REGRESSION DISCONTINUITY DESIGNS USING COVARIATES: IMPACT OF SCHOOL GRADE RETENTION ON LATER LIFE OUTCOMES IN CHILE

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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. Motivated by the problem of estimating the impact of grade retention on educational and juvenile crime outcomes in Chile, we propose a framework and methods for complex discontinuity designs that encompasses multiple treatment rules. 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. 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 complementary regression modeling adjustments. We present assumptions for generalization; that is, for identification and estimation of average treatment effects for target populations. We also describe two approaches to select the neighborhood for analysis. We find that grade retention in Chile has a negative impact on future grade retention, but is not associated with dropping out of school or committing a juvenile crime.

1. Introduction.

1.1. Impact of grade retention in Chile. What is the impact of childhood grade retention on later life outcomes? This is a central question in education where two visions compete. From one perspective, through a negative impact on motivation and self esteem, which may in turn reduce the effectiveness of educational inputs, repeating a grade is associated with dropout later in schooling and other negative long-term effects. Alternatively, children who are lagging behind may benefit from repeating a grade by learning the content that they have missed and fitting better with younger peers.

In Chile, for example, school grades vary between 1 and 7, by increments of 0.1. In this grade system, 7 stands for “Outstanding,” 4 denotes “Sufficient,” and 1 is “Very Deficient.” The child fails a subject with a grade below 4.0. They repeat the year under either of the following two 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 this context, we ask what is the effect of grade retention during childhood on later educational and criminal outcomes.

To answer this question, one option is to compare students who passed to those who repeated by carefully adjusting for differences in educational and socioeconomic background characteristics or covariates. However, this comparison will likely be biased by differences in covariates between the two groups that we fail to observe such as the students’ ability.

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and motivation. Manifestly, in this question there are notions of proximity and chance, in
which two similar students (with similar past grades and socioeconomic backgrounds) have
marginally different results in a school subject, yet one ends up barely passing the grade
while the other must repeat, in a haphazard way. These notions evoke the idea of a regression
discontinuity design, where treated and control units are compared in a vicinity of a threshold
to receive treatment. However, to our knowledge, existing methods do not readily apply to
this and other related problems that simultaneously involve multiple treatment assignment
variables and thresholds.

1.2. Review of discontinuity designs.

1.2.1. Elements and frameworks. 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 a covariate called the assignment, forcing, or run-
nning variable, such that for values of this covariate greater or smaller than a given cutoff,
subjects are assigned to treatment or control. The basic intuition behind this design is that
subjects just below the cutoff (who are not assigned to treatment) are good counterfactual
comparisons to those just above the cutoff (who are assigned to treatment). 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, Idrobo
and Titiunik (2019, 2020).

Following Cattaneo, Idrobo and Titiunik (2019, 2020), there are two frameworks for in-
terpreting and analyzing discontinuity designs: the continuity-based framework, which is
asymptotic and identifies the effect of treatment at the cutoff (e.g., Hahn, Todd and Van der
Klaauw, 2001; Calonico, Cattaneo and Titiunik, 2014; Gelman and Imbens, 2018; Imbens
and Wager, 2019), and the local randomization framework, which is limitless, and formu-
lates the design as a local randomized experiment around the cutoff (e.g., Cattaneo, Frandsen
and Titiunik, 2015; Li, Mattei and Mealli, 2015; Mattei and Mealli, 2016; Sales and
Hansen, 2020). Two important cases are addressed under both frameworks: the sharp design,
where the treatment assignment rule is deterministic, and the fuzzy design, where the rule
is not deterministic (e.g., if there is imperfect compliance with the treatment assignment). In
both frameworks, most applications involve a single running variable with a single cutoff;
however, as we describe in Section 1.3, many policies actually rely on more than one such
variables and thresholds to determine treatment assignment.

1.2.2. Discontinuity designs with multiple cutoffs. To the best of our knowledge, there
are methods that separately incorporate multiple cutoffs and multiple running variables, but
not both simultaneously. One set of methods encompasses single running variables with mul-
tiple cutoffs; the other, multiple running variables each with a single cutoff. Examples of the
former include Chay, McEwan and Urquiola (2005), who estimate the effect of government
school funding on students’ achievement in Chile, where a school test score cutoff that varies
across geographic regions determines funding eligibility; Egger and Koethenbuerger (2010),
who analyze the effect of the size of city government councils on municipal expenditures in
Germany, where the size of the council depends on population cutoffs at the municipality
level; and De La Maza (2012), who studies the impact of Medicaid benefits on health care
utilization, where household income cutoffs determine Medicaid benefits.

A widespread analytic approach in the presence of multiple cutoffs is to pool the data from
the multiple cutoffs to produce a single effect estimate. More specifically, in this approach
the running variable of each observation is centered around its corresponding cutoff, the data is pooled across the observations, and a single treatment effect estimate is obtained under the continuity-based framework via local linear regressions (e.g., Cattaneo, Idrobo and Titiunik, 2020). By pooling the information across different cutoffs, this estimator can have improved efficiency over the analogous estimator at each cutoff, yet at the cost of using the same kernel and bandwidth for all the cutoffs (see Cattaneo et al., 2016 for an analysis and interpretation of this pooled estimator under different assumptions). An alternative approach leverages the multiple cutoffs to estimate more general average treatment effects than those for the individuals observed around the cutoffs. For instance, Bertanha (2020) proposes an estimator for the average treatment effect when heterogeneity is explained by certain cutoffs characteristics.

