, in the local randomization framework conditional on covariates, there is more going on in a neighborhood of the cutoff(s) than the discontinuity of the mean outcome, regression, function. We are advocating for more flexible analyses in discontinuity designs; for example, using simple graphical displays of the outcomes as in clinical trials and potentially learning heterogeneous effects using statistical machine learning methods. In this paper, we have used matching to adjust for covariates and select the neighborhood, but under the assumptions of local strong ignorability other methods can be used. In observational studies, discontinuities in treatment assignment rules offer a keyhole to see causality. In this paper, we have proposed a different way of understanding and leveraging them with complex treatment rules.

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Appendix A: Covariate balance

Figure 3: Standardized differences in means before and after matching inside the neighborhood $N = \{N_1, N_2\} = \{[[3.5, 4.4] \times [4.3, 4.6]], [[3.5, 4.4] \times [4.8, 5.1]]\}$

Notes: inside $N$, we match exactly on gender, year and month of birth, school and grade attended in 2007, and times of past grade retention. In addition, we match with mean balance on the SIMCE scores in language and mathematics. As a result, the mother and father’s schooling and the household income are also balanced.
Appendix B: Outcome analyses

We have $I = 1,141$ matched pairs. In each pair $i \in \{1, 2, \ldots, I\}$, one student $j \in \{1, 2\}$ repeated the grade and the other passed. Following Rosenbaum (2002b), let $Z_{ij} = 1$ if student $j$ in matched pair $i$ repeats and $Z_{ij} = 0$ otherwise, so $Z_{i1} + Z_{i2} = 1$ for all $i \in \{1, 2, \ldots, I\}$. Student $j$ in pair $i$ exhibits potential outcome $Y_{ij}(1)$ if $Z_{ij} = 1$, and potential outcome $Y_{ij}(0)$ if $Z_{ij} = 0$. In the set $Z$, we collect the $2^I$ possible treatment assignments, $Z = (Z_{11}, Z_{12}, \ldots, Z_{I1}, Z_{I2})^\top$. Under paired randomization, $\Pr\{Z_{ij} = 1 | Y_{ij}(1), Y_{ij}(0), u_{ij}, X_{ij}, Q_{ij} \in \mathcal{N}, Z\} = 1/2$, where $u_{ij}$ is an unobserved covariate. Let $Y = (Y_{11}, Y_{12}, \ldots, Y_{I1}, Y_{I2})^\top$ denote the vector of the observed outcomes for the $2I$ students and let $Y(0) = (Y_{11}(0), Y_{12}(0), \ldots, Y_{I1}(0), Y_{I2}(0))^\top$ stand for the vector of potential outcomes under control for the $2I$ students. If $t(Z, Y)$ is a test statistic, then in a paired randomized experiment under Fisher’s null hypothesis of no treatment effect, the distribution of $t(Z, Y)$ is the permutation distribution

$$\Pr(t(Z, Y) > k | Y_{ij}(1), Y_{ij}(0), u_{ij}, X_{ij}, Q_{ij} \in \mathcal{N}, Z) = \frac{|\{Z \in Z : t(Z, Y(0)) > k\}|}{2^I}. \quad (20)$$

In our running example, the three outcomes of interest are binary. The first outcome takes the value 1 if the student committed a crime after 2007 and 0 otherwise; the second takes the value 1 if the student repeated a grade after 2007 and 0 otherwise; and the third takes the value 1 if the student dropped out of school after 2007 and 0 otherwise. To test $H_0$, we use McNemar’s test statistic: $t(Z, Y) = \sum_{i=1}^I \sum_{j=1}^2 Z_{ij} Y_{ij}$, i.e., the number of responses equal to 1 among treated students. The results are as follows.