1.2.3. **Discontinuity designs with multiple running variables.** In discontinuity designs with multiple running variables and single cutoffs, most existing methods again fall under the continuity-based framework and use local polynomial regression methods. Several of these developments have been motivated by problems in educational and in geographic settings. For example, Jacob and Lefgren (2004) study the effect of grade retention on student achievement, where retention is determined by cutoffs in both math and reading exams; and Matsudaira (2008) analyzes the impact of summer school on student attainment, where summer school attendance is defined by cutoffs in two standardized tests. In geographic settings, this design arises when units are assigned to treatment on the basis of a geographic boundary. For instance, using latitude and longitude as running variables, Keele and Titiunik (2015) analyze the impact of political advertisements on voter turnout, and Branson et al. (2019) study the effect of school district location on house prices (see also Keele et al., 2017). Naturally, in these settings the selection of the neighborhood and other relevant parameters such as the kernel function is more difficult than in one-dimensional settings (see, e.g., Imbens and Wager, 2019 for a discussion). Some applied works avoid these choices by restricting the data (e.g., Jacob and Lefgren, 2004; Matsudaira, 2008) or aggregating the running variables into a single dimension (e.g., Cattaneo, Idrobo and Titiunik, 2020) to then apply local polynomial regression methods for a single running variable. Alternatively, Imbens and Wager (2019) directly minimize a bound of the conditional mean-squared error of a weighting estimator via explicit optimization. As the authors discuss, an important aspect of this method is the selection of two tuning parameters, which can be challenging with multiple running variables.

1.2.4. **Discontinuity designs with discrete running variables.** In discontinuity designs, an added complication arises when the running variables are discrete. The literature distinguishes between two cases, where the running variable is truly discrete (as in our study), and where this variable is rounded or measured with error (see Pei and Shen, 2017 and Bartalotti, Brummet and Dieterle, 2020). Examples of discontinuity designs with discrete running variables include Card and Shore-Sheppard (2004), who estimate the impact of Medicaid expansions on the health insurance status of low-income children, where a child age cutoff determines Medicaid coverage, and reported children age is rounded in years; Almond et al. (2010) and Barreca et al. (2011), who study the impact of low birth weight classification on infant mortality, where birth weight is rounded to different gram and ounce multiples; and Manacorda (2012) who analyzes the impact of grade retention on students’ achievement, where retention is mandatory for students that fail three or more school subjects, and the running variable is the number of failed subjects. Under the continuity-based framework, Dong (2015) shows that estimation using a discrete running variable leads to inconsistent estimates of treatment effects at the cutoff, even when the true regression function is known and is correctly specified. This implies that when the running variable takes only a few distinct values,
the methods for estimation, inference, and bandwidth selection under the continuity-based framework via local polynomials do not apply. See Kolesar and Rothe (2018) who propose methods for constructing confidence intervals with guaranteed coverage properties when the running variable takes a moderate number of distinct values (see also Lee and Card, 2008). As Cattaneo, Idrobo and Titiunik (2020) argue, the local randomization framework is more natural for analysis in this case.

1.3. Other areas of application. Besides our motivating example, there are other areas of application where complex treatment rules with multiple (possibly discrete) running variables and multiple cutoffs that lead to the same treatment arise. For example, in education, an important question relates to the effects of free-college tuition programs on long-term 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 Grade Point Average (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 health care policy, the effect of the US Medicare program on health care utilization and patient well-being is a question of great 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 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 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 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 outcome, that procedure could be triggered due to various reasons such as bleeding, infection, or trauma, each with its own threshold. 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.

1.4. Contribution and outline of the paper. Motivated by an observational study of the impact of grade retention on educational and juvenile crime outcomes in Chile, 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 lead to the same treatment. To our knowledge, existing approaches to discontinuity designs cannot readily accommodate such complex treatment rules. While it may be possible to analyze complex discontinuity designs under existing frameworks, e.g., by collapsing and simplifying the components of a complex treatment rule into a single dimension, this process may overlook important aspects of the assignment mechanism, such that its original structure is not preserved. Moreover, relevant information can be lost, and one may end up comparing treated and control units that are distant from the cutoff on one of the running variables. Fundamentally, the identification assumptions of existing frameworks are not plausible in the context of our case study, because relevant covariates are imbalanced even in the smallest possible neighborhood around the cutoffs. In the proposed framework, the observed covariates other than the running variable(s) play a central role for identification of average treatments effects in a neighborhood of the cutoffs via a local strong ignorability assumption; that is,
via local unconfoundedness and local positivity assumptions. Our work builds on the local randomization framework as formalized by Cattaneo, Frandsen and Titiunik (2015) and Li, Mattei and Mealli (2015), where treatment assignment is analyzed as in a local randomized experiment. More specifically, we follow Keele, Titunik and Zubizarreta (2015), who use the local unconfoundedness assumption in the context of a geographic discontinuity design (see also Keele et al., 2017), and Branson and Mealli (2018), who extend the local randomization framework and formalize general assignment mechanisms with varying propensity scores. Similar assumptions have been invoked by Battistin and Rettore (2008), Angrist and Rokkanen (2015), and Forastiere, Mattei and Mealli (2017). Mattei and Mealli (2016) articulates how this assumption and positivity form local strong ignorability. All these frameworks and methods, however, encompass simple treatment rules.

From a methodological standpoint, one of the points we wish to make in this paper is that, under the assumption of local strong ignorability, we cannot only handle complex discontinuity designs in a straightforward manner, but also facilitate different outcome analyses than those conventionally done in discontinuity designs. As we illustrate, this framework facilitates simple graphical displays of the outcomes across treatment groups as is done in the analysis of clinical trials, sensitivity analyses on near equivalence tests in matched observational studies, and methods for outcome analysis that combine matching and weighting with regression adjustments. To our knowledge, none of these analyses have been performed in discontinuity designs and some of them are facilitated by our framework. Further, under assumptions that we formalize for the first time in the context of a discontinuity design, we can also generalize the study findings of the discontinuity design to a target population. The validity of these analyses is predicated in forms of the strong ignorability assumption. 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.

From a substantive standpoint, our work contributes to the educational policy literature by providing new evidence of the impact of grade retention on crime and educational outcomes in a developing country. The case of Chile is relevant because its educational system is especially market-oriented and economically segregated, with high levels of grade retention among socially vulnerable students. ¹ In the United States, previous works that examine the effect of school grade retention using discontinuity designs include: Jacob and Lefgren (2004), who documented that grade retention increased the probability of dropping out of school among students in Chicago; Schwerdt, West and Winters (2017), who found that third grade retained students in Florida tended to have higher grade point averages, but no difference in their probability of graduating; and Erena, Depew and Barnes (2017), who found no effect of grade retention on juvenile crime for 4th grade students and a small negative effect for 8th grade students in Louisiana, while Eren, Lovenheim and Mocan (2021) found that grade retention had a positive impact on 8th graders’ future likelihood of committing crime during adulthood. In Chile, Díaz et al. (2021) estimated the effect of grade retention on school dropout and juvenile crime, finding a negative effect on both outcomes. Our work expands on the latter by covering more study cohorts and analyzing a more comprehensive set of educational and criminal outcomes. Furthermore, we use a framework for complex decision rules in order to analyze the effect of grade retention in terms of the primitive rules that determine it. All previous works follow the continuity-based framework, which is not viable in our case study because its basic assumptions are not satisfied in the observed data.

¹In fact, in 2007 the overall grade retention rate was 5.8%, with a rate of 6.7% among students attending public schools (more vulnerable students), as opposed to just 1.7% among students in private schools.
This paper is organized as follows. In Section 2, we describe the data of our case study. In Section 3, we present our framework for complex discontinuity designs, including the notation, estimands, and assumptions for identification and generalization. In Section 4, we discuss estimation using matching, weighting, and regression-assisted matching and weighing approaches. In Section 5, we propose methods for selecting the neighborhood for analysis. In Section 6, we present the results of our case study. In Section 7, we outline extensions to the fuzzy case. In Section 8, we conclude with some remarks.

2. Educational and criminal administrative records. In our case study, we use an 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 includes all students who were enrolled in the Chilean educational system. The data set contains detailed educational and sociodemographic variables of the students, their families, and their schools. In particular, between 2002 and 2006, our baseline covariates include the student’s gender, age, attendance, grades, and standardized test scores, the parents’ education and incomes, and the school and grade attended. In 2007, we have the school grades of all students, disaggregated by subject. 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 as outcomes the yearly average school grade in 2008-2011, and the binary variables of grade retention, school dropout and prosecution for a criminal offense in 2008-2016. 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.

3. A framework for complex discontinuity designs. We consider complex 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. To describe this framework, we introduce the following notation.

3.1. Notation. For each unit $i$, let $R_{ij}$ be the vector of running variables that determine treatment assignment rule $j$, with $j = 1, ..., J$. For each $R_{ij}$, let $R_{ijk}$ be its $k$th component, with $k = 1, ..., K_j$. Based on $R_{ij}$, each unit is assigned to treatment $j$ according to the rule

$$Z_{ij} = \begin{cases} 
1, & \text{if } R_{ijk} \in (-\infty, c_{jk}) \text{ for all } k = 1, ..., K_j \\
0, & \text{otherwise},
\end{cases}$$

where $c_{jk}$ is the cutoff for running variable $k$ under rule $j$. Let $Z_i$ denote the treatment indicator, with $Z_i = 1$ if unit $i$ receives the treatment and $Z_i = 0$ otherwise. In our case study, we consider treatment assignment rules that lead to the same treatment so

$$Z_i = \begin{cases} 
1, & \text{if } Z_{ij} = 1 \text{ for some } j = 1, ..., J \\
0, & \text{otherwise}.
\end{cases}$$

2Specifically, we measure grade retention during elementary education, when students generally take the same subjects and retention rules 1 and 2 apply nationally. In Chile, students are evaluated within schools (for this reason, our outcome comparisons are within schools and school grades, among other covariates, by exact matching; see Section 6 for details).
Let \( N_{ij} \) be an indicator variable that denotes if unit \( i \) is in a neighborhood of the cutoffs of assignment rule \( j \). Specifically,

\[
N_{ij} = \begin{cases} 
1, & \text{if } R_{ijk} \in [c_{jk} - \delta_{jk}, c_{jk} + \delta_{jk}] \text{ for all } k = 1, \ldots, K_j \\
0, & \text{otherwise,}
\end{cases}
\]

where \( \delta_{jk} \) and \( \delta_{jk} \) are positive scalars that define the neighborhood below and above cutoff \( c_{jk} \), respectively. Let \( N_i \) be an indicator variable that denotes if unit \( i \) is in a neighborhood of cutoffs where the identification assumptions (described in Section 3.3) hold

\[
N_i = \begin{cases} 
1, & \text{if } N_{ij} = 1 \text{ for some } j = 1, \ldots, J \\
0, & \text{otherwise.}
\end{cases}
\]

By way of illustration, in our case study there are \( J = 2 \) treatment rules, with \( R_1 = (R_{11}, R_{12})^\top \), where \( R_{11} \) is the lowest grade across all school subjects and \( R_{12} \) is the average grade across all subjects, and \( R_2 = (R_{21}, R_{22})^\top \), where \( R_{21} \) is the second lowest grade across all school subjects and \( R_{22} \) is the average grade across all subjects, so \( R_{12} = R_{22} \). Units are in the neighborhood of rule 1 \( (N_1 = 1) \) if \( R_i \in [c_{11} - \delta_{11}, c_{11} + \delta_{11}] \times [c_{12} - \delta_{12}, c_{12} + \delta_{12}] \), and/or in the neighborhood of rule 2 \( (N_2 = 1) \) if \( R_i \in [c_{21} - \delta_{21}, c_{21} + \delta_{21}] \times [c_{22} - \delta_{22}, c_{22} + \delta_{22}] \). Here, a student can be simultaneously in the neighborhood of two cutoffs, for example, of \( c_{11} \), corresponding the lowest grade across all school subjects, and of \( c_{12} = c_{22} \), the average grade across all subjects. See Figure 1 for details.