Table 3: Committing a juvenile crime in matched pairs. The table counts pairs, not students.

| Passed | Juvenile crime = 0 | Juvenile crime = 1 |
|--------|-------------------|-------------------|
| Repeated | 1018 | 55 |
| Juvenile crime = 0 | 62 | 6 |

Table 4: Dropping out of school in matched pairs. The table counts pairs, not students.

| Passed | Dropped out = 0 | Dropped out = 1 |
|--------|----------------|----------------|
| Repeated | 945 | 84 |
| Dropped out = 0 | 87 | 25 |
Table 5: Repeating another grade in matched pairs. The table counts pairs, not students.

| Repeated Future retention | Passed Future retention = 0 | Passed Future retention = 1 |
|---------------------------|-----------------------------|-----------------------------|
| Future retention = 0      | 290                         | 341                         |
| Future retention = 1      | 222                         | 288                         |

Appendix C: Sensitivity analyses

In the absence of hidden bias, there is strong evidence in our running example that current grade retention causes a reduction in future grade retention. How much hidden bias would need to be present to explain away this result? To answer this question, we implement the sensitivity analysis described in Rosenbaum (2002b, Chapter 4).

Let \( \pi_{ij} \) denote the probability that student \( j \) in pair \( i \) repeats the grade (receives treatment). For each each pair \( i \), two students match on their observed covariates, \( X_{ij} = X_{ij'} \), but may differ on an unobserved covariate \( u_{ij} \neq u_{ij'} \), such that \( \pi_{ij} \neq \pi_{ij'} \). Suppose that the odds of repeating the grade differ at most by a factor \( \Gamma \geq 1 \)

\[
\frac{1}{\Gamma} \leq \frac{\pi_{ij}(1 - \pi_{ij'})}{\pi_{ij'}(1 - \pi_{ij})} \leq \Gamma
\]

for each pair \( i \). If \( \Gamma = 1 \), then there is no hidden bias and the randomization distribution with \( \pi_{ij} = \pi_{ij'} = 1/2 \) for McNemar’s test statistic is valid. If \( \Gamma > 1 \), then there is hidden bias and there is a range of possible inferences for \( \pi_{ij} \neq \pi_{ij'} \). These inferences are bounded by \( \Gamma \) and \( 1/\Gamma \). For these two values, we obtain two extreme-case \( p \)-values. We look for the largest value of \( \Gamma \) such that we reject the null hypothesis of no treatment effect.

In our running example, we are able to reject the null hypothesis that current grade retention does not causes future grade retention for \( \Gamma = 1.34 \) (the upper bound of the \( p \)-value is 0.049) but not for \( \Gamma > 1.34 \) (the upper bound of the \( p \)-value is greater than 0.05). In other words, two students matched for their observed covariates could differ in their odds of grade retention by 34% without materially altering the conclusions about the effect of current retention on future grade retention.
Appendix D: Sensitivity analyses on a near equivalence test

As said in Section 7.2.2 in the absence of hidden bias, we have found evidence that grade retention does not cause dropping out of school or committing a juvenile crime. However, bias from a hidden covariate can give the impression that a treatment effect does not exist when in fact there is one. How much bias from a hidden covariate would need to be present to mask an actual treatment effect? We answer this question by conducting a sensitivity analysis on a near-equivalence test. For details, see Rosenbaum and Silber (2009) and Zubizarreta et al. (2013).

Using the parameter $\Gamma$, we will test the null hypothesis that the effect of treatment is larger than a given effect size against the alternative hypothesis that it is lower. We take the effect size from previous studies in the literature. Our results will be insensitive to hidden bias if large values of $\Gamma$ are required to mask a given effect size. Before presenting the results of this sensitivity analysis, it is necessary to introduce the concept of an attributable effect (Rosenbaum 2002a).

For each of the two outcomes, committing a juvenile crime or dropping out of school, let $\delta$ denote the $2I$-dimensional vector of treatment effects, with $\delta_{ij} = Y_{ij}(1) - Y_{ij}(0)$. Consider the hypothesis $H_{\delta_0} : \delta = \delta_0$, where $\delta_0$ is a $2I$-dimensional vector with values $\delta_{0ij} \in \{-1, 0, 1\}$. Clearly, not all hypotheses of this form are consistent with the data at hand, since $Y_{ij} - Z_{ij}\delta_{0ij} = Y_{ij}(0)$ and $Y_{ij} + (1 - Z_{ij})\delta_{0ij} = Y_{ij}(1)$ must both be in $\{0, 1\}$. Rosenbaum (2002a) summarizes hypotheses of the form $H_{\delta_0} : \delta = \delta_0$ by introducing the attributable effect, which is defined as $\Delta = \sum_{i=1}^{I} \sum_{j=1}^{2} Z_{ij}\delta_{ij}$. In other words, the attributable effect is the number of treated students who experienced events caused by the treatment.

We assume that $Y_{ij}(1) \geq Y_{ij}(0)$ for each student $j$ in pair $i$; that is, that the exposure to treatment may cause the outcome in a student who would not otherwise experience it, but the treatment does not prevent the outcome in a student who would otherwise experience it. We consider the hypothesis $H_{\delta_0} : \delta = \delta_0$, where $\delta_{0ij} \in \{0, 1\}$. We can repeat this exercise reversing the roles of treatment and control. We again use McNemar’s test statistic. Under $H_{\delta_0} : \delta = \delta_0$, $\Delta$ may be calculated using $\delta_0$, $\Delta_0 = \sum_{i=1}^{I} \sum_{j=1}^{2} Z_{ij}\delta_{0ij}$ and $t(Z, Y(0))$ can be computed. A value of $\Delta_0$ is rejected if every hypothesis $H_{\delta_0} : \delta = \delta_0$ with $\delta_{0ij} \in \{0, 1\}$ that gives rise to this value of $\Delta_0$ is rejected. Given $\Delta_0$, among all the hypotheses $H_{\delta_0} : \delta = \delta_0$ with $\delta_{0ij} \in \{0, 1\}$ that give rise to this value of $\Delta_0$, there is one that is the most difficult to reject, such that if it is rejected then the associated value of $\Delta_0$ is rejected. Rosenbaum (2001) proves that the hypothesis that is the most difficult to reject has $\sum_{j=1}^{2} Z_{ij}\delta_{ij} = 1$ for as many pairs with $Y_{i1} + Y_{i2} = 2$ as possible.
When conducting the test of equivalence on the matched pairs, we use two values for the attributable effect on juvenile crime ($\Delta^{\text{crime}}_0 \in \{30, 40\}$) and also two values for the attributable effect on dropping out of school ($\Delta^{\text{drop}}_0 \in \{40, 50\}$). For example, an attributable effect on juvenile crime of 40 is equivalent to saying that 40 juvenile crimes are attributed to grade retention. The order of magnitude of these attributable effects are taken from previous studies in the literature. Utilizing a standard fuzzy RDD, Erena et al. (2017) estimate the impact of grade retention on dropping out of school in the state of Louisiana, finding a point estimate of 4.8% (equivalent to 55 cases of dropping out of school attributed to grade retention in our context), whereas Díaz et al. (2018) estimate the effect of grade retention on juvenile crime in Chile, obtaining a point estimate of 4.3% (equivalent to 49 juvenile crimes attributed to grade retention in our application).

We are able to reject the null hypothesis that $H_0 : \Delta^{\text{crime}} \geq 30$ for $\Gamma = 1.12$ with an upper bound of the $p$-value of 0.049 (Table 6 presents the adjusted pairs), whereas we reject the null hypothesis that $H_0 : \Delta^{\text{crime}} \geq 40$ for $\Gamma = 1.46$ with an upper bound of the $p$-value of 0.047 (Table 7 presents the adjusted pairs). These results reveal that two students matched on the basis of their observed covariates could differ in their odds of repeating the grade in 2007 by almost 12% before masking an attributable effect on juvenile crime of 30, and by almost 46% before masking an attributable effect of 40. In the same manner, $H_0 : \Delta^{\text{drop}} \geq 40$ is rejected for $\Gamma = 1.16$ with the upper bound of the $p$-value of 0.047 (Table 8 presents the adjusted pairs), whereas $H_0 : \Delta^{\text{drop}} \geq 50$ is also rejected for $\Gamma = 1.33$ with the upper bound of the $p$-value of 0.045 (Table 9 presents the adjusted pairs). In words, these results suggest that two students matched for their observed covariates could differ in their odds of repeating the grade in 2007 by almost 16% before masking an attributable effect of 40 on dropping out of school, and by almost 33% before masking an attributable effect of 50.