Fig 1: In our case study, there are two treatment rules that lead to the same treatment, which is grade retention. Under treatment rule 1, the treatment assignment region is given by the area with dashed lines in the left figure. Treatment rule 2 is depicted in the right figure. The grey shaded areas denote the neighborhoods where the identification assumptions plausibly hold. In general, these areas do not need to be symmetric around the cutoffs.

Now, let \( Y_i(1) \) and \( Y_i(0) \) denote the potential outcomes of unit \( i \) under treatment and control, respectively. 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 and that there are no hidden versions of the treatment beyond those encoded by the treatment assignment indicator. In our framework, SUTVA is required only for units in a neighborhood of the cutoffs. Finally, let \( X_i \) denote a vector of observed, pretreatment covariates other than the running variables.
3.2. Estimand. We wish to estimate the average treatment effect in a neighborhood of the cutoffs where the treatment assignment is unconfounded given the observed covariates (which we term the “discontinuity neighborhood”). We call this estimand the Neighborhood Average Treatment Effect (NATE) and define it as

$$\tau_{\text{NATE}} = \mathbb{E}\{Y_i(1) - Y_i(0) \mid N_i = 1\} = \mathbb{E}\left[\mathbb{E}\{Y_i(1) - Y_i(0) \mid X_i, N_i = 1\} \mid N_i = 1\right].$$

In our case study, this is the average effect of grade retention on criminal and educational outcomes for similar students in the discontinuity neighborhood. Possibly, this average effect varies across treatment rules; e.g., the effect of repeating under rule 1 differs from rule 2. Likewise, the effect of repeating could vary across running variables within the same rule; e.g., the effect of repeating differs across school subjects (see Wong, Steiner and Cook, 2013, and Choi and Lee, 2018, for a related discussion and methods). Furthermore, the effects could be modified by covariates; e.g., the effect of repeating is different for boys and girls. In brief, within the discontinuity neighborhood the effects can be heterogeneous on the treatment rules, running variables, and the covariates. From (5), we note that \( \mathbb{E}\{Y_i(1) - Y_i(0) \mid X_i, N_i = 1\} \) is the Conditional Average Treatment Effect (CATE) in the discontinuity neighborhood (see Chapter 12 of Imbens and Rubin, 2015, for a discussion of the CATE). Under the following assumptions, all these heterogenous effects can be analyzed as in observational studies with strongly ignorable treatment assignment, albeit confined to a discontinuity neighborhood.

3.3. Assumptions.

3.3.1. Assumptions for identification. In this section we state the assumptions needed to identify the NATE.

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

**Assumption 1a**

\[
\{R_{ij} : j = 1, \ldots, J\} \perp \perp \{Y_i(1), Y_i(0)\} \mid X_i, N_i = 1
\]

**Assumption 1b**

\[0 < \Pr(Z_i = 1 \mid X_i, N_i = 1) < 1\]

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 — and, therefore, the assignment to treatments — are independent of the potential outcomes given the observed covariates. Assumption 1b states that in this discontinuity neighborhood every unit has a positive probability of receiving any version of the treatment given the observed covariates. See Cattaneo, Frandsen and Titiunik (2015) and Li, Mattei and Mealli (2015) for a discussion of these assumptions under the local randomization framework with simple treatment rules.

Under Assumptions 1a and 1b, we can non-parametrically identify \( \tau_{\text{NATE}} \) in (5) using the following identity:

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

In other words, under Assumptions 1a and 1b, a discontinuity design can be analyzed as an observational study under strong ignorability (Rosenbaum and Rubin, 1983), but confined to a discontinuity neighborhood. As we discuss below, this “local strong ignorability”
assumption facilitates a series of tasks that are typically precluded in discontinuity designs under usual assumptions such as handling multiple treatment rules and generalizing effect estimates. It also suggests using other methods for outcome analyses normally used in observational studies such as those described in Section 4. For a more detailed discussion about the assumption of strong ignorability in observational studies, see, e.g., Chapter 12 of Imbens and Rubin (2015).

Assumptions 1a and 1b 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 typical ones in the continuity-based framework; 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. In both cases, these are local estimands (as opposed to estimands for the general population), although the local randomization framework can facilitate the generalization of causal inferences to other populations (see Section 3.3.2). We can say 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 typical local randomization settings, because in many applications local independence conditional on covariates is a more realistic assumption than unconditional local independence. In our case study, even in the smallest possible discontinuity neighborhood there are imbalances in relevant covariates measuring the student’s ability. However, after adjusting for these and other educational and sociodemographic covariates, treatment assignment can be considered as-if randomized in a discontinuity neighborhood, because other covariates are balanced, but not across the entire range of the running variables. The validity of these assumptions is, of course, context-specific and must be carefully evaluated in the setting under study.

### 3.3.2. 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 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 because they require treatment assignment to be strongly ignorable throughout the entire range of the running variables.

Let $\mathcal{P}$ be a target population and $\mathcal{S}$ be the sample of units from $\mathcal{P}$ selected into the neighborhood of the cutoffs where assumptions 1a and 1b are believed to 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; Kern et al. 2016)

$$\tau_{TATE} := \mathbb{E}_{\mathcal{P}} \{ Y_i(1) - Y_i(0) \},$$

that is, the average treatment effect in the target population $\mathcal{P}$.