Table 6: Juvenile crime indicators in matched pairs adjusted for the null hypothesis $H_0 : \delta^{\text{crime}} = \delta^{\text{crime}}_0$ that attributes $\sum_{i=1}^{T} \sum_{j=1}^{2} Z_{ij} \delta^{\text{crime}}_{ij} = 30$ juvenile crimes to grade retention.

| Passed  | Juvenile crime = 0 | Juvenile crime = 1 |
|---------|-------------------|-------------------|
| Repeated|                   |                   |
| Juvenile crime = 0 | 1042             | 61               |
| Juvenile crime = 1 | 38              | 0                |

The table counts pairs, not students.
Table 7: Juvenile crime indicators in matched pairs adjusted for the null hypothesis $H_0: \delta^{crime} = \delta^{crime}_0$ that attributes $\sum_{i=1}^{I} \sum_{j=1}^{2} Z_{ij}^{\delta^{crime}} = 40$ juvenile crimes to grade retention. The table counts pairs, not students.

|                | Passed                |
|----------------|-----------------------|
|                | Juvenile crime = 0    | Juvenile crime = 1 |
| Juvenile crime = 0 | 1052                  | 61                 |
| Juvenile crime = 1 | 28                    | 0                  |

Table 8: Dropping out of school indicators in matched pairs adjusted for the null hypothesis $H_0: \delta^{drop} = \delta^{drop}_0$ that attributes $\sum_{i=1}^{I} \sum_{j=1}^{2} Z_{ij}^{\delta^{drop}} = 40$ cases of dropping out of school to grade retention. The table counts pairs, not students.

|                | Passed                |
|----------------|-----------------------|
|                | Dropped out = 0       | Dropped out = 1    |
| Dropped out = 0 | 960                   | 109                |
| Dropped out = 1 | 72                    | 0                  |

Table 9: Dropping out of school indicators in matched pairs adjusted for the null hypothesis $H_0: \delta^{drop} = \delta^{drop}_0$ that attributes $\sum_{i=1}^{I} \sum_{j=1}^{2} Z_{ij}^{\delta^{drop}} = 50$ cases of dropping out of school to grade retention. The table counts pairs, not students.

|                | Passed                |
|----------------|-----------------------|
|                | Dropped out = 0       | Dropped out = 1    |
| Dropped out = 0 | 970                   | 109                |
| Dropped out = 1 | 62                    | 0                  |
Appendix E: Generalizing the results

Here we generalize our results to a target population. Under the Assumptions 1a', 1b', 2a, and 2b, we can estimate the average treatment effect on a target population with running variables in the neighborhood. We define the target population on the basis of household income. Among students with values of the running variables in the neighborhood previously determined, the target population consists of students in the poorest 50% of households.

Following Zubizarreta et al. (2018), we use cardinality matching to find the largest matched sample that is not only balanced across treatment groups, but is also balanced around the distribution of a target population of interest. Within the window previously determined, Table 10 compares the means of the treated and control students in the matched sample to the means of students in the poorest 50% of households, whereas Table 11 compares the means of the treated and control students in the representative matched sample to the means of students in the poorest 50% of households. To find the representative matched sample, we require that the absolute standardized differences in means between the matched treated students and all students in the target population, and between the matched control students and all students in the target population, are all smaller than 0.05.