- **Case 1: $\mathcal{P}$ such that $N_i = 1$ 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**

$$\{ Y_i(1), Y_i(0) \} \perp S_i \mid X_i, N_i = 1$$

**Assumption 2b**

$$0 < \Pr(S_i = 1 \mid X_i, N_i = 1) < 1$$

Assumption 2a states that selection into the sample is independent of the potential outcomes given the observed covariates and that the running variables take values in the neighborhood of the cutoffs. Assumption 2b states that every unit in the target population $\mathcal{P}$ has a positive probability of selection into the sample given identical conditions on the observed covariates and running variables.

Under assumptions 1a, 1b, 2a, and 2b, we can identify $\tau_{TATE}$ from

$$\mathbb{E}_{\mathcal{P}} \{ Y_i(z) \mid N_i = 1 \} = \mathbb{E}_{\mathcal{P}} \left\{ \mathbb{E}(Y_i \mid S_i = 1, Z_i = z, X_i, N_i = 1) \mid N_i = 1 \right\},$$

for $z = 0, 1$.

- **Case 2: $\mathcal{P}$ such that $N_i \neq 1$ for some $i \in \mathcal{P}$**. In this case, we assume the following two conditions hold.

**Assumption 3** (strong ignorability of study selection).

**Assumption 3a**

$$\{ Y_i(1), Y_i(0) \} \perp S_i \mid X_i$$

**Assumption 3b**

$$0 < \Pr(S_i = 1 \mid X_i) < 1$$

Assumptions 3a and 3b are stronger than Assumptions 2a and 2b. Under these assumptions, we need to modify assumptions 1a and 1b in order to identify $\tau_{TATE}$. In fact, under these assumptions we must have strong ignorability of treatment assignment via the running variables; that is, $\{ R_i j : j = 1, \ldots, J \} \perp \{ Y_i(1), Y_i(0) \} \mid X_i$ and $0 < \Pr(Z_i = 1 \mid X_i) < 1$. In other words, the assignment of the running variables must be strongly ignorable given the observed covariates throughout the entire range of the running variables. Clearly, these are strong assumptions that deviate from traditional discontinuity settings.

\[\text{This assumption can be relaxed to require conditional mean independence only; that is, } \mathbb{E}(Y_i(z) \mid X_i, S_i = 1, N_i = 1) = \mathbb{E}(Y_i(z) \mid X_i, N_i = 1).\]
4. Estimation and inference.

4.1. Matching approaches. Let \( I_T \) and \( I_C \) be the set of indices of the treated and control units in \( S \). Specifically, let \( I_T = \{ i : Z_i = 1 \text{ and } N_i = 1 \} \) and \( I_C = \{ i : Z_i = 0 \text{ and } N_i = 1 \} \). Following Zubizarreta, Paredes and Rosenbaum (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

\[
\arg\max_m \left\{ \sum_{t \in I_T} \sum_{c \in I_C} m_{tc} : \right. \\
\left. \left| \sum_{t \in I_T} \sum_{c \in I_C} m_{tc} \left\{ B_q(X_t) - \sum_{i \in \mathcal{P}} B_q(X_i) \right\} \right| \leq \delta_q \sum_{t \in I_T} \sum_{c \in I_C} m_{tc}, \quad q = 1, \ldots, Q; \right. \\
\left. \left| \sum_{t \in I_T} \sum_{c \in I_C} m_{tc} \left\{ B_q(X_c) - \sum_{i \in \mathcal{P}} B_q(X_i) \right\} \right| \leq \delta_q \sum_{t \in I_T} \sum_{c \in I_C} m_{tc}, \quad q = 1, \ldots, Q; \right. \\
\left. m_{tc} \in \{0, 1\}, t \in I_T, c \in I_C \right\}
\]

where \( m_{tc} \) are binary decision variables that determine whether treated unit \( t \) is matched to control unit \( c \); \( B_q(X_t) \) are suitable transformations of the observed covariates that span a certain function space; and \( \delta_q \) is a tolerance that restrains the imbalances in the functions \( B_q(\cdot) \) of the covariates (see Wang and Zubizarreta, 2020, for a tuning algorithm). Thus, the matching given by (6)-(9) is the maximal size pair-matching that approximately balances the transformations \( B_q(\cdot) \) of the covariates relative to the target population \( \mathcal{P} \). Following Zubizarreta, Paredes and Rosenbaum (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 (2002a) or the large sample approximations in Abadie and Imbens (2006).

4.2. Weighting approaches. The previous matching approach (6)-(9) can be seen as a form of weighting where the weights are binary (or a constant fraction) that encode an assignment between matched units. An alternative is to relax this matching problem into a weighting problem as follows,

\[
\arg\min_w \left\{ \sum_{i \in I_C} (w_i - \bar{w})^2 : \right. \\
\left. \left| \sum_{i \in I_C} w_i Z_i B_q(X_i) - \frac{1}{|I_T|} \sum_{i \in I_T} B_q(X_i) \right| \leq \delta_q, \quad q = 1, \ldots, Q; \\
\left. \sum_{i \in I_C} w_i = 1, w_i \geq 0, i \in I_C \right\}
\]

These are the weights \( w^* \) 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).
4.3. Regression-assisted approaches. The above matching and weighting approaches can be complemented with additional regression adjustments. 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

$$\hat{\tau}_{\text{RAM}} = (\bar{y}_T - \bar{y}_C) - (\bar{x}_T - \bar{x}_C)\hat{\beta}$$

where $\bar{y}_T$ and $\bar{y}_C$ are the means of $Y$ in the matched treated and control samples, and analogously, $\bar{x}_T$ and $\bar{x}_C$ are the means of $x$. The vector $\hat{\beta}$ comprises the estimated linear regression coefficients of the matched-pair differences in outcomes on the matched-pair differences in covariates.