Having found a representative matched sample of 283 pairs of students, we assume that treatment is as-if randomly assigned to students conditional on the matched pairs. Table 12 presents the results when generalizing the effect estimates to the population of the poorest 50% of students with grades in the selected neighborhood, while Tables 13, 14, and 15 detail the discordant pairs for committing a crime, dropping out of school, and repeating another grade, respectively. In the absence of hidden bias, there is no evidence that grade retention causes juvenile crime (the two-sided p-value is 0.359) nor dropping out of school (p-value = 0.464), whereas there is statistically significant evidence that grade retention reduces the probability of future grade retention (p-value = 0.049).
Table 10: Description of the target population and the matched sample within the window $\mathcal{N} = \{\mathcal{N}_1, \mathcal{N}_2\} = \{[[3.5, 4.4] \times [4.3, 4.6]], [[3.5, 4.4] \times [4.8, 5.1]]\}$

| Covariate                      | Full sample mean of the poorest 50% of households | Matched sample mean Treated | Matched sample mean Control |
|--------------------------------|---------------------------------------------------|-----------------------------|-----------------------------|
| Male                           | 0.60                                              | 0.60                        | 0.60                        |
| Born in 1992                   | 0.05                                              | 0.01                        | 0.01                        |
| Born in 1993                   | 0.14                                              | 0.12                        | 0.12                        |
| Born in 1994                   | 0.22                                              | 0.26                        | 0.26                        |
| Born in 1995                   | 0.18                                              | 0.24                        | 0.24                        |
| Born in 1996                   | 0.10                                              | 0.17                        | 0.17                        |
| Born in 1997                   | 0.07                                              | 0.11                        | 0.11                        |
| Born in 1998                   | 0.08                                              | 0.09                        | 0.09                        |
| Born in January                | 0.09                                              | 0.10                        | 0.10                        |
| Born in February               | 0.08                                              | 0.08                        | 0.08                        |
| Born in March                  | 0.08                                              | 0.09                        | 0.09                        |
| Born in April                  | 0.08                                              | 0.08                        | 0.08                        |
| Born in May                    | 0.09                                              | 0.06                        | 0.06                        |
| Born in June                   | 0.09                                              | 0.07                        | 0.07                        |
| Born in July                   | 0.08                                              | 0.08                        | 0.08                        |
| Born in August                 | 0.08                                              | 0.09                        | 0.09                        |
| Born in September              | 0.08                                              | 0.08                        | 0.08                        |
| Born in October                | 0.09                                              | 0.09                        | 0.09                        |
| Born in November               | 0.09                                              | 0.09                        | 0.09                        |
| Born in December               | 0.09                                              | 0.09                        | 0.09                        |
| Public School                  | 0.68                                              | 0.43                        | 0.43                        |
| Subsidized School              | 0.32                                              | 0.57                        | 0.57                        |
| 3th grade                      | 0.07                                              | 0.05                        | 0.05                        |
| 4th grade                      | 0.08                                              | 0.08                        | 0.08                        |
| 5th grade                      | 0.17                                              | 0.17                        | 0.17                        |
| 6th grade                      | 0.31                                              | 0.21                        | 0.21                        |
| 7th grade                      | 0.23                                              | 0.32                        | 0.32                        |
| 8th grade                      | 0.13                                              | 0.17                        | 0.17                        |
| Previous grade retention       | 0.39                                              | 0.09                        | 0.09                        |
| SIMCE Math                     | -0.53                                             | -0.26                       | -0.25                       |
| SIMCE Language                 | -0.51                                             | -0.27                       | -0.26                       |
| Father schooling               | 9.27                                              | 11.37                       | 11.42                       |
| Mother schooling               | 9.08                                              | 11.13                       | 11.24                       |
| Household income per capita    | 40,720.06                                         | 92,373.58                   | 92,297.38                   |

Notes: in a neighborhood of the cutoffs, we do exact matching on school, grade attended in 2007, gender, birth month and year, and times of past grade retention, while mean balance matching on SIMCE’s score in math and language. The secondary covariates are father and mother schooling, and household income per capita.
Table 11: Description of the target population and the representative matched sample within the window $\mathcal{N} = \{N_1, N_2\} = \{[3.5, 4.4] \times [4.3, 4.6], \{3.5, 4.4] \times [4.8, 5.1]\}$.