For weighting, following Athey, Imbens and Wager (2018), we may use a Regression Assisted Weighting (RAW) estimator of the form

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

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, Imbens and Wager (2018) and Hirshberg and Wager (2018). See Robins, Rotnitzky and Zhao (1994) and Abadie and Imbens (2011) for related estimators.

5. Selection of the neighborhood. A central question in practice is how to select a neighborhood for analysis; that is, the region around the cutoffs where the identification assumptions are likely to hold. Assumptions 1a and 1b require that at least one neighborhood exists. In order to maximize the precision of the study, we will find the largest neighborhood where the assumptions are plausible.

Assumption 1a is not directly testable from the observed data. However, there are implications of this assumption that can be tested. In this paper, we present two sets of methods for testing such implications and selecting the neighborhood. The first set of methods is design-based in the sense that they do not use outcome information. The second set of methods is semi-design-based in that they use outcome information, but in a planning sample, separate from the sample used for the actual outcome analyses. We present this second set of methods in Appendix G in the Supplementary Materials.\(^6\)

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

To assess Assumption 1a, we may test for joint, multivariate covariate balance on $X^{\text{test}}$ conditional on $X$. For instance, we may use the cross-match test to compare multivariate distributions (Rosenbaum, 2005; Heller, Rosenbaum and Small, 2010). We will consider the assumption of local unconfoundedness to be plausible if we fail to reject the null hypothesis that $X^{\text{test}}$ has the same multivariate distribution across treatment groups after adjusting for $X$. In addition, we may test the validity of Assumption 1a by checking the following balance conditions

\(^6\)See Cattaneo, Frandsen and Titiunik (2015), Li, Mattei and Mealli (2015), and Cattaneo and Vazquez-Bare (2016) for methods for selecting a neighborhood in discontinuity designs in the local randomization framework. Also, see Branson and Mealli (2018) for related methods in the setting of local randomization given covariates with general assignment mechanisms under simple treatment rules.
(15) \[ E \{ h(X_i^{\text{test}}) \mid Z_i = 1, X_i, N_i = 1 \} - E \{ h(X_i^{\text{test}}) \mid Z_i = 0, X_i, N_i = 1 \} = 0, \]

for any function \( h(\cdot) \). For instance, (15) can be tested after matching using randomization techniques. The idea is to find the largest neighborhood of the cutoffs such that \( X_i^{\text{test}} \) is balanced after adjusting for \( X \) by matching. In practice, a conservative way to select this neighborhood is to implement different tests for univariate and multivariate covariate balance and select the largest neighborhood where we fail to reject the null hypothesis of covariate balance for any of the tests. See Appendix G for details.

6. Impact of grade retention on later life outcomes.

6.1. Finding a neighborhood for analysis. To find the discontinuity neighborhood, we follow the design-based approach described in Section 5, where secondary covariates are available. Here, \( X^{\text{test}} \) is a three-dimensional vector that includes both parents’ education (measured in years of schooling), and the household per capita income (measured in pesos). \( X \) 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, \( X^{\text{test}} \) should be balanced across treatment groups in the discontinuity neighborhood \((N = 1)\), after conditioning on \( X \). We can check the following balance conditions

\[
E(X_i^{\text{test}} \mid Z_i = 1, X_i, N_i = 1) = E(X_i^{\text{test}} \mid Z_i = 0, X_i, N_i = 1).
\]

We use matching to adjust for the observed covariates and select the largest neighborhood that satisfies these conditions. In the spirit of Cattaneo, Frandsen and Titiunik (2015), we implement a sequence of balance tests on \( X^{\text{test}} \) conditioning on \( X \) for a sequence of expanding neighborhoods and select the largest one where we fail to reject the above balance tests. We take the following steps in order to select a neighborhood for analysis.

1. Start with a small neighborhood around the cutoffs.
2. Find the largest matched sample in the neighborhood 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 permutational t-test for mean balance.
   a) If the minimum \( p \)-value is less than the critical value \( p^* \), then repeat steps 1 to 3 starting with a smaller neighborhood.
   b) If the minimum \( p \)-value is greater than or equal to \( p^* \), then expand the neighborhood and repeat steps 2 and 3 until the new minimum \( p \)-value is smaller than \( p^* \).
4. Retain the largest neighborhood with a minimum \( p \)-value greater than or equal to \( p^* \).

In our case study, we set \( p^* \) to 0.1 (Cattaneo, Frandsen and Titiunik, 2015). As \( p^* \) increases, the balance criteria becomes more stringent, and the resulting neighborhood is smaller; we set \( p^* \) to 0.1 because it is is more conservative than setting it to 0.05. In step 2 we match students exactly on age (in months), gender, school, school grade, and past grade retention (number of times). In addition, we match for mean balance for the standardized test scores in language and mathematics. More specifically, we use cardinality matching (Zubizarreta, Paredes and Rosenbaum, 2014) to find the largest pair-matched sample that is balanced according to these two exact matching and mean balance requirements. See Appendix A for a summary of covariate balance before and after matching.