| Covariate                  | Full sample mean of the poorest 50% of households | Matched sample mean |
|----------------------------|--------------------------------------------------|---------------------|
| Male                       | 0.60                                             | 0.61                |
| Born in 1992               | 0.05                                             | 0.04                |
| Born in 1993               | 0.14                                             | 0.12                |
| Born in 1994               | 0.22                                             | 0.20                |
| Born in 1995               | 0.18                                             | 0.18                |
| Born in 1996               | 0.10                                             | 0.09                |
| Born in 1997               | 0.07                                             | 0.08                |
| Born in 1998               | 0.08                                             | 0.10                |
| Born in January            | 0.09                                             | 0.07                |
| Born in February           | 0.08                                             | 0.07                |
| Born in March              | 0.08                                             | 0.08                |
| Born in April              | 0.08                                             | 0.09                |
| Born in May                | 0.09                                             | 0.07                |
| Born in June               | 0.09                                             | 0.07                |
| Born in July               | 0.08                                             | 0.07                |
| Born in August             | 0.08                                             | 0.08                |
| Born in September          | 0.08                                             | 0.10                |
| Born in October            | 0.09                                             | 0.10                |
| Born in November           | 0.09                                             | 0.09                |
| Born in December           | 0.09                                             | 0.09                |
| Public School              | 0.68                                             | 0.66                |
| Subsidized School          | 0.32                                             | 0.34                |
| 3th grade                  | 0.07                                             | 0.07                |
| 4th grade                  | 0.08                                             | 0.07                |
| 5th grade                  | 0.17                                             | 0.17                |
| 6th grade                  | 0.31                                             | 0.33                |
| 7th grade                  | 0.23                                             | 0.25                |
| 8th grade                  | 0.13                                             | 0.12                |
| Previous grade retention   | 0.39                                             | 0.36                |
| SIMCE Math                 | -0.53                                            | -0.55               |
| SIMCE Language             | -0.51                                            | -0.54               |
| Father schooling           | 9.27                                             | 9.41                |
| Mother schooling           | 9.08                                             | 9.20                |
| Household income per capita| 40,720.06                                         | 42,641.51           | 43,895.37 |

Notes: in a neighborhood of the cutoffs, we balance the above covariates around the distribution of a target population of interest.
Table 12: Generalizing the results in the target population of the poorest 50% of students with running variables in $N = \{N_1, N_2\} = \{[[3.5, 4.4] \times [4.3, 4.6]], [[3.5, 4.4] \times [4.8, 5.1]]\}$ in the absence of hidden bias.

| Outcome variable            | Matched sample mean | $\hat{\tau}_{\text{TATE}}$ | $H_0: \tau_{\text{TATE}} = 0$ |
|-----------------------------|---------------------|-----------------------------|---------------------------------|
|                             | Treated             | Control                     |                                 |
| Committing a crime          | 0.078               | 0.095                       | -0.017                          | 0.359                           |
| Future grade retention      | 0.477               | 0.555                       | -0.078                          | 0.049                           |
| Dropping out of school      | 0.191               | 0.173                       | 0.018                           | 0.464                           |

Notes: we report the two-sided $p$-value for McNemar’s test of no effect on the TATE.

Table 13: Generalizing the results: juvenile crime indicators in the representative matched pairs sample. The table counts pairs, not students.

|                | Passed                           |
|----------------|----------------------------------|
| Repeated       | Juvenile crime = 0 | Juvenile crime = 1         |
| Juvenile crime = 0 | 238                   | 23                        |
| Juvenile crime = 1 | 18                    | 4                         |

Table 14: Generalizing the results: dropping out of school indicators in the representative matched pairs sample. The table counts pairs, not students.

|                | Passed                           |
|----------------|----------------------------------|
| Repeated       | Dropped out = 0 | Dropped out = 1           |
| Dropped out = 0 | 198                   | 31                        |
| Dropped out = 1 | 36                    | 18                        |

Table 15: Generalizing the results: future grade retention indicators in the representative matched pairs sample. The table counts pairs, not students.

|                | Passed                           |
|----------------|----------------------------------|
| Repeated       | Future retention = 0 | Future retention = 1      |
| Future retention = 0 | 68                         | 80                        |
| Future retention = 1 | 58                         | 77                        |