7 Despite the potential cost of dropping a large number of students from the analysis, we view exact matching on age, gender, school, school grade, and past grade retention of primary importance, as it makes Assumption 1a
Table 1 summarizes the process of selecting a neighborhood in our case study. In the first row of the table, we start with a neighborhood defined by $\delta_{qr} = \bar{\delta}_{qr} = 0.1$ 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. After expanding the neighborhood each time by 0.1 for $R_{11}$ and $R_{22}$ symmetrically (see Appendix G for details), we obtain a matched sample of 1,141 pairs comprising 2,282 students, with a neighborhood of rule 1 given by $[3.5, 4.4] \times [4.3, 4.6]$ and a neighborhood of rule 2 given by $[3.5, 4.4] \times [4.8, 5.1]$. For the subsequent larger neighborhood, we reject the null hypothesis of balance on secondary covariates (the minimum $p$-value is 0.03). In our notation, the selected neighborhood of rule 1 is $[c_{11} - \delta_{11}, c_{11} + \bar{\delta}_{11}] \times [c_{12} - \delta_{12}, c_{12} + \bar{\delta}_{12}]$, and the corresponding neighborhood of rule 2 is $[c_{21} - \delta_{21}, c_{21} + \bar{\delta}_{21}] \times [c_{22} - \delta_{22}, c_{22} + \bar{\delta}_{22}]$, where $c_{11} = 3.9$ and $c_{12} = 4.4$ are the cutoff values of $R_{11}$ and $R_{12}$ under the rule 1, $c_{21} = 3.9$ and $c_{22} = 4.9$ are the cutoff values of $R_{21}$ and $R_{22}$ under the rule 2, with $\delta_{11} = 0.4$, $\delta_{12} = 0.5$, $\delta_{21} = 0.1$, and $\delta_{22} = 0.2$.

Table 1

| Selection of a neighborhood for analysis |
|-----------------------------------------|
| Neighborhood                           | Matched sample size | $p$-value for the cross-match test | $p$-value for no effect on secondary covariates | Plausibility of Assumption 1a |
|-----------------------------------------|---------------------|-----------------------------------|-----------------------------------------------|-------------------------------|
| Rule 1 $[3.8, 4.1] \times [4.3, 4.6]$ | 243                 | 1.00                              | 0.33                                          | ✓                            |
| Rule 2 $[3.8, 4.1] \times [4.5, 5.1]$ | 243                 | 1.00                              | 0.87                                          | ✓                            |
| Rule 1 $[3.7, 4.2] \times [4.3, 4.6]$ | 524                 | 1.00                              | 0.87                                          | ✓                            |
| Rule 2 $[3.7, 4.2] \times [4.5, 5.1]$ | 524                 | 1.00                              | 0.87                                          | ✓                            |
| Rule 1 $[3.6, 4.3] \times [4.3, 4.6]$ | 841                 | 1.00                              | 0.87                                          | ✓                            |
| Rule 2 $[3.6, 4.3] \times [4.5, 5.1]$ | 841                 | 1.00                              | 0.87                                          | ✓                            |
| Rule 1 $[3.5, 4.4] \times [4.3, 4.6]$ | 1,141               | 1.00                              | 0.87                                          | ✓                            |
| Rule 2 $[3.5, 4.4] \times [4.5, 5.1]$ | 1,141               | 1.00                              | 0.87                                          | ✓                            |
| Rule 1 $[3.4, 4.5] \times [4.3, 4.6]$ | 1,359               | 1.00                              | 0.87                                          | ✓                            |
| Rule 2 $[3.4, 4.5] \times [4.5, 5.1]$ | 1,359               | 1.00                              | 0.87                                          | ✓                            |
| Rule 1 $[3.3, 4.6] \times [4.3, 4.6]$ | 1,578               | 1.00                              | 0.87                                          | ✓                            |
| Rule 2 $[3.3, 4.6] \times [4.5, 5.1]$ | 1,578               | 1.00                              | 0.87                                          | ✓                            |

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 permutation $t$-test for matched pairs of differences on secondary covariates.

Because it does not require outcome information, this procedure for selecting the neighborhood is part of the design (as opposed to the analysis) stage of the study (Rubin, 2008). As such, it can help investigators preserve the objectivity of the study and maintain the validity of its statistical tests. In this procedure, the choice of the covariates in $X$ and $X^{test}$ requires careful consideration of the treatment assignment mechanism in the problem at hand. In our study, the assignment mechanism can vary across schools and grades, hence we matched exactly for both of these covariates in $X$. Conditional on these covariates, around a neighborhood of the cutoffs, the student’s educational readiness can play an important role in the assignment, hence we further adjusted for all the available standardized test scores.8

Given all these covariates in $X$, we have applied the ideas in Imbens and Rubin (2015, Chapter 21) for assessing unconfoundedness. Ideally, $X^{test}$ will include variables known not to be affected by the treatment given the covariates in $X$. These can be proxy or pseudo-outcomes, such as lagged outcomes measured before treatment. In our data set, we do not have lagged measures of all the study outcomes, but a proxy for them are the measures of social capital corresponding to the mother and father’s education, and the household per capita

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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 school 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.

8As in any observational study based on the assumption of strong ignorability, a possible criticism of our implementation is that we are failing to adjust for a relevant covariate (for example, beyond the household income and education of both parents, their employment status). In this case, it is possible that our estimates are biased. This is something we study through a sensitivity analysis to unmeasured bias.
income. Here, the decision of which variables to include in $X^{\text{test}}$ relies on substantive knowledge about the research question; however, one could consider more data-driven approaches that learn the covariates in $X$ and $X^{\text{test}}$ from the data itself. These approaches, nonetheless, should ponder the importance of separating the design and analysis stages of the study and preserving the validity of the statistical tests if they use the outcomes.

6.2. *Three analyses.* Having found the neighborhood, 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. We consider 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 regression. These analyses have contrasting strengths and can be complementary in practice.

6.2.1. *Visualizing patterns of effects.* In Figure 2, we plot the average school grades of the matched students within the selected neighborhood in years 2008-2011; 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 (that is, in the year immediately after repeating or passing), the students who repeated had higher average grades than the matched students who passed. The difference is approximately 0.2 points (see Section 6.2.3 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 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.\(^9\)

Fig 2: Average school grades of the matched students in the selected neighborhood.

\(^9\)See Appendix B for a similar visualization of average school grades for students from the same grade, for the first time they were taught the material after grade retention.
6.2.2. Estimating effects using randomization tests. Here we estimate the effects (risk differences) of grade retention on repeating another grade, dropping out of school, and committing a juvenile crime. We use the methods in Rosenbaum (2002b) and Zubizarreta et al. (2013).

We test Fisher’s null hypothesis of no treatment effects using McNemar’s test statistic. In our example, this test statistic 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. Figure 3 presents the point estimates and confidence intervals of the NATE on these three outcome variables. The point estimate of the effect of grade retention on juvenile crime is \( \hat{\tau}_{\text{crime NATE}} = 0.006 \) (p-value = 0.229). 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 C for details). Thus, in the absence of hidden bias, there is no evidence that grade retention causes juvenile crime. Similarly, the estimated effect of grade retention on dropping out of school is also very small (\( \hat{\tau}_{\text{drop NATE}} = 0.002 \)) and not statistically significant (p-value = 0.379). However, the estimated effect of grade retention on repeating another grade is \( \hat{\tau}_{\text{ret NATE}} = -0.104 \). 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 C). Thus, in the absence of hidden bias, there is significant evidence that current grade retention causes a reduction in future grade retention. In Appendix H, we analyze the stability of these results to varying the neighborhood size, finding that they are generally robust to modifying the neighborhood for the lowest and second lowest grades, but somewhat sensitive to modifying the neighborhood for the average grade, emphasizing the importance of the selection of the neighborhood. In Appendix F, we provide guidance for generalizing these results.

We also study how these effects vary by gender. This is straightforward because we matched exactly by that covariate. The results are reported in Tables 4 and 5 in Appendix C, and suggest that the effect of grade retention is not modified by gender. Under Assumptions 1a and 1b it is possible to study treatment effect heterogeneity in the discontinuity neighborhood using other regression approaches rather than matching.

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10For these values, however, the covariate balance conditions are not satisfied (hence, these neighborhoods would not be eligible for analysis by the discussed neighborhood selection procedure).
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 address this question with a sensitivity analysis on a near equivalence test (Rosenbaum and Silber, 2009; Zubizarreta et al., 2013). 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 previously documented in the literature on juvenile crime. 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 Appendices D and E for details.

6.2.3. *Estimating effects using balancing weights and regularized regression.* We use the regression assisted weighting estimator (14) to estimate the effect of grade retention on average school grades in subsequent school grades. More specifically, we use the method by Athey, Imbens and Wager (2018) which combines approximately balancing weights and regularized linear regression. We 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 points higher than students who 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 dissipates over time — all this, among students who are comparable in terms of observed covariates and that barely pass or repeat the academic year. A number of mechanisms can explain this result. One of them is that retained students know the material better and are more mature. This is consistent with our other result that students who barely pass tend to repeat more in the future. It is also possible that school grades measure the students’ true ability with error, so that in the neighborhood below the cutoff, we have more students who suffered a negative transitory shock, while above the cutoff we have more students who suffered a positive transitory shock. In that case, it is possible that students that barely repeat are more likely to improve next year, and vice-versa, even if retention has no effect. Although it merits further investigation, this explanation may be limited as we adjust for educational and socioeconomic variables in the four years preceding the intervention of grade retention. This and other possible causal mechanisms that may explain our findings deserve closer examination. Finally, we estimate an almost null effect of grade retention on juvenile crime, with these results being insensitive to small and moderate hidden biases.

7. Further extensions. The proposed framework can also accommodate intermediate outcomes (i.e., principal stratification analyses). For example, it can be extended to instrumental variable (IV) analyses with noncompliance (Angrist, Imbens and Rubin, 1996), yet within a neighborhood of the cutoffs of the running variables. As mentioned, in discontinuity designs the IV approach to noncompliance is known as the fuzzy design, whereas the case with full compliance is known as the sharp design (Hahn, Todd and Van der Klaauw, 2001). In a complex fuzzy discontinuity design (that is, when the treatment assignment is determined by multiple rules that possibly lead to the same treatment, but there is noncompliance), we can nonparametrically identify the average treatment effect for the compliers in the discontinuity neighborhood under two assumptions beyond assumptions 1a and 1b.
Specifically, under the assumptions of local strong ignorability of treatment assignment (1a and 1b), local monotonicity (i.e., that the treatment assignment has a non-negative effect on the treatment exposure for all the units in a neighborhood of the cutoffs), and the local exclusion restriction (i.e., that the treatment assignment affects the outcome only through the treatment exposure for all the units the neighborhood), the average treatment effect for the compliers in the discontinuity neighborhood can be identify following the same arguments as those in the IV approach to noncompliance in randomized experiments (Imbens and Rubin, 2015, chapters 23 and 24), but restricting the analysis to a neighborhood of the cutoffs.

8. Concluding remarks. Since their introduction in 1960 by Thistlethwaite and Campbell, regression discontinuity designs have proven to be 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 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 complex treatment rules, such as those found in our case study. In this paper we have conceptualized a complex discontinuity design as a local randomized experiment conditional on covariates; or more specifically, as an observational study with strongly ignorable treatment assignment given covariates in a neighborhood of the cutoffs. In our case study, we found that grade retention in Chile has a negative impact on future grade retention, but is not associated with dropping out of school or committing a juvenile crime. As discussed, this framework can facilitate the generalization and transportation of causal inferences in discontinuity designs. Under this framework, it is straightforward to take advantage of methods not normally used in traditional regression discontinuity designs, such as simple graphical displays of outcomes as in clinical trials, and potentially statistical machine learning methods to estimate heterogeneous effects. Here, 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. Future work can study the properties of regression assisted estimators in discontinuity designs and extend this framework to principal stratification analyses (see Li, Mattei and Mealli, 2015). In observational studies, discontinuities in treatment assignment rules offer a keyhole through which to see causality. Motivated by the problem of estimating the impact of grade retention on later life outcomes, in this paper we have proposed a framework and methods to leverage them with complex treatment rules.

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